War! What Is It Good For? *

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Abstract

We examine the long-run impact of interstate conflict on real GDP per capita for a cross section of countries over the 1960-2000 period. In order to account for the endogeneity of war and economic outcomes, we adopt an instrumental variables approach that builds on the existing literature. We find strong evidence that interstate war has a negative impact on long-run real GDP per capita. A one standard deviation increase in conflict between 1960 and 2000 results in an 18% reduction in real GDP per capita in 2000. We also find weak evidence that oil-exporting countries are immune to the long run costs of interstate war. This last result is driven almost entirely by the Iran-Iraq War.

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Have you ever witnessed the anger of the good shopkeeper, James Goodfellow, when his careless son happened to break a square of glass? ... every one of the spectators... offered the unfortunate owner this invariable consolation—"It is an ill wind that blows nobody good. Everybody must live, and what would become of the glaziers if panes of glass were never broken?"... But if, as is too often the case, you come to the conclusion that it is a good thing to break windows, that it causes money to circulate, and that the encouragement of industry in general will be the result of it, you will oblige me to call out, "Stop there! Your theory is confined to that which is seen; it takes no account of that which is not seen."

-Frédéric Bastiat, *That Which is Seen and That Which is Not Seen*

I. INTRODUCTION

Frédéric Bastiat's Parable of the Broken Window highlights the difference between something's *cost* and its *opportunity cost*. The bystanders mistakenly assume that if the window had not been broken, then nothing would have been produced. In fact, the full opportunity cost of the broken window includes the bread that the shopkeeper can no longer afford to purchase because he now needs to fix his shop's window. While simple when applied to the shopkeeper, the Parable of the Broken Window is often misunderstood, perhaps nowhere with more unfortunate consequences than with regards to the economic cost of war.

Despite the logic of the parable, there is no consensus on the economics of interstate conflicts. For example, Olson (1982) argues that war eliminates distributional coalitions, thereby reducing rent seeking, especially for the "losers" in the conflict. Similarly, Schumpeter (1939) argues that economic growth is stimulated by government spending during wartime, while Kuznets (1964) argues that war stimulates technological improvements. These researchers predict that war, therefore, will have a positive impact on long-run economic performance. In the political science literature, Kugler (1973), Organski & Kugler (1977), and Rasler & Thompson (1985) argue that war has a negative effect in the short-run, especially for the losers, but little to no effect on long-run growth. This has been dubbed the "phoenix" factor. Using cross-country data, Koubi (2005) finds support for the predictions of Organski and Kugler (1977) as well as Olson (1982) by estimating a negative contemporaneous and positive long-run relationship between different measures of war and growth.

In this paper, we investigate the long-run economic impact of interstate conflict. We construct a measure of cross-country conflict which accounts for both the severity and endogeneity of individual confrontations. We then estimate the effect of conflict on the level of real GDP per capita in 2000 controlling for other deep determinants (institutions, integration, and geography). We find strong evidence that war has a negative impact on long-run economic outcomes. An increase in bellicosity of one standard deviation between 1960 and 2000 comes at the cost of an 18% reduction in real GDP per capita in 2000. This would be like Kenya, El Salvador, Ethiopia, or Honduras becoming more like Somali, Zimbabwe, or Congo. Another way of stating our main result is that a 10% increase in bellicosity between 1960 and 2000 comes at a cost of a 2% reduction in 2000 real GDP per capita. An important caveat to our findings is that they depend on us controlling for one influential observation, the Iran-Iraq War (1980-1988). The Iran-Iraq War was one of the most intense conflicts in our period and exerts a tremendous amount of influence on our estimates of the effect of conflict on development.[1] There is little evidence that these countries were adversely affected by their devastating conflict. We attribute this to the fact that oil revenues are a major component of each country's GDP.

There are three issues that need to be considered to properly assess the impact of conflict on economic activity. First, the time horizon is important because the effect of war is likely to differ over time. The data frequency chosen will determine whether short- or long-run impacts are identified. We use 1960 to 2000 cross-sectional data to ensure that we capture a long-run relationship. Second, interstate conflict is a bilateral outcome, while economic performance is largely an independent (or autonomous) outcome. While the determinants of bilateral conflict have been estimated by researchers, past studies of the relationship between conflict and growth have not accounted for the bilateral nature of conflict. Following the trade and growth literature, we estimate the determination of conflict using bilateral data and then sum up to generate an aggregate measure of conflict for each country. Third, conflict is potentially an endogenous variable. Koubi (2005) splits her sample and examines the impact of first-

[1] More precisely, it was the most intense conflict of the period *and* for which the participants still exist. There is a selection bias issue that is unavoidable in growth studies of this sort driven by the fact that we only have GDP data for countries that *existed* in 2000. Thus, we can calculate a conflict measure for North and South Vietnam, but we cannot determine the impact of conflict on their economic growth (though we can for Unified Vietnam and the United States). In general, this bias should result in an *underestimate* of the negative effect of war on output.

period war on second-part growth, while Bloomberg, Hess and Thacker (2006) use lagged conflict to instrument for current conflict. We take a different approach by using the exogenous determinants of bilateral conflict to construct country-specific instruments for conflict.

Our empirical model has three parts. In the first part, we model conflict as a jointly determined outcome between two nations. We use a probit model to estimate the geographic, political, and historical determinants of interstate conflict. In the second part, we create fatality-weighted sums of actual and predicted bilateral conflict. For each country, we add up the actual and predicted bilateral conflict across all potential combatants. We weigh these actual and predicted values by fatalities to control for the severity of each bilateral conflict. In the third part, we include our conflict measure in a deep determinants growth regression where the level real GDP per capita in 2000 depends on institutions, integration, and geography.[2] We find the deep determinants approach appealing for its consistent results, data coverage, and ability to control for endogeneity.

The remainder of our paper proceeds as follows. Section II details our empirical approach and estimation strategy while section III provides information on the data we use. Section IV outlines our core empirical results as well as subsequent sensitivity analysis and Section V concludes.

II. EMPIRICAL APPROACH AND ESTIMATION STRATEGY

A. *Bilateral Conflict Equation*

We model conflict between two nations as the outcome of a joint decision. If the level of joint utility of conflict surpasses that of a state of peace then a conflict occurs, regardless of the initiator. Let u^* represent the unobservable difference in joint utility levels for countries i and j engaging in a conflict relative to the state of peace at time t:

$$u^*_{ijt} = \pi_0 + \pi_1 g_{ijt} + \pi_2 h_{ijt} + \pi_3 p_{ijt} + v_{ijt} \quad (1)$$

[2] Papers which have recently brought the notion of deep determinants of growth to the forefront include Frankel and Romer (1999), Sachs (2003), and Acemoglu, Johnson and Robinson (2001). Rodrik, Subramanian and Trebbi (2004) bring together the most likely candidates for deep determinants in a single empirical framework.

where g are geography variables (i.e. bilateral distance, common border, common language); h are historical factors (i.e. common language, common colonizer); p are political measures (i.e. relative values of democracy, number of communist countries); and v is an error term which is uncorrelated with g, h and p. We denote bilateral variables with lower case letters and cross-country variables with upper case letters.

With u^* unobservable, we define an indicator variable c, which is 1 if the two countries are engaged in a conflict ($u^* > 0$) and 0 otherwise ($u^* \leq 0$). The response probability for a conflict then is,

$$P(c_{ijt} = 1 | g,h,p) = P(u^*_{ijt} > 0 | g,h,p) = \Phi(\pi_0 + \pi_1 g_{ijt} + \pi_2 h_{ijt} + \pi_3 p_{ijt}) \quad (2)$$

where Φ is the standard normal cumulative distribution function and the standard errors of the estimates of π_0, π_1, π_2, π_3 are asymptotically standard normal.

B. *Aggregate Conflict*

We create an aggregate measure of actual conflict for each country for year t as a weighted sum of their bilateral conflicts that year:

$$C_{it} = \frac{1}{J}\sum_{j \neq i}^{J} w_{ijt} c_{ijt} \quad (3)$$

where J is the number of countries in the sample and w is a weight. We also create an aggregate measure of predicted conflict ($\hat{C}$) based on the predicted values for bilateral conflict ($\hat{c}_{ijt}$) yielded by equation (2). We then use equation (3) to produce aggregate measures of actual (C) and predicted ($\hat{C}$) conflicts by then summing over their annual aggregate conflict measures for the years 1960 to 2000.

The weight (w_{ijt}) should capture the severity of the conflict. Potential measures of severity include hostility level, duration, and fatalities. Although each measure is potentially endogenous, we believe that fatalities per day best measures severity, while at the same time limiting the endogeneity.[3]

[3] *Ex-ante*, it is unlikely that leaders know how many fatalities will be sustained when entering a conflict. Further, this should be especially true for conflicts resulting in large fatalities, *ex post*, which are the ones receiving the most weight in our framework.

C. *Cross-Country Income Regression*

The final piece of our empirical approach is a per capita income equation which follows the deep determinants approach of Rodrik, Subramanian and Trebbi (2004).[4] This strategy makes a distinction between "proximate" (i.e. capital accumulation and education) and fundamental, or "deep" (i.e. geography and institutions) determinants of economic growth. Rather than attempting to isolate the impact of each proximate source, we specify that the level of per capita income *Y/pop* depends upon geography (*G*), institutions (*I*), integration (*T*), and conflict (*C*):

$$\ln\left(\frac{Y_i}{pop_i}\right) = b_0 + b_1 G_i + b_2 I_i + b_3 T_i + b_4 C_i + \varepsilon_i \tag{4}$$

where ε is an error term.

We estimate the cross-country income equation (4) using 2SLS. There are three potentially endogenous variables: Institutions (*I*), Integration (*T*) and Conflict (*C*). In the first stage, we estimate the following three regressions:

$$I_i = c_0 + c_1 G_i + c_2 X_i + c_3 Z_i + c_4 \hat{C}_i + \varepsilon_i \tag{5}$$

$$T_i = d_0 + d_1 G_i + d_2 X_i + d_3 Z_i + d_4 \hat{C}_i + v_i \tag{6}$$

$$C_i = e_0 + e_1 G_i + e_2 X_i + e_3 Z_i + e_4 \hat{C}_i + w_i \tag{7}$$

where *G* are exogenous geography measures included in (4), *X* are excluded instruments for *I* (proportion of population speaking English or a European language), *Z* are excluded instruments for *T* (Frankel and Romer's fitted measure for "trade openness"), and $\hat{C}_i$ is our fitted aggregate conflict measure. In the second stage, we regress *Y/pop* on the three fitted values from (5)-(7) and *G*.

Instrumental variables must satisfy two requirements for asymptotic consistency. They must be orthogonal to the error term (validity) and must be correlated with the included endogenous variable (relevance). To test the validity of our instrumental variables, we employ the Hansen *J* statistic to test for orthogonality, keeping in mind that

[4] Easterly and Levine (2003), Dollar and Kraay (2003) and Glaeser, La Porta, Lopez-de-Silane and Shleifer (2004) also follow a deep determinants approach in their analysis.

we can test for validity only when the number of excluded instruments exceeds the number of endogenous variables (over-identification). Relevance is checked by examining the first-stage *R*-square and *F*-statistics.

The recent literature on weak instruments (c.f. Stock, Wright and Yogo, 2002) has shown that mere instrument relevance is insufficient. In other words, rejection of the null of under-identification does not ensure reliable IV inference. With more than one endogenous variable, we use the Shea (1997) partial *R*-square statistic and the Stock and Yogo (2002) weak instrument test to assess the strength of our instruments. The Shea partial *R*-square records the additional explanatory power of the excluded instruments, and unlike the conventional partial *R*-square, takes the inter-correlations of the instruments into account. The Stock and Yogo test for weak instruments tests the null hypothesis that the instruments are weak by comparing the Cragg-Donald statistic to pre-determined critical values under which the size and bias of a nominal 5% test about a parameter of interest were actually *r* percent.[5]

III. DATA

We construct bilateral and cross-country datasets for 150 countries from 1960 to 2000. We use the Dyadic Militarized Interstate Dispute (DYDMID) Dataset Version 2.0 as our source of bilateral conflicts. DYDMID codes each dispute as "Threat to Use Force", "Display of Force", "Use of Force", and "War". We follow Martin, Mayer and Thoenig (2007) and define conflict as those disputes recorded as "Use of Force" and "War". Table 1 shows the size and breakdown of our bilateral sample.

[Insert Table 1 About Here]

We follow Martin, Mayer and Thoenig (2007) in choosing the geographical, cultural and political determinants of bilateral conflict. For geography, we use years since last conflict, log of bilateral distance, log of the sum of surface area, and dummies

[5] The Cragg-Donald statistic is the minimum eigenvalue of the generalized *F*-statistic from the first stage regression.

for conflict in previous year and common border. For cultural variables, we use dummies for common language, common legal system, common colonizer, and colonizer-colonist pair. We obtain the geography and cultural data from the CIA World Factbook (2007) and CEPR (2006). For political factors, we include the number of GATT/WTO members, number of communist states, dummy of a lagged defense alliance, difference in Polity, sum of Polity, log sum of military personnel, and lagged difference in military personnel. The defense alliance data comes from the COWS Project. The Polity measure is a composite of democratic institutions and comes from the Polity IV database. The military personnel data comes from National Material Capabilities (v3.02) dataset.

We estimate causalities per conflict using two sources. Our primary source is the Battle Deaths dataset of The Centre for the Study of Civil War (CSCW). The CSCW provides estimates of casualty battle deaths (soldiers and civilians killed in combat) for over 200 armed conflicts. Our secondary source is the categorical fatality variable from the DYDMID dataset where 0 = 0, 1 = 1-25, 2 = 26-100, 3 = 101-250, 4 = 251-500, 5 = 501-999, 6 = 1000+ deaths. We use the median value of each fatality category to generate a continuous conflict estimate for fatality categories 1-4. For casualties greater than 500, we use the more precise estimates from the CSCW so that our weights are not bounded by 1,000.

For the cross-country regressions, we obtain estimates of real GDP per capita in 2000 from the NYU DRI Global Development Network Growth Database. We measure institutional quality using "Rule of Law" from Kaufman, Kraay and Mastruzzi (2005). We use the proportion of a country's population that speaks English or a European language from Alesina, Devleeschauwer, Easterly, Kurlat and Warziarg (2003) to instrument rule of law. Our measure of integration is the sum of nominal imports and exports divided by nominal GDP. We use fitted trade from Frankel and Romer (1999) to instrument integration. We measure geography as distance from the equator reported in the CIA Factbook (2006).

IV. EMPIRICAL RESULTS

A. *Probit Results*

In order to disentangle the effect of conflict on output from the effect of output on conflict we need to construct an instrument which is highly correlated with actual conflict but uncorrelated with other factors that determine economic outcomes. To do this we rely on a probit model based on equation (2), which is then used to predict conflict between country pairings. We construct our weighted predicted conflict measure for each country as,

$$\hat{C}_{it} = \frac{1}{J}\sum_{j \neq i}^{J} w_{ijt}\hat{c}_{ijt} \tag{8}$$

where $\hat{c}_{ijt}$ is the predicted probability of conflict between dyad *ij* in year *t* and w_{ij} is the weight used for each predicted conflict. The weight used is the number of casualties per day for country *i* in dyad *ij* in year *t*:

$$w_{ijt} = \left[\frac{\textit{total casualties}}{\textit{conflict days}}\right]\gamma_i \tag{9}$$

where *total casualties* is the total amount of casualties in the conflict occurring between countries *i* and *j* at time *t*, *conflict days* is the length of the conflict in days, and γ_i is the share of casualties borne by country *i* in the war.[6] We assume that casualties are evenly distributed across the length of the conflict.

We sum from 1960 to 2000 to create a single predicted conflict variable for each country.

$$\hat{C}_i = \sum_{t=1960}^{2000} \hat{C}_{it} \tag{10}$$

The procedure for constructing the actual conflict variable is the same as (9) and (10) except we sum using the dichotomous variable indicating the actual occurrence of

[6] When data is not available on shares, we assume the casualties are distributed evenly.

conflict rather than the predicted probability of conflict. We consider a conflict only those disputes with the highest hostility levels. Any conflict labelled "use of force" or "war" are considered a conflict and thus recorded with a 1, while those disputes of hostility level of "threat" or "display of force" or "none" are recorded with a 0.

We present the results of the probit model described by equation (2) in Table 2. We consider three specifications. The first includes only geographic variables, the second adds historical factors, and the third adds political variables. Variable choice was dictated by both relevance and coverage in the data set.[7] We use the results for the last specification to construct our predicted conflict for each country pair in every year, $\hat{c}_{ij}$.

The results of all three specifications are consistent with those found in the conflict literature.[8] Consistent with the idea that conflict might be endemic, the coefficient for years since last conflict is negative, while that for past conflict history is positive. In the second specification, the coefficient for surface area is positive, consistent with the idea that countries with larger surface areas are likely to have large minorities that can be a source of conflict with bordering countries (Martin, Mayer and Thoenig, 2007). Interestingly, the coefficient on common language has a positive sign, while that of common legal system is negative. We interpret these results to mean that a common legal system is a more appropriate measure of cultural likeness than a common language. The third specification includes political measures which are often justified under the democratic peace hypothesis (c.f. Levy and Razin, 2004). We find democracies fight each other less than do autocracies and that the more different the political regimes, the more likely they are to fight.

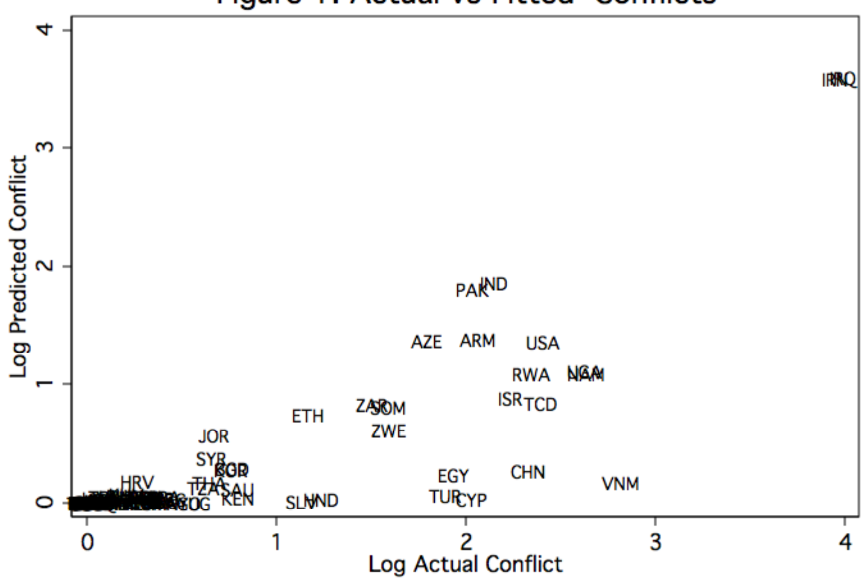
[Insert Table 2 About Here]

[7] Subsoil wealth and UN Affinity were the only two variables dropped in order to assure data coverage. They each had only marginal explanatory power in the model.
[8] See Altfield and De Mesquita (1979) and Martin, Mayer and Thoenig (2007) for examples.

B. *Conflict Measures*

We use equations (8) – (10) to construct the actual conflict (C) and predicted conflict ($\hat{C}$) cross-country variables. For the predicted, we use the predicted values from the last specification to create our bilateral conflict probabilities ($\hat{c}$). Figure 1 plots the log of casualty weighted conflict vs. log of casualty weighted predicted conflict for 150 countries.[9] There is a strong positive relationship (correlation coefficient of 0.83), which indicates that our predicted conflict measure should provide a strong instrument for actual conflict. Further, as one might expect, countries such as Iraq (IRQ), Iran (IRN), Israel (ISR), India (IND), Pakistan (PAK), and the United States (USA) all have fairly high values for causality-weighted conflict (both actual and predicted). Iran and Iraq are the most extreme observations in the data, which is not surprising given the severity of the Iran-Iraq war.[10]

Figure 1: Actual vs Fitted Conflicts



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[9] We actually use the natural log of (1+ conflict measure) due to the presence of zero values.
[10] Removing Iran and Iraq from the data set does not significantly reduce the relevance of the instrument. The correlation between actual and predicted conflict is reduced to 0.79.

C. *Cross-country Regressions*

Table 3 presents the baseline results from our deep determinants specification of cross-country output. The dependent variable is real per capita GDP in 2000. Our variable of interest is conflict. Panel A shows the OLS results and second stage results from our 2SLS regressions. Panel B shows the first stage results from our preferred 2SLS specifications. Tests of the validity and relevance of our instruments are shown at the bottom of the table.

In specification (1) we run an OLS regression on our full sample. The coefficients on institutions, integration, and geography are all positive and significant, as expected. The coefficient on conflict, however, is also positive and significant, indicating that war is beneficial to economic development. This result is consistent with the claims of Koubi, Olson, and Organski & Kugler. In specification (2) we use 2SLS in an attempt to control for endogeneity. This yields results that are consistent with those of Rodrik and Subramanian in that the coefficient on institutions retains its significance and positive sign, while geography becomes insignificant. Integration, surprisingly, retains its significance, but has a negative sign. More surprisingly still, the sign on conflict remains positive and significant.

One possible reason why conflict has a positive impact in specifications (1) and (2) is that there is an observation exerting undue influence on our estimates. A quick glance at the list of conflicts in Appendix B suggests that there are several extreme observations in the conflict data. In particular, Iran, and Iraq stand out as particularly bellicose countries.[11] Their high conflict numbers are a result of the the Iran-Iraq War which was fought from 1980 and 1988 and included trench warfare, chemical attacks, and "human wave" attacks similar to those used on the Western Front in World War I, resulting in tremendous loss of life on both sides. Appendix C details the tests we perform that lead us to conclude that Iran and Iraq are driving the positive coefficient on conflict. According to these tests, the two conflict observation for Iran and Iraq exhibit extremely high leverage and influence on the estimates of the coefficient on conflict.

[11] North and South Vietnam also have extremely high values for conflict. They are not included in our cross-country regressions, however, since they no longer exist (though Unified Vietnam is included, as is, obviously, the United States).

Leverage is a measure of how far the observations on conflict for Iran and Iraq differ from the mean of the observations on conflict for the entire data set. The leverage of Iran and Iraq are an order of magnitude greater than any of the other observations. Influence is a measure of how much the estimated coefficient on conflict changes when Iran or Iraq is removed from the data set. For Iran, this number is 0.46, for Iraq, it is 0.38 (again, a full order of magnitude greater than any other country's influence). Combined, Iran and Iraq raise the coefficient on conflict by 0.84 standard errors.[12]

We adopt two approaches to deal with Iran and Iraq. In specifications (3) and (4) we run OLS and 2SLS regressions on the sample with all outliers excluded, including Iran and Iraq.[13] The OLS results in (3) are similar to those with the full sample, with the exception that the coefficient on conflict is now negative, though still insignificant. When we control for endogeneity using instrumental variables in (4) we find that the negative sign on conflict quadruples and becomes statistically significant at the 5% level. Furthermore, consistent with results in the literature on cross-country growth, geography and integration lose their significance, while the coefficient on institutions doubles and retains its significance. According to the estimate in (4) a one standard deviation increase in interstate conflict between 1960 and 2000 results in an 18% decrease in real GDP per capita for the country involved.

While excluding Iran and Iraq is easily justified on econometric grounds, this strategy does not sit entirely well with us. This is, after all, a paper on the effect of conflict on economic outcomes, and the Iran-Iraq war was among the most destructive conflicts during our period of study. Therefore, in specifications (5) and (6) we keep all of the countries in our sample, but control for the possibility that oil "insulates" a country from the long-run cost of conflict by including a dummy variable if oil is a country's primary export and by interacting oil with our conflict measure. This approach is similar

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[12] A common cut-off value used to determine if a standardized DFBeta is large is DFBeta > $\frac{2}{\sqrt{N}}$ = .166 for our sample. Also, the standardized DFBeta is calculated based on a regression that does not calculate robust standard errors (using them would be inappropriate). As such, the influence of Iran an Iraq combined easily flip the sign on conflict from negative to positive.

[13] We also exclude Singapore, Belarus, and Turkmenistan. We define an outlier as a country whose measures of leverage, Cook's D, and Dfits are *all* above the critical values suggested by the literature. This is a high standard. See the appendix for details.

(though less extreme) to that taken by Mankiw, Romer, and Weil (1992) in their study of the proximate determinants of growth.[14]

After controlling for oil exports, our OLS result in (5) yields a negative, but still insignificant, sign on the conflict variable. As expected, the sign on the interaction term is positive. The results in (6) using 2SLS show the sign on conflict increasing by a factor of eight and attaining 5% significance. The point estimate on conflict of –0.064 is virtually identical to the coefficient estimate in specification (3). As expected, the sign on the interaction term is positive and significant. Furthermore, the size of the coefficient on the interaction term is almost exactly the same size as the coefficient on conflict, but with the opposite sign. Taken together, this implies that war is costly for long-run development, unless your economy is based on oil, in which case war has little effect on your long-run output. We reserve further discussion of how to interpret this result to the Conclusion.

The first stage results for regressions (4) and (6) indicate that the instruments for our study are both valid and strong. The Hansen tests on specifications (4) and (6) fail to reject the null of orthogonality. The F-test of the joint significance of the excluded instruments is significant at the 1% level in each case (in 6 of 7 cases the F-test value exceeds 10), while the Shea Partial R-squares range between 0.17 and 0.45. The instrument for conflict is particularly relevant. The Cragg-Donald F-statistic exceeds critical values outlined in Stock and Yogo (2002) for both size and bias, indicating that we can reject the null of weak instruments. Each excluded variable enters in with the correct sign and is strongly significant in their respective first-stage regression. We conclude that regressions (4) and (6) provide strong evidence that interstate conflict has a negative impact on long run economic development.

[Insert Table 3 About Here]

What are the channels through which conflict impacts economic growth? Elbadawi (1999) argues that civil conflict increases poverty by reducing the stock of both

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[14] They actually split the sample into "Oil" and "Non-oil" producing countries and base their results on the Non-oil sample.

human and physical capital. Collier (1999) argues that there are five primary channels through which intrastate conflict affects growth: destruction, disruption, diversion, dissaving, and portfolio substitution. "Destruction" is simply the direct negative impact war has on labor and capital. "Disruption" is the increase in cost of "doing business" caused by damaged transportation networks etc…. "Diversion" represents the tendency for governments to reduce spending on police and other social services during periods of war. "Dissaving" occurs if individuals perceive the conflict to be short-run and draw down on their savings in order to smooth consumption. Lastly, "portfolio substitution" will occur if individuals perceive the level of risk at home as increasing due to the conflict. They will therefore invest their money abroad rather than at home.

[Insert Table 4 About Here]

Collier argues that only portfolio substitution will result in a long-run reduction in growth. Thus, to the extent that civil conflict impacts post-war growth , he argues that it should be through physical and human capital accumulation. Our simple OLS regressions of conflict on output per worker, capital per worker, human capital per worker, and total factor productivity are reported in Table 4. The results are consistent with Collier's framework. We find that greater interstate conflict plays a role in reducing a country's level of capital per worker. This effect is significant when we use the sample excluding outliers. The negative effect of conflict on capital per worker is barely insignificant (p-value = 0.125) using the specification that includes Iran and Iraq but controls for oil exports. The sign on human capital per worker is negative but insignificant in both regressions. We hypothesize that the reason we find an effect on physical capital but not on human capital is due to the mobility of factors of production. To the extent that the destructive and disruptive effects of civil conflict exceed those of interstate conflict, we would expect to see an effect on the more mobile factor, physical capital. We also hypothesize that crowding out due to large government debts run up for military expenditures may play a significant role. We leave this as an area for future research.

D. Robustness

Levine and Renelt (1992) show that the results of cross-country growth regressions are highly sensitive to the specification and samples used. In this section we identify and test three possible ways our results could go wrong: (1) sensitivity to alternate measurements of conflict, (2) sensitivity to alternate specifications for growth, and finally, (3) robustness to the inclusion of civil conflict variables.

Recall that our baseline conflict measure for country *i* is based on equation (8) and that our measure of conflict is constructed from a yearly panel containing every dyadic pairing of countries in the sample. For each pair, War = 1 if they are engaged in a conflict that year (use of force or war), otherwise War = 0. When constructing the aggregate measure of conflict for country *i* we add up all the zeros and ones, weighting the instances of war by number of casualties sustained by country *i* per day in the conflict. To the extent that there exists a "true" measure of what constitutes an instance of conflict, there are several sources of bias in our measure.

To begin, in Table 5 we run our baseline IV specification (regression (6) in Table 3) after logging the conflict variable. This actually improves the significance of the coefficient on conflict, while also allowing us to interpret the coefficient estimates as an elasticity. According to the estimate in (1), a 10% increase in conflict leads to about a 2% reduction in 2000 GDP per capita. By weighting conflict using casualties per day, rather than say casualties per year, we give greater weight to short, intense, conflicts. This could be of particular concern for a country like Israel which has fought several relatively brief, but severe, conflicts (i.e. The Six Day War, Yom Kippur War). To test if our measure is driving our results we construct an alternate conflict measure using casualties per year rather than casualties per day and run our base IV specification on it. This result of this measure of conflict is seen in regression (2) where the coefficients retain their correct signs and significance. They coefficient on conflict is smaller, but, given the increased scale of the conflict measure, it is comparable to the coefficients in our baseline specifications.

[Insert Table 5 About Here]

Third, we try another alternate weighting where our measure of conflict is counted only for the first year of a war and the weighting for that year includes the total casualties suffered during the entire conflict (rather than conflict for the given year or day). To see why this may be important, consider that for much of the 1960's, West Germany was engaged in low intensity, long standing, militarized interstate disputes with various eastern block countries. Every year these disputes drag on contributes to the conflict measure we use for West Germany in our baseline regressions. Therefore it may be more appropriate to count only new conflicts, as opposed to ongoing conflicts, in our measure. In regression (3) the coefficient on conflict retains its negative sign and significance.

A fourth potential source of bias in our measure is that it only considers interstate disputes categorized as "use of force" or "war". Therefore we are not capturing lesser forms of conflict (i.e. show of force and threat) in our conflict measure. In order to examine the sensitivity of our results to this assumption we consider all possible conflicts and weigh them according to casualties per day. These results are seen in regression (4) of Table 5. The result are, again, consistent with our core results.

In Table 6, we consider a number of alternate specifications to ensure our results hold up to the inclusion of additional control variables beyond our basic deep determinants approach. We consider in (1) a specification based on Barro (1992) in which we control for real investment as a proportion of GDP, real government expenditures as a proportion of GDP, inflation, rule of law, and life expectancy in 1960. The coefficient on conflict is negative and retains its 5% significance, though it is cut in half.

[Insert Table 6 About Here]

In specification (2) we include a dummy equal to one if a country is "developing", this has no effect on our results. A more subtle approach to geography is taken by Jeffrey Sachs (2003) who argues strongly for including indicators of geographic capacity such as whether a country is landlocked and the percentage of a nation's population susceptible to malaria. We include these as well as the percentage of the population living in temperate areas in specification (3). When we do this conflict becomes insignificant (p-value = 0.199), though, it does retain its negative sign. In specification (4) we include ethnic and language fractionalization, as suggested by Easterly and Levine (1998). When both are included in the baseline specification conflict retains its significance and sign.

As a last test of robustness, we look at whether measures of intrastate conflict drive our results. Echoing the sentiments of other researchers, Paul Collier writes that, “Civil wars are liable to be damaging than international conflicts in several respects. They are inevitably fought entirely on the territory of the country. They are likely to undermine the state: both its institutions such as property rights, and its organizations such as police. By contrast… international wars tend to strengthen the state (Collier 1999, p. 168).” We do not dispute the claim that intrastate conflict may be more harmful to economic development than interstate conflict. We are interested, however, in whether our results are driven by civil conflict. Is it possible that our measure of interstate conflict is simply picking up the variation in civil conflict? Given the propensity for interstate conflict to turn into civil conflict and vice versa, this may be the case.

[Insert Table 7 About Here]

In Table 7 we report the results of our regressions using controls for civil conflict. Specifications (1) through (5) investigate the effect of each of the individual components of the Political Instability Task Force data set: average years of ethnic conflict, revolutionary war, adverse regime change, genocide, and state failure between 1960 and 2000. Ethnic Conflict, Regime Change, and State Failure have the correct, negative, signs but are not significant. Interstate Conflict retains its significance and negative sign when each of these variables are entered into the specification individually. However, when all of the components are included in regression (6) the coefficient on conflict retains its negative sign but becomes barely insignificant (p-value = 0.138).

IV. CONCLUSION

This paper makes three fundamental contributions to the growth literature. First, we develop a unique measure of the intensity of interstate conflict which captures both the frequency and severity of war. Second, we estimate a strong instrument for conflict which is orthogonal to other possible explanations of economic outcomes. Lastly, we find strong evidence that, in addition to its human cost, interstate conflict results in a significant decrease in long-run economic development.

Increasing bellicosity by one standard deviation between 1960 and 2000 comes at the cost of an 18% reduction in real GDP per capita in 2000. This would be like Kenya, El Salvador, Ethiopia, or Honduras becoming more like Somali, Zimbabwe, or Congo. Stated another way, a 10% increase in bellicosity between 1960 and 2000 comes at a cost of a 2% reduction in 2000 real GDP per capita. These estimates seem consistent with common sense and are stable across different specifications for our cross-country regressions.

Our results are consistent with both the literature on intrastate conflict and work by others on interstate conflict. In particular, our conclusions point to a "conflict-poverty nexus" similar to that identified by Bloomberg, Hess, and Thacker (2006). They model interstate conflict as a choice variable for leaders and argue that when a leader is either very "selfish", or if the return to capital is very low, then she can signal her competence during an economic downturn by engaging in conflict. As a result, low growth can lead to conflict which, in turn, leads to continued low growth and, hence, a conflict-poverty trap. They test this model using a simultaneous equation framework and find that there is a significant negative effect of both civil conflict and international conflict on short-run economic growth. While Bloomberg, Hess, and Thacker identify the endogeneity of short-run growth and conflict, they do not identify a long-run effect of conflict on growth. As such, our findings suggest that further research on the conditions under which interstate conflict leads to a poverty trap is warranted.

We were surprised by the result that oil may "insulate" a country from the cost of war. An alternative hypothesis is that there is a non-linear relationship between conflict and economic development. Under such a model, relatively low intensity conflicts (like most of those in our sample) would harm development, but high intensity conflicts (like the Iran-Iraq War) would result in either a "wash" or an improvement in economic outcomes.[15] A nonlinear relationship would be consistent with the theories of Olson, Schumpeter, Kuznets, and Organski & Kugler. Of these theories, Olson's seems to fit the history of post-war Iran best.[16] Olson claimed in the *Rise and Decline of Nations* that war could have a beneficial effect by destroying distributional coalitions which caused a society's institutions to become ossified. Some argue that one consequence of the Iran-Iraq War was that the redistributive coalitions set up by the revolutionaries of 1979 were largely undermined.[17] Others take the more extreme point of view that in the immediate aftermath of the war, "… a handful of academics, politicians, philosophers, and theologians… embarked on a new revolution in Iran, not to secularize the country but to refocus it on genuine Islamic values like pluralism, freedom, justice, human rights, and above all, democracy (Aslan, 2005, p. 253)."

Despite this evidence, the fact that there were economic and political reforms in Iran following its war with Iraq does not constitute sufficient evidence for an Olsonian story about war and economic growth. First, the centerpiece of the fundamentalist revolution, the Ayatollah Khomeini, died in the year following the end of the Iran-Iraq War. To the extent that the War prevented Khomeini from cementing power during the previous eight years, it was responsible for the subsequent reforms, but this is not the story of Olson. To us, a simpler explanation for why Iran appears to have emerged relatively unscathed from its war with Iraq is oil. Oil is a major reason why the USSR supported Iran and why the United States supported Iraq during their long struggle. Furthermore, while the productive capacity of each country was severely diminished by their war, there was no shortage of investors after the conflict to rebuild the infrastructure. In the final analysis, however, it is simply impossible to draw a general conclusion from a single data point. In future research, we will investigate the possibility that oil, or other natural resources, may insulate a country from the cost of interstate conflict.

[15] When we include a quadratic term in the regressions this yields similar results to controlling for oil. The coefficient on conflict is negative and significant, the coefficient on $(\text{conflict})^2$ is positive and significant. The negative effect of conflict on economic development is almost exactly the same.
[16] Iraq is a very difficult observation to interpret given the immediacy with which the First Gulf War followed the Iran-Iraq war (1988 marked the end of one and 1990 the beginning of the other) and the immediacy with which consequences of those conflicts are still playing out in that country.
[17] See Mazarei (1996) for a description of the "populist and redistributive nature of the revolution (p. 289)" as well as the liberalizing economic reforms and undertaken in its aftermath (p. 309).

This paper began with Basquiat's *Parable of the Broken Window* which suggests that a full accounting of the costs of interstate conflict could only result in the conclusion that increased conflict is bad for long-run outcomes. For the most part, Basquiat's intuition has been vindicated by our econometric results. In addition to the horrific human cost of interstate war it also brings serious economic consequences. Policy makers deciding whether to negotiate or fight should understand that conflict does not come cheap in the short run or in the long run.

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Appendix A: Summary Statistics and Regression Tables

Summary Statistics

	Mean	S.D.	Min	Max
lypop6200	8.452	1.186	5.884	10.445
conflict	1.824	6.471	0	52.887
rule9500	-0.001	0.997	-1.885	2.242
nopen6000	70.679	41.058	13.335	318.525
disteq	0.293	0.188	0	0.669
engfracnew	0.055	0.207	0	0.974
eurofracnew	0.195	0.358	0	1.004
ropenfitnew	19.469	14.845	2.3	71.808
predictedconfl	0.721	4.137	0	35.75

*lypop6200=real GDP per capita in 2000 from PWT 6.2, conflict is conflict weighted using casualties per day, rule9500 is rule of law from KKZ, nopen6000 is nominal openness from Frankel and Romer, disteq is distance from the equator, engfrac and eurofrac

Correlations

	lypop6200	conflict	rule9500	nopen6000	disteq	engfracnew	eurofracnew	ropenfitnew	predictedconflict
lypop6200	1								
conflict	-0.07	1							
rule9500	0.82	-0.14	1						
nopen6000	0.24	-0.12	0.14	1					
disteq	0.59	-0.01	0.49	0.05	1				
engfracnew	0.27	-0.02	0.33	0.05	0.06	1			
eurofracnew	0.39	-0.1	0.36	-0.14	0.02	0.51	1		
ropenfitnew	0.14	-0.08	0.22	0.39	-0.1	-0.05	-0.07	1	
predictedconflict	-0.04	0.94	-0.13	-0.1	0.05	-0.02	-0.08	-0.06	1

*lypop6200=real GDP per capita in 2000 from PWT 6.2, conflict is conflict weighted using casualties per day, rule9500 is rule of law from KKZ, nopen6000 is nominal openness from Frankel and Romer, disteq is distance from the equator, engfrac and eurofrac

Table 1: Highest Hostility Level and Count of Bilateral Data, 1960-2000

Highest Level of Hostility	Sample
No Militarized Action	346,070
Threat to Use Force	100
Display of Force	400
No Conflict Subtotal	**346,570**
Use of Force	1,161
War	159
Conflict Subtotal	**1,320**
Total	347,890

Table 2: Probit Specifications (Dependent Variable: Actual Conflict)

Model	(1)	(2)	(3)
Intercept	-2.011*** (0.166)	-2.050*** (0.171)	-1.932*** (0.197)
Years Since Last Conflict	-0.004*** (0.000)	-0.004*** (0.000)	-0.003 (0.000)
Conflict in Previous Year	1.627*** (0.039)	1.618*** (0.039)	1.583*** (0.041)
Distance (log)	-0.287*** (0.015)	-0.287*** (0.016)	-0.253*** (0.018)
Contiguous	0.283*** (0.038)	0.318*** (0.040)	0.391*** (0.044)
Sum of Surface Area (log)	0.072*** (0.005)	0.075*** (0.005)	0.035*** (0.007)
Common Language		0.058* (0.032)	0.130*** (0.036)
Common Legal System		-0.171*** (0.030)	-0.104*** (-0.033)
Common Colonizer		0.068 (0.042)	0.144*** (0.049)
Colonial Pair Post-1945		0.303*** (0.085)	0.126 (0.096)
Number of GATT/WTO Members			-0.079*** (0.023)
Lagged Defense Alliance			-0.102* (0.053)
Sum of Polity2			-0.057*** (0.018)
Difference in Polity2			0.141*** (0.022)
Number of Communist States			-0.126*** (0.034)
Lagged(2) Sum of Military Personnel (log)			0.078*** (0.007)
Lagged(2) Difference in Military Personnel (log)			0.015 (0.011)
N	520,217	507,248	347,890
R^2	0.48	0.48	0.49

Year dummies included but not reported. Robust standard errors in parenthesis

Table 3: The Effect of Conflict on Economic Development

Dependent Variable: Log of real per capita GDP in 2000

Panel A: OLS and Second Stage Results for 2SLS

Specification	(1)OLS	(2)IV	(3)OLS	(4)IV	(5)OLS	(6)IV
Conflict	0.008* (0.005)	0.012** (0.006)	-0.014 (0.015)	-0.063** (0.029)	-0.007 (0.014)	-0.064** (0.029)
Oil*Conflict					0.004 (0.012)	0.064** (0.029)
Geography (Dist. From Equator)	1.549*** (0.321)	0.738 (0.553)	1.219*** (0.337)	0.449 (0.548)	1.690*** (0.311)	0.802 (0.554)
Institutions (Rule of Law	0.822*** (0.050)	1.175*** (0.152)	0.881*** (0.051)	1.178*** (0.147)	0.808*** (0.049)	1.150*** (0.145)
Integration (Trade Share	0.004*** (0.001)	-0.006* (0.004)	0.003*** (0.001)	-0.007 (0.004)	0.003*** (0.001)	-0.008** (0.004)
N	150	150	145	145	150	150
R^2	0.74	0.58	0.75	0.62	0.77	0.59

Panel B: First Stage For Endogenous Variables (full specifications only)

	Specification (4)			Specification (6)			
	Conflict	Institutions	Integration	Conflict	Oil*Conflict	Institutions	Integration
Distance from Equator	-1.627* (0.830)	2.958*** (0.320)	22.115 (14.411)	-1.685** (0.849)	0.004 (0.018)	2.784*** (0.353)	17.903 (18.537)
Fitted Trade Share	-0.005** (0.014)	0.017*** (0.003)	0.903*** (0.181)	-0.004 (0.014)	-0.000 (0.000)	0.020*** (0.004)	1.012*** (0.260)
Predicted Conflict	2.248*** (0.493)	0.027 (0.066)	-6.771** (2.774)	2.240*** (0.494)	-0.001 (0.001)	0.041 (0.065)	-7.495*** (2.887)
Oil*Predicted Conflict				-0.784 (0.493)	1.451*** (0.016)	-0.080 (0.066)	6.615** (2.903)
English Fraction	0.406 (0.494)	0.757** (0.306)	31.880* (16.637)	0.370 (0.500)	-0.002 (0.006)	0.823** (0.327)	36.120** (16.634)
European Fraction	-0.467 (0.317)	0.773*** (0.157)	-21.804*** (6.301)	-0.469 (0.318)	0.000 (0.008)	0.797*** (0.168)	-25.182*** (6.597)
F-Statistic	7.03	16.10	12.69	1555.41	1669.85	16.07	13.68
Shea Part. R^2	0.38	0.27	0.18	0.38	0.45	0.34	0.22

Robust standard errors in parentheses. *, **, *** indicates significance at 10, 5, and 1% level. An oil dummy is included in specifications (5) and (6) but not reported.

Table 4: Channels of Influence								
	Sample Excluding Outliers				Full Sample			
Variable	Output per worker	Capital per worker	Human Capital per worker	TFP	Output per worker	Capital per worker	Human Capital per worker	TFP
Conflict	-0.033* (0.018)	-0.068* (0.037)	-0.002 (0.007)	-0.012 (0.016)	-0.023 (0.019)	-0.057 (0.037)	-0.003 (0.007)	-0.009 (0.016)
Oil*Conflict					0.031 (0.019)	0.055 (0.037)	0.001 (0.007)	0.014 (0.017)
Geography (Dist. From Equator)	1.386** (0.538)	2.183** (0.888)	0.388** (0.166)	0.702 (0.440)	1.493*** (0.502)	2.487*** (0.806)	0.426*** (0.161)	0.644 (0.422)
Institutions (Rule of Law 1995-2000)	0.675*** (0.084)	0.886*** (0.143)	0.175*** (0.029)	0.359*** (0.069)	0.669*** (0.078)	0.875*** (0.130)	0.168*** (0.028)	0.383*** (0.066)
Integration (Trade Share 1960-2000)	0.000 (0.002)	0.003 (0.003)	0.000 (0.513)	-0.003* (0.001)	-0.000 (0.001)	0.003 (0.002)	-0.000 (0.000)	-0.001 (0.001)
R^2	0.69	0.64	0.66	0.44	0.72	0.69	0.66	0.46
N	116	112	112	112	118	114	114	114

Robust standard errors in parentheses. *, **, *** indicates significance at 10, 5, and 1% level. An oil dummy is included in the "full" specifications but not reported.

Table 5: Robustness to Different Weightings for Conflict				
Specification	(1)	(2)	(3)	(4)
Geography (Dist. From Equator)	0.798 (0.557)	0.767 (0.579)	0.850 (0.544)	0.801 (0.560)
Institutions (Rule of Law 1995-2000)	1.151*** (0.144)	1.183*** (0.149)	1.135*** (0.143)	1.159*** (0.145)
Integration (Trade Share 1960-2000)	-0.008** (0.004)	-0.008** (0.004)	-0.007** (0.004)	-0.008** (0.004)
Conflict (per day)*	-0.228** (0.101)			
Oil* Conflict (per day)*	0.230* (0.129)			
Conflict (per year)		-0.0002* (0.0001)		
Oil*Conflict (per year)		0.0002* (0.0001)		
New Conflict			-0.093** (0.041)	
Oil*New Conflict			0.094* (0.050)	
All Conflict				-0.053** (0.026)
Oil*All conflict				0.053** (0.026)
N	150	150	150	150
R^2	0.59	0.58	0.60	0.59

* indicates log(1+ variable). Robust standard errors in parentheses. *, **, *** indicates significance at 10, 5, and 1% level. An oil dummy is included in each regression but not reported.

Table 6: Alternate Specifications				
Specification	(1)	(2)	(3)	(4)
Conflict	-0.031** (0.015)	-0.057* (0.032)	-0.029 (0.023)	-0.046* (0.024)
Oil*Conflict	0.036** (0.016)	0.060* (0.032)	0.026 (0.023)	0.045** (0.023)
Geography (Dist. From Equator)		0.867* (0.501)	0.219 (0.494)	0.931** (0.472)
Institutions (Rule of Law 1995-2000)	0.531** (0.239)	1.449*** (0.271)	0.841*** (0.119)	1.017*** (0.124)
Integration (Trade Share 1960-2000)		-0.005	-0.003 (0.003)	-0.005 (0.003)
Developing		0.917* (0.501)		
Landlocked			-0.222* (0.125)	
Malaria			-1.056*** (0.169)	
Temperate			-0.205 (0.236)	
Ethnic Fractionalization				0.119 (0.384)
Linguistic Fractionalization				-0.524* (0.283)
Real Investment/GDP (1960-2000)	0.015 (0.012)			
Real Government Exp/GDP (1960-2000)	-0.003 (0.005)			
log Inflation (1960-2000)	-0.018 (0.044)			
Life Expectancy (1960)	0.043*** (0.009)			
N	125	150	144	145
R^2	0.86	0.58	0.80	0.68

Robust standard errors in parentheses. *, **, *** indicates significance at 10, 5, and 1% level. Oil dummy included in each specification but not reported.

Table 7: Robustness to Civil Conflict

Specification	(1)	(2)	(3)	(4)	(5)	(6)
Conflict	-0.061*	-0.063**	-0.059**	-0.064**	-0.064**	-0.050
	(0.032)	(0.029)	(0.029)	(0.029)	(0.029)	(0.033)
Oil*Conflict	0.061*	0.063**	0.060**	0.064**	0.064**	0.049
	(0.032)	(0.029)	(0.029)	(0.028)	(0.029)	(0.033)
Geography (Dist. From Equator)	0.804	0.819	0.725	0.809	0.791	0.707
	(0.555)	(0.551)	(0.526)	(0.550)	(0.549)	(0.537)
Institutions (Rule of Law 1995-2000)	1.145***	1.153***	1.138***	1.151***	1.148***	1.144***
	(0.151)	(0.147)	(0.148)	(0.147)	(0.148)	(0.150)
Integration (Trade Share 1960-2000)	-0.008**	-0.008**	-0.008**	-0.008**	-0.008**	-0.007**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Ethnic Conflict	-0.084					-0.149
(avg. 1960-2000)	(0.420)					(0.404)
Revolutionary War		0.121				2.317
(avg. 1960-2000)		(0.541)				(2.234)
Adverse Regime Change			-0.902			0.077
(avg. 1960-2000)			(0.977)			(1.020)
Genocide				0.111		0.375
(avg. 1960-2000)				(0.699)		(0.627)
State Failure					-0.053	-2.252
(avg. 1960-2000)					(0.545)	(2.353)
N	150	150	150	150	150	150
R^2	0.59	0.59	0.60	0.59	0.59	0.61

Robust standard errors in parentheses. *, **, *** indicates significance at 10, 5, and 1% level. An oil dummy is included in all specifications but not reported.

Appendix B: Data

Country	Actual Conflict	Predicted Conflict	Real GDP PC 2000	Rule of Law	Nominal Openness	Distance from Equator
Sweden	0	0	25231.77	1.979101	57.9714	0.658644
Gabon	0	0	10438.83	-0.45251	95.48087	0.004133
Belarus	0	0	10005.07	-1.02838	119.4443	0.588889
Georgia	0	0	3885.835	-0.71594	65.21684	0.467044
Burundi	0	0	698.8438	-0.65939	29.90164	0.037389
Comoros	0	0	1358.78	-1.08308	57.01788	0.129678
New Zealand	0	0	20422.92	2.074844	53.03418	0.409911
Slovenia	0	0	18205.51	0.767062	120.5163	0.511889
Austria	0	0	26999.77	2.056296	64.25744	0.5359
Swaziland	0	0	8517.029	0.050771	158.7142	0.294944
Uruguay	0	0	10739.74	0.573944	33.75251	0.386911
Macedonia	0	0	5270.727	-0.40496	83.40728	0.461111
Bulgaria	0	0	7257.503	-0.14694	97.13074	0.467478
Italy	0	0	22487.21	0.961127	39.42955	0.504611
Germany, Unified	0	0	25061.34	1.897104	45.27875	0.535122
Fiji	0	0	4571.948	-0.33179	107.1849	0.198078
Mauritius	0	0	15121.01	0.842791	110.1065	0.2248
Romania	0	0	5211.109	-0.25198	48.64315	0.494733
Hungary	0	0	11382.95	0.758926	97.23515	0.526878
Uzbekistan	0	0	3543.241	-1.00198	63.47161	0.458578
Ireland	0	0	24947.55	1.806471	101.4295	0.606789
Liberia	0	0	472.437	-1.83008	94.15903	0.070944
Kazakhstan	0	0	6519.556	-0.76711	81.17136	0.492311
Trinidad And Tobago	0	0	14770.03	0.401994	99.21803	0.115756
Lesotho	0	0	1833.902	-0.16383	111.1815	0.328833
Benin	0	0	1251.474	-0.25717	58.21096	0.070711
Norway	0	0	33092.16	2.099875	73.69978	0.666411
Japan	0	0	23970.56	1.706342	20.98419	0.3968
Djibouti	0	0	4375.841	-0.44	122.1072	0.127833
Sierra Leone	0	0	683.7297	-0.88115	46.43814	0.096678
Haiti	0	0	2069.288	-1.24027	52.8351	0.210356
Switzerland	0	0	28831.25	2.242196	64.46687	0.526756
Ukraine	0	0	5002.87	-0.71873	88.79951	0.558678
Poland	0	0	8611.005	0.552551	45.99826	0.558267
Finland	0	0	22740.69	2.082901	53.21679	0.669022
Taiwan	0	0	19183.93	1.013783	80.9557	0.258889
Bhutan	0	0	828.1631	-0.55178	69.79604	0.305322
Slovak Republic	0	0	9696.866	0.185319	105.1298	0.537778
Czech Republic	0	0	13616.58	0.615272	105.1799	0.549444
Moldova	0	0	2217.594	-0.29147	116.2634	0.524067
Papua New Guinea	0	0	4354.577	-0.36084	89.17004	0.073333
Colombia	0	0	6079.678	-0.59198	29.16821	0.0532
Belgium	0	0	24661.91	1.520109	117.5418	0.564856
Madagascar	0	0	822.8821	-0.84265	43.04241	0.210645

Country	Actual Conflict	Predicted Conflict	Real GDP PC 2000	Rule of Law	Nominal Openness	Distance from Equator
Guyana	0	0	3733.182	-0.02009	183.2227	0.064011
Denmark	0	0	27827.28	1.990501	66.73052	0.619089
Singapore	0	0	29433.77	2.154402	318.5249	0.015056
Estonia	0	0	11080.91	0.5343	134.2753	0.652056
Latvia	0	0	8998.11	0.16937	99.99356	0.631756
Dominican Republic	0	0	6497.367	-0.2575	76.38242	0.206233
Spain	0	0	19536.38	1.312453	33.44948	0.415533
Guatemala	0	0	3859.467	-0.70597	39.43448	0.162467
Sri Lanka	0	0	4046.63	0.003562	69.47575	0.076311
Bolivia	0	0	2929.186	-0.51066	49.25203	0.168778
Lithuania	0	0	9160.769	0.09807	114.8018	0.614645
Malawi	0	0	838.9891	-0.39111	58.51167	0.175678
Brazil	0	0	7193.598	-0.16603	13.33487	0.2173
Kyrgyz Republic	0	0	3389.278	-0.75559	84.40914	0.455556
Gambia, The	0	0	953.8588	-0.12608	90.37457	0.1473
Turkmenistan	0	0	7624.23	-1.1733	160.4749	0.444444
Mexico	0	0	8082.091	-0.29488	31.59522	0.186211
Mongolia	0	0	1500.781	0.238397	89.1293	0.5277
Nepal	0.000679	0.000346	1421.009	-0.32332	30.37131	0.307911
Albania	0.002861	0.000141	3796.805	-0.66947	53.69395	0.459022
Mozambique	0.002866	4.11E-05	1093.179	-0.96667	32.9019	0.205545
Togo	0.003861	0.000245	823.1664	-0.99713	68.43782	0.068822
Cote D'ivorie (Ivory Coast)	0.004065	2.08E-05	2171.659	-0.59016	70.97507	0.061067
Malaysia	0.004485	0.002075	11405.5	0.732037	118.0147	0.036322
Kuwait	0.007212	0.002286	25135.37	0.993145	103.0951	0.325844
Tajikistan	0.008652	0.005162	1660.401	-1.371	160.7213	0.420067
Chile	0.008808	0.00538	11430.19	1.281085	44.21433	0.372822
Greece	0.012835	0.003348	13982.39	0.726225	39.79996	0.422867
Guinea-Bissau	0.013	4.27E-05	762.4012	-1.36802	45.16006	0.136244
Zambia	0.014081	0.001179	865.6494	-0.37406	76.37124	0.1438
Panama	0.014171	0.002599	7934.798	0.062861	156.0373	0.102289
Mauritania	0.016165	0.004052	1521.481	-0.54121	103.5747	0.199167
Russia	0.019204	0.004434	9263.46	-0.83013	58.59772	0.618611
Sudan	0.023026	0.012135	1047.713	-1.28976	28.81122	0.156033
Yemen, Republic Of	0.030093	0.011028	1081.908	-0.87321	59.62946	0.1692
Canada	0.030153	6.52E-05	26820.73	1.947712	52.48045	0.485856
Jamaica	0.033977	3.62E-05	4520.838	-0.20016	90.15939	0.200611
Indonesia	0.058599	0.019135	3771.861	-0.75308	43.65985	0.072911
Costa Rica	0.06356	0.028968	8341.47	0.773313	64.52334	0.110456
Laos	0.064104	0.043331	1257.347	-1.14472	34.80877	0.183856
United Arab Emirates	0.064698	0.014526	32181.68	1.152931	115.143	0.259889
Netherlands	0.07416	0.014843	26293.09	1.972693	102.9736	0.576378
Mali	0.07434	0.001993	1046.719	-0.68362	48.56896	0.138978
Burkina Faso	0.080535	0.002159	933.2079	-0.57049	40.78975	0.133878
Tunisia	0.09939	0.054221	6993.312	0.315392	68.23241	0.409067
Australia	0.145262	0.029775	25834.54	1.952671	31.50126	0.357989

Country	Actual Conflict	Predicted Conflict	Real GDP PC 2000	Rule of Law	Nominal Openness	Distance from Equator
C.A.R	0.161892	0.000476	945.1021	-0.57347	50.69456	0.048122
Ghana	0.162021	0.000377	1392.201	-0.08535	41.58611	0.074378
Guinea	0.162403	0.000176	2546.122	-0.97518	54.81076	0.129678
South Africa	0.165335	0.000392	8226.063	0.282406	48.27124	0.323667
Botswana	0.176051	0.061317	7256.446	0.710304	105.2981	0.239289
Qatar	0.188192	0.00036	32260.65	1.145324	87.10003	0.281211
Bahrain	0.190777	0.00263	18652.15	0.847048	178.4671	0.289156
Philippines	0.204571	0.064702	3825.615	-0.22208	54.83133	0.154689
Cambodia	0.235501	0.077775	513.9064	-0.80234	27.84459	0.133622
Croatia	0.307467	0.203056	8979.602	-0.14455	107.5322	0.501111
Venezuela	0.317073	0.001237	7322.97	-0.70745	46.79303	0.109367
Paraguay	0.317073	0.025591	4965.414	-0.69289	46.58158	0.284256
Portugal	0.325463	0.052003	17323.14	1.267467	57.34774	0.431289
Cameroon	0.339182	0.007248	2471.727	-1.05223	48.91125	0.119222
Algeria	0.351351	0.052654	5753.122	-0.73548	53.71523	0.408
Senegal	0.356174	0.058284	1571.367	-0.25785	59.62198	0.164133
Niger	0.361111	0.001769	807.4544	-0.9255	42.82078	0.154178
Ecuador	0.364626	0.014092	4314.442	-0.57939	47.71077	0.022911
Peru	0.373742	0.014444	4204.5	-0.45599	40.85686	0.131044
Nicaragua	0.374694	0.043038	3437.851	-0.80393	53.08548	0.135678
Morocco	0.380873	0.047626	3720.048	0.342184	47.42936	0.373256
United Kingdom	0.452556	0.044979	24666.41	1.966988	49.67957	0.572333
Oman	0.464286	0.005266	16193.18	1.210023	90.3683	0.227167
Nigeria	0.467387	0.007956	1073.93	-1.16567	43.56894	0.0727
Congo, Republic	0.469834	0.009978	1286.19	-1.23176	117.3745	0.040933
France	0.501328	0.058286	25044.54	1.521253	39.00508	0.542856
Argentina	0.547694	0.033181	11331.96	0.20643	13.96021	0.407511
Angola	0.669381	0.002445	1974.977	-1.45301	149.5204	0.098256
Serbia & Montenegro	0.755073	0.003676	2094.67	-1.03177	31.3497	0.488889
Tanzania	0.837468	0.133941	816.6624	-0.42158	46.95716	0.023933
Thailand	0.910305	0.190491	6473.596	0.434043	56.1769	0.153
Syrian Arab Republic	0.932694	0.464998	2000.888	-0.36989	49.56049	0.371756
Jordan	0.952958	0.781259	3901.837	0.449834	99.34966	0.351133
Korea, South	1.140945	0.33248	15702.27	0.755596	54.22968	0.417256
Bangladesh	1.156704	0.343336	1851.156	-0.68593	21.59539	0.265333
Saudi Arabia	1.210739	0.128682	15826.52	0.754303	86.3139	0.256322
Kenya	1.215001	0.041652	1267.716	-0.9106	59.70788	0.0057
El Salvador	2.100906	0.014645	4732.127	-0.3625	56.35361	0.153056
Ethiopia	2.20876	1.121077	725.3652	-0.29672	28.11532	0.100078
Honduras	2.419791	0.032247	2239.656	-0.76784	68.73326	0.157711
Congo, DR. of (Zaire)	3.491198	1.295767	359.1472	-1.88452	75.91726	0
Zimbabwe	3.890726	0.847398	3255.93	-0.32313	61.29331	0.198622
Somalia	3.903275	1.247364	681.633	-1.73807	19.75797	0.118144
Azerbaijan	5.003132	2.961929	3590.968	-0.88574	88.57474	0.448356
Turkey	5.582087	0.062853	5714.591	0.089545	24.5875	0.4578
Egypt	5.922878	0.274927	4535.832	0.205512	47.06608	0.333289

Country	Actual Conflict	Predicted Conflict	Real GDP PC 2000	Rule of Law	Nominal Openness	Distance from Equator
Cyprus	6.614225	0.035118	20456.78	0.827088	104.0097	0.389789
Pakistan	6.637417	5.087451	2477.129	-0.59047	22.96332	0.346367
Armenia	6.860275	3.011856	3471.409	-0.44273	78.45362	0.447267
India	7.52234	5.42813	2643.851	0.139247	13.93653	0.280822
Israel	8.286524	1.427722	22236.9	1.1095	64.03972	0.356489
China	9.273971	0.311005	4001.824	-0.33507	19.44568	0.328456
Rwanda	9.452138	1.9818	1018.07	-0.74016	24.21564	0.022578
Chad	9.932787	1.3224	829.5051	-0.66035	54.11518	0.1153
United States	10.09044	2.903545	34364.5	1.821489	16.92912	0.381778
Uganda	12.78421	2.043629	1057.792	-0.52312	40.06282	0.002533
Namibia	12.84778	1.974847	5268.552	0.854915	129.3955	0.199767
Viet Nam, Unified	15.67118	0.186408	2189.408	-0.68189	83.08805	0.119978
Iran, Islamic Republic Of	50.68167	35.29433	6045.526	-0.56726	46.30224	0.393111
Iraq	52.88725	35.74978	2445.349	-1.5833	50.45964	0.370156

Appendix C: Outlier Analysis

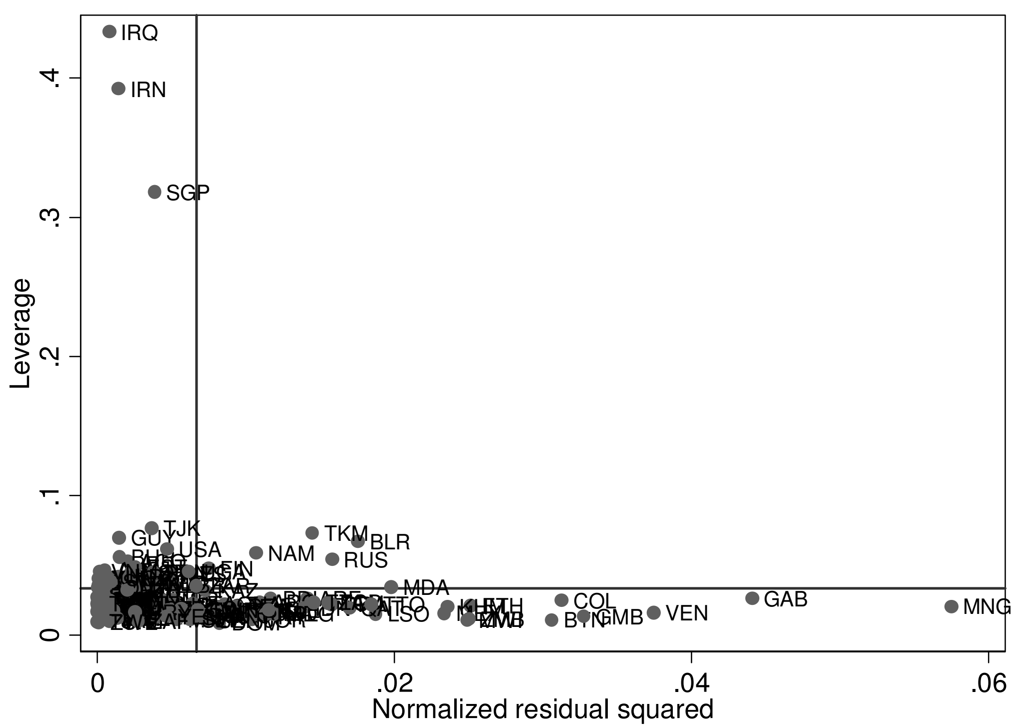
We consider three ways in which an observation can have an unusually large effect on our regression analysis. We first allow for the possibility the observation may be an outlier, which is an observation with a large residual. In the case of an outlier, its dependent variable is not well explained by the predictor variables of the regression. Second we consider leverage, which indicates how far an observation is from its mean, and finally we test for influence, which indicates the degree the removal of an observation from our sample changes an estimated coefficient.

We calculate residuals, leverage, and influence for the 150 observations we have using OLS for the following specification detailed earlier in the paper[18]:

$$\ln\left(\frac{Y_i}{pop_i}\right) = b_0 + b_1G_i + b_2I_i + b_3T_i + b_4C_i + \varepsilon_i$$

Figure C1 plots the leverage of the data points against their squared residual.

Figure C1: Leverage vs. Residual[2]



Several data points are potentially troublesome candidates. In particular, Iran, Iraq, and Singapore exhibit extremely high leverage, while Mongolia and Gabon have particularly high residuals. As well, Turkmenistan, Belarus, Russia (Former USSR), and Namibia all exhibit moderate amounts of leverage and are outliers.

[18] Note that we do not use robust standard errors in this analysis.

Figure C1 suggests that some observations may therefore exert undue influence on the estimates of our coefficients. Accordingly, we test for the observations with the most influence by calculating Cook's D and Dfits for each observation. In Table C1 we list the observations in the sample with values for leverage, Cook's D, and Dfits that are all above their critical values.[19]

Table C1: Outlier Statistics for Influential Observations

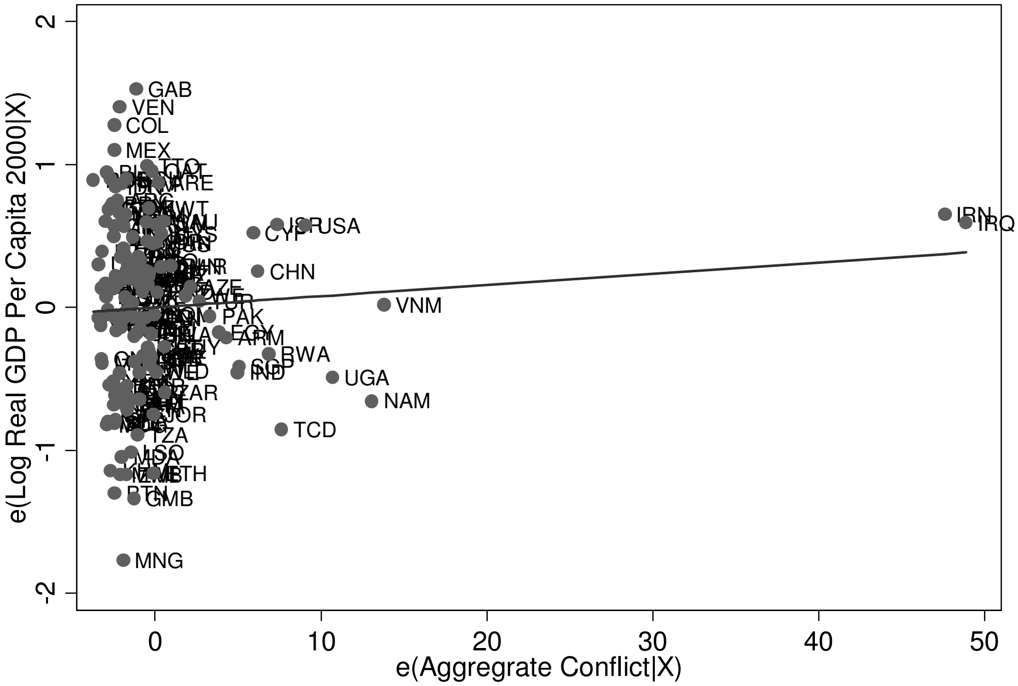
wbcode	Dfits	Cook's D	Leverage	Dfconflict	Dfgeography	Dfintegration	Dfinstitutions
BLR	0.4455835	0.0392324	0.0670817	-0.0643595	0.3460701	0.1918852	-0.3229624
IRN	0.4676142	0.0439329	0.3922635	0.4577787	0.0249995	0.0159662	0.0103099
IRQ	0.3976941	0.031806	0.4329499	0.3805537	0.041331	0.0268735	-0.0455362
SGP	-0.6199849	0.07697	0.3182403	-0.0716368	0.253425	-0.5186808	-0.2320154
TKM	0.4246075	0.0357413	0.0730992	-0.0408934	0.2151854	0.3063054	-0.2765238

Belarus, Iran, Iraq, Singapore, and Turkmenistan all have values of leverage, Dfits, and Cook's D above their critical values. Accordingly, we next examine the actual influence each of these observations has on the estimated coefficients. To do this, we calculate DFBeta for each of the influential observations. DFBeta measures the change in the estimated coefficient (multiplied by the respective standard error) if the influential observation was not included in the regression. For example, Table C1 indicates that if Iran were omitted from the sample, then the coefficient on conflict would be 0.458 standard errors lower than when Iran is in the sample. We have highlighted all the observations with DFBetas greater than 0.20. We see Iran and Iraq exert significant influence on the conflict coefficient, and Singapore on integration. Belarus and Turkmenistan both exert a large amount of influence on coefficients for geography and institutions.

As a final, visual, confirmation of the influence exerted by Iran and Iraq on the coefficient estimate for conflict, consider the following partial regression plots in which Iran and Iraq are included and then excluded from the sample. These plots illustrate the effect of conflict on real GDP per capita, holding constant the effect of institutions, geography, and integration. The change in the estimated slope from positive to negative is clear evidence of what is claimed in the text: That Iran and Iraq are driving the estimated positive coefficients in specifications (1) and (2) in Table 3.

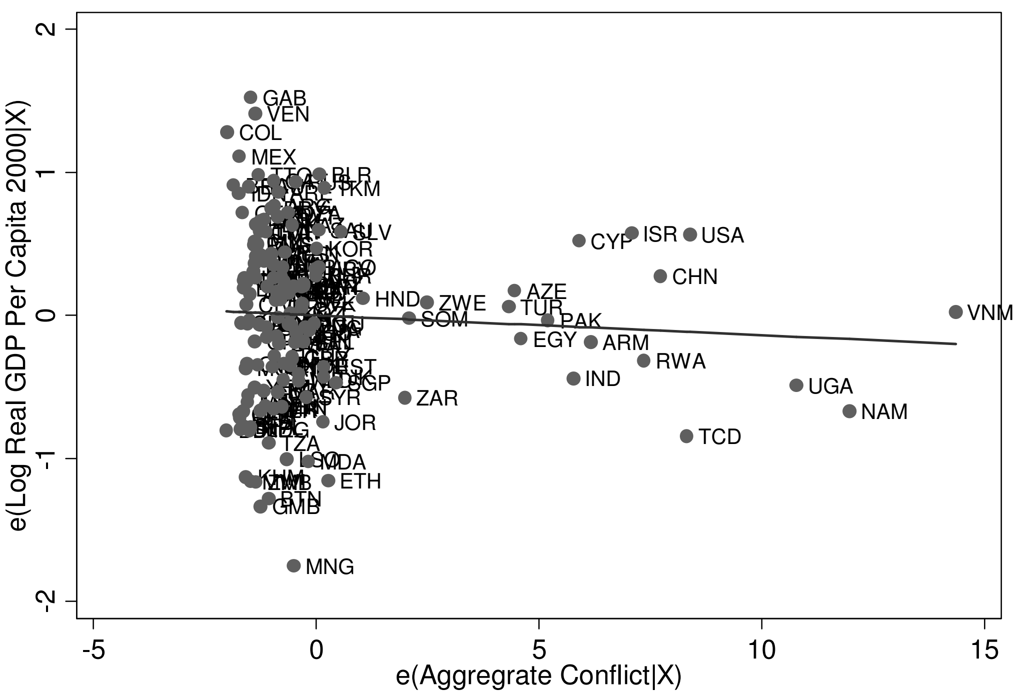
[19] These critical values are: leverage > (2k+2)/n, Cook's D > 4/n, abs(Dfits) > 2*sqrt(k/n), where k is the number of independent variables and n is the sample size. In our case, k = 4 and n = 150.

Figure C2: Partial Regression Plot Using Full Sample



coef = .00786777, se = .00782814, t = 1.01

Figure C3: Partial Regression Plot with Iran and Iraq Excluded



coef = -.01378424, se = .01806982, t = -.76