War! What Is It Good For? *

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Abstract

Whatever gains may come from fighting wars, economic growth is not among them. We examine the long-run impact of interstate conflict on real GDP per capita for a cross section of countries between 1960 and 2000. We construct a fatality-weighted conflict variable that accounts for both the severity and endogeneity of individual confrontations. We include our conflict measure in a deep determinants income regression in which we control for trade, institutions and geography. We find that a 10 percent increase in fatality-weighted conflict over the period 1960 to 2000 results in an average decrease of 1.2 to 1.6 percent in 2000 real GDP per capita.

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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 *cost* and *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. [1]

Despite the logic of the parable, there is no consensus on the economic consequences of interstate conflict or war. Among economists, Olson (1982) argues that war eliminates distributional coalitions, thereby reducing rent seeking, especially for the "losers" of the conflict. Similarly, Schumpeter (1939) and Kuznets (1964) argue that war leads to greater innovation and technological improvements. These researchers predict that war, therefore, will have a positive impact on long-run economic performance. Among political scientists, Kugler (1973), Organski & Kugler (1977), and Rasler & Thompson (1985) argue that war has a negative effect in the

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[1] In fact, Walter E. Williams accused Paul Krugman of committing the broken-window fallacy following the terrorist attacks of September 11th. In a September 14, 2001 article "After the Horror" in the *New York Times*, Krugman wrote:

"Ghastly as it may seem to say this, the terror attack - like the original 'day of infamy' which brought an end to the Great Depression - could even do some economic good. [...] the driving force behind the economic slowdown has been a plunge in business investment. Now, all of a sudden, we need some new office buildings. As I've already indicated, the destruction isn't big compared with the economy, but rebuilding will generate at least some increase in business spending."

While the article did not go as far as claiming the attacks would lead to overall economic gain for the United States, Williams inferred that Krugman was talking about a net economic good for the U.S., and wrote in response in an article "There's No Free Lunch" in October 4, 2001 *Jewish World Review*:

"Would there have been even greater 'economic good' had the terrorists succeeded in destroying buildings in Los Angeles, San Francisco, Chicago, Philadelphia, Boston and all other major cities? Of course, you and I know that is utter nonsense. Property destruction always lowers the wealth of a nation."

short-run, but little to no effect on long-run growth. This has been dubbed the "Phoenix" factor. Using cross-country data, Koubi (2005) estimates a negative contemporaneous and positive long-run relationship between war and economic growth. She interprets her result as support for the predictions of Organski and Kugler (1977) as well as Olson (1982).

In this paper, we examine the long-run economic impact of interstate war. We construct a fatality-weighted index of conflict that accounts for both the severity and endogeneity of individual confrontations. We then estimate the effect of interstate war on the level of real GDP per capita in 2000 controlling for differences in trade intensity, institutional quality and geographic location. Using a sample of 158 countries, we find strong evidence that more interstate conflict results in lower levels of real GDP per person. In particular, we find that, on average, a 10 percent increase in fatality-weighted conflict during 1960 to 2000 leads to a 1.2 to 1.6 percent reduction in real GDP per capita in 2000.

There are three issues that need to be considered to properly assess the impact of interstate 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 cross-sectional data over the 1960 to 2000 period 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. You don't always get to choose whether or not to go to war. 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 probability of conflict using annual bilateral data. We then sum up the probabilities to generate a single measure of conflict for each country. Third, conflict is potentially an endogenous variable. Koubi (2005) splits her sample and examines the impact of first-period war on second-part growth, while Blomberg, Hess and Thacker (2006) use lagged conflict to instrument for current conflict. We choose a different approach and use the exogenous determinants of bilateral conflict to construct country-specific instruments of interstate war.

We construct a three-part empirical model to examine the long-run economic impact of interstate conflict. In the first part, we model conflict as a jointly determined outcome between two nations. We use a probit model to estimate the probability of a war between two countries based on variables which are plausibly unrelated to economic outcomes such as the geographic distance between the nations and the difference in their polity scores. In the second part of the model, we create fatality-weighted sums of actual and predicted bilateral conflict. For each country, we then add up the actual and predicted bilateral conflict across all potential combatants. We weigh these actual and predicted values by average fatalities per day to control for the severity of each bilateral conflict. In the third part, we include our conflict measure in a deep determinants income regression in which we control for trade, institutions and geography.[2] We find the deep determinants approach appealing since it allows for increased data coverage and easier implementation of an instrumental variables approach.

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. Section V concludes.

II. EMPIRICAL APPROACH AND ESTIMATION STRATEGY

A. *Bilateral Conflict Equation*

We subscribe to the "Rationalist" interpretation for why bilateral conflict occurs.[3] According to the Rationalist, the joint utility of two countries at peace is always greater than their joint utility at war. However, the presence of imperfect information may cause disagreements over how to distribute the gains from peace.[4] Accordingly, the probability that two countries will go to war depends on two sets of contributing factors. The first set of factors contributes to the probability of a disagreement over the distribution of the gains from peace (e.g. bilateral distance, common language, prior disagreements, etc.). The second set of factors causes

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[2] Papers by Hall and Jones (1999), Frankel and Romer (1999), Acemoglu, Johnson and Robinson (2001) and Sachs (2003) examine the role of trade, institutions and/or geography as determinants of the long-run level of real GDP per person. Rodrik, Subramanian and Trebbi (2004) combine the three factors together into a single deep determinants framework.

[3] See Grossman (2003) for an example of the Rationalist approach. Powell (1999) surveys different approaches to explaining war. Martin, Mayer, and Thoenig (2008) present a formal model based on the Rationalist approach which results in a probit specification similar to ours.

[4] Each country wishes to maximize its share of the gains from peace. In a world of imperfect information, it is possible to demand a larger share than the other country is willing to accept. This results in escalation towards war. A famous example of such a miscalculation is the underestimation of Chinese tolerance towards the U.S. occupation of North Korea by General MacArthur in October 1950. This miscalculation directly contributed to the run-up to the Korean War.

a disagreement to escalate because of imperfect information (e.g. the nature of political institutions, formal alliances, size of respective militaries).[5]

We define an indicator variable c_{ijt}, which is 1 if the two countries, *i* and *j*, are engaged in a conflict during year *t*; and 0 otherwise. $P(c_{ijt} = 1 | g,h,p)$ is then the probability that *i* and *j* are at war in year *t* conditional on geographical characteristics *g* (e.g. bilateral distance, common border); shared historical factors *h* (e.g. common language, common colonizer); and relative political measures *p* (e.g. relative values of democracy, number of communist countries) that contribute to disagreement and escalation.[6] The response probability for a conflict is then

$$P(c_{ijt} = 1 | g, h, p) \;=\; \Phi(\pi_0 + \pi_1 g_{ijt} + \pi_2 h_{ijt} + \pi_3 p_{ijt}) \tag{1}$$

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. In our approach, the key identifying assumption is that the *bilateral* geographic, historical, and political factors are exogenous to each member pair's *individual* economic outcomes. This assumption allows us to generate a valid instrument for conflict based on the predictions from the probit specification.

B. *Cross-Country Conflict Measure*

We create an aggregate measure of conflict for each country *i* in year *t* as a weighted sum of bilateral conflicts with all countries *j* during year *t*:

$$C_{it} = \frac{1}{J}\sum_{j \neq i}^{J} w_{ijt} c_{ijt} \tag{2}$$

where *w* is a weight, *c* is bilateral conflict and *J* is the number of potential combatants. The weight w_{ijt} should capture the severity of each conflict. Potential measures of severity include hostility level, duration and fatalities. We use fatalities per day of country *i* in conflict between *i*

[5] We recognize that the set of factors which determines the probability of disagreement overlaps with the set which determines the probability of escalation. This is irrelevant, however, to our main goal which is to identify factors which contribute to war that are exogenous to the long-run determinants of economic growth.

[6] The political measures are often justified by the democratic peace hypothesis (c.f. Levy and Razin, 2004).

and j. We believe that fatalities per day provide a good indicator of severity, while limiting potential endogeneity.[7]

We sum equation (2) across 1960-2000 to create a cross-sectional conflict variable for each country i:

$$C_i = \sum_{t=1960}^{2000} C_{it} \ . \tag{3}$$

The procedure for constructing the predicted cross-country conflict variable is the same except we use the predicted probability of bilateral conflict $\hat{c}_{ijt}$ from (1). We then use $\hat{c}_{ijt}$ to create $\hat{C}_{it}$ in (2) and $\hat{C}_i$ in (3).

C. *Cross-Country Income Regression*

The final piece of our empirical approach is a per capita income equation. We focus on the level of real GDP per person as opposed to the growth rate for three reasons. First, levels capture differences in long-run economic performance that are most relevant to welfare as measured by the consumption of goods and services (Hall and Jones, 1999). Second, empirical work by Easterly et al. (1993) document low correlation of growth rates across decades, which suggests that differences in growth rates across countries may be mostly transitory.[8] Third, economic growth models based upon the accumulation of ideas imply that all countries will grow at a common rate in the long run (Parente and Prescott, 1994).

We adopt a deep determinants approach based on the work of Rodrik, Subramanian and Trebbi (2004).[9] The deep determinants approach makes a distinction between "proximate" (e.g. capital accumulation and education) causes and fundamental or "deep" (e.g. geography and institutions) causes of economic development. Rather than attempting to isolate the impact of each proximate source, the deep determinants approach relates the level of real GDP per capita to

[7] *Ex-ante*, it is unlikely that leaders know how many war 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.
[8] In addition, Hauk and Wacziarg (2009) find that OLS applied to averaged cross section data generates less-biased estimates under Monte Carlo simulations than panel data with fixed effects type estimators.
[9] Acemoglu, Johnson and Robinson (2001); Easterly and Levine (2003); Dollar and Kraay (2003); and Glaeser, La Porta, Lopez-de-Silane and Shleifer (2004) also use a deep determinants approach in their analysis.

its essential fundamental sources. We specify that the level of per capita income *Y/pop* depends upon conflict *C*, trade *T*, institutions *I*, and geography *G*:

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

where ε is an *i.i.d.* error term.[10]

We estimate the cross-country income equation (4) using two-stage least squares (2SLS). The identification strategy is to use fitted cross-country conflict to instrument actual cross-country conflict, fitted trade intensity from Frankel and Romer to instrument for trade intensity, and English and European language fractionalization to instrument for institutions. In the first-stage, we estimate the following three regressions:

$$\log(C_i) = c_0 + c_1 \log(\hat{C}_i) + c_2 \log(\hat{T}_i) + c_3 LANG_i + c_4 G_i + u_i \tag{5}$$

$$\log(T_i) = d_0 + d_1 \log(\hat{C}_i) + d_2 \log(\hat{T}_i) + d_3 LANG_i + d_4 G_i + v_i \tag{6}$$

$$I_i = e_0 + e_1 \log(\hat{C}_i) + e_2 \log(\hat{T}_i) + e_3 LANG_i + e_4 G_i + w_i \tag{7}$$

where $\hat{C}$ is fitted conflict, $\hat{T}$ is fitted trade intensity, *LANG* are the two language fractionalization variables, *G* is geography, and (*u*,*v*,*w*) are *i.i.d.* error terms. In the second stage, we regress *Y*/*pop* on the three predicted 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 support the validity of our instrumental variables, we report the Hansen *J* statistic, keeping in mind that 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 *F* statistics.

The recent literature on weak instruments (c.f. Stock, Wright and Yogo, 2002) has shown that mere instrument relevance may not be sufficient. In other words, rejection of the null of under-identification does not ensure reliable IV inference. Thus, we also use the Shea (1997)

[10] The deep determinants approach does not predict the exact functional form (i.e. logs or levels) of the relationship between each deep determinant and real GDP per capita. We therefore conducted specification tests using a generalized R-squared criterion to determine the proper specification of conflict. The generalized R-squared test is a measure of explanatory power for models with endogenous explanatory variables (Pesaran and Smith, 1994). The tests results found that the log of conflict did much better in fitting the data than the level of conflict.

partial *R*-squared statistic and the Stock and Yogo (2002) weak instrument test to assess the strength of our instruments. The Shea partial *R*-squared records the additional explanatory power of the excluded instruments taking the inter-correlations of the instruments into account. The Stock and Yogo tests the null hypothesis of weak instruments 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.[11]

III. DATA

We construct bilateral and cross-country datasets of 159 current plus 9 former nation states from 1960 to 2000.[12] We use the State System Membership List from the Correlates of War (2008) for the entry date and possible exit date of each state.[13] The bilateral dataset is all country pairs or dyads in existence for each year. Between 1960 to 2000, there are 456,484 country pairs, of which 369,999 are in our preferred specification for the probit in equation (1) (results in column 3 of Table 2). The cross-country dataset is a cross-section of 158 current states. We lose Myanmar and the 9 former nations due to a lack of real GDP measure in 2000.[14]

We use the Dyadic Militarized Interstate Dispute Dataset Version 2.0 (DYDMID2.0) of Maoz (2005) as our source of bilateral conflicts. DYDMID2.0 codes each dispute as "Threat to Use Force", "Display of Force", "Use of Force", and "War". We follow Martin, Mayer and Thoenig (2008) and define conflict as those *forceful* disputes recorded as "Use of Force" and "War".

Table 1 shows the frequency of conflict in our bilateral dataset. In the complete sample, there are 1,463 conflicts out of 456,484 possible country pairs for a 0.32% incidence rate. In the preferred sample, there are 1,406 conflicts out of 366,999 country pairs for a 0.38% incidence

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

[12] The 9 former states with exit dates in parenthesis are South Vietnam (1975), North Vietnam (1975), South Yemen (1990), North Yemen (1990), East Germany (1990), West Germany (1990), Soviet Union (1991), Yugoslavia (1991) and Czechoslovakia (1992).

[13] The criteria for being a state after 1920 is that the entity must be a member of the United Nations or League of Nations, or have population greater than 500,000 and receive diplomatic missions from two major powers. Of the 158 current states, 60 had an entry date after 1960.

[14] The fact that our outcome measure is real GDP per person in 2000 introduces a potential selection bias in our sample. It is likely that countries that fought wars and then did very poor economically didn't survive until 2000. This selection bias would bias our results against finding a negative impact of war on income.

rate. Thus, the use of our smaller sample appears to have little effect on the frequency rate of interstate conflict.

[Insert Table 1 about here]

For continuity, we use the variables suggested by Martin, Mayer and Thoenig (2008) to choose the geographical, historical and political determinants of bilateral conflict.[15] For geographical factors, we use years since last conflict, log of bilateral distance, log sum of surface area, and dummies for conflict in previous year and common border. For historical variables, we use dummies for common language, common legal system, common colonizer post-1945, colonizer-colonist post-1945, and were/are part of the same country.[16] We obtain the geography and historical data from CEPII (2008). For political factors, we include the number of GATT/WTO members, number of communist states, dummy of a lagged defence alliance, sum of Polity, difference in Polity, lagged log sum of military personnel, and lagged log difference in military personnel. The defence alliance data comes from Gibler and Sarkees (2004). Polity is a composite measure of democratic institutions and comes from the Polity IV Project (2008). The military personnel data comes from the National Material Capabilities Version 3.02 dataset of Singer (1987).

We obtain fatalities per conflict estimates from two sources. Our primary source is the UCDP/PRIO Armed Conflict Dataset Version 4 of Gleditsch et al. (2002). The Armed Conflict dataset provides estimates of battle deaths (fatalities resulting directly from combat) for over 200 armed conflicts. Our secondary source is the categorical fatality variable from the DYDMID2.0 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 category to generate a continuous conflict estimate for fatality categories 1-3. For fatalities greater than 250, we use the more precise estimate from the Armed Conflict dataset.

We measure fatalities per day for country i with country j in year t as:

[15] We also included resource variables such as subsoil wealth, number of oil producers, number of gas producers, and access to seas. Each variable only had marginal explanatory power in the model.

[16] The common country variable is one if the two countries were or are the same state or the same administrative entity for a long period (25-50 years in the twentieth century, 75 year in the ninetieth and 100 years before). See CEPII (2008) for details.

$$w_{ijt} = \left[\frac{total\ \ fatalities}{conflict\ days} \right] \phi_t \gamma_i \tag{8}$$

where *total fatalities* is total battle deaths of the conflict, *conflict days* is the length of the conflict in days, ϕ_t is the percentage of conflict occurring in year *t*, and γ_i the share of fatalities borne by country *i*. For the large conflicts (i.e. Vietnam war, Gulf war), we use the ARMED CONFLICT DATABASE estimates of fatalities per participant to measure γ_i. However, for most conflicts, we do not have estimates of fatalities per participant so we assume that fatalities are uniformly distributed across bilateral pairs in the conflict to measure γ_i.

For the cross-country regressions, we use estimates of real GDP per capita in 2000 from Penn World Tables 6.2 in Heston, Summers and Aten (2006). We measure trade as openness or trade intensity – sum of nominal imports and exports divided by nominal GDP. The data for trade intensity are obtained from the Penn World Tables 6.2. We use the fitted trade intensity from Frankel and Romer (1999) to instrument trade. We measure institutional quality as "rule of law" from Kaufman, Kraay and Mastruzzi (2005). The instruments English and European language fractionalization is the proportion of a country's population that speaks English and the proportion that speaks a major European language, respectively. We use data from Hall and Jones (1999) and fill in missing values using Gordon (2005). We measure geography as the absolute distance from the equator reported in the CIA Factbook (2008).

IV. EMPIRICAL RESULTS

A. *Probit Results*

We use a probit estimator to generate the predicted bilateral conflict probabilities in (1). We use bilateral data from 1960 to 2000. Table 2 presents the results of three specifications. The first specification includes the geography variables; the second adds the historical factors, and the third adds the political measures. We use the results of the third specification to construct our bilateral conflict probabilities.

[Insert Table 2 about Here]

The results are similar to those found in the conflict literature (c.f. Altfield and De Mesquita, 1979 and Martin, Mayer and Thoenig, 2008). In each specification, the coefficient for years since last conflict is negative, while that for conflict in previous year is positive. The coefficient signs support the idea that wars are endemic. Likewise, the coefficient for surface area is positive. Martin, Mayer and Thoenig (2008) argue that countries with large surface areas are more likely to have large minorities that can be a source of conflict with neighboring countries (like Turkey with Iraq). Interestingly, the coefficient for common language is positive, while that for common legal system is negative. We interpret these signs to mean that a common legal system is a more appropriate measure of cultural likeness than a common language. The coefficients for colonial linkages and same state are all positive, indicating the greater likelihood of conflicts with colonial masters and breakaway states. In the third specification, we find that the coefficient for sum of Polity is negative and that for absolute difference is positive. This suggests that autocracies *in general* are more likely to fight than democracies, but more likely to wage war vs. democracies than other autocracies.

B. *Cross-country Conflict Measures*

We use equations (1), (2), (3), and (8) to construct our fatality-weighted actual (C) and fitted conflict ($\hat{C}$) variables. Appendix B presents the data sorted by actual conflict. The mean values for actual and fitted conflict are 2.2 and 0.8 fatalities per day. However, 37 percent (59 of 158) of our sample experienced no fatalities and thus have a zero for actual and fitted conflict. At the other end of the spectrum; Iraq (IRQ), Iran (IRN), Eritrea (ERI), Unified Vietnam (VNM), Uganda (UGA), Namibia (NAM) and the United States (USA) have an average value of 10 or higher fatalities per day. In particular, Iraq and Iran have values above 50, which are seven standard deviations away from the mean! [17]

Figure 1 plots the log of actual conflict vs. log of fitted conflict.[18] There is a strong positive relationship between actual and fitted conflict. The correlation coefficient is 0.88 in logs and 0.91 in levels. The high correlation suggests that our fitted conflict measure is a good instrument for our actual conflict variable.

[Insert Figure 1 about here]

C. *Cross-country Income Regressions*

[17] In addition, Former South Vietnam and North Vietnam had average fatalities per day of 355 and 380, respectively.
[18] We actually use the log of (1+ conflict measure) due to the presence of zero values.

Table 3 presents the results of our cross-country per capita income regression. The dependent variable is the log of real GDP per capita in 2000. Our variable of interest is fatality-weighted conflict. Panel A presents the OLS and second-stage results of the 2SLS regressions. Panel B shows the first-stage *F* statistics and Shea partial *R*-squared for our preferred 2SLS specifications in columns (4) and (6). The test results for overidentification (Hansen *J* statistic) and weak instrument (Cragg-Donald statistic) are shown at the bottom of panel A.

[Insert Table 3 about Here]

Columns (1) and (2) present the baseline OLS and 2SLS results for our 158-country sample. The coefficients for trade intensity, rule of law and geography are all positive and significant under OLS. Using 2SLS, we get the common result that institutions dominate in that the coefficient for rule of law increases markedly, while those for trade and geography decrease in value and in precision (Rodrik, Subramanian and Trebbi, 2004). For our variable of interest, the coefficient for interstate conflict has its expected negative sign in both (1) and (2), but is not *statistically* significant. However, the point estimates do imply a potentially important *economic* significance for conflict. In particular, a 10 percent increase in fatality-weighted conflict over the 1960-2000 period results in an average decrease in 2000 real GDP per capita of 0.5 to 0.8 percent, holding constant differences in trade, institutions and geography.

One possible reason for the insignificant relationship between conflict and real GDP per capita is the presence of observation(s) exerting undue influence on our estimates. For example, Iraq, Iran and Eritrea are particularly bellicose and stand out as possible outliers in Figure 1.[19] This is especially true for Iran and Iraq whose high conflict values are primarily the result of a single observation, the Iran-Iraq War of 1980 to 1988. The Iran-Iraq War involved trench warfare, chemical weapons (including mustard gas) and "human wave" attacks similar to those used in the Western Front of World War I. According to the Armed Conflict database, there were 644,500 total battle deaths for an average of 133 fatalities per day for Iran and 87 for Iraq over a nine-year period.[20]

[19] North and South Vietnam also have extremely high values for causality-weighted conflict. They are not included in our cross-country regressions since they no longer exist and thus have no value for real GDP in 2000. Unified Vietnam is included although their cross-country data is calculated from 1975 to 2000.

[20] As a comparison, the Armed Conflict database reports that U.S. battle deaths were 45,783 in the Vietnam War (1964-1973) for an average of 14 causalities per day over a ten-year period.

In order to assess the possible influence of Iraq, Iran, Eritrea and other individual countries, we report details on the leverage and squared residuals of each observation in Appendix C. Leverage is a measure of how far an observation differs from the mean of the sample, while a large residual indicates that the observation is not well explained by the model. Iran and Iraq both exhibit a large degree of leverage, while Eritrea has both a large residual and leverage. As a result, these certainly support our prior belief based on Figure 1 that Iraq, Iran, and Eritrea may be possible outliers.

Another possibility for the insignificant relationship between conflict and real GDP per capita is omitted variable bias. Although the deep determinants approach explains close to two-thirds of the variation in our sample, important unobserved factors may remain. These unobserved factors are likely a product of a complex set of historical and geographical issues. There are two approaches taken in the growth literature to control for unobserved effects. The first is to include fixed effects in a panel data structure (c.f. Islam, 1995). The second is to include a set of continental 0-1 dummy variables (c.f. Barro, 1991 or Sala-i-Martin, 1997). We chose the second approach in order to utilize the cross-sectional information of conflict and the other deep determinants.

In columns (3) and (4) of Table 3, we include dummy variables for major oil producers and sub-Saharan Africa.[21] The dummy variables have their predicted signs and are strongly significant in each column. Under OLS, the coefficient for conflict increases in magnitude to -0.089 and is now marginally significant (*p*-value of 0.11). Under 2SLS, the coefficient for conflict increases even more to -0.154 and is significant at the 5 percent level.

In columns (5) and (6), we add dummy variables for Latin America and East Asia. Our results are similar to those in (3) and (4). The coefficient for conflict remains negative though not significant under OLS, and negative and significant under 2SLS. In terms of impact, the point estimates imply that a 10% increase in fatality-weighted conflict decrease real GDP per capita by 1.3 to 1.5% on average.

The 2SLS coefficients in (4) and (6) are unbiased and consistent only if the instruments are valid, relevant and strong. The Hansen *J* statistic fails to reject the null of orthogonality at most conventional levels, which implies that the instruments are valid. In Panel B, the first-stage

[21] There are 12 major oil producers in our sample: Algeria, Bahrain, Gabon, Indonesia, Iran, Iraq, Kuwait, Nigeria, Oman, Saudi Arabia, United Arab Emirates, and Venezuela.

F statistic and Shea partial *R*-squared are high, indicating that the instruments are relevant. The Cragg-Donald statistic exceeds the critical values of Stock and Yogo (2002) in each column. We can therefore reject the null of weak instruments. Taken together, the results in (4) and (6) provide solid evidence that interstate conflict has a negative impact on long-run economic development.

D. *Robustness*

It is well-known that cross-country growth regressions are highly sensitive to specification choice (c.f. Levine and Renelt, 1992 and Sala-i-Martin, 1997). In this section, we test the robustness of the negative link between conflict and economic development to the inclusion of additional explanatory variables and civil conflict measures. To examine the robustness of our specification, we include additional explanatory variables to our deep determinants regression found in (6) of Table 3. We include those variables found to be important by other researchers. These alternative specifications are reported in Table 4. To conserve space, we report the coefficient value (and standard error) for each deep determinant *C*, *T*, *I* and *G* and the *p*-value of a test of significance of the additional variable(s). We report the Hansen *J* and Cragg-Donald statistics at the bottom. For comparison purposes, we show the benchmark result in column (1) where the coefficient for conflict is -0.13 in value.

[Insert Table 4 about Here]

The first three specifications consider the additional roles of geography, disease and health differences. In column (2), we include the geographic controls – landlocked and log of surface area (see Frankel and Romer, 1999). These two variables are jointly significant, while the coefficient for conflict remains negative and significant. Recent work by Sachs (2003) and Carstensen and Gundlach (2006) show that malaria has direct effects on real GDP per capita. In column (3), we include malaria prevalence as an additional factor. Although the coefficient for malaria is negative and strongly significant, the coefficient for conflict remains negative and significant. To control for health differences, we add the log of life expectancy in 1960 in column (4). The coefficient for life expectancy is positive and significant, while the coefficient for conflict remains close to its benchmark value.

The next two specifications examine institutional and historical differences. In column (5), we include ethnic, language, and religious fractionalization measures of Alesina et al.

(2004). The fractionalization measures record the heterogeneity in the population, but are jointly insignificant. The coefficient for conflict remains close to its benchmark value. We next add French and British legal origin in column (6). As with fractionalization, the legal origin variables are jointly insignificant, while conflict remains negative and significant. Although not reported, we also included colonial origin variables and found that the coefficient for conflict remained close to its benchmark value.

Column (7) considers democratic differences where we include the mean and standard deviation of Polity during 1960-2000. The democracy variables are jointly insignificant and not surprisingly have little impact on the coefficient for conflict. In column (8), we examine whether measures of intrastate or civil conflict lie behind our result. Echoing the sentiments of other researchers, Paul Collier writes that, "Civil wars are liable to be more 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. Given the propensity for interstate conflict to become civil conflict and vice versa, this may be the case.

In column (8), we jointly include five measures for civil conflict within our regression: ethnic conflict, revolutionary war, adverse regime change, genocide/politicide, and state failure. The data are from the Political Instability Task Force data set. We average each measure over the 1960 to 2000 period. In our 158 country sample, more than half experienced some form of civil conflict. With these 5 civil war measures included, the coefficient for interstate conflict remains strong and significantly negative. The test for joint significance of our civil conflict measures, however, is insignificant. This is somewhat surprising given the findings of Collier and others (c.f. Collier, 1999 or Murdoch and Sandler, 2004). However, there is a strong negative correlation between civil conflict and rule of law. As a result, most if not all of the explanatory power of civil conflict is taken away by rule of law.[22]

[22] We confirmed this by re-estimating regression (8) excluding rule of law. The coefficients for 2 of our 5 civil conflict measures were negative and statistically significant (the remaining 3 were insignificant) and jointly the 5 measures were highly statistically significant (*p*-value of 0.001). The coefficient for interstate conflict in this regression remained statistically significant and relatively stable with a -0.188 estimate.

Lastly, we examine the impact of interstate conflict on developing economies. To do so, we estimate the model excluding the 31 OECD or developed countries. With a point estimate of -0.15 for conflict, we find a stable negative impact of interstate war on the level of per capita income in the developing world.

IV. CONCLUSION

This paper makes three fundamental contributions to the literatures on growth and conflict. First, we develop a unique measure of interstate conflict that captures both the frequency and severity of war. Second, we construct a strong instrument for conflict which is orthogonal to other possible explanations of economic outcomes. Third, when added to a deep determinants regression, we find evidence that interstate conflict results in a significant decrease in long-run economic development.

We find that a 10 percent increase in fatality-weighted conflict results in an average reduction in real GDP per capita of 1.2 to 1.6 percent. Our estimate is consistent with common sense and is fairly stable across different specifications of our cross-country regression. Our results are also consistent with both the literature on intrastate conflict and works by others on interstate conflict. In particular, our conclusions point to a "conflict-poverty nexus" similar to that identified by Blomberg, 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 Blomberg, 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.

This paper began with Bastiat'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, Bastiat's intuition has been vindicated by our econometric results. In addition to the horrific human cost, interstate war also delivers 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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Figure 1: Actual vs. Fitted Conflicts

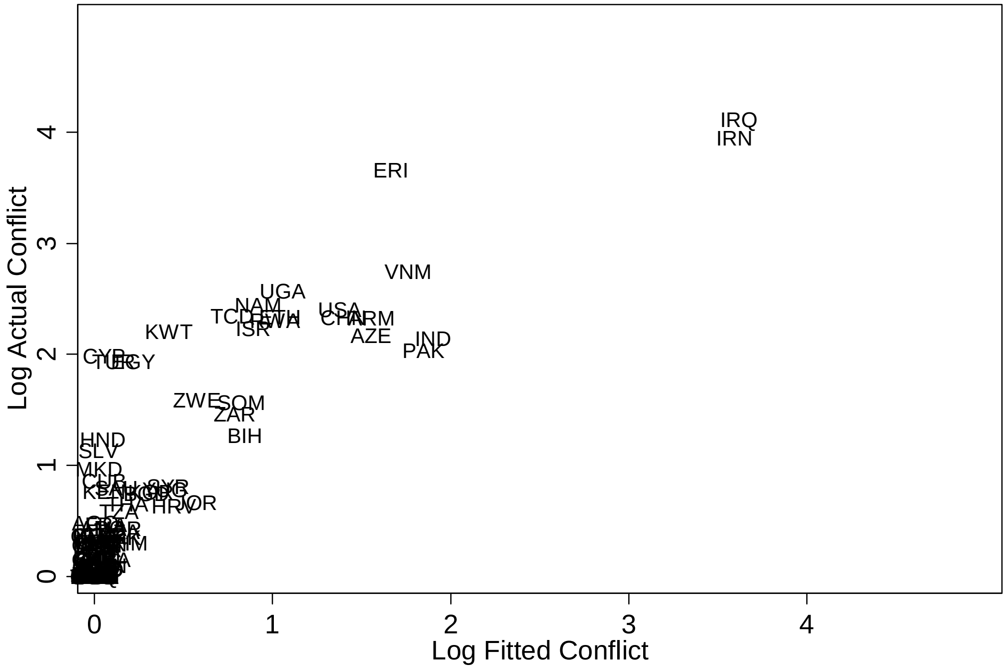


Table 1: Highest Hostility Level and Count of Bilateral Data

Highest Level of Hostility	Frequency	Percentage
Complete Sample		
No Militarized Action	454,467	99.558
Threat to Use Force	106	0.023
Display of Force	448	0.098
No Conflict Subtotal	*455,021*	*99.679*
Use of Force	1,296	0.284
War	167	0.037
Conflict Subtotal	*1,463*	*0.321*
Total	**456,484**	**100.00**
Preferred Sample		
No Militarized Action	365,069	99.474
Threat to Use Force	103	0.028
Display of Force	421	0.115
No Conflict Subtotal	*365,593*	*99.617*
Use of Force	1,241	0.338
War	165	0.045
Conflict Subtotal	*1,406*	*0.383*
Total	**366,999**	**100.00**

Table 2: Probit Results
(Dependent Variable: Actual forceful conflict)

	(1)	(2)	(3)
Intercept	-1.778***	-1.939***	-2.184***
	(0.166)	(0.175)	(0.194)
Years since last conflict	-0.005***	-0.004***	-0.004***
	(0.000)	(0.000)	(0.000)
Conflict in previous year	1.756***	1.747***	1.740***
	(0.041)	(0.041)	(0.044)
Log of distance	-0.262***	-0.256***	-0.259***
	(0.016)	(0.016)	(0.018)
Log sum of surface area	0.059***	0.063***	0.048***
	(0.005)	(0.005)	(0.007)
Common Border	0.283***	0.398***	0.432***
	(0.038)	(0.041)	(0.044)
Common language		0.143***	0.161***
		(0.032)	(0.035)
Common legal system		-0.193***	-0.141***
		(0.031)	(-0.033)
Common colonizer post-1945		0.077*	0.086*
		(0.047)	(0.053)
Colonial-Colonizer pair post-1945		0.465***	0.331***
		(0.081)	(0.085)
Same Country		0.010	0.126**
		(0.055)	(0.059)
Number of GATT/WTO members			-0.095***
			(0.022)
Lagged defense alliance			-0.172*
			(0.053)
Number of Communist states			-0.165***
			(0.034)
Sum of Polity			-0.005***
			(0.018)
Difference in Polity			0.013***
			(0.022)
Lagged log sum of military personnel			0.078***
			(0.007)
Lagged log difference of military personnel			-0.004
			(0.105)
Pseudo *R*-squared	0.498	0.502	0.514
No. of observations	456,484	456,484	366,999

Note: Year dummies are included but not reported. Robust standard errors clustered by bilateral pair are in parenthesis

Table 3: The Effect of Conflict on Economic Development
(dependent variable: Log of real GDP per capita in 2000)

Panel A: OLS and Second-Stage Results for 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Log (Conflict)	-0.045 (0.080)	-0.078 (0.108)	-0.089 (0.056)	-0.154** (0.069)	-0.061 (0.062)	-0.130* (0.069)
Log (Trade Intensity)	0.203** (0.094)	-0.381* (0.205)	0.198** (0.084)	-0.232 (0.180)	0.228*** (0.082)	-0.067 (0.199)
Rule of Law	0.812*** (0.054)	1.067*** (0.144)	0.755*** (0.053)	0.924*** (0.118)	0.764*** (0.054)	0.792*** (0.075)
Distance from equator	1.472*** (0.319)	0.940** (0.480)	0.657** (0.275)	0.480 (0.371)	1.163** (0.504)	1.025** (0.491)
Oil Producer			0.646*** (0.149)	0.781*** (0.172)	0.714*** (0.152)	0.794*** (0.164)
Africa			-0.733*** (0.131)	-0.617*** (0.151)	-0.516** (0.220)	-0.546** (0.224)
Latin America					0.463** (0.192)	0.346** (0.210)
East Asia					-0.033 (0.247)	-0.110 (0.243)
R-squared	0.70	0.59	0.78	0.73	0.79	0.77
Hansen *J* (p-value)		0.22		0.37		0.83
Cragg-Donald statistic		10.82		9.49		12.48
No. of observations	158	158	158	158	158	158

Panel B: First-Stage Statistics

	(4)			(6)		
	Fitted Conflict	Institutions	Trade Intensity	Fitted Conflict	Institutions	Trade Intensity
F Statistic	40.50	12.71	13.41	13.19	32.48	15.88
Shea partial *R*-squared	0.64	0.22	0.26	0.28	0.31	0.25

Note: We estimated each equation using 2SLS using the log of fitted conflict, English fractionalization, European fractionalization, and the log of fitted trade intensity as instruments in the first-stage. Robust standard errors are in parentheses where *, **, *** indicates significance at 10, 5, and 1% level, respectively.

Table 4: Robustness to Alternative Specifications
(dependent variable: Log of real GDP per capita in 2000)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log (Conflict)	-0.130*	-0.162**	-0.118**	-0.119*	-0.138**	-0.121*	-0.130**	-0.139**	-0.150*
	(0.069)	(0.085)	(0.063)	(0.064)	(0.068)	(0.069)	(0.069)	(0.064)	(0.082)
Log (Trade Intensity)	-0.067	-0.373	-0.059	-0.044	-0.035	-0.060	-0.055	-0.059	-0.268
	(0.199)	(0.440)	(0.180)	(0.190)	(0.191)	(0.200)	(0.203)	(0.198)	(0.281)
Rule of Law	0.792***	0.731***	0.709***	0.567***	0.727***	0.836***	0.754***	0.776***	1.001***
	(0.075)	(0.089)	(0.072)	(0.102)	(0.087)	(0.098)	(0.105)	(0.079)	(0.229)
Distance from equator	1.025**	1.246***	-0.271	-0.091	0.678	0.591	1.029**	0.874	1.301**
	(0.491)	(0.479)	(0.524)	(0.537)	(0.553)	(0.686)	(0.479)	(0.548)	(0.623)
Geographic Controls		[0.096]*							
Malaria			[0.000]***						
Life Expectancy				[0.000]***					
Fractionalization					[0.314]				
Legal Origin						[0.507]			
Democracy							[0.147]		
Civil Conflict								[0.381]	
R-squared	0.77	0.74	0.81	0.82	0.78	0.77	0.78	0.78	0.53
Hansen *J* (p-value)	0.83	0.68	0.65	0.25	0.43	0.63	0.30	0.69	0.45
Cragg-Donald statistic	12.48	2.98	12.88	9.85	14.11	11.15	8.82	12.53	2.73
No. of observations	158	158	154	152	152	158	158	158	127

Note: We estimated each equation using 2SLS using the log of fitted conflict, English fractionalization, European fractionalization, and the log of fitted trade intensity as instruments in the first-stage. Robust standard errors are in parentheses where *, **, *** indicates significance at 10, 5, and 1% level, respectively. Dummy variables for major oil producer, sub-Saharan Africa, Latin America and East Asia are included in the second stage of all specifications, but not reported.

Appendix A: Summary Statistics of Cross-Country Data

	Mean	Stan. Dev.	Minimum	Maximum
Real GDP pc in 2000	8,403	8,864	359	34,365
Conflict	2.1656	6.7563	7.3084	60.3596
Rule of Law	-0.0433	0.9962	-1.8845	2.2422
Trade Intensity	69.4029	40.3520	12.1950	319.5578
Distance from Equator	0.2938	0.1856	0.0000	0.6690
Predicted Conflict	0.7940	4.0919	0.0000	36.2239
English Fraction	0.0526	0.2020	0.0000	0.9740
European Fraction	0.1850	0.3509	0.0000	1.0040
Fitted Trade Intensity	19.8105	14.3266	2.3002	71.8083

*real GDP per capita in 2000 is from PWT 6.2, conflict is the sum of fatality-weighted actual bilateral conflicts for 1960-2000, rule of law is for 1995-2000, trade intensity is nominal exports plus imports divided by nominal GDP for 1960-2000, predicted conflict is the sum of fitted bilateral probabilities of conflict times casualities per day for 1960-2000, English fraction is the percentage of population speaking English language, European fraction is the percentage of population speaking one of the five major European languages, and fitted trade intensity is the sum of fitted nominal exports plus imports nominal GDP derived from a geographically-based gravity model.

Appendix B: Data

Country	Start Date	Actual Conflict	Real GDP per capita	Rule of Law	Nominal Openness	Distance from Equator
Austria	pre-1960	0	27,000	2.06	58.93	0.5359
Belgium	pre-1960	0	24,662	1.52	108.11	0.5649
Bolivia	pre-1960	0	2,929	-0.51	45.30	0.1688
Brazil	pre-1960	0	7,194	-0.17	12.19	0.2173
Bulgaria	pre-1960	0	7,258	-0.15	97.13	0.4675
Colombia	pre-1960	0	6,080	-0.59	28.28	0.0532
Denmark	pre-1960	0	27,827	1.99	66.41	0.6191
Dominican Republic	pre-1960	0	6,497	-0.26	76.76	0.2062
Finland	pre-1960	0	22,741	2.08	50.36	0.6690
Guatemala	pre-1960	0	3,859	-0.71	36.64	0.1625
Haiti	pre-1960	0	2,069	-1.24	52.84	0.2104
Hungary	pre-1960	0	11,383	0.76	97.24	0.5269
Ireland	pre-1960	0	24,948	1.81	93.60	0.6068
Italy	pre-1960	0	22,487	0.96	35.58	0.5046
Japan	pre-1960	0	23,971	1.71	21.11	0.3968
Mexico	pre-1960	0	8,082	-0.29	30.83	0.1862
Mongolia	pre-1960	0	1,501	0.24	89.13	0.5277
New Zealand	pre-1960	0	20,423	2.07	52.92	0.4099
Norway	pre-1960	0	33,092	2.10	73.64	0.6664
Poland	pre-1960	0	8,611	0.55	46.00	0.5583
Romania	pre-1960	0	5,211	-0.25	48.50	0.4947
Spain	pre-1960	0	19,536	1.31	28.37	0.4155
Sri Lanka	pre-1960	0	4,047	0.00	74.63	0.0763
Sweden	pre-1960	0	25,232	1.98	54.88	0.6586
Switzerland	pre-1960	0	28,831	2.24	61.64	0.5268
Taiwan, China	pre-1960	0	19,184	1.01	69.82	0.2589
Uruguay	pre-1960	0	10,740	0.57	29.63	0.3869
Benin	1960	0	1,251	-0.26	56.58	0.0707
Madagascar	1960	0	823	-0.84	42.76	0.2106
Sierra Leone	1961	0	684	-0.88	46.44	0.0967
Burundi	1962	0	699	-0.66	29.80	0.0374
Trinidad And Tobago	1962	0	14,770	0.40	110.65	0.1158
Malawi	1964	0	839	-0.39	57.10	0.1757
Gambia, The	1965	0	954	-0.13	89.91	0.1473
Singapore	1965	0	29,434	2.15	319.56	0.0151
Guyana	1966	0	3,733	-0.02	183.22	0.0640
Lesotho	1966	0	1,834	-0.16	109.54	0.3288
Equatorial Guinea	1968	0	6,495	-1.60	109.35	0.0258

Country	Start Date	Actual Conflict	Real GDP per capita	Rule of Law	Nominal Openness	Distance from Equator
Mauritius	1968	0	15,121	0.84	104.62	0.2248
Swaziland	1968	0	8,517	0.05	158.71	0.2949
Fiji	1970	0	4,572	-0.33	107.18	0.1981
Bhutan	1971	0	828	-0.55	69.80	0.3053
Comoros	1975	0	1,359	-1.08	57.03	0.1297
Papua New Guinea	1975	0	4,355	-0.36	89.17	0.0733
Djibouti	1977	0	4,376	-0.44	122.11	0.1278
Germany, Unified	1990	0	25,061	1.90	42.98	0.5351
Belarus	1992	0	10,005	-1.03	119.44	0.5889
Estonia	1992	0	11,081	0.53	134.28	0.6521
Georgia	1992	0	3,886	-0.72	65.22	0.4670
Kazakhstan	1992	0	6,520	-0.77	81.17	0.4923
Kyrgyz Republic	1992	0	3,389	-0.76	84.41	0.4556
Latvia	1992	0	8,998	0.17	99.99	0.6318
Lithuania	1992	0	9,161	0.10	114.80	0.6146
Moldova	1992	0	2,218	-0.29	116.26	0.5241
Slovenia	1992	0	18,206	0.77	120.52	0.5119
Turkmenistan	1992	0	7,624	-1.17	160.47	0.4444
Ukraine	1992	0	5,003	-0.72	88.80	0.5587
Czech Republic	1993	0	13,617	0.62	105.18	0.5494
Slovak Republic	1993	0	9,697	0.19	105.13	0.5378
Nepal	pre-1960	0.0007	1,421	-0.32	30.02	0.3079
Liberia	pre-1960	0.0011	472	-1.83	94.16	0.0709
Mozambique	1975	0.0026	1,093	-0.97	32.60	0.2055
Togo	1960	0.0037	823	-1.00	67.70	0.0688
Cote D'ivorie (Ivory Coast)	1960	0.0039	2,172	-0.59	70.75	0.0611
Malaysia	pre-1960	0.0045	11,406	0.73	113.18	0.0363
Chile	pre-1960	0.0088	11,430	1.28	40.40	0.3728
Guinea-Bissau	1974	0.0120	762	-1.37	44.85	0.1362
Greece	pre-1960	0.0128	13,982	0.73	35.92	0.4229
Zambia	1964	0.0133	866	-0.37	82.72	0.1438
Panama	pre-1960	0.0134	7,935	0.06	150.85	0.1023
Tajikistan	1992	0.0135	1,660	-1.37	160.72	0.4201
Mauritania	1960	0.0153	1,521	-0.54	103.57	0.1992
Russia	pre-1960	0.0230	9,263	-0.83	58.60	0.6186
Sudan	pre-1960	0.0230	1,048	-1.29	28.81	0.1560
Yemen, Republic Of	1990	0.0246	1,082	-0.87	59.63	0.1692
Canada	pre-1960	0.0302	26,821	1.95	49.37	0.4859
Jamaica	1962	0.0322	4,521	-0.20	85.68	0.2006
Lebanon	pre-1960	0.0507	6,175	-0.07	103.69	0.3790

Country	Start Date	Actual Conflict	Real GDP per capita	Rule of Law	Nominal Openness	Distance from Equator
Indonesia	pre-1960	0.0586	3,772	-0.75	43.27	0.0729
Costa Rica	pre-1960	0.0602	8,341	0.77	60.08	0.1105
United Arab Emirates*	1971	0.0604	32,182	1.15	115.14	0.2599
Laos	pre-1960	0.0641	1,257	-1.14	34.81	0.1839
Mali	1960	0.0707	1,047	-0.68	48.41	0.1390
Afghanistan	pre-1960	0.0729	478	-1.55	53.41	0.3841
Netherlands	pre-1960	0.0742	26,293	1.97	100.96	0.5764
Burkina Faso	1960	0.0763	933	-0.57	41.38	0.1339
Uzbekistan	1992	0.0903	3,543	-1.00	63.47	0.4586
Gabon	1960	0.0943	10,439	-0.45	94.92	0.0041
Tunisia	pre-1960	0.0994	6,993	0.32	68.23	0.4091
Australia	pre-1960	0.1453	25,835	1.95	31.64	0.3580
Central African Republic	1960	0.1531	945	-0.57	50.69	0.0481
Ghana	pre-1960	0.1620	1,392	-0.09	44.74	0.0744
Botswana	1966	0.1630	7,256	0.71	105.30	0.2393
Guinea	pre-1960	0.1634	2,546	-0.98	54.61	0.1297
South Africa	pre-1960	0.1653	8,226	0.28	50.61	0.3237
Bahrain*	1971	0.1781	18,652	0.85	178.47	0.2892
Qatar*	1971	0.1815	32,261	1.15	87.10	0.2812
Philippines	pre-1960	0.2046	3,826	-0.22	48.32	0.1547
Paraguay	pre-1960	0.3171	4,965	-0.69	43.42	0.2843
Venezuela*	pre-1960	0.3171	7,323	-0.71	46.62	0.1094
Albania	pre-1960	0.3207	3,797	-0.67	53.69	0.4590
Cameroon	1960	0.3226	2,472	-1.05	49.43	0.1192
Portugal	pre-1960	0.3255	17,323	1.27	52.75	0.4313
Senegal	1960	0.3388	1,571	-0.26	58.93	0.1641
Niger	1960	0.3421	807	-0.93	42.24	0.1542
Cambodia	pre-1960	0.3500	514	-0.80	27.84	0.1336
Ecuador	pre-1960	0.3646	4,314	-0.58	45.18	0.0229
Peru	pre-1960	0.3646	4,205	-0.46	43.19	0.1310
Libya	pre-1960	0.3724	10,335	-1.01	51.03	0.3623
Nicaragua	pre-1960	0.3747	3,438	-0.80	55.41	0.1357
Korea, North	pre-1960	0.4195	1,379	-1.11	15.81	0.4392
Oman*	1971	0.4333	16,193	1.21	90.37	0.2272
Nigeria*	1960	0.4446	1,074	-1.17	40.93	0.0727
Congo, Republic	1960	0.4457	1,286	-1.23	117.58	0.0409
United Kingdom	pre-1960	0.4526	24,666	1.97	48.33	0.5723
Algeria*	1962	0.4997	5,753	-0.74	54.85	0.4080
Morocco	pre-1960	0.5391	3,720	0.34	47.42	0.3733
Argentina	pre-1960	0.5477	11,332	0.21	12.59	0.4075

Country	Start Date	Actual Conflict	Real GDP per capita	Rule of Law	Nominal Openness	Distance from Equator
France	pre-1960	0.5887	25,045	1.52	36.38	0.5429
Angola	1975	0.6179	1,975	-1.45	67.66	0.0983
Tanzania	1961	0.7956	817	-0.42	47.28	0.0239
Croatia	1992	0.8781	8,980	-0.14	107.53	0.5011
Thailand	1887	0.9103	6,474	0.43	51.79	0.1530
Jordan	pre-1960	0.9434	3,902	0.45	94.02	0.3511
Bangladesh	1971	1.1154	1,851	-0.69	21.60	0.2653
Korea, South	pre-1960	1.1409	15,702	0.76	47.30	0.4173
Kenya	1963	1.1511	1,268	-0.91	61.49	0.0057
Serbia & Montenegro	1992	1.1904	2,095	-1.03	31.35	0.4889
Saudi Arabia*	pre-1960	1.2107	15,827	0.75	86.31	0.2563
Syrian Arab Republic	pre-1960	1.2517	2,001	-0.37	49.52	0.3718
Cuba	pre-1960	1.3555	5,699	-0.62	51.68	0.2565
Macedonia, FYR	1993	1.6250	5,271	-0.40	83.41	0.4611
El Salvador	pre-1960	2.1009	4,732	-0.36	54.12	0.1531
Honduras	pre-1960	2.4198	2,240	-0.77	65.09	0.1577
Bosnia-Herzegovina	1992	2.5362	3,037	-0.69	96.54	0.4889
Congo, Dem. Rep.	1960	3.3209	359	-1.88	75.92	0.0000
Somalia	1960	3.7787	682	-1.74	19.76	0.1181
Zimbabwe	1965	3.8907	3,256	-0.32	65.43	0.1986
Turkey	pre-1960	5.8992	5,715	0.09	20.98	0.4578
Egypt	pre-1960	5.9229	4,536	0.21	45.95	0.3333
Cyprus	1960	6.2916	20,457	0.83	104.01	0.3898
Pakistan	pre-1960	6.6374	2,477	-0.59	21.64	0.3464
India	pre-1960	7.5223	2,644	0.14	13.36	0.2808
Azerbaijan	1992	7.7666	3,591	-0.89	88.57	0.4484
Kuwait*	1961	8.0693	25,135	0.99	103.10	0.3258
Israel	pre-1960	8.3398	22,237	1.11	54.06	0.3565
Rwanda	1962	8.9674	1,018	-0.74	24.06	0.0226
Armenia	1992	9.2110	3,471	-0.44	78.45	0.4473
China	pre-1960	9.2740	4,002	-0.34	17.06	0.3285
Ethiopia	pre-1960	9.3097	725	-0.30	25.35	0.1001
Chad	1960	9.4100	830	-0.66	53.78	0.1153
United States	pre-1960	10.0904	34,365	1.82	15.21	0.3818
Namibia	1990	10.5118	5,269	0.85	129.40	0.1998
Uganda	1962	12.1114	1,058	-0.52	48.83	0.0025
Viet Nam, Unified	1976	14.5641	2,189	-0.68	83.09	0.1200
Eritrea	1993	37.8788	555	-0.10	98.87	0.1701
Iran, Islamic Republic of*	pre-1960	50.6817	6,046	-0.57	46.19	0.3931
Iraq*	pre-1960	60.3596	2,445	-1.58	50.46	0.3702

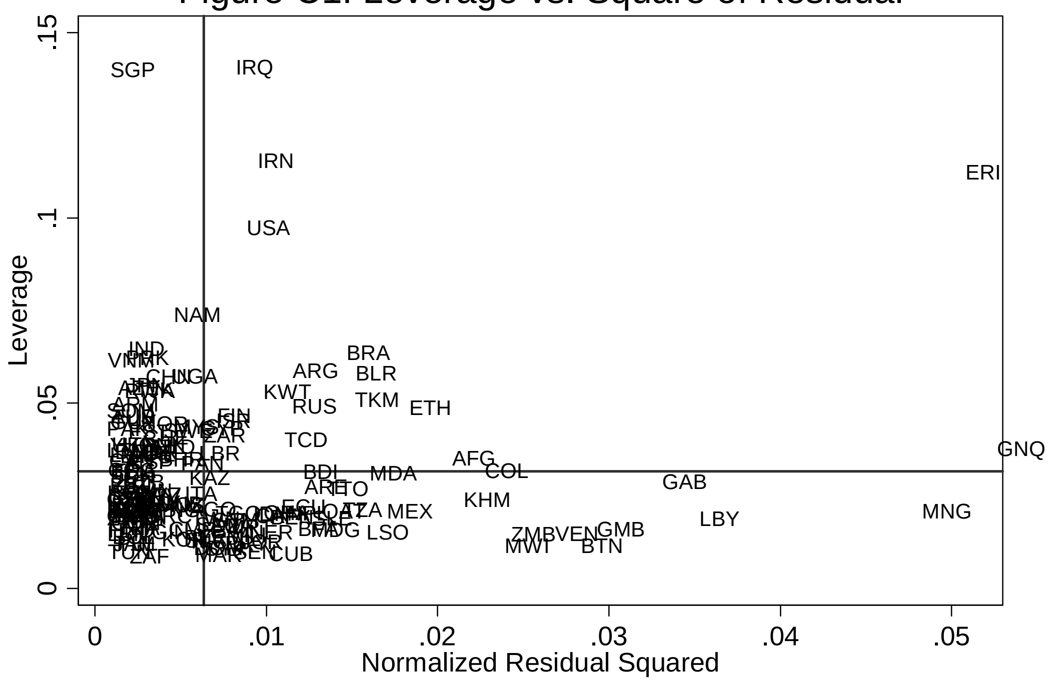
* indicates a major oil producer.

Appendix C: Outlier Analysis

In order to consider the possibility that Iran, Iraq, and Eritrea are outliers, we consider three issues. First, an observation may not be well-explained by the model and thus have a *large residual*. As a result, the observation is not well-explained by the data. Second, an observation may be far from the mean of the distribution and thus have *leverage*. A least squares regression fit will attempt to prevent such a point from having a sizable residual. Third, an observation may have an impact on the point estimate and thus have *influence*. Influence is a combination of *large residual* and *leverage*. Figure C1 provides visual evidence of possible outliers.

Figure C1 plots leverage vs. normalized residual squared for the 158 observations.

Figure C1: Leverage vs. Square of Residual



Based on figure C1, Iraq (IRQ) and Iran (IRN) exhibit extremely high leverage, while Eritrea (ERI) has a large amount of leverage and residual. As a result, it appears accounting for these observations is necessary.