Economic Shocks and Civil Conflict:
Evidence from the Constraints of the Open-Economy Trilemma

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Abstract

We exploit the open-economy trilemma to identify externally-driven components of short-run income shocks over 105 countries from 1973-2004 and explore the statistical nature of the income-civil conflict nexus suggested by past empirical and theoretical work. Our results show that movement in foreign interest rates has important effects on civil conflict risk by affecting domestic economies. More importantly, the income-conflict relationship is found to be nonlinear – countries with more ethno-linguistically fragmented populations tend to fall much more easily into civil conflict during economic downturns. These results suggest an important mechanism by which short-term economic shocks affect the trajectory of the political and economic performance of ethnically divided states.

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I. Introduction

The degree to which internal conflict has burdened economic development in recent history is difficult to overstate. Since 1960, one third of all countries have experienced at least one year of intranational war, defined as conflict that claims over 1,000 lives within its borders. Moreover, twenty percent of the world has seen at least ten years of civil war in this period (Blattman and Miguel, 2010). Internal war has steadily surpassed the destructive legacy of international war, claiming at least 16.2 million casualties since 1945 – five times as many as the number of lives lost in comparable conflict between states (Fearon and Laitin, 2003). Ultimately, these data may greatly understate the human cost – off the battlefield, the long-term costs of disease, disability, and social fragmentation that result indirectly from civil war extend well past an arrival at peace (Ghobarah, Huth, and Russett, 2003).

Beyond the social burden, the economic costs of civil conflict are well-established. As Hirshleifer (1995) and others note, fighting is almost always Pareto-inefficient.[1] The immediate consequences of civil conflict – the demolition or weakening of infrastructure, loss of technology, reduction of physical and human capital, and the diversion and destruction of the productive labor force – can slow or even reverse the process of economic development (Collier, 2007; Sandler, 2000; Dupas and Robinson, 2009). Moreover, civil war can weaken political institutions and increase the risk of expropriation (Collier, 2007; Blattman and Miguel, 2010),

[1] The most common theoretical explanations for civil war focus on the informational asymmetries, commitment problems, and the presence of inherently non-contractible issues that prevent the attainment of otherwise socially optimal agreements. See Blattman and Miguel (2009) for an extensive overview of this literature.

two trends found to significantly hamper economic growth (Keefer and Knack, 1997; Rodrik, 1999; Acemoglu, Johnson, and Robinson, 2001a, 2002, 2005).[2]

Although the economic and social consequences of civil war are widely-recognized, its underlying causes remain elusive. As war has been shown to affect the economy, it is reasonable to ask whether, in turn, the economic environment of a given country affects its propensity to fall to war. In theory, economic conditions may affect the likelihood of civil war through changes to the potential warrior's opportunity costs of fighting (which includes foregone non-conflict income) as shown by Grossman (1991), Dal Bó and Dal Bó (2004), Garfinkel and Skaperdas (2007), and Chassang and Padro-i-Miquel (2009).[3] The implication of this theory is particularly important – if the state of the economy affects the likelihood of sudden civil war by altering the incentives of potential combatants, then countries may be more likely to become mired within a "conflict trap." That is, if negative economic shocks encourage civil war which, in turn, worsens economic conditions through typical neoclassical and institutional channels, countries may find themselves likely to be trapped in the kind of vicious cycles of persistent, economically-destructive wars characterized by Collier (2007).

Although the theoretical models linking poor economic conditions to an increased probability of civil violence are well-developed, the body of empirical literature supporting such

[2] As a quantitative measure, Hess (2003) estimates that for countries that have experienced war since 1960, the economic benefits of peace are comparable to up to 8 percent of current consumption. Cerra and Saxena (2008) similarly show that the immediate economic effect of civil war is output loss of 6 percent, and, more importantly, that these economic effects can persist for up to a decade.

[3] There may be multiple channels for the link between participation in violence and economic conditions. For example, Hovland and Sears (1940), and later Hepworth and West (1988) largely attribute the correlations between economic depression and the incidence of lynchings in the American south as a form of irrational scapegoating. Oster (2004) similarly finds that the waves of mass scapegoating violence in witchcraft trials in 16th and 17th century Europe were linked to macroeconomic downturns associated with exogenous climate change, and Miguel (2005) demonstrates such trends continue today in rural Tanzania. For the purposes of this paper, we do not attempt to distinguish the nature of this causal relationship but rather seek to simply establish its existence.

predictions with credible and robust evidence is still young. Earlier cross-country works of Collier and Hoeffler (2004) and Fearon and Laitin (2003), although finding strong negative correlations between economic conditions and the incidence of civil conflict, are likely to suffer from omitted variable and endogeneity biases as suggested by Djankov and Reynal-Querol (2008) and Blattman and Miguel (2010).

More recent empirical works make use of plausibly exogenous shocks to economic growth in an attempt to find unbiased estimates of the income-conflict relationship through an instrumental variable framework.[4] Miguel, Satyanath, and Sergenti (2004) use variations in weather patterns to identify and relate exogenous components of annual economic growth to the incidence of war in agrarian Sub-Saharan African economies. The authors find that local rainfall rates act as a strong predictor of economic performance in these countries and that second-stage instrumented aggregated economic performance has a large negative effect on the probability of conflict within this limited sample. Besley and Persson (2009) and Brücker and Ciccone (2010) propose an alternative identification strategy in relating terms of trade shocks to domestic income. They similarly find that exogenous real economic shocks affect the probability of war.

However, there are several reasons why both weather and terms of trade may be invalid instruments for economic conditions in the face of war. Psychological and socio-political observation notes the tendency for participation in violent activities to increase in locations and during periods of higher temperatures and decline with inclement weather (Anderson, 2001),

[4] Recent empirical work also includes studies that utilize rare disaggregated microeconomic data. Benmelech, Berrebi, and Klor (2010) make use of detailed data on Palestinian suicide bombers collected by the Israeli Security Agency to examine whether economic conditions affects the quality or productivity of suicide bombers. Krueger and Malečková (2003) study a connection between poverty and participation in terrorism (one form of civil conflict). Beber and Blattman (2010) use a hand-collected dataset on the characteristics of young soldiers in Uganda to measure the role of coercion and economic reward in military recruitment. Such data, however, are virtually nonexistent for terrorists or other civil warriors in any other country in the world.

suggesting that weather patterns may affect civil war risk both through its effects on the economic environment and by the psychological state of individuals.[5] Moreover, a large body of political science literature finds that water scarcity, which is directly affected by the annual supply of rainfall (especially in the Middle East and Africa), triggers internal conflicts due to ill-defined property right or dislocation (Homer-Dixon, 1990; Gleick, 1993; Benjaminsen, 2008). Given the possibility that weather may affect the risk of civil conflict through multiple channels, Miguel, Satyanath, and Sergenti's chosen identification strategy might not satisfy the exclusion restriction assumption in the second-stage regression. As for the terms of trade instrumentation, political instability arising from civil conflict in even moderately influential commodity-producing countries may drastically change the global price of those commodities, creating endogeneity between the outcome variable and the instrument (Blattman and Miguel 2010).[6]

Beyond the econometric issue of how best to identify domestic economic shocks, the literature on civil conflict lacks agreement on the extent to which internal institutional quality and socio-political factors affect the conflict-income relationship. On one hand, Besley and Persson (2009) show that institutional quality matters: economic shocks inspired by movement in the terms of trade shift the probability of civil war only in countries with weak socio-political

[5] Crime economists have exploited the psychological effects of weather conditions to examine whether individuals first motivated towards violent crime for reasons unrelated to socio-economic conditions exhibit persistent violent behavior over their subsequent lifetime (e.g., Jacob, Lefgren, and Moretti, 2007). Developmental economists and policy leaders also find that political instability, the incidence of riots, and the probability of regime change are more likely in years with lower rainfall and higher temperatures (Dell, Jones, and Olken, 2009; United States Riot Commission, 1968).

[6] Bazzi and Blattman (2008) reexamine Besley and Persson's findings of the effects of trade shocks on civil war using an expanded and more disaggregated database of international commodity prices and country trade shares. The authors show that the relationship between trade shocks and political instability are highly sensitive to the estimation specification and are not sufficiently robust. Note that if civil war affects commodity prices positively by shifting the aggregate supply schedule to the left, then the resulting bias may go *against* finding a significant relationship. The fragile relationship between civil war and terms of trade might thus be due to the general inability to separate foreign shocks that exogenously affect conflict risk through its effects on domestic income from domestic political shocks that shift the probability of civil war and terms of trade simultaneously.

institutions. Miguel, Satyanath, and Sergenti (2004), however, find that interactions of institutional quality or ethnic fragmentation with instrumented economic shocks are uncorrelated with the probability of conflict. In other words, better political institutions do not seem to mitigate the conflict risk effect of a sudden recession, and ethnically fragmented countries do not seem to be at a higher risk of civil war during an economic downturn. These results notwithstanding, it is important to remember that Miguel, Satyanath, and Sergenti (2004) restrict their focus to Sub-Saharan African countries where there may not be enough cross-country variation in institutional quality or the degree of ethnic fragmentation necessary for sufficient statistical leverage to reveal nonlinearities in the main income-conflict relationship. An important contribution to the empirical literature, therefore, may come in the form of a valid instrumentation strategy that can be applied to a wide sample of countries with large variation in political and social institutions.

This paper offers one such possible strategy by developing a new identification strategy that is plausibly cleaner and applicable to a larger set of countries than those used in Miguel, Satyanath, and Sergenti (2004), Besley and Persson (2009) and Brücker and Ciccone (2010). Specifically, we incorporate the famous macroeconomic trilemma (impossible trinity), originally derived from the seminal Mundell-Flemming model (Mundell, 1962; Flemming, 1962), to construct an identification method to tease out exogenous shocks in annual real domestic output growth over a global sample of countries from 1973-2004 and relate these shocks to the probability of conflict in a framework parallel to that of Miguel, Satyanath, and Sergenti (2004). The trilemma predicts that a country with a fixed exchange rate regime and an unrestricted flow of capital must align its monetary policy with that of the country to which the domestic currency is anchored and thereby forego the ability to set domestic interest rates to stabilize its own economy. In such a situation, fluctuations in foreign interest rates should have heterogeneous effects on the short-run economic performance of a given country depending on the prevailing exchange rate regime and degree of financial openness. The interaction of variables that measure a given country's trilemma configuration with base country interest rates may thus constitute a valid instrument, provided that these variables and interest rates in base countries (that is, countries to which currencies are typically pegged, such as the United States and Germany) are determined exogenously to the domestic economic and political conditions of small developing countries at risk of conflict.

This paper's instrumentation approach is similar in spirit to the terms of trade instrumentation of Besley and Persson (2009) and Brücker and Ciccone (2010) in that we exploit the fact that some statistical variation in economic conditions in the developing world is externally driven by shocks originating in foreign markets. Nonetheless, unlike terms of trade that can shift due to domestic political shocks, financial flows between any given developing country and its base make up only a small fraction of the global financial market (Lucas, 1990), and thus domestic political shocks are expected to have only a negligibly small, if any, effect on base country interest rates. Moreover, as di Giovanni and Shambaugh (2008) suggest, it is unlikely that conditions in a given small open economy directly and systematically affect the monetary policy decisions of base country central banks.

The use of the trilemma for instrumentation additionally improves on the external validity of past studies. While weather and term of trade shocks are powerful instruments for income growth in Sub-Saharan African countries whose economies are agrarian, do not have adequate irrigation systems, and are highly reliant on export revenue derived from certain primary commodities, these shocks are less relevant outside of this particular region where economic structures may be more broadly based and sophisticated. As over 64% of countries experiencing civil conflict since 1960 lie outside of the Sub-Saharan region (panel c, figure 1), the ability to globally test the causality of the income-conflict hypothesis is particularly relevant.[7] In contrast to these earlier studies, the instrumental variables of this paper can apply to countries around the world as the theory of the trilemma is not constrained geographically.[8] Within the context of this paper, over 71% of sampled country-year observations with a fixed exchange rate lie outside of Sub-Saharan African and over 79% of observations with capital account openness more than one standard deviation above the mean are non-African.[9] In addition to delivering better external validity, expanding the sample of countries outside of Africa also yields an empirical advantage as it provides more cross-country variation in institutional quality and ethnolinguistic diversity that may be crucial for statistically capturing nonlinear effects of economic shocks on civil war. This may be particularly important with regards to ethnic diversity. Easterly and Levine (1997) show that African countries tend to have uniformly high levels of cultural fractionalization when compared to other developing countries.

[7] Few familiar with the constant destruction of the National Liberation Army (ELN) in Colombia, the history of violent coups and subsequent anti-communist purging of Indonesia, the rise of the Shining Path in Peru, or the bloody backlash against the economic policy of the Burmese government in 1988 may easily suggest that civil conflict is primarily the concern of Sub-Saharan Africa.

[8] The predictions of the trilemma are well-supported by a large body of empirical works that use wide range of data from both developed and developing economies in both contemporary and historical times. See Borensztein, Zettelmeyer, and Philippon (2001), Frankel, Schmukler, and Servén (2004), Aizenman, Chinn, and Ito (2008), Berg, Borensztein, and Mauro (2004), di Giovanni and Shambaugh (2008), di Giovanni, McCrary, and von Wachter (2009), and Obstfeld, Shambaugh, and Taylor (2005).

[9] See panel A and B of figures 1 for graphical representations of these distributions for a particular year

We find three notable results from this new empirical strategy. First, we reproduce first-stage estimations of the relationship between base country interest rates and real domestic output growth that characterizes the literature of the trilemma; i.e., a large part of economic fluctuations in small open economies are found to be explained by base country interest rates and the interaction of base rates with exchange rate regime and capital account openness. Second, instrumental variable regressions show that the estimated effect of domestic income shocks on the probability of civil violence is statistically significant and of a substantial magnitude. Across different specifications of the model, instrumentation strategy, and sample, we find that a negative shock in GDP growth by four percentage points in a given year increases the probability of civil conflict by approximately six percentage points.[10] As the sample statistics suggest that in each year a country has on average a 17.9% chance of experiencing civil conflict (table 1), this represents an increase in conflict risk by over a third. While this estimate is smaller in magnitude than that estimated over a sample of Sub-Saharan African countries, these results support the qualitative conclusions of Miguel, Satyanath, and Sergenti (2004) and Brückner and Ciccone (2010) by finding them robust to the choice of instrument. More importantly, the findings of this paper suggest that the income-conflict relationship may be found globally, not only locally in Sub-Saharan Africa.

Third, we find one result that differs from previous instrumental variable analyses. Miguel, Satyanath, and Sergenti (2004) do not find that political, social, or geographical conditions affect the propensity of a country to fall into civil war in response to negative economic shocks. Although we find no statistically significant evidence supporting the relevancy

[10] The economic shock of this magnitude is typical in the developing world; a within-country annual variation in GDP growth rates is found to be 4.02 percentage points (see table 1).

of religious diversity, reliance on oil exports, political institutional quality, or country terrain, we find evidence that is consistent with the view that higher levels of ethnolinguistic diversity for a given country makes a country more conflict-prone when its economy suffers from a recession. For a country at the 25th percentile of global ethnolinguistic fragmentation (such as Chile or Kuwait), a sudden decrease in GDP growth by one percentage point increases the probability of civil conflict by only half a percentage point, while for a country at the 75th percentile (such as Pakistan or Ethiopia) the same economic shock increases the probability of internal violence by 3.39 percentage points (18.9% from the typical annual likelihood of conflict). Although the instrumental variable performance of these interaction effects is weaker than that of the main results and should thus be interpreted with care, these results support the claim that economic development may widen pre-existing ethnic rifts in countries with particularly fractionalized social institutions (Fearon, 2007).

In addition to shedding new light on the income-conflict nexus, our findings are relevant to the literature that explores the deep determinants of long-term economic performance. A large body of research shows that ethnolinguistic fragmentation affects economic performance via its relevance to the quality of political institutions and economic policies (e.g., Easterly and Levine, 1997; Mauro, 1995). In particular, this literature attributes Africa's long-standing underdevelopment and so-called growth tragedy to high ethnic diversity. This paper's results suggest an additional and perhaps complementary mechanism by which ethnolinguistic diversity might matter in the particular context of African development: temporary economic shocks are more likely to cause civil conflicts in ethnically fragmented countries, and these conflicts may translate into deeper, medium- to long-term economic malaise or even a "growth collapse" (Rodrik, 1999).

The remainder of this paper is organized as follows. Section II discusses and summarizes the data used in this paper, and section III describes in detail the empirical strategy. Section IV provides a discussion of the main results and summarizes the findings from extensions that consider various socio-political and geographic factors. Section V describes in detail various robustness checks and their implications for both the main results and their extensions. Section VI concludes.

II. Data

An annual panel dataset consisting of 105 countries from 1973 to 2004 is constructed from a variety of sources. Variables of primary interest include the presence of internal violence, annual real output growth rates, measures of exchange rate regime and capital account openness, and base country interest rates. In addition, we consider various macroeconomic, social, and political variables suggested by Fearon and Laitin (2003). In selecting all of these variables, we follow closely the work of di Giovanni and Shambaugh (2008), Miguel, Satyanath, and Sergenti (2004), and others who provide a detailed description of the various benefits and shortcomings of such alternatives.

For the incidence of intranational violence, we follow Miguel, Satyanath, and Sergenti (2004) and Brückner and Ciccone (2010) for comparability and use the Armed Conflict Database of the International Peace Research Institute of Oslo, Norway and the University of Uppsala, Sweden (PRIO/Uppsala), which defines civil conflict as "an [internal] contested incompatibility which concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths." The civil conflict indicator variable for country *i* in year *t* is denoted C*onflict*$_{it}$. This variable is coded as one if a civil conflict against the state with at least 25 battle deaths per year is ongoing in a given country-year observation, zero if PRIO/Uppsala does not identify a civil conflict but the country-year pairing is under their observation, and missing otherwise. By this classification, 17.9% of country-year observations saw civil conflict from 1961 to 2004. This proportion is slightly smaller than that found in previous research that uses the same PRIO/Uppsala database. This discrepancy may be explained both through the inclusion in this paper of countries where conflict is more rare (such as relatively more developed countries and countries outside of Sub-Saharan Africa) and by the consideration of years where conflict was less common (prior to 1980).[11]

For exchange rate regime, we closely follow the methodology of Shambaugh (2004) and di Giovanni and Shambaugh (2008) in using a *de facto* classification of exchange rate regime type, as the official *de jure* exchange rate policy can be quite misleading (some countries do not declare a peg despite maintaining one while others fail to maintain a declared peg[12]) and capturing the underlying true exchange rate regime type is essential for strong identification. Following common definitions in the literature (e.g. Obstfeld and Rogoff, 1995), a country is classified as pegged if its official nominal exchange rate remains within ±2% bands over a given

[11] Econometrically, this paper's identification strategy, dependent on large variation in base interest rate movements, necessitates the inclusion of the generally low interest rate period preceding the higher-rate period of the 1980s.

[12] See Quinn and Toyoda (2008) and of Shambaugh (2004) for an in-depth discussion of the limitations of the cross-county data on de jure exchange rate policy.

year against its base country. [13] Base countries are identified and matched to the domestic economy through an examination of the officially declared base (if available), the history of a country's exchange rate, a comparison of exchange rate movement across all major currencies, and consideration of dominant regional currencies (di Giovanni and Shambaugh, 2008).[14] The resulting measure, Peg_{it}, which is summarized in table 1, is a binary variable set equal to one if country i in time t is characterized by Shambaugh (2004) as pursuing a *de facto* fixed exchange rate and zero otherwise. Approximately 42% of country-time observations in this paper's global sample follow a pegged exchange rate.[15]

In measuring the degree of financial liberalization for each country in the sample, we use the $KAOPEN_{it}$ variable constructed by Chinn and Ito (2008). Using standard principle component analysis of reversed values of the four IMF AEREAR dummy variables, Chinn and Ito construct a continuous index that allows for a more detailed measure of both the extensity and intensity of capital account liberalization (that is, the lack of various types of capital controls), which again is important because the reduction in variation and misclassification of countries can weaken the instrument and thus the power of instrumental variable inference.[16] Summary statistics for $KAOPEN_{it}$ across all countries and time are given in table 1.

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[13] Single year "pegs" identified in this manner are dropped as they likely represent a random lack of variation in the exchange rate rather than a temporary change in policy.

[14] See table C1 (Appendix C) for a full list of included countries and a classification of their base countries.

[15] See figure 1 for a graphical summary of the geographical distribution of pegged exchange rate regimes in 1980.

[16] The literature on financial liberalization emphasizes the difficulty of consistently identifying and quantifying a given level of capital account openness (Edison et al., 2002; Eichengreen, 2002). Default measures of capital liberalization rely once again on the IMF's AREAER in the form of various binary variables indicating the presence of multiple exchange rates, restrictions on the current account, capital account transactions, and the enforced surrender of export proceeds. As suggested by Chinn and Ito (2006), such variables have at least two shortcomings. First, as dichotomous variables they do not capture variability in the intensity of capital controls and thus may not accurately describe a given country's nuanced capital account policy. Second, as mentioned in the discussion of exchange rate regimes, the restriction of study to de jure classifications of policy choices rather than observed de

Annual interest rate data are collected from the International Monetary Fund's International Financial Statistics database (IFS). In order to maximize coverage, we follow the empirical strategy of di Giovanni and Shambaugh (2008) in selecting short-term rates of the money market and treasury bills for each country in each year under observation. Which short-term rate is ultimately used in constructing the relevant variable, R_{it}, depends on the availability of the data – if both money market and short-term t-bill rates are available, the money market rates are given preference.[17] With the base country identified for each country-year observation, we construct the variable R^{b}_{it} which takes on the value of the interest rate of the country to which the domestic country's currency is pegged if the country is identified as pursuing a fixed exchange rate, or the value of the interest rate of the country determined by Shambaugh (2004) as the best base country otherwise. Summary statistics for both variables are reported in table 1. On average, interest rates from the base country pool are lower (6.7%) than those drawn from the entire global sample (10.1%) and display a much smaller range and variance, which is to be expected given typical considerations of what makes a desirable base country. Panels A and B of figure 2 plots the movement of a selection of the various base countries over time, and summarizes the distribution of countries over each base. These graphs suggest a wide variation in (potentially-transmitted) monetary policies both between and within base countries during this paper's sample period.

facto reality may be misleading (Quinn and Toyoda, 2008). As Chinn and Ito report, their measure highly correlates with the large set of alternative capital liberalization indices such as those derived by Quinn (1997, 2003), Miniane (2004), and Lane and Milesi-Ferretti (2003) but provides for a much higher coverage across countries and time, increasing the available sample size for this study (Chinn and Ito, 2008).

[17] As a practical matter the two types of interest rates tend to be highly correlated such that little precision is lost with this kind of substitution between them (di Giovanni and Shambaugh, 2008).

Annual growth rates of GDP at market prices deflated by a constant local currency (y_{it}) are given by the World Bank's World Development Indicators (WDI) database. For robustness checks, we also gather data from the WDI to construct a variable ($inflation_{it}$) that captures annual domestic inflation rates. Summary statistics for these two macroeconomic variables are given in table 1. Data for ethnolinguistic and religious fractionalization, democracy, reliance on oil exports, and roughness of terrain come from and are discussed in detail in Fearon and Laitin (2003) and Miguel, Satyanath, and Sergenti (2004). We take an ethnolinguistic fractionalization ($ethfrac_{it}$) variable from the Soviet ethnographic index *Atlas Marodov Mira* that measures the probability that two randomly selected individuals in a country will belong to different ethnolinguistic groups, [18] a measure of religious fractionalization ($relfrac_{it}$) from the *CIA Factbook*, a variable capturing the presence of democratic institutions ($democracy_{it}$) from the standard Polity IV data set, the logged proportion of a country categorized as mountainous by geographer A.J. Gerard ($lmtest_{it}$, taken from Fearon and Laitin, 2003), and a binary variable (oil_{it}) set equal to one if the World Bank WDI database reports that oil constitutes more than one-third of export revenues for country *i* in year *t*.

Summary statistics for these variables are reported in table 1. Consistent with the observations of Easterly and Levine (1997), the probability of two representative individuals from a given country belonging to different ethnic groups is lower (0.399) in the global sample than in Miguel, Satayanath, and Sergenti (2004) (who find the average of this variable as 0.65). Additionally, when compared to the limited African sample, this paper's global sample exhibits a

[18] Our main results are based on the *Atlas Marodov Mira* to be comparable with Miguel, Satayanath, and Sergenti (2004). For an additional robustness check, we also use the more recent measure developed by Alesina et al. (2003). The results are qualitatively similar and not reported to conserve space.

higher heterogeneity in terms of ethnolinguistic and religious fractionalization, democracy, terrain, and oil exporting. The standard deviation of ethnolinguistic fractionalization increases from 0.24 to 0.29 in the global sample, while that of religious fractionalization increases from 0.19 to 0.23 and the Polity IV score for democracy grows in variance from 31.3 to an impressive 56.7. As noted previously, higher variation in measures of political and social institutional quality may be crucial for improving the statistical leverage necessary to fairly test its relevancy in the conflict-income nexus.

After collecting the data, the sample is cleaned in various ways corresponding with the methodology given by di Giovanni and Shambaugh (2008) that is meant to minimize the effects of outliers, misspecification, and measurement error. First, we drop countries from the sample that either always peg or always float their currency for all of their included years. This is intended to limit attention only to countries that are likely to experience both floating and fixed exchange rate regimes to avoid the possibility that we include pegged countries that are inherently more dependent on base country interest rates for reasons independent of their exchange rate regime choice (di Giovanni and Shambaugh, 2008). Next, we eliminate periods of hyperinflation, defined as an inflation rate of 50% or higher in a given year, as they are generally viewed by the literature as outliers for domestic interest rate movement. In a similar fashion, we eliminate observations where the real output growth rate is reported as either above 20% or below -20%, seeing these cases either as coding errors or outliers. Dropping these country-year observations also addresses a possible source of endogeneity since a country that is experiencing large-scale economic collapse and hyperinflation due to internal violence might have stronger incentives to give up a pegged currency or tighten the control of financial outflows. Finally, countries with a population less than 250,000 are viewed as too small to be representative of the global sample and are dropped. The full list of the 105 included countries, the years for which conflict data are available, the number of years conflict occurred, and the corresponding base country, are given in the Appendix (table A1).[19]

III. Empirical Strategy

This paper's empirical strategy draws from precedence set by two distinct bodies of economic literature. First, following di Giovanni and Shambaugh (2008) and others, we use the trilemma constraints in the first-stage regression to identify exogenous components of within-country variation in annual GDP growth. For the second-stage, civil conflict is regressed on the instrumented annual growth rate in a specification originated by Miguel, Satyanath, and Sergenti (2004). That is, for all countries in the 1973-2004 sample for which both second- and first-stage data are available, we estimate the following system of two equations by a linear Two-Stage Least Squares estimator:

$$\begin{aligned} y_{it} = \beta_i + \beta_i^{trend} t + \beta_1 R^b_{i(t-1)} + \beta_3 (R^b_{i(t-1)} \times Peg_{i(t-1)}) + \beta_5 (R^b_{i(t-1)} \times KAOPEN_{i(t-1)}) \\ + \beta_6 (R^b_{i(t-1)} \times Peg_{i(t-1)} \times KAOPEN_{i(t-1)}) + v_{it} \end{aligned} \tag{1}$$

$$Conflict_{it} = \gamma_i + \gamma_i^{trend} t + \delta \hat{y}_{it} + \varepsilon_{it} \tag{2}$$

[19] Despite these data limitations, di Giovanni and Shambaugh (2008) find that their results (which I replicate in the first-stage estimation) do not vary significantly with modifications to these cutoffs, and in some cases are strengthened. Correspondingly, we test the robustness of first- and second-stage specifications of this paper to a variety of marginal deviations in these sub-sampling parameters to find virtually no change in results (the various estimates from these robustness checks are omitted for clarity and space).

where y_{it} represents the annual growth rate in real GDP for country i in time t, R^b_{it} denotes the short term nominal interest rate of the base of country i, Peg_{it} is a binary variable equaling one if country i maintains a fixed exchange rate to its base country, $KAOPEN_{it}$ is the Chinn-Ito (2008) measure of capital account openness, $Conflict_{it}$ is the PRIO/Uppsala binary variable indicating the presence of civil conflict (more than 25 combat-fatalities) in country i at year t, and v_{it} and ε_{it} represent error terms.

Country-specific intercepts (β_i and γ_i) are included to control for unobserved country-level characteristics (e.g., institutional quality) that are potentially correlated with the propensity to experience civil war. The inclusion of these effects ensures that the results are driven only by the parts of within-country variation in annual economic growth that are correlated to within-country variation in base country interest rates. As with Miguel, Satyanath, and Sergenti (2004), country-specific time trends (β_i^{trend} and γ_i^{trend}) are included for comparability. Finally, estimated standard errors are clustered at the country level to adjust for possible within-country correlation in the error term.[20]

The first-stage regression provides two sources of exogenous movement in GDP growth. First, each country's GDP growth responds to its base country interest rates, thereby generating within-country variation in GDP growth. To the extent that each base rate is imperfectly correlated with other rates (see figure 2, panels A and B), this gives cross-country variation in GDP growth at any particular point in time. Second, within a group of countries that use the same base country for monetary policy guidance, some adhere to a pegged regime and have a

[20] To be specific, we use xtivreg2 in Stata with the option of clustered standard errors by country. We also estimate all specifications with standard errors clustered by base country or by country *and* year to examine whether the level of statistical significance is sensitive either to correlation within countries sharing the same base country or to contemporaneous correlation across country. The results are essentially the same if not stronger and thus are not reported to be comparable to prior works and to conserve space.

more open capital account than others. Since the trilemma predicts that the sensitivity of a small open economy to base country interest rates depends crucially on exchange rate regime and capital account openness, this cross-country heterogeneity in trilemma configuration generates further cross-country variation in domestic monetary policy and hence GDP growth.

Prior literature on the trilemma (e.g., di Giovanni and Shambaugh, 2008; Frankel, Schmukler, and Servén, 2002) consistently shows that the coefficients on both base interest rates and their interaction with a binary variable for the presence of a fixed exchange rate regime are negative. These results suggest that while countries with a floating currency may experience economic downturns in response to higher base interest rates (perhaps due to trade channels or "fear of floating"[21]), the negative economic impacts of interest rates hikes in base countries are larger for countries with fixed exchange rates. We expect similar results here, given that we follow the standard specification.

Due to the difficulty in measuring capital account openness (Edison et al., 2002; Eichengreen, 2002), past studies have found less statistical significance in the interaction between base country rates and domestic financial liberalization (see di Giovanni and Shambaugh, 2008) even though the coefficient on this variable should be negative by the theory of the trilemma. In order to test for possible nonlinearities in the effects of the trilemma, we also include a triple interaction term $R^{b}_{i(t-1)} \times Peg_{i(t-1)} \times KAOPEN_{i(t-1)}$. Theory suggests that the coefficient on this term, if significant, should also be negative, though there is little robust evidence in the empirical literature supporting this prediction.

[21] Theory of the fear of floating suggests that factors of credibility, exchange rate pass-though, and foreign currency liabilities prevent countries with *de jure* floating regimes from sustaining total monetary independence (Calvo and Reinhart, 2002; Hausmann, Panizza, and Stein, 2001)

To obtain consistent estimates of the effects of economic growth on civil war (i.e., δ), two standard conditions for instrumental variable analysis must be met. First, the instruments must strongly predict movement in output growth to avoid the possibility of weak instrumentation that would result in estimates biased towards the uninstrumented Ordinary Least Squares results (Nagar, 1959; Angrist and Krueger, 2001; Stock and Yogo, 2005). The vast empirical literature on the trilemma suggests that this paper's instrumentation is strong – earlier work has consistently demonstrated the relationship between base country interest rates and measures of the prevailing exchange rate regime to be strong and robust to a variety of specifications and samples.[22] For each specification, we calculate the Kleibergen-Paap (2006) rk statistic to test for the strength of the instrumentation.

The second, more subtle, and fundamentally untestable requirement for valid identification is that the instrumental variables must be uncorrelated with the error term of the second-stage equation; that is, the instruments in the first-stage must be unrelated to all domestic conditions correlated with the incidence of internal violence that are not otherwise controlled for. If this exclusion requirement is not met, the resulting bias in instrumental variable estimation

[22] The conjectures of the trilemma have received wide support from empirical investigation. Borensztein, Zettelmeyer, and Philippon (2001) find within an event study methodology for the emerging markets of Latin America and Asia that countries with rigid exchange rate regimes like Hong Kong and Argentina adapt their interest rates much more closely to those of the United States than countries that freely float their currencies. Di Giovanni, McCrary, and von Wachter (2009) show that for the sample of European countries that collectively peg their exchange rates and are committed to open capital accounts amongst one another, the monetary policy of the German Bundesbank affects each individual country's monetary policy and its short-run economic performance. Frankel, Schmukler, and Servén (2004) find this pattern to hold more generally in a global sample of countries since 1970, and Berg, Borensztein, and Mauro (2004) and di Giovanni and Shambaugh (2008) examine how a given position in the trilemma, particularly the choice of exchange rate regime, determines the vulnerability of domestic output to movement in base interest rates. In particular, di Giovanni and Shambaugh (2008) find that hikes in foreign interest rates have a contractionary effect on domestic real output growth rates through the transmission of higher domestic rates, but that this effect is centered on countries that peg their currency with relatively open capital flows. Finally, historical research by Aizenman, Chinn, and Ito (2008) and Obstfeld, Shambaugh, and Taylor (2005) shows that the constraints on exchange rate, capital account, and monetary policy tools implied by the trilemma have consistently guided the short-run economic development of a vastly diverse sample of countries.

may be significantly worse than that due to from measurement error, endogeneity, and omitted variable bias in the Ordinary Least Squares estimation (Angrist and Krueger, 2001).

There are compelling reasons to expect that this exclusion requirement is satisfied. Compared to the terms of trade instrumentation of Besley and Persson (2009) and Brücker and Ciccone (2010), foreign interest rates are more plausibly exogenous to domestic conditions as financial flows between any given developing country and its base make up only a small fraction of movement in the global financial market (Lucas, 1990). Furthermore, as di Giovanni and Shambaugh (2008) suggest, it is unlikely that economic and political conditions in a small open economy directly and systematically affect the monetary policy decisions of base country central banks. Although theory thus predicts the plausible exogeneity of base country interest rates, we run a series of robustness checks to address lingering concerns of possible short-run endogeneity.

Even if base country interest rates *per se* are reliably exogenous to domestic social and political conditions, this paper's instrumentation strategy may be invalid if domestic policy choices that determine either the base country or the *de facto* configuration of the trilemma are systematically related to unobservable determinants of civil conflict. The literature on the choice of exchange rate regime type, however, finds no robust or systematic relationship between exchange rate regimes and relevant politico-economic characteristics (see Juhn and Mauro, 2002 for an extensive survey).[23] On the other hand, the literature on capital controls and liberalization suggests that domestic political conditions may be important determinants of a country's decision to restrict international movement of financial capital (e.g., Quinn and Inclan, 1998;

[23] A recent study by Meissner and Oomes (2009) suggests that choice of base countries seems to be largely shaped by network externalities whereby originally idiosyncratic choices by influential national actors encourage coordination and path dependency rather than by any contemporaneous domestic economic or social conditions.

Johnson and Mitton, 2003; Stulz, 2005), which may give lingering concerns for instrument validity. In response, we estimate the first- and second-stage equations omitting the interaction of base rates with capital account openness from the set of instruments in order to determine whether the main results are driven in part by an endogenous relationship between capital account liberalization and civil war risk.

IV. Results

Table 2 reports the main results of estimating equations (1) and (2) as well as the un-instrumented first- and second-stage relationship for comparison.[24] Column 1 confirms that the transmission of higher base country interest rates is reflected in contractions in domestic output growth rates. GDP growth in country-year observations with fixed exchange rates declined on average by 0.14 percentage points for each one percentage point increase in base country interest rates. The coefficient on this interaction term is statistically significant at the 99% level and virtually identical with that reported in earlier empirical studies (e.g. di Giovanni and Shambaugh). Column 2 reports the simple (un-instrumented) correlation between within-country variation in GDP growth rates and the likelihood of internal conflict with a simple linear probability model.[25] The coefficient estimate, although significant at the 95% level, suggests a

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[24] We also check how well the theoretical implications of the trilemma hold for this paper's dataset by correlating domestic interest rates to base country interest rates, given its implications in delivering proper identification for the subsequent analyses of civil conflict. We are able to replicate the results of Frankel, Schmukler, and Servén (2004), Shambaugh (2004), Obstfeld, Shambaugh, and Taylor (2005). The results are not reported to conserve space but available from the authors upon request.

[25] This relationship is also estimated with a logit specification. The results are similar to those in the linear probability model and results of this estimation are omitted for clarity and space. We follow Miguel, Satyanath, and Sergenti (2004) in restricting attention to linear specifications for reasons described by the authors.

very small effect of economic contraction on civil war: a one percentage point drop in output growth increases the probability of internal violence on average by only 0.35 percentage points.

Columns 3 through 5 report the main instrumental variable results. The first-stage results in column 3 show that once the lagged base country interest rates and interactions are included as additional explanatory variables for local economic fluctuations, the coefficients on the contemporary base rates and interactions lose statistical significance, suggesting that the real economic impacts of base country interest rates are primarily felt by a one-year lag. [26] We additionally include the one-year lead of base country interest rate variable and its interaction with the pegged exchange rate indicator variable (column 4) and find the coefficients on these variables as statistically insignificant as those on the contemporaneous variables. These results serve as a simple and yet important placebo test: if a recession in small open economy systematically causes a large enough capital flow into base countries to lower base country rates, then one would expect to observe a contemporaneous positive correlation between economic growth and base country interest rates or a positive correlation between current economic growth and future base interest rates.[27] Moreover, these placebo tests suggest that our instruments are unlikely to capture serially correlated unobservable factors.

The coefficient on lagged uninteracted base country rates is statistically significant and negative, consistent with the idea that although pursuing a floating exchange rate may in theory

[26] In order to test for the potential persistence of monetary policy on the economy the estimation is also run with variables lagged longer than one period. We find virtually no statistical significance in the longer lags and otherwise consistent results. We omit these additional robustness checks for clarity.

[27] One endogeneity story that these simple placebo tests cannot adequately address is that the *expectation* of recession causes a large enough capital flight into base countries to lower base rates. If this were the case, the first-stage results would understate the negative correlation between current GDP and past base interest rates to the extent that the expectation turns out to be correct.

allow for monetary independence, a given country may be unwilling to pursue a completely independent policy agenda or may be restricted in part by a fear of floating (Calvo and Reinhart 2002; Hausmann, Panizza, and Stein, 2001). The second-stage results show that the coefficient on the instrumented GDP growth is negative and statistically significant: the effect of a one percentage point decline in annual GDP growth in any given year is found to be a 1.3 percentage point increase in the probability of civil conflict. Interestingly, the estimated effect in the instrumental variable results (columns 3 and 4) is found to be larger than that in a simple OLS estimate (column 2), suggesting that the OLS estimates may suffer from a large attenuation bias due to measurement error in income growth rates.[28] With the typical country facing a 17.9% risk of internal conflict each year (see table 1), the decline in GDP growth rates by a single percentage point translates to more than a 7% increase in conflict risk in a single year.

The strength of the causal relationship found between GDP growth and civil conflict is a smaller, though still statistically significant (at the 10% level), estimate of that found by Miguel, Satyanath, and Sergenti (2004), who report a nearly 2.5 percentage point increase in conflict risk with the same economic shock in Sub-Saharan Africa. That is, the average effect of economic shocks on the probability of conflict may be smaller over a global sample of countries than over a sample restricted to Africa. This suggests that while the income-conflict relationship documented in Miguel, Satyanath, and Sergenti (2004) might be applicable to non-African

[28] This attenuation bias from measurement error in GDP is likely both pervasive and very large. Johnson et al. (2009) show that for at least one particular measure of GDP, the Penn World Table (PWT), estimates in average growth rates may vary across revisions of the dataset by 1.1% on average. As a particularly dramatic example, Equatorial Guinea, which was ranked as the second-fastest growing African country in version 6.2 of the PWT, was listed as the slowest growing country in version 6.1, released just four years earlier. This observation has motivated many researchers, such as Henderson, Storeygard, and Weil (2009) to seek creative proxies and instruments to GDP growth rates in developing countries that limit the effect of measurement error. Incidentally, Miguel, Satyanath, and Sergenti (2004) also find that the OLS estimates are smaller than IV estimates.

countries, African countries have peculiar conditions that make them more vulnerable to civil conflict during a recession.

In columns 5 and 6, we include as instruments the interactions of the Chinn-Ito measure of capital account openness with base country interest rates and the pegged exchange rate indicator variable. Neither enters the first-stage with much statistical significance, though the inclusion of these variables does not alter the approximate magnitude or statistical significance of the other instruments. In the full model (column 6), however, the lagged interaction of base rates with $KAOPEN_{it}$ enters the specification negatively, as predicted by the theory of the trilemma.

As noted previously, empirical research of the trilemma's effect on domestic economic conditions has found less consistent support for the relevancy of difficult-to-measure financial openness than in the simple matter of pegged exchange rates (e.g. di Giovanni and Shambaugh, 2008). To the extent that even the best available measures of financial liberalization are highly prone to measurement error, attenuation bias may push coefficient estimates towards zero. However, since the coefficient estimates on GDP growth in the second-stage are highly robust to the inclusion (or exclusion) of $KAOPEN_{it}$, we choose to use the full model of column 6 as the preferred set of instruments so that this paper's first-stage specification is consistent with the theory of trilemma for the remainder of this paper's empirical exposition.

It is worth emphasizing that the second-stage results are highly robust to the choice of instruments. Recall that one of the possible concerns with this paper's instrumentation strategy is that the trilemma configuration consists of policy choice variables that could be endogenous to the risk of civil war. The results show that the second-stage results from the full model with all possible interactions (column 6) are similar to those obtained by other specifications that omit some (or all) of the interaction terms (columns 3, 4, and 7). In particular, we obtain similar estimates of the effects of GDP growth on civil war risk even when the model relies only on base rates as an instrument (column 7), although a lower first-stage R^2 suggests that the variables of the trilemma better explain the relationship between base country monetary policy and domestic GDP growth, and when we only use the base rates and their interaction with the pegged exchange rate regime dummy (columns 3 and 4). These results give some assurance that the endogeneity of capital openness is unlikely to drive the main results.

Postestimation tests are run on each of the instrumental variable results in columns 3-6 to provide further empirical support for the strength and validity of the identification strategy. The Kleibergen-Paap rk LM statistic is large enough to reject the null hypothesis that the endogenous variable (GDP growth) is underidentified and that the estimator is not full rank with over 99.9% confidence. Out instruments seem strong as indicated by large Kleibergen-Paap rk Wald F statistic.[29] Finally, the Hansen J suggests that the base country interest rate and trilemma identification framework does not violate the exclusion requirement. That is, even if exchange rate and capital control policy decisions are endogenous to the probability of civil conflict, these results indicate that the interactions of these variable with base country interest rates are exogenous enough for proper identification.

[29] This strategy differs from the typical approach for testing for weak instrumentation given by Stock and Yogo (2005), who compile and use critical values of the Cragg-Donald (1993) statistic under the assumption of i.i.d. well-behaved error terms. In the presence of heteroskedastic error terms, and in particular the clustering of second-stage residuals used in this paper, the preferred statistic is a robust Wald F statistic based on the same Kleibergen-Paap rk LM statistic used in the underidentification test.

B. Socio-Political Interaction Effects

This paper is also concerned with testing the degree to which socio-political and other factors affect the main relationship between domestic GDP growth rates and the outbreak of civil conflict. Fearon and Laitin (2003) posit a variety of ways that such factors – namely, ethnolinguistic fractionalization, the lack of democratic political institutions, the presence of rough terrain and a mountainous geography, and the reliance on oil exportation – make civil conflict more likely (Fearon and Laitin, 2003; Karl, 1997). We follow Miguel, Satyanath, and Sergenti (2004) in estimating coefficients on instrumented interactions between these factors and GDP growth to investigate a more nuanced story for how non-economic conditions affect the income-conflict nexus.

Closely following the empirical strategy of Miguel, Satyanath, and Sergenti (2004), we estimate following system of three equations:

$$\begin{aligned} y_{it} = \beta_i + \beta_i^{trend} t + \beta_1 R^b_{i(t-1)} + \beta_3 (R^b_{i(t-1)} \times Peg_{i(t-1)}) + \beta_5 (R^b_{i(t-1)} \times KAOPEN_{i(t-1)}) \\ + \beta_6 (R^b_{i(t-1)} \times Peg_{i(t-1)} \times KAOPEN_{i(t-1)}) + \beta_7 (R^b_{i(t-1)} \times c_{it}) \\ + \beta_8 (R^b_{i(t-1)} \times Peg_{i(t-1)} \times c_{it}) + \beta_{10} (R^b_{i(t-1)} \times KAOPEN_{i(t-1)} \times c_{it}) \\ + \beta_{11} (R^b_{i(t-1)} \times Peg_{i(t-1)} \times KAOPEN_{i(t-1)} \times c_{it}) + v_{it} \end{aligned} \tag{3}$$

$$\begin{aligned} y_{it} \times c_{it} = \lambda_i + \lambda_i^{trend} t + \lambda_1 R^b_{i(t-1)} + \lambda_3 (R^b_{i(t-1)} \times Peg_{i(t-1)}) + \lambda_5 (R^b_{i(t-1)} \times KAOPEN_{i(t-1)}) \\ + \lambda_6 (R^b_{i(t-1)} \times Peg_{i(t-1)} \times KAOPEN_{i(t-1)}) + \lambda_7 (R^b_{i(t-1)} \times c_{it}) \\ + \lambda_8 (R^b_{i(t-1)} \times Peg_{i(t-1)} \times c_{it}) + \lambda_{10} (R^b_{i(t-1)} \times KAOPEN_{i(t-1)} \times c_{it}) \\ + \lambda_{11} (R^b_{i(t-1)} \times Peg_{i(t-1)} \times KAOPEN_{i(t-1)} \times c_{it}) + v_{it} \end{aligned} \tag{4}$$

$$Conflict_{it} = \gamma_i + \gamma_i^{trend} t + \delta_1 \hat{y}_{it} + \delta_2 (\widehat{y_{it} \times c_{it}}) + \varepsilon_{it} \tag{5}$$

for each socio-political or geographic variable c_{it}. As with the main estimation of equations (1) and (2), and as with Miguel, Satyanath, and Sergenti, 2004, both country-specific intercepts (β_i and γ_i) and country-specific time trends (β_i^{trend} and γ_i^{trend}) are included, and estimated standard errors are clustered to allow for arbitrary correlation within countries. [30]

Second-stage Two-Stage Least Squares estimates of equation (5) are given in table 3 for each of the five socio-political and geographic variables. Curiously most of these interactions are insignificant. In particular, the degree to which a country pursues democratic political institutions or relies on oil exports does not affect the main income-conflict relationship, as found in Miguel, Satyanath, and Sergenti (2004). That is, despite a dramatic increase in the variation of the Polity IV democracy measure in this global sample, we are unable to reject the null hypothesis that highly democratic and highly authoritarian countries are equally susceptible to conflict during negative short-term domestic income shocks.

Interestingly, however, in column (1) we find that the inclusion of the interacted ethnolinguistic fractionalization variable eliminates the statistical significance of the main effect of uninterested GDP growth and is itself statistically significant with 90% confidence and quantitatively important. For a country such as Chile, Kuwait, or Venezuela at the 25th percentile of ethnolinguistic fragmentation (0.102), a one percentage point decline in domestic GDP growth results in an increase in the probability of conflict by only 0.5 percentage points. However, for a country at the 75th percentile (0.678), the resulting increase in conflict risk by the same shock in internal GDP growth is 3.39 percentage points (examples of such countries include Pakistan,

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[30] Again, we also estimate all specifications with standard errors clustered by base country or by country *and* year. The results turn out to be essentially the same and thus are not reported to be comparable to prior works and to conserve space.

Ethiopia, and Bolivia). For countries at the maximum of the scale of ethnolinguistic fractionalization such as Cameroon, Nigeria, India, Kenya, and Sierra Leone, extrapolation of this interaction effect is more dire still – with a measure of ethnolinguistic fragmentation of 0.892, a country like Cameroon is expected to see a 4.47 percentage point spike in conflict risk with only a single percentage point fall in GDP growth. This represents an increase in the probability of conflict by nearly 25%. Thus, as predicted by the political science literature, a more socially fragmented country appears much more likely to fall to internal violence in the face of sudden economic contraction.[31]

The results of this section should be interpreted with particular care as the small values of the Kleibergen-Paap rk Wald F statistic (never above 6) bring into question the strength of these instruments. As a weak instrument tends to bias coefficients towards their corresponding Ordinary Least Squares estimates (Angrist and Krueger, 2001), it may be the case that the estimates found here (and also in earlier empirical literature) present a lower bound to the true relationship between domestic economic and socio-political conditions and the probability of civil conflict.[32]

[31] It is important to note that these results are unlikely to be driven by the correlation between exchange rate regime and ethnic diversity as the simple correlation coefficient between these two variables is found to be less than 0.09.

[32] Although not reported to preserve clarity and space, I estimate equation (5) for each socio-political interaction effect with a simple OLS estimator to determine the possible direction of the weak instrumentation bias. All are found to be consistent in sign though much smaller in absolute value, supporting the hypothesis that this paper's weakly-instrumented estimates represent a lower bound to those that might be obtained from a stronger identification. For example, the interaction coefficient for ethnolinguistic fractionalization is estimated as -0.324 by the OLS framework.

V. Robustness Checks

In this section, we consider a series of robustness checks and sub-sample analyses in order to further rule out alternative explanations for the main results and support this paper's identification strategy and conclusions.

A. Endogeneity of the Trilemma

Within the context of this paper's pseudo-experimental design, a typical "control" country, namely one with a floating currency and/or a tightly controlled capital account, may be intrinsically different than a typical "treatment" country with a fixed exchange rate and open capital account. To the extent that countries that pursue currency stability and financial liberalization are substantially different from those that choose other specifications of the trilemma, the comparison of average treatment effects might be misleading (Angrist and Krueger, 2001).

As noted previously, we include country fixed effects in all specifications to ensure that unobservable time-invariant institutional factors that could drive both the choice of trilemma conficturation and the incidence of civil war are held constant throughout the analysis. We also drop country-year observations that correspond to episodes of extreme economic conditions (namely, hyperinflation and dramatic collapses in GDP) as they may arise alongside destructive civil conflicts and are most likely to influence the contemporaneous choice of the trilemma configuration. We follow di Giovanni and Shambaugh (2008) in dropping all countries that either never peg or always peg their currency throughout the sample period, leaving us with a sample of countries that have plausibly similar likelihoods of engaging in strict currency stabilization. Finally recall that we obtain similar results with a simple specification that relies only on the variation in base country interest rates.

Nonetheless, there may be lingering concerns that the decision to pursue a fixed exchange rate or more open capital account is systematically related to the outbreak of civil conflict. As an example, the costs of containing and defeating a potential insurrection may tighten the budget constraints of the ruling domestic government and increase the incentives of that government to monetize debts, inflate the currency, turn to a floating exchange rate, or tighten the control of financial flows. To check the plausibility of these alternative explanations, we explore whether there is a meaningful correlation between the exchange rate regime, financial openness, and inflation of the domestic economy with the incidence of civil conflict. Additionally, we check the robustness of the income-conflict relationship to the inclusion of these (un-interacted) macroeconomic choice variables as controls.

Coefficient estimates for these robustness checks are reported in table 4. We find that the exchange rate regime indicator does not simply correlate with the probability of civil war (column 1). That is, exchange rate regime appears unrelated to the probability of internal war *except* by affecting the channel of interest rate transmission from base country in the first-stage regression. Furthermore, directly controlling for exchange rate regime type in the second-stage does not alter the sign, magnitude, or statistical significance of either the first-stage trilemma relationship or the main income-conflict relationship (column 5).

The degree of financial openness and the contemporaneous inflation rate are found to significantly correlate with the incidence of civil conflict (columns 2 and 3). Violence is associated with countries with more controlled capital flows and a more quickly expanding currency, as expected. Nonetheless, the inclusion of these variables as controls to the main specification does not greatly change the results, with the full model (column 8) still predicting a statistically significant, though smaller, effect of negative income shocks on the likelihood of war. This again reinforces the validity of the identification strategy as it demonstrates that the coefficients on the interaction of base rates to the trilemma configuration are not greatly biased even with the omission of Peg_{it} and $KAOPEN_{it}$ in the first-stage. Furthermore, although the variables of the trilemma may relate to civil conflict directly, the interactions of base rates to the trilemma configuration do not seem to be significantly correlated with the second-stage error term. Postestimation statistics for these specifications suggest that each is strongly identified (by the Kleibergen-Paap rk Wald F statistic) by plausibly exogenous instruments (by the Hansen J statistic).

B. Endogeneity of Base Country Interest Rates

Although the trilemma variables may be plausibly exogenous to domestic social and political conditions, it remains an open question whether or not base country interest rates similarly satisfy the exclusion requirement. In particular, if financial crises coinciding with domestic civil conflicts (as with Indonesia in 1998) occur in relatively large and financially-integrated economies, they can endogenously affect base country interest rates via at least two channels: a “flight to quality” in which international investors seek safer haven in investments in the base country, pushing down base country rates with higher demand, and/or the deliberate action taken by base country central banks.[33] Even in the absence of financial crises to which the base country central bank may respond, a sufficiently large and economically influential country may cause enough movement in international capital flows to affect base country interest rates endogenously.

We address this concern in two ways. First, we follow di Giovanni and Shambaugh (2008) in including base country and worldwide GDP growth rates as controls to test the degree to which civil conflicts and base country interest rates are driven by external political and economic events may be unobservable yet important. Parameter estimates are included in table 5 – as with the exchange rate regime variable, neither base country nor global GDP growth appears correlated with the incidence of domestic conflict (columns 1-3) and the inclusion of these variables as controls in the second-stage serves only to strengthen the primary income-conflict relationship (columns 4-6).

Second, we drop country-year observations from the sample that might introduce endogeneity between domestic conditions and base country monetary policy. We identify episodes of financial crises from 1973-2004 using data compiled by Laeven and Valencia (2008) and follow di Giovanni and Shambaugh (2008) in identifying "large economies" – that is, countries whose economic size (measured by real GDP) is reported as greater than 10% the size of their respective base. We examine both the simple instrumented income-conflict relationship and the fully-controlled model over three sub-samples defined by these variables: one excluding

[33] Although rare, there are historical cases where the central banks of major base country economies have responded to financial shocks originating in small foreign countries. A particularly dramatic example is given by the East Asian financial crisis of the late 1990s. From 1997-1998, as the crises spread from the proximate devaluation of the Thai baht to affect the financial systems of neighboring Asian countries (one of which, Indonesia, subsequently suffered from internal violence), the U.S. Federal Reserve Board moved to cut its lending rate three times at regularly scheduled and emergency meetings (Eichengreen, 1999).

all observations of financial crises in influential countries, one dropping all episodes of financial crisis, and one without any observations from influential countries.

The results of theses sub-sample analyses are reported in table 6. Dropping country-year observations that correspond to financial crises in influential domestic economies (columns 1 and 4) produces nearly identical first- and second-stage estimates. Moreover, the results are virtually unchanged when omitting all 73 observations of crisis (columns 2 and 5). Coefficients on base country rates and their interaction with the two trilemma variables are found to be practically and statistically significant and of the expected negative sign, and the effect of an instrumented decrease in GDP growth rates by one percentage point is found to be an increase in conflict risk by more than 1.6 percentage points. Thus, although historically it may be the case that base country interest rates react to some of the large scale economic turmoil and crisis in small domestic countries, excluding these cases does not alter this paper's main conclusions.

Coefficient estimates for the main equations over a sample of countries with economies no bigger than 10% of that of their base country are given in columns 3 and 6. This requirement excludes a total of 15 countries from the global sample, including the potentially influential economies of China, India, Japan, Singapore, Switzerland, and the United Kingdom.[34] Casewise deletion of missing observations further reduces the total number of countries to 92, representing a drop in sample size and likely in overall variation that may reduce statistical leverage. Nevertheless, first-stage results are unchanged by this subsetting, with the full model in column (6) yielding statistically significant interactions of base country interest rates with measures of

[34] A full list of countries excluded by this subsetting criterion is available in table C2 (Appendix C)

the trilemma with 95% confidence. Second-stage coefficients are less significant but of comparable size and magnitude.

C. Robustness of Ethnolinguistic Fractionalization Effects

Finally, we test the robustness of the interaction result – that of GDP growth and ethnolinguistic fractionalization – to the inclusion of the same country-level macro-policy controls, regional and world income growth controls, and to various subsample analyses. Second-stage estimation results are given in table 7. The results remain fairly consistent over each robustness check. Controlling for the variables of the trilemma and inflation in the second-stage (column 2) shrinks the coefficient on the interaction term to -4.38, though this estimate remains statistically significant at the 90% confidence level. In the full model (column 4) that controls for capital account openness, exchange rate regime, inflation, and world and base country GDP growth rates, this parameter is further decreased in magnitude to -4.33 (statistically significant with 87% confidence) but is relatively consistent with that found by the simpler specification. Reducing the sample by dropping episodes of financial crisis actually strengthens these interaction results (table 8, columns 2 and 5), inflating the magnitude and statistical significance both with and without the matrix of controls. In the sample restricted to small economies (columns 3 and 6), the estimated coefficient remains negative and is of the expected sign and size, but statistical significance is lost in the fully controlled model. All of these coefficients may thus represent some kind of lower bound to the true relationship between short-term income shocks and the likelihood of civil conflict, as was the case with the baseline specification.

VII. Conclusions

This paper develops a new identification methodology based on the theory of trilemma to reevaluate the potential economic causes of civil conflict while addressing the core empirical problems of previous cross-country investigations. We find that base country interest rates and their interaction with domestic measures of capital account openness and exchange rate regime type significantly predict domestic output growth rates while remaining plausibly uncorrelated with the second-stage disturbance term. This approach reveals that earlier findings regarding the causal impact of output growth shocks on the likelihood of civil conflict in Sub-Saharan Africa might extend across the world: a negative growth shock of four percentage points increases the probability of internal conflict by over a third for the typical country in a global sample. Moreover, while it does not appear that most social and political institutional characteristics affect this causal relationship, we find some evidence to suggest that more culturally and ethnically diverse countries fall more easily into conflict in the face of sudden economic hardship, a theory promoted by modernist political science literature.

These interaction results are particularly relevant for Sub-Saharan African countries, most of which are characterized by the highest level of ethnic diversity. As Easterly and Levine (1997) among others describe, the high degree of cultural fractionalization in Africa has grave consequences for the development of political and economic institutions and the public policy choices that determine long-run economic growth. They propose that more polarized countries are made more susceptible to competitive rent-seeking across different ethnic groups and are thus unlikely to develop the necessary public goods of infrastructure, education, and political policy.[35] This paper's results suggest an additional and perhaps complementary mechanism by which ethnolinguistic diversity might matter in the particular context of African development: temporary economic shocks are more likely to cause civil conflicts in ethnically fragmented countries, and these conflicts may translate into deeper, medium- to long-term economic malaise. This is a view consistent with the growth puzzle observed by Rodrik (1999), who notes that although external economic shocks and civil conflict explain much of the large cross-country variation in growth rates in the late 1970s and early 1980s, countries that saw the largest "growth collapses" from these shocks were ones that were divided ethnolinguistically.

[35] See Alesina, Baqir, and Easterly (1999), Tornell and Lane (1999) for similar arguments.

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Table 1 – Descriptive Statistics

	Mean	St. Dev.	Min.	Max.	Obs.
Conflict	0.179	0.384	0	1	2778
Between Countries		0.269			106
Within Countries		0.264			
Peg	0.420	0.494	0	1	2803
Between Countries		0.320			107
Within Countries		0.369			
KAOPEN	0.0413	1.48	-1.81	2.54	2722
Between Countries		1.25			107
Within Countries		0.874			
Interest Rates	0.101	0.0736	0.00000851	0.861	1861
Between Countries		0.0613			91
Within Countries		0.0538			
Base Interest Rates	0.0666	0.0352	0.0101	0.213	2752
Between Countries		0.0164			106
Within Countries		0.0321			
GDP Growth	0.0369	0.0436	-0.190	0.198	2803
Between Countries		0.0182			107
Within Countries		0.0402			
Inflation	0.101	0.0949	-0.217	0.497	2803
Between Countries		0.0581			107
Within Countries		0.0776			
Ethnolinguistic Fract.	0.399	0.287	0.00412	0.892	2187
Between Countries		0.278			102
Religious Fract.	0.361	0.224	0.00	0.778	2187
Between Countries		0.226			102
Democracy	2.13	7.53	-10.0	10.0	2185
Between Countries		6.60			101
Within Countries		3.44			
Logged Mountainous	2.19	1.46	0.00	4.32	2187
Between Countries		1.47			102
Oil-exporting Country	0.143	0.350	0	1	2050
Between Countries		0.331			102
Within Countries		0.135			

Notes: Data are from the global sample, 1973-2004, and are cleaned in the manner described in section IIIF. Some social and geographic variables are time-invariant and thus do not vary within countries.

Table 2 – Main Estimation Results

First-stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Base R	-0.0488		0.00123	-0.0132			
	(0.0367)		(0.0639)	(0.0695)			
Base R × Peg	-0.136***		0.00944	0.0291			
	(0.0507)		(0.0380)	(0.0401)			
Peg	0.0144***						
	(0.004)						
Base R (t-1)			-0.282***	-0.279***	-0.288***	-0.291***	-0.317***
			(0.0602)	(0.0592)	(0.0408)	(0.0401)	(0.0422)
Base R × Peg (t-1)			-0.118***	-0.125***	-0.102***	-0.0943**	
			(0.0429)	(0.0428)	(0.0386)	(0.0381)	
Base R (t+1)				0.0292			
				(0.0598)			
Base R × Peg (t+1)				-0.0774			
				(0.0558)			
Base R × KAOPEN (t-1)					0.00938	-0.00290	
					(0.0199)	(0.0184)	
Base R × Peg × KAOPEN (t-1)						0.0304	
						(0.0269)	
R^2	0.0128		0.0473	0.0513	0.0472	0.0480	0.0413
Second-stage							
GDP growth		-0.351**	-1.29*	-1.29*	-1.41	-1.54*	-1.22
		(0.169)	(0.793)	(0.730)	(0.888)	(0.899)	(0.931)
Obs.	2727	2746	2572	2429	2496	2496	2575
No. of Countries	105	105	104	104	104	104	104
Root MSE	0.041		0.220	0.212	0.219	0.220	0.219
Kleibergen-Paap rk LM Statistic			38.4***	38.1***	37.7***	39.2***	37.4***
			[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Kleibergen-Paap rk F Statistic			15.8	11.4	20.5	16.6	56.3
Hansen J Statistic			1.80	1.78	3.01	3.33	0.00
			[0.615]	[0.879]	[0.222]	[0.343]	---

Notes: The dependent variable is GDP growth in the first-stage and the civil conflict indicator in the second-stage. Robust standard errors clustered at the country level are included in parentheses. Asterisks denote statistical significance at the 90% (*), 95% (**), and 99% (***) levels. All regressions are run with country fixed effects and country-specific time trends (estimates omitted to conserve space). P-values for the null hypotheses of underidentification and instrument exogeneity are given in brackets underneath the Kleibergen-Paap rk LM and Hansen J statistics, respectively. In column 7, GDP growth is exactly identified by a single instrument and overidentifying restrictions cannot be tested

Table 3 – Socio-Political Interaction Effects

	(1)	(2)	(3)	(4)	(5)
GDP Growth	0.963 (1.20)	-1.56 (1.29)	-1.27* (0.752)	-1.42 (1.08)	-2.31* (1.42)
Ethnolinguistic fractionalization × GDP growth	-5.01* (2.73)				
Religious Fractionalization × GDP growth		0.656 (2.24)			
Democracy, *t*-1 × GDP growth			0.142 (0.253)		
Log(mountainous) × GDP growth				0.00205 (0.368)	
Oil-exporting country × GDP growth					7.58 (6.17)
Obs.	1938	1938	1938	1983	1938
No. of Countries	93	93	93	93	93
Kleibergen-Paap rk LM Statistic	18.9*** [0.0086]	19.4*** [0.0072]	13.4* [0.0640]	20.9*** [0.0039]	2.98 [0.887]
Kleibergen-Paap rk Wald F statistic	3.51	5.91	2.16	4.49	0.494
Hansen J statistic	5.87 [0.437]	8.87 [0.181]	9.23 [0.161]	3.42 [0.755]	7.85 [0.250]

Notes: The dependent variable is the civil conflict indicator variable. The instrumental variables are base interest rates at time *t*-1, their full interaction with variables of the trilemma, and their subsequent interaction with the appropriate socio-political variable. Robust standard errors clustered at the country level are included in parentheses. Asterisks denote statistical significance at the 90% (*), 95% (**), and 99% (***) levels. Estimations of country-specific time trends are included but not reported to conserve space. P-values for the null hypotheses of underidentification and instrument exogeneity are given in brackets underneath the Kleibergen-Paap rk LM and Hansen J statistics, respectively.

Table 4 – Main Estimation Results with Controls

First-stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Base R (t-1)					-0.268***	-0.289***	-0.285***	-0.267***
					(0.0420)	(0.0402)	(0.0400)	(0.0420)
Base R × Peg (t-1)					-0.126***	-0.0958**	-0.104***	-0.128***
					(0.0422)	(0.0379)	(0.0384)	(0.0426)
Base R × KAOPEN (t-1)					-0.00339	-0.0433*	-0.0164	-0.0489**
					(0.0185)	(0.0236)	(0.0189)	(0.0234)
Base R × Peg × KAOPEN (t-1)					0.0308	0.0260	0.0335	0.0299
					(0.0270)	(0.0265)	(0.0270)	(0.0267)
Peg (t-1)					0.00612**			0.00443
					(0.00297)			(0.00296)
KAOPEN (t-1)						0.00598***		0.00486**
						(0.00202)		(0.00203)
Inflation							-0.0875***	-0.0829***
							(0.0205)	(0.00207)
R^2					0.0497	0.0534	0.0688	0.0735
Second-stage								
GDP Growth					-1.52*	-1.13	-1.40*	-1.14*
					(0.878)	(0.735)	(0.839)	(0.709)
Peg (t-1)	0.0152			0.0211	0.00215			0.00506
	(0.0212)			(0.230)	(0.203)			(0.0212)
KAOPEN (t-1)		-0.0351**		-0.0306*		-0.0337*		-0.0320**
		(0.0161)		(0.0158)		(0.0159)		(0.0163)
Inflation			0.281**	0.296***			0.129	0.111
			(0.114)	(0.105)			(0.116)	(0.120)
Obs.	2592	2513	2592	2513	2496	2493	2496	2493
No. of Countries	105	105	105	105	104	104	104	104
Kleibergen-Paap rk LM Statistic					37.2***	39.7***	29.4***	38.7***
					[<0.001]	[<0.001]	[<0.001]	[<0.001]
Kleibergen-Paap rk Wald F Statistic					15.6	18.6	16.6	17.6
Hansen J Statistic					3.18	1.65	3.15	1.72
					[0.364]	[0.647]	[0.369]	[0.634]

Notes: The dependent variable is GDP growth in the first-stage and the civil conflict indicator in the second-stage. Robust standard errors clustered at the country level are included in parentheses. Asterisks denote statistical significance at the 90% (*), 95% (**), and 99% (***) levels. All regressions are run with country fixed effects and country-specific time trends (estimates omitted to conserve space). P-values for the null hypotheses of underidentification and instrument exogeneity are given in brackets underneath the Kleibergen-Paap rk LM and Hansen J statistics, respectively.

Table 5 – Main Estimation Results with Controls (continued)

First-stage	(1)	(2)	(3)	(4)	(5)	(6)
Base R (t-1)				-0.263***	-0.246***	-0.245***
				(0.0464)	(0.0407)	(0.0438)
Base R × Peg				-0.0890**	-0.0848**	-0.0846**
(t-1)				(0.0381)	(0.0390)	(0.0387)
Base R ×				-0.00380	-0.00239	-0.00255
KAOPEN (t-1)				(0.0182)	(0.0183)	(0.0182)
Base R × Peg				0.0337	0.0335	0.0338
× KAOPEN (t-1)				(0.0268)	(0.0267)	(0.0267)
Base GDP Growth				0.000895		0.000127
				(0.000733)		(0.00101)
World GDP Growth					0.00213**	0.00201*
					(0.000892)	(0.00122)
R^2				0.0494	0.0511	0.0511
Second-stage						
GDP Growth				-1.90*	-2.27*	-2.27*
				(1.17)	(1.29)	(1.32)
Base GDP Growth	-0.00269		-0.00208	0.00358		-0.000248
	(0.00232)		(0.00356)	(0.00417)		(0.00442)
World GDP Growth		-0.00352	-0.00127		0.0106	0.0109
		(0.00339)	(0.00523)		(0.00774)	(0.00831)
Obs.	2746	2746	2746	2496	2496	2496
No. of Countries	105	105	105	104	104	104
Kleibergen-Paap rk				29.8***	29.1***	27.5***
LM Statistic				[<0.001]	[<0.001]	[<0.001]
Kleibergen-Paap rk				10.2	10.7	9.49
Wald F Statistic						
Hansen J Statistic				3.23	3.15	3.13
				[0.258]	[0.369]	[0.371]

Notes: The dependent variable is GDP growth in the first-stage and the civil conflict indicator in the second-stage. Robust standard errors clustered at the country level are included in parentheses. Asterisks denote statistical significance at the 90% (*), 95% (**), and 99% (***) levels. All regressions are run with country fixed effects and country-specific time trends (estimates omitted to conserve space). P-values for the null hypotheses of underidentification and instrument exogeneity are given in brackets underneath the Kleibergen-Paap rk LM and Hansen J statistics, respectively.

Table 6 – Main Estimation Results and Controls over three Sub-Samples

First-stage	(1) No Large Crises	(2) No Crises	(3) No Large Countries	(4) No Large Crises	(5) No Crises	(6) No Large Countries
Base R (t-1)	-0.290***	-0.274***	-0.310***	-0.228***	-0.211***	-0.267***
	(0.0401)	(0.0376)	(0.0467)	(0.0471)	(0.0455)	(0.0523)
Base R × Peg (t-1)	-0.0945**	-0.101**	-0.0868**	-0.122***	-0.125***	-0.116**
	(0.0381)	(0.0404)	(0.0420)	(0.0430)	(0.0452)	(0.0484)
Base R × KAOPEN (t-1)	-0.00280	-0.00503	-0.0105	-0.0499**	-0.0468**	-0.0594**
	(0.0184)	(0.0179)	(0.00222)	(0.0230)	(0.0231)	(0.0280)
Base R × Peg × KAOPEN (t-1)	0.0303	0.0257	0.0362	0.0327	0.0277	0.0384
	(0.0269)	(0.0283)	(0.0311)	(0.0265)	(0.0279)	(0.0306)
Peg (t-1)				0.00486	0.00452	0.00468
				(0.00302)	(0.00297)	(0.00338)
KAOPEN (t-1)				0.00510**	0.00487**	0.00498**
				(0.00204)	(0.00195)	(0.00224)
Inflation				-0.0790***	-0.0742***	-0.0820***
				(0.00214)	(0.0215)	(0.0228)
Base GDP growth				0.000240	0.000241	-0.00120
				(0.00100)	(0.00104)	(0.000832)
World GDP growth				0.00142	0.00141	0.00294**
				(0.00119)	(0.00122)	(0.00120)
R^2	0.0478	0.0448	0.0480	0.0752	0.0697	0.0770
Second-stage						
GDP Growth	-1.52*	-1.65*	-1.31	-1.59*	-1.61*	-1.18
	(0.900)	(0.950)	(0.931)	(0.960)	(0.992)	(0.987)
Peg (t-1)				0.00696	0.00714	0.0150
				(0.0216)	(0.0225)	(0.0241)
KAOPEN (t-1)				-0.0296*	-0.0307*	-0.0385**
				(0.0165)	(0.0163)	(0.0178)
Inflation				0.0928	0.127	0.113
				(0.125)	(0.130)	(0.135)
Base GDP growth				-0.00113	-0.000321	-0.00791*
				(0.00414)	(0.00410)	(0.00434)
World GDP growth				0.00892	0.00824	0.0160*
				(0.00708)	(0.00706)	(0.00879)
Obs.	2493	2423	2134	2490	2420	2132
No. of Countries	104	104	92	104	104	92
Kleibergen-Paap rk LM Statistic	39.1***	41.3***	33.6***	29.2***	28.4***	29.4***
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Kleibergen-Paap rk Wald F Stat.	16.6	17.5	14.2	10.8	10.3	10.5
Hansen J Statistic	3.35	4.40	4.00	1.60	2.89	1.96
	[0.341]	[0.221]	[0.262]	[0.659]	[0.410]	[0.581]

Notes: The dependent variable is GDP growth in the first-stage and the civil conflict indicator in the second-stage. Robust standard errors clustered at the country level are included in parentheses. Asterisks denote statistical significance at the 90% (*), 95% (**), and 99% (***) levels. All regressions are run with country fixed effects and country-specific time trends (estimates omitted to conserve space). P-values for the null hypotheses of underidentification and instrument exogeneity are given in brackets underneath the Kleibergen-Paap rk LM and Hansen J statistics, respectively

Table 7 –Ethnolinguistic Fractionalization Interaction Effects with Controls

	(1)	(2)	(3)	(4)
GDP Growth	0.963	0.899	1.18	1.13
	(1.202)	(1.17)	(1.57)	(1.44)
Ethnolinguistic Fractionalization × GDP growth	-5.01*	-4.38*	-5.03*	-4.33
	(2.73)	(2.79)	(2.85)	(2.86)
KAOPEN (t-1)		-0.0271		-0.0289
		(0.0181)		(0.0181)
Peg (t-1)		-0.0174		-0.0176
		(0.0285)		(0.0283)
Inflation		0.0101		0.0129
		(0.148)		(0.148)
Base GDP growth			-0.00560	-0.00586
			(0.00373)	(0.00366)
World GDP growth			0.00415	0.00368
			(0.00833)	(0.00752)
Obs.	1938	1936	1938	1936
No. of Countries	93	93	93	93
Kleibergen-Paap rk LM Statistic	18.9***	23.0***	20.0***	23.2***
	[0.0086]	[0.0017]	[0.0056]	[0.0016]
Kleibergen-Paap rk Wald F Statistic	3.51	5.36	3.56	4.65
Hansen J Statistic	5.87	7.03	6.19	7.25
	[0.437]	[0.319]	[0.402]	[0.299]

Notes: The dependent variable is the civil conflict indicator variable. The instrumental variables are base interest rates at time *t*-1, their full interaction with variables of the trilemma, and their subsequent interaction with ethnolinguistic fractionalization. Robust standard errors clustered at the country level are included in parentheses. Asterisks denote statistical significance at the 90% (*), 95% (**), and 99% (***) levels. Estimations of country-specific time trends are included but not reported to conserve space. P-values for the null hypotheses of underidentification and instrument exogeneity are given in brackets underneath the Kleibergen-Paap rk LM and Hansen J statistics, respectively.

Table 8 –Ethnolinguistic Fractionalization Effects and Controls over three Sub-Samples

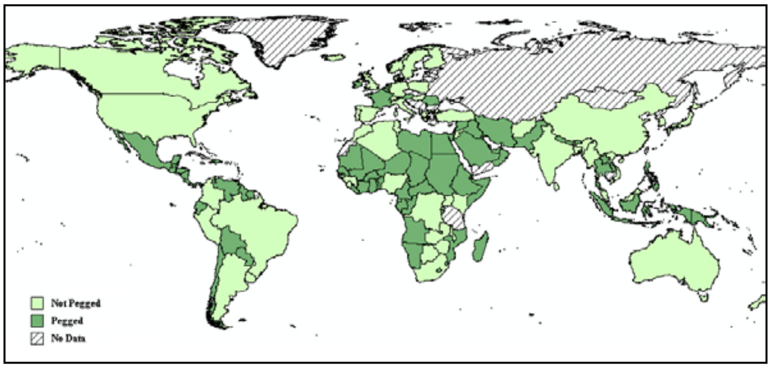
	(1) No Large Crises	(2) No Crises	(3) No Large Countries	(4) No Large Crises	(5) No Crises	(6) No Large Countries
GDP Growth	0.973	1.46	0.699	1.13	1.78	0.681
	(1.21)	(1.11)	(1.19)	(1.44)	(1.29)	(1.40)
Ethnolinguistic Fractionalization × GDP growth	-5.01*	-6.09**	-4.14*	-4.33	-5.43*	-2.89
	(2.73)	(2.74)	(2.54)	(2.87)	(2.89)	(2.64)
Peg (t-1)				-0.0174	-0.0179	-0.0109
				(0.0284)	(0.0296)	(0.0322)
KAOPEN (t-1)				-0.0287	-0.0296*	-0.0368*
				(0.0181)	(0.0176)	(0.0199)
Inflation				0.0154	0.0333	0.0181
				(0.148)	(0.159)	(0.157)
Base GDP growth				-0.00590	-0.00580	-0.0127***
				(0.00368)	(0.00368)	(0.00356)
World GDP growth				0.00384	0.00240	0.0134
				(0.00747)	(0.00738)	(0.00843)
Obs.	1935	1870	1644	1933	1868	1643
No. of Countries	93	92	81	93	92	81
Kleibergen-Paap rk LM Statistic	18.9***	18.6***	17.6	23.2***	22.9***	21.8***
	[0.0086]	[0.0097]	[0.0140]	[0.0016]	[0.0018]	[0.0027]
Kleibergen-Paap rk Wald F Statistic	3.51	3.26	3.46	4.64	4.35	4.02
Hansen J Statistic	5.88	5.05	5.17	7.23	6.19	7.24
	[0.437]	[0.538]	[0.523]	[0.300]	[0.403]	[0.299]

Notes: The dependent variable is the civil conflict indicator variable. The instrumental variables are base interest rates at time *t*-1, their full interaction with variables of the trilemma, and their subsequent interaction with ethnolinguistic fractionalization. Robust standard errors clustered at the country level are included in parentheses. Asterisks denote statistical significance at the 90% (*), 95% (**), and 99% (***) levels. Estimations of country-specific time trends are included but not reported to conserve space. P-values for the null hypotheses of underidentification and instrument exogeneity are given in brackets underneath the Kleibergen-Paap rk LM and Hansen J statistics, respectively.

Appendix

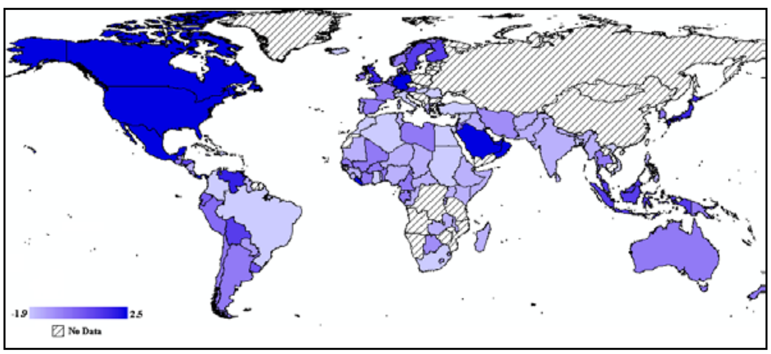
Figure 1: Civil Conflict and Open Economy Trilemma

Panel A – Exchange Rate Regime Types: 1980



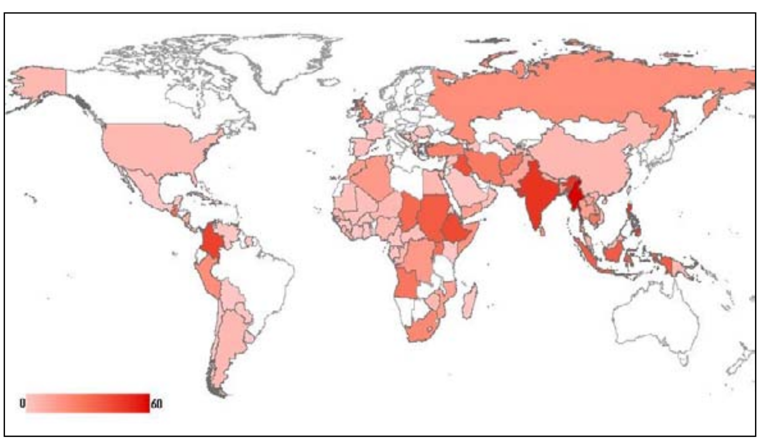
Notes: Coloring represents the 1980 classification of exchange rate regimes from Shambaugh (2004). See section IIIB for a detailed description of this variable.

Panel B – Capital Account Openness: 1980



Coloring represents the 1980 Chinn-Ito KAOPEN score of capital account openness, where higher values denote a more liberalized capital account. See section IIIC for a detailed description of this variable. Source: Chinn and Ito (2008)

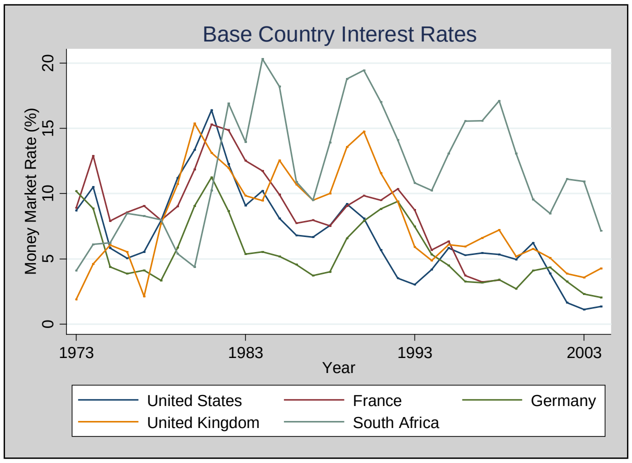
Panel C – The Global Presence of Civil Conflict: 1946-2007



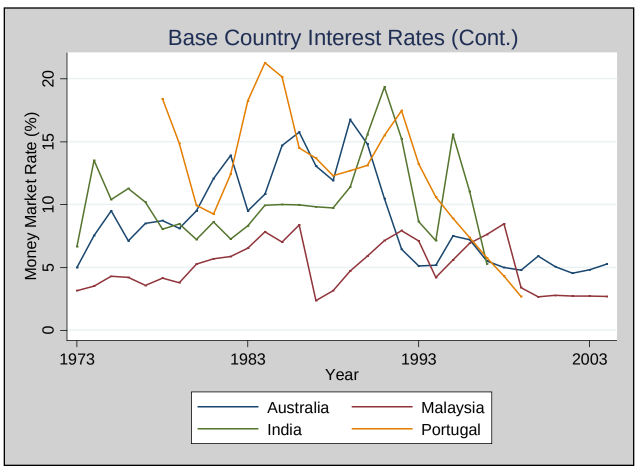
Coloring represents the number of years from 1946-2006 a given country has seen internal armed conflict (greater than 25 conflict-related fatalities in a year) within its borders. Source: PRIO/Uppsala, Gleditsch et al. (2002)

Figure 2 – Variation Across and Within Base Country Monetary Policies

Panel A

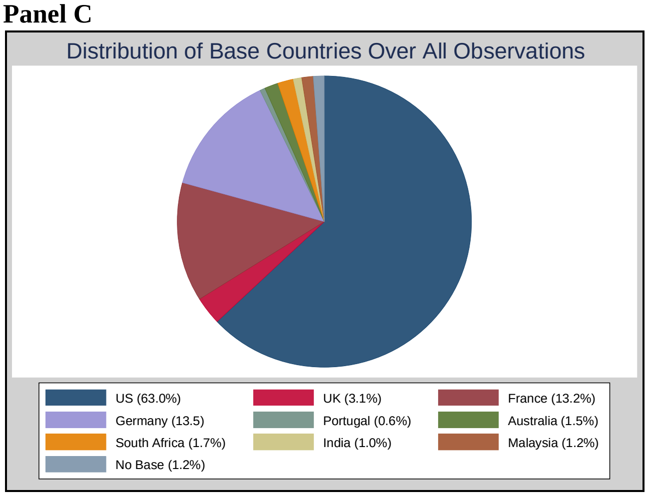


Panel B



Source: IMF International Financial Statistics Database.

Panel C



Source: Shambaugh (2004)

Table A1 – Sampled Countries, with Base Countries and Total Years Engaged in Civil Conflict

Name	Obs.	C.	Name	Obs.	C.	Name	Obs.	C.
Algeria[2]	29	13	Guatemala[7]	31	22	Niger[2]	30	4
Argentina[7]	11	1	Guinea-Bissau[2,6]	9	1	Nigeria[7]	24	1
Australia[7]	32	0	Guyana[7,8]	10	0	Norway[3]	31	0
Austria[3]	32	0	Haiti[7]	29	3	Pakistan[7]	32	8
Azerbaijan[7]	9	0	Honduras[7]	32	0	Papua New Guinea[8]	29	6
Bangladesh[7,8]	18	6	India[7,8]	31	26	Paraguay[7]	27	0
Belgium[3]	32	0	Indonesia[7]	28	22	Peru[7]	16	7
Bolivia[7]	22	0	Iran[7]	32	19	Philippines[7]	28	28
Botswana[7,9]	30	0	Ireland[3,8]	32	0	Portugal[3]	31	0
Bulgaria[3,7]	7	0	Israel[7]	22	22	Rwanda[7]	29	9
Burkina Faso[2]	30	1	Italy[3]	31	0	Saudi Arabia[7]	30	1
Burundi[7]	31	12	Jamaica[7]	27	0	Senegal[2]	29	9
Cameroon[2]	31	1	Japan[7]	31	0	Sierra Leone[7,8]	20	7
Canada[7]	32	0	Jordan[7]	26	0	Singapore[5]	32	0
Cape Verde[6]	20	0	Kasakhstan[7]	8	0	Solomon Islands[8]	22	0
Chile[7]	24	0	Kenya[7]	29	1	South Africa[7]	30	14
China[7]	18	0	Korea[7]	30	0	Spain[3]	30	5
Columbia[7]	29	29	Kuwait[7]	25	0	Sri Lanka[4,7,8]	30	18
Costa Rica[7]	31	0	Laos[7]	12	1	Sudan[7]	23	14
Cote D'Ivoire[2]	28	3	Lebanon[7]	2	0	Suriname[7]	21	2
Cyprus[2]	28	0	Macedonia[3]	10	1	Swaziland[9]	30	0
Denmark[3]	32	0	Madagascar[2]	27	0	Sweden[3]	30	0
Dominican Rep.[7]	27	0	Malawi[7]	21	0	Switzerland[3]	32	0
Ecuador[7]	27	0	Malaysia[7]	30	3	Thailand[7]	29	11
Egypt[7]	28	6	Malta[2]	32	0	Togo[2]	29	2
El Salvador[7]	30	11	Mauritania[2,7]	17	0	Trinidad and Tobago[7,8]	30	1
Ethiopia[7]	22	21	Mexico[7]	21	1	Tunisia[2]	20	0
Fiji[7,8]	31	0	Moldova[7]	8	0	Turkey[7]	13	6
Finland[3]	30	0	Mongolia[7]	10	0	Ukraine[7]	7	0
France[3]	32	0	Morocco[2]	29	12	United Kingdom[3]	32	20
Gabon[2]	29	0	Mozambique[7]	14	4	Uruguay[7]	12	0
Gambia[7,8]	31	1	Myanmar[7]	30	29	Venezuela[7]	24	1
Georgia[7]	8	1	Nepal[4,7]	29	9	Vietnam[7]	8	0
Ghana[7]	21	0	Netherlands[3]	32	0	Yemen[7]	12	1
Greece[3,7]	31	0	New Zealand[1]	32	0	Zimbabwe[7]	22	7

Table A1 (continued)

Australia (1)	India (4)	United States (7)
France (2)	Malaysia (5)	United Kingdom (8)
Germany (3)	Portugal (6)	South Africa (9)

Notes: Superscript refers to base country, as reported by di Giovanni and Shambaugh (2008). A country may have multiple bases over the sample period of 1973-2004. The United States is the only country without a base, and is thus excluded. "Obs." denotes the number of observations (years) for which conflict data are available for each country, and "C." denotes the number of years for which the country is listed by PRIO/Uppsala as engaging in civil conflict.

Table A2 – List of Countries with GDP >10% that of their Base Country

Austria	Italy	Singapore
Belgium	Japan	Spain
China	Myanmar	Sweden
France	Netherlands	Switzerland
India	New Zealand	United Kingdom

Source: World Bank WDI database