Journal of Development Economics 103 (2013) 77–89

Contents lists available at SciVerse ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Economic shocks and civil conflict: Evidence from foreign interest rate movements ☆

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ARTICLE INFO

Article history:
Received 13 October 2010
Received in revised 5 August 2012
Accepted 2 February 2013

JEL classification:
D74
F41
F43

Keywords:
Civil conflict
Open economy
Ethnolinguistic fragmentation

ABSTRACT

We exploit annual variation in influential foreign interest rates to identify externally-driven components of short-run income shocks in small open economies from 1971 to 2004 and explore the statistical nature of the income–civil conflict nexus. Our results show that movements in foreign interest rates have important effects on civil conflict risk through domestic economic channels. More importantly, the income–conflict relationship is found to be nonlinear – the conflict risk of ethnolinguistically fragmented countries is found to be much more sensitive to shifts in economic conditions than that of homogeneous countries. 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.

1. 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 1000 lives within its borders, and twenty percent of the world has seen at least ten (Blattman and Miguel, 2010). Internal war has steadily surpassed the destructive legacy of international war, claiming over 16.2 million casualties between 1945 and 1999 – five times as many as the number of lives lost in comparable conflict between states (Fearon and Laitin, 2003). Ultimately, these figures may greatly understate the human cost; off the battlefield, the long-term effects of disease, disability, and social fragmentation that result indirectly from civil war extend well past the arrival of peace (Ghobarah et al., 2003). Beyond the social burden, the economic costs of civil conflict are similarly high. Immediate consequences – 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 development (Abadie and Gardeazabal, 2003; Collier, 2007; Sandler, 2000).

Although the economic and social toll of civil war is widely-recognized, its underlying causes remain elusive. In theory, local short-run economic conditions may affect the likelihood of civil war through changes to the potential warrior's opportunity costs of fighting (which include foregone non-conflict income) as shown by Grossman (1991), Dal Bó and Dal Bó (2011), and Chassang and Padró-i-Miquel (2009)[2], yet the body of empirical literature supporting such predictions with credible and robust evidence is still young. Earlier cross-country works of Collier and Hoeffler (2004) and Fearon and Laitin (2003), though finding strong negative correlations between economic conditions and the incidence of civil conflict, likely suffer from omitted variable and endogeneity biases. More recent empirical studies make use of plausibly exogenous shocks to economic growth (e.g., rainfall, and terms of trade) in an attempt to find cleaner estimates of the income–conflict relationship and also to explore the extent to which it is affected by institutional

☆ We thank Rosa Hayes for the excellent research assistance, Jim McGuire for helping locate some of the data, and the participants in a workshop at the Federal Reserve Bank of Atlanta, seminars at Hitotsubashi University, Keio University, Colby College, and the Institute of Development Economics-Japan External Trade Organization, and the anonymous referees for their useful comments. We gratefully acknowledge the financial support of the Economics Department (Imai) and Quantitative Analysis Center (Hull) of Wesleyan University.

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[2] The implication of this theory is particularly relevant to economic development – 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" (Collier, 2007).

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http://dx.doi.org/10.1016/j.jdeveco.2013.02.001

quality and other socio-political factors (Besley and Persson, 2009; Brückner and Ciccone, 2010; Miguel et al., 2004)[3].

This paper revisits these issues by introducing a new identification strategy to test the causal links between economic growth, civil conflict, and the socio-political setting. Specifically, we borrow from a large literature of open economy macroeconomics the finding that when capital is internationally mobile, small open economies are highly sensitive to interest rate fluctuations in large influential economies as they explicitly or implicitly peg their currencies to the base currency of their choice, thereby giving up their monetary independence (e.g., Aizenman et al., 2008; Borensztein et al., 2001; Calvo and Reinhart, 2002; di Giovanni and Shambaugh, 2008; Hausmann et al., 2001). In the most extreme case, the literature on the open-economy "trilemma" suggests that a country facing a completely open capital account and a fixed exchange rate must align its monetary policy to exactly match that of its base country (Frankel et al., 2004).We thus identify economic fluctuations in small open economies by interest rate movement in their base countries and its interaction with measures of the domestic exchange rate regime and capital account openness. The main identifying assumption for our purpose is that interest rates in influential countries (that is, countries to which currencies are typically pegged, such as the United States, France, and Germany) are determined exogenously to the domestic economic and political conditions of small developing countries at risk of internal conflict[4].

Our identification strategy is similar in spirit to that of two closely related papers: Miguel et al. (2004) and Brückner and Ciccone (2010), which use rainfall and terms of trade shocks, respectively, as sources of exogenous variation in identifying the economic performance of sub-Saharan African countries. Our approach complements these papers and offers some additional benefits. One of the (non-) findings in Miguel et al. (2004) is that political, social, or geographical conditions do not seem to affect the propensity of a country to fall into civil war in response to negative economic shocks. These results notwithstanding, it is important to remember that in order to obtain strong identification with rainfall variation, Miguel et al. (2004) restrict their focus to sub-Saharan African countries where weather patterns have significant effects on economic performance. However, the exclusion of countries outside of sub-Saharan Africa reduces both sample size and the cross-country variation in these socio-political factors necessary to find meaningful nonlinearities in the income–conflict relationship. Since the movements in base country interest rates have been shown to exert powerful and broad effects on small open economies around the globe, we can expand our sample beyond sub-Saharan Africa. Our data set, thus, contains much richer variation in the institutional characteristics across countries, which, in turn, enables us to carry out more powerful tests on whether country-specific institutional characteristics amplify (or moderate) the effects of economic shocks on conflict risk[5]. In addition, examining the effects of economic conditions on civil conflicts through a different set of instruments is in itself a valuable exercise, given that the robustness of previous results has been questioned[6].

We find three notable results. First, our first-stage results reproduce the estimations of the relationship between base country interest rates and output growth that characterizes the literature of open economy macroeconomics; i.e., we show that a significant part of economic fluctuations in small open economies can be explained by base country interest rates and the interaction of base rates with domestic exchange rate regimes and capital account openness. Second, as with Miguel et al. (2004) and Brückner and Ciccone (2010), our second-stage results show that the estimated effect of domestic income shocks on the probability of civil violence is statistically significant and of a sizeable magnitude. Our most conservative estimates suggest that a negative shock in GDP growth by four percentage points in a given year (not uncommon in developing countries) increases the probability of civil conflict by approximately six percentage points. As the sample statistics suggest that in each year a country has on average an unconditional 16.7% chance of experiencing civil conflict (Table 1), this represents an increase in conflict risk by thirty percent. While this estimate is smaller in magnitude than those obtained from their sample of sub-Saharan African countries, it supports the qualitative conclusions of Miguel et al. (2004) and Brückner and Ciccone (2010) that negative short-run economic shocks elevate local conflict risk.

Lastly, we find one result that differs from previous papers. Miguel et al. (2004) do not find any nonlinearities in the conflict–income nexus; that is, political, social, or geographical conditions do not seem to amplify or moderate the effects of economic shocks on the propensity of a country to fall into civil conflict. Although we find no statistically significant evidence supporting the relevancy of religious diversity, reliance on oil or other natural resource exports, political institutional quality, or country terrain, we show that higher levels of ethnolinguistic diversity make a country more conflict-prone when its economy suffers a recession. This estimated nonlinearity is economically important. For a country at the 25th percentile of global ethnolinguistic fragmentation (such as Venezuela or Greece), a sudden decrease in GDP growth by one percentage point increases the probability of civil conflict by only a tenth of a percentage point. However, for a country at the 75th percentile (such as Kazakhstan or Ethiopia) the same economic shock increases the probability of internal violence by an average of 3.88 percentage points (23.2% from the typical annual likelihood of conflict). These results support the claim that slowed economic development may widen pre-existing ethnic rifts in countries with particularly fractionalized social institutions (Fearon, 2007).

The remainder of this paper is organized as follows. Section 2 discusses and summarizes the data used in this paper, and Section 3 describes in detail the empirical strategy. Section 4 provides a discussion of the main results and summarizes the findings from extensions that consider various socio-political and geographic factors. Section 5 describes the results of various robustness checks and their implications for both the main results and their extensions. Section 6 provides concluding remarks, with a brief case study presented in Section 7.

2. Data

An annual panel dataset consisting of 97 countries from 1971 to 2004 is constructed from a variety of sources. Variables of primary interest in the baseline model 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

[3] Recent empirical work also includes studies that utilize more disaggregated microeconomic data. Benmelech et al. (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, and 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.

[4] We briefly describe the case of Niger, a country with a strict currency peg to the French Franc, which fell into a series of violent civil conflicts shortly after Banque de France sharply raised its policy rate in the late 1980s in an attempt to match that of the Bundesbank. See Section 7.

[5] A paper that is closely related to ours in this respect is Nunn and Qian (2010) who examine the impact of the US food aid on civil conflicts with global sample of developing countries. They show that ethnic fractionalization is an important mediating variable; i.e., food aid causes fewer conflicts in ethnically homogeneous countries.

[6] See discussion of relevant econometric issues in Blattman and Miguel (2010), Ciccone (2011), Bazzi and Blattman (2011), and Miguel and Satyanath (2011). Notably, Miguel and Satyanath (2011) find sub-Sarahan African growth to be less sensitive to rainfall shocks after 1999 and emphasize the need for alternative identification strategies.

Table 1
Descriptive statistics.

	Mean	St. dev.	Min.	Max.	Obs.
Conflict	0.167	0.373	0	1	2698
Between countries		0.257			99
Within countries		0.262			
War	0.0497	0.217	0	1	2698
Between countries		0.118			99
Within countries		0.181			
Onset	0.0315	0.175	0	1	2321
Between countries		0.0709			97
Within countries		0.167			
Duration	0.838	0.369	0	1	433
Between countries		0.423			49
Within countries		0.277			
Conflict count	6.64	5.52	0	17	2792
Between countries		4.89			101
Within countries		2.48			
Peg	0.464	0.499	0	1	2721
Between countries		0.297			100
Within countries		0.395			
KAOPEN	−0.0162	1.44	−1.81	2.54	2675
Between countries		1.87			100
Within countries		0.884			
Base interest rate	0.0656	0.0348	0.0101	0.213	2668
Between countries		0.0164			99
Within countries		0.0318			
GDP growth	0.0383	0.0444	−0.190	0.199	2721
Between countries		0.0181			100
Within countries		0.0411			
Inflation	0.100	0.0944	−0.217	0.497	2721
Between countries		0.0552			100
Within countries		0.0791			
ETHFRAC	0.408	0.291	0.00412	0.892	2598
Between countries		0.282			95
RELFRAC	0.347	0.218	0	0.775	2598
Between countries		0.222			95
POLITY	2.20	7.32	−10.0	10.0	2596
Between countries		6.21			94
Within countries		3.82			
Log(mountainous)	2.17	1.47	0	4.32	2598
Between countries		1.48			95
Oil-exporting country	0.168	0.374	0	1	2598
Between countries		0.346			95
Within countries		0.141			
Primary exports/GNP	0.135	0.123	0	1	2483
Between countries		0.128			82
Within countries		0.0246			

Notes: Data are from the global sample, 1971–2004. Some social and geographic variables are time-invariant and thus do not vary within countries.

suggested by Fearon and Laitin (2003). In selecting all of these variables, we follow closely the work of di Giovanni and Shambaugh (2008), Miguel et al. (2004), and others who provide a detailed description of the various benefits and shortcomings of alternatives.

For the incidence of intranational violence, we follow Miguel et al. (2004) and Brückner and Ciccone (2010) in using the Armed Conflict Database of the International Peace Research Institute of Oslo, Norway and the University of Uppsala, Sweden (PRIO/Uppsala)[7]. PRIO/Uppsala 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, coded as one if a civil conflict against the state with at least 25 battle deaths per year is ongoing in country i in year t, is denoted $Conflict_{it}$[8]. By this classification, 16.7% of country-year observations in our sample saw civil conflict from 1971 to 2004[9].

To classify exchange rate regimes, we closely follow the methodology of Shambaugh (2004) and di Giovanni and Shambaugh (2008) in using a *de facto* classification as the official *de jure* exchange rate policy can be quite misleading[10]. Following common definitions in the literature (e.g. Obstfeld and Rogoff, 1995), a country is designated pegged if its official nominal exchange rate remains within ±2% bands over a given year against its base country[11]. 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 (see di Giovanni and Shambaugh, 2008). The resulting measure, Peg_{it}, is a binary variable set equal to one if country i in time t is characterized as pursuing a *de facto* fixed exchange rate and zero otherwise. Approximately 46% of country-time observations in this paper's global sample follow a pegged exchange rate. 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) based on standard principle component analysis of reversed values of the four IMF AEREAR dummy variables[12].

Annual money market interest rate data are collected from the IMF's International Financial Statistics database. 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 most relevant base country. Fig. 1 plots the movement of a selection of the rates of various base countries over time.

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. We also gather inflation rate data from the WDI to control for the general direction of domestic macroeconomic policy. 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 et al. (2004). We take an ethnolinguistic fractionalization variable ($ethfrac_{it}$) from the Soviet ethnographic index *Atlas Narodov Mira* that measures the probability that two randomly selected individuals in a country will belong to different ethnolinguistic groups[13], a measure of religious fractionalization ($relfrac_{it}$) from the *CIA Factbook*, a variable capturing the presence of democratic institutions ($polity_{it}$) from the standard Polity IV data set, the logged proportion of a country categorized as mountainous by geographer A.J. Gerard (taken from Fearon and Laitin, 2003), a binary variable 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, and from Sachs and Warner

[7] We use the latest version of PRIO database (version 4, 2010). We confirm the results are largely consistent across versions, although we omit these results to conserve space.

[8] Our focus is the incidence of internal conflicts. Nonetheless, to be certain that our results are not driven by larger-scale international conflict, we also control for incidence of extraterritorial war, whose data is also available in PRIO/Uppsala. We confirm that our results are not affected; these results are not reported to conserve space.

[9] This proportion is slightly smaller than that found in previous research that uses the same PRIO/Uppsala database. This discrepancy is due to both the inclusion in this paper of countries where conflict is rarer (such as relatively more developed countries and countries outside of sub-Saharan Africa) and the consideration of years where conflict was less common (prior to 1980 and after 1999). Econometrically, our identification strategy, dependent on large variation in base interest rate movements, necessitates the inclusion of these generally low interest rate periods (see Fig. 1).

[10] See Quinn and Toyoda (2008), Shambaugh (2004), and Eichengreen and Razo-Garcia (2011) for in-depth discussions of exchange rate classifications.

[11] 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.

[12] The literature on financial liberalization emphasizes the difficulty of consistently identifying and quantifying a given level of capital account openness. See Edison et al. (2002) and Eichengreen (2002) for detailed discussion of measurement issue.

[13] Our main results are based on the *Atlas Narodov Mira* to be comparable with Miguel et al. (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.

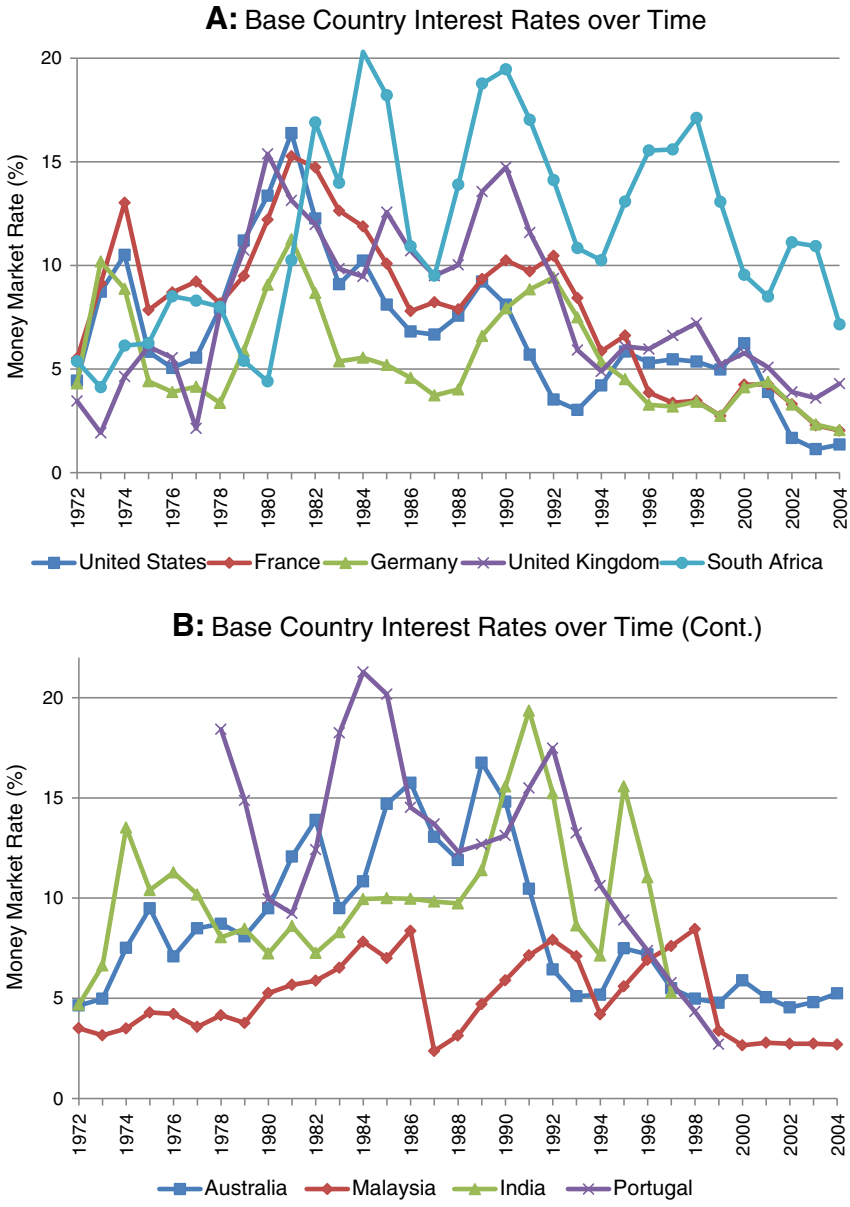

Fig. 1. Variation Across and Within Base Country Monetary Policies. Panel A: Base Country Interest Rates over Time. Panel B: Base Country Interest Rates over Time (Cont.).

(1995) the ratio of primary exports to GNP to capture reliance on natural resource exports more generally.

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 (0.408) in the global sample than in Miguel et al. (2004) (who find an average of 0.65). Additionally, when compared to the limited African sample, this paper's global sample exhibits a 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 0.22 and the Polity IV score for democracy grows in standard deviation from 5.6 to 7.32[14].

After collecting the data, the sample is cleaned in various ways corresponding with the methodology given by di Giovanni and Shambaugh (2008) 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. By using countries that are likely to experience both floating and fixed exchange rate regimes, we avoid the possibility of including pegged countries that are inherently more dependent on base country interest rates for reasons independent of their exchange rate regime choice. 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

[14] In some specifications, which we do not report to conserve space, we focus only on non-OECD countries and also ex-colonies to obtain more homogeneous group of countries and improve our statistical inference. The results turn out to be qualitatively the same with one important exception that the coefficient on the interaction of economic growth with ethnic diversity loses statistical significance. We interpret this as due to the fact that statistical variation in ethnic diversity declines when we use these sub-samples.

are viewed as too small to be representative of the global sample and are dropped[15].

3. Empirical strategy

We follow the open economy macroeconomics literature to estimate a first-stage regression that identifies exogenous components of within-country variation in annual GDP growth. For the second stage, civil conflict is regressed on the instrumented annual growth rate. That is, for all countries in the 1971–2004 sample for which both second- and first-stage data are available, we estimate the following system of linear equations by Two-Stage Least Squares:

$$\begin{aligned} y_{it} = {} & \theta_i + \theta_i^{trend} t + \beta_1 R^b_{i(t-1)} + \beta_2 \left(R^b_{i(t-1)} \times Peg_{i(t-1)} \right) \\ & + \beta_3 \left(R^b_{i(t-1)} \times KAOPEN_{i(t-1)} \right) \\ & + \beta_4 \left(R^b_{i(t-1)} \times Peg_{i(t-1)} \times KAOPEN_{i(t-1)} \right) + X_{it}\phi + v_{it} \end{aligned} \quad (1)$$

$$Conflict_{it} = \gamma_i + \gamma_i^{trend} t + \delta \hat{y}_{it} + X_{it}\psi + \varepsilon_{it} \quad (2)$$

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 the binary *de facto* exchange rate regime variable, $KAOPEN_{it}$ is the Chinn and Ito (2008) measure of capital account openness, $Conflict_{it}$ is the PRIO/Uppsala civil conflict indicator variable, X_{it} is a matrix of country-level controls (which includes the direct effects of *Peg* and *KAOPEN*), and v_{it} and ε_{it} represent error terms[16].

Country-specific intercepts (θ_i and γ_i) are included to control for unobserved time-invariant 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 et al. (2004), country-specific time trends (θ_i^{trend} and γ_i^{trend}) are included to capture additional variation. Finally, estimated standard errors are clustered at the country level to adjust for possible within-country correlation in the error term[17].

The first-stage regression captures two sources of exogenous movement in GDP growth. First, each country's GDP growth correlates with its base country interest rate, thereby generating within-country variation. To the extent that each base rate is imperfectly correlated with other rates (see Fig. 1), 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 more open capital account than others. Since the sensitivity of a small open economy to base country interest rates should depend crucially on exchange rate regime and capital account openness through the open-economy trilemma, heterogeneity in these two variables generates further cross-country variation[18].

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 weak instrumentation that would bias estimates towards the Ordinary Least Squares results (Stock and Yogo, 2005). For each specification, we calculate the Kleibergen and Paap (2006) rk LM and F statistics 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 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). For each specification we include the Hansen J statistic for a standard overidentification test. We address potential lingering endogeneity issues through our choice of second-stage controls in Section 4 and various sub-sample robustness checks in Section 5.

4. Results

4.1. The income–civil conflict nexus

Table 2 reports the main results of estimating Eqs. (1) and (2) as well as the un-instrumented second-stage relationship for comparison[19]. Column 1 reports the simple linear probability model relating contemporaneous economic growth to the incidence of civil conflict with country fixed effects and country-specific trends[20]. The coefficient estimate, although significant at the 95% level, suggests a very small link between economic contraction and internal conflict: a one percentage point drop

[15] di Giovanni and Shambaugh (2008) find that their results 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 to a variety of marginal deviations in these sub-sampling parameters to find virtually no change in results.

[16] We also explore the reduced form relationship between civil conflicts and base country interest rates. Figure A1 displays a panel of figures, showing the mean conflict in the countries pegged to US, France, and Germany, together with the interest rate in these base country. Table A1 shows the results of the reduced form equation in which civil conflict incidence is directly regressed on our first-stage set of instruments. The results confirm that civil conflicts are positively correlated with base country interest rates.

[17] To be specific, we use the command 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.

[18] As a robustness check, we include base country interest rates in the second stage so that the equation is identified only by the interaction of base interest rates and the trilemma configuration. We find that the interactions of the exchange rate peg indicator and capital account openness measure with base country interest rates remain significant at 5 percent error level in these specifications. However, without the direct effects of base country interest rates, our instruments become substantially weaker. We interpret this as reflecting the "fear of floating" – the fact that the central banks of countries whose currency is not strictly pegged to a base currency still tend to match the base country rates in order to maintain open-economy stability (Calvo and Reinhart, 2002; Hausmann et al., 2001). Thus, our estimates are based on both variation from the direct effects of base rates as well as the differential effects that depend on each country's trilemma configuration.

[19] We also check that 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 et al. (2004) and Shambaugh (2004), that the central banks of small open economies tend to follow the base country interest rates and this tendency is particularly strong in pegged regime with open capital account. The results are not reported to conserve space.

[20] 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.

Table 2
Main estimation results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
First-stage												
Base R (t-1)		−0.302*** (0.0403)	−0.228*** (0.0482)	−0.233*** (0.0471)	−0.236*** (0.0468)	−0.228*** (0.0468)	−0.195*** (0.0470)	−0.137*** (0.0517)	−0.202*** (0.0540)	−0.250*** (0.0485)	−0.277*** (0.0534)	−0.228*** (0.0482)
Base R×Peg (t-1)			−0.152** (0.0650)	−0.155** (0.0648)	−0.148** (0.0649)	−0.131* (0.0673)	−0.137** (0.0651)	−0.156** (0.0662)	−0.142** (0.0643)	−0.147** (0.0647)	−0.170** (0.0697)	−0.152** (0.0642)
Peg (t-1)			0.00873* (0.00454)	0.00888* (0.00467)	0.00860* (0.00474)	0.00643 (0.00482)	0.00828* (0.00471)	0.00879* (0.00474)	0.00865* (0.00470)	0.00903* (0.00478)	0.00752 (0.00533)	0.00889* (0.00465)
Base R × KAOPEN (t-1)				−0.0634** (0.0246)	−0.0697*** (0.0239)	−0.0745*** (0.0242)	−0.0700*** (0.0238)	−0.0701*** (0.0232)	−0.0732*** (0.0233)	−0.0670*** (0.0243)	−0.0567** (0.0277)	−0.0663*** (0.0240)
KAOPEN (t-1)				0.00695*** (0.00252)	0.00683*** (0.00252)	0.00565** (0.00265)	0.00679*** (0.00253)	0.00604** (0.00254)	0.00711*** (0.00250)	0.00733*** (0.00245)	0.00525* (0.00300)	0.00615** (0.00257)
Base R×Peg × KAOPEN (t-1)					0.0164 (0.0252)	0.0217 (0.0259)	0.0194 (0.0251)	0.0187 (0.0266)	0.0198 (0.0249)	0.0128 (0.0256)	0.0356 (0.0294)	0.0160 (0.0250)
Inflation						−0.0743*** (0.0200)						
World GDP growth							0.00165* (0.000886)					
Base GDP growth									0.000996 (0.000762)			
Conflict count										0.000846 (0.000552)		
Conflict (t-1)												−0.0119*** (0.00455)
Right											0.00441** (0.00214)	
R^2		0.0415	0.0449	0.0485	0.0488	0.0638	0.0507	0.0138	0.0505	0.0505	0.0553	0.0528
Second-stage												
GDP growth	−0.400** (0.158)	−2.40** (1.08)	−2.07** (0.956)	−1.98** (0.847)	−2.00** (0.846)	−1.99** (0.900)	−2.34** (1.17)	−1.21 (1.52)	−2.03* (1.08)	−1.44* (0.782)	−1.18* (0.710)	−1.42** (0.577)
Peg (t-1)			0.00567 (0.0199)	0.00699 (0.0205)	0.00701 (0.0205)	0.00765 (0.0214)	0.00731 (0.0207)	0.0116 (0.0219)	0.00708 (0.0204)	0.0162 (0.0189)	0.00930 (0.0214)	0.00866 (0.0154)
KAOPEN (t-1)				−0.0337*** (0.0132)	−0.0336** (0.0131)	−0.0329** (0.0136)	−0.0327** (0.0133)	−0.0333*** (0.0128)	−0.0335** (0.0134)	−0.0262** (0.0124)	−0.0402*** (0.0150)	−0.0189** (0.00797)
Inflation						0.0400 (0.133)						
World GDP growth							0.00454 (0.00613)					
Base GDP growth									0.000640 (0.00403)			
Conflict count										0.0133*** (0.00384)		
Conflict (t-1)												0.411*** (0.0594)
Right											0.0133 (0.0157)	
Obs.	2664	2494	2494	2449	2449	2449	2449	2449	2449	2449	2211	2449
No. of countries	98	97	97	97	97	97	97	97	97	97	96	97
Year FE	N	N	N	N	N	N	N	Y	N	N	N	N
Root MSE		0.230	0.226	0.225	0.225	0.225	0.229	0.216	0.225	0.218	0.216	0.201
K-P rk LM statistic		35.8*** [<0.0001]	35.4*** [<0.0001]	33.9*** [<0.0001]	34.6*** [<0.0001]	31.7*** [<0.0001]	27.0*** [<0.0001]	20.8*** [0.0003]	25.8*** [<0.0001]	35.0*** [<0.0001]	38.4*** [<0.0001]	34.9*** [<0.0001]
K-P rk F stat.		56.3	27.5	26.1	20.7	18.6	13.5	9.73	11.6	20.3	18.9	19.4
Hansen J stat.			2.39 [0.122]	2.05 [0.359]	3.11 [0.375]	3.24 [0.356]	3.12 [0.373]	5.11 [0.164]	3.10 [0.377]	3.19 [0.363]	2.79 [0.425]	2.23 [0.526]

Notes: 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.

in output growth correlates with an increased probability of internal violence on average by only 0.40 percentage points.

The first-stage results of columns 2–5 replicate the well-known results of the literature on the open-economy trilemma (e.g. di Giovanni and Shambaugh) and the "fear of floating" (Calvo and Reinhart, 2002; Hausmann et al., 2001) that higher base country interest rates correlate to contractions in domestic output growth rates and that these effects are stronger for countries with a pegged currency and an open capital account[21]. The second-stage results show that the coefficient on

[21] In a separate regression, we also estimate the same specifications with the interaction of private credit to GDP, a measure of financial development, with base country interest rates. We find that the transmission of base country monetary shocks is stronger in more financially developed economies. Since the second-stage results remain virtually unchanged, the results are not reported to conserve space.

instrumented GDP growth is negative and statistically significant, indicating that a one percentage point decline in domestic GDP growth leads to a 2 percentage point increase in the probability of civil conflict. Interestingly, the estimated effect in the instrumental variable results (columns 2–5) is found to be larger than that in a simple OLS estimate (column 1), possibly because the OLS estimates suffer from a large attenuation bias due to measurement error in income growth rates[22].

One possible concern with this paper's instrumentation strategy is that the trilemma configuration consists of policy choice variables (i.e., *Peg* and *KAOPEN*) that could be endogenous to the risk of civil war. As a simple 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. It is worth emphasizing, however, that while the goodness of fit improves with additional instruments in the first stage, the second-stage results are highly robust to the choice of instruments. In particular, we obtain similar estimates of the effects of GDP growth on civil war risk in column 2 where the model relies only on base rates, over which domestic governments in principle have no control. Furthermore, the income–conflict relationship seems robust to second-stage controls for the direct effects of *Peg* and *KAOPEN*, ensuring that the relationship is identified only by their interaction with the plausibly exogenous base country rates. Moreover, if a shift in domestic macroeconomic policy that accompanies civil conflict incidence is driving our results, then our results should be extremely sensitive to the inclusion of a proxy for macroeconomic policy such as inflation rates or budget deficits, but controlling for inflation generates essentially the same results (column 6). These results give some assurance that, although the choice of capital account openness and exchange rate regime is highly relevant for the transmission of foreign interest rate shocks, it is unlikely to drive the main results in the second stage.

We further examine the robustness of these results by adding covariates that may control for unobservable factors that affect the likelihood of civil war (columns 7–12). One concern might be that our results are driven by common global shocks to the world business cycle. To address this issue, we control for the growth rate of world GDP. World GDP growth is positively correlated with domestic economic growth as expected, but it is insignificant in the second stage, leaving our central results unaffected (column 7). That is, there is enough heterogeneity in civil conflict incidence among different countries that cannot be explained by global economic cycle alone. To further explore the possibility that our results are due to unobservable global shocks, we include year fixed effects (column 8). However, the explanatory power of our instrumental variables declines precipitously (as shown by a much smaller first stage R-squared) and the coefficient on GDP growth loses statistical significance in the second stage. This occurs in part because interest rates in the base countries are somewhat synchronized as financial markets are integrated amongst developed economies; that is, we are not able to entirely rule out the possibility that our results are driven by the unobservable correlates of global business cycles due to the weakness of the instrumentation in this setting.

Another issue is that we may expect unobservable shocks that are common to countries that share the same base currency; e.g., countries that use the US dollar as base might have become more entrenched in cold war politics during the 1980s when the US interest rates happened to have spiked due to tight monetary policy and expansionary fiscal policy. To address this concern, we include two time-varying variables that are specific to each base country. First, we simply control for the growth rates of base country's GDP, which proxy for economic shocks common to all countries that share the same base currency. Second, for each group of countries which share the same base currency, we calculate the number of countries with ongoing civil conflict and include it as a control for unobservable common shocks to each group. Base country GDP turns out to be insignificant in both the first- and second-stage results, and does not alter the central findings (column 9)[23]. The number of ongoing conflicts within each group turns out to be highly significant in the second stage (column 10) and it reduces the size of the coefficient on GDP growth. Nonetheless, the results remain significant at 10 percent error level. We further account for domestic politics in the base country (e.g. left vs right party in power) by including an annual indicator variable for right wing government control in each of the 9 base countries (column 11)[24]. The second-stage coefficient on GDP growth remains qualitatively the same, while the coefficient on the right wing government indicator variable is insignificant in the second stage[25].

Finally, we include as a control the lagged incidence of conflict as recent empirical papers have demonstrated that civil conflicts are highly persistent and that controlling for this may lead to starkly different results (Ciccone, 2011; Miguel and Satyanath, 2011; Nunn and Qian, 2012). Confirming the results of these papers, we find that (1) lagged incidence is positively correlated with conflict incidence in the second stage and (2) lagged incidence is negatively correlated with GDP growth in the first stage, suggesting that conflicts impose persistent economic costs (column 12). The average effects of GDP growth in the second stage declines from −2 to −1.4 with lagged incidence (approximately). However, our central results remain statistically significant because the inclusion of lagged incidence improves the goodness of fit in the second stage and significantly reduces standard errors.

It should be noted that the strength of the causal relationship between GDP growth and civil conflict we find is a smaller, though still statistically significant, estimate than that found by Miguel et al. (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 et al. (2004) might be applicable to non-African countries, African countries may have peculiar conditions that make them more vulnerable to civil conflict during economic downturns.

4.2. Socio-political interaction effects

The identification strategy we develop in this paper may be better equipped to test 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 –

[22] This attenuation bias from measurement error in GDP is likely both pervasive and 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 et al. (2009) to seek creative proxies and instruments to GDP growth rates in developing countries that limit the effect of measurement error. Incidentally, Miguel et al. (2004) also find that their OLS estimates are smaller than their IV estimates.

[23] We also control for the total number of ongoing conflicts in the world. Given that these results turned out to be nearly identical to the results with the total number of conflicts within each base country group they are not reported here for space.

[24] We use the Beck et al. (2001) Database of Political Institutions to gather information about the left vs. right wing ideology of ruling governments.

[25] Moreover, we estimate the same regression with only those countries that use US dollar as a base currency. We find no evidence to suggest that the conflict risk of these countries is more sensitive to economic fluctuations, making it unlikely that cold war politics contaminate our results in any significant way.

may make civil conflict more likely. We follow Miguel et al. (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.

In general, as in Miguel et al. (2004), we find that religious fragmentation, lack of democratic political institutions, the presence of rough terrain and a mountainous geography, and the reliance on oil or other natural resource exports are not significant amplifying factors in the income–civil war relationship (Table A2). However, we find that ethnolinguistic fractionalization strengthens the impact of economic growth on civil war risk and that the results are highly robust to the choice of instruments and the inclusion of covariates (Table 3)[26].

These results are quantitatively important. For a country such as Venezuela or Greece at the 25th percentile of ethnolinguistic fragmentation (0.107), a one percentage point decline in domestic GDP growth does not lead to any significant increase in the probability of conflict in all specifications. However, even based on conservative estimates (column 11), for a country such as Kazakhstan, Ethiopia, and Bolivia at the 75th percentile (0.694) the resulting increase in conflict risk by the same shock in internal GDP growth is 2.57 percentage points. For countries at the maximum of the scale of ethnolinguistic fractionalization such as Cameroon, Nigeria, India, Kenya, and Sierra Leone, simple linear 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 5.23 percentage point spike in conflict risk with only a single percentage point fall in GDP growth from the specification of column 5, Table 3, and still a 3.47 percentage point increase in the most conservative estimate (column 11). These represent increases in the typical unconditional probability of conflict by 31.3% and 20.8%, respectively. Thus, consistent with political science literature, a more socially fragmented country appears much more likely to experience internal violence in the face of sudden economic contraction[27].

The results of this section should be interpreted with particular care as the relatively small values of the Kleibergen-Paap rk F statistic bring into question the strength of these instruments. As a weak instrument tends to bias coefficients towards their corresponding Ordinary Least Squares estimates found in column 1 of Table 3 (Angrist and Krueger, 2001), however, it may be the case that the estimates found here (and also in earlier empirical literature) understate the true relationship between domestic economic and socio-political conditions and the probability of civil conflict.

5. Robustness checks

5.1. Sub-sample analysis

We consider a series of sub-sample analysis robustness checks to address the concern that base country interest rates may not satisfy an exclusion restriction. In particular, we test two possible stories by which civil conflicts in a country might have important effects on base country interest rates. First, 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 two channels: first by a "flight to quality" in which international investors seek safer haven in investments in the base country, thereby pushing down base country rates, and second through liquidity injections used by base country central banks to avert their own financial crisis[28]. Additionally, we may expect domestic political conditions to affect base country monetary policy if the local economy is large enough, even without a financial crisis. We address these concerns by dropping country-year observations from the sample that might introduce these kinds of endogeneity. We identify episodes of financial crises from 1971 to 2004 using data compiled by Laeven and Valencia (2008) and re-estimate our findings over data that excludes identified country-year observations. We also 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 whether our results are sensitive to the exclusion of these large economies with and without episodes of financial crisis.

The results of these sub-sample analyses are reported in Table 4. Dropping country-year observations that correspond to financial crises (columns 1 and 2) produces nearly the same estimates[29]. 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 based on a sample of countries with economies no bigger than 10% of that of their base country are given in columns 3 and 4. Dropping large countries similarly leads to little change in the average effects of economic growth on conflict risk (column 3). However, dropping the set of large countries reduces the magnitude of interaction effects (column 4), perhaps because the exclusion of these large countries leads to a more homogeneous sample in terms of ethnolinguistic diversity. Finally, in the most restrictive sample that drops both country-year observations corresponding to financial crises and large domestic economics, we find qualitatively the same estimates as in our main results (columns 5 and 6).

5.2. Alternative measure of civil conflicts

We also check the robustness of our results to the alternative measures of internal conflicts. First, we examine the impact of economic shocks on civil conflict onset and duration separately, since the impact might not be symmetric; e.g., deteriorating economic condition might cause the population to arm themselves and spark civil conflicts as predicted in various theoretical model (e.g., Grossman, 1991), while, once mired in civil conflict, a improvement in economic condition might not alter the political equilibrium (Collier, 2007). For conflict onset, we construct a dependent variable, *Onset*, that equals 0 for years of no conflict, 1 in the first year of conflict, and missing in subsequent years as long as conflict continues. *Onset* becomes zero again when civil conflict ends. Hence, the coefficient on GDP growth in the specification for civil conflict onset captures the effects of economic condition on the likelihood of countries to begin civil conflict but does not incorporate the average effect of continuing conflict. For duration, we construct a dependent variable, *Duration*, that equals 1 if conflict is ongoing, zero when it ends, and missing thereafter. The coefficient on GDP growth for civil conflict duration, hence, represents the impact of economic growth on the likelihood that civil conflict will continue given that it has started.

[26] Notably, although our main results are not statistically significant with year fixed effects, the ethnolinguistic fractionalization interaction results are qualitatively robust to their inclusion (Table 3, column 8).

[27] 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.

[28] 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 to 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).

[29] Although not reported to conserve space, the first-stage coefficients on base country rates and their interaction with the two trilemma variables are similarly found to be practically the same as the full sample results.

Table 3
Ethnolinguistic fractionalization interaction effects.

Second-stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
GDP growth	−0.165	0.685	0.743	0.706	0.836	0.824	1.21	1.78	1.24	1.13	0.599
	(0.275)	(1.48)	(1.52)	(1.32)	(1.26)	(1.23)	(1.57)	(1.71)	(1.41)	(1.25)	(0.892)
ETHFRAC × GDP growth	−0.572	−8.60	−7.93*	−7.12*	−6.80*	−6.80*	−7.01*	−6.33*	−6.90*	−6.46*	−4.56*
	(0.549)	(5.45)	(4.84)	(4.35)	(4.03)	(4.08)	(4.03)	(3.74)	(4.04)	(3.95)	(2.73)
Peg (t-1)			0.00319	0.00345	0.00346	0.00456	0.00309	0.00720	0.00280	0.0130	0.00638
			(0.0217)	(0.0224)	(0.0221)	(0.0236)	(0.0219)	(0.0237)	(0.0218)	(0.0205)	(0.0160)
KAOPEN (t-1)				−0.0304**	−0.0314**	−0.0302**	−0.0321**	−0.0329**	−0.0328**	−0.0236*	−0.0178**
				(0.0141)	(0.0140)	(0.0144)	(0.0138)	(0.0133)	(0.0140)	(0.0132)	(0.00828)
Inflation						0.0644					
						(0.155)					
World GDP growth							−0.00302				
							(0.00616)				
Base GDP growth									−0.00335		
									(0.00370)		
Conflict count										0.0132***	
										(0.00418)	
Conflict (t-1)											0.412***
											(0.0619)
Obs.	2564	2405	2405	2367	2367	2367	2367	2367	2367	2367	2367
No. of countries	94	93	93	93	93	93	93	93	93	93	93
Year FE	N	N	N	N	N	N	N	Y	N	N	N
Root MSE		0.263	0.255	0.247	0.242	0.242	0.240	0.229	0.238	0.234	0.209
K-P rk LM statistic		12.5***	15.6***	17.8**	19.8**	20.5**	24.7***	29.778***	19.3**	20.0**	18.9**
		[0.0004]	[0.0036]	[0.0128]	[0.0192]	[0.0149]	[0.0033]	[0.0005]	[0.0226]	[0.0179]	[0.0260]
K-P rk F stat.		8.97	4.33	4.83	4.13	4.17	5.40	5.13	3.86	4.10	3.66
Hansen J stat.		0.00	3.20	3.87	6.28	6.49	6.42	7.60	6.29	5.89	6.76
		–	[0.361]	[0.695]	[0.616]	[0.592]	[0.601]	[0.473]	[0.615]	[0.660]	[0.562]

Notes: The instrumental variables are those of the corresponding columns of Table 1 and their 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.

We also consider a more stringent indicator of civil conflicts, *War*, which equals 1 if the number of casualties exceeds 1000 people. Both Miguel et al. (2004) and Brückner and Ciccone (2010) show that economic shocks, as captured by rainfall and terms of trade, affect both civil conflicts (more than 25 deaths) and civil war (more than 1000). The results for these alternative measures of civil conflicts

Table 4
Sub-sample robustness checks.

Second-stage	(1)	(2)	(3)	(4)	(5)	(6)
	No crises	No crises	No large countries	No large countries	Both restrictions	Both restrictions
GDP growth	−1.59*	1.71*	−1.52*	0.594	−1.59	1.47
	(0.888)	(1.00)	(0.922)	(1.06)	(0.979)	(0.923)
ETHFRAC × GDP growth		−5.76**		−3.73		−5.04*
		(2.77)		(2.58)		(2.59)
Peg (t-1)	0.00577	0.00491	0.0220	0.0200	0.0179	0.0169
	(0.0160)	(0.0166)	(0.0164)	(0.0172)	(0.0166)	(0.0171)
KAOPEN (t-1)	−0.0192**	−0.0179**	−0.0208**	−0.0199**	−0.0219**	−0.0203*
	(0.00870)	(0.00842)	(0.00958)	(0.00933)	(0.00959)	(0.00907)
Inflation	0.0736	0.118	0.0503	0.0907	0.0719	0.124
	(0.00111)	(0.121)	(0.119)	(0.121)	(0.124)	(0.129)
World GDP growth	0.00464	0.000137	0.0121**	0.00885	0.0117*	0.00701
	(0.00497)	(0.00521)	(0.00609)	(0.00579)	(0.00620)	(0.00583)
Base GDP growth	−0.00152	−0.00385	−0.00746*	−0.00854**	−0.00696*	−0.00848**
	(0.00362)	(0.00341)	(0.00385)	(0.00396)	(0.00382)	(0.00402)
Conflict (t-1)	0.411***	0.424***	0.410***	0.419***	0.413***	0.428***
	(0.0615)	(0.0636)	(0.0626)	(0.0645)	(0.0650)	(0.0671)
Obs.	2380	2299	2137	2055	2070	1989
No. of countries	97	93	88	84	88	84
K-P rk LM statistic	24.5***	21.7***	25.9***	23.9***	24.5***	22.7***
	[0.0001]	[0.0099]	[<0.0001]	[0.0045]	[0.0001]	[0.0069]
K-P rk F stat.	10.6	4.37	9.83	4.28	8.94	4.10
Hansen J stat.	3.29	9.65	2.50	6.48	2.94	8.31
	[0.348]	[0.291]	[0.475]	[0.594]	[0.401]	[0.403]

Notes: The instrumental variables are those from column 11 of Table 1 (for columns 1, 3, and 5) and column 11 of Table 2 (for columns 2, 4, and 6). Columns 1–2 drop country-year observations coinciding with episodes of financial crises from 1971 to 2004 using data compiled by Laeven and Valencia (2008). Columns 3–4 drop "large economies" or, countries whose economic size (measured by real GDP) is reported as greater than 10% the size of their respective base. Columns 5–6 drop both. 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 5
Alternative measures of conflict.

Second-stage	(1)	(2)	(3)	(4)	(5)	(6)
	Onset	Onset	Duration	Duration	War	War
GDP growth	−1.00	0.765	−2.26	−2.28	−0.532	0.129
	(0.716)	(1.01)	(2.19)	(3.35)	(0.534)	(0.673)
ETHFRAC × GDP growth		−3.71		0.841		−1.13
		(2.67)		(5.64)		(1.40)
Peg (t-1)	0.0262**	0.0265**	−0.0691	−0.0629	0.0173***	0.0173***
	(0.0124)	(0.0121)	(0.0628)	(0.0700)	(0.00606)	(0.00623)
KAOPEN (t-1)	−0.00987	−0.00860	−0.0535**	−0.0535**	−0.00758	−0.00765
	(0.00729)	(0.00691)	(0.0257)	(0.0247)	(0.00516)	(0.00520)
Inflation	0.0282	0.0691	−0.0854	−0.0146	0.0504	0.0679
	(0.0838)	(0.0907)	(0.382)	(0.323)	(0.0581)	(0.0548)
World GDP growth	0.00438	0.00132	−0.0188	−0.0194	−0.000179	−0.00129
	(0.00447)	(0.00452)	(0.0220)	(0.0212)	(0.00273)	(0.00272)
Base GDP growth	−0.00264	−0.00331	0.00167	0.00214	0.000490	0.000126
	(0.00323)	(0.00341)	(0.0119)	(0.0113)	(0.00228)	(0.00237)
Conflict/war (t-1)	−0.190***	−0.186***			0.400***	0.397***
	(0.0459)	(0.0490)			(0.0858)	(0.0892)
Obs.	2103	2021	397	397	2449	2367
No. of countries	94	90	34	34	97	93
K-P rk LM statistic	20.2*** [0.0005]	15.6* [0.0756]	7.20 [0.126]	11.3 [0.255]	26.7*** [<0.0001]	22.8*** [0.0066]
K-P rk F stat.	9.12	2.72	2.70	2.04	11.9	4.51
Hansen J stat.	2.29 [0.515]	4.29 [0.830]	7.19* [0.0661]	11.4 [0.182]	3.22 [0.359]	9.75 [0.283]

Notes: The instrumental variables are those from column 11 of Table 1 (for columns 1, 3, and 5) and column 11 of Table 2 (for columns 2, 4, and 6). Onset equals 0 if no conflict, 1 in the first year of conflict, and missing thereafter. Duration equals 1 if there is conflict in both the current and previous year, zero for the first year conflict ends, and missing otherwise. War equals 1 if the number of casualties in conflict exceeds 1000 people. 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.

are shown in Table 5. For both *Onset* and *Duration*, the sign of coefficients are as expected (i.e., civil conflicts are more likely to erupt in a weak economy and they are less likely to end in a weak economy), but they are both insignificant and also not robust to alternative specifications, unlike the simple specification with conflict incidence. More specifically, for *Onset*, the coefficient on GDP growth is negative but smaller than that of incidence and statistically insignificant with a full set of control variables. For *Duration*, the impact of GDP growth is

Table A1
Reduced form specifications.

1Reduced form	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Base R (t-1)	0.765**	0.959***	0.879***	0.906***	0.885***	0.964**	0.894**	0.714**	0.618***	0.659**	0.499*
	(0.331)	(0.374)	(0.341)	(0.337)	(0.342)	(0.410)	(0.405)	(0.331)	(0.232)	(0.288)	(0.277)
Base R×Peg (t-1)		−0.370	−0.313	−0.369	−0.412	−0.354	−0.371	−0.366	−0.211	−0.225	−0.228
		(0.445)	(0.424)	(0.401)	(0.400)	(0.404)	(0.398)	(0.410)	(0.298)	(0.305)	(0.313)
Peg (t-1)		0.0340	0.0331	0.0351	0.0407	0.0347	0.0351	0.0414	0.0246	0.0283	0.0328
		(0.0384)	(0.0379)	(0.0374)	(0.0379)	(0.0375)	(0.0374)	(0.0376)	(0.0292)	(0.0296)	(0.0301)
Base R × KAOPEN (t-1)			0.0631	0.110	0.122	0.109	0.111	0.148	−0.0125	0.00476	0.0301
			(0.235)	(0.246)	(0.249)	(0.246)	(0.249)	(0.246)	(0.164)	(0.167)	(0.167)
KAOPEN (t-1)			−0.0425**	−0.0416**	−0.0386**	−0.0417**	−0.0417**	−0.0345**	−0.0173	−0.0155	−0.0106
			(0.0172)	(0.0170)	(0.0171)	(0.0170)	(0.0170)	(0.0168)	(0.0113)	(0.0114)	(0.0113)
Base R×Peg × KAOPEN (t-1)				−0.122	−0.135	−0.118	−0.123	−0.174	−0.108	−0.118	−0.156
				(0.187)	(0.188)	(0.187)	(0.188)	(0.189)	(0.138)	(0.137)	(0.139)
Inflation					0.191**					0.162**	−0.140*
					(0.0944)					(0.0740)	(0.0731)
World GDP growth						0.00230				0.00503	0.00298
						(0.00560)				(0.00458)	(0.00454)
Base GDP growth							−0.000360			−0.00203	−0.00120
							(0.00359)			(0.00311)	(0.00311)
Conflict count								0.0121***			0.00892***
								(0.00371)			(0.00273)
Conflict (t-1)									0.427***	0.426***	0.422***
									(0.0586)	(0.0585)	(0.0589)
R^2	0.0088	0.0095	0.0216	0.0220	0.0254	0.0222	0.0221	0.0334	0.198	0.200	0.206
Obs.	2449	2449	2449	2449	2449	2449	2449	2449	2449	2449	2449
No. of countries	97	97	97	97	97	97	97	97	97	97	97

Notes: 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.

Table A2
Alternative socio-political interaction effects.

	(1)	(2)	(3)	(4)	(5)	(6)
GDP growth	0.952	−2.04	−1.60*	−0.400	−1.89*	−2.42*
	(1.09)	(1.28)	(0.865)	(0.738)	(1.03)	(1.28)
ETHFRAC	−4.68*					
× GDP growth	(2.75)					
RELFRAC		2.15				
× GDP growth		(1.96)				
POLITY, *t*-1			−0.134			
× GDP growth			(0.0940)			
Log(mountainous)				−0.320		
× GDP growth				(0.261)		
Oil-exporting country					0.884	
× GDP growth					(1.49)	
Primary exports/GNP						4.40
× GDP growth						(2.69)
Peg (t-1)	0.00723	0.0107	0.0100	0.00734	0.0100	0.00794
	(0.0167)	(0.0161)	(0.0160)	(0.0168)	(0.0160)	(0.0166)
KAOPEN (t-1)	−0.0173**	−0.0195**	−0.0201**	−0.0205**	−0.0174**	−0.0163*
	(0.00864)	(0.00896)	(0.00962)	(0.00904)	(0.00836)	(0.00881)
Inflation	0.0872	0.0628	0.0341	0.0438	0.0233	0.0113
	(0.114)	(0.0988)	(0.111)	(0.111)	(0.113)	(0.124)
World GDP growth	0.00151	0.00459	0.00839	0.00373	0.00453	0.00414
	(0.00528)	(0.00492)	(0.00518)	(0.00467)	(0.00510)	(0.00563)
Base GDP growth	−0.00367	−0.00238	−0.00298	−0.00296	−0.00110	−0.00171
	(0.00355)	(0.00366)	(0.00413)	(0.00350)	(0.00433)	(0.00437)
Conflict (t-1)	0.416***	0.415***	0.416***	0.415***	0.409***	0.420***
	(0.0615)	(0.0600)	(0.0610)	(0.0596)	(0.0580)	(0.0624)
Obs.	2367	2367	2367	2367	2367	2241
No. of countries	93	93	93	93	93	79
K-P rk LM statistic	22.4***	26.8***	28.7***	25.9***	32.4***	25.2***
	[0.0077]	[0.0015]	[0.0007]	[0.0021]	[0.0002]	[0.0028]
K-P rk F stat.	4.32	4.65	7.10	5.19	7.25	3.50
Hansen J stat.	6.91	4.67	4.66	8.41	6.96	3.18
	[0.546]	[0.792]	[0.794]	[0.395]	[0.541]	[0.923]

Notes: The instrumental variables are those of column 11, Table 1, and their 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.

larger than with incidence, but its standard error is so large that we could not reject the null of no effect. It should be noted that since we are looking only at the set of countries with ongoing civil conflict, the sample size is much smaller (397, from 2449), greatly reducing the power of statistical test. Similarly, the results for civil war with over 1000 casualties show the expected sign (i.e., civil war tend to occur in a week economy). However, the level of statistical significance is considerably lower, compared to the one achieved when we use civil conflicts with 25 or more casualties.

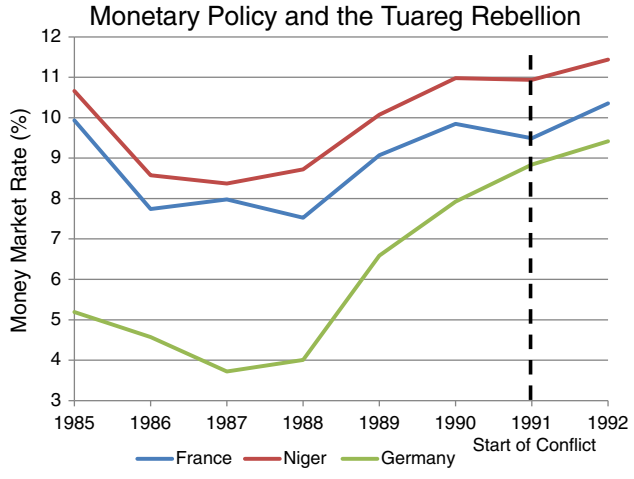

Fig. 2. Transmission of Monetary Policy to Niger, 1985–1992.

6. Conclusions

This paper develops a new identification methodology based on time-series movement in the interest rates of influential economies 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 significantly identify domestic output growth rates and that the instrumented components of growth in turn significantly predict civil conflict risk. 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: we estimate that a negative exogenous growth shock of four percentage points increases the unconditional probability of internal conflict by thirty percent for the typical country in the 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.

Although our results apply globally, these interaction results are particularly relevant for sub-Saharan African countries, most of which are characterized by the highest levels of ethnic diversity. As Easterly and Levine (1997) and 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[30]. 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.

Finally, there remain some open questions that are related to this paper's findings. Although it has been long known that the movement in major economies' interest rates has important economic consequence in small open economies, we show that such shocks can also have important political implications as they may trigger internal violence and threaten the ruling regime. Since we use annual data and primarily concern ourselves with estimating the relationship between economic shock and civil conflicts, we are not able to speak to the dynamic relationship that may operate between base country interest rates, domestic monetary policy, and, ultimately, conflict incidence. Opening up this black box with higher frequency data is of interest as it helps reveal the precise mechanism by which base country's monetary policy can have unintended political consequences in small developing countries.

7. An illustrative case study: Civil war in Niger

The Tuareg Rebellion and subsequent civil war in Niger and Mali in the early 1990s is often cited as an example of a social conflict stemming from poverty and economic deprivation (e.g. Miguel et al., 2004; Mekenkamp et al., 1999). The pastoral Tuareg people, largely politically and economically marginalized by the Nigerien government and military, began to form political opposition groups such as the Popular Front for the Liberation of Niger (FPLN) and the Front for Liberalization of Aïr and Azaouak (FLAA) in the late 1980s. After a failed small-scale attack on a police station in May 1990 by one such group, the Nigerian military led an attempt to crack down on operations of these Tuareg opposition groups. This began a bloody war that only served to further inflame insurgency forces (Amnesty International, 1999; Posthumus, 2000). Total casualties from the resulting eight-year civil war have been estimated between 650 and 1500 (Posthumus, 2000), indicating a significant loss of life and political upheaval.

Although the Tuareg Rebellion is often described as a social conflict, the experience of Niger and the Tuareg people may illustrate the unintended political consequence of foreign monetary policy and the effects of real economic conditions on the willingness of the population to engage in civil war. Niger maintained a strict (within $\pm 1\%$ bounds) exchange rate peg with its base country of France due to its close historical tie, and thus based its monetary policy closely on that of Banque de France. In the late 1980s, Banque de France sharply raised its policy rate and kept it high in the early 1990s to match that of the Bundesbank which, at the same time, was aggressively raising its policy rate, fearing that inflationary pressure was building up from the unification[31]. As a result of geopolitical development in Europe, the domestic interest rates in Niger were rising sharply and Niger's economy was sliding into recession in the late 1980s on the eve of the Tuareg Rebellion (see Fig. 2). Though the post *hoc ergo propter hoc* fallacy is always a concern

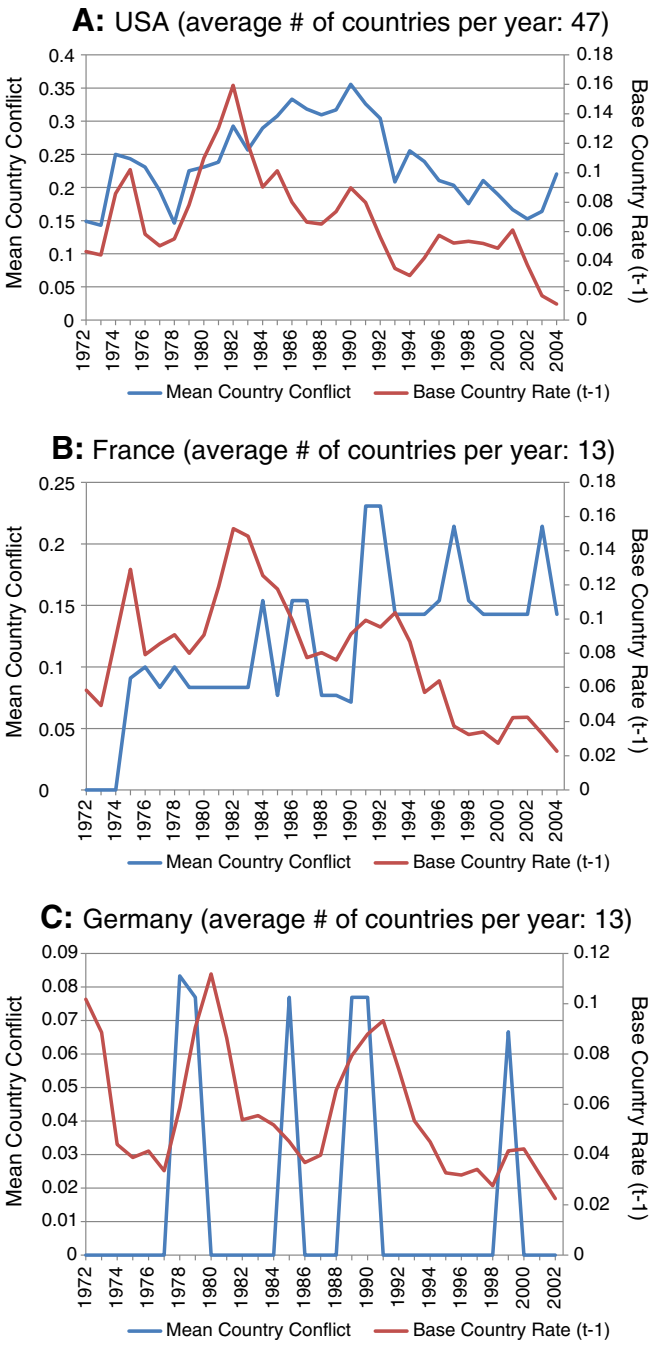

Fig. A1. Reduced Form Conflict-Base Rate Relationship for Major Base Countries. Panel A: USA (average # of countries per year: 47). Panel B: France (average # of countries per year: 13). Panel C: Germany (average # of countries per year: 13).

in any case study, historical accounts and political science research emphasizes that the main instigators of violence within the Tuareg opposition groups were the ishumar (from the French "chomeur," meaning an unemployed person), young former soldiers with poor education and virtually no support from their government who saw participation in rebellion as economically attractive (Azam, 2001; Benjaminsen, 2008). The history of the Tuareg Revolution, therefore, is consistent with the story proposed by this paper. External shocks in monetary policy that caused a sudden decline in real GDP growth in ethnically divided Niger in the early 1990s may have altered the opportunity cost for the typical young unemployed ishumar, motivating him to pursue conflict against the state.

[30] See Alesina et al. (1999), Tornell and Lane (1999) for similar arguments.

[31] Clarida et al. (1998) show that the monetary policy of Banque de France was "much higher than domestic macroeconomic conditions warranted."

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