Trade and civil conflict: Revisiting the cross-country evidence*

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We revisit and expand the evidence on the impact of trade shocks on intra-state conflict by using a large sample of developing countries in the 1960-2010 period. The results suggest that increases in the price of a country's exported commodities raise the country's risk of civil conflict and its duration. The effect on conflict risk is mainly driven by the price of point-source commodities, in line with the rapacity effect theory of conflict. Intense trading with contiguous countries is associated with lower duration of conflict, consistent with the idea that such trade reduces the incentive of these countries to fuel conflict in their neighbor. Trading with neighbors is also associated with a lower risk of conflict, when such trade occurs under trade agreements. On the other hand, we find no support for the opportunity cost theory via exported and imported commodities, nor via the economic cycle in export markets. We also identify a number of conditions under which the changes in the value of exported commodities cease to matter for conflict probability. We use these results to compute the additional conflict risk from hydrocarbon discovery in Lebanon.

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1. Introduction

The question of how international trade shocks affect civil conflict has been at the core of the recent economic research on the determinants of conflict. Several studies have focused on the role of commodity prices both using cross-country (e.g. Brückner and Ciccone, 2010; Bazzi and Blattman, 2014; Besley and Persson, 2008) and within-country analyses (e.g. Dube and Vargas, 2013; Berman et al., 2014). The results of the cross-country literature are mixed and recently Bazzi and Blattman (2014) (B-B henceforth) cast doubts that any effects of commodity price shocks on conflict can be detected empirically altogether through cross-country analyses.[1]

One possible explanation for the lack of evidence is that countries are vulnerable to international shocks to different degrees. Swings in the international price of oil may affect the probability of conflict differently in oil-producers, such as Norway and Nigeria, due to different underlying conditions. Systematic evidence of the conditions under which these external shocks have more influence on conflict is scarce. A better understanding of these conditions could help direct policy interventions to make countries less vulnerable to increases in conflict from trade shocks. In addition, while the focus on commodity prices is important, other possible important sources of trade changes may also impact on civil conflict, such as those induced by trade policy and by the demand in trading partners.

This paper expands the cross-country evidence in various ways along these lines. First, we consider a wider spectrum of trade shocks than in most other studies, by pooling together export and import price variation, changes in demand from trading partners, and the importance of trade with neighboring countries. Second, we modify the modelling of the international commodity price shock relative to some of the previous cross-country literature in a way that is closer to the theoretical

[1] On the other hand there is closer to an empirical consensus on the effect of commodity prices on conflict among the within-country studies, as we detail in section 2.

underpinnings. Finally, the analysis goes beyond the average effects across countries and identifies some of the conditions under which the trade shocks are more likely to spur conflict.

In a significant departure from some of the most recent literature, we find that price swings of exported commodities do matter for the probability of conflict. An exogenous increase in the value of a country's exported commodities raises the probability of conflict eruption. The effect is far from negligible: an increase of 10% in the export value raises the risk of conflict by between 2.2 and 2.5 percentage points. This result is primarily driven by changes in the prices of point-source commodities, in line with the rapacity effect theory of conflict.

Intense trading with neighbors reduces the duration of conflict, as such trade reduces the incentives of contiguous countries to fuel conflict in their neighbors. Importantly, trading with neighbors is also associated with a lower risk of conflict when such trade occurs under regional trade agreements (RTAs), although it is hard to determine causality for this result. On the other hand, neither the prices of imported commodities nor changes in demand in export markets appear to exert any influence on the probability or the duration of conflict.

We also identify a number of conditions under which changes in the value of exported commodities cease to affect the probability of conflict. The existence of grievances and conditions in neighboring countries appear to be particularly important in this respect. We then illustrate how these results can be used to predict the additional risk from hydrocarbon discovery on civil conflict taking the case of likely discovery in Lebanon.

The paper is organized as follows. The next section places the analysis in the context of the empirical literature – particularly across countries – on economics and conflict, highlighting also its main contributions; section 3 describes the data and how the variables are constructed; section 4 details the empirical methodology; section 5 presents the results; and section 6 summarizes the results and reviews the main policy implications.

2. Our analysis and the macro evidence

This paper is most closely related to the cross-country empirical literature on the impact of economic shocks on civil conflict. This is a relatively young but growing literature spurred by earlier seminal works, such as Collier and Hoeffler (1998; 2004), Fearon and Laitin (2003) and Miguel et al. (2004). These analyses provide empirical support for a negative relationship between income levels and shocks on one side, and coups, violence and war on the other. However, the interpretation of these findings differs. Collier and Hoeffler (2004) interpret the negative relationship as confirming the opportunity cost hypothesis, that the cost of recruiting rebels increases with income growth. Fearon and Laitin (2003) argue instead that the result is driven by the strong positive association between state capacity and income. When income is low, the ability of the state to contain possible rebellions is limited.

While these papers have been influential, their cross-country empirical work suffers from a number of drawbacks (Blattman and Miguel, 2010). Importantly, these studies do not fully account for cross-country heterogeneity, nor do they address the likely endogeneity of income (and other economic variables) with respect to conflict. The subsequent literature has tried to address these limitations. In a seminal contribution, Miguel at al. (2004) analyse the impact of income changes on conflict across sub-Saharan African countries, controlling for country fixed effects to capture the time-invariant country heterogeneity. More importantly, they isolate the portion of income changes explained by rainfall variation, which is exogenous to conflict. Their analysis confirms a significant negative effect of income on conflict incidence. As in Africa, changes in income due to rainfall are mainly related to labor-intensive agriculture; this result lends support to the opportunity cost hypothesis.

This work helped trigger an interest in the use of weather shocks as an instrument for income changes or as a direct determinant of conflict. Studies almost invariably find that large deviations from

normal weather patterns increase the probability of conflict (Hsiang and Burke, 2013). This finding is particularly clear in sub-Saharan Africa. Using small geographic cells as the unit of analysis, Harari and La Ferrara (2012) show that negative climate shocks affect conflict incidence in Africa only during the growing season. This is consistent with the effect of the shock channelled via changes in income.[2]

2.1. The evidence on commodity prices

Along with weather, several cross-country empirical studies have examined the impact on conflict of fluctuations in the international prices of exported commodities. This approach, which is more closely related to our work, has so far produced more ambiguous results than that using weather shocks.[3] Cross-country studies find a negative (e.g. Brückner and Ciccone, 2010; Carter and Bates, 2012), a positive (e.g. Besley and Persson, 2008), or no relationship (B-B) between exported commodity prices and conflict.

In particular, B-B argue that the mixed results may be due to a number of important drawbacks typical of this literature, concerning the data, the variables, and the empirical specification used. Their cross-country analysis tries to address these criticisms and their results suggest the lack of any significant relationship between changes in export prices and conflict.

We take these (non) results seriously. B-B deal carefully with several important theoretical and methodological issues, and their work arguably represents the state of the art of cross-country analysis on commodity prices and conflict. This paper builds on B-B both methodologically as well as

[2] This finding is also shared by within-country studies on the determinant of conflict at the local level in Somalia (Maydstadt and Eckers, 2013), Brazil (Hidalgo et al., 2010), and India (Gawande, 2012). And it also applies to cross-country studies using different sources of shock, as the movements in foreign interest rates in relevant partner countries used in Hull and Imai (2013).

[3] In fact there has been some controversy also over the actual effects of weather shocks on conflict, see e.g. Ciccone (2011) and the response by Miguel and Satyanath (2011). However the bulk of the literature seems to have converged on a consensus about the significant effect of climatic shocks on conflict (see Hsiang and Burke, 2013).

theoretically. Importantly, as in B-B we consider different types of commodities, in order to distinguish between different mechanisms whereby commodity price changes affect conflict.[4]

We first distinguish between extractive, point-source export commodities and diffused agricultural ones. The former are usually highly valuable, capital intensive and concentrated, which make them more easily appropriable by the fighting parties. An increase in their value has therefore two competing effects. On the one hand it provides incentives to rebels to fight over their control (the so-called 'rapacity' of 'state prize' effect), since they can use the resources to fund fighting. On the other hand if the state exerts control of the commodities and can tax production, a higher value may imply higher fiscal revenues for the state and thus it may increase its ability to repress or buy-off the rebels (what we call the 'state deterrence' effect). The recent within-country evidence finds a positive coefficient on the prices of point-source commodities suggesting that the rapacity effect dominates the state deterrence effect (Berman et al., 2014; Dube and Vargas, 2013; Maydstadt et al., 2014). While the cross-country evidence is more mixed. Unlike Besley and Burgess (2008), B-B find particularly little support for the idea of the state as a 'prize': in their analysis prices of extractive commodities have a weakly negative effect on the risk and duration of conflict. The authors interpret this finding as providing mild support for the state deterrence mechanism via changes in fiscal revenues. Using a slightly revised methodology, as we explain below, our analysis revisits this evidence, bringing it in line with the within-country evidence.

On the other hand, prices of diffused agricultural commodities should be more linked to the opportunity cost hypothesis, as their production is labor intensive and more difficult for the government to tax. Therefore, positive shocks to these exports should reduce the incentives for conflict. However, revenues from diffused commodities (as well as from mining activities) can also be an important source of funding for rebel groups controlling local areas. For example, in Myanmar the

[4] B-B is the first study we are aware of to distinguish between types of commodities in the cross-country literature. Dal Bó and Dal Bó (2011) introduced this distinction theoretically and Dube and Vargas (2013) employed it at the micro level within Colombia.

production and trade of timber and other agricultural products, as well as of mining products in the bordering areas with Thailand and China, was taxed by local rebel groups fighting the central government.[5] This channel would go in the opposite direction to the opportunity cost channel, and their net effect would be an empirical question. In the case of the Colombian conflict the opportunity cost channel appears to be dominant for crops such as coffee, bananas and other legal agricultural crops (Dube and Vargas, 2013; Berman and Couttenier, 2014), while the rebel funding mechanism is more important for coca production (Angrist and Kugler, 2008).

In addition to generating revenues, commodities can also be significant consumption items, which can be important in determining the impact on conflict. A positive price shock can favor producers but it can also penalize consumers, sparking unrest. Bellemare (2014) provides some support for this relationship by linking monthly spikes in international food prices with increased political unrest worldwide. Our analysis also distinguishes between consumed and non consumed commodities, which to the best of our knowledge is an innovation in the literature.

The macro literature has paid less attention to the impact of shocks in import prices on conflict. Nevertheless, swings in the international prices of imported commodities could affect consumers as much as prices of exported commodities affect producers. As the prices of commodities tend to be correlated, failure to include imported commodities' prices may lead to a bias in the measurement of the effect of the prices of exported commodities. Cross-country studies find some support for the conflict-inducing effects of increases in the international prices of imported food commodities (Arezki and Bruckner, 2011; Besley and Persson, 2008; Maydstadt et al, 2012).[6] In line with these findings, our analysis considers also the impact of the price of imported commodities on conflict. We are also aware of the limitations of this variable in capturing the impact on conflict via consumption, mainly as

[5] This information is based on the report by Kesan (2012) as well as personal communications with the main author of the report.

[6] Besley and Persson (2008) interpret this finding as supportive of a grievance mechanism, i.e. spikes in food prices increases poverty and grievances. This is essentially analogous to the association of a price increase to the reduction in real wage and thus in the opportunity cost of engaging in conflict.

imports make up only a small share of consumed commodities. We return to this limitation of the analysis below.

2.2. Beyond commodity prices

Beyond commodity prices, other trade-related shocks are also potentially relevant for conflict as they can determine substantial changes in individual incomes. These include for example demand shocks in destination markets, changes in a country's own trade policy and that of trading partners, and changes in the geography of trade.

Unfortunately, the evidence on the impact of these trade shocks on conflict is limited. A few studies consider the impact of changes in demand in trading partners, mostly finding that increases in external demand reduce the probability of conflict and its intensity (Bruckner and Ciccone, 2010; Chaudion et al., 2012 and Berman and Couttenier, 2014). This evidence is consistent with the opportunity cost hypothesis. Some evidence also is emerging on the role of trading partners' trade policy in affecting the intensity of conflict (Berman and Couttenier, 2014; Bhavnani and Jha, 2011).

This paper considers a wider range of trade variables than previous studies have done. Along with commodity prices, our analysis also includes changes in trading partners' demand and trade with neighboring countries. This latter variable is particularly relevant to capture the role of neighboring countries in fuelling the conflict at home (Gersovitz and Kriger, 2013; Gleditsch, 2007). This analysis allows us to test if greater trade integration with a country's neighbors reduces the risk of civil war in that country.

2.3. Conditioning factors

The existing macro evidence usually identifies the average effects of external shocks across countries. This average effect is likely to mask substantial heterogeneity in these effects. For example,

diamond trade has been associated with higher levels of conflict in the Central African Republic and in Sierra Leone but not in Botswana, where all groups in the society benefited from the diamonds rents (Robinson et al., 2001).

Understanding this heterogeneity is important for at least three reasons. First, it would help identify some of the conditions under which trade shocks are more important. Second, this knowledge would allow policy makers to identify (and possibly) address the conditions that make countries vulnerable to economic shocks. Third, modelling this heterogeneity across countries may allow for a more robust analysis of whether trade-related shocks affect conflict. That is, analysis which bundles together countries with very different conditions may conclude that trade-related shocks have no impact on conflict, while actually trade-related shocks may be important for conflict, but only in countries that meet certain conditions.

Scholars have acknowledged the importance of this heterogeneity. Blattman and Miguel (2010, p. 31) note that "there is good reason to believe that the relationships between civil conflict and income shocks…should be conditional ones, evident primarily when interacted with other contextual variables". However, the macro studies have rarely tested systematically for these conditioning factors, and no consensus has emerged on their importance.

On the one hand, some empirical evidence points to the fundamental importance of the underlying countries' conditions to explain the impact of income shocks on conflict and stability. Besley and Persson (2008) show that the impact of commodity prices on conflict apply only to countries with weak constraints on executive power. When checks and balances are strong, governments are probably more constrained in the way windfall revenues from commodities' price swings can be spent. This could in turn reduce the potential value of the 'state prize' to be appropriated

through fighting.[7] However, this result may just reflect other structural differences between countries with strong constraints, mostly high-income Western democracies, and countries with weak constraints, mostly developing countries. Other studies provide evidence that the role of ethnic divisions is important in understanding the impact of income shocks on civil conflict (Hull and Imai, 2013) and political risk (Bruckner and Gradstein, 2012).[8]

On the other hand, B-B also find that commodity price changes have no significant impact on conflict when considering only high-risk countries, defined by the type of political system, the level of economic inequality, and the degree of ethnic polarization.[9] Similarly, Miguel et al. (2004) do not find any difference in the impact of weather shocks on civil war between countries differing in terms of democratic rule, ethno-linguistic fractionalization, type of terrain, income per capita, or oil-exporting status. These studies thus conclude that commodity price changes have no significant impact on conflict, even in high-risk countries.

One reason for these different results is that the studies differ in significant details. The analysis in Miguel et al. (2004) is not strictly comparable with the others, as they restrict the sample to sub-Saharan African countries. Importantly, Besley and Persson (2008) and Hull and Imai (2013) use a different way of coding civil wars than B-B (explained below), and Bruckner and Gradstein (2012) examine the impact on political stability rather than on conflict per se. In addition, B-B use various conflict datasets and find a significant effect of the conditioning factors in some specifications, although it is not clear in which ones as they do not report the individual interaction terms.

Equally importantly, many of the conditioning factors tested by the studies are not necessarily the most important to spurring a conflict in the presence of adverse trade shocks. For example, it is not

[7] This argument appears to have less power in the case of rebel. If the rebels can physically take control over resources, then whatever legal arrangements are in place to limit authority for spending the funds would not be relevant.
[8] The former study captures ethnic divisions via the ethno-linguistic fractionalization index, while the latter focuses on ethnic polarization from Montalvo and Reynal-Querol (2005).
[9] In particular, these sub-sets of countries include non democracies, autocracies, regimes with low executive constraints, low-income countries, highly unequal countries, countries with high ethnic polarization and countries in sub-Saharan Africa.

clear whether the distinction between democracies and non democracies should matter in a country's response to external shocks. Miguel et al. (2004) argue that countries with stronger democratic institutions may be better able to negotiate compromises among social groups to avoid unrest. However, the effectiveness of the state's response may vary greatly even within democracies. In addition, authoritarian regimes may use the windfall revenues from positive shocks to strengthen the repression of potential challenges to their authority. In terms of the effect on conflict onset, this would make autocracies similar to effective democracies able to negotiate compromises among social groups.

This paper aims to overcome the limits of previous research in testing for the role of conditioning factors in explaining the relationship between trade shocks and conflict. It combines state of the art empirical methods, which improve on many existing studies, with a selection of theoretically relevant factors that may be expected to affect this relationship.

3. Data and variables

We collect data from different sources. For the conflict data we mainly rely on the Uppsala Conflict Data Programme PRIO.[10] Although there are other sources for conflict data (e.g. Fearon and Laitin, 2003; Sambanis, 2004 and the Correlates of War, Sarkees and Wayman, 2010), PRIO has become the key dataset for cross-country analyses of conflict determinants.

While all sources define a civil war as an internal conflict with at least 1,000 battle deaths, there are significant differences in the data on fatalities and the criteria used to code the onset of wars, what counts as a war, and how to treat breaks in violence. These differences lead to dramatically different civil war variables.[11] We are reluctant to test the relationship between trade shocks and conflict using all sources of conflict data, because the differences among them would likely affect the analysis, and could lead to very different results.

[10] These datasets are available at http://www.prio.no/Data/Armed-Conflict/UCDP-PRIO/.
[11] For example B-B report that in the PRIO dataset major civil wars are coded in 7% of the country-years, compared to 20% of the country-years in the Fearon and Laitin data.

Instead, we follow the same practice as B-B, who base their analysis on the less frequent PRIO and COW measures of conflict. B-B (p.13) assert that these databases are theoretically most relevant to measuring the impact of commodity price changes on conflict , "as they capture the ebb and flow of incentives for war as incomes rise or fall". We use the PRIO dataset as our main source of conflict data, and only use the COW data for robustness, for three main reasons. First, the PRIO dataset has effectively become the standard reference for cross-country studies on the determinants of conflict. The majority of the recent studies we have reviewed use only this dataset (e.g. Nunn and Qian, 2013; Bruckner and Ciccone, 2010; Hull and Imai, 2013; Lin and Michaels, 2011), while the rest use it along with other data sources (e.g., B-B, Besley and Persson, 2008). Second, we have performed independent checks on both datasets and found PRIO to be generally better at identifying civil conflicts than COW.[12] Finally, along with major civil conflicts, PRIO also codes minor conflicts as those above 25 battle deaths per year. We believe these smaller conflicts provide a relevant complement to the more episodic full blown civil conflicts. In fact the problem with estimating the determinants of a rare event as conflict onset strengthens the importance of adding these smaller conflicts to the database).

One issue with the conflict data is that temporary reductions in the yearly number of casualties (i.e. below 1,000) are automatically counted as peace years. For instance, in the Angolan Civil War (1975-2002) the number of fatalities was below 1000 in 1991 and 1995, even though the war was continuing with a large number of yearly casualties. To check the sensitivity of the results to this coding procedure, we also construct an additional conflict variable, which defines peace years in between war years as those with a number of battle related deaths that falls below 300. The results

[12] We first checked the conflict instances that were missing in PRIO according to Gersovitz and Kriger (2013) (i.e. Cameroon, Central African Republic, Congo-Brazzaville, Côte d'Ivoire, Kenya (Shifta War, 1963 to 1967), Mali, Niger and Senegal). All of these conflicts, with the exception of the one in Kenya, have been included in the latest versions of the PRIO dataset while they were still missing in COW. As an additional check, we identified those conflict incidences recorded in COW but missing in the PRIO data (e.g. Liberia in 1996). We find that in various instances, these conflict years had been there in earlier versions of the PRIO data but had been removed in the more recent versions, suggesting a frequent process of updating of the conflict episodes in the PRIO dataset.

presented below for *major conflict onset* are based on this variable although the results are very similar when using the *major conflict onset* from PRIO (results available upon request).

We use various trade variables in the empirical analysis. Two of them (i.e. commodities' export and import prices) rely on a combination of international commodity prices and country-specific trade shares. The former come from data coded by B-B, who combine price data for 65 commodities from various sources, including the IMF International Financial Statistics (IFS), the US Bureau of Labor Statistics (BLS), Global Financial Data (GFD) and additional sources.[13] We complement B-B data with the Commodity Price Data from the World Bank to obtain prices for 73 internationally traded commodities.[14] Using this data we are able to construct the export price index:

$$Px_{it} = ln\left[\left(\prod_{j} P_{jt}^{exp_{ij,t-k}}\right)^{\frac{1}{\sum_j exp_{ij,t-k}}}\right] \qquad (1)$$

where *Px* is defined as the log of the geometric average of the international commodity prices (P_{jt}).[15] Each commodity is weighted by its country i-specific average value of exports between *t-k* and *t-2*, where in our case k takes the value of 12 or of 4 (see below). The lagged structure of the export variable ensures the exogeneity of the weights.[16] In addition, the moving average weights can capture changes in the export structure such as large discoveries of oil or gas, as opposed to static weights (Deaton, 1995). To compute the weights we construct exports and imports series for the 1962-2010 period at 4-digit (SITC Rev. 2) level by combining the NBER-United Nations Trade data with data from UN Comtrade. This standard formulation allows the effects of commodity prices on countries to vary according to the commodity's share in a country's export bundle.

[13] See B-B's web appendix for the complete list of sources.
[14] The list of these commodities is available in a web appendix at the end.
[15] To avoid the effects of different units of measure across commodities, all international commodity prices are normalized to 100 in 2000.
[16] For example commodity exports could react in anticipation of a conflict or as a reaction to changes in international commodity prices themselves, thus making the contemporaneous export shares invalid as weights.

In the baseline specification we use "slow moving weights", defined as the average exports of each commodity from (t-2) to (t-12). Lagged averages of ten years of data ensure a balance between the time invariant weights and "fast moving weights" (t-2 to t-4) used by B-B. This weighting scheme also allows us to control for changes in the export structure without giving excessive weight to changes of the export structure due to temporary shocks.[17]

Two other differences in our index compared to what B-B use are important. First, the index in B-B is based on percentage changes in the commodities' price. In fact, we argue that it is the price level rather than its proportionate change over the previous period that matters most in shaping the incentives to engage in violence. Consider for instance a change in the price of oil for an oil-exporting country. During periods of low international prices, even a large percentage change in price in one period may be associated with a low price level at the end of the period. In this case the value of the oil vulnerable to predation would still be limited, thus keeping the incentives for fighting over its control relatively low.[18] We therefore believe that our price index is more suited to capture changes in the incentives to engage in conflict due to commodity price changes. This approach is also in line with other recent studies, for example Nunn and Qian (2013) and Dube and Vargas (2013). As it turns out, this difference is important for the results.

Second, our baseline index is not scaled by size of the exported commodities compared to GDP. This is, in principle, a desirable property of such an index (B-B reflect the size of exports relative to GDP by multiplying the index by the commodity exports-GDP ratio at the mid-point of the period). Using this scaling does not affect our results in any meaningful way, but it does slightly

[17] One important concern comes from the fact that some countries are large exporters of these commodities and their internal conditions may influence international prices. We address this issue in the next sections.
[18] In the model of Dal Bó and Dal Bó (2011) this would lead to a relatively small size of the appropriative conflict sector, i.e. a low probability of conflict.

reduce the number of observations, as GDP is not available for a few countries (results available upon request).[19]

In addition to the export price indices, we develop a similar country-specific variable for the import sector:

$$PM_{it} = ln\left[\left(\prod_j P_{jt}^{imp_{ij,t-k}}\right)^{\frac{1}{\sum_j imp_{ij,t-k}}}\right] \quad (2)$$

The variable is constructed by using import rather than export data to calculate a weighted average of international commodity prices.

Trade shocks arise from other sources than simply changes in international commodity prices. Demand shocks in partner countries may be as important as price shocks, especially for countries that are not diversified in terms of destination markets. To capture this potential effect, we define a demand shock variable – constructed as a market potential measure - as:

$$MP_{it} = ln\left[\sum_j w_{ij,t-k} GDP_{jt}\right] \quad (3)$$

where we use nominal GDP (normalized to 100 in 2000) from World Bank (2014a) to build an index that is a weighted average of trade partners' GDPs, with each weight *w* being the lagged average share of country j in total exports of country i over the period defined by t-k.

The last trade variable captures the trade relation with neighboring countries and is defined as:

$$TN_{it} = \frac{\sum_j border_{ij} * \left(import_{ijt} + export_{ijt}\right)}{\sum_j \left(import_{ijt} + export_{ijt}\right)} \quad (4)$$

We use the GeoDist dataset from CEPII to define contiguous country pairs. The variable *TradeNeighbors* (*TN*) proxies for the level of integration of country i with its neighbors. We expect

[19] The index scaled by export-GDP ratio is defined as: $Px_{it} = \left(\prod_j P_{jt}^{exp_{ij,t-k}} \times \left(\frac{\sum_j exp_{ij,midpoint}}{GDP_{midpoint}}\right)\right)^{\frac{1}{\sum_j exp_{ij,t-k}}}$

countries that trade less with neighbors to be at greater risk because the neighboring countries' cost of fueling conflict is lower.

Table 1 presents the summary statistics of the main variables used in the analysis.[20]

4. Methodology

The main empirical analysis estimates the impact of various trade-related variables on the onset of conflict. We follow B-B and model conflict onset and ending separately, using split samples.[21] This strategy allows incorporating the dynamic properties of conflicts (Beck and Katz, 2011). These are highly persistent so past years of conflicts affect current conflict. As the former are in turn affected by past shocks, not modeling the dynamics introduces a bias in the estimation. The basic specification reads as follows:

$$CO_{it} = \alpha_i + \sum_{j=0}^{n} \beta_j Px_{it-j} + \sum_{j=0}^{n} \gamma_j Pm_{it-j} + \sum_{j=0}^{n} \theta_j MP_{it-j} + \vartheta TN_{it-1} + \Gamma X_{it} + \rho_t + +\varepsilon_{it} \quad (5)$$

$$CE_{it} = \alpha_i + \sum_{j=0}^{n} \beta_j Px_{it-j} + \sum_{j=0}^{n} \gamma_j Pm_{it-j} + \sum_{j=0}^{n} \theta_j MP_{it-j} + \vartheta TN_{it-1} + \Gamma X_{it} + \rho_t + +\varepsilon_{it} \quad (6)$$

where *CO* and *CE* are conflict onset and ending (as defined above) for country *i* at time *t*, *Px* is the (country-specific) export price index, *Pm* is the import price index; *MP* is a country-specific market potential variable; *TN* is the share of i's trade with its neighbors (i.e. countries with which it shares a border) in total trade; *X* is a vector of time varying controls, α are country fixed effects, *ρ* are time effects and ε is the i.i.d. error term. The use of country fixed effects control for any time invariant factors that may influence the probability of conflict such as geography, ethnicity, religion, and

[20] The list and description of the variables used for interaction terms (on grievances, political system, governance, and price transmission) is given in the appendix. These variables are used to construct the interaction terms to tease out the heterogenous effects of trade shocks across countries (as explained below).

[21] This means that conflict onset is coded as zero in the non conflict years, 1 in the first year of conflict and missing in the following years of conflict. This is a departure from much of the previous literature which uses a dummy for the first year of conflict (and zero otherwise) to measure conflict onset. Ending would take the value of zero during the conflict; the value of 1 in the first year of peace and missing in the other years.

colonial history. In addition, the different lags in the price regressor account for the time-dependence of these shocks, which are usually negatively autocorrelated and can take many periods to affect earnings.[22]

Unlike the other trade variables, *TN* does not have a lag structure, since it is highly persistent and one single lag appears appropriate to capture its effect on conflict while reducing the potential endogeneity concerns. We argue that the other three trade variables – *Px*, *Pm* and *MP* - are also exogenous to the individual countries' conditions that are associated with the probability of conflict. These variables capture the variation in demand and supply in international goods markets and in a country's trading partners. These factors should not be influenced significantly by conditions in individual countries.

An exception to this rule is when the countries are large enough to influence the international supply and/or demand in certain markets. If world prices rise in anticipation of conflict, this may lead to a spurious positive correlation between conflict and the lagged price index. In our sample there are 16 cases of a country producing on average more than 20% of global exports of a particular commodity. One way to deal with this issue is to exclude the commodity in question from the country's price index, which is what B-B do. However, that may introduce another source of bias. Consider for instance coffee production in Colombia. The country is responsible for 14% of global coffee exports. Dube and Vargas (2013) show that the sharp fall in international coffee prices in the 1990s substantially increased conflict intensity in Colombia. Thus excluding coffee from Colombia's price index would bias the *Px* coefficient upward. Because of this reason we do not exclude any commodities from the price shock variables in the baseline specifications. Instead we check the robustness of the results to the exclusion of relevant commodities (according to different thresholds) as

[22] In particular B-B and Bruckner and Ciccone (2010) use $j \in [0; 2]$.

well as of some countries which are price-makers in at least one commodity (average share in global exports above the threshold).

The vector X contains controls that are likely to have an independent effect on conflict. First we include a dummy for whether the country has had any conflict since World War II, which captures the higher likelihood of starting a conflict for countries which already experienced one recently (World Bank, 2011). Second, in order to control for the spillover effects of conflict, we include a dummy for whether any of the neighboring countries has a civil conflict. The variable is lagged one year to reduce the endogeneity concerns. In some specifications we also include the incidence of coup attempts (whether successful or not) in the two years prior to *t*, based on Powell and Thyne (2011). This can be interpreted as an indicator of a weak state (Kuhn and Weidmann, 2013), but it could also act as a trigger for a civil conflict itself.

To differentiate the effects across classes of commodities, we split the Px variables into different groups of commodities. In particular, we distinguish between point-source and diffused commodities as well as between commodities that are consumed in the country and those that are not. This yields four different export price variables, each with a lag structure.

We use the linear probability model as in B-B to estimate equations (5) and (6), although the results are very similar when using the conditional fixed effect logit estimator as well (results available upon request).

One problem with estimating equations (5) and (6) is that the dependent variables have a very large number of zeros relative to the number of ones, i.e. the models try to explain rare events.[23] That is inherent in the nature of this type of empirical analysis, although it is rarely acknowledged in other studies. Importantly, this problem can lead to biased coefficients' estimates in finite samples (King and Zeng, 2001). In addition, the bias in the standard errors tends to go in the same direction as the bias in

[23] King and Zeng (2001) define 'rare events' dependent variables as those for which the number of zeros are larger than that of ones by at least an order of magnitude in the dozens.

the coefficients. Thus the rare event bias may cause the underestimation of event probabilities. King and Zeng (2001) suggest that one way to correct the problem is to decrease the rareness of the event. That could be done, for example, by lowering the threshold of what constitutes an event or by expanding the data selection period. In our case this would strengthen the case for using *any civil conflict* as the dependent variable. For this variable the number of events is 25 times smaller than the number of zeros while it is 50 times smaller for *major conflict onset.* We explore in more detail below the implications of this possible bias in interpreting the empirical results.[24]

In addition to investigating the average effect of the trade variables on conflict, we also model some of the heterogeneity of these effects across countries. In order to do so we interact the trade shocks with variables that proxy for underlying fragility of countries and thus their propensity to fall into conflict due to trade shocks. We include conditioning factors that reflect the different groups of potential determinants of fragility highlighted in Calì (forthcoming) (the full list is reported in Table A1 in the Appendix).

In particular, we include factors belonging to the four groups identified in Calì (forthcoming). The first group comprises factors that may affect grievances, including income inequality, ethno-linguistic and religious fractionalization and polarization, and the incidence of past conflicts. By influencing the extent of citizens' grievances, these conditions may influence the propensity of people to engage in violence for certain levels of shocks.[25]

The second group includes factors related to the country's institutional capacity and inclusiveness, as the type of political system (e.g. parliamentary versus non parliamentary democracies; federal versus unitary government), proxies for the quality of governance (e.g. bureaucratic quality and degree of government accountability) and proxies for the quality of parties in

[24] King and Zeng (2001) also propose a rare logit estimator to correct the rare event bias. Unfortunately the lack of convergence in the estimation of our model does not allow us to implement that estimator.
[25] For example, Blimes (2006) finds that the degree of ethnic fractionalisation increases the likelihood that other potential determinants of civil war will have an effect on civil war onset.

office (age of party in office and the extent to which parties are programmatic).[26] There are various ways in which the types of state institutions may affect the impact of a trade shock on conflict. For example, countries with more effective and/or inclusive state institutions may be better able to negotiate compromises among social groups to avoid unrest in the face of an adverse trade shock. Similarly, the capacity of the state can affect the degree to which increases in resource rents can be appropriated through fighting.

The third group of conditioning factors includes conditions in neighboring countries that may affect their general propensity to destabilize their neighbor. We explore two types of such conditions: the presence of a conflict in neighboring countries and the country's trade ties with its neighbors. While they could both have a direct effect on the probability of conflict, these factors could also change the propensity of an external shock to trigger a conflict. For example, a conflict in a neighboring country may make it easier for potential rebels to access weapons to stage a rebellion in their own country, taking advantage of political disaffection due to an adverse trade shock.

The final group of factors we consider are those affecting the transmission of international prices to the domestic economy. In particular we use the extent to which agricultural commodities' markets are distorted – i.e. the nominal rate of assistance to agricultural markets (Anderson et al., 2008) – to capture the propensity of governments to absorb international commodity price shocks via policy measures. Such measures may dampen the effects of commodity price shocks on conflict (Carter and Bates, 2012).

[26] The latter proxies are derived from the work by Hanush and Keefer (2013) and Cruz and Keefer (2013), who show that both the amount of time a party has been in office and the extent to which it is programmatic (that is, takes an ideological approach to economic issues) seem to affect the quality of political outcomes, such as political budget cycles and public sector reforms.

5. Results

We start by analyzing the determinants of conflict onset. Table 2 presents the results of the estimation of equation (5). The first four columns use *any* conflict onset as the dependent variable. To make our results comparable with previous studies column (1) includes only the export price index *Px* as the independent variable (along with the control variables and country and year effects). The sum of the contemporaneous and lagged coefficients is positive and significant; the effect is mainly driven by the contemporaneous price variable. Conditional on the changes in *Px* at *t-1* and *t-2*, a one standard deviation increase in the export price index raises the probability of conflict by 4 per cent in the same year. The signs of the lagged coefficients are consistent with the negative autocorrelation of commodity prices (i.e. the coefficient on $Px_{(t-1)}$ is negative and that on $Px_{(t-2)}$ is positive). Importantly, the sum of the three *Px* coefficients is positive and significant. It suggests that an increase of 10% in *Px* raises the risk of conflict by 2.2 percentage points.

Having had a past conflict (since 1946) raises the probability of conflict by 18%, which confirms the findings in World Bank (2011). On the other hand the presence of a major conflict in the neighboring countries the year before does not affect the conflict probability in the country of interest (columns 2-4).

Adding the other trade variables does not affect the size of either the contemporaneous *Px* coefficient or the sum of the three *Px* coefficients (contemporaneous plus two lags), which remain positive and significant (column 2). The other trade variables are not significant at the standard levels. The sums of the contemporaneous and lagged coefficients on *Pm* and on *MP* are both very close to zero, with their contemporaneous coefficients having the expected sign (positive for *Pm* and negative for *MP*). The share of trade with neighbors is negative but not significant. The results do not change when including a dummy for the incidence of a coup attempt in the preceding two years (column 3).

The positive and significant effect of *Px* on conflict contradicts the finding in the similar equation from B-B, where the coefficient on the commodity price index was not significant.[27] As the estimation strategy and the data are comparable, this difference has to do with the different way the price index is computed, i.e. using prices in levels as opposed to changes. Indeed when we compute *Px* using price changes, its contemporaneous coefficient becomes less significant and their sum becomes not significant, as in B-B (column 4). As explained above, we believe that using the level is more appropriate, therefore we take the positive *Px* coefficient as more representative of the effect of export price shocks on conflict.

The non significant results for *Pm* are different from the positive impact found by Arezki and Bruckner (2011). However, they are consistent with the note of caution highlighted above, i.e. a developing country's commodity imports account for only a limited share of total consumption of commodities, since a large share of consumption, especially among poorer households, comes from domestic production. At the same time, the lack of significance of the demand variable *MP* suggests that the economic cycles in the export destination markets do not affect the probability of conflict at home, unlike other economic shocks such as rainfall (Miguel et al., 2004) and foreign interest rate movements (Hull and Imai, 2013).[28]

In columns 5-8 we test for the effect of the same variables on major conflicts in the way we defined them above. The effects of *Px* on major conflicts is not significantly different from zero overall, although its lagged coefficient is negative and significant (column 5). These results are unchanged when adding the other trade variables (column 6) as well as the control for coups (column

[27] Our result is also different from the negative coefficient of Brückner and Ciccone (2010). However our specification is not comparable to theirs, as we run the regressions for onset and ending separately on split samples as in B-B. In addition we also use the log of price level instead of price change.

[28] This result appears to contradict those of two other recent studies, Berman and Couttenier (2014) and Chaudion et al. (2013). However the former considers only large economic downturns in trading partners (i.e. banking crises) and focuses on conflict eruption at the sub-national level. The latter uses the MP variable as an instrument for economic growth rather than as an independent regressor, and no lagged structure is used.

7). The other trade variables also have little impact on major civil conflicts. The trade coefficients are also not significant when computing the price indices with price changes instead of levels (column 8).

Taken at their face value these results suggest that export shocks could affect the eruption of minor conflicts but not of major civil wars. That would be the case, for instance, if these shocks influence local conflicts, which do not eventually spill into large scale civil wars. That would be consistent with evidence from Colombia (Dube and Vargas, 2013) and the DRC (Maystadt et al., 2014). However the lack of significance of the results for major conflicts could also be a product of the 'rare event' bias highlighted above. That bias is more likely to apply to the regressions with *major conflict onset* as the dependent variable, as this is a much rarer event than any civil conflict onset. Given this possible interpretation, we consider the specifications with *any conflict onset* as the dependent variable as our preferred ones.

5.1. Robustness

These results are robust to a wide array of checks presented in tables 3 and 4. First, the results for *any conflict onset* survive when adding country-specific time trends (column 1, table 3) as well as when constructing the price indices using a faster-moving average of the weights (i.e. the average between *t-4* and *t-2*) (column 2). When we add country-specific time trends to the latter specification, the contemporaneous *Px* coefficient remains positive and significant, while the sum of lagged *Px* coefficients becomes significant only at the 15% level (column 3). In fact, the size of the *Px* effect on conflict increases slightly with time trends, to a 2.5% rise in conflicts for a 10% increase in *Px* (column 1). The results are also qualitatively similar to those in table 2 when running the same battery of checks for *major conflict onset* (columns 4-6).

In Table 4 we also check the robustness of the results to the possible endogeneity resulting for countries that have a significant influence on the international prices of their exported commodity. We

use two strategies to deal with this concern. First, we exclude from country X's export basket the commodities for which X's share in world exports is above a certain threshold (10% in column 1 and 20% in column 3), and obtain results that are similar to our baseline results in table 2. The main difference is that the coefficient of *Px* is smaller but still significant at the 10% level. Second, we exclude the countries which are large exporters of at least one commodity (in one year) according to the 10% (column 2) or 20% (column 4) criterion. This approach has the advantage of not generating artificial biases in the countries' export baskets. However, it also leads to a reduced sample that may be less representative than the full sample of developing countries as a group. The results are again the same as in our baseline results.

Finally, we test whether our results are different if we use the COW dataset for the conflict data. In order to make the regressions comparable, we re-run the regression with PRIO data using the same battle deaths threshold of major conflict as in COW (i.e. at least 1,000 battle deaths over the entire course of the conflict). While we feel that the PRIO data is a more reliable data source, it is reassuring to see that the main results do not vary much across datasets. The comparison – reported in columns 5-6 – shows that the individual trade variables' coefficients are not significantly different across the datasets. The only exception is that the contemporaneous coefficient on *Pm* is positive and significant in COW and negative but not significant in PRIO. However, the sum of the *Pm* variables is not significant with either dataset.

5.2. Commodity types

In table 5 we test for the effects of the different types of commodities on civil conflict by splitting the price indices into point-source and diffused commodities and into commodities that are consumed domestically and those that are not. To save clutter, for each price variable we only report the value and significance of the sum of the three coefficients (the contemporaneous and the two

lagged variables).[29] This split suggests that the conflict-inducing impact of the export price rise is mainly driven by the price of point-source commodities, while the effect of diffused commodities on the probability of conflict is not significant (column 1). These results are consistent with the rapacity effect hypothesis, while they provide no support to the opportunity cost hypothesis of conflict.

In column 2 we split the commodities into those that are used for final consumption and those that are not, finding that only the former have a positive and significant impact on conflict onset (although the coefficient on the price index of commodities not for final consumption also has a positive sign). While this result appears consistent with the expectation that increases in the prices of commodities for final consumption reduce the opportunity cost of engaging in conflict, further analysis indicates that the result may be driven by the domestically-consumed, point-source commodities (i.e. oil and gas), and thus may reflect the 'state-prize' motivation for conflict.

This can be seen in column 3, where we further split the export price index into the four types of commodities that combine the two dimensions. While the coefficients of the sub-indices are not significant, the magnitudes suggest that point-source, consumed commodities exert the largest impact on conflict of all the sub-categories. This group comprises oil and gas commodities, which represent important consumption items in many developing countries, especially in the urban areas. In the absence of consumption data by country, it is not possible to disentangle the rapacity effect from the consumption effect in this case. Noting that our variable is constructed on the basis of the export shares, we interpret this effect mainly as a rapacity effect. The coefficients of the diffuse export commodities are also not significantly different from zero for both the consumed (which has a negative sign) and the non consumed (which has a positive sign).

This result confirms the lack of support for the opportunity cost hypothesis at least via the export sector, which is in line with the findings in B-B. Even the commodity group that should provide

[29] Unless otherwise stated, we also do not report the coefficients of the other trade variables as they are little affected by the splitting of the price indices (results available upon request).

the cleanest test for the opportunity cost hypothesis – i.e. diffused, not consumed commodities – does not provide any support for the hypothesis. On the other hand, the weakly positive sign suggests that some form of rapacity effect may be at work even with diffused commodities, for example as the revenues from these commodities may also be taxed by rebel groups to fund their struggle.

5.3. Impact on conflict ending

Next we test for the impact of the trade variables on the probability of a conflict ending by implementing equation (6). This specification uses the same variables as the conflict onset regression and essentially measures the impact of trade shocks on conflict duration. The results in this case – reported in table 6 – suggest a different impact of the trade variables than in the case of conflict onset.

No individual trade variable has a significant impact on the probability of a conflict ending (columns 1-4). The only exception is trade with neighbours, which significantly increases the likelihood of ending. This finding is consistent with the idea that when a country has strong trade links with neighboring countries, their incentive to de-stabilize their neighbors is lower. We leave it to future research to establish why this effect seems to play out only once the conflict is started, as the effect of this variable was not significant for conflict onset. In addition, the sum of the *Pm* coefficients is also positive and significant, although the result is not robust to the inclusion of country-specific time trends (columns 2 and 4). This suggests that increases in the prices of imported commodities cumulatively raise the probability of a conflict ending, which is a puzzling result in light of the opportunity cost theory. Partially countering this puzzle is the fact that the effect of the contemporaneous import price shock is negative (but not significant). The *Px* variables are not significant across the specifications, which is again different from the result in B-B, who instead find some support for the hypothesis that increases in commodity export prices reduce the duration of conflict.

In line with the previous results, the trade variables do not have a significant impact on the probability of a conflict ending when considering only major conflicts (columns 5-8). The only exception is Px_{t-1} which has a negative and significant effect on a conflict ending. However, this effect is offset by that of the other *Px* terms and the sum of the *Px* coefficients is not significant.

5.4. Trading with neighbors re-examined

One possible explanation for the lack of a significant impact of *trade with neighbors* on conflict onset comes from the findings of Martin et al. (2012). They show that the intention of nurturing peaceful relations via trade has been a key reason for countries to sign Regional Trade Agreements (RTAs) in the second half of the 20th century. The prospect of political gains was a necessary complement to the economic gains in convincing countries to sign RTAs. While the authors focus on inter-state conflicts, the same argument may also apply to domestic civil conflicts, as many such conflicts are fuelled by foreign countries, especially in the same region (Gersovitz and Kriger, 2013). This argument suggests that a country may sign RTAs with the goal of improving relationships with countries that otherwise could be a source of instability. In the extreme, only those neighbors of a country X with which X has signed an RTA may be important in destabilizing X. We can then slightly qualify the hypothesis of what trade with neighbors matters to trigger conflict in the following way: the higher the share of trade with those neighbours with which a country has signed an RTA, the lower the likelihood of conflict.

In order to test this hypothesis, we replace the *share of trade with neighbors* with the same variable interacted with an RTA dummy which takes the value of 1 for each country-pair that had an RTA between them by the year before the observation. In this way the variable becomes the share of trade with neighbors under RTAs in a country's total trade. We cannot rule out the endogeneity of RTAs, since part of the driver behind RTAs could be the desire of intensifying the economic relations

with neighbors that may otherwise be able to destabilize the country (similarly to the Martin et al. (2012) story for inter-state wars).

As showed in table 7 this variable has a negative and significant association with *any conflict onset* (columns 1-4), while it has no significant association with *major conflict onset* (columns 5-8). The potential endogeneity of the RTA dummy does not allow interpreting this result as causal.

In order to relieve some of the endogeneity concerns, we instrument the share of trade with neighbors under RTAs with a variable constructed in the same way, except that it considers only plurilateral RTAs. As they involve more than two countries such RTAs should be less driven than bilateral RTAs by one country's strategic motives, including also the risk of conflict considerations. The results of the instrumental variable regressions are very similar to the plain OLS, as the endogenous variable and the instrument are highly correlated (columns 9-16). The coefficients are only slightly more negative and significant, providing some evidence that if anything the endogeneity biases the absolute size of the coefficients downwards.

This instrument is not likely to fully address the endogeneity issue, and in the absence of a suitable instrument, we can only interpret this result as suggestive evidence of the importance of promoting trade via formal agreements with contiguous countries in order to prevent civil conflict. Future research would need to test this hypothesis more thoroughly.

5.5. Heterogeneity

So far the analysis has focused on the average effects of the trade variables on conflict across countries. As noted above, trade-related shocks are likely to affect countries' stability to different degrees according to various characteristics. We test for that by using a set of interaction variables belonging to the four groups of factors described above (and presented in table A1): conditions in neighboring countries; factors affecting grievances; types and characteristics of political institutions

(i.e. political regimes and quality of governance); and factors affecting the transmission of international prices to domestic markets.

We interact each trade shock variable separately with each of the conditioning variables using our baseline regression as in column 2, table 2 (with *any conflict onset* as the dependent variable).[30] Running these regressions one interaction term at a time maximizes the number of observations over which we can run the regressions, as most of the conditioning variables are not available for the entirety of our baseline sample. This strategy also has the advantage of maximizing the degrees of freedom, compared to including all the interactions in one regression.

With 25 conditioning variables and three types of trade variables to interact (*Px, Pm* and *MP*), we run a total of 75 regressions. We also add a set of regressions with the interaction of the *Px* for point-source commodities, as this is the main driver of the *Px* effect on conflict onset. To keep the number of coefficients to interpret manageable, we only include the contemporaneous trade shock variable (and its interaction with the relevant variable) in the regression, without its lagged terms. This choice is justified by two reasons. First, the coefficient of this variable incorporates to some extent the coefficients of the lagged trade variables, especially for the Px variable (see table A2 in the Appendix vis-à-vis the joint coefficients in table 2 column 2 and table 3, column 1). Second, the contemporaneous term is of particular interest in and of itself, as in our analysis (and in others) it tends to be the most important one to explain the impact of trade shocks on conflict onset.

It is important to note that the fixed effect specification effectively restricts the analysis of heterogeneity to those countries which have had a conflict onset in our period of analysis (i.e. since 1960). In this way we are under-estimating the cross-country heterogeneity with respect to their conflict propensity. A longer historical analysis would be able to exploit more fully this heterogeneity.

[30] We are mainly interested in modeling the cross-country heterogeneity in the effect of the trade shocks on conflict. Therefore, we do not interact the conditioning variables with the share of trade with neighbors' variables (results for this separate set of regressions are available upon request).

Table 8 summarizes the results of these regressions. It reports the range of values of the conditioning variable for which the marginal effect of the trade variable on conflict onset becomes not significantly different from zero (at the 10% level). We report this range only when in the rest of the values' range the marginal effect is significant. If the marginal effect of price shocks is always or never statistically different from zero across the entire values' range, then we report "No diff." (no difference). This allows us to identify those conditions that make certain countries particularly vulnerable or resilient to the shocks.

Figure 1 provides a graphic example of our results for *Px* using two different conditioning factors. The upper panel presents the curve describing the marginal effect of *Px* along with its 90% confidence interval across the range of values of the accountability measure (from ICRG data). The downward slope indicates that the effect of *Px* becomes less significant as one moves from low to high accountability country-years (i.e. from left to right). In particular, for accountability values above 3 the effect becomes not significantly different from zero. Similarly, the upward sloping curve in the lower panel indicates that the *Px* effect on conflict becomes larger for country-year characterized by at least one conflict in contiguous countries. In fact, the Px effect (while positive) is not significantly different from zero for country-years pairs with peaceful neighbors.

The first thing to note from the results in table 8 is that there appears to be a high degree of heterogeneity in the effects of Px (and of its sub-component point-source Px) on conflict. This heterogeneity goes by and large in the expected direction. Export shocks have lower effects on conflict in countries that are located in peaceful neighbourhoods, that have lower ethnic divisions and economic inequality, that have better governance, and that have some distortions to their agricultural markets.

Variables related to grievances and to conditions in neighboring countries appear to be particularly important in determining the effect of *Px* on conflict. In countries with sufficiently low

levels of economic inequality and of ethnic divisions (whether measured through ethnic polarization or fractionalization), *Px* and point-source *Px* have no statistically significant effect on conflict probability. Similarly, *Px* and point-source *Px* do not have a significant impact on conflict in countries which didn't experience any conflict in the previous 10 years. Similarly, the probability of conflict in countries whose neighbors are not in conflict, or that have sufficiently intense trade with their neighbours, is not significantly affected by export shocks.

The results are less clear cut for state institutions and for price transmission variables, although they also point to important conditioning effects. For political institutions, having a federal government appears to consistently reduce the probability that an export price shock triggers conflict. That may be because regions tend to have greater autonomy under federal systems than under unitary government systems, which may reduce the incentive for challenging the central state at the local level. For example, regions within federal countries tend to have more influence over the allocation of natural resource revenues in their territory (Brosio and Singh, 2014), which our empirical results suggest is a potentially contentious issue in fragile countries. The other political variables, including the share of programmatic parties (those with an ideological orientation with respect to economic policy) in power, the age of the party in office, whether elections were held the previous year, and the degree of democracy (including also parliamentary democracy) do not yield robust results across *Px* and point-source *Px*. Democratic countries (i.e. a score higher than 5 – out of 10 – in the polity index) are less subject to the conflict-inducing impact of point-source commodity prices shocks, a result broadly in line with Besley and Persson (2008).[31] These results suggest that the type of state institutions, at least as defined through commonly available measures, seem to have a limited impact in determining the effects of export shocks on conflict.

[31] However, unlike Besley and Persson (2008), these price shocks do not exert any differential impact on conflict in parliamentary democracies versus other forms of governments.

The quality of state institutions measured by various dimensions of governance seems to matter more in this context. In particular, changes in *Px* have no effect on conflict in countries with a high degree of government accountability, a low degree of corruption, and high bureaucratic quality, although the latter two results do not carry over to point-source *Px* as well. Other potentially relevant measures of governance, such as the rule of law and the presence of the military in the government, do not seem to be important in this context.

Finally, our results support the hypothesis that substantial nominal rates of assistance to agricultural commodities (whether positive or negative) reduce the risk of conflict due to export price shocks. This finding is consistent with the hypothesis that international prices are transmitted more rapidly to domestic prices in countries with low distortions to agricultural markets (Anderson et al., 2008). However, this result does not hold for shocks in the prices of point-source commodities. This finding comes with two notes of caution. First, these distortions also cause reductions in welfare relative to more direct policy instruments aimed to achieve similar domestic policy objectives (Bhagwati 1978; Corden 1971). Second, the effect we are capturing is inherently short term, as prices are eventually transmitted to domestic prices in the longer run (Ivanic and Martin, 2013).

On the other hand, the impact on conflict of the price of imports and of demand shocks in trade partners do not appear to be affected by heterogeneity. These trade variables have generally no significant impact on conflict across the entire range of the conditioning factors' values. This result underscores the weakness of these variables in explaining conflict onset in our model.

What is the relative importance of the conditioning factors in explaining the resilience to trade shocks? We use Bayesian Model Averaging (BMA) techniques to address this question. This approach allows us to cope with uncertainty on what is the "true model" (e.g. what controls to include) and to assess the relative importance of the interaction terms. The idea behind BMA is that the "true model" explaining the dependent variable is unknown and that different combinations of covariates may result

in different coefficients and levels of significance (Hoeting et al., 1999). In our case, the significance of the interaction terms may depend on the exclusion/inclusion of other covariates and interaction terms. The procedure runs all the possible combinations of regressors, and on the basis of the results it provides for each variable the posterior inclusion probability, i.e. the probability that it is part of the "true model". That allows us to rank the importance of each interaction term in determining the effect of *Px* on civil conflict onset.[32]

We include only those conditioning factors with more than 2,500 observations, to maximize the degrees of freedom in the model. This leaves us with 14 factors, among which Table 9 reports the most important ones, i.e. those with a probability of inclusion in the "true model" above 20%. These conditioning factors include holding elections in the previous year, the degree of ethnic fractionalization, the presence of at least one conflict in neighboring countries, and a recent history of civil conflict (in the past 10 years). All of these conditioning factors increase the impact on conflict of commodity export prices. This result confirms that grievances and conditions in neighboring countries are particularly important in increasing the impact of trade shocks on conflict. Interestingly, export shocks right after elections appear to be particularly dangerous, which is different from the weak effect of the election dummy in table 9. This suggests that the election effect can be identified only when including other control variables in the analysis.

These marginal effects of the mediating factors could be used also to predict the risk of conflict from an increase in the value or a new discovery of point-source commodities in a specific country. To illustrate it let us take the example of Lebanon, where recent 3D seismologic mapping of the country's offshore have revealed the likely presence of sizeable hydrocarbon resources. According to some estimates this can be worth several times Lebanon's annual GDP (World Bank, 2014b). We can apply

[32] We follow a three steps procedure proposed by Eicher et al. (2012). First, we partial out the effects of the other variables (including the fixed effects) from the interaction terms and dependent variable. Second, we estimate a posterior distribution for every candidate interaction term. Third, we combine all posterior distributions into a weighted averaged distribution using each model's posterior probability as model weight. We implement the procedure through a modified version of the BMA package for R that corrects the number of degrees of freedom due to the partialling out.

the estimated coefficients of the interaction terms computed above to the values of the relevant variables for Lebanon to calculate the additional risk of conflict that this discovery would entail.

The country has a relatively high level of economic inequality, of religious fractionalization and it is experiencing a low intensity civil conflict.[33] These characteristics increase the risk of conflict induced by hydrocarbons discovery in Lebanon relative to the average country. In Table 10 **Error! Reference source not found.**we quantify this increased risk due to all the factors that have a statistically significant marginal effect as computed in the regressions underlying Table 8. On the basis of these marginal effects we know for example that for a country with the average level of religious fractionalization (i.e., 0.35), a standard deviation increase in point-source commodity exports raises the risk of conflict by 2.5 percent. At the level of fractionalization of Lebanon (i.e., 0.79) the increased risk is more than double at 5.7 percent. A similar story applies to the two other main factors, economic inequality and recent conflict. On the other hand other factors, such as level of democracy, of accountability, the presence of conflict in neighboring countries and the share of trade with neighbors, yield smaller differences between Lebanon and the average country.

6. Conclusions

Do trade shocks affect civil conflict? We have revisited and expanded the evidence on this question by using a large sample of developing countries in the 1960-2010 period. We focus on four types of trade variables: price changes of exported and imported commodities, changes in income in export markets, and the size of trade with neighboring countries.

[33] This conflict categorization follows from the International Country Risk Guide (ICGR) data as the UDCP/PRIO armed conflict database. In the first seven months of 2014 ICRG assigns an average value of 2 to Lebanon in the 'civil war' category (in a scale to 1-4), placing the country in the top decile of that category. The current low intensity conflict in Lebanon would also be in line with the definition of UCDP/PRIO of "a contested incompatibility that 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."

In a significant departure from the evidence in B-B, the analysis finds that an exogenous increase in the value of a country's exported commodities raises the probability of conflict in that country. The magnitude is important: an increase of 10% in the commodities' export value raises the risk of conflict by between 2.2 and 2.5 percentage points. We show that the difference with the results in B-B is mainly due to the fact that we construct the price index using levels rather than changes in prices.

The impact of export prices on conflict is mainly driven by point-source extractive commodities such as oil and minerals, while the prices of diffused agricultural crops have no discernible impact on conflict. This finding is consistent with the idea that an increase in the value of exported extractive commodities raises the value of the prize to be gained through fighting. On the other hand, we find no support for the 'opportunity cost' theory via exported commodities. In fact our evidence is more in line with the hypothesis that agricultural exports may serve to fuel rather than to quell existing conflicts, possibly by providing a source of funding for active rebel groups.

These results (like the others in the analysis) are robust to a large array of battery checks, but they do not hold for major civil wars. This may suggest that the fighting could erupt in the areas where the resources are concentrated without eventually evolving into a civil war. However, this finding could also be the product of a large 'rare event bias' for the major conflict variable. In the absence of a clear interpretation of the results for major civil wars, we mainly rely on the results based on all conflicts.

Our results also suggest that trading with contiguous countries helps reduce the duration, but not the onset, of conflict. This is consistent with the idea that such trade reduces the incentives of contiguous countries to fuel conflict in their neighbour. In addition, we find that trading with neighbors is also associated with a lower risk of conflict when such trade occurs under RTAs. On the other hand,

neither imported commodity prices nor the economic cycle in export markets appear to exert any influence on the probability or the duration of conflict.

We also model some of the heterogeneity of the effects of trade shocks on conflict onset across countries by using four types of conditioning factors, i.e. grievances, type and quality of state institutions, conditions in neighboring countries, and measures affecting the transmission of international prices to domestic prices. Grievances and the conditions in neighboring countries appear to be particularly important in making a country more or less exposed to conflict as a result of export price shocks. But good governance and domestic policies affecting the transmission of international to domestic prices seem also to play some role in this respect. Finally, our BMA analysis also suggests that export shocks are particularly dangerous right after election years.

As with all cross-country analyses, it can be difficult to draw policy recommendations for specific countries. However, these findings do have a number of general policy implications, which we sketch here. First, revenues from exports of extractive resources need to be managed in a way that is sensitive to their potential to be associated with conflict, especially in countries located in unstable regions, with a recent history of conflict, and with weak governance. In these countries, sudden windfall revenues from swings in international prices (or new discoveries) can substantially increase the risk of conflict. For example, the eruption of civil conflict in South Sudan at the end of 2013 followed on the heels of the renewed increase in oil exports to Sudan. While it is unclear to what extent the renewed oil windfall may have played a role in this conflict, this case fits closely with the evidence presented here. Our results point to the importance of limiting the discretion of the governments in spending the resources revenues windfall. To the end countries may want to consider institutional arrangements as sovereign wealth funds and direct dividend transfers (Devarajan and Giugale, 2013).

Second, our analysis suggests that promoting trade with a country's neighbors is also important to reduce the risk of conflict, or at least its duration. Trade policy as well as trade facilitation can help

foster these trade relations and deter the possible adverse influence of neighboring countries on civil conflict.

Third, the results suggest a note of caution in evaluating the role of agricultural income in deterring civil conflict participation. While the opportunity cost of engaging in conflict may increase with farmers' income, the latter may also be an important source of revenues for local rebel groups. The tension between these two effects is likely to apply to different degrees across contexts, but it would be important to keep it in mind when attempting to promote peace through increasing agricultural income.

Finally, the analysis points to the importance of dealing with grievances from past conflict and from economic and ethnic inequalities, and to invest in the quality of governance, in order to reduce the likelihood that trade (and most likely economic) shocks could increase conflict. Reducing corruption and improving public accountability are key measures that policy-makers should pursue in this respect.

Tables and Figures

Table 1: Summary statistics

Type	Variable	N	Mean	SD	Min	Max
Onset	Civil Conflict UCDP/PRIO	3465	0.044	0.205	0	1
	Major Conflict UCDP/PRIO	4053	0.021	0.144	0	1
	Major Conflict (modified) UCDP/PRIO	3861	0.015	0.123	0	1
	Major Conflict COW	3830	0.027	0.161	0	1
Ending	Civil Conflict UCDP/PRIO	1135	0.1304	0.33689	0	1
	Major Conflict UCDP/PRIO	420	0.20714	0.40574	0	1
	Major Conflict Phasing UCDP/PRIO	559	0.10912	0.31207	0	1
Battle-related deaths	Constructed PRIO	4300	930.60	4908	0	150000
	High PRIO	4300	1474.4	7472	0	250000
	Low PRIO	4300	306.2	1802	0	50000
Trade variables	Px slow moving	4300	0.068	0.947	-4.332	2.629
	Pm slow moving	4300	0.072	0.940	-3.308	2.001
	MP slow moving	4300	0.041	1.002	-1.120	7.404
	Px fast moving	4300	0.067	0.949	-4.280	2.550
	Pm fast moving	4298	0.072	0.941	-3.373	1.937
	MP fast moving	4300	0.040	1.003	-1.103	7.157
Controls	Any conflict since 46	4300	0.646	0.478	0	1
	Share trade with border	4238	0.116	0.156	0	0.891
	Neighbors' conflict (any)	4300	0.243	0.429	0	1

Table 2: The impact of trade on conflict

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	any onset	any onset	any onset	any onset	major onset	major onset	major onset	major onset
Px (t)	0.040**	0.039**	0.039**	0.010*	0.012	0.008	0.008	0.003
	(0.016)	(0.016)	(0.016)	(0.005)	(0.011)	(0.011)	(0.011)	(0.004)
Px (t-1)	-0.014	-0.014	-0.015	0.003	-0.020**	-0.016*	-0.016*	-0.003
	(0.020)	(0.020)	(0.020)	(0.006)	(0.009)	(0.009)	(0.009)	(0.003)
Px (t-2)	0.000	0.001	0.001	-0.005	0.007	0.005	0.005	-0.003
	(0.017)	(0.017)	(0.017)	(0.005)	(0.009)	(0.008)	(0.008)	(0.003)
Pm (t)		-0.001	-0.001	0.002		-0.022	-0.022	-0.005
		(0.024)	(0.024)	(0.006)		(0.014)	(0.014)	(0.004)
Pm (t-1)		-0.002	-0.003	0.003		0.017	0.017	0.000
		(0.026)	(0.026)	(0.007)		(0.016)	(0.016)	(0.004)
Pm (t-2)		-0.009	-0.009	-0.001		-0.001	-0.001	0.002
		(0.023)	(0.022)	(0.006)		(0.014)	(0.014)	(0.003)
MP (t)		-0.006	-0.006	-0.001		0.005	0.005	0.000
		(0.023)	(0.023)	(0.004)		(0.010)	(0.010)	(0.002)
MP (t-1)		-0.041	-0.042	-0.008		-0.038	-0.038	-0.005
		(0.054)	(0.054)	(0.010)		(0.036)	(0.036)	(0.007)
MP (t-2)		0.061	0.063	0.007		0.052	0.052	0.006
		(0.066)	(0.065)	(0.012)		(0.047)	(0.047)	(0.011)
Trade with neigh. (t-1)		-0.033	-0.031	-0.037		-0.018	-0.018	-0.018
		(0.053)	(0.054)	(0.057)		(0.032)	(0.032)	(0.035)
Any conflict since 1946	0.179***	0.183***	0.185***	0.177***	0.046***	0.045***	0.045***	0.047***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.012)	(0.012)	(0.012)	(0.012)
War border (t-1)		-0.006	-0.007	-0.007		0.002	0.002	0.003
		(0.012)	(0.012)	(0.012)		(0.008)	(0.008)	(0.008)
coup			-0.018	-0.015			-0.001	-0.001
			(0.016)	(0.018)			(0.008)	(0.008)
Observations	3,465	3,428	3,428	3,327	3,861	3,812	3,812	3,704
R-sq. (within)	0.069	0.072	0.072	0.068	0.025	0.026	0.026	0.026
Countries	114	114	114	114	115	115	115	115
Shocks	logP	logP	logP	Δlogp	logP	logP	logP	Δlogp
Weight	[t-12; t-2]	[t-12; t-2]	[t-12; t-2]	[t-12; t-2]	[t-12; t-2]	[t-12; t-2]	[t-12; t-2]	[t-12; t-2]
Time trends	NO	NO	NO	NO	NO	NO	NO	NO
Sum Px	0.026**	0.026**	0.025**	0.008	-0.001	-0.002	-0.002	-0.002
Sum Pm		-0.013	-0.013	0.004		-0.006	-0.006	-0.003
Sum MP		0.015	0.016	-0.002		0.019	0.019	0.001

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1*

Table 3: robustness with fast-moving, country-specific time trends

	(1) any onset	(2) any onset	(3) any onset	(4) major onset	(5) major onset	(6) major onset
Px (t)	0.037**	0.035**	0.033**	0.010	0.006	0.007
	(0.017)	(0.015)	(0.016)	(0.011)	(0.010)	(0.010)
Px (t-1)	-0.015	-0.016	-0.015	-0.015*	-0.015*	-0.014*
	(0.020)	(0.020)	(0.020)	(0.009)	(0.008)	(0.008)
Px (t-2)	0.008	0.003	0.008	0.008	0.006	0.009
	(0.019)	(0.017)	(0.018)	(0.009)	(0.007)	(0.008)
Pm (t)	0.007	0.011	0.017	-0.021	-0.005	-0.003
	(0.026)	(0.023)	(0.025)	(0.015)	(0.011)	(0.012)
Pm (t-1)	-0.007	-0.021	-0.023	0.015	0.003	0.004
	(0.026)	(0.022)	(0.022)	(0.016)	(0.014)	(0.014)
Pm (t-2)	0.001	0.001	0.010	0.004	0.001	0.004
	(0.025)	(0.017)	(0.020)	(0.016)	(0.010)	(0.011)
MP (t)	-0.015	-0.016	-0.027	-0.007	-0.009	-0.024**
	(0.023)	(0.022)	(0.024)	(0.011)	(0.011)	(0.012)
MP (t-1)	-0.043	-0.031	-0.052	-0.038	-0.026	-0.031
	(0.055)	(0.048)	(0.050)	(0.038)	(0.038)	(0.042)
MP (t-2)	0.068	0.057	0.093	0.059	0.053	0.068
	(0.070)	(0.057)	(0.066)	(0.051)	(0.054)	(0.060)
Trade with neigh. (t-1)	-0.084	-0.030	-0.080	-0.038	-0.015	-0.036
	(0.059)	(0.054)	(0.057)	(0.041)	(0.033)	(0.041)
Controls	YES	YES	YES	YES	YES	YES
Observations	3,428	3,425	3,425	3,812	3,809	3,809
R-sq. (within)	0.156	0.072	0.157	0.064	0.027	0.066
Countries	114	114	114	115	115	115
Shocks	logP	logP	logP	logP	logP	logP
Weight	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]
Time trends	YES	NO	YES	YES	NO	YES
Sum Px	0.030*	0.022**	0.025	0.003	-0.003	0.002
Sum Pm	-0.00003	-0.009	0.003	-0.002	-0.002	0.005
Sum MP	0.010	0.010	0.014	0.013	0.018	0.013

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; controls include any conflict since 1946 and war border (t-1).*

Table 4: Robustness for price makers and conflict data source

	(1)	(2)	(3)	(4)	(5)	(6)
Conflict data	PRIO	PRIO	PRIO	PRIO	PRIO	COW
Dep. Var.	any onset	any onset	any onset	any onset	Major (1,000)	Major (1,000)
Px (t)	0.028*	0.040**	0.040**	0.043**	0.006	0.018**
	(0.016)	(0.018)	(0.016)	(0.016)	(0.010)	(0.009)
Px (t-1)	-0.012	-0.023	-0.022	-0.025	-0.019*	-0.025**
	(0.020)	(0.022)	(0.020)	(0.021)	(0.011)	(0.011)
Px (t-2)	0.001	0.007	0.005	0.008	0.004	0.011
	(0.017)	(0.019)	(0.017)	(0.017)	(0.008)	(0.009)
Pm (t)	-0.006	0.002	-0.001	0.009	-0.007	0.037**
	(0.024)	(0.027)	(0.024)	(0.025)	(0.017)	(0.017)
Pm (t-1)	-0.001	-0.006	-0.004	-0.009	0.005	-0.017
	(0.026)	(0.031)	(0.026)	(0.028)	(0.017)	(0.021)
Pm (t-2)	-0.008	-0.017	-0.008	-0.013	-0.008	-0.007
	(0.022)	(0.027)	(0.022)	(0.024)	(0.015)	(0.014)
MP (t)	-0.004	0.001	-0.007	-0.001	0.005	0.001
	(0.023)	(0.024)	(0.023)	(0.023)	(0.012)	(0.009)
MP (t-1)	-0.042	-0.062	-0.038	-0.035	-0.032	-0.030
	(0.054)	(0.059)	(0.054)	(0.056)	(0.046)	(0.043)
MP (t-2)	0.061	0.081	0.059	0.049	0.043	0.057
	(0.065)	(0.070)	(0.066)	(0.067)	(0.057)	(0.061)
Trade with neigh. (t-1)	-0.034	-0.053	-0.033	-0.025	-0.037	0.003
	(0.053)	(0.055)	(0.053)	(0.053)	(0.034)	(0.036)
Observations	3,428	2,491	3,428	2,992	4,001	3,834
R-sq. (within)	0.070	0.084	0.071	0.079	0.024	0.036
Countries	114	85	114	98	115	115
Threshold	10%	10%	20%	20%	None	None
Exclude	Commod.	Countries	Commod.	Countries	None	None
Sum Px	0.017*	0.024**	0.023**	0.025**	-0.009	0.004
Sum Pm	-0.015	-0.022	-0.014	-0.013	-0.010	0.013
Sum MP	0.015	0.020	0.015	0.013	0.016	0.028

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all regressions include country fixed effects, year effects and controls (any conflict since 1946 and war border (t-1)); trade shock variables are weighted using the slow-moving averages (no country-specific time trends).*

Table 5: Splitting the commodities' variables into different types

	(1) any onset	(2) any onset	(3) any onset
Sum *Px* point-source	0.018*		
	(0.01)		
Sum *Px* diffused	0.013		
	(0.013)		
Sum *Px* consumed		0.017*	
		(0.009)	
Sum *Px* not consumed		0.022	
		(0.017)	
Sum *Px* point-source consumed			0.032
			(0.033)
Sum *Px* point-source not consumed			0.013
			(0.015)
Sum *Px* diffused consumed			-0.019
			(0.015)
Sum *Px* diffused not consumed			0.021
			(0.015)
Other trade variables	YES	YES	YES
Controls	YES	YES	YES
Weight	[t-12; t-2]	[t-12; t-2]	[t-12; t-2]
Observations	3,403	3,426	3,104
R-sq. (within)	0.070	0.072	0.072
Countries	114	114	114

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all regressions include country fixed effects, year effects and controls (any conflict since 1946 and war border (t-1)); other trade variables include MP and Pm with their three lags and trade with neighbors (t-1)..*

Table 6: Models with ending as dependent variable

	(1) any ending	(2) any ending	(3) any ending	(4) any ending	(5) major ending	(6) major ending	(7) major ending	(8) major ending
Px (t)	-0.047	-0.053	-0.037	-0.038	0.019	0.020	0.038	0.061
	(0.036)	(0.034)	(0.033)	(0.030)	(0.059)	(0.077)	(0.041)	(0.057)
Px (t-1)	0.040	0.033	0.036	0.028	-0.004	-0.013	-0.045	-0.056
	(0.054)	(0.055)	(0.052)	(0.052)	(0.065)	(0.059)	(0.061)	(0.055)
Px (t-2)	-0.002	-0.024	-0.011	-0.043	0.009	0.034	0.014	0.030
	(0.036)	(0.036)	(0.037)	(0.036)	(0.053)	(0.058)	(0.050)	(0.057)
Pm (t)	-0.044	-0.046	-0.033	-0.042	0.242*	0.237*	0.094	0.179*
	(0.070)	(0.080)	(0.059)	(0.063)	(0.127)	(0.140)	(0.088)	(0.105)
Pm (t-1)	0.129	0.098	0.101	0.077	-0.142	-0.143	-0.131	-0.142
	(0.102)	(0.101)	(0.083)	(0.079)	(0.169)	(0.159)	(0.127)	(0.119)
Pm (t-2)	0.020	0.007	0.021	0.002	0.075	0.082	0.101	0.044
	(0.080)	(0.089)	(0.071)	(0.078)	(0.134)	(0.167)	(0.098)	(0.098)
MP (t)	-0.093	-0.078	-0.050	-0.030	0.123	0.181	0.357	0.399
	(0.180)	(0.191)	(0.148)	(0.172)	(0.347)	(0.343)	(0.252)	(0.305)
MP (t-1)	0.060	-0.064	0.187	0.094	0.114	-0.084	-0.379	-0.571
	(0.332)	(0.312)	(0.268)	(0.258)	(0.479)	(0.432)	(0.409)	(0.394)
MP (t-2)	-0.079	-0.018	-0.278	-0.249	-0.320	0.032	-0.038	0.229
	(0.293)	(0.334)	(0.234)	(0.260)	(0.426)	(0.604)	(0.331)	(0.412)
Trade with neigh. (t-1)	0.249**	0.199*	0.221**	0.155	0.402	0.463	0.359	0.375
	(0.095)	(0.116)	(0.094)	(0.108)	(0.276)	(0.436)	(0.268)	(0.415)
Observations	1,104	1,104	1,101	1,101	540	540	537	537
R-sq. (within)	0.058	0.196	0.056	0.196	0.168	0.297	0.168	0.303
Countries	80	80	80	80	41	41	41	41
Shocks	logP	logP	logP	logP	logP	logP	logP	logP
Weight	[t-12; t-2]	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]	[t-12; t-2]	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]
Time trends	NO	YES	NO	YES	NO	YES	NO	YES
Sum Px	-0.008	-0.045	-0.011	-0.053	0.024	0.041	0.007	0.035
Sum Pm	0.105***	0.058	0.089**	0.037	0.174**	0.177	0.064	0.081
Sum MP	-0.113	-0.16	-0.141	-0.185	-0.082	0.129	-0.059	0.056

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all regressions include country fixed effects, year effects and controls (any conflict since 1946 and war border (t-1)).*

Table 7: The effect of trading with neighbors on conflict, revisited

	(1) any onset	(2) any onset	(3) any onset	(4) any onset	(5) major onset	(6) major onset	(7) major onset	(8) major onset
Trade with neighbors$_{(RTA)}$ (t-1)	-0.077*	-0.106	-0.084**	-0.113*	0.000	-0.006	-0.005	-0.008
	(0.040)	(0.065)	(0.040)	(0.064)	(0.021)	(0.025)	(0.022)	(0.026)
Instrumented	NO	NO	NO	NO	YES	YES	YES	YES
Time trends	NO	YES	NO	YES	NO	YES	NO	YES
Weights	[t-12; t-2]	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]	[t-12; t-2]	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]
Observations	3,464	3,464	3,461	3,461	3,461	3,461	3,461	3,461
R-squared	0.071	0.158	0.071	0.158	0.071	0.158	0.071	0.158
Nr. of countries	114	114	114	114	114	114	114	114
First stage Kleibergen-Paap								

	(9) any onset	(10) any onset	(11) any onset	(12) any onset	(13) major onset	(14) major onset	(15) major onset	(16) major onset
Trade with neighbors$_{(RTA)}$ (t-1)	-0.077**	-0.108*	-0.077**	-0.108*	-0.007	-0.015	-0.007	-0.015
	(0.038)	(0.055)	(0.038)	(0.055)	(0.018)	(0.021)	(0.018)	(0.021)
Instrumented	NO	NO	NO	NO	YES	YES	YES	YES
Time trends	NO	YES	NO	YES	NO	YES	NO	YES
Weights	[t-12; t-2]	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]	[t-12; t-2]	[t-12; t-2]	[t-4; t-2]	[t-4; t-2]
Observations	3,860	3,860	3,857	3,857	3,857	3,857	3,857	3,857
R-squared	0.026	0.067	0.026	0.065	0.026	0.065	0.026	0.065
Nr. of countries	115	115	115	115	115	115	115	115
First stage Kleibergen-Paap	22743	6948	22743	6948	27094	8503	27094	8503

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all regressions include country fixed effects, year effects and the trade shocks variables as in the previous tables and other controls (any conflict since 1946 and war border (t-1));trade shock variables are weighted using the slow-moving averages (no country-specific time trends).*

Table 8: Under what conditions are the marginal effects of trade shocks not significant?

	Type	Range	Px	Point source	Pm	MP
Grievance						
Economic inequality	Contin.	[22;65]	x<26 or x>58	x<46 or x>52	No diff.	No diff.
gini_net	Contin.	[15;75]	<32	<48	>52	No diff.
gini_market	Contin.	[17;80]	<37	<52	No diff.	No diff.
Ethnic polarization	Contin.	[0;1]	<0.37	<0.42	No diff.	No diff.
Ethnic fractionalization	Contin.	[0;1]	<0.28	<0.30	No diff.	No diff.
Religious fract.	Contin.	[0;0.7]	No diff.	<0.28	No diff.	No diff.
Religious polarization	Contin.	[0;0.96]	No diff.	<0.49	No diff.	No diff.
Any conflict in last 10 yrs	Dummy	[0;1]	No conflict	No conflict	No diff.	No diff.
Political institutions						
Elections (t-1)	Dummy	[0;1]	No diff.	Elections	No diff.	No diff.
Federal Govt.	Dummy	[0;1]	Federal	Federal	No diff.	No diff.
polity2	Contin.	[-10;10]	No diff.	>5	No diff.	No diff.
parliamentary democracy	Dummy	[0;1]	No diff.	No diff.	No diff.	No diff.
Programmatic party	Bins	[0;3]	< or > 2nd tercile	No diff.	No diff.	No diff.
Age of party in office	Bins	[1;191]	No diff.	No diff.	No diff.	No diff.
Governance						
Law	Contin.	[0;6]	No diff.	No diff.	No diff.	No diff.
Military	Contin.	[0;6]	No diff.	No diff.	No diff.	No diff.
Accountability	Contin.	[0;6]	>3	>2.2	No diff.	No diff.
Corruption	Contin.	[0;6]	>2.5	No diff.	No diff.	No diff.
Bureaucratic	Contin.	[0;4]	>1.5	No diff.	No diff.	No diff.
composite index	Contin.	[12;84.7]	No diff.	No diff.	No diff.	No diff.
Neighbors						
Neighbors' conflict (any)	Dummy	[0;1]	No conflict	No conflict	No diff.	No diff.
Share trade neighbors RTA	Contin.	[0;0.8]	>0.35	>0.08	No diff.	No diff.
Share trade neighbors	Contin.	[0;0.9]	No diff.	>0.24	No diff.	No diff.
Price transmission						
NRA (output + input)	Bins	[-0.9;1.4]	< or > 2nd tercile	No diff.	No diff.	No diff.
NRA (output)	Bins	[-0.9;3.5]	< or > 2nd tercile	No diff.	No diff.	No diff.

Note: the column range indicates the range of values taken by each variable in our sample; the column type groups the variables into three types, i.e. continuous, dummy (0 or 1) and bins (the variable is split into three mutually exclusive continuous ranges of values). The latter category is included for those variables for which the marginal effect of Px on conflict appeared to be non linear. The other columns report the values of the interaction for which the marginal effect of the relevant trade variable becomes not significantly different from zero (at the 10% level); "No diff." indicates either that the marginal effect is always or never statistically different from zero across the distribution of the interactions' values.

Table 9: Bayesian modeling average results

	Prob. of inclusion	Marginal effect	Standard error
Elections (t-1)	99.1	0.117	0.034
Ethnic fractionalization	66.1	0.166	0.068
Neighbors' conflict (any)	27.4	0.074	0.037
Any conflict in last 10 yrs	20.6	0.066	0.037

Note: only variables with at least 2,500 observations in our sample are included

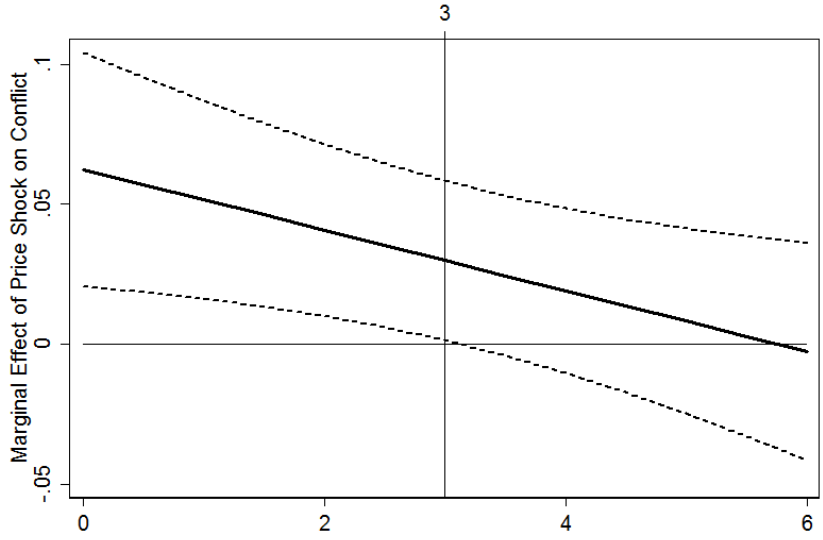
Table 10: Lebanon has a higher risk of conflict from hydrocarbons exports than the average country

	Mean	**Lebanon**	**Avg. Effect**	**LBN Effect**	**Signif.**
Inequality (Gini)	45.44	55.33	2.1%	4.2%	Y
Religious fractionalization	0.35	0.79	2.5%	5.7%	Y
Recent conflict	0.28	1	2.2%	4.1%	Y
Level of democracy	-1.21	7.00	1.9%	1.9%	N
Accountability	3.24	5.00	2.4%	0.9%	N
Neighbors in conflict	0.52	Yes	2.0%	2.3%	N
Share trade neighbors	0.12	0.03	2.0%	2.1%	N

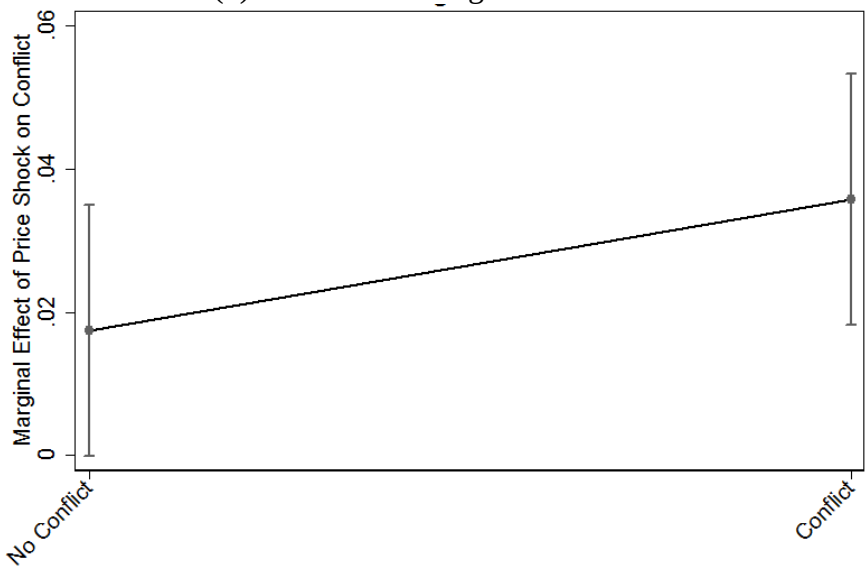
Note: Mean is the mean value of the variable across the entire sample over which the marginal effect of the export price index are computed in the regressions underlying Table 2.1; Lebanon is the value for Lebanon (latest available); Avg. effect is the increased probability of conflict due to a standard deviation increase in the point-source commodity export index measured for the mean value of the variable; LBN effect is the same increased probability measured at the value of the variable for Lebanon; Sig. indicates if the marginal effect of the variable is significant.
Source: Author's elaboration for the computations; for the mean and Lebanon values: Standardized World Income Inequality Database for inequality data; Alesina et al. (2003) for religious fractionalization; recent conflict: UCDP/PRIO (for mean) and ICRG for Lebanon (see text for more details); polity 2 data for level of democracy; ICRG for accountability; UCDP/Prio (for mean) and ICRG for neighboring conflict; COMTRADE for share of trade with neighbors.

Figure 1: Marginal effects of Px across the range of interaction variables' values

(a) Degree of accountability (1-6)



(b) Conflict in contiguous countries



Note: the bands indicate the confidence around the estimate line and points represent the 90% confidence interval

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Appendix

Table A1: Interaction variables for the trade shocks

Variable	Descrption	Source
Neighbors		
Neighbors' conflict (any)	Dummy equal to one if there is a conflict in a neighboring country.	PRIO
Share trade neighbors RTA	Share of trade with neighbors with an RTA.	COMTRADE + WTO
share trade neighbors	Share of trade with neighbors.	COMTRADE
Grievances		
Economic inequality	Dataset derived from the econometric relationship between UTIP-UNIDO, other conditioning variables, and the World Bank's Deininger & Squire data set.	EHII University of Texas
gini_net	Estimate of Gini index of inequality in equivalized (square root scale) household disposable (post-tax, post-transfer) income, using Luxembourg Income Study data as the standard.	SWIID
gini_market	Estimate of Gini index of inequality in equivalized (square root scale) household market (pre-tax, pre-transfer) income, using Luxembourg Income Study data as the standard.	SWIID
Ethnic fractionalization	The probability that two randomly selected individuals in a country will belong to different ethnolinguistic groups.	Montalvo and Reynal-Querol (2005)
Ethnic polarization	How far the distribution of the ethnic groups is from the bipolar distribution (i.e. 1/2, 0, 0, ... 0, 1/2)	Montalvo and Reynal-Querol (2005)
Religious fractionalization	The probability that two randomly selected individuals in a country will belong to different religious groups.	Montalvo and Reynal-Querol (2005)
Religious polarization	How far the distribution of the religious groups is from the bipolar distribution (i.e. 1/2, 0, 0, ... 0, 1/2)	Montalvo and Reynal-Querol (2005)
Any conflict in last 10 yrs	Dummy equal to one if there was a conflict in the last 10 years.	PRIO
Political system		
Elections	Dummy for election in that year	NELDA
Federal Govt.	Dummy for a government with a federal system.	Institutions and Elections Project
polity2	Captures the political regime characteristics.	Polity
parliamentary democracy	Dummy for being a parliamentary (from DPI) democracy (Polity).	DPI + Polity
Programmatic party	Share of the major four political parties with an ideological orientation with respect to economic policy, weighted by number of votes.	DPI
Age of party in office		DPI
Governance		
Law	Strength and impartiality of the legal system.	ICRG
military	Indicates the degree of military participation in politics.	ICRG

accountability	A measure of how responsive a government is to its people	ICRG
corruption	A measure of the level of corruption.	ICRG
bureaucratic	Indicates the "strength and quality of the bureaucracy".	ICRG
composite index	The mean value of the ICRG variables "Corruption", "Law and Order" and "Bureaucracy Quality".	ICRG
Price transmission		
nra_covt	Value of production-weighted average of covered products	Anderson et al. (2008)
nra_cov_o	Value of production-weighted average of covered products	Anderson et al. (2008)

Table A2: Trade variables without lag structure

	(1) any onset	(2) any onset	(3) major onset	(4) major onset
Px (t)	0.029***	0.028*	-0.000	0.005
	(0.010)	(0.015)	(0.005)	(0.009)
Pm (t)	-0.005	0.003	-0.007	-0.003
	(0.016)	(0.022)	(0.008)	(0.010)
MP (t)	-0.001	-0.021	0.003	-0.003
	(0.012)	(0.018)	(0.004)	(0.008)
Trade with neigh. (t-1)	-0.017	-0.060	-0.014	-0.030
	(0.048)	(0.056)	(0.029)	(0.036)
Controls	YES	YES	YES	YES
Observations	3,628	3,628	4,017	4,017
R-sq. (within)	0.074	0.152	0.025	0.059
Countries	115	115	115	115
Shocks	YES	YES	YES	YES
Time trends	logP	logP	logP	logP

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all regressions include country fixed effects, year effects and controls (any conflict since 1946 and war border (t-1)); trade shock variables are weighted using the slow-moving averages.*

Appendix only for online publication

Table: List of commodities used to generate Px and Pm and data availability

Commodity	Years	Commodity	Years	Commodity	Years	Commodity	Years
	[1962;		[1962;		[1962;		[1981;
Aluminium	2009]	Hides	2009]	Palm Oil	2009]	Steel	2009]
	[1962;		[1962;		[1996;		[1962;
Asbestos	2008]	Iron Ore	2009]	Palmkernel Oil	2009]	Sugar	2009]
	[1962;		[1962;		[1962;		[1962;
Bananas	2009]	Jute	2009]	Pepper	2009]	Sunflower Oil	2009]
	[1962;		[1962;		[1962;		[1962;
Barley	2009]	Lamb	2009]	Petroleum	2009]	Swine	2009]
	[1962;		[1962;		[1962;		[1962;
Beef	2009]	Lead	2009]	Phosphates	2009]	Tea	2009]
	[1962;		[1962;		[1962;		[1962;
Cashews	2009]	Linseed Oil	2009]	Platinum	2009]	Tin	2009]
	[1962;		[1962;		[1979;		[1962;
Coal	2009]	Live Cattle	2009]	Plywood	2009]	Tobacco	2009]
	[1962;		[1962;	Potassium	[1962;		[1962;
Cocoa	2009]	Live Poultry	2009]	Chloride	2009]	Tsp	2009]
Coconut	[1962;		[1962;		[1962;		[1962;
Copra Oil	2009]	Live Sheep	2009]	Poultry	2009]	Uranium	2009]
	[1962;		[1962;		[1962;		[1962;
Coffee	2009]	Live Swine	2009]	Pulp	2009]	Urea	2009]
	[1962;		[1962;		[1962;		[1962;
Copper	2009]	Lumber	2009]	Rice	2009]	Wheat	2009]
	[1962;		[1962;		[1962;		[1962;
Copra	2009]	Maize	2009]	Rubber	2009]	Wool	2009]
	[1962;		[1962;		[1962;		[1962;
Cotton	2009]	Manganese	2009]	Sawnwood	2009]	Zinc	2009]
	[1962;		[1962;		[1962;		
Dairy	2009]	Misc Meat	2009]	Shrimp	2009]		
	[1962;		[1962;		[1962;		
Diamonds	2009]	Natural Gas	2009]	Silver	2009]		
	[1962;		[1962;		[1962;		
Fish	2009]	Nickel	2009]	Sisal	2009]		
	[1962;		[1962;		[1962;		
Fishmeal	2009]	Olive Oil	2009]	Sorghum	2009]		
	[1962;		[1962;		[1962;		
Gold	2009]	Oranges	2009]	Soybean Meal	2009]		
Groundnut	[1962;		[1962;		[1962;		
Oil	2009]	Other Fruit	2009]	Soybean Oil	2009]		
	[1962;	Other Live	[1962;		[1962;		
Groundnuts	2009]	Animal	2009]	Soybeans	2009]		