Your Pain, My Gain?

Estimating the Trade Relocation Effects from Civil Conflict *

Tobias Korn Henry Stemmler

This version: March 25, 2022

Abstract

We derive a novel estimation approach to quantify three-party relocation effects in a dyadic framework. Applied to the effects of civil conflicts on trade, we find robust evidence that importers substitute away from exporters that are engaged in conflict. This trade relocation persists after the resolution of a conflict. As a potential explanation for the longevity of this effect, we provide evidence that trade relocation increases the likelihood of the two countries signing a Preferential Trade Agreement, which persistently decreases their bilateral trade costs. A heterogeneity analysis suggests that trade relocation does not occur in the fuels sector, and that highly integrated supply chains are less likely to relocate. We derive our estimation approach from the structural gravity model of international trade, translating the triadic relationship between a conflict country and an exporter-importer pair into an estimable dyadic relationship. Our estimation approach can be adapted to either cover alternative unilateral shocks, e.g. natural disasters, or to analyze other bilateral dependent variables, e.g. migration or FDI flows.

Keywords: Conflict and trade, trade diversion, gravity estimation, general equilibrium

JEL Classification: F14, D74, N41

---

*Korn: Institute of Macroeconomics, Leibniz University Hannover (*korn@mak.uni-hannover.de*) Stemmler: RTG1723 Globalization and Development, Göttingen University (*henry.stemmler@uni-goettingen.de)*.
We thank Axel Dreher, Martin Gassebner, Arevik Gnutzmann-Mkrtchyan, Daniel Mirza, Holger Strulik, and Yoto Yotov as well as participants at the GSIPE workshop 2021, the RIEF Doctoral Workshop 2021 and the HiCN Workshop 2021 for valuable feedback. Tobias Korn's work on this project was in part funded by the German Research Foundation (DFG, grant BL-1502/1-1). This study was furthermore funded by the DFG - project RTG 1723 in the framework of the research training group on "Globalization and Development".

1 Introduction

In a globalized world, bilateral decisions do not happen in isolation. For instance, realized trade flows are outcomes of competition, financial flows depend on expected returns of different investment opportunities, and migration flows are shaped by distances and the attractiveness of alternative options. International trade theory subsumes such effects under the term of multilateral resistances (Anderson and van Wincoop, 2003); each importer's and each exporter's average access to all other trade partners determine the value of bilateral trade. Hence, events that change the competitiveness of any relevant third party can have significant effects on bilateral trade flows between two countries. Estimating such indirect effects requires empirically isolating meaningful events in third countries that are likely to influence the dyadic relationship of two other countries. In this paper, we develop a novel strategy to estimate the effect of third party events on bilateral outcomes and use it to estimate the trade relocation effects of civil conflicts. This estimation procedure can easily be adapted to other unilateral shocks as well as alternative bilateral outcome variables.

Violent conflicts are known to displace people and heavily interrupt national production chains (see, e.g., Blattman and Miguel, 2010, Verwimp et al., 2019). Similarly, evidence abounds that civil conflicts significantly hurt countries' exports (see, e.g., Martin et al., 2008a, Novta and Pugacheva, 2021). In this paper, we estimate the trade relocation effects of such unilateral economic disruptions by investigating how civil conflicts shift global trade networks. In essence, we analyze whether and under which circumstances importers divert their demand from a conflict country to another, peaceful country. Our main results are based on Partial Equilibrium (PE) structural gravity estimations using bilateral trade data for over 150 countries during the period from 1995 to 2014. To augment the typical dyadic gravity specification by variation from a third country, we define a "relocation propensity" variable that indicates whether a dyad is likely to be subject to trade relocation from conflict in another country. This indicator variable combines yearly information on the relationship between any conflict country and the two countries in a given dyad. A given dyad is considered as likely to be affected by trade relocation if (1) a conflict country used to be a relevant exporter for the dyad's importer, and (2) that dyad's exporter offers a variety of goods similar to that of the conflict country.

On average, we find bilateral trade to increase by up to 6% in response to civil conflict in another country. Our analysis further reveals a significant heterogeneity with respect to the traded sector. We find that trade in agricultural, mineral, and manufacturing goods exhibit a trade relocation effect of up to 13%, whereas fuel exports do not respond at all. The fact that the international fuel trade does not react to civil conflict reflects the dependence of importers on specific suppliers of fuel exports. This was very well demonstrated recently by the European Union's hesitation to stop oil and gas imports from Russia in the light of Russia's invasion of Ukraine.[1] Similarly, oil and gas exports are of special financial importance for belligerents on either side of a conflict, who have an interest in maintaining fuel shipments.[2] Second, in the agricultural and manufacturing sectors, trade relocation only occurs if the prior value chain integration via Foreign Direct Investment (FDI) was negligible. However, the effect is the opposite in the minerals sector, where large amounts of FDI are associated with higher trade relocation. This difference may be driven by the mining sector's vulnerability to civil conflict, as especially foreign-owned mines attract violence (Berman et al., 2017). A final heterogeneity is the timing of the relocation decision. We find that in the minerals and manufacturing sectors, relocation is stronger after long conflict spells. This finding is in line with recent research in the business literature. Especially Multinational Enterprises (MNEs), who incorporate the threat of political tensions in their location decision of FDI, must weigh the costs from staying versus the costs of relocating. Depending on their vulnerability to conflict and local advantages for production, resuming production in a conflict zone can be the better option (Dai et al., 2017). For some, the possibility to stay is even worth investments to promote peace (Oetzel and Miklian, 2017).

Once a firm relocates its production sites or finds new providers of (intermediary) goods in another country, it has economic incentives to lobby for better and cheaper market access. Hence, trade relocation may persist after the end of a civil conflict if trading costs remain decreased. In a recent study, Freund et al. (2021) provide case study evidence for this argument for the automotive sector in response to Japan's 2011 earthquake. In our generalized setting, we find that trade flows remain relocated for up to nine years after the end of a civil conflict. This effect is mostly driven by the manufacturing sector. As a possible channel to explain this result, we find that a civil conflict in one country makes its main importers more likely to form Preferential Trade Agreements (PTAs) with alternative exporters. This supports the intuition that the persistent relocation is fostered by deeper market integration, and follows the idea of endogenous RTA formation (see, e.g Egger et al., 2008). In the end, international markets find themselves in a new equilibrium (Allen and Donaldson, 2020). Our findings suggest that civil conflicts can harm economic development in the long-run as trade flows remain diverted away from the conflict country, underlining the view of civil conflict as "development in reverse" (Collier et al., 2003).

Finally, we conduct a General Equilibrium (GE) analysis based on the recent civil war

[1] https://www.economist.com/the-economist-explains/2022/02/26/if-the-supply-of-russian-gas-to-europe-were-cut-off-could-lng-plug-the-gap (last accessed March 23, 2022).

[2] See https://www.economist.com/middle-east-and-africa/2014/11/01/a-sticky-problem as an example (last accessed February 15, 2022).

episodes in Colombia, Ukraine, and Turkey as case studies.[3] These case studies confirm our PE findings and indicate that importers who used to rely heavily on shipments from the conflict countries switch to shipments from alternative exporters. What is more, we estimate changes in overall national welfare measured by total consumption expenditures in response to these conflicts. Here, we find that national welfare decreases for almost all countries involved, even for those exporters on the receiving end of the relocated trade flows. This suggests that trade relocation cannot fully offset the global loss in economic activity.

Our paper contributes to several strands of the literature. First, we contribute to the literature investigating trade relocation effects. Since Anderson and van Wincoop (2003) pointed out the importance of multilateral resistance terms in the structural gravity framework, it is widely accepted that international competition is a decisive determinant of bilateral trade. The empirical trade literature provides various insights into the trade relocation effects of PTAs. Several papers provide evidence that PTAs increase trade flows between signees ("trade creation") while decreasing trade between any signee and non-signees ("trade diversion"). Among others, Dai et al. (2014) and Cheong et al. (2015) analyze how PTAs shift international trade flows by focusing on dyads in which one country joined a PTA and the other did not. We go one step further and measure trade relocation in a *triadic* relationship. That is, we estimate the effect of country A's economic shock on bilateral trade between countries B and C. The empirical specification we develop allows to include unilateral shocks that occur outside an observed dyad. While we apply this strategy to civil conflict as a shock and bilateral trade as an outcome variable, the same specification can be applied to alternative bilateral dependent variables like migration or financial flows, as well as to different unilateral shocks such as climate shocks (Jones and Olken, 2010), resource windfalls (Bahar and Santos, 2018), taxes and regulations (Grubert and Mutti, 1991, Emran, 2005), or currency devaluations (Krugman and Taylor, 1978, Rose, 2018).

Second, we add to the evidence of how civil wars affect the international economy. Recent findings emphasize that civil wars depress the quantity and prices of exported goods (Ksoll et al., 2018, Ahsan and Iqbal, 2020). These effects are not bound to the conflict country but often spill over to neighboring countries (Qureshi, 2013, De Sousa et al., 2018). Especially in the case of transnational terrorism, protective countermeasures persistently complicate the exchange of goods, multiplying the direct effects of violence (Mirza and Verdier, 2014). Similarly, international wars as well as non-violent disputes between countries reduce bilateral trade (Fuchs and Klann, 2013, Garfinkel et al., 2020a). These effects further persist when conflict erodes trust between parties (Rohner

[3]We selected these case studies as they constitute the most significant spikes in violence according to UCDP data which have clear start and/or end points during our period of observation.

et al., 2013). However, improved trade relationships can decrease the likelihood that international wars break out as gains from trade increase the opportunity-costs of starting a war (Martin et al., 2008a,b, 2012, Garfinkel et al., 2020b). Trade restrictions and competition can even foster political violence (Amodio et al., 2020). We extend this line of the literature by considering the general equilibrium effects of civil conflict. As international markets are tightly linked, civil conflicts are hardly a unilateral or bilateral phenomenon. By providing evidence that civil wars can affect trade flows between other, peaceful countries and provoke shifts in the international equilibrium, we consider new economic consequences from political violence.

Finally, our findings add to the discussion about the persistence of the economic consequences of civil violence. According to economic theory, an economic shock should affect nations only in the short-run, while their economy rapidly recovers after the conflict is resolved (Barro and Sala-i-Martin, 1992, Mankiw et al., 1992, Blattman, 2012). These theoretical considerations receive support from several empirical findings (see, e.g., Davis and Weinstein, 2002, Brakman et al., 2004, Miguel and Roland, 2011). However, recent micro-level evidence points toward a persistent effect of civil conflict on affected individuals (Akresh et al., 2012, Brück et al., 2019, Tur-Prats and Valencia Caicedo, 2020). We contribute to this literature by pointing out that general equilibrium effects can cause the effects of civil conflict to persist. Our findings suggest that temporary trade relocation fosters market integration via PTAs, which in turn leads to persistent trade diversion away from the (former) conflict country.

The rest of the paper is structured as follows. In the next section, we derive our empirical specifications from the structural gravity model of international trade and introduce our dataset. Afterwards, section 3 discusses our main results. Section 4 presents several extensions to our main estimations. Finally, we will discuss a number of robustness checks in section 5, before section 6 concludes.

2 Estimation and Data

Our analysis follows the structural gravity model of international trade derived in Anderson and van Wincoop (2003) and Anderson (1979), based on Armington (1969). Our main estimations are conducted at the sector level, where we observe trade in the manufacutring, agriculture, mining, and fuels sectors separately. We follow Anderson et al. (2018) and describe the exports of a variety of goods in sector s from country i to country j in year t with the equation:

$$X_{ijs,t} = \frac{Y_{is,t}E_{js,t}}{Y_{Ws,t}} \cdot \left[\frac{t_{ij,t}}{\Pi_{is,t}P_{js,t}}\right]^{1-\sigma} \quad (1)$$

Exports $X_{ijs,t}$ are positively related to the product of the exporter's level of production $Y_{is,t}$ and the importer's consumption expenditures $E_{js,t}$, relative to total world output $Y_{Ws,t}$ in that sector. Trade flows further depend on the bilateral "iceberg costs" of trade, denoted by $t_{ij,t}$. This term covers, among other things, the distance between two countries or the amount of tariffs paid on shipments. With the elasticity of substitution across varieties $\sigma > 1$, bilateral exports $X_{ijs,t}$ are negatively linked to the trade costs $t_{ij,t}$. Finally, bilateral trade depends on the multilateral resistances faced by the exporter and importer, respectively. The outward multilateral resistance Π_{is} describes the exporter's average (inverse) market access to all potential importers. The inward multilateral resistance P_{js} similarly describes the importer's average (inverse) market access to all potential exporters. Both these variables can be thought of as the competition on international markets in sector s that either i or j face with any other country to trade with country j or i, respectively.

We follow Head and Mayer (2014) in transforming Equation (1) into an estimating equation and include exporter-sector-year fixed effects $\pi_{is,t}$, importer-sector-year fixed effects $\lambda_{js,t}$, and sector-dyad fixed effects μ_{ijs}. We further decompose the iceberg trade costs into a time-varying and a time-invariant component: $t_{ij,t} = \bar{t}_{ij} + \tau_{ij,t}$. Conditional on the fixed effects, the time-varying component $\tau_{ij,t}$ is the only remaining variation in Equation (1) that affects bilateral trade between countries i and j. We control for bilateral trade agreements and sanctions, which are two important components of $\tau_{ij,t}$ as recently advocated in the literature (see, e.g., Dai et al., 2014, Felbermayr et al., 2019). Finally, we add an indicator $TR_{ijs,t}$ to identify dyad-sector-year observations that are likely to be affected by trade relocation effects. We arrive at the estimating equation:

$$X_{ijs,t} = exp\left[\pi_{is,t} + \lambda_{js,t} + \mu_{ijs} + \beta \cdot TR_{ijs,t} + \gamma \cdot Z_{ij,t}\right] + \eta_{ijs,t} \tag{2}$$

where $Z_{ij,t}$ is a vector of dyadic control variables, including bilateral trade agreements and sanctions, and $\eta_{ijs,t}$ accounts for the remaining variation in $X_{ijs,t}$ that is not explained by the fixed effects and independent variables. The challenge is to incorporate civil conflicts that take place in a third country k in the variable $TR_{ijs,t}$. In theory, we expect a civil war in country k to enter bilateral trade between countries i and j via the multilateral resistance terms P_{js} or Π_{is} in Equation (1). Since multilateral resistance terms are not observable, we develop a quantifiable proxy measure for the relocation propensity, which can be derived via $P_{js,t}$ or $\Pi_{is,t}$ based on the structural gravity model of international trade (see Appendix C for the formal derivation). We arrive at the following estimation equation for the trade relocation effect from conflict in country k on the dyad ijs:

$$X_{ijs,t} = exp\left[\pi_{is,t} + \lambda_{js,t} + \mu_{ijs} + \beta \cdot \sum_k (R_{jks,t-2} \times S_{ik,t-2} \times C_{k,t-1}) + \gamma \cdot Z_{ij,t}\right] + \eta_{ijs,t}. \quad (3)$$

Our coefficient of interest β indicates how bilateral trade between countries i and j in sector s reacts to conflict in another country k. We approximate relocation propensity as an interaction of three variables: (i) the conflict status of any country $k \neq i, j$, denoted by $C_{k,t-1}$, (ii) the relevance of country k as an exporter for country j in sector s, $R_{jks,t-2}$, and (iii) the similarity between exporters i and k, $S_{ik,t-2}$. Note that we lag conflict by one year and the relevance and similarity conditions by two years to (i) leave time for trade relocation effects to materialize and (ii) use country characteristics before the conflict in country k.[4]

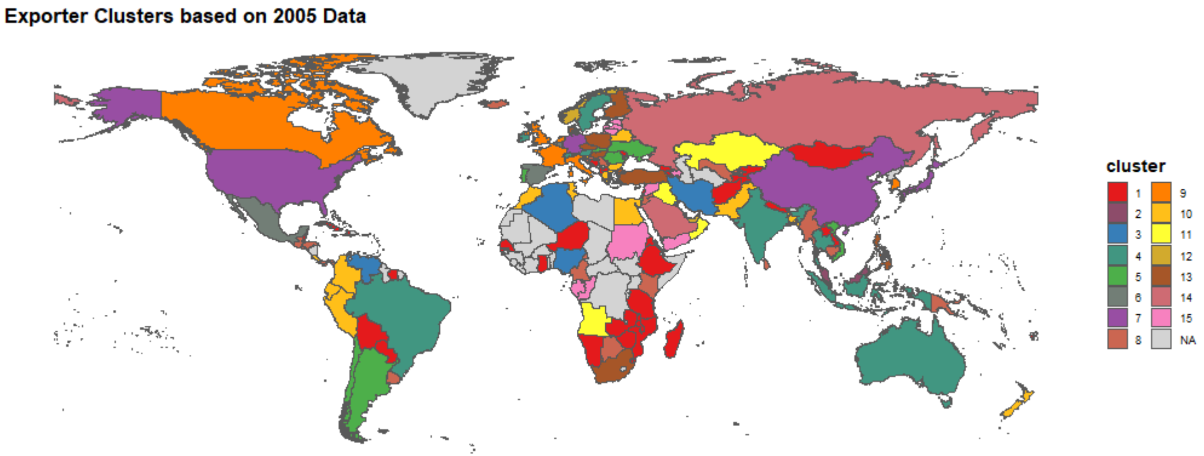
The relevance characteristic $R_{jks,t-2}$ indicates whether country j used to import relatively large amounts from country k in sector s prior to the conflict. We start by defining $R_{jks,t-2}$ broadly, indicating whether country k was among the top 7 exporters to country j in sector s.[5] Other measures, for instance top 5 or 10 exporters, are used as robustness tests. For an indicator of similarity $S_{ik,t-2}$, we leverage different variables to identify whether two countries i and k were exporters of similar goods before the conflict broke out in country k. All variables are based on disaggregated export data for 61 product lines according to the SITC classification. First, we construct clusters of countries with similar export structures. We apply a K-Means clustering algorithm as developed by Hartigan and Wong (1979), which allocates countries according to their similarity in production to a pre-defined number of clusters. For our preferred specifications, we divide all exporting countries in a given year into 15 or 20 different clusters. Our method is similar to the process applied by Kim et al. (2020), who assign trade-dyads to clusters according to the similarity in the sectoral composition of their trade flows. As this method of assigning export clusters inherits some degree of randomness, and there is no clear candidate for a "perfect" number of clusters, we test for robustness across different cluster sizes. Figure 1 below illustrates the allocation of clusters for the year 2005. To test whether our similarity measure is robust to other specifications, we additionally construct an export similarity index following Benedictis and Tajoli (2007a,b), which mirrors the correlation between sectoral exports across countries.

We then determine relocation propensity via the triple interaction of the indicators for relevance $R_{jks,t-2}$, similarity $S_{ik,t-2}$, and conflict $C_{k,t-1}$. In other words, whenever *all* three indicator variables take the value of one for *any* country k, our relocation

[4] As a robustness test, we also examine trade flows in the same year as the conflict takes place.

[5] In our sample, the top 7 exporters are on average responsible for the first quartile (25%) of a country's imports.

Figure 1 – Export Similarity Clusters



Notes: This graph shows the distribution of export clusters among 179 countries based on a K-Means clustering algorithm with 15 random centers in the year 2005. The algorithm randomly picks 15 countries as centers and then assigns all remaining countries to the center with the most similar export structure. Similarity is computed based on export-data for 61 product lines based on the SITC classification.

propensity variable takes the value of one for the ijs dyad.[6] We hence identify a positive relocation propensity as the specific case that conflict country k was a significant trading partner of importing country j in sector s in the past, *and* this same country k used to offer a similar variety of goods to exporter i.

A causal interpretation of our results requires the unexplained variation captured by the error term $\eta_{ijs,t}$ to be uncorrelated with our relocation propensity variable, conditional on our control variables and fixed effects. We hence must rule out that unobserved, non-random characteristics captured by $\eta_{ijs,t}$ are associated with a higher likelihood that our relocation propensity variable takes the value of one. For this, it is important to note that none of the three ingredients to our relocation propensity variable is dyad-year-specific. First, the incidence of civil conflict in country k, $C_{k,t-1}$, is an event observed by all dyads in a given year t and hence controlled for by year fixed effects. Second, the relevance characteristic $R_{jks,t-2}$ is specific to a dyad's importer only and hence does not vary across an importer's export partners in a given year. Hence, importer-sector-year fixed effects account for all characteristics that make an importer more likely to experience trade relocation from a given conflict country k. The same argumentation holds for the similarity condition $S_{ik,t-2}$ at the exporter side, which is controlled for by exporter-sector-year fixed effects. Finally, characteristics that are specific to a given dyad and might increase its average propensity that both $R_{jks,t-2}$ *and* $S_{ik,t-2}$ are one is accounted for by dyad fixed effects. A potential bias in

[6] Note that this constitutes the extensive margin, coding a dyad as subject to trade relocation if they are affected by at least one conflict. In the Appendix, we provide results for the intensive margin, using the number of identified relocation possibilities as the explanatory variable. While the results are very similar, we prefer the extensive margin due to the easier interpretation of the results.

our estimates hence requires the presence of unobserved characteristics that vary at the dyad-sector-year level and correlate with the interaction of our relevance and similarity conditions. One potential caveat could be, for example, that our results are mainly driven by one of the two variables, while the other only generates minimal identifying variation. In Appendix F, we therefore provide an in-depth discussion of the determinants and variation of both the relevance and similarity conditions and demonstrate that both variables exhibit sufficient variation. Furthermore, we provide several robustness checks below which demonstrate that both conditions are required together to estimate a significant relocation effect. Another caveat could be that during years when a conflict is active in a country that is relevant to a dyad's importer and similar to its exporter, the dyad's preferences for trading with each other systematically increase for reasons other than the civil conflict in the third country. One such possibility could be that importers apply bilateral sanctions to countries that are linked to the conflict country k, but strategically spare countries they identified as potential export substitutions for k. Here, it is reassuring that controlling for various types of sanctions leaves our results qualitatively unchanged. Our results are further not sensitive to controlling for pre-existing observable trade preferences in the form of PTAs. The non-sensitivity of our results to the inclusion of these bilateral, time-varying control variables makes us confident that the likelihood that unobserved characteristics are correlated with our trade relocation variable is low. Finally, we view reverse causation as an unlikely threat to our identification. Reverse causation would require that bilateral trade flows between two countries are significantly linked to the likelihood that a civil conflict emerged in another country that is relevant to the dyad's importer and similar to its exporter two years prior. While there is evidence that the US staged coups to increase trade with conflict countries (Berger et al., 2013), we are not aware of any evidence or anecdotes that governments stage civil wars in third countries to increase exports to or imports from a specific other, non-conflict country.

Our empirical analysis draws from various data sources related to civil conflict and international trade. For our main analysis, we include trade data for the manufacturing and primary sectors. Addressing the primary sector separately is important as civil conflicts predominantly erupt in resource-abundant countries (Ross, 2015).[7]

Manufacturing data come from the Comtrade dataset, which includes bilateral trade flows between 1980 and 2018 of approximately 180 countries.[8] As a measure for trade in primary goods, we use commodity trade data from CEPII's BACI dataset, which consists

[7]The relationship between natural-resource abundance and the likelihood of conflict depends on several factors, such as political stability, inequality, or type of resources (see, e.g. Bazzi and Blattman, 2014, among others).

[8]Trade values are primarily measured through imports, as these are usually more precisely computed. We complement missing import data with exports between the same dyad and year to maximize coverage.

of yearly bilateral trade-flows at the 6-digit HS level. According to recent advancements in the international trade literature, bilateral trade flows alone are not sufficient for a reliable empirical analysis. As Yotov (2021) shows, international trade flows need to be complemented with intra-national trade data to obtain unbiased and consistent estimates within the gravity framework. Unfortunately, the availability of consistent internal production data is still limited. Therefore, we combine several data sources to maximize the coverage across countries, sectors, and years. We follow the literature in computing internal trade flows (Baier et al., 2019). For the manufacturing sector, we compute internal trade as the difference between total manufacturing production and total manufacturing exports. To quantify total manufacturing production, we draw on data from the INDSTAT database. We proceed similarly to compute internal trade flows in the primary sector. Here, we use commodity production data from Fally and Sayre (2018). The authors combine production data of minerals, agricultural commodities and fuels from the British Geological Survey, the FAO and the Global Trade Analysis Project (GTAP). Based on these data, we compute internal trade flows for about 200 countries and across 169 commodities between 1995 and 2014. We complement our dataset with information on PTAs from CEPII's Gravity database.[9]

To identify civil conflict, we use the UCDP/PRIO Armed Conflict Dataset version 19.1 (Sundberg and Melander, 2013). We follow the established definition and code a country to experience a civil war in a given year if it has experienced violent events between government troops and a non-governmental entity, and if the number of battle deaths exceeded the threshold of 25 casualties. Our main dataset comprises 179 countries over the years 1995–2014. Table A1 reports descriptive statistics of our main variables.

3 Main Results

Table 1 presents our main results, which are obtained by estimating Equation (3). Panel A provides results based on 15 clusters for the similarity definition and Panel B on 20 clusters. The results in column 1 are based on estimations across all sectors within a respective dyad, which include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed-effects and control for bilateral trade agreements and sanctions on the exporter side. We find a statistically significant and positive trade relocation effect. On average, civil conflict increases trade between two other countries by 6%.[10] In columns 2-5, we investigate relocation effects by sector. Notably, we do not find any evidence of trade relocation in the fuels sector (Column 4). This finding is intuitive as fuel exports

[9]The PTA variable is based on the RTA-IS dataset of the World Trade Organization (WTO) and is constructed out of information on Partial Scope Agreements (PSA), Free Trade Agreements (FTA), Customs Unions (CU) and Economic Integration Agreements (EIA).

[10]The results are basically identical if we do not control for PTAs or sanctions (not shown for brevity).

commonly do not decrease during civil conflict in the first place. As we discuss in more detail in Appendix E, we find that civil conflicts depress exports in all sectors but in fuels, which confirms prior empirical and anecdotal evidence that warring parties have a joint interest of keeping up oil exports to finance their war efforts (Bazzi and Blattman, 2014). In the three other sectors, civil conflict in country k has a robust and significant effect on exports from country i to j, increasing bilateral shipments between 7% (manufacturing) and 13% (mining and agriculture).

We conclude from these findings that outside the fuels sector, civil conflicts provoke sizeable trade relocation effects. Next, we investigate how long these effects persist. Do trade flows return back to normal when the conflict is resolved?

Table 1 – Trade relocation main results - conflict in top 7 trading partner countries

Dependent:	Exports from country i to country j				
	Pooled	Agricult.	Minerals	Fuels	Manufact.
	(1)	(2)	(3)	(4)	(5)
Panel A:	15 clusters				
Conflict in country k	0.06***	0.12***	0.13***	-0.02	0.07**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Panel B:	20 clusters				
Conflict in country k	0.05**	0.13***	0.10***	-0.02	0.07**
	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
Observations	1269742	366662	322631	89446	491003
Exporter × sector × year	✓	✓	✓	✓	✓
Importer × sector × year	✓	✓	✓	✓	✓
Exp. × Imp. × sector	✓	✓	✓	✓	✓
PTA	✓	✓	✓	✓	✓
Sanctions	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, pooled over all sectors in column 1 and disaggregated by sectors in columns 2-5. The explanatory variables take a value of 1 if (i) country k had a conflict in the previous year, (ii) country k was a top-7 exporter for country j in the pre-conflict-year and (iii) country k and country i were similar exporters in the pre-conflict-year. Similarity is measured by being in the same exporter-cluster, with a total number of 15 clusters in Panel A and 20 clusters in Panel B. We estimate all specifications with the PPML estimator and include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed effects.

Standard errors in parentheses are clustered at the dyad-sector level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Considering the various micro-economic mechanisms that play out when trade flows relocate from one country to another, there is reason to expect that a temporary relocation can become persistent. As soon as retailers or producers of country j start importing their goods from country i instead of country k, they establish new connections and trade networks with exporting firms in country i. Companies in countries i and j integrate their supply chains and establish international branches via FDI. These newly established connections may, in turn, induce national governments to sign new trade agreements with each other. This re-drawing of international cooperation may persistently decrease bilateral trade costs. According to the dynamic equilibrium theory, a one-time shock can hence alter allocations and bilateral preferences such that economies end up converging to a new long-run equilibrium (Allen and Donaldson, 2020). In our case, this means that new supply chains and trade agreements tend to stay in place when a conflict ends, and trade relationships are unlikely to return to pre-conflict levels once country k resolves its conflict. Such a restructuring of international trade flows can hence exacerbate the conflict trap by pushing countries into the fringe of international trade, which is one explanation why conflict-ridden countries lack economic development in the long-run (Collier et al., 2003).

We analyze relocation persistence by estimating specifications similar to those presented in Equation (3). Instead of an indicator variable for country k being at war, we code how many years back exporter k's civil war ended. Moreover, to consistently define the similarity and relevance conditions over time, we use the values from the year prior to conflict onset in country k. Only for cases with very long conflict spells, we use the values from five years before the conflict ended, as going too far back would mask changes in countries' production structures that are unrelated to conflict.[11] We depict our results in Figure 2.

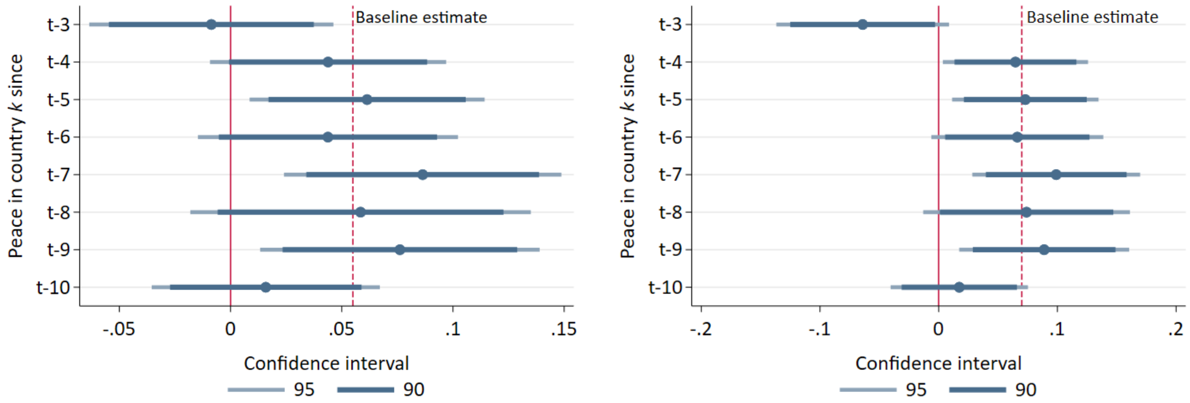
Panel (a) displays estimates for shipments from exporter i to importer j considering all sectors. The specifications include the same fixed effects and control variables as our main results in Table 1. We find that up to nine years after the end of a conflict in country k, shipments from i to j remain significantly higher than before the conflict. This effect is mainly driven by the manufacturing sector. Whereas the confidence intervals for the pooled sample are wide and do not always exclude zero, the persistence estimates for the manufacturing sector only hint at a statistically significant increased bilateral trade value of almost one percent for most lags (see Panel (b)).[12] For $t-3$, our estimate is however insignificant and even tilts towards the negative. One possible explanation for the insignificant result in $t-3$ might be our coding of peace. For a country to be coded peaceful for $t-\tau$ years, we require the absence of violent activity in the country

[11] Take as an example Bangladesh, which was at conflict in most years during the 1990s and early 2000s, but at the same time underwent a period of large industrialization and globalization. Using its pre-conflict production portfolio to identify similar exporters in 2010 or later would likely not give a realistic picture.

[12] We do not find a statistically significant effect of relocation persistence in the other sectors (not shown). As the time frame of our data is relatively short, the occurrence of long time-spells after conflicts is however limited.

Figure 2 – Relocation persistence

(a) Trade relocation (pooled) **(b)** Trade relocation (manufacturing)



Notes: This figure shows the coefficients and confidence intervals from regressing exports from country i to country j to various lags of our relocation propensity variable. Results in Panel (a) stem from regressing trade in all sectors on conflict in country k that ended in year t-τ, as in column 1 of Table 1. Panel (b) provides similar results specifically for the manufacturing sector as in column 5 of Table 1. The dashed vertical lines represent the baseline estimates from columns 1 and 5 of Table 1, respectively. The light and dark blue lines depict 95% and 90% confidence intervals, respectively.

for τ years. Many salient conflicts follow an on-off nature, with violent attacks in some years, and no attacks in others (Bluhm et al., 2021). Such on-off conflicts will be part of our coding for rather short periods of peace of up to three years, but not for longer periods. What is more, we discuss in the next section that manufacturing trade relocates rather slowly after conflict onset. Therefore, on-off conflicts are less likely to cause trade relocation in the first place, which adds noise to our short-run persistence estimates.

Table 2 – Linear Probability Model: Forming new Prefrential Trade Agreements

Dependent:	Likelihood of PTA between country i and country j (0-100)							
	Agricult.		Minerals		Fuels		Manufact.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conflict in country k	0.23 (0.17)	0.23 (0.17)	0.36** (0.18)	0.35** (0.18)	-0.06 (0.21)	-0.06 (0.21)	1.15*** (0.25)	1.15*** (0.25)
Observations	546129	546129	490379	490379	356116	356116	519129	519129
Exporter × year	✓	✓	✓	✓	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓	✓	✓	✓	✓
Exp. × imp.	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓

Notes: This table reports results from Linear Probability Models with the likelihood that country i and country j enter a trade agreement as the dependent variable. The explanatory variables is constructed as in Table 1, with the top-7 exporters defining relevance, and using 20 clusters to define similarity. All estimations include exporter-year, importer-year and exporter-importer fixed effects. We control for prior trade agreements and bilateral sanctions. Standard errors, in parentheses, are clustered at the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01

Persistent trade relocation can come about as countries i and j persistently decrease their bilateral trade costs. During k's civil war, the two countries have an incentive to tighten their trade relationships, something we can observe via the formation of new trade agreements. We construct sector-specific relocation propensity measures as in our main analysis and run bilateral OLS regressions with an indicator variable for having a PTA as the dependent variable. We report the results in Table 2, where we multiply the coefficients by 100 to ease display. We find a significant increase in the likelihood of entering a PTA if countries experience trade relocation in the minerals or manufacturing sector. If relevant exporters of manufacturing goods suffer from civil war, the chances to sign a PTA with another, similar exporter increases by up to 1.15%, with a similar increase of around 0.36% for mineral exporters. These effects are sizeable compared to an average likelihood of given dyad trading under a PTA in a given year of around 12.5%. For the agricultural and fuels sectors, we do not find significant evidence that trade relocation fosters market integration.

4 Extensions

To better grasp the mechanisms that lead to (persistent) trade relocation, we consider various extensions to our baseline estimates.

First, we investigate conflict duration. In Table 3, we extend our main specification by an indicator variable for trade relocation propensity from a conflict that already lasted more than ten years. The results reveal a noticeable difference across sectors. For minerals and manufacturing, we see that trade relocation occurs especially after long conflict periods, while the opposite is true for the agricultural sector. We interpret this as evidence that in the former two sectors, firms try to keep their supply chains intact during short periods of violence. Only when violence persists, firms move their production facilities to other countries. This finding is further in line with our result that trade relocation only fosters market integration via PTAs in the manufacturing and minerals sectors. It is fair to assume that firms optimizing their supply chains would lobby for cheaper access to alternative trading partners before relocating their supply chains. In agriculture, shifting supply chains may well be cheaper and easier than in the other sectors.

In Table 4, we analyze whether trade relocation varies conditional on the importance of country k as an FDI destination for firms from importer j. We would expect that substantial amounts of capital invested in conflict country k would reduce the incentive to switch trade partners. We define country k as an important FDI destination if it received more than 10% of importer j's total FDI prior to the civil conflict. In odd columns, we report trade relocation estimates for important FDI-destinations, while

Table 3 – Relocation heterogeneity: Conflict duration

Dependent:	Sectoral exports from country i to country j			
	Agricult.	Minerals	Fuels	Manufact.
Conflict in country k	0.11**	0.02	0.03	0.04
	(0.05)	(0.03)	(0.03)	(0.03)
Conflict is longer than 10 years	0.04	0.17***	-0.26***	0.11**
	(0.04)	(0.04)	(0.10)	(0.05)
Observations	366662	322631	89446	491003
Exporter × year	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓
Exp. × Imp.	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: This table reports PPML results from regressing bilateral exports on relocation propensity. The explanatory variable "Conflict in country k" is constructed as in Table 1, with the top-7 exporters defining relevance, and using 20 clusters to define similarity. We add another relocation propensity indicator for relocation from conflicts that lasted more than 10 years. If there are multiple conflict countries k, we use the shortest duration. All estimations include exporter-year, importer-year and exporter-importer fixed effects. We control for trade agreements and bilateral sanctions. Standard errors, in parentheses, are clustered at the exporter-importer level, * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$

even columns focus on trade relocation away from countries without a significant share of FDI. In the agricultural and manufacturing sectors, we find the expected effect that only less relevant FDI destinations cause trade relocation. In the minerals sector, we find the opposite result; if conflict country k received significant FDI inflows from firms in country j, imports are *more likely* to relocate to another exporter i. This result might hint at the vulnerability of mining-sector FDI to civil conflict. Recent evidence suggests that natural resource mines are preferred targets of violent groups (Berman et al., 2017). The destruction of foreign-held capital together with a more insecure environment for (new) investments might hence encourage firms to divert both FDI and imports to other countries. Furthermore, investments in the mining sector are more mobile. Whereas agricultural and manufacturing FDI usually involves acquiring land and building plants, mining-FDI often focuses on mining equipment which can easily be moved across borders.

In Table A2 in the Appendix, we test for heterogeneity with respect to the characteristics of traded goods. In Panel A, we distinguish between exports of commodities that are common (exported by several countries) or rare (exported by only a handful of countries) in the agricultural and minerals sectors.[13] In the minerals sector, trade relocation is only observable for very common commodities. In the agricultural sector, no coefficient is statistically significant, possibly due to a lack of variation in our independent variable as (very) common agricultural commodities are exported by almost all countries.

[13]Restricted data availability in the manufacturing sector does not allow to make the same comparison for this sector.

Table 4 – Relocation heterogeneity: FDI destination

Dependent:	Sectoral exports from country i to country j							
	Agricult.		Minerals		Fuels		Manufact.	
Sign. FDI (j to k):	No	Yes	No	Yes	No	Yes	No	Yes
Conflict in country k	0.08*** (0.02)	0.05 (0.05)	0.02 (0.02)	0.25*** (0.09)	-0.01 (0.03)	-0.13 (0.12)	0.06*** (0.02)	-0.03 (0.03)
Observations	212285	189594	188137	167925	49705	43403	277556	226926
Exporter × year	✓	✓	✓	✓	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓	✓	✓	✓	✓
Exporter × Importer	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table reports OLS estimates of the effects of conflict in country k on exports from country i to country j. The explanatory variables is constructed as in Table 1, with the top-7 exporters defining relevance, and 20 clusters defining similarity. To analyze the heterogeneity w.r.t FDI, we interact the explanatory variable with a dummy indicating that importer j has at least 10% of its FDI value in country k. This interaction variable takes the value of 1 in odd columns and 0 in even columns. All estimations include exporter-time, importer-time and exporter-importer fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered at the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01

In Panel B, we differentiate between intermediate and final goods. In the agriculture and mining sectors, we only find significant evidence for trade relocation among intermediate goods. For final goods, our estimates yield relatively precise zeroes. Likely, global value chains that rely on agricultural and mining commodities have the capacity and/or economic interest to pursue a quick substitution of export partners. For manufacturing goods on the other hand, we estimate a large and highly significant trade relocation effect for final goods, but an insignificant effect for intermediate goods. A likely explanation for this finding is that the final process in the manufacturing supply chain, which mainly consists of assembling ready-made parts, can more easily be offshored if civil conflict mandates relocation.

Finally, we conduct general equilibrium welfare computations for three case studies. We look at the recent peaks of civil violence in Colombia, Ukraine and Turkey, and estimate (i) changes in worldwide bilateral trade flows and (ii) changes in countries' overall welfare. We discuss the general equilibrium analysis in more detail in Appendix G. Based on the methodology discussed in Baier et al. (2019), we first estimate how civil conflict affected the conflict country's overall exports, and then use this estimate to compute hypothetical trade flows in case the respective conflict never happened. Deriving overall consumption from (hypothetical) internal and international trade flows, we further receive a proxy for countries' overall welfare levels. A comparison of actual to hypothetical trade flows and welfare levels then sketches the general equilibrium effects of the respective conflict.

The estimated welfare changes help us interpret the global effects of civil conflict.

As depicted in Figure B1, for basically every country in our sample, welfare levels are smaller relative to the hypothetical scenario where a given conflict had not occurred. While it is of little surprise that the conflict countries themselves as well as their main importers experience the largest welfare reductions, even those countries that experience bilateral export increases thanks to trade relocation are overall worse off. Indeed, we only estimate a slight welfare increase for Macao in response to the civil conflict in Colombia. Apparently, trade relocation can only partially offset the welfare losses countries encounter due to increased trading costs with the conflict country. Hence, even though trade relocation helps mitigate some of the global loss in trade and welfare due to civil wars, all members of the world economy are individually worse off compared to a world at peace.

5 Robustness

We estimate various alternative specifications to test our results for robustness. A first concern of our estimation approach is our selection of cut-offs to code our relevance and similarity conditions. Figure 3 provides results for our main specification using alternative thresholds to classify relevant and similar exporters, respectively. Panel (a) to the left varies the number of export partners we classify as relevant for a dyad's importer. This exercise suggests that our results are sensitive to the cut-off we choose to classify exporters as relevant. Only when we consider anything between the top seven and ten trade partners as relevant, we find significant trade relocation effects.[14] This finding fits the intuition behind our estimation approach. If we consider a too small number of trade-partners, we miss out on relevant relocation cases. This, in turn, results in only a very small number of dyads we code as subject to trade relocation, while many potential relocation cases end up in our control group, biasing the results towards zero. Similarly, a too broad classification of relevant trade-partners adds numerous cases which we would code as subject to trade relocation even though the actual propensity for trade relocation is very low. According to our raw data, trade partners that are ranked tenth or higher are responsible for less than one percent of a country's overall imports, on average. This again biases our results towards zero as cases where no trade relocation is to be expected end up in our treatment group. In Panel (b) to the right, we conduct a similar robustness test and vary the number of clusters we use to code exporters as similar. Similarly as above, for a very broad categorization into e.g. only two clusters or very narrow classification into fifty clusters or more, our results turn insignificant.[15] Hence, our results are much

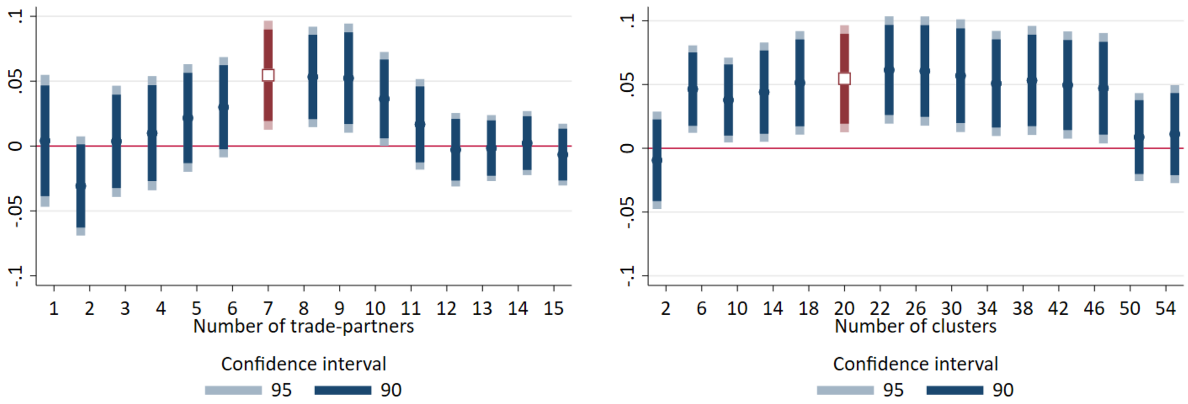
[14] A disaggregation into sectors in Figure B4 in the Appendix shows that a smaller number of trade-partners in the agricultural sector yields stronger results than in the minerals and manufacturing sectors. The coefficients for the fuels sector remain statistically insignificant, regardless of the number of trade-partners.

[15] The agricultural sector is the least sensitive to the number of clusters, while for the minerals and manufacturing sectors the estimated coefficient is more often insignificant (Figure B5 in the Appendix).

less sensitive to the number of clusters we select to code our similarity condition. This resembles the fact that the variation in the similarity classification is rather low across intermediate numbers of clusters. For example, countries that rely mostly on agricultural production will almost always end up together in the same cluster, no matter whether the world is divided into five or forty production clusters. Still, another concern inherent to our estimation approach is that a single cluster might drive our results. To check for this possibility, we conduct leave-one-out regressions, where we repeat our main estimations but drop one cluster at a time. As we show in Figure B3 in the Appendix, our results are basically identical regardless of which countries we drop from our sample.

Figure 3 – Alternative Relevance and Similarity Cut-Offs

(a) Alternating number of trade-partners **(b)** Alternating number of clusters



Notes: This figure displays the coefficients of our diversion propensity as defined in Table 1 with alternating numbers of trade-partners (20 clusters) in the left panel and alternating number of clusters (7 trade-partners) in the right panel. The baseline estimates from column 1 of Table 1 are displayed in red and squared markers. All estimations are run with the PPML estimator and include exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered on the exporter-importer-sector level. The light and dark blue lines depict 95% and 90% confidence intervals, respectively.

Two further robustness checks specifically concern our coding of exporter similarity. First, instead of clusters we construct a similarity index following Benedictis and Tajoli (2007a). This index measures the correlation of sectoral export values between two countries relative to other countries. We define countries i and k as similar if their export similarity is higher than 0.5, where 1 refers to identical and 0 to non-overlapping export patterns. Second, we change the input to our cluster calculations to allow for importer-specific considerations of which exporters they would treat as similar. Here, we classify all available exporters for each importer separately and include additional variables as inputs to the cluster algorithm. In addition to sectoral production shares, we also include various dyadic determinants of trade costs. Among other things, these are

Again, the coefficients for the fuels sector remain statistically insignificant throughout.

bilateral distance, common official language, and colonial heritage. Arguably, if importers search for substitution possibilities in response to a civil war in one of their main export origins, these cost factors may be as relevant as a country's production capabilities to make a trade relocation decision. We present the results of both alternative specifications in Table A5. Our main results remain qualitatively unchanged.

Next, we want to rule out the possibility that instead of the interaction of the similarity and relevance conditions, one of these conditions alone produces our results. Theoretically, our identification approach might mechanically single-out much-trading dyads or countries exporting specific goods via the relevance or similarity classification, respectively. While each indicator variable alone is controlled for by our fixed effects, there remains the possibility that due to missing variation in either one of the two conditions, the other may alone drive the effects. As our results are less sensitive to the number of clusters as shown in Figure 3 above, one concern could be that the condition of similarity is redundant. To check whether indeed the interaction of both variables is generating our results, we invert either the similarity or the relevance classification and repeat our main estimations. Table A6 reports the results. In Panel A, we use the seven least important trade-partners to importer j, while still using the original similarity classification between conflict country k and exporter i based on twenty clusters. We find no evidence that conflict in less-important trade partners leads to trade relocation. In Panel B, we retain the original relevance classification of the top seven trading partners, but turn around our similarity classification to include all exporters with an exporter similarity index below 0.1. Again, we do not find significant trade relocation effects. Overall, we conclude from this falsification test that our identification approach indeed captures trade relocation propensity, as both relationships to the conflict country, i.e. the exporter's similarity as well as the importer's relevance, are needed together to produce our main results.

Finally, Table A7 presents additional results in which we slightly change our main specification. In Panel A, in addition to our standard relocation propensity variable, we include a similar indicator for relocation propensity based on large conflicts with more than 1000 battle deaths. Our results mostly stem from small conflicts. The coefficients for our main indicator variable considering all conflicts together remains robustly positive, whereas the indicator variable based on big conflicts yields insignificant or even negative coefficients. These results should be treated with caution though, as the number of large conflicts in our sample is relatively small. In Panel B, we use the number of conflict countries that fulfill the relevance and similarity conditions instead of an indicator that the conditions are fulfilled for *any* country to estimate the intensive margin of trade relocation. The coefficients are almost identical to our main results, only in the minerals sector the effect is more precisely estimated. This may be a hint that in this sector, import demand is more likely to spill over from several conflict countries to some specific (peaceful) exporters. In Panel C, we estimate trade-flows in the same year as the conflict in country k. The weaker results for the minerals and manufacturing sectors indicate that trade-flows need some time to adjust. Looking at international instead of domestic wars, Panel D reports no statistically significant effects. This is most likely driven by the very small number of international wars during our sample period.

6 Conclusion

This paper introduces a novel estimation approach for trade relocation effects that result from economic shocks in third countries. According to the structural gravity model of international trade, unilateral economic shocks affect bilateral trade between other countries via changes to the overall competition on international markets (Anderson and van Wincoop, 2003). In the short-run, a reduced competitiveness of one country can thus increase trade between other countries. If such short-run trade increases market integration, e.g. via signing PTAs, bilateral trade costs remain lower than before the shock, which in turn provokes a persistent relocation of international trade.

We apply the estimation approach to civil conflicts, which have been shown to significantly depress countries' export capacity (Novta and Pugacheva, 2021). On average, we find that dyads increase bilateral trade flows by 6% in response to civil conflict in a third country. The agricultural, manufacturing and minerals sectors exhibit a trade diversion effect of up to 13%, whereas we find no trade diversion in the fuels sector. What is more, we find that in the manufacturing sector trade relocation persists still nine years after the end of a civil conflict due to reduced bilateral trade costs via PTAs. Hence, civil conflicts can induce long-term economic losses for affected countries as international markets end up in a new equilibrium.

This paper is the first to study the short- and medium-run trade relocation effects of unilateral shocks like civil conflicts. Our results add to prior findings that civil conflicts depress the international trade flows of conflict countries (Martin et al., 2008a) and their neighbors (Qureshi, 2013). Our findings are furthermore relevant for the design of post-conflict recovery policies. After a country resolves its internal disputes, it faces a different network of international trade with increased competition due to persistent shifts in the trade relationships of former trading partners. To reintegrate the now peaceful country back into international markets and support post-conflict recovery, improving the terms of trade, e.g. via the quick resolution of (temporary) preferential tariff margins in the spirit of post-conflict Generalised Scheme of Preferences (GSP) tariffs, may constitute valuable policy measures. Similarly, conflict-countries themselves may prioritize foreign policy to improve bilateral trade and hence spur the recovery of local production capacities.

Our estimation approach can easily be adapted to other settings. To analyze relocation effects, we construct a relocation propensity indicator variable, which translates the triadic relationship between a conflict country and any trading dyad into a dyadic observation. Besides civil conflicts, the approach can be applied to any other unilateral shock that can significantly alter a country's international competitiveness. Moreover, our estimation approach can be adapted to other bilateral outcome variables like migration or FDI by formulating similarity and relevance conditions that apply to the outcome variable of interest.

References

Ahsan, R.N., Iqbal, K., 2020. How Does Violence Affect Exporters? Evidence from Political Strikes in Bangladesh. Review of International Economics 28, 599–625.

Akresh, R., Bhalotra, S., Leone, M., Osili, U.O., 2012. War and Stature: Growing Up During the Nigerian Civil War. American Economic Review 102, 273–277.

Allen, T., Donaldson, D., 2020. Persistence and Path Dependence in the Spatial Economy. National Bureau of Economic Research Working Paper Series .

Amodio, F., Baccini, L., Di Maio, M., 2020. Security, Trade, and Political Violence. Journal of the European Economic Association 19, 1–37.

Anderson, J.E., 1979. A Theoretical Foundation for the Gravity Equation. American Economic Review 69, 106–116.

Anderson, J.E., Larch, M., Yotov, Y.V., 2018. GEPPML: General Equilibrium Analysis with PPML. The World Economy 41, 2750–2782.

Anderson, J.E., van Wincoop, E., 2003. Gravity with Gravitas: A Solution to the Border Puzzle. American Economic Review 93, 170–192.

Armington, P.S., 1969. A Theory of Demand for Products Distinguished by Place of Production. Staff Papers (International Monetary Fund) 16, 159–178.

Bahar, D., Santos, M.A., 2018. One More Resource Curse: Dutch Disease and Export Concentration. Journal of Development Economics 132, 102–114.

Baier, S.L., Yotov, Y.V., Zylkin, T., 2019. On the Widely Differing Effects of Free Trade Agreements: Lessons from Twenty Years of Trade Integration. Journal of International Economics 116, 206–226.

Barro, R.J., Sala-i-Martin, X., 1992. Convergence. Journal of Political Economy 100, 223–251.

Bayer, R., Rupert, M., 2004. Effects of Civil Wars on International Trade, 1950-92. Journal of Peace Research 41, 699–713.

Bazzi, S., Blattman, C., 2014. Economic Shocks and Conflict: Evidence from Commodity Prices. American Economic Journal: Macroeconomics 6, 1–38.

Benedictis, L.D., Tajoli, L., 2007a. Economic Integration and Similarity in Trade Structures. Empirica 34, 117–137.

Benedictis, L.D., Tajoli, L., 2007b. Openness, Similarity in Export Composition, and Income Dynamics. The Journal of International Trade & Economic Development 16, 93–116.

Berger, D., Easterly, W., Nunn, N., Satyanath, S., 2013. Commercial Imperialism? Political Influence and Trade during the Cold War. American Economic Review 103, 863–96.

Berman, N., Couttenier, M., Rohner, D., Thoenig, M., 2017. This Mine Is Mine! How Minerals Fuel Conflicts in Africa. American Economic Review 107, 1564–1610.

Beverelli, C., Keck, A., Larch, M., Yotov, Y., 2018. Institutions, Trade and Development: A Quantitative Analysis. Drexel University Working Paper Series, WP 2018-03 .

Blattman, C., 2012. Post-conflict Recovery in Africa: The Micro Level. Oxford Companion to the Economics of Africa , 124–130.

Blattman, C., Miguel, E., 2010. Civil War. Journal of Economic Literature 48, 3–57.

Bluhm, R., Gassebner, M., Langlotz, S., Schaudt, P., 2021. Fueling conflict? (de)escalation and bilateral aid. Journal of Applied Econometrics 36, 244–261.

Brakman, S., Garretsen, H., Schramm, M., 2004. The Strategic Bombing of German Cities During World War II and its Impact on City Growth. Journal of Economic Geography 4, 201–218.

Brück, T., Di Maio, M., Miaari, S.H., 2019. Learning The Hard Way: The Effect of Violent Conflict on Student Academic Achievement. Journal of the European Economic Association 17, 1502–1537.

Cheong, J., Kwak, D.W., Tang, K.K., 2015. It Is Much Bigger Than What We Thought: New Estimate of Trade Diversion. The World Economy 38, 1795–1808.

Collier, P., Hegre, H., Hoeffler, A., Reynal-Querol, M., Sambanis, N., 2003. Breaking the Conflict Trap: Civil War and Development Policy. World Bank Publications.

Dai, L., Eden, L., Beamish, P.W., 2017. Caught in the Crossfire: Dimensions of Vulnerability and Foreign Multinationals' Exit from War-Afflicted Countries. Strategic Management Journal 38, 1478–1498.

Dai, M., Yotov, Y., Zylkin, T., 2014. On the Trade-diversion Effects of Free Trade Agreements. Economics Letters 122, 321–325.

Davis, D.R., Weinstein, D.E., 2002. Bones, Bombs, and Break Points: The Geography of Economic Activity. American Economic Review 92, 1269–1289.

De Sousa, J., Mirza, D., Verdier, T., 2018. Terrorism Networks and Trade: Does the Neighbor Hurt? European Economic Review 107, 27–56.

Egger, H., Egger, P., Greenaway, D., 2008. The Trade Structure Effects of Endogenous Regional Trade Agreements. Journal of International Economics 74, 278–298.

Emran, M.S., 2005. Revenue-increasing and Welfare-enhancing Reform of Taxes on Exports. Journal of Development Economics 77, 277–292.

Fally, T., Sayre, J., 2018. Commodity Trade Matters. National Bureau of Economic Research URL: https://www.nber.org/papers/w24965, doi:10.3386/w24965.

Felbermayr, G., Syropoulos, C., Yalcin, E., Yotov, Y., 2019. On the Effects of Sanctions on Trade and Welfare: New Evidence Based on Structural Gravity and a New Database. LeBow College of Business Working Paper Series, Drexel University .

Freund, C., Mattoo, A., Mulabdic, A., Ruta, M., 2021. Natural Disasters and the Reshaping of Global Value Chains. Policy Research Working Paper Series 9719. The World Bank. URL: https://ideas.repec.org/p/wbk/wbrwps/9719.html.

Fuchs, A., Klann, N.H., 2013. Paying a Visit: The Dalai Lama Effect on International Trade. Journal of International Economics 91, 164–177.

Garfinkel, M., Syropoulos, C., Zylkin, T., 2020a. Prudence vs. Predation and the Gains from Trade. Drexel University Working Paper Series, WP 2020-06 .

Garfinkel, M.R., Syropoulos, C., Yotov, Y.V., 2020b. Arming in the Global Economy: The Importance of Trade with Enemies and Friends. Journal of International Economics 123, 103295.

Grubert, H., Mutti, J., 1991. Taxes, Tariffs and Transfer Pricing in Multinational Corporate Decision Making. Review of Economics and Statistics , 285–293.

Hartigan, J.A., Wong, M.A., 1979. Algorithm AS 136: A K-Means Clustering Algorithm. Journal of the Royal Statistical Society. Series (Applied Statistics) 28, 100–108.

Head, K., Mayer, T., 2014. Gravity Equations: Workhorse, Toolkit, and Cookbook. Handbook of International Economics 4, 131–195.

Jones, B.F., Olken, B.A., 2010. Climate Shocks and Exports. American Economic Review 100, 454–59.

Kim, I.S., Liao, S., Imai, K., 2020. Measuring Trade Profile with Granular Product-Level Data. American Journal of Political Science 64, 102–117.

Krugman, P., Taylor, L., 1978. Contractionary Effects of Devaluation. Journal of International Economics 8, 445–456.

Ksoll, C., Macchiavello, R., Morjaria, A., 2018. Guns and Roses: Flower Exports and Electoral Violence in Kenya. Global Poverty Research Lab Working Paper No. 17–102.

Mankiw, N.G., Romer, D., Weil, D.N., 1992. A Contribution to the Empirics of Economic Growth. Quarterly Journal of Economics 107, 407–437.

Martin, P., Mayer, T., Thoenig, M., 2008a. Civil Wars and International Trade. Journal of the European Economic Association 6, 541–550.

Martin, P., Mayer, T., Thoenig, M., 2008b. Make Trade Not War? Review of Economic Studies 75, 865–900.

Martin, P., Mayer, T., Thoenig, M., 2012. The Geography of Conflicts and Regional Trade Agreements. American Economic Journal: Macroeconomics 4, 1–35.

Miguel, E., Roland, G., 2011. The Long-run Impact of Bombing Vietnam. Journal of Development Economics 96, 1–15.

Mirza, D., Verdier, T., 2014. Are Lives a Substitute for Livelihoods? Terrorism, Security, and US Bilateral Imports. Journal of Conflict Resolution 58, 943–975.

Novta, N., Pugacheva, E., 2021. The Macroeconomic Costs of Conflict. Journal of Macroeconomics 68, forthcoming.

Oetzel, J., Miklian, J., 2017. Multinational Enterprises, Risk Management, and the Business and Economics of Peace. Multinational Business Review 25, 270–286.

Qureshi, M.S., 2013. Trade and Thy Neighbor's War. Journal of Development Economics 105, 178–195.

Rohner, D., Thoenig, M., Zilibotti, F., 2013. War Signals: A Theory of Trade, Trust, and Conflict. Review of Economic Studies 80, 1114–1147.

Rose, A.K., 2018. Currency Wars? Unconventional Monetary policy Does Not Stimulate Exports. National Bureau of Economic Research .

Ross, M.L., 2015. What Have We Learned about the Resource Curse? Annual Review of Political Science 18, 239–259.

Santos Silva, J.M.C., Tenreyro, S., 2006. The Log of Gravity. Review of Economics and Statistics 88, 641–658.

Sundberg, R., Melander, E., 2013. Introducing the UCDP Georeferenced Event Dataset. Journal of Peace Research 50, 523–532.

Tur-Prats, A., Valencia Caicedo, F., 2020. The Long Shadow of the Spanish Civil War. CEPR Discussion Paper DP15091.

Verwimp, P., Justino, P., Brück, T., 2019. The Microeconomics of Violent Conflict. Journal of Development Economics 141, 1–6.

Yotov, Y., 2021. The Variation of Gravity within Countries (or 15 Reasons Why Gravity Should Be Estimated with Domestic Trade Flows). CESifo Working Paper No. 9057 .

Yotov, Y.V., Piermartini, R., José-Antonio, Larch, M., 2016. An Advanced Guide to Trade Policy Analysis: The Structural Gravity Model. World Trade Organization .

Appendix

A Additional Tables

Table A1 – Descriptive statistics

Variable	Mean (1)	SD (2)	Min. (3)	Max. (4)	Obs. (5)
Panel A: General variables					
Export value (Mio. USD)	94.80	1871.00	0.00	4.9e+05	2175061
Production value (Mio. USD)	5.9e+07	4.2e+08	0.00	1.6e+10	12506
Trade agreement	0.12	0.33	0.00	1.00	2187567
Exporter sanctioned	0.08	0.27	0.00	1.00	2187567
Internal conflict (>25 deaths)	0.13	0.34	0.00	1.00	2187567
Large internal conflict (>1000 deaths)	0.03	0.17	0.00	1.00	2187567
Panel B: Cluster variables					
Conflict, 15 cluster, top 7 TP	0.04	0.19	0.00	1.00	2024615
Conflict, 15 cluster, top 10 TP	0.06	0.23	0.00	1.00	2024615
Conflict, 20 cluster, top 7 TP	0.03	0.18	0.00	1.00	2024615
Conflict, 20 cluster, top 10 TP	0.05	0.21	0.00	1.00	2024615
Panel C: Similarity variables					
Conflict, 0.50 similarity, top 7 TP	0.03	0.17	0.00	1.00	2024615
Conflict, dyadic cluster, top 7 TP	0.24	0.43	0.00	1.00	2024615

Notes: This table provides descriptive statistics of the main variables. The variables in Panels B and C depict the indicator variable as described in Equation (3). Variables take a value of 1 if exporter k has a conflict, countries i and k were in the same cluster (Panel B) or similar (Panel C), and country k was a top trade-partner (TP) of country j. The sample consists of 180 countries and the sectors agriculture, minerals, fuels and manufacturing between 1995-2014.

Table A2 – Trade relocation heterogeneity: Commodity characteristics

Dependent:	Sectoral exports from country i to country j					
Panel A:	Agriculture			Minerals		
	Very common	Common	Rare	Very common	Common	Rare
Conflict in country k	0.02 (0.02)	0.06 (0.04)	-0.08 (0.09)	0.06*** (0.02)	-0.03 (0.05)	0.27 (0.31)
Observations	324148	303658	80708	289851	216409	17951
Panel B:	Agriculture		Mining		Manufacturing	
	Final goods	Inter- mediates	Final goods	Inter- mediates	Final goods	Inter- mediates
Conflict in country k	0.01 (0.03)	0.09** (0.04)	-0.03 (0.19)	0.08*** (0.03)	0.17*** (0.06)	0.02 (0.09)
Observations	312996	313568	45709	322052	111595	111664
Exporter × year	✓	✓	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓	✓	✓
Exp. × Imp.	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j. In Panel A, the agriculture and minerals sector are disaggregated into 'very common' (top 10% traded commodities), 'common' (middle 80% traded commodities) and 'rare' (least 10% traded commodities). In Panel B exports from the agriculture, minerals and manufacturing sectors are disaggregated into into intermediate and final goods based on the BEC classification. The explanatory variables is constructed as in Table 1, with the top-7 exporters defining relevance, and 20 clusters defining similarity. All estimations include exporter-time, importer-time and exporter-importer fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered at the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01

Table A3 – Trade relocation heterogeneity: Market share of conflict country

Dependent:	Sectoral exports from country i to country j							
	Agricult.		Minerals		Fuels		Manufact.	
Country k has above 5% market share:	No	Yes	No	Yes	No	Yes	No	Yes
Conflict in country k	0.14*** (0.04)	-0.00 (0.05)	0.11*** (0.03)	-0.06 (0.08)	0.01 (0.03)	-0.20** (0.09)	0.08** (0.03)	-0.02 (0.03)
Observations	366662	366662	322631	322631	89446	89446	491003	491003
Exporter × year	✓	✓	✓	✓	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓	✓	✓	✓	✓
Exp. × Imp.	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j. The explanatory variables is constructed as in Table 1, with the top-7 exporters defining relevance, and 20 clusters defining similarity. To analyze the heterogeneity w.r.t the market share, we interact the explanatory variable with a dummy indicating that country k has a market share of at least 5% in the respective sector. This interaction variable takes the value of 0 in odd columns and 1 in even columns. All estimations are run with the PPML estimator and include exporter-time, importer-time and exporter-importer fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered at the importer-exporter level, * p< 0.1, ** p< 0.05, *** p< 0.01

Table A4 – PE Results for GE Computation

	Exports from country i to j		
	(1)	(2)	(3)
Peace × International	0.687***	0.410***	0.888***
	(0.149)	(0.139)	(0.127)
N	150719	150719	150719
Country	Colombia	Ukraine	Turkey
FTA-Control	✓	✓	✓

Standard errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$
Results from PPML Regressions.
All Regressions control for PTAs and include the typical fixed effects.

Table A5 – Trade relocation robustness: Alternative similarity and relevance definitions

Dependent:	Exports from country i to country j			
	Agricult.	Minerals	Fuels	Manufact.
Panel A:	Export similarity >0.5			
Conflict in country k	0.08**	0.06**	-0.06**	0.06**
	(0.03)	(0.03)	(0.03)	(0.02)
Panel B:	Dyadic clusters			
Conflict in country k	0.04**	0.06***	0.01	0.09***
	(0.02)	(0.01)	(0.03)	(0.02)
Observations	366662	322631	89446	491003
Exporter × year	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓
Exporter × Importer	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, disaggregated by sectors. The explanatory variables is constructed as in Table 1, with similarity being defined as the two countries having an abvove 0.5 similarity index, as defined by Benedictis and Tajoli (2007a,b) in Panel A, and the two countries being in the same dyadic cluster in Panel B, and relevance as the top-7 exporter countries. All estimations are run with the PPML estimator and include the trade exporter-time, importer-time and exporter-importer fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered on the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01

Table A6 – Trade relocation robustness: Wrong similarity and relevance conditions

Dependent:	Sectoral exports from country i to country j							
	Agricult.		Minerals		Fuels		Manufact.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:	20 clusters, bottom 7 or non-trading partner countries							
Conflict in country k	0.02 (0.02)	0.02 (0.02)	0.00 (0.02)	0.00 (0.02)	0.04 (0.03)	0.04 (0.03)	-0.07* (0.04)	-0.07* (0.04)
Panel B:	Dissimilar countries (<10% similarity)							
Conflict in country k	0.02 (0.02)	0.02 (0.02)	-0.04** (0.02)	-0.04** (0.02)	-0.01 (0.04)	-0.01 (0.04)	-0.12*** (0.03)	-0.12*** (0.03)
Observations	366662	366662	322631	322631	89446	89446	491003	491003
Exporter × year	✓	✓	✓	✓	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓	✓	✓	✓	✓
Exp. × Imp.	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table reports a placebo study to the previous estimations. It shows effects of conflict in country k on exports from country i to country j. The explanatory variables is constructed as in Table 1, but, in Panel A, relevance is measured with the 7 countries with smalles (or zero) exports, and, in Panel B, the similarity is measured with a below 0.1 similarity index, as defined by Benedictis and Tajoli (2007a,b). All estimations are run with the PPML estimator and include the trade exporter-time, importer-time and exporter-importer fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered on the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01

Table A7 – Trade relocation: Various robustness checks

Dependent:	Sectoral exports from country i to country j			
	Agricult.	Minerals	Fuels	Manufact.
Panel A:	Large conflicts			
Any conflict in country k	0.14*** (0.04)	0.12*** (0.03)	-0.02 (0.03)	0.07** (0.03)
Big conflict in country k	-0.07* (0.04)	-0.20** (0.08)	0.02 (0.07)	-0.13 (0.12)
Panel B:	Intensive margin			
Number of country k conflicts	0.11*** (0.04)	0.12*** (0.03)	-0.00 (0.02)	0.08** (0.03)
Panel C:	Conflict in same year			
Conflict in country k	0.13*** (0.03)	0.01 (0.02)	-0.02 (0.03)	0.06* (0.03)
Panel D:	International wars			
International conflict in country k	0.02 (0.03)	-0.01 (0.01)	-0.03 (0.02)	0.00 (0.01)
Observations	366662	322631	89446	491003
Exporter × year	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓
Exporter × Importer	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, disaggregated by sectors. In Panel A, the explanatory variables are indicator variables which counts the occurrences of (i) country k having had any conflict or large conflicts in the previous year, (ii) country k being a top-7 exporter for country j in the pre-conflict-year and (iii) country k and country i being similar exporters in the pre-conflict-year. In Panel B, the explanatory variables is a continuous variable which counts the occurrences of our diversion propensity indicator for each exporter-importer pair. In Panel C, the explanatory variable is an indicator variable but with conflict measured in the same year as exports. Panel D uses international instead of internal wars. Throughout, similarity is measured by being in the same exporter-cluster, with a total number of 20 clusters. All estimations are run with the PPML estimator and include exporter-time, importer-time and exporter-importer fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered on the exporter-importer level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

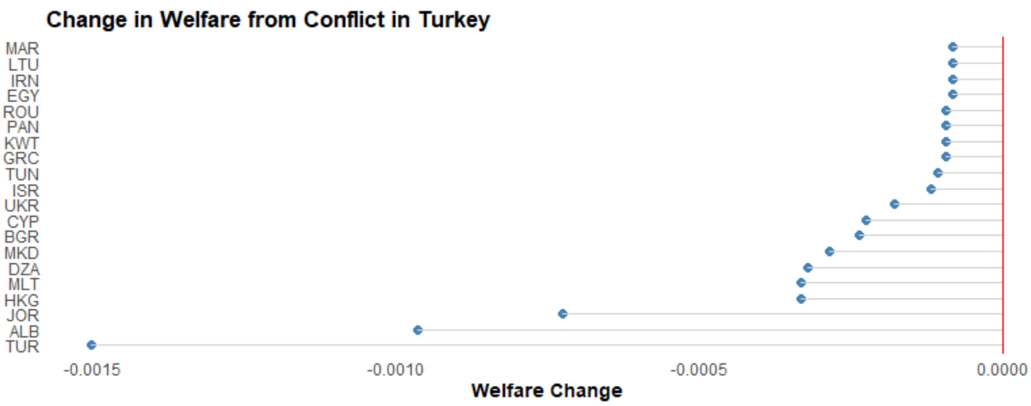
B Additional Graphs

Figure B1 – GE Results: Welfare Changes

(a) Welfare Changes, Conflict in Colombia

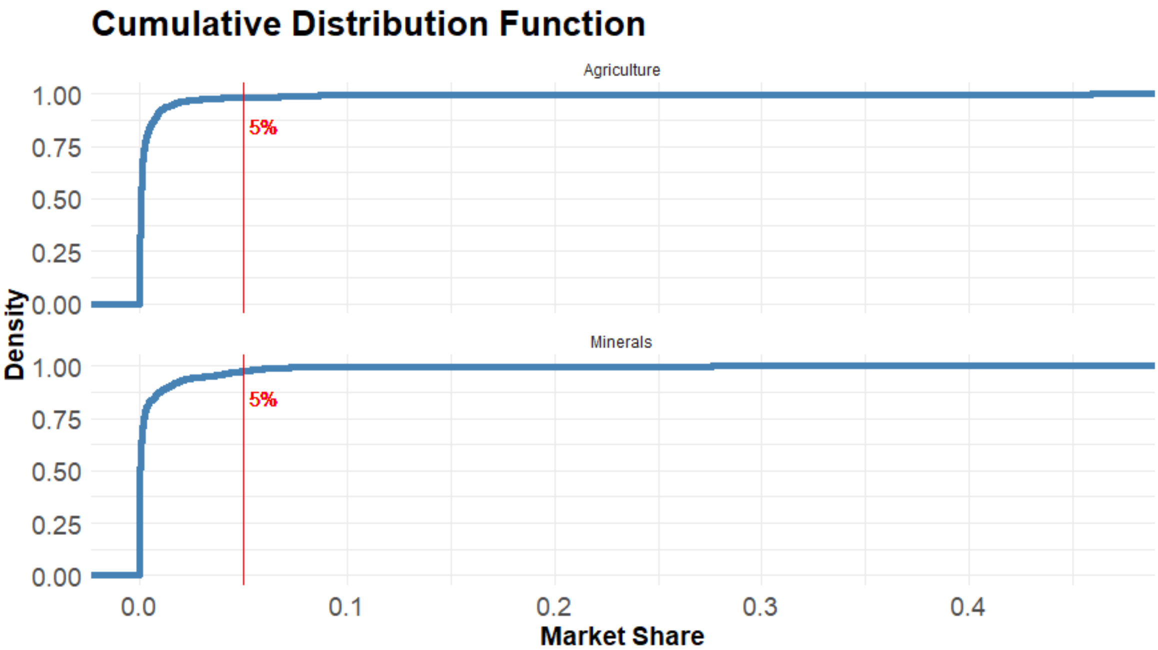
(b) Welfare Changes, Conflict in Ukraine

(c) Welfare Changes, Conflict in Turkey



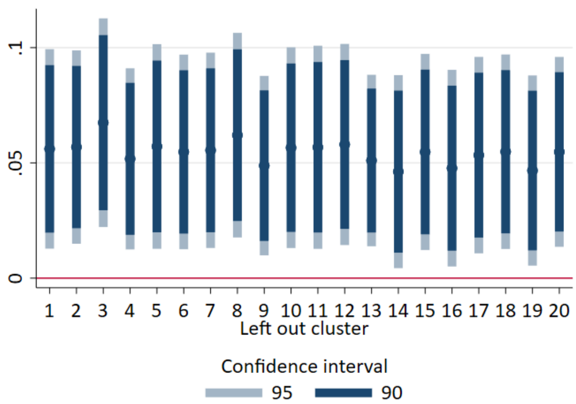
Notes: The graphs report the estimated welfare changes in the general equilibrium due to the civil wars in Colombia (Panel a), Ukraine (Panel b), and Turkey (Panel c). Each panel reports the 15 countries for whom our estimations reported the largest welfare changes. All estimates are derived based on a PE Regression of exports on peace, comparing the estimated trade flows during peace time to the actual trade flows during the civil war. See Table A4 for the respective PE results.

Figure B2 – Distribution of Market Power



Notes: This figure shows the cumulative distribution functions for the market share measures computed based on commodity-level bilateral trade data. We compute for each exporter-commodity-year observation the market share a given observation occupies in the year's total market for a given commodity. The red line indicates the threshold used in Table A3 to identify market leading countries in exports of a specific commodity. Hence, all observations on the right hand side of the red line constitute market leaders in our heterogeneity regressions. The light and dark blue lines depict 95% and 90% confidence intervals, respectively.

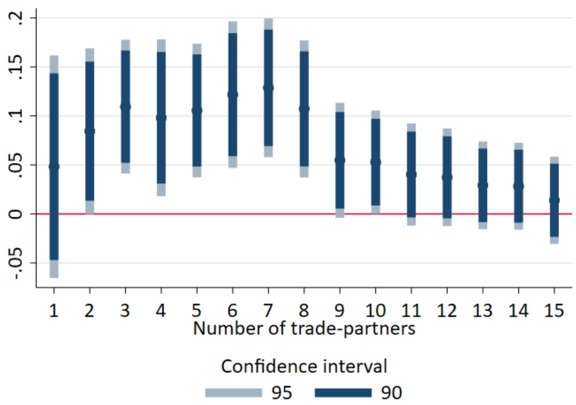
Figure B3 – Leave-one-out



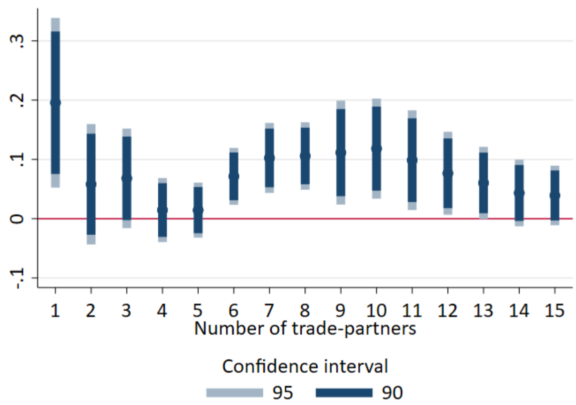
Notes: This figure displays the coefficients of relocation propensity as defined in Table 1 with similarity based on 20 clusters and relevance on the top 7 trade-partners. Each coefficient represents a regression leaving out one cluster. All estimations are run with the PPML estimator and include exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered on the exporter-importer-sector level.

Figure B4 – Number of trade-partners - Sector disaggregation

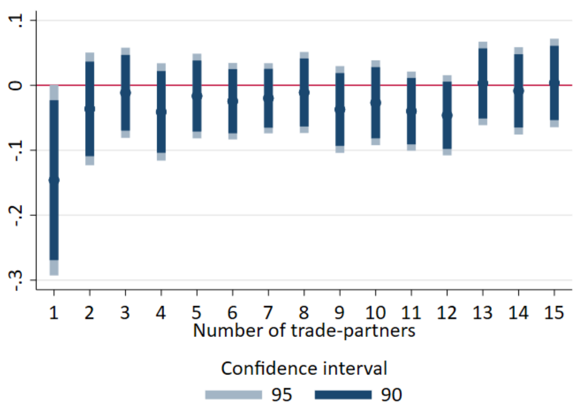
(a) Agriculture



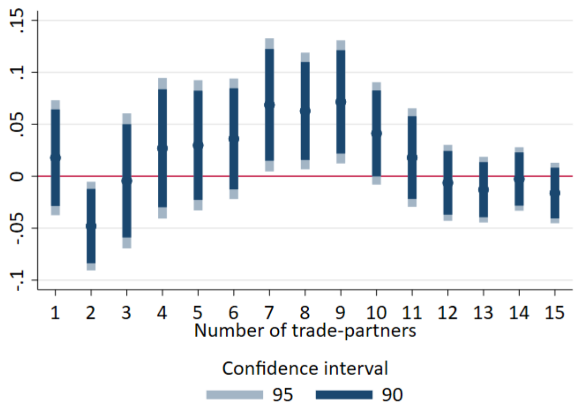
(b) Minerals



(c) Fuels



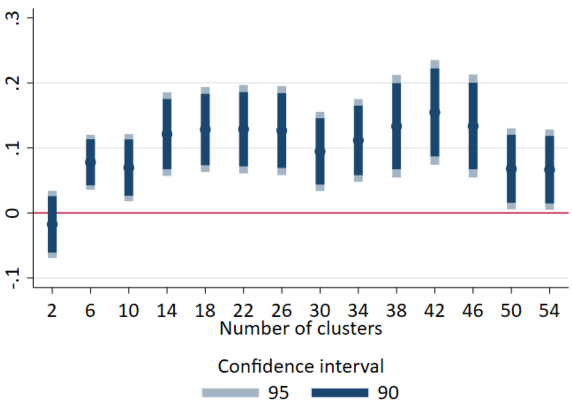
(d) Manufacturing



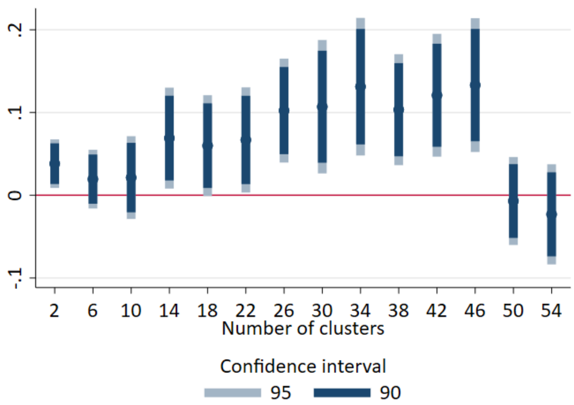
Notes: This figure displays the coefficients of relocation propensity as defined in Table 1 with similarity based on 20 clusters and relevance on a varying number trade-partners. All estimations are run with the PPML estimator and include exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered on the exporter-importer-sector level. The light and dark blue lines depict 95% and 90% confidence intervals, respectively.

Figure B5 – Number of clusters - Sector disaggregation

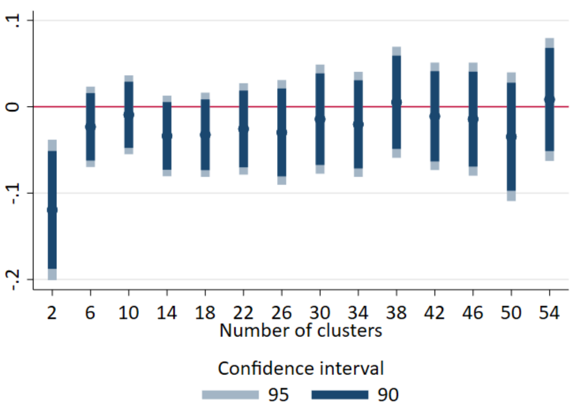
(a) Agriculture



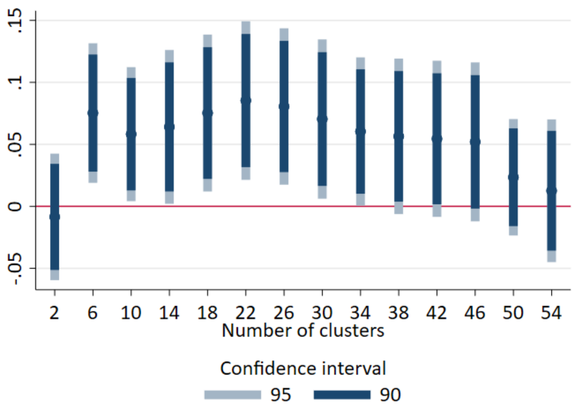
(b) Minerals



(c) Fuels



(d) Manufacturing



Notes: This figure displays the coefficients of relocation propensity as defined in Table 1 with similarity based on a varying number of clusters and relevance on the top 7 trade-partners. All estimations are run with the PPML estimator and include exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered on the exporter-importer-sector level. The light and dark blue lines depict 95% and 90% confidence intervals, respectively.

C Derivation of Relocation Estimation

We leverage a measure of relocation propensity to derive an estimating equation for trade relocation considering the inward multilateral resistance $P_{js,t}$.[16] Following Anderson et al. (2018), we can define $P_{js,t}$ as:

$$P_{js,t} = \sum_k \frac{t_{kj,t}}{\Pi_{ks,t}} \cdot \left[\frac{Y_{ks,t}}{Y_{Ws,t}} \right]^{\frac{1}{1-\sigma}} \tag{4}$$

According to Equation (4), importer j's inward multilateral resistance corresponds to its average access to exports from all other countries k. Theoretically, civil conflict in any other country k can enter Equation (4) either via $Y_{ks,t}$, if war and destruction decrease country k's overall production in sector s, or via $t_{kj,t}$, if civil violence leads to a tightened security situation and therefore increases bilateral shipping costs. W.l.o.g., we assume that civil conflict works via a decrease in overall production $Y_{ks,t}$, while the same argumentation holds for $t_{kj,t}$. Then, we can rewrite the production of each country k to incorporate a potential conflict-shock as $Y_{ks,t} = \bar{Y}_{ks,t} \cdot (1 - \Delta^Y_{ks,t})$, where $\Delta^Y_{ks,t}$ denotes the share of production lost due to civil conflict and $\bar{Y}_{ks,t}$ represents the level of production absent conflict. Next, note that this general way of specifying $P_{js,t}$ allocates the same weight to any exporter k affecting the dyad ij – i.e. a given shock $\Delta^Y_{ks,t}$ affects the inward multilateral resistance by the same amount across all countries k and for all export partners i. We however argue that two bilateral relationships, first between importer j and conflict country k, and second between the two exporters i and k, must be taken into account. While the standard gravity equation suggests that a change in P_{js} might lead to trade relocation from *any* conflict country k to *any other* non-conflict country i in sector s, we argue that the realized trade relocation actually depends on the relocation propensity inherent to the (sector-specific) triad ijk, which we pin down to two important bilateral characteristics underlying (*i*) the kj-dyad and (*ii*) the ik-dyad. To see this, we augment Equation (4) to represent the exporter-specific inward resistance by adding two weight matrices that indicate the relationships between k and j, and k and i, respectively, while also including the conflict shock to country k's production in sector s. Both weight matrices are lagged by one period to focus on the country characteristics before conflict emerged in country k (and potentially altered its characteristics). We arrive at the equation:

$$P_{ijs,t} = \sum_k \frac{t_{kj,t}}{\Pi_{ks,t}} \cdot \left[\boldsymbol{W}^R_{jks,t-1} \cdot \boldsymbol{W}^S_{ik,t-1} \cdot \frac{\bar{Y}_{k,t} \cdot (1 - \Delta^Y_{k,t})}{Y_{W,t}} \right]^{\frac{1}{1-\sigma}} \tag{5}$$

The first weight matrix $\boldsymbol{W}^R_{jks,t-1}$ refers to the relevance of each country k as an

[16]Note that the same argument holds from the importer side via the outward multilateral resistance $\Pi^{1-\sigma}_{is} = \sum_l \frac{E_{ls}}{Y_{Ws}} \cdot \left[\frac{t_{ils}}{P_{ls}} \right]^{1-\sigma}$.

exporter for importer j.[17]. We expect that the realized trade relocation effect is larger if there is a bigger trade value to be relocated, i.e. if dyad kj used to trade a lot in sector s before the conflict emerged in country k. Note that for each (sector-specific) importer j, only the j'th row of the matrix $\boldsymbol{W}^R_{jks,t-1}$ will affect $P_{ijs,t}$, which essentially reduces the weight matrix to the js-specific weight vector $\boldsymbol{w}^R_{jks,t-1}$. The second weight matrix $\boldsymbol{W}^S_{iks,t-1}$ denotes each country k's similarity to a dyad's exporter i, which is not sector-specific. We argue that not all countries are equally suited to "fill in" the gap left by the diminished exports from country k to country j. In theory, we usually assume that countries trade with each other because of the specific varieties of goods that each exporter i has to offer (Armington, 1969). Hence, as country k can provide less of its varieties, country j will turn to country i only if it offers a variety of goods similar to those of conflict country k. Therefore, the relocation propensity arguably depends on exporter i exhibiting a similar export composition as conflict country k. Note here that the variation brought in from the weight matrix $\boldsymbol{W}^S_{ik,t-1}$ is the same for all importers j and hence does not affect $P_{ijs,t}$ differentially across importers. For simplicity, assume that the elements of both matrices only take the values 0 and 1, where 1 indicates that country k is relevant for importer j or similar to exporter i, respectively. Then, rewriting (5) to represent the remaining variation $\hat{P}$ in P when the importer-sector-year and exporter-sector-year fixed effects of the gravity equation are accounted for yields:

$$\hat{P}_{ijs,t} = \sum_k \left(\boldsymbol{w}^R_{jks,t-1} \cdot \boldsymbol{W}^S_{ik,t-1} \cdot \bar{Y}_{k,t} \cdot (1 - \Delta^Y_{k,t})\right)^{\frac{1}{1-\sigma}} \tag{6}$$

Equation (6) demonstrates two things. First, by specifying the gravity equation with the correct fixed effects as outlined in Head and Mayer (2014), the triple-interaction of the (i) conflict-shock to a country k, (ii) the similarity condition for countries i and k, and (iii) the relevance condition for countries j and k in sector s is the only variation left in the multilateral resistance term. Second, it follows from the negative income shock to country k, $-\Delta^Y_{k,t}$, and the elasticity of substitution $\sigma > 1$, that a conflict-shock to any country k increases importer j's inward multilateral resistance P (i.e. $\frac{d\hat{P}_{ijs,t}}{d\Delta^Y_{k,t}} > 0$). Finally, we can separate the general part of the inward multilateral resistance $\bar{P}_{js,t}$, which can be accounted for by fixed effects, from the remaining variation outlined in Equation (6) and insert both into Equation (1) to arrive at:

$$X_{ijs,t} = \frac{Y_{is,t} E_{js,t}}{Y_{Ws,t}} \cdot \left[\frac{t_{ij,t}}{\Pi_{is,t} \cdot \bar{P}_{js,t} \cdot \hat{P}_{ijs,t}}\right]^{1-\sigma} \tag{7}$$

[17] A typical element of matrix $\boldsymbol{W}^R_{jks,t-1}$ would be $\frac{X_{kjs}}{E_{js}}$, which denotes the share of imports from country k in country j's overall consumption expenditures.

Proceeding as above by adding fixed effects and taking logs, we arrive at our main estimating specification:

$$X_{ijs,t} = exp\left[\pi_{is,t} + \lambda_{js,t} + \mu_{ijs} + \beta_2 \cdot \sum_k (R_{jks,t-2} \times S_{ik,t-2} \times C_{k,t-1}) + \gamma \cdot Z_{ij,t}\right] + \eta_{ijs,t} \tag{8}$$

D Construction of GE Dataset

Our GE estimates require a symmetric dataset which also includes internal trade flows of all sample countries. We calculate internal trade flows by subtracting a country's exports from its total production. In the next step, we construct a symmetric dataset. This is, we require bi-directional trade flows between all available exporters and importers in the sample as well as non-negative internal trade flows for each country and in every year. Due to differing data availability across years, we restrict our sample to the manufacturing sector and the years 1992-2016. Additionally, we reduce the number of countries to 68 importers and exporters. As a decision rule for our sample construction, we decided to only keep years or countries whose numbers of observations amount to at least 80% of the year and 80% of the importer/exporter with the most observations, respectively. Our results remain unchanged for stricter and looser restrictions.

E Direct Effects

A prerequisite for finding significant trade relocation effects of civil conflicts is that conflict countries decrease their amount of exports. Prior findings emphasize that civil wars depress international trade (see, e.g., Bayer and Rupert, 2004, Qureshi, 2013). To replicate these findings with our data and adapt the empirical strategy to the gravity framework of international trade, we follow Head and Mayer (2014) and extend Equation (1) accordingly. When we include the usual fixed effects, the effect of civil conflict in a country i on that same country's exports cannot directly be estimated as the variable is collinear with the exporter-year fixed effects $\pi_{is,t}$. We therefore follow Yotov et al. (2016) and include intranational trade flows along with bilateral trade flows in our dataset.[18] This allows estimating the effect of a unilateral shock like civil conflict on bilateral trade by interacting the variable of interest with an indicator variable for international trade flows (Beverelli et al., 2018). We arrive at an estimating equation of the form:

$$X_{ijs,t} = exp\left[\pi_{is,t} + \lambda_{js,t} + \mu_{ijs} + \beta_1 \cdot (C_{i,t} \times I_{ijs}) + \gamma \cdot Z_{ij,t}\right] + \eta_{ijs,t} \tag{9}$$

where $\eta_{ijs,t}$ accounts for the remaining variation in $X_{ijs,t}$ not explained by the fixed

[18] Yotov (2021) provides an extensive overview of the benefits of adding intranational trade in bilateral trade estimations.

effects and control variables. The variable $C_{i,t}$ indicates the presence of civil conflict in country i at year t, and I_{ijs} indicates international trade flows (i.e. that $i \neq j$). This form of the gravity specification affects the interpretation of the coefficient β_1. Here, β_1 constitutes the elasticity of exports from origin i to destination j in sector s relative to internal consumption of country i to civil conflict emerging in country i.

Table E1 – Direct Effects: Internal conflicts hurt exports

Dependent:	Total exports from country i					
	All sectors		Agri culture	Minerals	Fuels	Manuf acturing
	(1)	(2)	(3)	(4)	(5)	(6)
Conflict (t-1)	-0.05	-0.06**	-0.07*	-0.10**	0.07	-0.06*
× international trade	(0.03)	(0.03)	(0.04)	(0.04)	(0.12)	(0.03)
Conflict (t-2)	-0.07**	-0.08**	0.02	0.06	-0.09	-0.09***
× international trade	(0.03)	(0.03)	(0.04)	(0.04)	(0.12)	(0.04)
Observations	1290234	1290234	354750	314425	88550	532509
Exporter × sector × year	✓	✓	✓	✓	✓	✓
Importer × sector × year	✓	✓	✓	✓	✓	✓
Exp. × imp. × sector	✓	✓	✓	✓	✓	✓
Controls		✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of civil conflict on a country's exports. We interact a dummy variable for lagged civil conflict with an indicator variable for international trade flows. Coefficients must hence be interpreted as change in exports relative to a country's internal trade. All estimations are run with the PPML estimator, exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Controls are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered at the exporter-importer-sector level, * p< 0.1, ** p< 0.05, *** p< 0.01

We use the Pseudo-Poisson Maximum Likelihood (PPML) estimator as suggested by Santos Silva and Tenreyro (2006). To account for differences in the duration and velocity of the effect, we lag civil conflict by one as well as two years. The results are presented in Table E1 and confirm our priors based on the literature. Columns (1) & (2) consider trade data across all four sectors and include sector fixed effects to account for sector-specific shocks. Column (1) does not include any bilateral control variables, while we control for bilateral trade agreements and sanctions starting from column (2). Without control variables, we only find a significantly negative effect of conflict on bilateral exports if conflict is lagged by two years, whereas both lags are statistically significant when the bilateral control variables are included. On average, a conflict country's exports decrease by around 6% and 8% relative to the country's internal consumption one and two years after civil conflict, respectively.[19] In columns (3)–(6), we test for heterogeneity across sectors by restricting the sample to trade flows from the respective sector.[20] Overall,

[19]According to the formula $(e^{\beta} - 1) \times 100\%$.

[20]Note that the gravity equation is separable by sectors as outlined in Yotov et al. (2016) and hence Equation (9) can be applied separately by sector.

the effect of civil conflict on international trade is quite heterogeneous. Agricultural exports only suffer slightly one year after conflict with an effect that is barely statistically significant. Exports of mineral goods, however, are significantly reduced by around 10% one year after conflict, whereas the second lag is not statistically different from zero. For manufacturing exports, we find significant reductions for both lags of the conflict variable. Interestingly, fuel exports do not appear to decline at all during civil conflict. This could, on the one hand, indicate that importers are so dependent on fuel imports that trade-flows continue even in the presence of civil unrest. On the other hand, fuel exports are an important financing tool for civil wars (Bazzi and Blattman, 2014). Therefore, the government as well as the rebels are eager to maintain fuel exports during conflict. Hence, our results suggest that, on average, ongoing civil conflicts depress national exports *relative to internal consumption*.[21] This effect is most immediate in the minerals sector and longer lasting in manufacturing trade, while it does not seem to occur in the fuels sector. Note however that all these estimates likely constitute lower-bound estimates of the actual effect, since we estimate reductions in international trade *relative to* internal trade. Hence, as internal trade is likely to also be negatively affected by civil conflict, our results mirror the additional deterioration of international trade flows with respect to the themselves as well internal trade flows.

[21]Note again that the interaction term in Equation (9) mandates this interpretation.

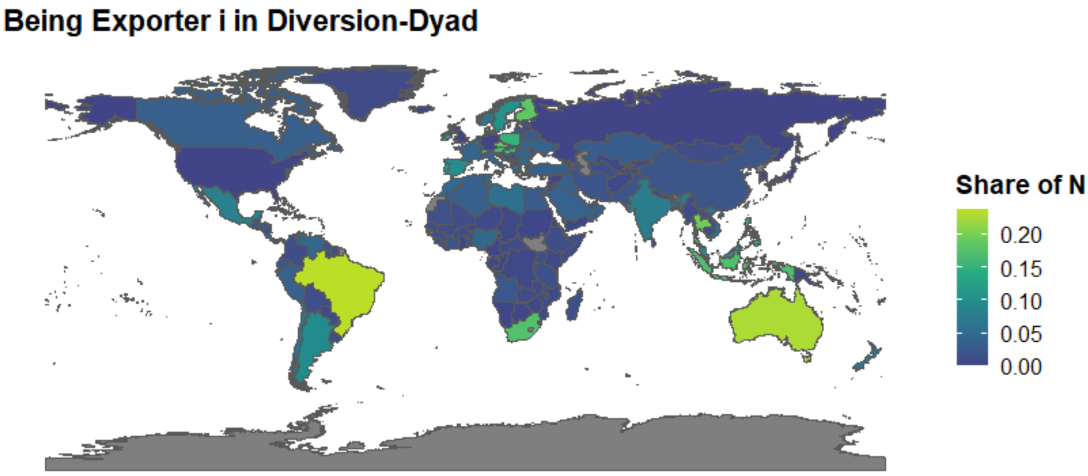
F Relocation Propensity

Figure F1 gives some intuition to the distribution of our relocation propensity variable. The two maps report the geographic distribution of the likelihood to appear as exporter i or importer j in a relocation dyad. The odds of being affected as an importer, i.e. having a relevant trade partner starting a civil war, are distributed quite homogeneously across the globe. While East Africa and the Middle East stick out with a slightly higher propensity and Europe appears only rarely affected, the overall propensity is fairly equally distributed across all regions. The likelihood that in at least one of a country's trading sectors a relevant exporter starts a civil war for most countries lies close to 5 percent. The picture is different when looking at the likelihood of being an affected exporter, i.e. the odds that a country with a similar export structure starts a civil war. Here, Brazil and Australia stick out with a very high likelihood of around 20 percent, followed by South Africa, Argentina, Indonesia, Eastern Europe and Scandinavia. On the other hand, the USA and several other countries, especially in Africa, Asia and Central Europe, are almost never coded as exporters benefiting from trade relocation.

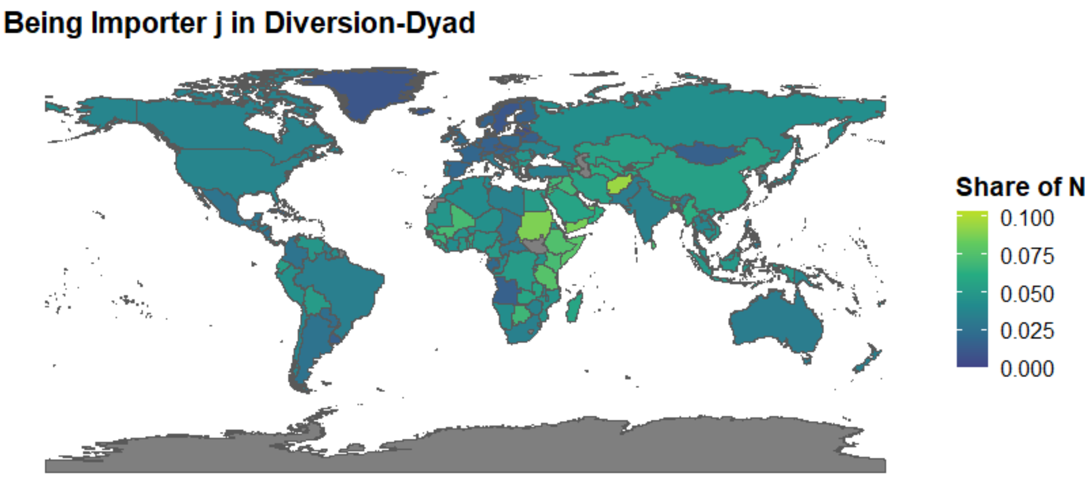
In Figure F2, we further investigate the determinants of the similarity and relevance characteristics. Here, we regress the likelihood that a country is a similar exporter (Panel (a)) or a relevant importer (Panel (b)) to a conflict country k on the common gravity variables. As is to be expected, these variables only play little role for the similarity characteristic. Among the bilateral variables, only inverse distance and an indicator for common legal origins are significantly positive, which likely mirrors local clusters of resources or similar production techniques based on the legal environment. Furthermore, conflict countries have on average a lower GDP, while beneficiary exporters are more likely to be WTO members. For the relevance characteristic however, most gravity variables turn out highly significant and with the expected sign. Important trade partners of a conflict country are on average closer and have the same official language or colonial history. Similarly, higher economic masses of both countries j and k as well as an existing Regional Trade Agreement between the two are significant determinants of the relevance characteristic. This emphasizes that, as is to be expected by construction, the relevance characteristic we identify is strongly related to the classical determinants of bilateral trade.

Figure F1 – Geographic Distribution of Diversion Propensity

(a) Exporter i



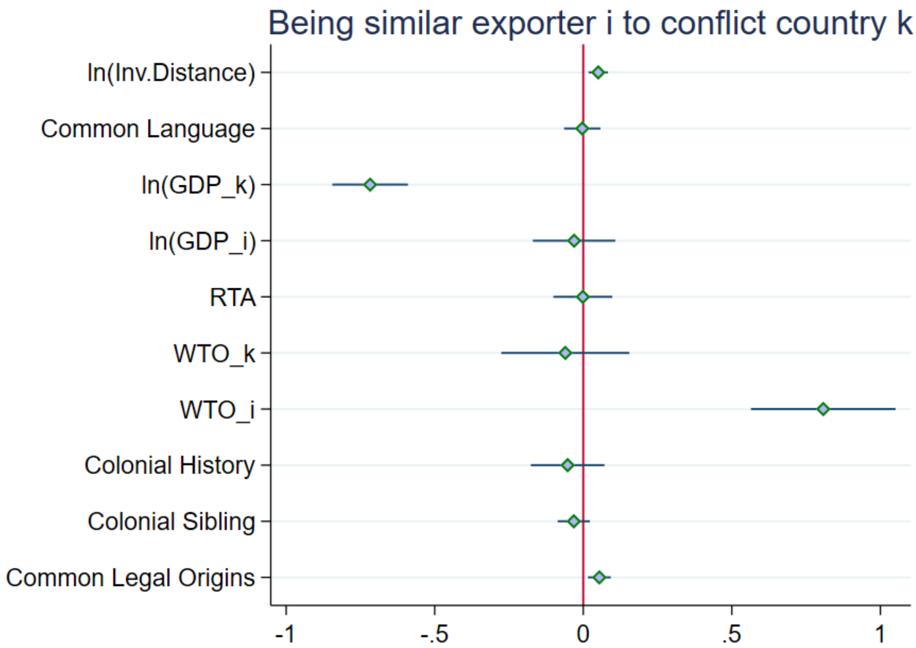
(b) Importer j



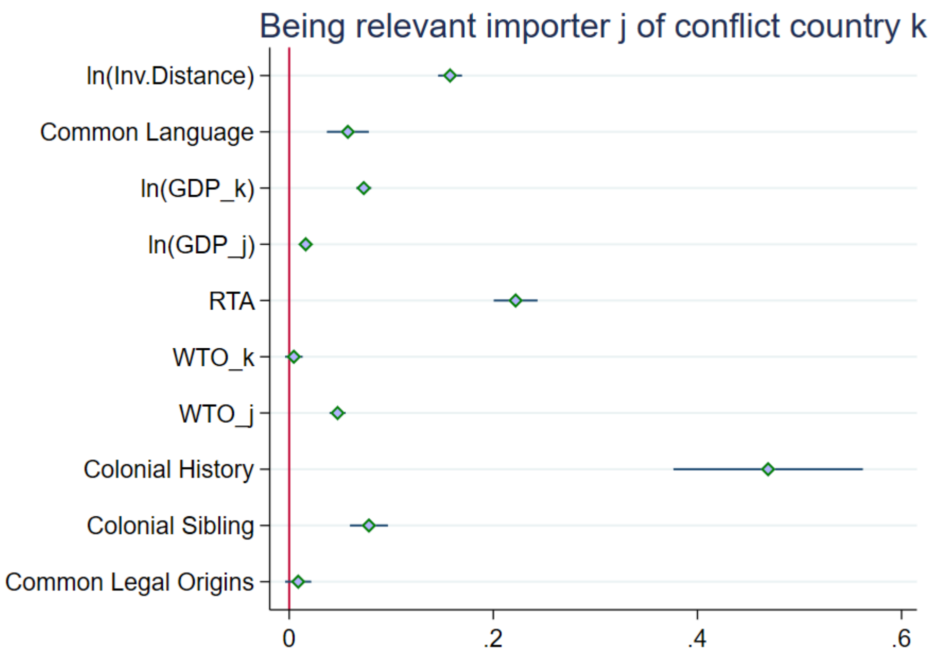
Notes: This figure shows a country's likelihood to appear as an exporter i or importer j in a dyad with positive trade relocation propensity. Panel (a) shows the geographic distribution of the likelihood to be an exporter affected by trade relocation, while panel (b) plots the same distribution for importers. The different shades display the share of a country's observations that it is coded as having a positive relocation propensity. For example, in panel (a) a share of 0.1 means that 10 percent of a country's export observation across all sample years and all importers are coded as being an exporter profiting from trade relocation due to civil conflict in some country k.

Figure F2 – Explaining Propensity of being i or j

(a) Being Similar Exporter i



(b) Being Relevant Importer j



Notes: This figure reports the results from regression the status of being a similar exporter i (Panel a) or a relevant importer j (Panel b) for conflict country k on the most common gravity variables All regressions include importer, exporter, and year fixed effects. Standard errors are clustered at the dyad level. Lines depict 95% Confidence Intervals.

G General Equilibrium

We analyse three case studies in a General Equilibrium (GE) framework. These case studies allow us to focus on specific conflicts and accurately trace the relocation effects. As recent examples of significant violent episodes, we focus on (*i*) the peak of clashes between the FARC rebels and AUC paramilitary forces in Colombia in the 1990s and until 2005, (*ii*) the Ukrainian civil war from 2014 to present, and (*iii*) the violent 1990s in Turkey where the PKK fought for local independence. The case studies were selected based on the significance of the respective conflict shocks (at least two years of violence with more than 1000 battle deaths) among a handful of countries where international and internal trade data were available during and before or after the conflict period. For these three cases, we proceed in two steps. First, we construct an indicator variable for each case that takes the value of one for all dyads that include the respective conflict country as an exporter during years of peace. We then regress trade on the interaction of this variable with an indicator variable for international trade flows including country-year and dyad fixed effects similar to Equation (9). From this, we receive an estimate for the effect of peace on the respective country's exports relative to its internal consumption of self-produced goods.[22]

Second, we use the respective estimates and compute hypothetical trade changes in the general equilibrium during a conflict-year. Following Baier et al. (2019), we apply a one sector Armington-CES model, assuming a constant trade elasticity of $\theta = 4$.[23] This computation generates counterfactual trade flows for all sample countries in case the civil war in either Colombia, Ukraine or Turkey had not happened. Finally, the comparison of hypothetical to actual trade flows provides an estimate for the effect of one country's civil war on its and *all other countries'* trade. These computations require a symmetric dataset; i.e. trade flows must be provided for all potential dyads in the sample in every year and always in both directions. Further, for all countries, information on positive internal trade flows must be included in every year. We follow Baier et al. (2019) to adjust our main dataset accordingly.[24] In the end, we receive a dataset that contains 81 countries and covers the years 1993–2015.

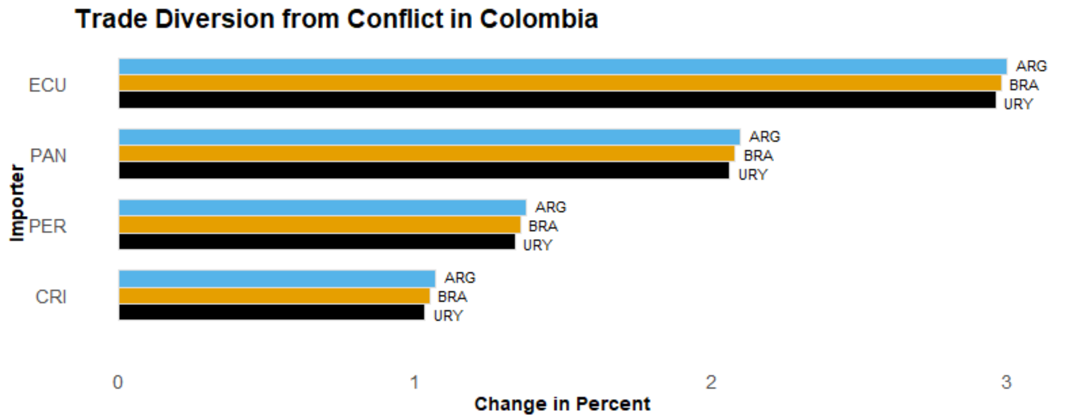
[22]The results are equivalent when estimating the effect of conflict and then computing hypothetical trade flows during a peace year. However, when comparing the hypothetical conflict scenario to the actual peace outcomes, the resulting diversion estimates would have to be inverted to show the trade diversion effects from conflict as opposed to the trade diversion effects from peace. To present the unchanged results, we hence estimate the effect of peace instead of conflict for our GE computations.

[23]We run these computations via the "ge_gravity" Stata Command provided by Thomas Zylkin and discussed in Baier, Yotov and Zylkin (2019).

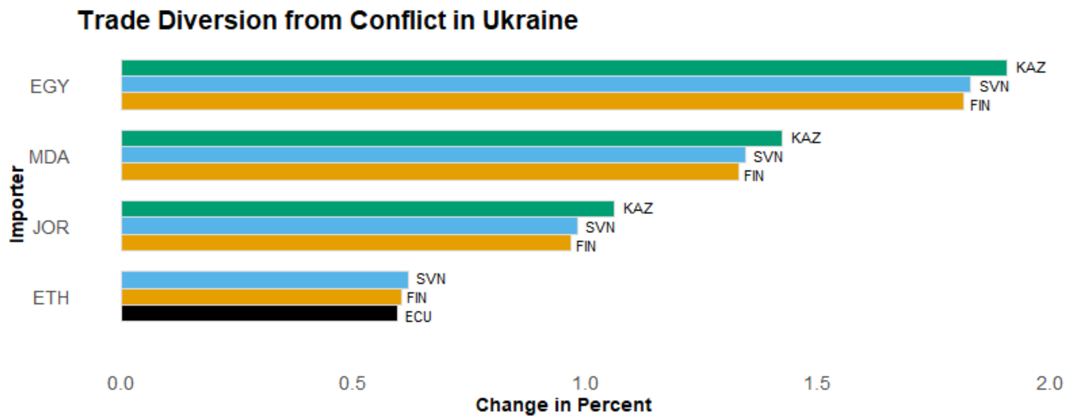
[24]Appendix D provides more details on the dataset construction.

Figure G1 – GE Results Trade Diversion

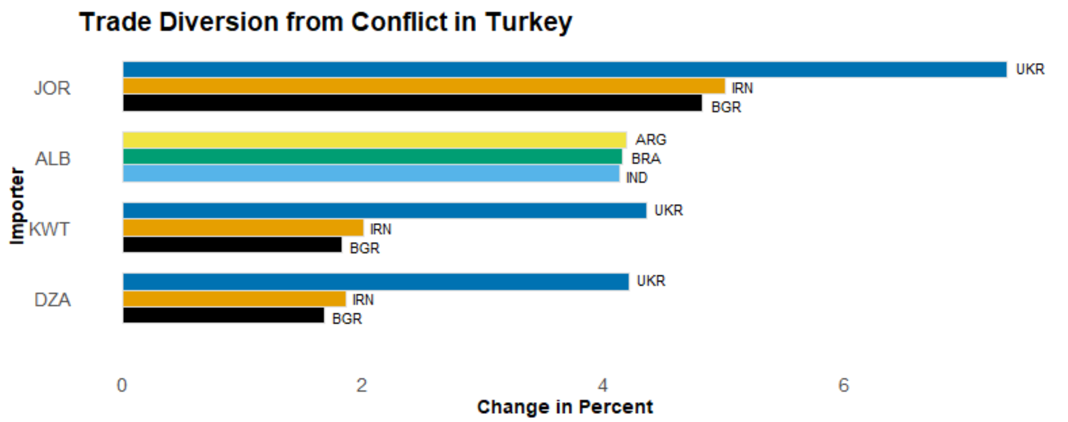
(a) Trade Changes, Conflict in Colombia



(b) Trade Changes, Conflict in Ukraine



(c) Trade Changes, Conflict in Turkey



Notes: The graphs report the estimated trade changes in the general equilibrium due to the civil wars in Colombia (Panel a), Ukraine (Panel b), and Turkey (Panel c). See Table A4 for the respective PE results.

Figure G1 presents the results of the GE analyses for our three case studies. Each panel of Figure G1 reports, for each of the four importers most affected by trade diversion, the export changes for the three origins with the largest export changes. In the case of

Colombia, for example, primarily its neighbors Ecuador, Panama and Peru increased imports from various countries by up to three percent. The picture of the beneficiary exporters for Colombia is quite homogeneous. To all four destinations, Argentina increased its shipments the most, closely followed by Brazil and Uruguay. The effects of the civil wars in Ukraine and Turkey had a larger geographic reach, as both countries are important exporters for Northern African and Middle Eastern countries. During the civil war in Ukraine, mainly other regional exporters increased their shipments to the former destinations of Ukrainian exports. Kazakhstan, Slovenia, and Finland register the largest export increases to Egypt, Moldova and Jordan. In response to the civil war in Turkey, Jordan, Albania, Kuwait and Algeria registered the largest trade diversion effects. Here however, the group of affected origins is more heterogeneous. Jordan, Kuwait and Algeria mainly turned towards Ukraine, Iran and Bulgaria to substitute for Turkish shipments. Albania, on the other hand, instead increased its shipments rather from large but non-regional suppliers, i.e. Argentina, Brazil, and India.

Trade diversion does however not totally mitigate the welfare loss from civil conflict. In Figure B1, we report the estimated international welfare changes in response to the civil conflicts in Colombia, Ukraine and Turkey. We mostly find negative welfare changes, with the biggest losses borne by the conflict-countries as well as the importers mainly affected. Indeed, even the benefiting exporters bear welfare losses, meaning that their increase in exports could not offset the loss of imports from and exports to the conflict countries. Overall, this emphasizes that, even though trade diversion can mitigate the effects of conflict, global welfare still decreases.