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The Trade Disruption Hypothesis Fails for State-Sponsored Genocides and Mass Atrocities: Why It Matters

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Abstract: Our research question is: Do state-sponsored genocides and mass atrocities disrupt trade? In the "conflict disrupts trade" literature there is substantial research on how interstate and intrastate conflict and terrorism affect trade, but very little research on the possible trade disruption effects of genocides and mass atrocities. Our work helps fill this research gap. We bring a suite of estimation methodologies and robustness checks to the question for a pooled sample of 175 countries for the time period 1970–2017. We also test for trade disruption individually for 26 countries that experienced genocide or mass atrocity. Unlike much of the "conflict disrupts trade" literature, we find little empirical support that genocide disrupts trade and at best weak evidence that mass atrocity disrupts trade. Our results have important implications for atrocity prevention policy; when potential atrocity architects evaluate the expected benefits and costs of carrying out atrocity, it seems that, in most cases, they need not worry about trade disruption costs. Our results also matter for empirical research on risk factors for genocides and mass atrocities, particularly for studies that hypothesize risk reduction properties associated with trade.

Keywords: genocide, mass atrocity, civil war, conflict, trade disruption, gravity model

1 Introduction

Among the approximately five dozen published large-sample empirical studies of state-sponsored mass atrocity risks, about a dozen consider the hypothesis that states with substantial external trade will be less likely to experience atrocity, all

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else equal.[1] The hypothesis is similar to the "trade promotes peace" proposition (albeit with varying theoretical underpinnings), which has been extensively tested in empirical studies of interstate and civil conflict risks.[2] Nevertheless, there is a major research gap regarding the hypothesis that greater trade will reduce the risk of state-perpetrated genocide and mass atrocity, all else equal. The hypothesis often rests upon three premises: (1) states receive salient economic and/or political gains from trade, (2) state-perpetrated genocide or other form of mass atrocity would disrupt the perpetrating state's trade, and (3) the first two premises enter the calculations of political decision-makers. In this article, we focus exclusively on premise 2. We do *not* empirically test the hypothesis that greater trade reduces atrocity risk. Instead, we test hypotheses that state-sponsored genocide and mass atrocity disrupt trade. Premise 2 has been substantially empirically tested for interstate and intrastate conflicts and terrorism. To the best of our knowledge, no studies have empirically tested premise 2 for mass atrocities and only one has done so for genocide.

To preview our results, we find little empirical support that *genocides* disrupt trade and weak empirical evidence that *mass atrocities* disrupt trade. We arrive at these results from various pooled times-series cross-section analyses including OLS with fixed- and random-effects, Driscoll–Kraay standard error corrections, and Poisson pseudo maximum likelihood for a sample of 175 nations for the period 1970–2017. The results are further supported through application of interrupted times-series methodology to data for various countries. Numerous robustness checks available in Supplementary Tables are generally consistent with these

1 Empirical studies of *genocide* risk that consider trade include the seminal contributions of Krain (1997) and Harff (2003). Follow-on genocide risk studies that consider trade include Hazlett (2011), Esteban, Morelli, and Rohner (2015), Colaresi and Carey (2008), Krain (2012, 2014, 2017), Anderton and Carter (2015), Brehm (2017), and Nichols (2018). Empirical studies of *mass atrocity* risk that consider trade include Aydin and Gates (2008), Ulfelder and Valentino (2008), Lee (2015), Anderton and Ryan (2016), Krcmaric (2018), and the Early Warning Project (2020). Country trade measures vary across these studies and include exports plus imports as a fraction of world trade (Krain's studies), volume of trade (exports plus imports) (Colaresi and Carey 2008), and, most often, trade openness (exports plus imports divided by GDP) (all remaining studies noted above). The results associated with trade vary across these studies including those that find a significant inverse relationship between trade and genocide or mass atrocity risk (Early Warning Project 2020; Esteban, Morelli, and Rohner 2015; Harff 2003, Hazlett 2011; Lee 2015; Ulfelder and Valentino 2008) and those that report mixed or insignificant effects (Anderton and Carter 2015; Anderton and Ryan 2016; Aydin and Gates 2008; Brehm 2017; Colaresi and Carey 2008; Krain 1997; Krcmaric 2018; Nichols 2018). For an extensive survey of this empirical literature see Anderton and Brauer (2020).

2 For interstate conflict, e.g. Martin, Mayer, and Thoenig (2008), Kinne (2012), and Morelli and Sonno (2017). For intrastate conflict, e.g. Barbieri and Reveuny (2005), Bussmann and Schneider (2007), and Schneider (2014).

findings. We believe these results matter for both public policy and future research. From a policy perspective, our results suggest that, in most circumstances, state perpetrators of mass atrocity need not be too concerned that such acts will disrupt their state's trade. For future research on possible atrocity reduction associated with trade, scholars will need to explore why such a result should be expected if in fact mass atrocities do not generally disrupt trade.

The article proceeds as follows. We conceptually distinguish genocide and mass atrocity in Section 2. Section 3 surveys relevant empirical literature. Section 4 offers theoretical grounding based on the gravity model of trade. In Section 5 we empirically test the trade disruption premise for genocide and mass atrocity using pooled times-series cross-section analyses, while Section 6 does the same for various countries based upon interrupted times-series methods. Section 7 concludes.

2 Mass Atrocity and Genocide: Conceptual Distinctions and Datasets

In the field of genocide studies, *mass atrocity* is used by some as an umbrella category that encompasses genocides, politicides, war crimes, and crimes against humanity (Anderton and Brauer 2020; Gurr 2019, 60; Scheffer 2006). *Genocide* is defined in international law by the 1948 UN Genocide Convention as "acts committed with intent to destroy, in whole or in part, a national, ethnical, racial, or religious group, as such." *Politicide* is the intentional destruction, in whole or in part, of a political group, but it is not part of the UN Genocide convention and is not codified in international law. *War crimes* are violations of the laws and customs applicable to war between or within states, while *crimes against humanity* are systematic acts of harm committed against civilians as individuals rather than as members of a group. Harff (2019, 2) distinguishes genocides and politicides from nongenocidal mass atrocities; for the latter, she emphasizes that "there is no evident intent to destroy the group(s) to which they [the victims] belong." On the other hand, Valentino (2004, 10) lumps genocidal and non-genocidal atrocities together based on the rationale that "understanding the causes of the systematic murder of noncombatants is important, regardless of the group identity of the victims." Reflecting these distinct conceptualizations (and other diverse views in the genocide studies literature), we treat mass atrocity as an umbrella term and genocides and politicides as distinct from other forms of mass atrocity owing to the latter's targeting of a specifically identifiable group(s).

We emphasize three additional points regarding atrocity concepts in genocide studies. First, lack of consensus on central concepts permeates the field

(Anderton and Brauer 2020). Our objective is not to "solve" such debates.[3] Second, for convenience we use the word "genocide" to encompass genocides and politicides. This does not imply that we view them as conceptually indistinct. Third, and most important given our research objective, is that two major datasets in the field of genocide studies allow us to test hypotheses on the possible trade disruption effects of genocides and mass atrocities, respectively. The first is the Political Instability Task Force Genocide-politicide Dataset (Marshall, Gurr, and Harff 2019), which provides information on state-perpetrated *genocides* from 1955 to 2018. The second is the Ulfelder and Valentino (UV) (2008) *mass atrocity* dataset, which provides information on state-perpetrated mass atrocities from 1950 to 2006. To extend the data to recent years, we use the Ulfelder (2017) dataset for the period 2007–13. To generate data for the years 2014–8, we apply the UV coding protocols to the UCDP One-sided Violence Dataset (Eck and Hultman 2007; Pettersson, Högbladh, and Öberg 2019). Hence, state-perpetrated mass atrocity data cover the period 1950–2018. This data conceptualizes mass atrocity broadly based on Valentino (2004).

3 Overview of Empirical Literature

There is substantial empirical literature on conflict's disruption of trade. The literature includes empirical studies of the impacts on trade of interstate conflict (Anderton and Carter 2001; Barbieri and Levy 1999; Blomberg and Hess 2006; Feldman and Sadeh 2018; Karam and Zaki 2016; Keshk, Pollins, and Reuveny 2004; Long 2008, Glick and Taylor 2010; Mansfield and Bronson 1997; Mansfield and Pevehouse 2000; Marano, Cuervo-Cazurra, and Kwok 2013; Morrow, Siverson, and Taberes 1998, 1999; Pollins 1989a, 1989b; Qureshi 2013; Wu et al. 2016). A majority of these studies find some significant empirical evidence that interstate conflict disrupts trade, but there are exceptions (e.g. 1999; Barbieri and Levy 1999; Blomberg and Hess 2006; Mansfield and Pevehouse 2000; Morrow, Siverson, and Taberes 1998). The trade disruption premise has also been tested for civil conflict (Ahsan and Iqbal 2020; Bayer and Rupert 2004; Blomberg and Hess 2006; Calì et al. 2015; Karam and Zaki 2016; Long 2008; Marano, Cuervo-Cazurra, and Kwok 2013; Muhammad, D'Souza, and Amponsah 2013; Qureshi 2013). All of these studies

3 On just geno-politicide alone, Straus (2001) surveys 21 definitional variations. Importantly, scientific progress does not require consensus on key concepts. Giere (1979, 66) states that "good theories *do not have* to give precise definitions of their key concepts. Indeed, it is almost a necessary (but not sufficient) condition for a theory to be really *new* and important that it introduce new concepts, which, just because they are genuinely new, cannot be defined solely in terms of the familiar, well-understood concepts" (his emphases).

report at least some significant evidence that civil conflict disrupts trade. Other empirical research provides evidence that terrorism (Blomberg and Hess 2006; De Sousa, Mirza, and Verdier 2018), nonstate conflict (Karam and Zaki 2016), and aggregate indices of conflict and insecurity (Blomberg and Hess 2006; Chacha and Edwards 2019; Gupta et al. 2019) correlate to disrupted trade.

Despite extensive literature testing trade disruption for various forms of conflict and insecurity, there are few empirical studies that test the trade disruption premise for genocide and mass atrocity. Regarding *genocide*, to the best of our knowledge, only Blomberg and Hess (2006) test the trade disruption premise. They find that genocide significantly disrupts trade based on a sample of 177 countries for 1968–99. Karam and Zaki (2016) test the trade disruption premise based on the UCDP One-sided Violence Dataset, but a large majority of the observations are acts of "low-level" violence against civilians (VAC) as distinct from cases of genocide or mass atrocity. Karam and Zaki (2016) find that acts of VAC significantly disrupt trade in some regressions depending on the trade sector. To the best of our knowledge, there are no empirical studies of the possible impacts of *mass atrocity* on trade. Hence, we add to the literature by testing the trade disruption premise for both genocide and mass atrocity.

4 Theoretical Considerations and Hypotheses Based on the Gravity Model of Trade

A large majority of the empirical literature on conflict's disruption of trade is theoretically grounded in the gravity model of trade.[4] Long (2008, 84), for example, notes that the gravity model of trade assumes that general equilibrium holds for all country dyads and, therefore, "world trade equals import demand and export supply for each pair of countries." If one additionally assumes that bilateral trade flows are small relative to a state's total trade, each state has a negligible impact on prices and on the incomes of states (small country assumption), identical utility and production functions across countries, perfect substitutability of goods in production and consumption across countries, and no arbitrage opportunities owing to differences in spot exchange rates, then the following equation will hold (Long 2008, 84–5):

4 E.g. Bayer and Rupert (2004), Blomberg and Hess (2006), Long (2008), Glick and Taylor (2010), Marano, Cuervo-Cazurra, and Kwok (2013), Qureshi (2013), Calì et al. (2015), Karam and Zaki (2016), De Sousa, Mirza, and Verdier (2018), Feldman and Sadeh (2018), Chacha and Edwards (2019), Gupta et al. (2019), and Ahsan and Iqbal (2020). For overviews of the gravity model of trade, see Anderson and van Wincoop (2003, 2004) and Head and Mayer (2014).

$$TV_{ij} = (Y_i)^{\beta_1} (Y_j)^{\beta_2} (C_{ij})^{\beta_3} (T_{ij})^{\beta_4}. \tag{1}$$

In (1), TV_{ij} is the value of trade from country i to country j, Y_z ($z = i, j$) is GDP for country z, C_{ij} is the transportation cost of goods shipped from i to j, and T_{ij} represents the tariff and other barriers (assumed > 0) against i's goods being shipped to j. The log-linear version of (1) is:

$$LN(TV_{ij}) = \beta_1 LN(Y_i) + \beta_2 LN(Y_j) + \beta_3 LN(C_{ij}) + \beta_4 LN(T_{ij}). \tag{2}$$

Based on the theoretical foundations of Equation (1), empirical estimation of (2) would lead one to expect $\beta_1 > 0$, $\beta_2 > 0$, $\beta_3 < 0$, and $\beta_4 < 0$.

We emphasize three additional points. First, the assumptions undergirding (1) are heroic. We therefore are not attempting to test gravity equation theory nor to come up with refined empirical estimations of gravity equations (but see Anderson and van Wincoop 2003, 2004). Rather, we look to the gravity model of trade for insights into the variables that we should consider in our empirical tests of the effects of genocide and mass atrocity on trade. These variables include the value of a country's trade, the GDP values of a country and its trade partner(s), any "hindrances" to trade such as trade restrictions, and "helps" to trade such as free trade agreements. Given the many possible "hindrances" and "helps" to a country's trade, we find many variables considered in gravity-like empirical estimations even within the "conflict disrupts trade" literature.[5] We too will use several additional variables beyond those in the basic gravity model in our empirical analyses. Second, many empirical studies based on gravity models use real (inflation-adjusted) measures of trade and GDP or include price index information. We also use real values for trade and GDP. Finally, many empirical applications of gravity equation ideas focus on trade between pairs of states (i and j) and their GDPs (Y_i and Y_j) such as in Equations (1) and (2). In our empirical approach, we treat "country" j as the rest-of-the-world and our unit of analysis is the country-year. We thus focus on each country's real exports plus real imports (total trade) vis-à-vis the rest-of-the world. The GDP measures in our empirical estimations are then the real GDP of country i and the world real GDP minus country i's real GDP. One advantage of this approach is that even if a genocide- or mass atrocity-perpetrating country is being sanctioned by several actors, the overall trade impact on

5 Such variables include alliances (Bayer and Rupert 2004; Long 2008), democracy (Bayer and Rupert 2004; Long 2008), preferential trade arrangements (Calì et al. 2015; De Sousa, Mirza, and Verdier 2018; Gupta, et al. 2019; Karam and Zaki 2016; Long 2008), shared border (Chacha and Edwards 2019; De Sousa, Mirza, and Verdier 2018; Glick and Taylor 2010; Gupta et al. 2019; Karam and Zaki 2016; Long 2008), whether the country is landlocked (Glick and Taylor 2010), population (Glick and Taylor 2010; Marano, Cuervo-Cazurra, and Kwok 2013), business freedom (Marano, Cuervo-Cazurra, and Kwok 2013), a neighbor experiencing war or terrorism (De Sousa, Mirza, and Verdier 2018; Qureshi 2013), and an index of state fragility (Chacha and Edwards 2019).

the country could be minimal if the country has a set of amenable trade partners to which it can substitute its trade behavior.[6] Our empirical testing below empirically assesses the impact of genocide and mass atrocity on countries' *total real trade*. Hence, trade substitutions, if any, are built into the aggregated trade data.

Based on previous literature on the "conflict disrupts trade" premise and the gravity model of trade, we view genocides and mass atrocities, like other large-scale conflicts, as hindrances to real economic activity including real trade. An example from the field of genocide studies of the potential for trade disruption is provided by Straus (2016, 234): "highly visible mass atrocities will invite strong sanctions against them [atrocity perpetrators], whether in the form of criminal prosecution, arms embargoes, economic sanctions, or military intervention." Trade could also be hindered in atrocity contexts as businesses reorient their economic activities owing to increased uncertainty, supply chain disruptions, threats of consumer boycotts, and ethical obligations of business leaders. We thus test the following hypotheses:

H1. Genocide incidence in a country disrupts (reduces) the country's total real trade, all else equal.

H2. Mass atrocity incidence in a country disrupts (reduces) the country's total real trade, all else equal.

5 Empirical Research Design for Pooled Times-Series Cross-Section Analysis

5.1 Model and Variables

We use various empirical estimators to test the atrocity disrupts trade hypotheses for a sample of 175 countries over the period 1970–2017.[7] Guided by our literature review and theory section, our basic empirical model is:

6 Martin, Mayer, and Thoenig (2008) find empirical evidence of this for interstate war's disruption of trade.

7 The countries and country-years in the sample are based upon the Correlates of War State System Membership Dataset (https://correlatesofwar.org/data-sets/state-system-membership). Countries with population below 500 thousand in 2017 based on the World Bank Development Indicators are excluded from the sample (https://databank.worldbank.org/reports.aspx?source=world-development-indicators). The temporal domain of our main regressions, 1970–2017, is determined by the same temporal domain of one of our control variables. In supplementary regressions, we use substitute measures of selected variables, which allows us to test our hypotheses for genocide for 1955–2010 and for mass atrocity for 1950–2010.

$$\begin{aligned} LN(\text{Real Trade}) = \beta_0 &+ \beta_1 LN(\text{Real GDP}) + \beta_2 LN(\text{World Real GDP}) \\ &+ \beta_3 \text{Trade Globalization} + \beta_4 \text{Landlocked} + \beta_5 \text{Civil War} \\ &+ \beta_6 \text{Atrocity} + \epsilon. \end{aligned} \tag{3}$$

The country-year is the unit of analysis. Each variable in (3) is (initially) specified at time t. Country and time notations for the variables have been suppressed for ease of notation.

Real Trade: Our dependent variable is measured as a country's real (inflation-adjusted) exports plus imports per year. Trade data come from World Bank Development Indicators for 1960–2018. The data encompass the sum of the value of a country's exports and imports of goods and services per year measured in billions of 2010 US dollars. Unless otherwise specified, real trade data are log-transformed.

Real GDP: Data on real GDP per country in billions of 2010 U.S. dollars come from World Bank Development Indicators for 1960–2018. Real GDP data are log-transformed. We expect real GDP to positively impact real trade.

World Real GDP: Data on world real GDP in billions of 2010 US dollars come from World Bank Development Indicators for 1960–2018. Per country, the data encompass the real value of the world's GDP minus the country's real GDP. World real GDP data are log-transformed. We expect world real GDP to positively impact real trade.

Trade Globalization: The post-World War II world economy has experienced growth in trade agreements and reductions in tariffs and nontariff trade barriers (NTBs). The KOF Swiss Economic Institute's Globalisation Index contains a trade globalization *de jure* component, *KOFTrGldj*, which "measures policies and conditions that, in principle, enable, facilitate and foster [trade] flows and activities" (Gygli et al. 2019, 544). The *KOFTrGldj* index is not based on actual or *de facto* trade flows, but reductions of frictions that hamper a country's international trade (i.e. tariffs and NTBs) and a country's number of bilateral and multilateral free trade agreements. The *KOFTrGldj* index ranges from 0 to 100 and is available for 1970–2017. We expect trade globalization to positively impact real trade.

Landlocked: Owing to lack of direct access to open seas shipping, landlocked countries can face greater friction to international trade than non-landlocked countries (Glick and Taylor 2010). Landlocked information is not part of the *KOFTrGldj* index, so we add it as a measure. Data on landlocked states come from the World Bank and are generally time-invariant over our sample period.[8] We expect landlocked to negatively impact real trade.

8 An exception is Yugoslavia, which was not landlocked given its border on the Adriatic Sea. Following the breakup of Yugoslavia in 1992, the newly formed states had their own characteristics of being landlocked or not, including Serbia, Kosovo, and Macedonia, which are landlocked according to World Bank data.

Civil War: Genocide and mass atrocity often overlap with civil war. To assess genocide and mass atrocity impacts on trade, we control for civil war impacts on trade. Civil war data come from the UCDP/PRIO Armed Conflict Dataset version 19.1 and cover 1950–2018 (Gleditsch et al. 2002; Pettersson, Högbladh, and Öberg 2019). We code the presence (1) or absence (0) of civil war per country-year. We expect civil war to negatively impact real trade.

Atrocity: We code the incidence (1) or absence (0) of genocide per country-year for 1955–2018 using the Political Instability Task Force Genocide-politicide Dataset (Marshall, Gurr, and Harff 2019). As noted above, we draw upon the Ulfelder and Valentino (UV) (2008) dataset and its extensions to code the incidence (1) or absence (0) of mass atrocity per country-year for the period 1950–2018. We expect genocide and mass atrocity to exert a negative effect on real trade. Table 1 presents descriptive statistics.

5.2 Empirical Results

We begin with OLS. Table 2, column 1 shows estimated coefficients and cluster robust standard errors for genocide's impact on trade. We show two-sided *p*-values, noting that expected signs of our coefficient estimates are properly one-sided. Each coefficient estimate in column 1 has the correct sign and is significant except for our main variable of interest, genocide. Column 2 estimates the model with country fixed-effects and column 3 with random-effects. The results show no significant trade disruption from genocide. In columns 4–6, we substituted mass atrocity for genocide. All three coefficient estimates for mass atrocity are

Table 1: Descriptive statistics.

Variable	Obs	Mean	Standard deviation	Minimum	Maximum
Real trade	5982	157.5	411.6	0.004	5568.2
LN real trade	5982	3.2	2.1	−5.4	8.6
Real GDP	7628	295	1102	0.2	17,856
LN real GDP	7628	3.4	2.1	−1.8	9.8
World real GDP	7628	44,043	19,875	8188	82,456
LN world real GDP	7628	10.6	0.5	9.0	11.3
Trade globalization	6920	47.7	24.2	5.0	97.8
Landlocked	10,219	0.2	0.4	0	1
Civil war	10,050	0.05	0.22	0	1
Genocide	9287	0.03	0.17	0	1
Mass atrocity	10,008	0.15	0.36	0	1

Definitions and sources are indicated in the text.

Table 2: Empirical models of impact of genocide and mass atrocity on trade.

	(1) OLS (Std. error) [*p*-Value]	(2) OLS fixed effects (Std. error) [*p*-Value]	(3) OLS random effects (Std. error) [*p*-Value]	(4) OLS (Std. error) [*p*-Value]	(5) OLS fixed effects (Std. error) [*p*-Value]	(6) OLS random effects (Std. error) [*p*-Value]
Constant	−5.961***	−5.915***	−6.239***	−5.996***	−5.922***	−6.232***
	(0.765)	(1.002)	(0.785)	(0.771)	(1.005)	(0.792)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Real GDP	0.815***	0.990***	0.946***	0.820***	0.987***	0.944***
	(0.026)	(0.100)	(0.062)	(0.026)	(0.100)	(0.062)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
World real GDP	0.528***	0.479***	0.527***	0.534***	0.481***	0.528***
	(0.071)	(0.126)	(0.089)	(0.072)	(0.126)	(0.089)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Trade globalization	0.011***	0.006***	0.006***	0.011***	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
	[0.000]	[0.003]	[0.001]	[0.000]	[0.003]	[0.001]
Landlocked	−0.269*		−0.103	−0.268*		−0.107
	(0.145)		(0.131)	(0.144)		(0.132)
	[0.066]		[0.429]	[0.066]		[0.417]
Civil war	−0.320***	−0.095**	−0.099**	−0.222***	−0.077*	−0.079*
	(0.093)	(0.040)	(0.039)	(0.079)	(0.045)	(0.044)
	[0.001]	[0.019]	[0.011]	[0.005]	[0.089]	[0.075]
Genocide	−0.134	0.020	0.015			
	(0.144)	(0.081)	(0.080)			
	[0.354]	[0.804]	[0.850]			
Mass atrocity				−0.181	−0.031	−0.039
				(0.124)	(0.076)	(0.075)
				[0.146]	[0.678]	[0.605]
Fixed effects	No	Yes	No	No	Yes	No
Random effects	No	No	Yes	No	No	Yes
R^2	0.879	0.872 (overall)	0.874 (overall)	0.880	0.872 (overall)	0.874 (overall)
N	5181	5181	5181	5180	5180	5180

Dependent variable is real trade (logged). Estimations in columns 1–6 are with cluster robust standard errors in parentheses and *p*-values in brackets. *$p \le 0.10$, **$p \le 0.05$, ***$p \le 0.01$ (two-sided).

negative, but none reach conventional levels of significance. One could argue that mass atrocity has a significant negative effect on trade at the 10% level for a one-sided test in column 4, but this is weak evidence of trade disruption. We conclude from Table 2 that there is no empirical evidence that genocide disrupts trade and weak evidence at best that mass atrocity disrupts trade.

The OLS methods used in Table 2 are not without serious criticisms. Pooled times-series cross-section (PTSCS) analysis usually suffers from one or more violations of the assumptions of standard methods including: (1) serially correlated residuals, (2) residuals that have different variances for different countries (panel heteroscedasticity), (3) residuals of different countries are contemporaneously correlated, (4) residuals of country *i* co-varies with residuals of country *j* at different points in time, and (5) expected mean of the error term differs from zero for different countries (Troeger 2019, 3). Given such challenges, alternative methods have been employed in the PTSCS literature. For example, Salim, Kabir, and Mawali (2011) apply Driscoll and Kraay standard error corrections to estimate a gravity model of trade between Gulf Cooperation Council states. According to Hoechle (2007, 284), the Driscoll–Kraay correction "is robust to very general forms of cross-sectional as well as temporal dependence."[9] In studying the impact of the Syrian conflict on Lebanon's trade, Calì et al. (2015), following Silva and Tenreyro (2006), maintain that log transformation of the gravity model undermines the consistency of the OLS estimator under heteroscedasticity. Hence, they use the level rather than the log of trade and deploy a Poisson pseudo-maximum-likelihood (PPML) estimator, as do others in the conflict disrupts trade literature (Glick and Taylor 2010; Gupta et al. 2019; Karam and Zaki 2016). [10,11] For our dataset, we find significant evidence for both autocorrelation and heteroscedasticity, so alternative estimation methods to standard OLS are essential.[12] Following this literature, we use OLS fixed-effects with Driscoll–Kraay standard error corrections and PPML estimations.

9 Owing to heteroscedasticity and autocorrelation, Newey–West standard error corrections are also sometimes chosen in the PTSCS literature. In fact, our estimations of Equation (3) using Newey–West corrections weakly support, for a one-sided test, hypothesis H1 (for genocide) and strongly support hypothesis H2 (for mass atrocity). We believe, however, that Driscoll–Kraay corrections are preferred to Newey–West because the latter can correct for heteroscedasticity and autocorrelation, but the former can correct for heteroscedasticity, autocorrelation, and cross-sectional dependence (Hoechle 2007, 285).

10 In the conflict disrupts trade literature, Karam and Zaki (2016) and Gupta et al. (2019) use robust standard errors and Glick and Taylor (2010) and Calì et al. (2015) use cluster robust standard errors when deploying PPML. We use cluster robust standard errors with PPML because it allows for autocorrelation and heteroscedasticity (both present in our data) and it is appropriate with a large cross-section (175 in our case) and a not too large times-series (47 years) (Wooldridge 2020, 798).

11 Silva and Tenreyro (2006, 645) note that PPML does not require the data to be Poisson and the dependent variable measure does not have to be integer.

12 The Wooldridge test for no first-order autocorrelation can be rejected ($F = 385.58$, $p < 0.000$). The Breusch–Pagan test for no heteroscedasticity based on all right-side variables can be rejected ($F = 16.73$, $p < 0.000$).

Table 3 shows the results of our empirical model based on the various estimations for the impact of genocide (columns 1–2) and mass atrocity (columns 3–4) on trade. None of the coefficient estimates on our variables of interest, genocide in columns 1 and 2 and mass atrocity in columns 3 and 4, empirically support our trade disruption hypotheses.

Table 3: Additional empirical tests of genocide and mass atrocity impacts on trade.

	(1) Driscoll–Kraay fixed effects (Std. error) [*p*-Value]	**(2) Poisson pseudo Maximum likelihood (Std. error) [*p*-Value]**	**(3) Driscoll–Kraay fixed effects (Std. error) [*p*-Value]**	**(4) Poisson pseudo Maximum likelihood (Std. error) [*p*-Value]**
Constant	−5.915***	−8.265***	−5.922***	−8.330***
	(0.482)	(0.616)	(0.463)	(0.596)
	[0.000]	[0.000]	[0.000]	[0.000]
Real GDP	0.990***	0.661***	0.987***	0.661***
	(0.040)	(0.078)	(0.039)	(0.076)
	[0.000]	[0.000]	[0.000]	[0.000]
World real GDP	0.479***	0.879***	0.481***	0.896***
	(0.062)	(0.078)	(0.059)	(0.090)
	[0.000]	[0.000]	[0.000]	[0.000]
Trade globalization	0.006***	0.007	0.006***	0.005
	(0.001)	(0.007)	(0.001)	(0.008)
	[0.000]	[0.312]	[0.000]	[0.487]
Landlocked		−0.143		−0.157
		(0.289)		(0.298)
		[0.621]		[0.597]
Civil war	−0.095**	−0.464	−0.077*	−0.291*
	(0.038)	(0.287)	(0.044)	(0.165)
	[0.017]	[0.106]	[0.083]	[0.078]
Genocide	0.020	−0.358		
	(0.062)	(0.463)		
	[0.744]	[0.440]		
Mass atrocity			−0.031	−0.411
			(0.043)	(0.375)
			[0.469]	[0.273]
R^2	0.845	0.770	0.845	0.768
N	5181	5181	5180	5180

Dependent variable is real trade (logged) except for PPML in columns 2 and 4; see text for rationale. Columns 1 and 3 estimations are with Driscoll–Kraay SEs. Columns 2 and 4 estimations are with cluster robust SEs. Standard errors in parentheses and p-values in brackets. *$p \le 0.10$, **$p \le 0.05$, ***$p \le 0.01$ (two-sided).

5.3 Robustness Checks

We ran many robustness checks of our PTSCS regressions (available in Supplementary Tables S1–S7). Specifically, we reran the models in Tables 2 and 3 with lagged right-side variables (except landlocked), a lower intensity measure of civil strife (civil conflict) substituting for civil war, mass atrocity/civil war and genocide/civil war interactions, a Cold War dummy variable, and a measure of genocide severity substituting for genocide incidence. We also reran the columns (1) and (4) regressions of Table 2 with measures for interstate war and extra-state war. Furthermore, we reran the models in Table 3 for alternative temporal domains, 1955–2010 and 1950–2010, for genocide and mass atrocity, respectively.[13] Lastly, we ran the model using a standard error correction similar to panel-corrected standard error methodology.[14] Results were generally unaffected in the supplementary regressions.[15] We considered collinearity between our measures of civil war and genocide/mass atrocity, but we concluded this was not a major problem.[16]

13 To do this, we used Penn World Table (PWT) 7.1 data on real merchandise trade (logged) to measure trade, Year to proxy trade globalization, PWT 9.1 data to measure a country's real GDP, and we substituted PWT 9.1 data on real income per capita for world real GDP.

14 Owing to the unbalanced nature of our sample, we are unable to correct for heteroscedasticity and autocorrelation using panel-corrected standard errors (pcse). We do present in our Supplementary Table S7, however, the results of our model based upon what Stata (2020, 8) characterizes as "not quite PCSEs" estimation method that accounts for autocorrelation and panel-level heteroscedasticity.

15 Genocide and mass atrocity lead to significant trade disruption in the Driscoll–Kraay regressions for the alternative temporal domains. Nevertheless, across the 34 regressions (18 for genocide and 16 for mass atrocity) in the Supplementary Tables, genocide significantly disrupts trade in one regression and mass atrocity does so in two regressions. Meanwhile, civil war or civil conflict significantly disrupts trade in 25 of the 34 supplementary regressions.

16 There is multicollinearity in our empirical model, but we do not believe it is serious enough to undermine our results. First, multicollinearity does not lead to biased coefficient estimates. Second, the pairwise correlation coefficients for civil war and genocide and civil war and mass atrocity are 0.340 and 0.395, respectively, and thus not too large (Gujarati and Porter 2009, 338). Third, the variance inflation factor (VIF) for civil war and genocide based on Table 2 column (1) regression is 1. 13 for each. The VIFs for civil war and mass atrocity based on Table 2 column (4) regression are 1.29 and 1.41, respectively. These are well below the value of 10, which is sometimes suggested as indicating that multicollinearity is a problem (Wooldridge 2020, 92). Lastly, we checked out Klein's rule of thumb, as summarized in Gujarati and Porter (2009, 339), in which the R^2 values on our auxiliary regressions of each X variable on the other X variables are quite low relative to the R^2 value of the main regression. None of these checks are formally definitive, but together they suggest that multicollinearity is not likely a serious problem.

6 Empirical Research Design for Country-Specific Times-Series Analysis

6.1 Model and Variables

We use the interrupted times-series (ITS) model of Lewis-Beck and Alford (1980) to empirically test the trade disruption hypotheses for individual countries over time. We begin with each country in the world that experienced a state-perpetrated genocide or mass atrocity during the period 1950–2018. For each genocide and mass atrocity case, we require that real trade data exist for each year during the genocide or atrocity as well as 10 years before and after the genocide/atrocity period. For each of the nine genocide and 17 mass atrocity cases that met these requirements, we estimated the following equation:

$$LN\,(\text{RealTrade}_t) = \beta_0 + \beta_1\text{Trend}_t + \beta_2\text{AtrocityLevel}_t + \beta_3\text{AtrocityTrend}_t + \beta_4\text{PeaceLevel}_t + \beta_5\text{PeaceTrend}_t + \epsilon_t. \tag{4}$$

Real Trade: Our dependent variable is measured as a country's real (inflation-adjusted) exports plus imports per year. As in the previous section, the trade data come from the World Bank Development Indicators and cover the years 1960–2018. The trade data are log-transformed. In some cases of genocide and mass atrocity, World Bank trade data were not sufficiently available to complete a country's 10 year before, during atrocity, and 10 years after times-series. In such cases we derived real trade data from Penn World Table (PWT) 7.1, which is available for 1950–2010.[17]

Atrocity: As in the previous section, we code the incidence (1) or absence (0) of genocide per country-year for the period 1955–2018 based on the Political Instability Task Force Genocide-politicide dataset (Marshall, Gurr, and Harff 2019). Similarly, we use the previous section's extended Ulfelder and Valentino dataset to code the incidence (1) or absence (0) of mass atrocity per country-year for the period 1950–2018.

Trend and Level Measures: Following Lewis-Beck and Alford (1980), Trend is a counter (1, 2, 3, …) for each year of a country's series. Atrocity Level is a dichotomous variable equal to zero for each year prior to genocide or atrocity onset and one for each year thereafter. Atrocity Trend is zero for each year prior to

17 The World Bank trade data are real values of exports and imports of goods *and* services. The PWT 7.1 data are real values of exports and imports of goods (i.e. merchandise) only. The PWT trade data were derived by multiplying a country's real GDP shares of merchandise exports and merchandise imports by its real GDP. Real trade data per country could not be derived from PWT versions after PWT 7.1.

genocide or atrocity onset and then 1, 2, 3, … from onset to the end of the series. Peace Level is equal to zero for each year before and during genocide or atrocity and then 1, 2, 3, … to the end of the series. Peace Trend years are zero up through the last year of the genocide or atrocity and then 1, 2, 3, … to the end of the series. Of the nine genocide and 17 mass atrocity cases that align with our requirements for interrupted times-series analysis, Rwanda 1994 and Sri Lanka 1971 experienced the case within one calendar year. For such cases, Atrocity Trend and Peace Level drop from the model, and Peace Trend is set to zero for each year prior to atrocity or genocide onset and then 1, 2, 3, … to the end of the series.[18]

Figure 1 is a prototype of the ITS model. Time periods (years) are plotted on the x axis and the country's real trade on the y axis. Parameters β_0 and β_1 in Equation (4) represent the level and rate of growth of real trade prior to atrocity. Suppose the country's real trade is growing at 8% per year along segment ABC in the figure. At time 3, suppose atrocity occurs. If this diminishes the country's real trade trend from B to D, it would indicate a disruption in trade as represented by $\beta_2 < 0$. This is one way in which the ITS methodology can detect trade disruption. A second type of trade disruption occurs even if β_2 is not significantly negative. Suppose in Figure 1 that real trade grows at 2% during the atrocity period rather than the 8% rate that existed previously. If the rate of growth of trade diminishes during the atrocity period, it would be captured by $\beta_3 < 0$. Note that empirical evidence for trade disruption is present if β_2 *or* β_3 is negative and significant. The remaining parts of Figure 1 deal with real trade in the post-atrocity period. The termination of atrocity might lead to an updraft in real trade as shown by segment EF in the figure, which would be represented by $\beta_4 > 0$. Lastly, if real trade grew at say 5% after the atrocity, it would be represented by $\beta_5 > 0$. In the post-atrocity period, real trade might be damped down as the country takes time to recover (β_4 and/or β_5 not significantly positive) or it might ramp back up (β_4 and/or β_5 significantly positive). We are not concerned in this article about trade in the post-atrocity period. Rather, we focus on whether there is significant evidence that $\beta_2 < 0$ or $\beta_3 < 0$.

6.2 Empirical Results

For each of the nine genocide and 17 atrocity cases that meet our criteria for ITS analysis, we estimate Equation (4). We use OLS if there is no significant evidence of autocorrelation and heteroscedasticity. If autocorrelation is present but not

18 Some countries experienced both genocide and mass atrocity with some years that overlapped (genocide is a form of mass atrocity). We did not blend cases of genocide and mass atrocity; rather, we applied interrupted times-series methodology separately to genocide and mass atrocity.

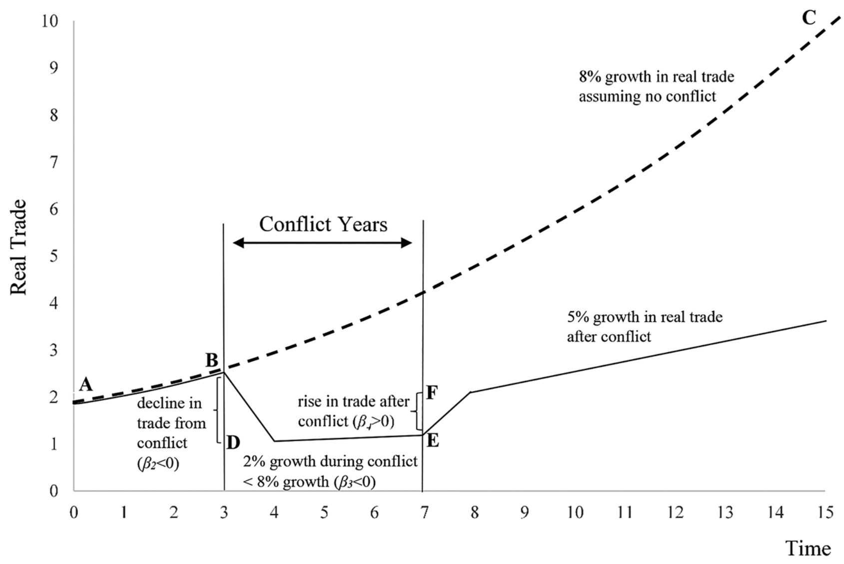

Figure 1: Prototype of the interrupted times-series model. Source: Adapted from Anderton and Carter (2001. p. 450).

heteroscedasticity, we estimate the model using Prais–Winsten. If heteroscedasticity but not autocorrelation is present, we use OLS with robust standard errors. If autocorrelation and heteroscedasticity are present, we use Newey–West standard errors in which the lag is determined by the formula in Stock (2015, 5).

Table 4 presents the coefficient estimates for the ITS model for our nine genocide cases and Table 5 does the same for our 17 mass atrocity cases. Our particular interest is whether there is significant evidence of a downdraft in trade ($\beta_2 < 0$) or a decline in the rate of growth of trade ($\beta_3 < 0$) associated with genocide or mass atrocity. With the exception of Rwanda, Table 4 provides no significant empirical evidence that genocide disrupts trade. In Table 5 (focusing on mass atrocity and trade), β_2 or β_3 is negative in 13 of the 17 cases, but only four show β_2 or β_3 being negative and significant and the other not being positive and significant (i.e. Dominican Republic, El Salvador, Sri Lanka, Syria).[19] Our results in Tables 4 and 5 contrast sharply with the ITS results of the effects of interstate war on trade presented in Anderton and Carter (2001) wherein they report statistically significant evidence of trade disruption

19 If β_2 or β_3 is negative and significant but the other is positive and significant, we did not count the case as one of significant trade disruption. This is a "conservative" protocol for the three cases in Table 5 in which β_2 is negative and significant and β_3 is positive and significant or vice versa.

Table 4: Impact of genocide on trade.

Country (genocide years in parentheses)	Constant (β_0)	Trend (β_1)	Atrocity Level (β_2)	Atrocity Trend (β_3)	Peace Level (β_4)	Peace Trend (β_5)	Rho	Chi2	R^2
Argentina, 1966-90	23.45***	0.02**	−0.06	0.12***	−0.21***	−0.13*			0.95
(1976–80)	(0.000)	(0.028)	(0.480)	(0.000)	(0.010)	(0.000)			
Chile, 1963-86	22.70***	0.04***	−0.08	−0.02	0.34***	0.00		3.59*	0.95
(1973–76)	(0.000)	(0.000)	(0.416)	(0.516)	(0.007)	(0.996)		(0.058)	
Dem. Rep. Of Congo, 1967–89	22.05***	−0.01	0.16	−0.06	0.07	0.16	0.34*		0.69
(1977–79)	(0.000)	(0.820)	(0.419)	(0.593)	(0.668)	(0.120)	(0.100)		
Iran, 1971-2002	28.77***	−0.35***	−0.20	0.37***	−0.04	−0.02	0.53***		0.43
(1981–92)	(0.000)	(0.000)	(0.216)	(0.000)	(0.790)	(0.656)	(0.001)		
Philippines, 1962–86	22.28***	0.04***	−0.02	0.01	0.25*	−0.02	0.59***	7.24***	——
(1972–76)	(0.000)	(0.000)	(0.651)	(0.323)	(0.085)	(0.274)	(0.002)	(0.007)	
Rwanda, 1984-2004	20.03***	0.02	−0.41***			0.10***			0.88
(1994)	(0.000)	(0.193)	(0.003)			(0.000)			
Somalia, 1978-2001	12.45***	−0.09***	0.25	−0.10	−0.15	0.12			0.80
(1988–91)[a]	(0.000)	(0.008)	(0.493)	(0.441)	(0.613)	(0.117)			
Sri Lanka, 1979–2000	22.39***	0.05***	−0.09	0.03	0.02	0.01			0.99
(1989–90)	(0.000)	(0.000)	(0.301)	(0.635)	(0.694)	(0.902)			
Syria, 1971-92	16.10***	0.07***	−0.06	−0.16	−0.09	0.16			0.94
(1981–82)[a]	(0.000)	(0.000)	(0.731)	(0.160)	(0.365)	(0.159)			

Dependent variable is real trade (logged). *p*-values are shown in parentheses. *$p \le 0.10$, **$p \le 0.05$, ***$p \le 0.01$ (two-sided). To save space, standard errors are suppressed in the table. If there is no information in the Rho and Chi2 columns, OLS results are reported. If only Rho information is present, AR(1) results are reported. If only Chi2 information is present, OLS results with robust standard errors are reported. If both Rho and Chi2 are present, the case was estimated with Newey–West standard errors. [a]indicates that the case used PWT 7.1 rather than World Bank trade data.

Table 5: Impact of mass atrocity on trade.

Country (atrocity years in parentheses)	Constant (β_0)	Trend (β_1)	Atrocity Level (β_2)	Atrocity Trend (β_3)	Peace Level (β_4)	Peace Trend (β_5)	Rho	Chi2	R^2
Algeria, 1981–2015	24.91***	0.00	−0.17***	0.04***	0.04	−0.02***	0.53***	5.54**	——
(1991–2005)	(0.000)	(0.901)	(0.004)	(0.000)	(0.335)	(0.000)	(0.001)	(0.019)	
Argentina, 1966-93	23.49***	0.02	0.07	0.02	−0.13	0.03	0.36*		0.61
(1976–83)	(0.000)	(0.663)	(0.580)	(0.688)	(0.266)	(0.518)	(0.077)		
Chile, 1963-88	22.70***	0.04***	−0.20**	0.04*	0.20**	−0.06***		2.99*	0.96
(1973–78)	(0.000)	(0.000)	(0.046)	(0.056)	(0.031)	(0.010)		(0.083)	
Congo (Rep.), 1982–2013	22.28***	0.04	−0.10	0.01	−0.09	0.00	0.41**		0.78
(1992–2003)	(0.000)	(0.184)	(0.181)	(0.716)	(0.230)	(0.857)	(0.026)		
Dominican Rep., 1955–88	14.94***	0.08**	−0.42***	0.01	0.12	−0.09***	0.38**		0.83
(1965–78)	(0.000)	(0.027)	(0.000)	(0.852)	(0.235)	(0.001)	(0.036)		
El Salvador, 1967–2002	21.82***	0.04***	0.03	−0.06***	0.53***	0.09***	0.58***	4.09**	——
(1977–92)	(0.000)	(0.000)	(0.840)	(0.003)	(0.005)	(0.000)	(0.000)	(0.043)	
Ethiopia, 1951–2001	16.03***	−0.10	0.01	0.11	−0.15	0.05	0.57***		0.21
(1961–91)[a]	(0.000)	(0.351)	(0.964)	(0.255)	(0.244)	(0.224)	(0.000)		
Jordan, 1960-81	14.92***	0.08***	−0.25	−0.02	0.07	0.12	−0.58***		0.99
(1970–71)	(0.000)	(0.000)	(0.152)	(0.851)	(0.338)	(0.266)	(0.006)		
Papua New Guinea, 1978–2008	15.61***	0.01	−0.01	0.02	0.04	0.02	0.41**		0.90
(1988–98)[a]	(0.000)	(0.403)	(0.938)	(0.370)	(0.542)	(0.123)	(0.026)		
Peru, 1970-2002	23.47***	0.02**	0.13**	−0.04***	0.25***	0.08***			0.95
(1980–92)	(0.000)	(0.011)	(0.036)	(0.001)	(0.000)	(0.000)			
Rwanda, 1980-2009	19.89***	0.03	−0.17	0.02	0.16	0.06*	0.41**		0.86
(1990–99)	(0.000)	(0.413)	(0.277)	(0.672)	(0.274)	(0.089)	(0.029)		
Sierra Leone, 1981–2012	19.75***	−0.01	0.16	0.02	0.20	0.14**	0.45**		0.70
(1991–2002)	(0.000)	(0.810)	(0.54)	(0.762)	(0.436)	(0.017)	(0.015)		

Table 5: (continued)

Country (atrocity years in parentheses)	Constant (β_0)	Trend (β_1)	Atrocity Level (β_2)	Atrocity Trend (β_3)	Peace Level (β_4)	Peace Trend (β_5)	Rho	Chi2	R^2
Somalia, 1972-2000	12.54***	−0.03	−0.14	−0.03	−0.64***	0.09**			0.87
(1982–90)[a]	(0.000)	(0.235)	(0.532)	(0.512)	(0.007)	(0.043)			
South Africa, 1966–2004	25.05***	0.00	−0.07	0.02	0.12**	0.03**	0.40**		0.82
(1976–94)	(0.000)	(0.976)	(0.169)	(0.460)	(0.030)	(0.028)	(0.012)		
Sri Lanka, 1961-81	22.08***	0.00	−0.15*				0.48**		0.43
(1971)	(0.000)	(0.955)	(0.098)				(0.036)		
Syria, 1969-95	15.94***	0.07***	0.00	−0.08***	0.05	0.08***		2.84*	0.96
(1979–85)[a]	(0.000)	(0.000)	(0.962)	(0.005)	(0.632)	(0.003)		(0.092)	
Turkey, 1974-2009	16.82***	0.07***	0.15	0.02	−0.01	−0.02	0.55***	6.14**	——
(1984–99)[a]	(0.000)	(0.003)	(0.265)	(0.478)	(0.910)	(0.187)	(0.001)	(0.013)	

Dependent variable is real trade (logged). *p*-values are shown in parentheses. *$p \le 0.10$, **$p \le 0.05$, ***$p \le 0.01$ (two-sided). To save space, standard errors are suppressed in the table. If there is no information in the Rho and Chi2 columns, OLS results are reported. If only Rho information is present, AR(1) results are reported. If only Chi2 information is present, OLS results with robust standard errors are reported. If both Rho and Chi2 are present, the case was estimated with Newey–West standard errors. [a]indicates that the case used PWT 7.1 rather than World Bank trade data.

in 21 of 27 cases. Using the same ITS methodology, we find statistically significant evidence of genocide's and mass atrocity's disruption of trade in only five of 26 cases.

7 Conclusions

We find no broad empirical support that genocide disrupts trade and at best weak evidence that mass atrocities disrupt trade. Our results contrast sharply with the trade effects of other conflict types reported in the literature. For interstate, intrastate, and nonstate conflict and terrorism, numerous studies have found some significant empirical evidence of trade disruption. Even within our own study we find that civil war, which we treat as a control variable, is much more prone to have a significant negative impact on trade than either genocide or mass atrocity. In all 10 regressions in Tables 2 and 3, the coefficient estimate on civil war is negative and it is significant in nine of them. Across these tables, only two of the five coefficient estimates on genocide are negative and none are significant. For mass atrocity, all five coefficient estimates are negative, but again, none reach conventional significance levels. For our interrupted times-series applications, we also find little support for our trade disruption hypotheses. Only one of nine genocide cases and four of 17 mass atrocity cases achieve empirical support.

Future empirical research on risk factors for genocide and mass atrocity should reassess hypotheses on why measures of trade might correlate to reduced risk. This is especially true for empirical research on genocide risks because there is little empirical support that genocide disrupts trade broadly conceived. Further research is necessary to determine whether potential and actual atrocity perpetrators perceive that they have enough trade partners that won't care about their atrocity acts so that any disrupted trade can easily be accommodated. Perhaps growth in globalization since the end of World War II makes it relatively easy for atrocity perpetrating regimes to find alternative trade partners when they face trade sanctions from a limited number of states. Of course, future research may find that certain bilateral trade relationships (as distinct from the broad-based monadic measures of trade used in our article) or trading in key or strategic commodities or products are disrupted by genocide (or mass atrocity). Hence, even if broad measures of trade do not seem to be disrupted by mass atrocity or genocide, finer-grained aspects of trade could be disrupted. These issues remain unaddressed in the literature and are thus topics for future research.

Our final conclusion focuses on public policy. Evidently, state perpetrators of genocide and other forms of mass atrocity need not worry too much that such acts will disrupt their trade. Of course, other aspects of a perpetrator's economy and society may be disrupted when they choose an atrocity path, but evidently trade is not one of them. Myanmar's treatment of the Rohingya people is a contemporary example of how mass atrocity does not seem to disrupt a perpetrating regime's trade, broadly conceived. Doctors Without Borders/Médecins Sans Frontières (MSF) (2020) provides a timeline (1977–present) of the severe persecutions and harsh living conditions afflicting the Rohingya people of Myanmar. MSF estimates that 860,000 Rohingya now reside in refugee camps in neighboring Bangladesh. The treatment of the Rohingya has been so severe that some have classified the case as genocide.[20] Nevertheless, the sustained mistreatment of hundreds of thousands of people from a particular group and the associated mass refugee flows have not disrupted Myanmar's aggregate trade. According to data from the World Bank, total real (inflation-adjusted) trade (exports + imports) for Myanmar has more than doubled from 2010 to 2018.[21] While some countries have imposed trade sanctions against Myanmar over the treatment of the Rohingya, other countries and businesses seem willing to cover for any lost trade.[22] Indeed, Rosyidin and Dir (2020) find it "puzzling" that ASEAN states have not imposed economic sanctions against Myanmar over the Rohingya crisis. Whatever benefits and costs are considered by political and military leaders in Myanmar over the treatment of the Rohingya people, evidently (to date) they need not worry too much about trade disruption costs. From a policy point of view, those seeking to raise the cost of atrocity and promote prevention should consider the potential for new laws and institutions to "lock in" *significant* trade disruption costs when a regime chooses atrocity. Our research suggests that, as things stand today, trade disruption costs from conducting atrocity are minimal at best.

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20 E.g. some members of the International Association of Genocide Scholars (2020), CBS News (2018), Yanghee Lee, the Special UN Rapporteur on human rights in Myanmar (CNN 2018), Gambia (Human Rights Watch 2020), the International Court of Justice (ICJ) (Human Rights Watch 2020), and the Global Centre for the Responsibilty to Protect (2020).

21 The data, measured in constant 2010 US dollars, show that Myanmar's real trade increased from $17.2 billion in 2010 to $37.3 billion in 2018.

22 Countries imposing sanctions on Myanmar over the treatment of the Rohingya people since 2018 include Australia, Canada, USA, and EU countries (Financial Times 2018; States News Service 2018a, 2018b; Times 2018). Meanwhile, China and Myanmar have recently signed various agreements to promote, among other things, their bilateral trade (Aung and McPherson 2020; States News Service 2020). PricewaterhouseCoopers (2020) touts Myanmar as a business opportunity and provides a business guide for corporations seeking to do business there.

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