UNIVERSITY OF CALIFORNIA,
IRVINE

Essays on terrorism, trade, and religious hostilities: Three causal analyses

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics-Public Choice

by

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2021

DEDICATION

To

my parents and grandparents

in recognition of their love and encouragement

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ACKNOWLEDGEMENTS

I would first like to express the deepest appreciation to my committee chair, Professor Michelle Garfinkel, who is brilliant, generous, and a real treasure of the academic profession. Without her wholehearted support at the beginning and scrupulous guidance throughout, this manuscript would not have been possible, and the trajectory of my life would have been very different.

I would also like to thank my committee members, Distinguished Professor Jan Brueckner and Professor Priyaranjan Jha, along with Professor Jose Antonio Rodriguez-Lopez, whose support and constructive feedback pushed the writing and empirics of each chapter to greater heights.

In addition, a thank you to Dr. Prosper Raynold of Miami University, who introduced me to the economic study of religion and terrorism, and whose enthusiasm for trying to bring seemingly nonempirical theories to life through the data had lasting effect.

Financial support was provided by the University of California at Irvine's School of Social Sciences and Department of Economics.

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--with Prosper Raynold and Jing Li. *Applied Economics* 50, 5366-5380.

ABSTRACT OF THE DISSERTATION

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Doctor of Philosophy in Economics-Public Choice

University of California, Irvine, 2021

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New directions in terrorism have been enabled and invigorated by the extraordinary 9/11 terrorist attacks. This surge of interest in terrorism has led to the investigation of some overlooked, but potentially important empirical questions involving the causes and consequences of terrorism. However, on certain empirical questions, the causal analyses are fairly superficial, leaving ample room for more thorough investigations into the causal claims embedded in the terrorism literature. For example, previous empirical studies have separately studied the causal relationship running from both terrorism to trade and trade to terrorism, but neither of these literatures seriously grapple with issues of reverse causality. Broadly speaking, I contribute to both empirical literatures by using two distinct instrumental variables approaches. For this reason, the first two chapters speak not only to whether the causal relationship between terrorism and trade is bidirectional but also to the sign of each causal effect. Furthermore, in the third chapter, I make use of a third instrumental variables approach, which reflects that of the second chapter, to examine the understudied empirical link running from trade to religious hostilities.

Regarding the results of the first chapter, the two-stage least squares estimate of terrorism is negative, statistically significant, and substantially stronger in absolute value compared to that

of OLS. Hence, I conclude that OLS estimates systematically downplay the negative impact of terrorism on trade. To determine if this upward bias on the terrorism estimate can be explained by trade's positive effect on terrorism, I study the reverse causal process in the second chapter. Naturally, the two-stage least squares estimate of the effects of trade on future terrorism are significant, positive, and stronger than OLS. Now, in part because the specific channel through which trade increases terrorism is not obvious, the third chapter explores one possible channel, namely religious hostilities. The two-stage least squares estimate of the effects of trade on future religious hostilities is significant and positive, while OLS exhibits a significantly negative estimate. Therefore, the third chapter provides evidence that trade does foster religious hostilities, which potentially reflects a transmission channel through which trade has an amplifying effect on terrorism.

CHAPTER 1: TERRORISM-TO-TRADE

1. Introduction

This chapter examines the empirical relationship between terrorism and international trade. From the Iranian Hostage Crisis, to the 9/11 attacks and more recently the Charlie Hebdo shooting, the issue of terrorism has plowed into the limelight of academic research without warning and without qualification. Terrorist events have huge consequences not only by way of taking lives or through its destruction, but also by way of the subsequent shockwave that reverberates throughout the global economy. In this regard, an important byproduct of terrorism is a reduction in international trade.

Terrorism increases the cost of doing business, which is particularly evident when conducting business on an international scale. Terrorism creates additional business expenses because it produces greater uncertainty (e.g. higher insurance and wage premiums) and makes the task of transporting items far more onerous (e.g. increased security measures). Both of these consequences discourage trade between countries. For example, because of the increased risk of transporting valuable items in the wake of the 9/11 attack, freight shipping companies faced higher insurance premiums on their liabilities for worker injuries or deaths as well as on the destruction of their land or property. Additionally, these same freight shipping companies also faced stricter security requirements, which potentially limits the amount they can ship on a single trip in addition to having to invest more resources in monitoring operations or verifying that their shipments are untampered. These freight shipping companies, in turn, generally pass these

additional costs onto their customer base (e.g. exporting and importing businesses), which reduces the volume of trade between countries. In light of the foregoing, terrorism can be reduced down to an ad valorem trade cost variable, suggesting that, in theory, terrorism will have a disruptive impact on international trade.

To date, only a handful of studies have attempted to empirically examine the impact of terrorism on trade using a gravity model (Nitsch and Schumaker, 2004; Blomberg and Hess, 2006; Mirza and Verdier, 2014; Glick and Rose, 2015; and Bandyopadhyay, Sandler, and Younas, 2018). Beyond the standard OLS approach to estimating the effects of terrorism on trade, the existing literature has made great strides towards addressing the problem of omitted variables. The most common approach involves applying dynamic fixed-effects regressions to account for year-specific (e.g. Nitsch and Schumaker, 2004) in addition to country-specific (e.g. Blomberg and Hess, 2006) or country-pair-specific (e.g. Bandyopadhyay, Sandler, and Younas, 2018) factors that are not directly observable. Moreover, lagged values for terrorism as an additional regressor (e.g. Tavares, 2004) in addition to longer sample periods and a relatively small number of cross-sectional units (e.g. Gaibulloev, Sandler, and Sul; 2014) are also used to help address the issue of Nickell bias in one-way fixed effects models.[1] While these approaches help correct for omitted variables, an instrumental variables approach is noticeably absent from the literature, despite its capacity for addressing reverse causality (i.e. the possibility that the trade and terrorism variables are jointly determined). What's more, a reverse causality between trade and terrorism has been acknowledged as a problem within the existing literature.[2] But

[1] Nickell bias is particularly worrisome in the context of using a panel dataset with many observations and few years in a one-way fixed effects model. If Nickell bias is present, there is a correlation between a regressor and the error term, where this bias arises from the demeaning process (i.e. subtracting the individual's mean value of the dependent variable and each regressor from the respective variable) (Nickell, 1981).
[2] See Bandyopadhyay et al. (2018)

given that the successful identification of good instrumental variables is a notoriously difficult task and, even if good instruments appear to have been found, such claims are typically met with unwavering skepticism, it makes sense that this lacuna persists.

This chapter, to the best of my knowledge, is the first within the terrorism and trade literature to take a more optimistic attitude regarding the identification of proper instruments for terrorism. The selection process began by recognizing that it is easiest to first identify the instruments that are likely to satisfy the relevance condition (i.e. highly correlated with terrorism). For instance, the existing literature has identified relevant instruments for terrorism by the mere fact that they have found several measures to be strong and significant predictors of terrorism, including political, geographic, and demographic variables (Krueger and Maleckova, 2003; Abadie, 2006; Kurrild-Klitgaard, Justesen, and Klemmensten, 2006; Krueger and Laitin, 2008; Dreher and Gassebner, 2008; and Basuchoudhary and Shughart, 2010). Morales et al. (2018) take a slightly different approach. They draw from the theoretical literature on terrorism which, claims that the most lethal terrorist groups (namely, violent extremist religious sects (VERSs)) possess an extraordinary ability to 1) screen out free-riders by imposing strict behavioral requirements on their members that, if followed, put members at odds with the rest of society (i.e. sacrifice and stigma) and 2) generate collective production (i.e. social service provision). Both sacrifice and stigma (S&S) and social service provision (SSP) may be thought of as *commitment-enhancement devices* which are deployed by VERSs to help them build a coalition of highly committed members. With these ideas in mind, Morales et al. (2018) identify several measures that are likely to be reliable indicators of variation in the employment of S&S and SSP by VERSs. In particular, the measures claimed to fit these criteria for variation in the employment of sacrifice and stigma (S&S) include government restrictions on religion, social

hostilities towards religion, and tensions between groups. In estimating the effects of each proxy on terrorism in 2010, all of the S&S proxies and just one of the SSP proxies are found to be highly significant predictors of terrorism and the effects of each proxy are found to be stronger than that of geography and political rights.

In light of the foregoing, I take the results in the terrorism literature seriously and use them as a springboard to exploit a potentially fruitful opportunity. Namely, I attempt to draw conclusions regarding which measures are likely to be both relevant and exogenous instruments for terrorism. So, considering that, in Morales et al (2018), the political and social service provision proxies were not uniformly statistically significant (when the sacrifice and stigma proxies succeeded in this regard), we rule these measures out as relevant instruments for terrorism. Plus, geography and demography variables more directly influence the cost of trading between countries, meaning that I can rule these measures out as being exogenous variables in a model of trade. For example, geography variables determine how costly the task of transporting goods/services across countries will be (e.g. bilateral distance or whether two countries share a border) and demography variables determine informational costs (e.g. cultural factors that determine how easy it is for firms to become familiar with the business environment of a different country).

Yet measures used to proxy for the employment of sacrifice and stigma reasonably only affect trade through its effect on terrorism. While one can tell a story regarding how further tensions between groups and/or stricter religious requirements in (say) Saudi Arabia might deter Western businesses or Western governments from trading with Saudi Arabia (perhaps due to Western consumers threatening to boycott firms who trade with countries whose human rights record is worsening), this effect is reasonably negligible. After all, firms wish to maximize profit

and insofar as their expected profits through trade with Saudi Arabia exceeds their expected loss from a Western consumer boycott (which is arguably the most plausible scenario), then trading behavior between a Western country and Saudi Arabia would be unaffected by further tensions between groups and/or stricter religious restrictions in Saudi Arabia.

In contrast, the primary channel through which tensions between groups and/or religious restrictions might have an impact on trade is through the terrorism channel. For instance, further tensions between groups and stricter restrictions on religion (brought on by either the government or society) both create a more intolerant majority, which increases the cost of remaining a member in a violent extremist religious sect (VERS), thereby improving the productive capacity of VERSs. To elaborate, the effectiveness of a given level of sacrifice and stigma (S&S) employment is improved in response to harsher restrictions on religion in the sense that this particular *commitment-enhancement technology* performs better in terms of ensuring that each and every member, among their current pool of members, are indeed committed to the cause (as the halfhearted members leave the organization due to it being prohibitively costly for them to continue to adhere to the stricter behavioral requirement). And given that radical religious groups make for potent terrorist organizations and that terrorist operations rely heavily on trust (and, implicitly, forming a coalition of highly committed members), the waging of a terrorist attack is more likely to be successful in an environment where harsh restrictions on religion prevail. Consequently, to the extent that variation in either tensions between groups or restrictions on religion impacts trade, it is plausible that this effect is primarily generated through the terrorism channel, meaning that tensions between groups and religious restrictions can both be correctly excluded from a gravity model of trade.

Beyond claiming that certain measures are valid instruments for terrorism, the construction of my bilateral panel dataset of 161 countries between 2007 and 2016 requires further elaboration. Notably, an observation is defined as two distinct countries (i.e. a trading-pair or a country-pair), where I manually combine four naturally dyadic datasets: three yearly bilateral datasets (The Direction of Trade Statistics' bilateral trade data; Mario Larch's Regional Trade Agreements Dataset; and de Sousa's Currency Union dataset) and a time-invariant bilateral dataset (The Center for Prospective Studies and International Information's dyadic geography trade data, which includes distance and colonial history dummies). And considering that each variable describes a relationship between two distinct countries and that my economic and terrorism variables along with my instrumental variable datapoints are all specific to a single country and a single year, I convert these measures into bilateral terms by taking the product of the respective yearly country-specific measure for the two countries in a trading-pair. For instance, for the France/Germany trading-pair in 2007, I identify the Real GDP value for both France and Germany in 2007, and simply multiply these two values together. The resulting number is deemed the "product" of Real GDP for the France/Germany trading-pair in 2007. Essentially, the process of calculating the "product" allows me to convert yearly country-specific data into bilateral data, which not only describes the environment of the home country but also that of their trading partner. This process is applied to all of the yearly country-specific measures in my dataset and subsequently merged with the other two naturally dyadic datasets.

So, with my dyadic panel dataset and my set of instrumental variables in hand, I set out to consider not only how each set of instrumental variables performs in terms of passing both the relevance and exogeneity tests but also how the terrorism estimates under IV 2sls compare to that of OLS. Specifically, in the year/trading-pair fixed effects model (where I include an

exhaustive set of year and trading-pair dummies), both sets of instrumental variable candidates combine to strike a robust balance of being correlated with the endogenous regressor (terrorism) and being uncorrelated with the error term (the factors that are unaccounted for in a gravity model but still explain variations in bilateral trade), where this result is robust to first-differences. Additionally, I find that the IV 2sls terrorism estimate, in the year/trading-pair fixed effects model (in which the omitted variables problem is thought to be suitably addressed), is statistically significant at the 5% level and about seventy times stronger than the OLS terrorism estimate. These results not only suggest that a large endogeneity bias on the OLS estimate for terrorism exists but also that this bias is driven by a reverse causality between trade and terrorism. Therefore, an OLS investigation of the effects of terrorism on trade leads researchers to substantially understate the strength of this negative relationship.

These results are especially revelatory because, at first blush, the direction of the bias would seem to go in the opposite direction. To begin, consider that the year/trading-pair fixed effects model reasonably solves the problem of omitted variables, where the most plausible alternative for why there remains an endogeneity bias on the OLS terrorism estimate (save a story of measurement error on the terrorism variable) centers on the existence of a reverse causality between my dependent variable (trade) and my endogenous variable (terrorism). On the one hand, an increase in trade-induced economic activity will increase the opportunity cost of participating in terrorist activities, which leads *ceteris paribus* to less terrorism. In this regard, trade would have a negative effect on terrorism, resulting in the OLS terrorism estimate *overstating* the strength of the negative relationship between terrorism and trade. On the other hand, while international trade creates efficiency gains, it also creates losers within the domestic economy, meaning that international trade can be viewed as a threat to the livelihood of many

individuals within a country. As such, a growing international trade presence in the world (which can reasonably be viewed as being paid for through the harming of domestic firms and workers) can lead to pushback by those who stand to lose from this outcome, which conceivably increases a country's preference for nationalism. Continuing on this point while also motiving the validity of my instrumental variables, strong nationalistic sentiments can cultivate a tribal mentality within countries, which may leave marginalized communities feeling more stigmatized than before. Such a scenario, as the existing theoretical literature on commitment and terrorism suggests, would actually *increase* terrorism. After all, terrorist groups, who demand that their members sacrifice and subsequently feel the stigma associated with the sacrifice, improve their productive capabilities when marginalized communities are further stigmatized because only the most committed members would continue to have an incentive to sacrifice for the group (Iannaccone and Berman, 2006; Berman and Laitin, 2008; and Raynold, 2014, 2017 & 2018). Basically, the task of forming a coalition of highly committed members becomes much easier in the wake of *ceteris paribus* a religious sacrifice being met with greater stigmatization, making it more likely that citizens within a country will be more heavily impacted by terrorism. In this regard, trade (and, implicitly, nationalism) would have a positive impact on terrorism, resulting in the OLS terrorism estimate *understating* the true strength of the negative relationship between terrorism and trade. What's more, the OLS terrorism estimate might even report a positive sign if the bias term is sufficiently large. In light of the preceding discussion, the direction and magnitude of the bias on the OLS terrorism estimate is not immediately obvious, meaning that, without my results, researchers are permitted to choose the story that best fits their narrative.

The primary contribution of this chapter, then, is that my empirical results make the process of selecting among these competing stories much less arbitrary. Again, the proceeding

discussion is based on the aforementioned premise: the remaining endogeneity bias on the OLS terrorism estimate in my model of trade is a direct result of terrorism and trade being jointly determined. But conditional on this premise being true and bearing in mind the empirical results of this chapter and the stories offered in the previous paragraph, I can draw some nontrivial conclusions that are relevant for public policy. Namely, the fact that the OLS terrorism estimate consistently *understates* the true terrorism estimate suggests that a growing international trade presence (and, implicitly, a growing preference for nationalism) may unwittingly lay the groundwork for terrorist groups to thrive. Hence, this chapter provides new evidence that sheds light on the possibility that international trade (plausibly through its positive impact on nationalism) has a positive effect on terrorism, which motivates the need to reconsider standard approaches to counterterrorism policy in the future.

I review the literature, describe my data and empirical methodology, as well as discuss the fixed-effects and first-difference results in what follows. I end by offering some concluding remarks which involves a discussion of the policy implications and some potential avenues for future research.

2. Related Literature

There are a variety of reasons for why the frequency and lethality of a terrorist attack (i.e. the impact of terrorism on a venue country) might influence bilateral trade flows. The prevailing sentiment of the existing literature is that a larger impact of terrorism increases the cost of doing business, suggesting that terrorism is merely an ad valorem trade cost variable. Much of the theory behind this idea examines the relationship between the negative impact of terrorism on foreign investment. Specifically, foreign direct investment is shown to decline in response to

terrorist attacks, a result that holds for both developed and developing countries (Abadie and Gardeazabal, 2008; Bandyopadhyay, Sandler, and Younas, 2014; Enders, Sachsida, and Sandler, 2006; and Enders and Sandler, 1996). Terrorism is thought to be costly because it raises the requirement of security standards for cross-border transactions, it devalues trustworthiness in international business relations, and it can physically destroy transport infrastructure or even the goods themselves. For example, in the wake of the Charlie Hebdo attack, exporting firms to France would need to improve their compensation package offered to employees due to the increased risk they take on by participating in the delivery of the firms' product or service to French consumers. This increases the cost of doing business for exporting firms to France, resulting not only in less imports for France but also less exports for France's trading partner. Other reasons for why terrorism might influence bilateral trade flows is that a larger impact of terrorism leads to a transfer of resources away from more productive investments or activities. Continuing with the implications of the Charlie Hebdo attack, private decision makers within France might shift their allocation of resources towards government purchases to help prevent future terrorist incidents. In theory, this would lead to a reduction in the productive capacity of the French economy, resulting in less exports for France (or less imports for France's trading partner). All these examples help explain why, in practice, terrorism leads to a reduction in trade volume.

Bandyopadhyay and Sandler (2014) adapt standard neoclassical models of trade (e.g. two-good, two-factor, small open economy) to identify the conditions under which terrorism will reduce or (perhaps even) raise trade. They find that if terrorism impacts a country through the physical destruction of the supply of an intensive production factor within the import industry, then production of the exportable good will rise, implying that terrorism can increase trade.

Although this result clearly goes against conventional wisdom, it is important to recognize that the explicit goal of many terrorist organizations is to hurt the target countries' economies (Enders and Sandler, 2012). Hence, if the specific objective of a rational terrorist organization is to disrupt a country's economy through the trade channel, then the possibility of increasing trade through terrorism would be accounted for when attacking a target country. Under these circumstances, terrorist organizations would avoid physically destroying the intensive factor of production within an import industry whenever possible to maximize the negative impact that the attack will have on a target country's economy. In any case, the extant theoretical research provides information regarding the impact of terrorism on trade and identifies the alternative scenarios through which the negative impact of terrorism on trade is likely to be small or even reversed.

Beyond providing theoretical insights regarding the actual impact of terrorism on international trade, the empirical literature indicates that there are two avenues through which I can estimate the effects of terrorism on international trade. The first avenue involves examining how variations in terrorism affect the consumer allocation of tourism relative to other goods (e.g. Enders et al, 1992; Drakos and Kutan, 2003; Arana and Leon, 2008; Llorca-Vivero, 2008; Yaya, 2009; Neumayer and Plumber, 2016; Bassil et al., 2017) or, more narrowly, international air travel (Lee, 2005; and Mitra et al., 2018). As an example, Enders et al. (1992) developed a forecasting technique to investigate the effect of terrorism on tourism using a quarterly dataset between 1970 and 1988 consisting of only European countries and found that terrorists were successful in deterring tourism. Moreover, they show a generalization effect wherein an act of terror in one nation results in a decline in the demand for tourism in neighboring nations. Similarly, Mitra et al. (2018) develop an adapted structural gravity model to examine the effects

of terrorism on air passenger traffic between nations. They find that terrorism adversely impacts bilateral air passenger travel both by reducing national output and especially by increasing psychological distress.

The second avenue involves examining the effects of terrorism on international trade using a gravity model, which is the strategy employed for this chapter. Specifically, the econometric methodology used here is most closely related to Nitsch and Schumaker (2004). They analyze trade data between more than 200 countries from 1960 to 1993 and use several measures of terrorism and large-scale violence to separately examine the impacts of both terrorism and warfare on trade, finding strong evidence that, for given pairs of countries, acts of terror reduce the volume of trade. Along this same line, Blomberg and Hess (2006) use a more wide-ranging panel data set from 1968 to 1999 to examine the empirical effects of *all* types of conflict on trade flows, choosing to also explore this issue through an augmented gravity model. They find that the joint presence of terrorism together with internal and external conflict has a strong effect on trade. In fact, the effect of violence on trade is reported to be even stronger than the presence of bilateral trade-promoting vehicles such as the incorporation of generalized systems of preference (e.g. giving a discount in terms of lower-than-normal tariffs to a select group of developing countries). Using similar methods, Qureshi (2013) finds that both international and intrastate warfare in neighboring states have a statistically significant effect on bilateral trade and that the impact of regional conflicts is persistent and increasing in duration.

Transitioning to a discussion of terrorism data used for this chapter, the Institute of Economics and Peace's Global Terrorism Index defines terrorism as "the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation" (pg. 6). Given that an event fits this

description (i.e. the event is considered a terrorist attack), it can be categorized into two distinct classes: domestic or transnational terrorism. Domestic terrorism is homegrown, and home directed. If the victims and perpetrators are citizens of the venue country (the country in which the terrorist attack occurs), then the terrorist attack is categorized as domestic terrorism. On the other hand, transnational terrorism involves two or more countries, meaning that the venue country in addition to the nationalities of the victims and perpetrators are not uniform. To elaborate on this point, if the plans for a terrorist attack are hatched in one country but the attack occurs in another country, then the attack is considered transnational. Moreover, if terrorists cross a border to engage in an attack, then the terrorist event is transnational. If at least one of the victims or perpetrators are not from the venue country, then a terrorist attack is also considered transnational.

Regarding investigations into the economic consequences of terrorism, most studies focus primarily on the effects of transnational terrorism (as opposed to domestic terrorism). Gaibulloev and Sandler (2008, 2009b, 2011) help motivate this approach by estimating the separate effects of both domestic and transnational terrorism on the growth of GDP per capita. They consider only particular regions of the world (specifically, Africa, Asia, and Western Europe) and find that terrorism adversely impacts the growth of GDP per capita. Furthermore, the driver of this effect is believed to be transnational terrorism since domestic terrorism largely fails to be a statistically significant predictor of GDP growth. Additional support for this claim is found in Blomberg, Hess, and Orphanides (2004), who investigate the macroeconomic consequences of transnational terrorism and alternative forms of collective violence. They find that, on average, transnational terrorism has an economically significant negative effect on growth and that this effect is most pronounced in less-developed countries. More specific to the

research question of this chapter, Blomberg and Hess (2006), De Sousa, Mirza, and Verdier (2009), and Mirza and Verdier (2014) finds transnational terrorism, on the whole, to be a significantly negative predictor of bilateral trade. It is important to note, however, that these chapters derive their results by exclusively using measures of transnational terrorism where they entirely refrain from considering the effects of domestic terrorism on trade. An exception is Egger and Gassebner (2015) who finds transnational terrorism to have a negligible and non-robust effect on trade in the short run once general equilibrium effects are considered, suggesting that the economic costs of terrorism are miniscule.

While most work on terrorism and trade exclusively considers the effects of transnational terrorism, I take a more expansive view in this chapter by considering the effects of both transnational and domestic terrorism. The most promising economic outcome that is negatively impacted by overall terrorism is foreign direct investment. For example, Bandyopadhyay, Sandler, and Younas (2014) consider a sample of developing countries between 1984 and 2008 and use dynamic GMM estimation to unearth a negative relationship between both transnational and domestic terrorism and foreign direct investment. More specific to the research question of this chapter, Bandyopadhyay, Sandler, and Younas (2018) separate bilateral trade data according to sectors (specifically, primary commodities and manufactured goods) for 151 countries between 1995 and 2012 and examine the consequences of both transnational and domestic terrorism on trade. Consistent with Blomberg and Hess (2006), they find that overall terrorism influences trade, but that this effect is miniscule compared to that of other forms of violence such as civil war. In addition to showing that transnational terrorism is found to have a detrimental impact on trade, they show that domestic terrorism impacts trade similarly and that this effect is particularly apparent when it comes to explaining variations in manufactured imports. Given

that the empirical effects of both domestic and transnational terrorism have demonstrated predictive power in gravity models of trade and that most terrorist groups engage in both domestic and transnational attacks (as Blomberg, Gaibulloev, and Sandler (2011) and Gaibulloev and Sandler (2013) both indicate), I account for both types of terrorism in this chapter.

Other concerns are also worth mentioning when it comes to estimating the effects of terrorism on trade. A clear endogeneity bias arises from any OLS regression of trade on terrorism (e.g. potential omission of factors that may influence both terrorism and trade flows), yet an instrumental variables analysis continues to be neglected due to concerns that such an undertaking would be futile. My chapter aims to fill this void by generating instrumental variables estimates, using combinations of country-level measures thought to, at the very least, be relevant instruments for terrorism. Notably, the existing literature has identified several significant predictors of terrorism and, while economic variables are largely excluded from this list, several political, geographic, and demographic variables are shown to be strong and highly significant predictors of terrorism (Krueger and Maleckova, 2003; Abadie, 2006; Kurrild-Klitgaard, Justesen, and Klemmensten, 2006; Krueger and Laitin, 2008; Dreher and Gassebner, 2008; Basuchoudhary and Shughart, 2010; Morales et al., 2018).

For the purpose of identifying relevant instruments that are also correctly excluded from a gravity model of trade (i.e. valid), Morales et al. (2018) is a particularly notable article. They themselves draw from the theoretical literature on terrorism, which claims that radical religious groups (who are proven to be the most lethal terrorist organizations) are exceptionally strong 1) at screening out halfhearted members (i.e. Sacrifice and Stigma (S&S)) and 2) in the production of collective goods and services (i.e. Social Service Provision (SSP)) (e.g. Berman and Laitin, 2006). These characteristics are essential in controlling defection which allows radical religious

groups to become remarkably effective in the deployment of lethal terrorists. Hence, these characteristics may be thought of as "commitment-enhancement devices" which are inputs in the production of terrorism for radical religious groups. With these ideas in mind, Morales et al. (2018) identify several measures that are likely to be reliable indicators of variation in the employment of sacrifice and stigma (S&S) and social service provision (SSP) (three proxies for S&S and three proxies for SSP). While I defer to Morales et al. (2018) for a thorough justification of the proxies, their rationale requires further elaboration. For instance, they recognize the difficulty if not impossibility of obtaining direct measures for the extent to which radical religious groups deploy S&S and SSP and, as a result, incorporate a reasonable workaround. First off, S&S and SSP are complementary inputs used by radical religious groups to assemble coalitions of highly committed operatives/members (i.e. commitment is output; the two inputs are S&S and SSP). Additionally, exogenous variation in prevailing conditions can shift the marginal product schedules and in so doing cause radical religious groups to alter their employment of S&S and SSP in the same direction (if I assume fixed marginal costs and diminishing marginal productivity). As such, if observable shifters of the marginal product schedules can be found, such measures are likely to be reliable indicators of variation in the employment of S&S and SSP by radical religious groups. This line of logic is used to identify proxies for the use of both S&S and SSP, where all of the S&S proxies (and only one of the SSP proxies) are found to be significant predictors of terrorism and stronger than that of political, geography, and demographic variables.

In this chapter, I begin with the strikingly simple premise that there might exist observable (and untapped) measures that are both 1) significant predictors of terrorism and 2) correctly excluded from gravity models of trade. I then use these conditions as guidance for the

kinds of measures that are likely to be suitable instruments for terrorism and sift through the extant terrorism and trade literature to see if there exist measures which are likely to satisfy both conditions. In the end, I exploit the results put forth by Morales et al. (2018) to identify three measures (tensions between groups, government restrictions on religion, and social hostilities towards religion) that I anticipate being good instrumental variables for terrorism in a gravity model of trade. Furthermore, I evaluate this claim by examining the impact of terrorism on trade through an instrumental variables analysis. As such, this chapter is the first attempt to address the endogeneity bias on the OLS terrorism estimate in this way.

3. Data

Table 1.1 provides definitions of the variables in the dataset along with descriptive statistics. I use a panel dataset of 161 countries between 2007 and 2016, where each variable describes a relationship between two distinct countries (i.e. a trading pair). Hence, even if a variable is specific to an individual country (e.g. terrorism and Real GDP), I convert them into bilateral measures by taking the product of the respective country-specific measure for the two countries in a trading pair. Additionally, each of the 161 countries generates no more than 160 observations in any given year, meaning that there are (at most) 25,760 total trading pairs in the dataset (or, at most, 25,760 observations per year). The fact that I consider a ten-year period implies that no more than 257,600 total observations are expected. The extent to which I can expect to generate 257,600 observations, of course, depends on there not being any missing datapoints for the 25,760 trading pairs between 2007 and 2016, which unfortunately is not the case. Namely, there are tens of thousands country-pair trade datapoints that are missing and, since a missing valued datapoint does not necessarily imply a zero-valued datapoint, I choose to

only study the country-pair datapoints that are non-missing rather than manually convert a missing-valued datapoint into a zero-valued datapoint. In any case, to preserve as many observations as possible, when I take the natural logarithm of variables that are both non-missing

Table 1.1 – Descriptive statistics

Variable	Definition	Obs	Mean	S.D.	Min	Max
Trade Variable:						
Real bilateral Imports	DOTS' (Constant $US 2010) value of merchandise imports (Cost, Insurance, and Freight) with a distinct trading partner.	199,053	7.29e08	6.28e09	.91	4.43e11
Terrorism Variable:						
Impact of Terrorism (Product)	Institute of Economics and Peace's Global Terrorism Index	257,280	4.3382	9.1307	0	94
Instrumental Variables for Terrorism:						
Tensions between groups (Product)	The Fragile States Index's Group Grievances indicator	248,040	37.505	18.176	1	100
Governmental restrictions on Religion (Product)	The Pew Research Center's Government Restrictions Index	257,600	10.746	11.507	0	78.32
Societal restrictions on Religion (Product)	The Pew Research Center's Social Hostilities Index	257,600	7.312	11.064	0	96
Economic Variable:						
Real GDP (Product)	The World Bank's (constant $US 2010) GDP	246,296	1.63e23	1.77e24	5.78e17	1.52e26
Real GDP per capita (Product)	The World Bank's (constant $US 2010) GDP divided by the WB's Population, total measure	245,784	1.54e08	4.28e08	36296.3	1.06e10
Time-varying gravity variables:						
Regional Trade Agreement	Whether two countries are in a customs union, an economic integration agreement, a free trade agreement, or a partial scope agreement in year t	241,800	.22205	.41562	0	1
Common Currency	Whether two countries share a currency in year t	254,400	.01738	.13069	0	1
Time-invariant gravity variables:						
Bilateral distance	The distance (in kilometers) between each respective country's most populous city for a given trading pair	254,380	7136.25	4172.91	10.479	19951.2
Common official language	Whether two countries share an official language	254,380	.12045	.32549	0	1
Common border	Whether two countries are contiguous	254,380	.02312	.15027	0	1
Common colonizer	Whether two countries have had a common colonizer after 1945	254,380	.07434	.26555	0	1
Same Nation	Whether two countries were/are the same country	254,380	.00971	.09806	0	1
Colonial relationship	Whether two countries have ever had a colonial link	254,380	.01258	.11145	0	1

Note: The terrorism, economic, and instrumental variables for terrorism were calculated on an annual basis between 2007 and 2016 and are particular to just one country. For the purposes of translating all of these measures into "bilateral" terms, the product of two countries' yearly datapoints (from a given trading pair) are used and are reported in this table. In theory, each of the 161 countries under consideration should have 160 datapoints per year for each variable in the dataset, generating 25,760 observations per year for each variable (or 257,600 total observations over a 10-year period for each variable). "DOTS" refers to the Direction of Trade Statistics, which disaggregates the value of merchandise imports according to a country's trading partner. The Regional trade agreement variable is derived from "Mario Larch's Regional Trade Agreements Database from Egger and Larch (2008)," the common currency variable is derived from de Sousa's (2012) dataset on currency unions, and the time-invariant gravity variables are derived from Mayer and Zignago (2011). Also, because the common currency variable was measured each year between 2007 and 2015 (but not in 2016), I assume that the common currency variable did not change between 2015 and 2016.

and occasionally zero-valued (e.g. terrorism and two of the measures I use to instrument for terrorism sometimes yield non-missing, zero-valued datapoints), I add one to each of these variables before taking the natural logarithm. If I did not do this, many non-missing observations would be dropped since the natural logarithm of zero is undefined.

The dependent variable (real bilateral imports) is obtained from the International Monetary Fund's Direction of Trade Statistics (DOTS), which disaggregates the value of merchandise imports according to a country's trading partner. I use imports instead of exports since, for the considered 25,760 trading pairs, there are thousands of more import datapoints between 2007 and 2016. I also use imports instead of volume (i.e. the sum value of imports and exports) to avoid the issue of duplicate observations within my dataset (e.g. the France/United States and United States/France trading pairs could not be considered distinct observations if trade volume is my dependent variable). Regarding the explanatory variables, each of the 25,760 trading pairs are assigned a bilateral distance value and five bilateral dummy values (including a dummy variable that results in unity whenever a trading pair shares a border). These variables are derived from the Center for Prospective Studies and International Information (CEPII), which I reasonably assume did not change between 2007 and 2016. The use of bilateral distance and the five included bilateral dummy variables are identical to the included dummies in Nitsch and Schumaker (2004) and are typically found in gravity equations. Moreover, I also include two time-varying bilateral dummy variables, including Mario Larch's regional trade variable from Egger and Larch (2008) and the common currency variable from de Sousa (2012). Notably, the common currency variable is only available, for each year, between 2007 and 2015. Consequently, I reasonably assume that this variable did not change between 2015 and 2016 by carrying the 2015 datapoints forward, into 2016, which allows me to control for currency unions

without losing the 2016 datapoints. The additional inclusion of these time-varying dummies as explanatory variables for trade is identical to Bandyopadhyay, Sandler, and Younas (2018) and is more broadly consistent with the empirical trade literature.

The economic, terrorism, and instrumental variables were calculated on an annual basis between 2007 and 2016 but are particular to just one country. For the purposes of translating all of these measures into bilateral terms, the product of two countries' yearly datapoints (from a given trading-pair) are used. This strategy is consistent with Nitsch and Schumaker (2004), who convert all of their non-bilateral variables (both their economic and terrorism variables) into bilateral terms. In essence, I am assuming that the economic environment along with the impact of terrorism for one country can have cross-border effects that are relevant for explaining both their trade partner's willingness to import goods from them as well as their own willingness to import goods from other countries. For instance, a terrorist attack in one country might reduce the extent to which one of their trading partners wishes to import merchandise from them, perhaps because they deem it necessary to be more thorough in verifying that the imported goods from their trading partner are untampered. A terrorist attack in the destination country might also reduce the extent to which the source country wishes to export goods to that country, perhaps because they want to avoid (unwittingly) enriching terrorist organizations who might launch an attack against them.

Contrary to the extant literature (the exception being Morales et al. (2018)), I make use of an overlooked annual proxy for a country's terrorism impact. Because I aim to consider the most recent 10-year period, I am permitted to make use of the Institute of Economics and Peace's Global Terrorism Index (IT), which was first established in 2001 and continues to be calculated in every subsequent year. Specifically, I take the yearly IT score of two countries and multiply

them together (which constitutes the yearly IT "product" score for a given trading pair). In any case, the Global Terrorism Index measures the extent to which (up to) 161 countries are impacted by both transnational and domestic terrorism between 2001 and 2017. The factors within the IT score include: the number of incidents, fatalities, injuries, and property damage resulting from terrorism along with a five-year weighted average to account for the lingering psychological damage caused by terrorism. The IT score, for each country, ranges from 0-10 with higher scores reflecting a higher impact of terrorism.

Furthermore, by using the Institute of Economics and Peace's Global Terrorism Index (IT) as my proxy for terrorism, I effectively consider the effects of overall terrorism. The underlying rationale for this approach is to highlight the similarities between both domestic and transnational terrorism. Although the policy implications are reasonably different if (say) transnational terrorism creates more destruction within a country compared to domestic terrorism, the decision to focus on this aspect of the problem may steer the policy discussion into a direction that overstates the importance of mitigating transnational terrorism at the expense of developing ideas on how to mitigate terrorism more generally. To elaborate further, consider a country wherein transnational terrorism is deemed the greater threat. This country may focus their counterterrorism efforts on rooting out foreign terrorist groups (as opposed to domestic terrorist groups) who appear to be both able and willing to mount an attack against them. Additionally, it might be reasonable to also shift the allocation of resources more towards efforts aimed at keeping potential foreign terrorists from entering the country, which might come in the form of increased border security or tougher restrictions on refugee status, foreign travelers, or immigration. But, even if transnational terrorism is deemed the greater threat, I maintain that a single-minded focus on thwarting this specific form of terrorism is suboptimal. For instance, the

Charlie Hebdo attacks, a mass shooting perpetrated by French citizens who were born to Algerian immigrants at the offices of a satirical magazine in France, is defined as transnational terrorism because there is compelling evidence that the plan was hatched outside of France (specifically, in Yemen or Syria). In contrast, the 2013 Boston Marathon bombing, which was perpetrated by two American citizens, is defined as domestic terrorism. Even though the perpetrators of the Boston Marathon bombing claimed to have been motivated by radical Islamist beliefs, there was no evidence that the plan originated outside of the United States, resulting in the perpetrators being considered lone-wolf actors.

In short, despite being categorized as a different form of terrorism, similarities between these attacks exist and have important policy implications. Not only were both terrorist attacks carried out by citizens of the venue country, but these attacks also appear to have been prompted by terrorist groups who espoused similar ideologies (specifically, radical Islamists). By invoking a broad measure of terrorism which includes (and makes no distinction between) domestic and transnational terrorism, I argue that connections can be made between each act of terror, regardless of whether the attacks under consideration are domestic or international, and that the identification of these connections is useful to consider when developing effective counterterrorism policy. As such, the Global Terrorism Index is tailor-made for the purposes of this chapter and is consequently the exclusive variable used to capture the impact of terrorism.

Beyond selecting my terrorism measure, my hypothesis that the OLS terrorism estimate understates the true estimate for terrorism necessitates that I also select instrumental variables for terrorism. To accomplish this task, I draw from Morales et al. (2018), which respectively identify proxies for commitment-enhancement devices employed by radical religious groups (specifically, for both Sacrifice and Stigma (S&S) and Social Service Provision). Since I

contend that measures proxying for the use of sacrifice and stigma (S&S) by radical religious groups will end up being valid instruments for terrorism in a model of trade, I make use of all the yearly S&S proxies in Morales et al. (2018). The first S&S proxy I use is the Fragile State Index's Group Grievances indicator, where the product of this country-specific measure for the two countries in a trading pair are taken and henceforth referred to as my "tensions between groups" instrumental variable. Similarly, the second and third S&S proxies I use as instruments for terrorism are the Pew Research Center's Government Restrictions Index and Social Hostilities Index, where the product of each of these country-specific measures for the two countries in a trading pair are taken and henceforth referred to as my "*governmental* restrictions on religion" and "*societal* restriction on religion" instrumental variables respectively. These three variables, used in concert, describe my set of instrumental variables.

4. Empirical Methodology

Considering that the standard approach for empirically examining international trade is a gravity model of trade, my empirical strategy broadly follows that of the existing gravity and terrorism literature. In what follows, I precisely discuss how I define trade costs as well as how I control for the unobserved characteristics that are relevant for explaining trade flows. What's more, throughout this section, I build the models that I will later estimate through ordinary-least-squares and in the second stage of my two-stage-least-squares regression. The section ends by describing the first stages of the two-stage-least squares model.

4.1 Defining trade cost (*C*)

An important component of any trade estimation model is how the trade cost variable is defined. In this chapter, I follow Nitsch and Schumaker (2004) by modelling bilateral terrorism

(i.e. the product of two countries impact of terrorism scores) as a transportation cost, where terrorism is thought to affect trade costs in a manner that is similar to distance. For instance, transport costs increase when the physical distance between the most populous cities of two countries increase. Likewise, trade is costlier when either an exporting or importing country suffers from larger impact of terrorism scores, as the task of transporting items becomes more onerous (e.g. increased security measures). In a similar regard, I also follow Bandyopadhyay, Sandler, and Younas (2018) in proxying for the "transaction cost" component of trade. Namely, I include a dummy for whether the two countries, in a trading pair, are both in a customs union, an economic integration agreement, a free trade agreement, or a partial scope agreement in a particular year. In addition to this "regional trade agreement" dummy variable, I also include a dummy for whether two countries, in a trading pair, use the same currency in a particular year. Both of these variables are allowed to vary over time and are inversely related to the transaction cost of trade. For instance, when two countries participate in the same trade agreement or share the same currency, both countries face a lower transaction cost in trading with the other country, which makes it more likely that trade will occur between them. Beyond transportation costs, cultural factors also increase the cost of trade, as it requires both effort and resources to become familiar with the business environment of foreign countries. I again follow Nitsch and Schumaker (2004) in proxying for the cultural component of trade costs. In this regard, I include five cultural dummies, including whether the two countries share an official language, a present/former colonizer, whether one country was a colony of the other at some point in time, whether a country pair shares a border, and whether the two countries were once part of the same nation. Basically, all five of these cultural dummies are modeled as time-invariant and reflect how easy it is to become familiar with the business environment of a trading partner. For

instance, the more cultural factors a trading-pair shares, the more similar is the business environment, and the more likely it will be for trade to occur between them. In summary, equation 4.1 describes the precise definition of trade costs in this chapter.

$$C_{i,j,t} = (d_{i,j})^{\delta_1}(IT_{i,t}*IT_{j,t})^{\delta_2}*exp(\delta_3 lang_{i,j} + \delta_4 cont_{i,j} + \delta_5 comcol_{i,j} + \ldots$$

$$\ldots + \delta_6 smctry_{i,j} + \delta_7 colony_{i,j} + \delta_8 rta_{i,j,t} + \delta_9 comcur_{i,j,t}) \quad (4.1)$$

where $d_{i,j}$ reflects the distance between the exporting country i and the importing country j; $IT_{i,t}$ and $IT_{j,t}$ are the impact of terrorism scores for the exporting country i and importing country j respectively in year t; $lang_{i,j}$, $cont_{i,j}$, $comcol_{i,j}$, $smctry_{i,j}$ and $colony_{i,j}$ are dummy variables denoting whether countries i and j share an official language, a border, a colonizer, whether countries i and j were once part of the same nation, or whether country i and j were ever in a colonial relationship; $rta_{i,j,t}$, and $comcur_{i,j,t}$ are dummy variables respectively denoting whether countries i and j participate in the same regional trade agreement in year t and whether countries i and j use the same currency in year t.

4.2 Proxying for multilateral trade-resistance

International trade is an example of an ever-changing dyadic relationship between two heterogenous agents: an exporting (a.k.a. source) country and an importing (a.k.a. destination) country. Hence, a valid empirical model of international trade must suitably account for specific time periods and specific country-pairs, as this helps proxy for unobserved characteristics that are relevant for explaining trade flows. More generally, my estimation model relates the natural logarithm of the monetary value of trade between two countries to the natural logarithm of their respective GDPs, to the natural logarithm of a composite term measuring barriers and incentives to trade between them (i.e. trade costs), and to multilateral trade-resistance (i.e. unobserved

factors that are relevant for explaining bilateral trade such as how remote a country is to world markets or how open a country is to international trade). This general specification is described in equation 4.2, which mimics the reduced-form estimation model described in Anderson and van Wincoop (2003).

$$lnM_{i,j,t} = \alpha + a_1 ln(Y_{i,t}*Y_{j,t}) + a_2 ln((Y_{i,t}*Y_{j,t})/(P_{i,t}*P_{j,t})) + a_3 lnC_{i,j,t} + (MTR_{i,j,t}) + \varepsilon_{i,j,t} \quad (4.2)$$

where $M_{i,j,t}$ is the (constant $US 2010) value of imports (cost, insurance and freight) that flow from source country *i* to destination country *j* in year *t*; $Y_{i,t}$ and $Y_{j,t}$ are the (constant $US 2010) value of GDP for source country *i* and destination country *j* respectively in year *t*; $P_{i,t}$ and $P_{j,t}$ are the population, total for source country *i* and destination country *j* respectively in year *t*; $C_{i,j,t}$ is the trade cost variable for trading pair *ij* in year *t*; $MTR_{i,j,t}$ refers to a measure of multilateral trade-resistance for trading pair *ij* in year *t*; $i \neq j$; α is the constant term; *a* is the coefficient; ε is the pure white noise error term.

Notably, some of my variables (as discussed in the previous section) are converted from country-specific measures (including Real GDP, Real GDP per capita, impact of terrorism, and all of my instrumental variables for terrorism) into bilateral terms by taking the product of these country-specific measures for the two countries in a trading pair. This strategy closely resembles that of Nitsch and Schumaker (2004), where they also examine the effect of terrorism on international trade. For their paper, they exclusively use *year* fixed effects to proxy for multilateral trade-resistance. Given the length of their time horizon (which spans from 1960 to 1993) and given that they, like me, convert two time-varying country-specific factors into bilateral terms (including Real GDP and each of their three terrorism variables), this strategy can be justified. Though time-varying (or, even, non-time-varying) exporter/importer fixed effects

might seem like the ideal way to proxy for multilateral trade-resistance, the fact that yearly country-specific factors such as exporter/importer GDP and terrorism are already included as explanatory variables renders this less necessary. In fact, the inclusion of time-varying exporter and importer fixed effects, when yearly Real GDP and impact of terrorism scores are already included in the model, will lead to severe computational issues, as there would be too many redundancies. To help motivate this claim, consider that the yearly economic and terrorism measures, which are specific to each of the two countries in a trading pair, already do much of the work when it comes to controlling for the unobserved country effects that vary over time. For instance, it is unlikely that the two countries in a trading pair can become more linked to world markets in a particular year without simultaneously experiencing increases in Real GDP. Likewise, if terrorism becomes less frequent for a country in a particular year, it is likely that this change will be accompanied by a greater openness to international trade. Basically, it is very likely that the exporting country's Real GDP level, the exporting country's impact of terrorism score, the importing country's Real GDP level, and the importing country's impact of terrorism score all combine to explain nearly all of the variation in the unobservable country-specific factors, especially when these variables are all measured for each year in the dataset. Hence, at the very least, controlling for time-varying exporter/importer fixed effects while also controlling for exporter/importer Real GDP and exporter/importer terrorism would create far-too-many collinearities among the explanatory variables to generate a reliable estimate for terrorism. This means that if I wish to compute this model, I would need to either omit most of the time-varying importer/exporter fixed effects or omit the terrorism variable. Both of these ideas, however, are suboptimal, as the former would omit important variables from the model and the latter would, untenably, prevent me from being able to evaluate the hypothesis of this paper. With these

points in mind, my first estimation model explicitly follows the methodology of Nitsch and Schumaker (2004), who proxy multilateral trade-resistance with year fixed effects:

$$lnM_{i,j,t} = \alpha + a_1 ln(Y_{i,t}*Y_{j,t}) + a_2 ln((Y_{i,t}*Y_{j,t})/(P_{i,t}*P_{j,t})) + a_3 lnC_{i,j,t} + a_4 I_t + \varepsilon_{i,j,t} \quad (4.3a)$$

where I_t is plugged into *MTR* from equation 4.2; and I_t specifically refers to the ten (total) year dummies in my panel dataset, one for each year between 2007 and 2016. Plugging 4.1 into 4.3a yields the log-linearized "year fixed effects" equation (4.3b):

$$\begin{aligned} lnM_{i,j,t} = {} & \alpha + a_1\, ln(Y_{i,t}*Y_{j,t}) + a_2 ln((Y_{i,t}*Y_{j,t})/(P_{i,t}*P_{j,t})) + a_3\delta_1 ln(d_{i,j}) + a_3\delta_2 ln(IT_{i,t}*IT_{j,t}) \\ & + a_3\delta_3 lang_{i,j} + a_3\delta_4 cont_{i,j} + a_3\delta_5 comcol_{i,j} + a_3\delta_6 smctry_{i,j} + a_3\delta_7 colony_{i,j} \\ & + a_3\delta_8 rta_{i,j,t} + a_3\delta_9 comcur_{i,j,t} + a_4 I_t + \varepsilon_{i,j,t} \end{aligned} \quad (4.3b).$$

Considering that I already control for two time-varying country-specific measures (an economic and a terrorism variable), I follow the methodology of Bandyopadhyay, Sandler, and Younas (2018) in primarily using trading-pair fixed effects to suitably account for the possibility that there remains unobservable and country-pair-specific factors within equation 4.3b that are relevant for explaining trade flows. For instance, given that consumers in the United States have a relatively high inelastic demand for both coffee (which is one of Columbia's primary exports) and wine (which is one of France's primary exports), a trading pair, that includes either Columbia or France as the exporting country and the United States as the importing country, is probably more resilient than other trading-pairs when it comes to consistently exhibiting a high level of imports over time. Hence, the extent to which a country relies on particular trading partners to obtain highly demanded goods is relevant for explaining trade, yet this concept cannot be easily measured across trading-pairs. The use of "trading-pair" fixed effects, which controls for each distinct exporter/importer combination in the dataset, helps control for these

kinds of unobserved characteristics. A byproduct of this approach, it must be said, is that it necessarily omits all of the time-invariant variables (including most of the trade cost variables such as bilateral distance and whether the two countries in a trading-pair share a border), leaving only my economic, terrorism, and time-varying gravity variables as explanatory variables. What's more, there are 161 countries in my dataset, which means that, in theory, I will end up with 161*160=25,760 "trading pair" groups (or 25,760 trading-pair dummies). And to control for the possibility that both specific trading-pairs and specific years are relevant for explaining bilateral imports, I leave the year dummies from equations 4.3a and 4.3b in this model. Consequently, I also estimate the following equation:

$$lnM_{i,j,t} = \alpha + a_1 ln(Y_{i,t}*Y_{j,t}) + a_2 ln((Y_{i,t}*Y_{j,t})/(P_{i,t}*P_{j,t})) + a_3 lnC_{i,j,t} + a_4 I_t + a_5 I_{i,j} + \varepsilon_{i,j,t} \qquad (4.4a)$$

where the sum of $I_{i,j}$ along with the previously included I_t term (from 4.3a) are plugged into multilateral trade-resistance term (*MTR*) from equation 4.2; and $I_{i,j}$ specifically refers to the 25,760 trading-pair dummies in my panel dataset, one for each trading-pair. Plugging 4.1 into 4.4a yields the year/trading-pair fixed effects equation (4.4b):

$$lnM_{i,j,t} = \alpha + a_1 ln(Y_{i,t}*Y_{j,t}) + a_2 ln((Y_{i,t}*Y_{j,t})/(P_{i,t}*P_{j,t})) + a_3\delta_2 ln(IT_{i,t}*IT_{j,t}) + a_3\delta_8 rta_{i,j,t} + a_3\delta_9 comcur_{i,j,t} + a_3 I_t + a_3 I_{i,j} + \varepsilon_{i,j,t} \qquad (4.4b).$$

In summary, equations 4.3b and 4.4b can all be estimated through ordinary least squares. Moreover, if terrorism is the only endogenous variable in these equations and I have successfully identified at least one valid instrumental variable for terrorism, then equations 4.3b and 4.4b can represent the second stage of a two-stage least squares regression. With this in mind, it is necessary to also describe the first stages of these two-stage least squares regressions. Given that I claim to have a valid set of instrumental variables, the following equation (4.5a) represents the

first stage regressions:

$$ln(IT_{i,t}*IT_{j,t}) = \pi_0 + \pi_1 Z_1 + \pi_2 ln(GRoR_{i,t}*GRoR_{j,t}) + \pi_3 ln(SRoR_{i,t}*SRoR_{j,t}) + \pi_4 ln(TbG_{i,t}*TbG_{j,t}) + v_{i,j,t} \quad (4.5a)$$

where Z_1 is all of the right-hand-side variables in either equation 4.3b or 4.4b respectively (save bilateral terrorism); $GRoR_{i,t}$ and $GRoR_{j,t}$ are the *government* restrictions on religion score for the exporting country *i* and the importing country *j* respectively in year *t*; $SRoR_{i,t}$ and $SRoR_{j,t}$ are the *societal* restrictions on religion score for the exporting country *i* and the importing country *j* respectively in year *t*; $TbG_{i,t}$ and $TbG_{j,t}$ are tensions between groups for the exporting country *i* and the importing country *j* respectively in year *t*; π_0 is the constant term; π_1, π_2, and π_3 are coefficients; v is the pure white noise error term.

5. Results and Interpretation

In this section, I use the 2007 through 2016 country-pair panel dataset, which is described in section 3, to better understand the true impact of terrorism on international trade. More specifically, I evaluate my hypothesis that an OLS investigation understates the extent to which terrorism negatively impacts trade by using the four estimation models represented by equations 4.3b and 4.4b.

While this information can be obtained by inspecting either of the first-stage regressions in 4.5a and 4.5b, it is important to emphasize that, in all of the IV 2sls generated in this section, the natural logarithm of one plus the product of two countries' impact of terrorism (*IT*) scores is being instrumented by the natural logarithm of one plus the product of two countries' *government* restrictions on religion (*GRoR*) scores, the natural logarithm of one plus the product

of two countries' *societal* restrictions on religion (*SRoR*) score, and the natural logarithm of one plus the product of two countries' tensions between groups (*TbG*) score. Using this set of measures to instrument for terrorism in each IV 2sls specification, Table 1.2 contrasts the OLS and IV 2sls results, using each of the two estimation models described in equations 4.3b and 4.4b. The first model estimated is the year fixed effects model (equation 4.3b), where the OLS specification for this model is reported in specification (1). The IV 2sls results (with *governmental* restrictions on religion, *societal* restriction on religion, and tensions between groups being used as instruments for terrorism) are reported in specification (2). The other model, the year/trading-pair fixed effects model (equation 4.4b), follows the same pattern, as the OLS and the IV 2sls models are respectively reported in specifications (3) and (4).

Crucially, my contribution relies upon my ability to make distinctions between the OLS and IV 2sls terrorism estimates, where this can only be accomplished if my instrumental variables are valid. As such, I discuss my use of the probability values (p-values) of both the Hansen J statistic and the first stage regression F statistic at the outset. For instance, the higher the p-value generated from the Hansen J statistic, the more confident I can be that my instrumental variables satisfy the exogeneity condition. Additionally, the lower the p-value generated from the F statistic on all of the instrumental variables in the first stage regression, the more confident I can be that my instrumental variables satisfy the relevance condition. While a high Hansen J statistic p-value and a low first stage F statistic would provide evidence in favor of my instrumental variables being valid, I only find this in the year/trading-pair fixed effects models. This is unsurprising, though, if I consider that, compared with the year fixed effects model, an additional 20,000 pairwise dummies are included as explanatory variables, which presumably captures the remaining unobservable factors that are both relevant for explaining

Table 1.2 –OLS and IV 2sls of terrorism on trade using fixed effects

Dependent variable: natural log of DOT's real (constant 2010 $US) value of bilateral imports (cost, insurance and freight) [2007-2016]				
	(1)	(2)	(4)	(5)
(Specification notes)	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])
Terrorism variable:				
Impact of terrorism (*IT*) Log(1 + $IT_{i,t}$*$IT_{j,t}$)	**-.1832***** **(.0061)**	**-.5165***** **(.0147)**	**-.0019** **(.0059)**	**-.1391***** **(.0430)**
Economic variables:				
Real (constant 2010 $US) GDP (*Y*) (Log Product)	1.247*** (.0032)	1.333*** (.0046)	.7784*** (.0515)	.7908*** (.0517)
Real (constant 2010 $US) GDP per capita (*Y*/*P*) (Log Product)	.0156*** (.0042)	-.0842*** (.0059)	.2069*** (.0575)	.1191* (.0637)
Time-varying gravity variables:				
Regional trade agreement (*rta*)	.5532*** (.0146)	.5545*** (.0147)	.0198 (.0203)	.0072 (.0207)
Common Currency (*comcur*)	.1016*** (.0377)	.0785** (.0375)	.1194*** (.0336)	.0936*** (.0346)
Time-invariant gravity variables:				
Distance (*dist*) (Log)	-1.206*** (.0083)	-1.261*** (.0086)	-	-
Common official language (*lang*)	.6389*** (.0186)	.6414*** (.0188)	-	-
Common land border (*contig*)	.8128*** (.0393)	.7626*** (.0389)	-	-
Common colonizer (*comcol*)	.9532*** (.0258)	.9358*** (.0258)	-	-
Same nation (*smctry*)	.9685*** (.0538)	.9073*** (.0539)	-	-
Colonial relationship (*colony*)	.7866*** (.0344)	.8831*** (.0348)	-	-
[P-value (↓)]	-	**[.0000]**	-	**[.7488]**
Hansen J statistic	-	293.328	-	.579
Year fixed effects	Yes	Yes	Yes	Yes
Pairwise fixed effects	-	-	Yes	Yes
Number of (total) groups	10	10	20,356	20,356
Number of observations	180,686	180,686	179,712	179,712

[1]: Terrorism is instrumented by Log(1 + $GRoR_{i,t}$*$GRoR_{j,t}$), Log(1+$SRoR_{i,t}$*$SRoR_{j,t}$), and Log($TbG_{i,t}$*$TbG_{j,t}$)

Notes: Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. In each relevant specification, the 10-year dummies are all included. In the year/trading-pair fixed effects specifications, one of the trading-pair dummies are deemed redundant and, thereby, omitted due to collinearity. "Number of (total) groups" refers to the sum of the year and trading-pair dummies included in the model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level

Table 1.3 – First stages of IV 2sls regressions in Table 1.2

Dependent variable: Natural logarithm of (1 + (product) IEP's Impact of Terrorism) [2007-2016]		
	(1)	(2)
(Specification Notes)	(OLS)	(OLS)
Instrumental variables for terrorism:		
Governmental Restrictions on Religion (*GRoR*)	.0532***	.0139***
Log(1 + $GRoR_{i,t}$*$GRoR_{j,t}$)	(.0027)	(.0046)
Societal Restrictions on Religion (*SRoR*)	.3304***	.0999***
Log(1 + $SRoR_{i,t}$*$SRoR_{j,t}$)	(.0026)	(.0027)
Tensions between groups (*TbG*)	.3385***	.3516***
Log(1 + $TbG_{i,t}$*$TbG_{j,t}$)	(.0047)	(.0117)
Economic variables:		
Real (constant 2010 $US) GDP (*Y*)	.1370***	.1883***
(Log Product)	(.0012)	(.0241)
Real (constant 2010 $US) GDP per capita (*Y*/*P*)	-.1269***	-.6435***
(Log Product)	(.0018)	(.0267)
Time-varying gravity variables:		
Regional trade agreement (*rta*)	.0601***	-.0942***
	(.0055)	(.0091)
Common currency (*comcur*)	.1704***	-.2167***
	(.0138)	(.0169)
Time-invariant gravity variables:		
Distance (*dist*)	.0218***	
(Log)	(.0031)	
Common official language (*lang*)	.1586***	
	(.0069)	
Common land border (*contig*)	.0206	
	(.0155)	
Common colonizer (*comcol*)	-.2022***	
	(.0086)	
Same nation (*smctry*)	.0321*	
	(.0182)	
Colonial relationship (*colony*)	.1676***	
	(.0190)	
[P-value (↓)]	**[.0000]**	**[.0000]**
F statistic (only on the 3 instrumental variables)	12,070.06	798.13
Year FE	Yes	Yes
Trading-pair FE	-	Yes
Number of (total) groups	10	20,356
Number of observations	180,686	179,712

Notes: Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. $IT_{i,t}$ refers to country *i*'s impact of terrorism score in year *t*. In each relevant specification, the 10-year dummies are all included. In the year/trading-pair fixed effects specifications, one of the trading-pair dummies are deemed redundant and, thereby, omitted due to collinearity. "Number of (total) groups" refers to the sum of the year and trading-pair dummies included in the model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

bilateral imports in the current year (i.e. my dependent variable) and correlated with my terrorism (product) variable (i.e. my independent variable of interest). Hence, the inclusion of trading-pair fixed effects helps ensure that the error term is uncorrelated with my instruments for terrorism. With this qualification in mind, the year/trading-pair fixed effects specifications provide evidence in favor of my instruments being valid. For instance, specification (4) in Table 1.2 reports that the Hansen J statistic p-value is above .74 and specification (2) in Table 1.3 reports that the first stage F-statistic on all of the instrumental variables for bilateral terrorism is .0000. All in all, these p-values enable us to feel confident that my set of instrumental variables are valid in this case, which means that the year/trading-pair IV 2sls terrorism estimate more closely reflects reality compared to its OLS counterpart.

Consistent with Nitsch and Schumaker (2004), my year fixed effects OLS investigation (specification (1) in Table 1.2) finds all of the economic and trade cost variables (including terrorism) to be highly statistically significant predictors of real bilateral imports with all the expected signs. More specifically, the coefficient on terrorism is indeed negative with a t-statistic above 30. The effect of bilateral terrorism on real bilateral trade is also economically large, as the point estimate of -.1832 implies that a 30% increase in the extent to which two countries are jointly impacted by terrorism is associated with a *ceteris paribus* decrease in real bilateral imports of about 5.5%. To make a 30% increase in the terrorism (product) more concrete, it is useful to consider the United States/France trading pair (i.e. the United States is the exporting country and France is the importing country) between 2015 and 2016. For instance, both countries were already meaningfully impacted by terrorism as of 2015 (the impact of terrorism scores for the United States and France in 2015 were 4.613 and 4.553 respectively), where both countries happened to be struck by particularly deadly terrorist attacks in 2016.

Specifically, the United States experienced the Orlando night club shooting, which injured 53 people and killed 50. Additionally, France suffered from Bastille Day, in which over 80 people were killed and over 400 people were injured. All things considered in 2016, the United States/France and the France/United States trading-pairs suffered from a 30% increase in the product of their respective impact of terrorism scores from 2015 to 2016. So, given that the ($US 2010) value of merchandise that was imported to France from the United States was about $44 billion, this means that France is expected to reduce the real value of their imports from the United States by nearly $2.5 billion as a result of how both countries were affected by terrorism in 2016. In contrast, the IV 2sls results reported in specifications (2) tell a much bleaker story, where the IV 2sls terrorism estimate is nearly three times stronger than that of the OLS terrorism estimate in specification (1). Yet the Hansen J statistic is very high, meaning that the year fixed effects IV 2sls terrorism specification is unreliable, as the residual appears to be highly correlated with my instrumental variables.

In the last two columns of Table 1.2, I use trading-pair fixed effects to account for the unobservable factors that are not only relevant for trade but also correlated with my instrumental variables. Consistent with Bandyopadhyay, Sandler, and Younas (2018), who showed that controlling for specific trading-pairs and years is necessary to avoid overestimating terrorism's adverse effect on trade flows, I find that, in comparison to my OLS year fixed effects results (specification (1)), the OLS estimate for terrorism in the year/trading-pair fixed effects model (specification (3)) remains negative, declines substantially in strength, and becomes a statistically insignificant predictor of bilateral trade at the 10% level. In fact, the t-statistic on the terrorism coefficient in specification (3) is a meager .32, providing compelling evidence in favor of the null hypothesis that the effect of terrorism on trade is not statistically different from zero.

On the contrary, the IV 2sls estimate (reported in specifications (4)) is not only reliable (as the Hansen J statistic p-value is above .74 and the first stage F-statistic on the instruments is nearly 800), but it also tells a much different story compared with specification (3). For instance, the IV 2sls coefficient on terrorism in specification (4) remains negative but becomes statistically significant at the 1% level with a t-statistic above 3.2. Plus, compared with specification (3), the effect of bilateral terrorism on real bilateral imports is more than seventy times stronger in magnitude. Continuing to keep in mind that a 30% increase in bilateral terrorism is approximately equal to what the United States/France trading-pair faced between 2015 and 2016, specification (3) would predict that a 30% rise in bilateral terrorism will lead to a .057% reduction in the real value of merchandise goods imported from the United States to France, whereas specification (4) would predict that a 30% rise in bilateral terrorism will lead to a 4.017% reduction. The difference between the OLS and IV 2sls estimates are clearly meaningful from an economic standpoint, as the IV 2sls terrorism estimate suggests that the United States will suffer an additional 1.75 ($US 2010) billion compared with the suggestion of the OLS terrorism estimate in specification (3). And if that's not enough, it is also worth mentioning that the way in which France and the United States were impacted by terrorism in 2015 would *ceteris paribus* have a similar (albeit smaller) negative impact on real imports in any other trading pair that includes either France or the United States. The point of the foregoing exercise is to demonstrate the steep informational cost incurred by relying too heavily on OLS when examining the negative impact of terrorism on trade.

Notably, though, unlike Bandyopadhyay et al. (2018), my terrorism measure makes no distinction between domestic and transnational terrorist incidents. Yet, while their study finds *domestic* (but not transnational) terrorism to be a statistically significant predictor of trade at the

1% level, I find *overall* terrorism to be a statistically significant predictor of trade at the 1% level. Hence, considering that the inclusion of pairwise fixed effects controls for the country-pair-specific unobservable factors (which are relevant for explaining trade flows) and that the number of explanatory variables in the regression are increased by a substantial amount (specifically, by 20,344), I can reasonably assume that the issue of omitted variables is sufficiently accounted for in specification (3). Hence, the OLS terrorism estimate in specification (3) (i.e. the year/trading-pair fixed effects model) can be viewed as a truer depiction of reality compared to that of the OLS terrorism estimate in specification (1) (i.e. the year fixed effects model), where the presence of omitted variables causes the OLS terrorism estimate in specification (1) to be biased away from zero (i.e. specification (1) *overstates* the true negative relationship between terrorism and trade).

An omitted variable story, especially between 2007 and 2016, plausibly centers on the omission of nationalism, as this concept is highly elusive (and, therefore, difficult to suitably measure) and, at least conceptually, correlated with both trade and terrorism. For example, many countries in the West, on the whole, have developed a growing and sustained distaste for international trade in recent years, reasonably due to a wave of nationalism that has come to the fore. What's more, nationalism can have an effect on bilateral imports, regardless of whether strong nationalistic sentiments are taking root in the exporting or importing country, as both cases *ceteris paribus* lead to the creation of additional trade barriers. After all, if a wave of nationalism takes root in the exporting country, it could plausibly lead to more export tariffs, which raises the cost of exporting goods to other countries, thereby leading to less bilateral imports with their importing trading partners. Similarly, if a wave of nationalism takes root in the importing country, it could plausibly lead to a reduction in import quotas, which further

limits the quantity of merchandise goods that can be imported into the affected country, thereby leading to less bilateral imports with exporting trading partners. In essence, the inclusion of (non-time-varying) trading-pair fixed effects helps control for the negative impact of nationalism on trade between 2007 and 2016. Furthermore, the fact that I find the OLS terrorism estimate in specification (1) to be greater (in absolute value) compared to the OLS terrorism estimate in specification (3) (i.e. the truer estimate for terrorism), it seems to suggest that nationalism is positively correlated with terrorism. Though this result does not necessarily imply that nationalism has a positive effect on terrorism (as the positive impact of terrorism on nationalism may be driving the positive correlation between nationalism and terrorism), this result does nothing to suggest that the opposite story holds (i.e. that nationalism has a negative impact on terrorism). In any case, the failure to sufficiently control for nationalism in the year fixed effects model, helps explain why the OLS terrorism estimate in specification (1) is more strongly negative than that of the OLS estimate in specification (3).

And insofar as the inclusion of trading-pair fixed effects helps chip away at the problem of omitted variables, the IV 2sls trading-pair fixed effects regression may be chipping away at the problem of reverse causality. After all, if the remaining endogeneity bias in specification (3) is being driven by a reverse causality between terrorism and trade, I can intuit how trade affects terrorism by comparing specification (3) to specification (4). Since the OLS and IV 2sls terrorism estimates report the same sign and since the IV 2sls terrorism estimate is more than seventy times stronger than that of OLS, the reverse-causality problem appears to account for a substantial part of the endogeneity bias on the OLS terrorism coefficient. More specifically, the reverse causality between terrorism and trade biases the OLS terrorism estimate toward zero (i.e. the OLS terrorism estimate understates the true negative estimate for terrorism).

Table 1.3 reports the first stage of the instrumental variables two-stage least-squares (IV 2sls) regressions in Table 1.2. In addition, Table 1.3 contrasts the first stage results under both estimation models described in equations 4.3b and 4.4b. The first model estimated is the year fixed effects model (equation 4.3b), which is reported in specification (1) of Table 1.3. The second model estimated is the year/trading-pair fixed effects model (equation 4.4b), which is reported in specification (2). The first stage results provide further support that my findings in Table 1.2 are valid. Namely, regardless of which estimation model is used, the F statistic on the three instrumental variables are enormous, where the minimum first stage F statistic exceeds a value of 263. What's more, all of the instrumental variables are statistically highly significant predictors of terrorism, each reporting a t-statistic above 3. Moreover, as expected, each instrument is shown to be positively associated with terrorism, where the tensions between groups measure is shown to have the strongest impact on terrorism compared with the other instruments. For instance, further tensions between groups or stricter restrictions on religion (brought on by either the government or society) both facilitate the recruitment and deployment process for terrorist groups, resulting in more frequent and/or more lethal terrorism.

6. Robustness: Model and Results

In the previous section, I used trading-pair fixed effects to help control for the country-specific or country-pair-specific factors that are not directly observable but are, nonetheless, relevant for explaining international trade (e.g. multilateral trade-resistance). And because my regressor of interest (terrorism) and my economic measure both vary over time, an opportunity arises to additionally generate first-difference results. Hence, if my first-difference results resemble those of the fixed effects results from the previous section (particularly those of the

year/trading-pair fixed effects model), then it would lend credence to the notion that my empirical results are robust. The purpose of this section is to both precisely describe the first-difference estimation model and discuss the results.

6.1 Robustness Model

First off, recall that an observation was defined as an *ij* trading pair in year *t* in the previous section. Hence, the use of first-difference data necessarily omits explanatory variables that are time-invariant with respect to the *ij* trading-pair (i.e. for many of the variables, there is no difference between the *i,j,t* and *i,j,t-1* datapoints). The variables that will get canceled out during the differencing include bilateral distance, the five dummy variables that proxy for the cultural component of trade, and the trading-pair fixed effects. For instance, the distance and the colonial history between countries *i* and *j* did not change over the course of any given year (particularly, between 2007 and 2016), which means that these kinds of variables are subsequently dropped from any first-difference estimation model. Moreover, none of the fixed effects models contain a time trend variable. With these points in mind, only the dependent variable (real bilateral imports), the independent variable of interest (terrorism), the economic variables (Real GDP and Real GDP per capita), and the two time-varying gravity variables (regional trade agreement and common currency dummies) remain in the first-difference model, where the constant is excluded. But since the year dummies, by definition, vary over time, I also include year fixed effects (or, what I can more appropriately call "one-year-difference" fixed effects) to control for the unobserved characteristics of each specific one-year time horizon. Accordingly, the results reported when using first-differences are based on the following:

$$\Delta^1 \ln(M_{i,j,t}) = a_1\Delta^1 \ln(Y_{i,t}*Y_{j,t}) + a_2\Delta^1 \ln((Y_{i,t}*Y_{j,t})/(P_{i,t}*P_{j,t})) + a_3\delta_2\Delta^1 \ln(1 + IT_{i,t}*IT_{j,t})$$

$$+ \; a_3\delta_8\Delta^1 rta_{i,j,t} + a_3\delta_9\Delta^1 comcur_{i,j,t} + a_6 I_{t,t-1} + \varepsilon_{i,j,t} \qquad (6.1)$$

where Δ^1 refers to the first-difference operator (which takes the difference of the $x_{i,j,t}$ and $x_{i,j,t-1}$ datapoints for variable x); *1* refers to the 1-year lag; $I_{t,t-1}$ specifically refers to the nine "year-difference" dummies in my panel dataset, one dummy for each one-year time horizon; a is the coefficient; ε is the pure white noise error term; $M_{i,j,t}$ is the (constant $US 2010) value of imports (cost, insurance and freight) that flow from exporter country i to importer country j in year t; $Y_{i,t}$ and $Y_{j,t}$ are the (constant $US 2010) value of GDP for exporter country i and importer country j respectively in year t; $P_{i,t}$ and $P_{j,t}$ are the population, total for the exporter country i and the importer country j in year t; and $IT_{i,t}$ and $IT_{j,t}$ are the impact of terrorism scores for the exporting country i and importing country j respectively in year t. For purposes of clarity, I also rewrite equation 6.1 using the same above definitions, but this time without the first-difference operator:

$$\begin{aligned}
[ln(M_{i,j,t}) - ln(M_{i,j,t-1})] = \; & a_1[ln(Y_{i,t}*Y_{j,t}) - ln(Y_{i,t-1}*Y_{j,t-1})] \\
& + a_2[ln(Y_{i,t}*Y_{j,t})/(P_{i,t}*P_{j,t}) - ln((Y_{i,t-1}*Y_{j,t-1})/(P_{i,t-1}*P_{j,t-1}))] \\
& + a_3\delta_2[ln(1 + IT_{i,t}*IT_{j,t}) - ln(1 + IT_{i,t-1}*IT_{j,t-1})] + a_3\delta_8[rta_{i,j,t} - rta_{i,j,t-1}] \\
& + a_3\delta_9[comcur_{i,j,t} - comcur_{i,j,t-1}] + a_6 I_{t,t-1} + [\varepsilon_{i,j,t} - \varepsilon_{i,j,t-1}] \qquad (6.1).
\end{aligned}$$

Notably, equation 6.1 can be estimated through ordinary least squares. However, if terrorism is the only endogenous variable and I have successfully identified at least one instrumental variable for terrorism, then equation 6.1 reflects the second stage of a two-stage least squares regression. In this regard, I represent the first stages of the two IV 2sls regressions with the following equation:

$$\begin{aligned}
\Delta^1 ln(IT_{i,t}*IT_{j,t}) = \; & \pi_1 Z_1 + \pi_2\Delta^1 ln(1+TbG_{i,t}*TbG_{j,t}) + \pi_3\Delta^1 ln(1+GRoR_{i,t}*GRoR_{j,t}) \\
& + \pi_4\Delta^1 ln(1+SRoR_{i,t}*SRoR_{j,t}) + \nu_{i,j,t} \qquad (6.2a)
\end{aligned}$$

where Z_1 represents all of the right-hand side terms in equation 6.1 (except for terrorism); $TbG_{i,t}$ and $TbG_{j,t}$ are tensions between groups for the exporting country *i* and the importing country *j* respectively in year *t*. $GRoR_{i,t}$ and $GRoR_{j,t}$ are the *governmental* restrictions on religion score for the exporting country *i* and the importing country *j* respectively in year *t*; $SRoR_{i,t}$ and $SRoR_{j,t}$ are the *societal* restrictions on religion score for the exporting country *i* and the importing country *j* respectively in year *t*.

To clarify verbally: in all of the IV 2sls difference results generated in this chapter, the first-difference of the natural logarithm of one plus the product of two countries' impact of terrorism (IT) scores is being instrumented by the first-difference of the natural logarithm of the product of two countries' tensions between groups (*TbG*) score in year *t*, the first-difference of the natural logarithm of one plus the product of two countries' *government* restrictions on religion (*GRoR*) score in year *t*, and the first-difference of the natural logarithm of one plus the product of two countries' *societal* restrictions on religion (*SRoR*) score in year *t*.

6.2 Robustness Results

Table 1.4 reports the first-difference results. Despite converting my panel data into first-differences, the main results from the previous section largely remain the same. To clarify, the first-difference OLS and IV 2sls coefficients are not expected to be identical to that of the fixed effects coefficients, since I am considering more than two time periods. Nonetheless, my instrumental variables (or, more specifically, the first-difference of my instruments) continue to pass the relevance and exogeneity tests with flying colors, reporting a Hansen J statistic p-value above .65 and a first stage F-statistic above 154. Moreover, and this is similar to the year/trading-pair fixed effects OLS model (specification (3) in Table 1.2), the first-difference OLS terrorism estimate fails to be statistically significant at the 10% level, with a t-statistic of

.875, in addition to being miniscule in size, with a coefficient that is over 50 times weaker than the comparable IV 2sls terrorism estimate. In fact, in terms of the distance between the OLS and IV 2sls terrorism estimates and in terms of the qualitative implications of the estimate, the magnitude of the bias is larger when using first-difference data, where the sign on the terrorism estimate reverses through OLS. Furthermore, the IV 2sls terrorism estimate remains statistically significant at the 1% level, despite using first-difference data, with a t-statistic above 2.7. Regarding the first-difference of the time-varying control variables, only Real GDP reports a statistically significant estimate and the effects of Real GDP on the first-difference of real bilateral imports is larger than in the year/trading-pair fixed effects IV 2sls model.

Table 1.4 –OLS with IV 2sls of terrorism on trade using first differences

Dependent variable: Δ1 [natural log of DOT's real (constant 2010 $US) value of bilateral imports (Cost, Insurance and Freight)] (2007-2016)		
	(1)	(2)
(Specification Notes)	(OLS)	(IV 2sls[1])
Terrorism Variable:		
Δ^1[Log(1 + (Product) Impact of Terrorism)]	**.0063**	**-.3338*****
(i.e. Δ^1Log(1 + $IT_{i,t}$*$IT_{j,t}$))	(.0072)	(.1223)
Economic Variable:		
Δ^1 [Log((Product) Real (Constant 2010 $US) GDP)]	.9607***	.9687***
(i.e. Δ^1Log($Y_{i,t}$*$Y_{j,t}$))	(.1205)	(.1214)
Δ^1 [Log((Product) Real (Constant 2010 $US) GDP per capita)]	-.0127	-.0847
(i.e. Δ^1Log(($Y_{i,t}$/$P_{i,t}$)*($Y_{j,t}$/$P_{j,t}$)))	(.1143)	(.1187)
Time-varying gravity variables:		
Δ^1 [Regional Trade Agreement ($RTA_{i,j,t}$)]	.0078	.0098
	(.0279)	(.0280)
Δ^1 [Common Currency ($ComCur_{i,j,t}$)]	-.0195	-.0268
	(.0535)	(.0558)
[P-value (↓)]	-	**[.6519]**
Hansen J Statistic	-	.856
One-year difference fixed effects	Yes	Yes
Number of Observations	154,354	154,354

[1]: The terrorism variable is instrumented by Δ^1Log(1 + $GRI_{i,t}$*$GRI_{j,t}$), Δ^1Log(1 + $SHI_{i,t}$*$SHI_{j,t}$), and Δ^1Log($TbG_{i,t}$*$TbG_{j,t}$)

Notes: Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. Δ^1 refers to the difference operator, which takes the difference, for a given trading pair (*ij*), of the x_{ijt} and $x_{ij(t-1)}$ datapoints for variable *x*. All of the ($x_{ijt} - x_{ij(t-1)}$) datapoints are in terms of ($x_{ij2016} - x_{ij2015}$), ($x_{ij2015} - x_{ij2014}$), ($x_{ij2013} - x_{ij2012}$), ($x_{ij2012} - x_{ij2011}$), ($x_{ij2010} - x_{ij2009}$), ($x_{ij2009} - x_{ij2008}$), or ($x_{ij2008} - x_{ij2007}$). "One-year difference fixed effects" refers to the inclusion of 9 dummy variables, one dummy for each one-year difference in the dataset. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table 1.5 reports the first stages of the IV 2sls regressions from Table 1.4, which

provides further support that the results in Table 1.4 are valid. Namely, all of the instrumental variables are statistically significant predictors of terrorism at the 1% level, each reporting a t-statistic above 3.8. Plus, the F statistic on the three instrumental variables exceeds 154. Furthermore, the societal restrictions on religion measure and the tensions between groups measure both report a positive sign, as expected, where tensions between groups is shown to affect terrorism by four times more than the societal restrictions on religion measure. For instance, more *societal* restrictions on religion or further tensions between groups both facilitate the recruitment and deployment process of terrorist groups, resulting in more frequent and/or more lethal terrorism. In contrast, when going from the fixed effects model to the first-

Table 1.5 – First Stage of IV 2sls regressions from Table 1.4

Dependent variable: Δ^1 [Log(1 + (Product) Impact of Terrorism] (2007-2016)	
	(1)
(Specification Notes)	(OLS)
Instrumental Variables (IV's):	
Δ^1 [Log(1 + (Product) *Government* Restrictions on Religion)]	-.0146***
(i.e. Δ^1Log(1 + $GRI_{i,t}$*$GRI_{j,t}$))	(.0038)
Δ^1 [Log(1 + (Product) *Societal* Restrictions on Religion)]	.0418***
(i.e. Δ^1Log(1 + $SHI_{i,t}$*$SHI_{j,t}$))	(.0022)
Δ^1 [Log((Product) Tensions between Groups)]	.1607***
(i.e. Δ^1Log($TbG_{i,t}$*$TbG_{j,t}$))	(.0138)
Economic Variables:	
Δ^1 [Log((Product) Real (Constant 2010 $US) GDP)]	.0920**
(i.e. Δ^1Log($Y_{i,t}$*$Y_{j,t}$))	(.0383)
Δ^1 [Log((Product) Real (Constant 2010 $US) GDP per capita]	-.2653***
(i.e. Δ^1Log(($Y_{i,t}$/$P_{i,t}$)*($Y_{j,t}$/$P_{j,t}$)))	(.0369)
Time-varying gravity variables:	
Δ^1[Regional Trade Agreement ($RTA_{i,j,t}$)]	.0024
	(.0097)
Δ^1[Common Currency ($Comcur_{i,j,t}$)]	-.0315
	(.0334)
[P-value (↓)]	**[.0000]**
F statistic (on the 3 instrumental variables)	154.98
One-year difference fixed effects	Yes
Number of Observations	154,354

Notes: Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. Δ^1 refers to the first-difference operator, which takes the difference, for a given trading pair (*ij*), of the x_{ijt} and $x_{ij(t-1)}$ datapoints for variable *x*. All of the ($x_{ijt} - x_{ij(t-1)}$) datapoints are in terms of ($x_{ij2016} - x_{ij2015}$), ($x_{ij2015} - x_{ij2014}$), ($x_{ij2013} - x_{ij2012}$), ($x_{ij2012} - x_{ij2011}$), ($x_{ij2010} - x_{ij2009}$), ($x_{ij2009} - x_{ij2008}$), or ($x_{ij2008} - x_{ij2007}$). "One-year difference fixed effects" refers to the inclusion of 9 dummy variables, one dummy for each one-year difference in the dataset. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

difference model, the sign on the *government* restrictions on religion instrument switches from positive to negative. A possible explanation for this result, which I have abstracted from so far, is that a one-unit increase in government restrictions may actually be an effective counterterrorism measure, at least in the short-run. For instance, stricter religious restrictions brought on by the government (e.g. ban on face covering) increases security, which, in the short-run, may be particularly effective in combatting the immediate threat of terrorism. There is, of course, a counterforce to this kind of action, as I have previously discussed, in that further religious restrictions brought on by the government help facilitate the recruitment and deployment process of terrorist groups (which helps explain why my other instruments are positively associated with terrorism). Nonetheless, it appears as though the counterterrorism benefits of imposing stricter governmental religious restrictions may outweigh the costs, at least in the short-run. In any case, the fact that all three of my instrumental variables for terrorism are statistically significant predictors of terrorism, as shown in Table 1.5, provides evidence in favor of my claim that my instruments are valid.

7. Concluding Remarks

I draw from the literature to identify three measures I intuit being valid instruments for terrorism within an augmented gravity model of trade. I capitalize on this opportunity by using two-stage least squares and comparing the resulting estimates to that of ordinary least squares, which shines an important light on the direction of the bias on the OLS terrorism estimate. Crucially, in the year/trading-pair fixed effects IV 2sls regressions, my set of instruments for terrorism (consisting of tensions between groups, *government* restrictions on religion, and *societal* restrictions on religion) combine to strike an especially robust balance of being

correlated with the endogenous regressor (terrorism) and being correctly excluded from a model of trade. Moreover, the validity of the set of instruments is contingent upon the inclusion of trading-pair fixed effects, where the year/trading-pair IV 2sls terrorism estimate is statistically significant at the 1% level. Consequently, the year/trading-pair IV 2sls terrorism estimates are suitable proxies for the "true" negative estimate for terrorism in a model of trade, where the IV 2sls terrorism estimates are uniformly stronger compared with OLS. What's more, this result is robust to the use of first-differences and, in many ways, further highlights the importance of my findings, as the magnitude of the bias on the OLS terrorism estimate is much larger under first-differences compared with year/trading-pair fixed effects. On the whole, my IV 2sls results are unrelenting in suggesting not only that terrorism is a statistically highly significant predictor of trade but also that the OLS terrorism estimate understates the true negative impact of terrorism on trade.

Yet, prior to these results being realized, researchers would (perhaps rightly) anticipate that the direction of the bias goes in the opposite direction. For instance, this chapter and Bandyopadhyay, Sandler, and Younas (2018) both show that, once country-pair fixed effects are included in an OLS investigation of the effects of trade on terrorism, the magnitude of the OLS terrorism estimate declines substantially. The rationale for this result being that country-pair fixed effects help solve the problem of omitted variables, which causes OLS models (which often do not include country-pair fixed effects) to overstate the extent to which terrorism negatively impacts trade. What's more, if I additionally assume that there is a problem of reverse causality between terrorism and trade (which this chapter is the first to address), the effect of this simultaneity bias might be expected to further bias the negative OLS terrorism estimate away from zero. This is because the simultaneity bias crucially depends on the sign of

the trade coefficient in a model of terrorism, where this sign is (perhaps most plausibly) negative. After all, it makes sense that an increase in bilateral trade would provide some security benefits, as greater economic activity would, in theory, increase individuals' opportunity costs of participating in terrorist activities, thereby reducing the frequency and lethality of terrorism in a country. Yet this story does not fit my results. When year and trading-pair fixed effects are included, I find both the OLS and IV 2sls terrorism estimates to be negative, but the OLS terrorism estimate is more than seventy-four times weaker than the IV 2sls terrorism estimate, suggesting that the OLS estimate substantially *understates* the extent to which terrorism negatively impacts trade. And, considering that the IV 2sls model is valid (and, thereby, suitably addresses the problem of reverse causality between trade and terrorism), if a reverse causality is, indeed, driving the endogeneity bias in this chapter, I can infer that, in a model of terrorism, the effect of trade on terrorism is, in fact, positive. In light of the foregoing, I offer a new story for the purposes of explaining my results, which, if true, could have far-reaching implications for counterterrorism policy in the future.

In essence, my results shed light on a few issues that are worth considering. The first (and more obvious) issue is that an OLS investigation into the effects of trade on terrorism will cause researchers to discount the true economic costs of terrorism. For instance, it could be argued that, while terrorism negatively impacts trade, the effect of terrorism on trade is economically insignificant (or, perhaps even, positive), as the OLS terrorism estimate is biased toward zero or biased in such a way that the sign is reversed. Yet, my results suggest that, once the problem of reverse-causality between terrorism and trade is suitably addressed, the preceding notion is highly dubious, where the cost of terrorism (in terms of reducing trade) is far greater than what an OLS investigation would suggest. Hence, this chapter, first and foremost, urges

policymakers to refrain from becoming disillusioned with the demanding task of developing effective counterterrorism measures where, I, instead, hope to motivate policymakers to reconsider and improve upon existing counterterrorism strategies.

The second (and more surprising) issue is that, because the OLS terrorism estimate understates the true terrorism estimate (once the problems of omitted variables and reverse causality are suitably addressed), it appears as though a growing international trade presence will actually increase terrorism. Though this result challenges the prevailing sentiment that free trade can make all countries better off (which is perhaps most prominently stated as one of the 10 Principles of Economics by Gregory Mankiw), it has an intuitive basis. To begin, it seems reasonable to say that when more trade is coupled with a failure to sufficiently compensate those who stand to lose from this outcome, it will lead to a more nationalistic country. A greater preference for nationalism can, then, lead to rhetoric and policies that are hostile to the communities from which terrorist organizations build a coalition of highly committed members. Hence, the further stigmatization of marginalized groups can actually aid the production process of terrorist organizations, which leads to more frequent or more lethal terrorist attacks. In this regard, the positive effect of trade on terrorism runs through the "nationalism" channel.

If nationalism is indeed the most plausible channel through which trade links positively with terrorism, it would be particularly clarifying on a couple of policy fronts. First, while various social safety net programs may exist to protect low-income citizens from poverty and hardship, these policies generally do not explicitly target citizens who stand to lose from international trade. So, in the midst of a country increasing their international trade presence, it is likely that the losers of trade may not be compensated in a way that is proportional to the severity of the shock. Consequently, the losers of trade conceivably become frustrated with the

current situation, where their frustration is directed towards those who both benefit from trade and who are thought to be undeserving of their benefits (namely, foreign firms and workers). In this regard, a subset of a country's electorate will become more willing to strengthen their preferences for nationalism. And, extrapolating the implications even further, a more nationalistic country is problematic because it seems to lay the groundwork for terrorist organizations to thrive. Hence, a surprising insight from this chapter is that, insofar as policymakers are able to "fairly" compensate the losers of international trade, it will actually chip away at the problem of terrorism. What's more, an additional insight that is implicit to this story is that the enactment of nationalistic policies or the ratcheting up of nationalistic rhetoric may, in fact, create more terrorism within a country.

To make these policy insights more concrete, consider the "French ban on face covering," which is a law that prevents people from wearing face-covering headgear in public places located in France. The justification for this policy broadly centered on nationalistic concerns (e.g. concerns over security, immigration, and the social environment), whereby the enactment of this policy is perhaps best described as a response to France's persistent battle against terrorism in addition to its long-standing trend toward globalization (or, specifically, its growing international trade presence). Yet, regardless of what permitted this idea to become law, it stands to reason that this policy, either wittingly or unwittingly, creates a more intolerant majority (in the sense that more pressure is applied to those who actively challenge the norms within a particular society or country). This, in turn, raises the cost of remaining a member of a group wherein membership is contingent upon subversion (e.g. a radical religious group might require members to physically cover their face in public despite such acts being forbidden). So, given that the most lethal terrorist groups in the world are radical Islamists and that these kinds

of groups generally require women to (at minimum) cover their faces in public, this kind of law raises the cost of continuing to be a Radical Islamist in France. Hence, the more halfhearted members will tend to relinquish their ties to Radical Islam as it is relatively costly for them to retain membership compared to that of the more committed members. This means that the pool of French Radical Islamists will become more concentrated with highly committed members. And, to the extent that these groups wish to inflict terror on a country, a ban on face covering helps them overcome the critical challenge of finding members who are willing to die for the organization's cause, which is necessary if a group is to successfully launch a terrorist attack. As such, a terrorist attack or a trend toward globalization can increase a country's overall preference for nationalism, which may give rise to policies that increase the likelihood of a successful terrorist attack by a violent extremist religious sect, which results in more frequent or lethal terrorist attacks in that country. In this regard, trade increases terrorism through the "nationalism" channel.

To sum up, by motivating the endogeneity bias through a reverse-causality story and by finding that the OLS terrorism estimate substantially understates the true terrorism estimate in a model of trade, this chapter highlights the possibility that it may be better, in terms of developing effective counterterrorism policy, to sever the positive link between trade and terrorism by weakening the force that binds them (namely, nationalism). In this regard, if the effects of nationalism can be successfully contained, then countries can potentially avoid the apparent pitfalls of trending towards globalization and, in so doing, propel themselves into a pareto-improving situation, whereby countries can benefit from the additional wealth created by more trade while simultaneously making improvements to its national security.

Although this chapter sheds light on the true impact of terrorism on international trade, it remains an open question whether my instrumental variables approach addressed the problem of reverse causality, measurement error, or some combination of the two. While I tend to presume that the endogeneity bias is being driven by a reverse causality between terrorism and trade, it stands to reason that the OLS terrorism estimate is biased toward zero, not because trade affects terrorism positively, but because there was an error regarding how the terrorism variable was measured. Consequently, future work might consider empirically testing the hypothesis that trade has a positive effect on terrorism. Furthermore, given that the results of this chapter show that terrorism is a negative and statistically significant predictor of trade, a standard OLS approach would produce a biased terrorism estimate, where the OLS trade estimate (in a model of terrorism) would exhibit a downward bias. Consequently, an instrumental variables approach, in the context of understanding the effect of trade on terrorism, would be a welcomed contribution to both the trade and the terrorism literature. In fact, future work might consider applying the empirical strategy outlined in this chapter to identify measures that are both relevant for trade and correctly excluded from models of terrorism. For if valid instrumental variables for trade (in a model of terrorism) can be found, we would be one step closer to understanding the true impact of trade on terrorism in addition to understanding why the OLS terrorism estimate (in a model of trade) is biased towards zero. This sentiment provides a segue into my next chapter.

CHAPTER 2: TRADE-TO-TERRORISM

1. Introduction

Do countries with higher current levels of international trade experience more or less overall terrorism in the future?[3] James Anderson (2015) uniquely develops a rational-choice model that allows for an explicit examination of this very question. He "effectively juxtaposes two important opposing forces—larger markets tend to be safer but larger markets also are more attractive targets" (p. 188). The second force, which he calls the "bang for the buck" effect, very succinctly explains why trade volume can have a positive effect on terrorism. This theoretical claim, however, is not borne out in the data. For example, Freytag et al. (2011) report that the effect of trade on terrorism is negative, while Burgoon (2006) in addition to Bravo and Dias (2006) suggest that there is no effect. But, as Gaibulloev and Sandler (2019) point out in their recent survey of the terrorism literature since 9/11, none of the empirical studies within the self-proclaimed trade-to-terrorism literature account for the "obvious endogeneity concern"—namely, that terrorism may have a significant and disruptive impact on international trade (p. 312-313). This failure of the trade-to-terrorism literature to account for a reverse causality calls into question its fundamental results, particularly when it comes to the reported sign and statistical significance of the trade coefficient.

[3] Overall terrorism refers to both domestic and transnational terrorism. Domestic terrorism is homegrown, and home directed whereby the victims and perpetrators are citizens of the venue country (i.e. the country in or against which the terrorist attack is launched). In contrast, transnational terrorism involves two or more countries, meaning that the venue country in addition to the nationalities of the victims and perpetrators are not uniform.

The present chapter makes two distinct though related contributions to the terrorism literature. First, it addresses possibility of reverse causality running from terrorism to trade. It does so through the creation of three measures related to a country's total common currency relationships along with two economic indices of remoteness and colonial history that are used as instruments for aggregate trade in a cross-country model of terrorism. Second, it characterizes the simultaneity bias that any study which investigates the association between terrorism and trade needs to address. This is important because, like the trade-to-terrorism literature, the terrorism-to-trade literature fails to account for a reverse causality running from trade to terrorism with instrumental variables. If, in fact, trade affects the incidence of terrorism, then any empirical analysis of the effects of terrorism on trade would likely produce misleading results.

To elaborate further on the second contribution, more focused attention, to date, has been paid to the terrorism-to-trade literature compared with the trade-to-terrorism literature. This is perhaps due to the direction of the causal relationship running from terrorism to trade being self-evident, especially compared to the direction of the reverse causal process. Indeed, an uptick of terrorist activity in a country seems to raise the cost of trade (e.g. greater security requirements involved in foreign trade and travel), which *ceteris paribus* results in less trade. Furthermore, the expected negative relationship running from terrorism to trade is also borne out in the data, where terrorism is generally found to have a significant and negative effect on trade (Nitsch and Schumaker, 2004; Blomberg and Hess, 2006; Mirza and Verdier, 2014; Bandyopadhyay et al., 2018; and Morales, 2019). But, with the exception of Morales (2019), these empirical studies fail to account for the existence of a reverse causal relationship running from trade to terrorism, leaving important questions regarding the biased nature of the negative terrorism coefficient

unanswered. Of course, if trade has no effect on terrorism, then the common approach of the terrorism-to-trade literature to ignore issues of reverse causality would be valid. Yet, if alternatively, trade does indeed have a meaningful impact on terrorism, then the direction of this relationship could necessarily suggest the sign for the simultaneity bias within the terrorism-to-trade literature. For instance, if trade has a positive effect on terrorism, then not accounting for a reverse causality would bias the negative terrorism coefficient up, towards zero (i.e. the extent to which terrorism negatively impacts trade would be understated). In contrast, if trade has a negative effect on terrorism, then not accounting for a reverse causality would bias the negative terrorism coefficient down, away from zero (i.e. the extent to which terrorism negatively impacts trade would be overstated). As such, this chapter's first contribution to more credibly investigate both the existence and direction of the causal relationship running from trade to terrorism is underscored by its added ability to better understand the terrorism-to-trade empirical literature.

Methodologically, this chapter builds on Abadie's (2006) empirical model of an index of terrorism that accounts for issues of reverse causality running from terrorism to the economy through an instrumental variables approach. Notably, while Abadie (2006) primarily uses Real GDP per capita as his economic control variable, I alternatively use aggregate trade. In the end, my central finding is that the two-stage least squares estimates of the effect of aggregate imports on future terrorism in the destination country are significant, positive, and stronger than OLS. This finding provides empirical support not only for the notion that there exists a causal relationship running from trade to terrorism, but also for the hypothesis that the effect of trade on terrorism is positive. Besides a direct relationship running from trade to terrorism through the "bang for the buck" effect, trade may also influence various economic and social conditions that in turn lead to terrorism. This chapter, then, motivates further investigation into certain

economic and social factors that could plausibly constitute a transmission channel from trade to terrorism.

The chapter proceeds as follows: Section 2 helps motivate the data and empirical methodology of the present chapter; Section 3 describes this chapter's data and empirical methodology in greater detail; Section 4 discusses the results; and Section 5 draws the main conclusions and discusses some potentially fruitful avenues for future research.

2. Details of the Empirical Approach

In the years that immediately followed the extraordinary 9/11 terrorist attacks, scholars became increasingly interested in understanding the root causes of terrorism. Unsurprisingly, poverty was believed to be not only a contributing factor of terrorism, but, at least initially, terrorism's preeminent root cause. For instance, in the December 2001 gathering of Nobel Peace Prize laureates, the previous year's winner, Kim Dae-jung, made the following comment: "At the bottom of terrorism is poverty. That is the main cause. Then there are other religious, national, and ideological differences" (cited in Jai, 2001). The potential importance of poverty explains why scholars who empirically analyze the root causes of terrorism make sure to include an economic control variable in their model among other classes of control variables. In this section, I first motivate the various factors that are expected to influence terrorist activity, including prevailing economic conditions. After this, I discuss the empirical trade-to-terrorism literature and later reveal its deficiencies before alluding to a path forward.

2.1 Explanatory variables for terrorism

The sentiment of Kim Dae-jung notwithstanding, the theoretical basis for including an economic control variable in an empirical model of terrorism primarily stems from "relative

deprivation" theory. Indeed, this theory was coined by Ted Robert Gurr (1970) and has been a particularly influential school of thought, inspiring a large body of multidisciplinary research on whether political violence, of which terrorism is a specific form, is rooted in poverty. This theory specifically builds on Festinger's (1954) social comparison theory in which individuals necessarily compare their economic situation to a reference group. If this hypothesis holds true, one would naturally predict that impoverished individuals, and/or unequal societies provoke particularly aggressive responses leading to heightened political violence or even terrorism. As an important qualification to "relative deprivation" theory, Charles Tilly's (1978) "political opportunity structures" framework is also worth discussing. In short, he argues that economic deprivation alone does not lead to violence. For instance, even if the citizenry is deprived economically, this should not increase the incidence of terrorism if there is a paucity of outlets available for citizens to express their frustration. Of course, regardless of a country's level of deprivation, the government could overreach in terms of repressing their citizenry, thereby extending beyond a threshold level of political repression above which a violent backlash, like terrorism, is likely to ensue. Either way, in order to determine the true impact of economic deprivation on terrorism, it is important to control for the extent to which a country lacks political rights or civil liberties.

Furthermore, the previous sentiment of Kim Dae-jung suggests that, in addition to a measure of poverty or political ideology, religious differences should also have explanatory power for terrorism. This notion stems primarily from Samuel Huntington's (1996) well-known "Clash of Civilizations" thesis, which suggests that conflict results from the different cultural groups that shape civilizations. More precisely, because countries are generally built atop different linguistic, ethnic, or religious groups, he reasoned that these differences could create the

basis for conflict and, more specifically, terrorism. And, although this thesis focuses mostly on religious differences, a similar line of reasoning can be made for either linguistic or ethnic differences leading to more terrorism. Or, put differently, linguistic, ethnic, and religious homogeneity in a country is expected to *ceteris paribus* lessen the extent to which this country is impacted by terrorism in the future. In this regard, "fractionalization" measures, which reflect the degree to which it is likely that two randomly-selected individuals chosen from a country belong to different linguistic, ethnic, or religious groups, should be controlled for in a model of terrorism.

A country's geography and climate should also influence terrorism. Firstly, a large-sized country provides terrorists with more locations within its borders that they can use as both hiding spots and potential targets to attack. Secondly, and using a similar rationale, countries with large differences in elevation also favor terrorism. This idea is perhaps best exemplified by Afghanistan where the mountainous terrain has and continues to promote terrorism in this country as its geography makes it particularly difficult for counter-terrorist forces to flush out terrorists. Thirdly, the extent to which a country's climate is tropical can also affect terrorist activity in that country. For example, a country whose climate is mostly tropical is both warm and near water. Being located near water can potentially facilitate terrorism by making it easier for terrorist agents to enter or exit the country. Warm-weather climates can also facilitate terrorism in the same way that warmer weather is believed to have an amplifying effect on the incidence of criminal activity, which is partly due to the increased social interactions that take place when the weather is warm.[4]

[4] See, for example, Cohn (1990), which discusses how the physical environment can influence criminal behavior.

In light of the foregoing, four classes of control variables are especially important to include in any empirical model of terrorism: economic factors, political rights, fractionalization, and geography/climate.

2.2 The trade-to-terrorism empirical literature and a path forward

Most attempts to examine the empirical relationship running from trade to terrorism separately focus on either transnational or domestic terrorism.[5,6] However, these studies necessarily lose valuable information by excluding either domestic or transnational terrorism from their analysis. In recognition of these concerns, three studies use overall (both domestic and transnational) terrorism data to investigate the effect of international trade on terrorism. First, Freytag et al. (2011) run a series of negative binomial regressions with data on 110 countries between 1971 and 2004. Trade openness (defined as a country's sum total of exports and imports divided by its level of GDP) is used as a control variable and is ultimately found to have a statistically significant and negative association with the total number of terror (both transnational and domestic) incidents within a country per year. In contrast, Burgoon (2006) uses a cross-sectional and time-series analysis and concludes that trade has a null effect on both overall terrorism more generally and transnational terrorism more specifically. Likewise, Bravo and Dias (2006) use trade openness as a control variable in an empirical model of overall terrorism and find there to be no statistically significant association between trade openness and overall terrorist incidents in a country. In summary, these studies find trade to have either a

[5] Because data on transnational terrorism was readily available in the wake of the 9/11 terrorist attacks, most of the empirical trade-to-terrorism studies focus on the effects of trade on transnational terrorism: Li and Schaub (2004), Kurrild-Klitgaard et al. (2006), Drakos and Gofas (2006), Koch and Cranmer (2007), Blomberg and Hess (2008a, 2008b), Braithwaite and Li (2007), and Campos and Gassebner (2009).

[6] After Enders et al. (2011) devised a method to separate transnational versus domestic terrorism for the Global Terrorism Database, some empirical trade-to-terrorism studies began to focus on the effects of trade on domestic terrorism: Gries and Meierrieks (2013) and Gaibulloev et al. (2017).

negative or a null effect on overall terrorism. However, their failure to account for a reverse causality running from terrorism to international trade creates some ambiguity as to the appropriate interpretation of their findings.

Of course, the problem of simultaneity bias is hardly an insurmountable one, as evidenced by Abadie's (2006) empirical model of terrorism. Incidentally, his model includes each of the four classes of control variables that were mentioned in the previous subsection. But, beyond this, his model of terrorism is the first and, to the best of my knowledge, the only one to use an instrumental variables approach with respect to an economic control variable. To this end, he identifies a "geographic landlocked" variable as being a suitable instrument for poverty, which enables him to account for the very reasonable possibility that terrorism worsens the economic environment.[7] Importantly, the contribution that Abadie (2006) makes to the terrorism literature is strikingly similar to that of the present chapter in that we both aim to address a reverse causality running from terrorism to an economic control variable, but, unlike Abadie (2006), trade (as opposed to poverty) remains my economic control variable which motivates the identification of a distinct set of instrumental variables.[8] Our differences notwithstanding, the commonalities that Abadie (2006) and the present chapter share in terms of our contribution to the terrorism literature are what motivates me to build on his primary OLS specification.

3. Empirical Methodology

To measure the effects of international trade on the impact of terrorism (IT) and to

[7] To motivate this claim, Abadie (2006) refers to both Abadie and Gardeazabal (2003) for a case study of terrorism effects on economic outcomes and Frey (2004) for a survey of this literature.

[8] This is just the main difference, where our empirical models have other meaningful differences. For instance, the present chapter uses a panel regression framework, whereas Abadie (2006) uses a cross-sectional framework.

maintain continuity with Abadie (2006), I use country-level data for each year between 2001 and 2017 within a panel regression framework using IT as a dependent variable and a list of explanatory variables that includes one proxy for trade, namely aggregate imports. The evaluation of the unidirectional effect of trade on terrorism through OLS is problematic due to the reverse impact that terrorism could have on trade. For example, a handful of studies use a gravity model of trade to provide empirical evidence suggesting that terrorism has a negative impact on trade (e.g. Nitsch and Schumaker, 2004; Bandyopadhyay et al., 2018). As such, an important contribution of this chapter is to address the apparent simultaneity bias through an instrumental variables approach.

3.1 Data

Tables A.1 and A.2 contain the definitions of the variables in the dataset and descriptive statistics. In this chapter, terrorism is measured by the Institute of Economics and Peace's Global Terrorism Index (IEP-GTI), which measures the extent to which (up to) 161 countries are impacted by both transnational and domestic (i.e. overall) terrorism between 2001 and 2017. The factors within the IEP-GTI score include the number of incidents, fatalities, injuries, and property damage resulting from terrorism along with a five-year weighted average to account for the lingering psychological damage caused by terrorism. Relatedly, the IEP-GTI can serve as a suitable proxy for terrorism risk,[9] which is also the case for Abadie's (2006) terrorism measure, the World Market Research Center's Global Terrorism Index (WMRC-GTI). I view the IEP-GTI to be a superior terrorism measure compared with the WMRC-GTI. For instance, the WMRC-GTI was only calculated for the 2003-04 period and has also never been made available to the public, whereas the IEP-GTI continues to be made available, both readily and publicly, for

[9] See, e.g., Procasky and Ojah (2016) which operationalizes the risk of terrorism through the IEP-GTI.

each year since 2001. In any case, I denote the terrorism score for each country as IT (i.e. the impact of terrorism), which ranges from 0-10, with higher scores reflecting a higher impact of terrorism.

3.1.1 Aggregate Imports

To measure international trade, I use the World Bank's merchandise imports measure expressed in Purchasing Power Parity (PPP). Admittedly, a measure of imports alone is not the standard variable for trade in cross-country models of conflict. Instead, trade openness (i.e. the sum of a country's exports and imports divided by its GDP) is more frequently used.[10] Yet, meaningful problems arise with trade openness measures, as Fujii (2018) suggests, where this measure mostly captures variations in GDP, rather than variations in the sum total of exports and imports. Furthermore, there is no way to distinguish between the consequences of imports versus exports while using this trade measure. Now, even if aggregate exports were instead used as the trade proxy in this chapter, the empirical results should produce strikingly similar results because, as shown in Table A.5, the correlation coefficient between aggregate exports and aggregate imports is nearly (positive) one.[11] To help illuminate why this is so, consider that, on the one hand, trade liberalization policies generally increase both imports and exports and that, on the other hand, protectionist policies typically decrease both imports and exports. In light of the foregoing, a measure of aggregate imports is used in this chapter, but, given the close, positive association between imports and exports, it cannot be determined whether the link from trade to terrorism is being driven by variations in exports or imports.

[10] Tables A.3 and A.4 report the results when I use trade openness as the proxy for international trade. But, importantly, the validity of the instrumental variables that I use for trade are less clear under these specifications.
[11] The empirical results regarding the use of a measure of aggregate exports are reported in Tables A.3 and A.4.

3.1.2 Instrumental Variables for Aggregate Imports

An important contribution of this chapter is to mitigate concerns over reverse causality running from terrorism to international trade. To this end, I use three bilateral measures to generate three instrumental variables for aggregate trade. One of these bilateral measures is derived from de Sousa (2012) and reflects the two-year lag of whether two countries share a currency. The two-year lag is used for this "same currency" dummy, largely because each year between 1996 and 2015 is provided by the data source whereas I examine each year between 2001 and 2017 in this chapter. And, because I wish to study the effects of trade on terrorism (where the terrorism measure does not directly describe a dyadic relationship), I must convert these bilateral datapoints into country-level terms. I do this by calculating the total amount of "same currency" pairs that exist between each (i) country and all of the (i,j) dyads provided by de Sousa (2012) in year t-2. The other two bilateral measures are derived from Mayer and Zignago (2011): one reflects the simple distance between the most populous cities or agglomerations of two countries and the other reflects whether two countries were ever in a colonial relationship. But, since the bilateral data of Mayer and Zignago (2011) do not vary over time, I make use of the calculation procedure proposed by Wei (1996), which effectively converts time-invariant bilateral variables (e.g. distance) into country-level data that can, in fact, vary over time. For one, the product of a time-varying "GDP weight" with respect to country j and bilateral distance is calculated for every (i,j) dyad. Each GDP-weighted bilateral distance value is, then, aggregated across all of the possible dyads for country i, which generates a time-varying country-level "remoteness" measure reflecting the extent to which a country is distant from large-income countries in the current year. This remoteness measure is also normalized between 0 and 10 using min-max normalization. A similar calculation is performed to generate a time-

varying country-level "colony" measure reflecting the extent to which a country had ever been engaged in a colonial relationship with large-income countries in the current year. The same calculation for the remoteness measure is used to generate this "colony" measure, except the product of a time-varying "GDP weight" with respect to country *j* and the "colony (ever)" dummy (rather than bilateral distance) is calculated for every (*i*,*j*) dyad prior to the aggregation and max-min normalization processes being performed.[12]

3.1.3 Other Covariates

In addition to an economic variable (specifically, aggregate imports), I also control for several other factors including political rights, fractionalization, and geography/climate. Again, controlling for each of these four classes of variables coheres with Abadie (2006). First, I control for the (expected) non-linear impact of political rights with both the raw and squared versions of the Freedom House's (2019) political rights variable. Second, I control for the three fractionalization variables (linguistic, ethnic, and religious), which are all provided by Alesina et al. (2003). Thirdly, I control for three measures that reflect either the importing country's geography or climate, including country area, elevation differences, and tropical area percentage, all of which are based on Nunn and Puga (2012). Lastly, since the size of a country's population could be an important control variable to include in a model of terrorism, I also control for the World Bank's total population measure.[13]

3.2 Empirical Specification

Before discussing the instrumental variables (IV) approach in greater detail, it is useful to

[12] The formulaic basis of each instrumental variable is presented on in greater detail in section 3.2.

[13] Because Table A.5 shows that the correlation coefficient between the aggregate import/export measures and population is high (above .45), I also report results when only the population measure is excluded from the model.

first present the basis for the ordinary least squares (OLS) results, which are derived from the following empirical model:

$$IT_{i,t} = \alpha + \beta_1 \ln(M_{i,t-1}) + \beta_2 \ln(Pop_{i,t}) + \beta_3 IT_{i,t-1} + \beta_4 LoPR_{i,t} + \beta_5 LoPR^2_{i,t} + \beta_6 Lang_i + \beta_7 Ethnic_i + \beta_8 Relig_i + \beta_9 Area_i + \beta_{10} Elev_i + \beta_{11} \%Trop_i + \beta_{12} Region_{i,t} + \varepsilon_{i,t} \quad (1)$$

where $IT_{i,t}$ represents country i's impact of terrorism score for year t as measured by the Institute of Economics and Peace's (2018) Global Terrorism Index; $M_{i,t-1}$ represents real merchandise imports that flows from any other country in the world to country i in year t-1;[14] $Pop_{i,t}$ is the total population of country i in year t; $LoPR_{i,t}$ is the lack of political rights score for country i in year t; $Lang_i$, $Ethnic_i$, and $Relig_i$ respectively represent a country's degree of language, ethnic, and religious fractionalization for country i; $Area_i$, $Elev_i$, and $\%Trop_i$ respectively represent country i's land area, elevation differences, and percentage of area that is tropical; $Region_{i,t}$ represents the regional-year dummy for country i in year t; ln$x$ denotes the natural logarithm of variable x; α is a constant term; β is a coefficient; and ε is the error term.

Furthermore, it is important to justify certain aspects of equation (1) before elaborating upon the variables. Firstly, since the instrumental variables of this chapter reasonably have less predictive power for the "Trade (% of GDP)" measure, I purposely use aggregate imports as my primary proxy for international trade. At the same time, I control for population size because this measure is expected to closely (but far from perfectly) reflect the size of a country's overall economy.[15] Secondly, in recognition of Abadie's (2006) report of a statistically significant, non-linear relationship between lack of political rights and terrorism, I include a quadratic term for

[14] By "real," I mean that each datapoint is expressed in terms of Purchasing Power Parity (PPP).
[15] Table A.5 reports the correlation coefficient between GDP and population size, which is above .56. Tables A.3, A.4, A.7 and A.8 also report results when I instead use "Trade (% of GDP)" as the proxy for international trade.

the lack of political rights control variable. Thirdly, while the inclusion of a lagged dependent variable differs from Abadie's (2006) cross-sectional empirical model, this approach reflects both the panel regression framework at hand and my attempt to control for the unobserved factors that equation (1) would otherwise fail to account for. Hence, the use of a lagged dependent variable is especially helpful in mitigating issues of omitted variables. Fourthly, regional-year fixed effects are employed to control for the interaction effects between each year and the region in which a country is located. The nine regional groups are taken from the Institute of Economics and Peace's (2018) Global Terrorism Index, which is the same source as the dependent variable. Fifthly, the measure of aggregate imports enters equation (1) logarithmically but the dependent variable (terrorism) does not because one unit increases in aggregate imports are expected to increase terrorism at a decreasing rate.[16] After all, a one unit increase in import levels will have a larger impact on countries with relatively low import levels because this rise in imports represents a greater percentage increase for low-import countries. For this reason, equivalent increases in aggregate imports reflect a larger (smaller) step toward globalization for countries with smaller (larger) import volumes. Hence, the positive effect of aggregate imports on terrorism is expected to be larger the smaller is a country's volume of imports. These considerations taken together justify a semi-log functional form with respect to the control variable of interest (aggregate imports) and the dependent variable (terrorism). A similar rationale motivates the use of a semi-log functional form with respect to the population variable as well.

3.2.1 Details of the instrumental variables (IV) approach

[16] See, e.g. Studenmund (2016), for a thorough discussion of why this semi-log form is appropriate for situations in which a one-unit increase in the independent variable increases the dependent variable at a decreasing rate.

To mitigate issues of reverse causality running from terrorism to trade, I use the existing literature to help identify valid instruments for trade in a model of terrorism. To begin, the validity of my IV approach can, first, be gleaned by noticing that same currency, distance, and colonial history variables are not expected to be weak instruments for trade (i.e. they satisfy the relevance condition). As a matter of fact, efforts within the gravity model of bilateral trade literature not only consistently show that trade between two countries rises whenever they share a currency (see, e.g. Rose, 2000) or a colonial past (see, e.g., Anderson and Marcouiller, 2002), but the idea that bilateral trade decreases with distance is one of the most well-established empirical results in international economics.[17] Additionally, Frankel and Romer (1999) identified multiple, non-weak instruments for aggregate trade, including an aggregate distance variable, in order to mitigate the reverse causal relationship running from income to trade. By extension, it stands to reason that the total amount of "same currency" relationships (i.e. currency unions) of a country in addition to its degree of remoteness from or colonial history with large-income countries in the current year should (in tandem) have explanatory power for aggregate trade. More specifically, the "same currency" and "colony" variables should each have a positive effect on aggregate trade, whereas the remoteness variable should have a negative effect on trade.

In addition to this, total currency unions along with remoteness and colonial past variables must neither be impacted by terrorism nor by other factors, such as political rights, that affect terrorism. And, while one cannot be absolutely certain that this identifying assumption holds, it would seem to be a rather difficult task to try and come up with a compelling story that

[17] Referring to a particularly influential meta-analysis study, Disdier and Head (2008) examines the 1467 distance estimates from 103 papers to offer systematic evidence of distance having a significant and persistently high negative impact on trade, "even after controlling for many important differences in samples and methods" (p. 37).

suggests how (say) a terrorist attack would have an important impact on a country's amount of currency unions or on its "remoteness" or "colony" index scores. Alternatively, to the extent that a country's number of currency unions, degree of remoteness, or colonial past are meaningfully correlated with terrorism, it is much easier to argue that this will occur through the (benign) channel of aggregate trade. As such, country-level versions of the "same currency" and "colony (ever)" dummies along with bilateral distance are (in tandem) expected to be valid instruments for aggregate trade in a model of terrorism,[18] which would mitigate the reverse (and negative) causal process running from terrorism to trade. Accordingly, the first stage results of the instrumental variables two-stage least squares (IV 2sls) regression (i.e. a model in which two measures are used to instrument for aggregate imports) are based on the following equation:[19]

$$\ln(M_{i,t-1}) = \mu + \pi_1 Currency_{i,t-2} + \pi_2 Remote_{i,t-1} + \pi_3 Colony_{i,t-1} + \pi_4 Pop_{i,t} + \pi_5 IT_{i,t-1}$$
$$+ \pi_6 LoPR_{i,t} + \pi_7 LoPR^2_{i,t} + \pi_8 Lang_i + \pi_9 Ethnic_i + \pi_{10} Relig_i + \pi_{11} Area_i$$
$$+ \pi_{12} Elev_i + \pi_{13} \%Trop_i + \pi_{14} Region_{i,t} + v_{i,t} \quad (2)$$

where $Currency_{i,t-2}$ reflects the number of countries with which country *i* shares a currency among the other countries of the world in year *t-2* and is specifically equal to:

$$Currency_{i,t-2} = \sum_{i \neq j} SmCur_{i,j,t-2}$$

[18] A discussion of the aggregation process for the two instrumental variables are discussed further in Section 3.1, specifically under the "Instrumental Variables for Aggregate Imports" heading.

[19] While equation (2) is an empirical model of trade, it hardly reflects the standard gravity model of trade. In essence, equation (2) represents a typical first stage regression of an IV 2sls model. More specifically, the dependent variable of equation (2) is the variable to be instrumented for (i.e. aggregate imports) and the list of control variables include: the two instruments for aggregate trade in addition to all of the control variables from equation (1), which includes the lagged dependent variable and excludes the measure being instrumented for.

$Remote_{i,t-1}$ and $Colony_{i,t-1}$ are indices that respectively reflect the extent to which a country is remote to or shares a colonial history with large-income countries in year *t-1*;[20] the remote and colony indices are respectively equal to the min-max normalized version of the following:

$$Remote_{i,t-1} = \sum_{i \neq j} w_{j,t-1} \, ln(dist_{i,j})$$

$$Colony_{i,t-1} = \sum_{i \neq j} w_{j,t-1} ColEver_{i,j}$$

$dist_{i,j}$ represents the simple distance between country *i* and country *j*; $ColEver_{i,j}$ is a dummy of whether countries *i* and *j* have ever engaged in a colonial relationship; $w_{j,t-1}$ is the "GDP weight" placed on country *j* (i.e. each possible trading partner of country *i*) in year *t-1* and is equal to:

$$w_{j,t-1} = \frac{GDP_{j,t-1}}{\sum_{j \neq k} GDP_{k,t-1}}$$

$GDP_{j,t-1}$ is the Real GDP of country *j* in year *t-1*; μ is a constant term; π is a coefficient; v is the error term. Additionally, I follow the gravity model of trade literature in that the trade/distance variables enter equation (2) logarithmically but the currency/colony variables do not. In brief, equations (2) and (1) respectively represent the first and second stages of the IV 2sls regression.

4. Empirical Results

Table 2.1 first reports results based on a model that is comparable to the existing literature. In particular, specification (1) includes all the explanatory variables included in the empirical model of Abadie (2006). Abadie (2006), though, primarily uses Real GDP per capita

[20] The lagged remote/colony index scores are used since they represent instruments for a lagged measure of trade.

as the economic control variable, which necessarily precludes the additional use of other economic variables such as trade from his model. As such, specification (1) facilitates direct comparison with results reported in the existing literature based on empirical models that do not instead account for the effects that international trade could have on terrorism. Like Abadie (2006), I find that the lack of political rights variable has a statistically significant, non-linear, and positive effect on terrorism in addition to finding linguistic fractionalization (or, alternatively, linguistic diversity) to have a positive effect on terrorism. Also, like Abadie (2006), I find no significant relationship between GDP per capita and terrorism. This particular result is noteworthy because it suggests that GDP per capita has no effect on a country's impact of terrorism score.

Due to data limitations, Abadie (2006) uses cross-sectional data whereas I use panel data, which motivates the additional use of a lagged dependent variable in the remaining specifications. Moreover, this approach reflects an attempt to mitigate the bias created by the omission of unobserved factors. For instance, while I control for other factors that are hypothesized to be relevant for explaining terrorism, the use of a lagged dependent variable helps ensure that the results are not misleading, which is especially important given the miniscule amount of research that has been devoted to developing an empirical model of an index of overall terrorism. As reported in Table 2.1, the coefficient on the lagged dependent variable is positive, statistically significant at the 1% level, and between .92 and .95 in magnitude for specifications (2) through (6). So, considering that the effect of the lagged dependent variable on terrorism is both positive and very strong, the inclusion of this variable appears to indeed do much of the work in controlling for the myriad unobserved factors that I fail to account for with the other explanatory variables.

Table 2.1 - OLS and IV 2sls of aggregate imports on terrorism
(using 2001-2017 data)

Dependent Variable: Impact of terrorism [t]						
	(1)	(2)	(3)	(4)	(5)	(6)
(Specification Notes)	(OLS)	(OLS)	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])
Trade proxies:						
ln(Real aggregate imports [t-1])			**-.0126**	**.1601****	**.0348*****	**.1667*****
			(.0138)	**(.0756)**	**(.0114)**	**(.0624)**
Economic variable:						
Real GDP per capita [t]	-.0448	-.0033				
	(.0321)	(.0138)				
Other explanatory variables:						
ln(Population, total) [t]	.7928***	.0785***	.0882***	-.0373		
	(.0274)	(.0143)	(.0173)	(.0567)		
Lagged dependent variable [t-1]		.9217***	.9213***	.9283***	.9410***	.9144***
		(.0092)	(.0092)	(.0098)	(.0076)	(.0151)
Lack of political rights [t]	.3343***	.0435	.0351	.1923**	.0894**	.1995***
	(.1040)	(.0397)	(.0385)	(.0786)	(.0392)	(.0656)
Lack of political rights (squared) [t]	-.0308**	-.0039	-.0031	-.0176**	-.0090*	-.0184***
	(.0127)	(.0051)	(.0050)	(.0081)	(.0050)	(.0068)
Language fractionalization	1.510***	.1063	.1060	.1390	.1203	.1316
	(.2184)	(.0911)	(.0910)	(.0939)	(.0910)	(.0931)
Ethnic fractionalization	-.7107***	-.0364	-.0463	.1146	-.0180	.1274
	(.2488)	(.1008)	(.1018)	(.1312)	(.1006)	(.1308)
Religious fractionalization	-.4750**	.0060	-.0187	-.1836	-.0381	-.1869*
	(.1819)	(.0761)	(.0770)	(.1165)	(.0769)	(.1050)
Country area	.0080***	.0002	.0002	-.0008	.0012	-.0017
	(.0025)	(.0008)	(.0008)	(.0009)	(.0008)	(.0016)
Elevation differences	.1625***	-.0033	-.0045	.0185	-.0081	.0247
	(.0287)	(.0129)	(.0128)	(.0148)	(.0129)	(.0179)
% Tropical area	.0055***	-.0005	-.0006	.0000	-.0004	.0002
	(.0013)	(.0006)	(.0006)	(.0006)	(.0006)	(.0006)
Kleibergen-Paap rk Wald F statistic				**38.852**		**38.774**
Critical value of IV relative bias				13.91		13.91
Critical value of IV size				22.30		22.30
[P-value (↓)]	-	-	-	**[.5092]**	-	**[.7306]**
Hansen J statistic	-	-	-	1.350	-	.628
Regional-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of regional-year Groups	153	144	144	144	144	144
Number of observations	2,489	2,344	2,343	2,343	2,349	2,349

[1]: Real aggregate imports [t-1] is instrumented by $Currency_{i,t-2}$ and $Remote_{i,t-1}$ and $Colony_{i,t-1}$

Notes: Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. The critical value of both the IV relative bias and IV size are provided by Stock and Yogo (2005) and are respectively based on a 5% maximal IV relative bias and 10% maximal IV size. The Regional groups are derived from the nine regional categories of the Institute of Economics and Peace's (2018) Global Terrorism Index: South Asia, North America, Middle East and North Africa, Sub-Saharan Africa, South America, Asia-Pacific, Russia and Eurasia, Europe, Central America and the Caribbean. In each "lagged-dependent variable" specification, 144 regional-year dummies (9 regions times the 16 years of available data) are controlled for in each model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Compared with specification (1), specification (2) is the same except for the additional inclusion of the lagged dependent variable. Specification (2) shows that all of the control variables, besides the lagged dependent variable and population size, fail to have a statistically significant effect on terrorism. So, using OLS, neither the lack of political rights measure nor Real GDP per capita appear to have explanatory power for terrorism.

Beyond specifications (1) and (2), the primary function of Table 2.1 is to contrast the OLS and IV 2sls estimation results for the empirical model described in the previous section. To this end, specification (3) is based on OLS and specification (4) is based on an overidentified IV 2sls model.[21] Similarly, but with the important exception of the population measure being excluded, specifications (5) and (6) are respectively based on an OLS and an overidentified IV 2sls model. But, before discussing the IV 2sls results, it is instructive to first discuss the OLS results. Like specification (2), specification (3) indicates that the coefficient on the economic variable (here, trade) has a negative sign and is statistically insignificant. For purposes of interpretation, it is important to note that the control variable of interest enters logarithmically but the dependent variable does not. In this regard, the OLS result in specification (3), though dubious, indicates that a 1% increase in aggregate imports for a country *decreases* its impact of terrorism score by .0126/100=.000126. Alternatively, specification (5) demonstrates that the exclusion of a population control variable matters greatly. Not only does the OLS result in specification (5) indicate that a 1% increase in aggregate imports for a country *increases* its impact of terrorism score by .0348/100=.000348, but the coefficient on trade becomes statistically significant at the 1% level. Of course, Table A.5 reveals that the correlation

[21] By "overidentified," I mean that, in the IV 2sls model under consideration, at least two measures are used to instrument for the (one) endogenous regressor.

coefficient between the population measure and aggregate imports is nearly .48, suggesting that one cannot rule out the possibility that the omission of population is driving the results on the trade coefficient in specification (5).

To set the stage for a discussion of the IV 2sls results, consider that the inclusion of a lagged dependent variable appears to substantially mitigate the issue of omitted variables. Consequently, the impact of reverse causality problems can conceivably be evaluated by comparing the ordinary least squares (OLS) results to that of instrumental variables two-stage least squares (IV 2sls). Crucially, however, reliable distinctions can only be made between OLS and IV 2sls if the instrumental variables are valid (i.e. both the relevance and exogeneity conditions are satisfied). First, because I use heteroskedasticity-robust standard errors in the IV 2sls model, I primarily test the relevance condition for my instrumental variables by examining the Kleibergen-Paap Wald rk F statistic. Using the critical values provided by Stock and Yogo (2005), I then compare this statistic to that of the two critical values for both the 5% maximal IV relative bias and the 10% maximal IV size.[22] Essentially, the magnitude of the Kleibergen-Paap Wald rk F statistic must exceed both of the relevant critical values in order for me to confidently reject the null hypothesis that my set of instruments is weak. And, encouragingly, the Kleibergen-Paap Wald rk F statistic in specifications (4) and (6) are above 38, which easily eclipses the two relevant critical values. Hence, these results suggest not only that the bias for the IV trade coefficient is less than 5% of OLS, but also that the size distortion of the rejection region on the IV trade coefficient is not excessively large. Similarly, Table 2.2 shows that, when population is omitted from the model, all three of the instruments for trade are statistically

[22] The critical value associated with the lowest-available percentage for the "maximal IV relative bias" and "maximal IV size" is 5% and 10% respectively. Incidentally, generating the "maximal IV relative bias" critical value is contingent upon the use of at least three instrumental variables for the (one) endogenous regressor in the model.

significant at the 1% level in addition to displaying a reasonable sign,[23] which further underscores confidence that my set of three instrumental variables satisfy the relevance condition. Secondly, the Hansen J statistic p-value must minimally exceed that of .05, whereby my confidence in the exogeneity condition being satisfied would seem to rise with the size of this p-value. As shown in Table 2.1, my three instrumental variables easily pass the test of exogeneity in specifications (4) and (6), yielding a Hansen J statistic p-value above .5. The relevance and exogeneity test results taken together suggest that my set of three instrumental variables do, indeed, serve as valid instruments for aggregate imports in a model of terrorism. Consequently, the IV 2sls results sufficiently account for issues of reverse causality whereas the OLS results, in contrast, are subject to simultaneity bias.

Given that the overidentified IV 2sls model is, *ceteris paribus*, reasonably more credible than that of OLS, I can now discuss the IV 2sls results. First, specification (4) in Table 2.1 indicates that a 1% increase in a country's level of aggregate imports *increases* their impact of terrorism score by .1601/100=.001601, which actually changes the sign from ambiguous to positive compared with the OLS trade coefficient in specification (3). The IV 2sls trade coefficient in specification (4) is also statistically significant at the 5% level, where the positive effect of population size on SHI from specification (3) is nullified. This last result of specification (3) suggests that this population measure serves primarily as an intervening variable between the currency/remoteness/colony variables and trade.[24] Second, specification (6) indicates that a 1% increase in a country's level of aggregate imports *increases* their impact of terrorism score by .1667/100=.001667, which is more than four times stronger than what is

[23] See section 3, specifically under the "Details of the instrumental variables (IV) approach" heading, for a possible explanation of each of the realized signs on the instrumental variable coefficients that are reported in Table 2.
[24] In other words, an increase in a country's currency/remoteness/colony variables is likely to increase the size of their population and, in turn, the amount of imports that are demanded (or amount of exports that are produced).

indicated by the OLS trade coefficient in specification (5). Both the OLS and IV 2sls trade coefficients in specification (5) and (6) are also statistically significant at the 1% level. As

Table 2.2 - First stages of IV 2sls regressions from Table 2.1
(using 2001-2017 data)

Dependent variable: Natural log of Real aggregate imports [t-1]		
	(1)	(2)
(Specification Notes)	(OLS)	(OLS)
Instrumental Variables:		
Number of "same currency" relationships [t-2]	**.0321*** **	**.0330*** **
	(.0032)	**(.0043)**
Remoteness index [t-1]	**-.0082**	**-.0933*** **
	(.0224)	**(.0326)**
Colony index [t-1]	**.0102**	**.0434*** **
	(.0100)	**(.0144)**
Other explanatory variables:		
ln(Population, total) [t]	.7184***	
	(.0204)	
Impact of terrorism [t-1]	-.0446***	.1841***
	(.0134)	(.0153)
Lack of political rights [t]	-.7916***	-.6955***
	(.0718)	(.0823)
Lack of political rights (squared) [t]	.0701***	.0558***
	(.0088)	(.0101)
Language fractionalization	-.3015***	-.1800
	(.1001)	(.1324)
Ethnic fractionalization	-.8350***	-1.004***
	(.1217)	(.1477)
Religious fractionalization	1.253***	1.260***
	(.1043)	(.1478)
Country area	.0063***	.0226***
	(.0010)	(.0014)
Elevation differences	-.1219***	-.2189***
	(.0199)	(.0237)
% Tropical area	-.0040***	-.0056***
	(.0008)	(.0011)
Regional-year fixed effects	Yes	Yes
Number of regional-year Groups	144	144
Number of observations	2,343	2,349

Notes: Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. The Regional groups are derived from the nine regional categories of the Institute of Economics and Peace's (2018) Global Terrorism Index: South Asia; North America; Middle East and North Africa; Sub-Saharan Africa; South America; Asia-Pacific; Russia and Eurasia; Europe; Central America and the Caribbean. In each specification, 144 regional-year dummies (9 regions times the 16 years of available data) are controlled for in each model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

suggested earlier, the present chapter's results provide reason to think that the OLS model will suffer from issues of reverse causality, where the OLS coefficient on trade is uniformly biased down.[25] Hence, the failure to account for reverse causality not only leads to an underestimation of the strength of the positive association between trade and terrorism but it also could lead researchers to wrongly conclude that measures of trade have either no effect on overall terrorism (e.g. Burgoon, 2006; Bravo and Diaz, 2006) or a negative effect on overall terrorism (e.g. Freytag et al., 2011).

5. Conclusion

A compelling theoretical channel through which international trade can increase the level of terrorist activity in a country is what Anderson (2015) calls the "bang for the buck" effect of attacking large-sized economies. For instance, the benefits of launching a terrorist attack inside a country are expected to grow with the size of that country's market, in large part, because the successful undermining of larger (as opposed to smaller) markets confers greater visibility for terrorist organizations. Thus, a proportionate terrorist attack against a larger market will more effectively induce the desired political or social goals of terrorist organizations than would an attack against a smaller market. However, to date, there is no empirical documentation of a positive association between international trade and overall (both domestic and transnational) terrorism. This chapter addresses this lacuna, both by replacing GDP per capita with aggregate trade in an existing model of an index of terrorism and by accounting for the reverse causal relationship running from terrorism to aggregate trade with instrumental variables. The panel

[25] See Tables A.3 and A.4 for further empirical evidence that the OLS trade coefficient is biased down, where this result appears to hold when aggregate imports are replaced with a different proxy for trade, including for aggregate exports and more dubiously for the "Trade (% of GDP)" measure.

data analysis using 161 countries over 17 total years (2001-2017) yielded two-stage least squares estimates of the effects of international trade on terrorism that are statistically significant, positive, and stronger than OLS. As such, this chapter empirically validates the relative strength of the "bang for the buck" effect. More indirectly, this chapter also suggests that existing attempts within the terrorism-to-trade empirical literature that fail to account for the issue of reverse causality are understating the true negative impact of terrorism on trade.

With this chapter's results coupled with the theoretical conclusions of the trade-to-terrorism literature in mind, trade would seem to affect terrorism primarily through trade's close association with the size of a country's economy. However, the issue could easily run much deeper than this. In particular, it could be argued that various economic conditions plausibly serve as transmission channels from trade to terrorism. For instance, Li and Schaub (2004) found globalization to have no direct effect on terrorism but instead to have an indirect and negative effect on terrorism through its impact on either economic development or economic growth. Relatedly, Freytag et al. (2011) use a standard opportunity cost of violence argument to suggest that, insofar as trade improves the socio-economic environment of a country, trade should indirectly reduce terrorist activity. These economic transmission channels, in their current form, fail to cohere with this chapter's primary finding that trade has a positive effect on terrorism. Of course, if trade instead worsens a country's socio-economic environment and/or reduces its overall level of economic development or economic growth,[26] then these economic transmission channels would be consistent with this chapter's results. Although much attention has been paid to research questions such as these, the present chapter potentially motivates a

[26] See, e.g., Rodriguez and Rodrik (2000) in which they specifically cast doubt on the empirical studies (specifically Dollar, 1992; Sachs and Warner, 1995; Edwards, 1992) that suggest globalization, on balance, reduces poverty.

deeper investigation related to establishing the true direction of the relationship running from trade to other economic variables.

Furthermore, novel avenues for future research exist, namely, to determine whether certain social factors, specifically religiosity, constitute a transmission channel from trade to terrorism. A useful starting point to motivate the plausibility of the "religiosity" transmission channel is Carvalho and Koyama (2016), who study a setting wherein religious club members choose between time and money contributions to the group. Basically, a religious club member that supplies their labor within a low-wage environment implies that the opportunity cost of their time contributions to the religious club is relatively small whereas the opportunity cost of their money contributions to the religious club is relatively large. Hence, it stands to reason that religious members who experience a reduction in their real wages would prefer to make time (as opposed to money) contributions to the religious club good. So, insofar as groups that lose from trade experience a reduction in their real wages, such groups *ceteris paribus* are more likely to pledge their time rather than their money to the religious group.

To connect religious time contributions to terrorism, it is important to first note that higher levels of time contributions are associated with higher levels of commitment to the group. For this reason, trade can end up improving the productive capacity of terrorist organizations. After all, terrorism is a highly illegal activity that brings forth the wrath of the authorities if perpetrators are caught or identified. As such, the opportunity cost of providing one's services to a terrorist organization is exceedingly large. Given this, it is critical for terrorist organizations to control defection by both ingratiating themselves to the surrounding community and building a coalition of highly committed members. And, on both of these fronts, terrorist organizations are made better off when there is an uptick in religiosity. More specifically, in addition to the

further cover with which the surrounding community provides terrorist organizations, an uptick in religiosity also enables terrorist organizations to impose even stricter behavioral restrictions on members, which further improves their ability to identify the most highly committed members.

To elaborate on the last two points, consider Laurence Iannaccone's (1992) seminal rational-choice framework which conceptualized religious groups as robust social clubs. In effect, religious groups, like social clubs, inherently suffer from a free-rider problem whereby they have an incentive to screen out free-riders by requiring members to behave in a manner that is inconsistent with normal behavior. Although this mandate raises the cost of membership, it better ensures that only the most committed members will remain in the group, which ultimately produces a more satisfying overall experience for group members. Hence, a strategy of imposing large behavioral restrictions on members, known as "sacrifice and stigma," can be rendered effective due to its ability to build a more highly committed group. In this way, larger membership costs appear to be associated with higher levels of commitment to the group, which explains why especially strict religious groups have been and continue to be responsible for the most lethal terrorist attacks.[27] Basically, it stands to reason that, in response to greater religiosity, members of especially strict religious groups will now be required to send an even stronger signal of their commitment to the group in order to successfully differentiate themselves from less-committed types. So, to the extent that these strict religious groups wish to engage in terrorism, these groups will be in an even better position to launch a terrorist attack as a result of an uptick in religiosity.

[27] For more details, the following studies investigate the association between commitment and terrorism: Iannaccone and Berman (2006), Berman and Laitin (2008), Raynold (2014, 2017, 2018), and Morales et al. (2018).

In any case, the point of the above discussion is to demonstrate why the study of religiosity, as a possible transmission channel from trade to terrorism, is deserving of the research community's attention. The next chapter is motivated in part by this call to action.[28]

[28] Chapter 3 implicitly investigates this "religiosity" transmission channel from trade to terrorism by analyzing the causal link from trade to religious hostilities.

CHAPTER 3: TRADE-TO-RELIGIOUS HOSTILITIES

1. Introduction

Does economic globalization or, specifically, increased integration of world trade play an important role in the perpetration of social hostilities involving religion? Taking a political psychology approach, Kinnvall (2004) prominently suggests that individual communities suffer from economic globalization in that it causes them to lose an identity of which they were once proud. Thus, communities compensate for their loss by increasing their identification with fundamental characteristics, namely religion. Relatedly, Carvalho and Koyama (2016) use an economic approach to suggest that religious leaders in low-wage environments demand substantial time contributions out of their members. Hence, if economic globalization has the capacity to create low-wage environments in a trading country, then economic globalization is expected to amplify religion's importance for domestic communities negatively affected by trade. Moreover, while differences in race, country, or socioeconomic status seemingly only separate individuals for life, religion has the exceptional capacity to separate individuals for eternity. This transcendent quality of religion, Kinnvall (2004) argues, is why greater religious identification is particularly likely to engender "intolerance against those who do not share in these beliefs" (p. 759). In effect, economic globalization should have explanatory power for religious intensity and, thereby, religious hostilities. While this research question has been explored in a theoretical context, it remains an untapped empirical question at the country-

level.[29] Indeed, to the best of my knowledge, this present chapter is the first of its kind to investigate the existence of a causal relationship running from international trade to social hostilities involving religion in addition to its direction at the country-level.

To date, only a couple of papers have developed an empirical model of social hostilities involving religion at the country-level, namely Grim and Finke (2007) and Somasundram et al. (2017). Both papers use Real GDP per capita as their only economic control variable, finding no association between economic strength and an index of social hostilities involving religion (SHI).[30] Yet, one could reasonably argue that international trade is a superior economic measure to control for because, unlike Real GDP per capita, it additionally reflects the degree to which certain communities can blame out-groups for their plight which, in turn, has a positive effect on SHI. In this regard, international trade is an economic variable which is especially likely to exhibit a positive association with SHI. Additional steps must be taken, however, to establish causality, because there remains an issue of reverse causation running from SHI to population size to trade. For instance, greater religious hostilities can lead to population migration, which, in turn, reduces a country's demand for merchandise imports. Consequently, I identify a set of three time-varying instruments for trade that appear to be valid in a cross-country model of SHI.

The present chapter builds on the existing literature to develop an empirical model of social hostilities involving religion that accounts for issues of reverse causality. As suggested above, my model departs from the existing literature in two distinct ways. First, I take an instrumental variables approach and, second, I use aggregate trade instead of Real GDP. The

[29] For a discussion of the region-specific empirical evidence, see Jha (2018), who links trade to conflict theoretically and substantiates this link with detailed case studies and summaries of historical and contemporary patterns of violence and tolerance in South Asia, Latin America, and other regions.

[30] I refer to social hostilities involving religion as "SHI" to remain consistent with the acronym chosen by my data source (Pew Research Center, 2018), which annually provides the public with country-level data on SHI.

central finding is that the two-stage least squares estimates of the effect of aggregate imports on future SHI in the destination country are significant, positive, and stronger than OLS. This finding provides empirical support not only for the notion that there exists a causal relationship running from trade to SHI, but also for the hypothesis that the effect of trade on SHI is positive.

This chapter proceeds as follows: Section 2 discusses the relevant theoretical and empirical literatures; Section 3 describes the data and empirical model; Section 4 discusses the results; and Section 5 draws the main conclusions and discusses some potentially fruitful avenues for future research.

2. The Link from Trade to Religious Identities and Conflict

While globalization is hardly a new phenomenon, the scale and speed of disruption that has resulted from it and the subsequent vigor with which working-class communities have pushed back against globalization is striking. Since the 1990's, the world has enjoyed tremendous improvements in both technology and the way in which information is spread. While these characteristics have clearly improved the lives of many by making it easier and cheaper to exchange goods and ideas throughout the world, they have also led to some wholesale changes in the way that the economy and society are structured. These changes serve not only as a threat to traditional structures, but also as a threat to those who once benefitted from these structures as globalization can decimate a person's ability to identify with an economic or societal role of which they were once proud. And, it is this sense of loss—of losing a part of oneself that once provided meaning (e.g. job, economic status, or other privileges)—that explains why globalization, for many, prompts an identity restructuring. In this regard, the identity

construction process provides an organizing principle that bridges the causal relationship running from globalization to religiosity and, by extension, religious hostilities.

Previous attempts to link globalization with religion through an identity construction framework typically follow Kinnvall's (2004) political psychology theory of the global-local nexus and ontological insecurity. In this seminal article, she theorizes that, for many, globalization is synonymous with "intensified levels of insecurity as the life they once led is being contested and changed at the same time" (p. 742). Those affected by the destabilizing forces of globalization seek, above all else, safety from this onslaught of existential uncertainty. Comparatively, Giddens (1991) draws from Erikson's (1950) work to argue that a necessary condition for psychological wholeness is placing trust in other people—not necessarily trusting *all* people but trusting *enough* people to successfully weather whatever emotional storm they might face. This need to trust other people, Kinnvall (2004) argues, propels individuals to redefine themselves in a way that makes them feel more secure with their circumstances. Because of its prominence and exceptional "ability to convey a picture of security, stability, and simple answers," religion is a particularly likely identity construction to arise in response to globalization (p. 742). But, by placing welcomed structure on the problem of overcoming existential uncertainty, religion "provides a foundation for the creation of intolerance against those who do not share in these beliefs" (p. 759). Hence, religious identification is expected to have a positive impact on religious hostilities. In light of the foregoing, economic globalization is theorized to have a positive impact on social hostilities involving religion through an "identity construction" channel.

While the political psychology literature can be credited with linking globalization to religious hostilities most directly, the economics of religion literature has linked negative

economic shocks to religiosity. An influential paper to study this issue empirically was Chen (2010), which investigates the religious implications of the 1998 Indonesian financial crisis. This crisis was specifically characterized by massive and widespread inflation whereby those who were hit hardest by the crisis were more likely to enroll themselves and/or their children into Quran-related coursework. This increased interest in receiving an Islamic education even occurred despite its low educational returns and its rising tuition cost. Basically, in the aftermath of the 1998 Indonesian financial crisis, a heightened level of economic distress was associated with a more frequent engagement in religious activities. And, because this association was substantially weakened in places where credit was readily available, Chen (2010) suggested that this association seemed to be most consistent with a social insurance framework. To explain, an important function of religious institutions is to provide credit, when credit would otherwise be unavailable, which allows them to facilitate consumption smoothing across and within households. So, especially during a crisis, the less religious households have an incentive to increase their religious participation in order to dampen the effect of a negative consumption shock. Regarding non-crisis periods, Ferrara and Testa (2020) suggest that a social insurance framework can link economic factors to religiosity when negative economic shocks are merely anticipated. By noticing that oil-rich regions in certain areas of Texas experience substantial economic volatility, they find empirical validation for the social insurance framework by finding a "strong and persistent relationship between oil discoveries and religious participation" (p. 30).

Relatedly, and building on Berman's (2000) theory of Ultra-Orthodox men living in Israel, Carvalho and Koyama (2016) explore how a group's members choose between time and money contributions to the group. With this extension, Carvalho and Koyama (2016) can explain why Jewish emancipation in various nations of Europe during the nineteenth century led

to the emergence of both a relatively strict religious group (Ultra-Orthodox Judaism) in the East and a relatively liberal religious movement in Germany. Specifically, during the nineteenth-century, Germany paid (on average) much higher wages to their workers compared to Eastern Europe, and the respective wage environment seemingly influenced how the Jewish leadership fostered commitment out of their members. On the one hand, the low-wage environment of Eastern Europe meant that the opportunity cost of time contributions to a religious club good was relatively small. Hence, because of the standard substitution effect, the Jewish leadership had an incentive to increase the cost of outside activity for their members by imposing stigmatizing rules on them, which would increase the likelihood that their members' relatively cheap resource (time) would be used to contribute to the religious club good as opposed to outside activities. On the other hand, the high-wage environment of Germany meant that the opportunity cost of time contributions to a religious club good was relatively large. Hence, the Jewish leadership had an incentive to focus on extracting money contributions from their members (as opposed to time contributions), which is why they chose to decrease the cost of outside activity (e.g. by relaxing restrictions on behavior). This way, the Jewish leadership of Germany focused on expanding their base of support to exploit the high wages for both members and potential members alike.

Considering that the destabilizing effects of globalization can, among other things, produce negative economic shocks for religious communities, the economics of religion literature describes two mechanisms by which economic globalization can lead to greater religiosity and, thus, greater religious hostilities. Using the social insurance framework, globalization increases economic uncertainty in the lives of many domestic communities, which leads to greater religiosity (e.g. church attendance) and, ultimately, greater religious hostilities. Similarly, Carvalho and Koyama's (2016) framework suggests that, to the extent that trade

reduces the outside economic opportunities for certain religious members, trade will reduce the opportunity cost of time contributions to the religious club good, thereby making it more likely that religious leaders will impose stricter behavioral requirements on their members. These stigmatizing policies can, then, increase religious intolerance against non-members as abiding religious members become more emboldened in the truth of their religious ideology while also increasing their degree of separation between themselves and the rest of society. Consequently, the foregoing suggests that, in theory, globalization can have a positive impact on religious hostilities through either a "social insurance" or "outside economic opportunities" channel.

Despite economic variables such as international trade plausibly having explanatory power for religious hostilities, no study has examined this empirical relationship at the country-level, thereby leaving a clear void in the literature that this chapter aims to fill. Nonetheless, there does exist a handful of studies which, in combination, provide a useful context for understanding the present chapter's data and empirical model. To this end, the most noteworthy empirical studies are discussed for the remainder of this section.

The first study of relevance is Huntington (1996), which includes one of the earliest and most prominent theories within the social conflict literature, namely the "Clash of Civilizations" thesis. He argues that conflict will result from civilizational divide or, most importantly, from the different religions that have shaped these civilizations. More precisely, because countries are generally built atop different religions and because countries can clash at two distinct levels (either within or across countries), Huntington reasoned that countries will be riddled with conflict, both domestically and abroad. Effectively, Huntington embeds two central claims in his "Clash of Civilizations" thesis: 1) countries consisting of a large number of civilizations *ceteris*

paribus are more likely to suffer from a greater incidence of religious conflict[31] and 2) given the number of civilizations, the more religiously homogeneous countries *ceteris paribus* will enjoy a lower incidence of religious conflict. Both claims are subject to empirical testing assuming, of course, that the necessary data can be obtained. However, due to a lack of religious data prior to 2000, Huntington (1996) is noteworthy because it establishes the inclusion of religious homogeneity and/or civilizational divide measures in empirical models of social conflict.

In the wake of religious data becoming more available, Grim and Finke (2007) used data from the Association of Religious Data Archives (ARDA) to develop a cross-sectional model of an index of social regulation on religion,[32] which is essentially a model of social hostilities involving religion (SHI).[33] Furthermore, the control variables of this model include economic strength, civilizational divide, government regulations on religion, percent Christian, and percent Muslim. For instance, Christian and Islamic ideologies not only have the most subscribers around the world, but their adherents, at times, have been attributed with being particularly intolerant against those who do not share their beliefs. Hence, overwhelmingly Christian or Muslim countries could *ceteris paribus* be more likely to experience consistently high levels of SHI. Ultimately, Grim and Finke (2007) find civilization divide and percent Muslim measures to have a direct, positive and statistically significant effect on SHI, whereas measures of economic strength, government restrictions on religion, and percent Christian had no direct effect on SHI.

Building on Grim and Finke's (2007) model of SHI, Somasundram et al. (2017) took

[31] In his book, Huntington (1996) divides the world into major civilizations. Each major civilization is distinguished by the number of religions by which it was originally shaped, and each country is said to belong to one of these major civilizations.

[32] Grim and Finke (2007) define social regulation on religion as the "restrictions placed on the practice, profession, or selection of religion by other religious groups, associations, or the culture at large" (p. 645).

[33] See, e.g., Somasundram et al. (2017) which uses the Pew Research Center's index of social hostilities involving religion and ARDA' (2001, 2003, 2005) index of social regulation on religion interchangeably.

advantage of the emergence of more recent (and comparable) annual measures of societal and governmental restrictions on religion, which first became available for 2007, thanks to the survey efforts of the Pew Research Center.[34,35] In this way, their study is the first to develop a panel-data framework of SHI. More specifically, they use data on 45 European countries for 2001, 2003, 2005, and 2007-2011 and use various religious, economic, and democratic factors in addition to an index of government restrictions on religion (GRI) as control variables. What's more, the decision to include both GRI and a measure of democracy reflects an attempt to address an unintended problem that arises in constructing a model of SHI. Namely, the absence of social hostilities involving religion (SHI) could simply be a result of repressive government actions (e.g. the Soviet Union under communist rule). After all, the more repressive a country is, the less competition there will be between religious groups, which not only lessens overall religious tensions, but the visibility of these tensions is also expected to fall as the costs of public expression rise. Consequently, countries which exhibit a low SHI score reasonably will be observed, not because there is an absence of religious intolerance in said country, but because the government is either highly intolerant of certain religious ideologies or willing and effective at squelching general forms of public expression. This, in part, explains why it is important to control for an index of government restrictions on religion (GRI) as well as for a measure of democracy in a model of SHI.

In the end, Somasundram et al. (2017) find no evidence in support of measures of economic strength, religious homogeneity, or the prevalence of Christianity having explanatory

[34] In each year between 2007 and 2016, the Pew Research Center (2018) provides data for two indices, specifically social hostilities involving religion and government restrictions on religion, both of which are updated versions of the ARDA' (2001, 2003, 2005) social and government regulation of religion measures respectively.
[35] The relevant Pew Research Center (2018) and ARDA' (2001, 2003, 2005) indices are comparable in that they both reflect the extent to which religious restrictions are brought on by either *society* or the *government*.

power for SHI. By contrast, they find evidence in favor of the GRI, the lagged dependent variable, the level of democracy, and the percentage of Muslims in a country being significant and positive predictors of SHI. Notably, a primary focus of Somasundram et al. (2017) is to test the hypothesis that GRI has a positive effect on SHI, where they do provide evidence in support of this claim. And although economic strength is found to have no effect on SHI, the apparent reverse (and uniformly negative) causality running from SHI to population size to economic strength is completely unaccounted for. As such, an important contribution of the present chapter is to account for the negative impact that SHI is expected to have on the economy (through the "population migration" channel) by using trade as the economic strength variable in addition to using an instrumental variables approach with respect to trade. This key difference in our approaches notwithstanding, Somasundram et al. (2017) establishes a coherent panel-data framework for examining the determinants of SHI at the country-level, which is why I specifically build on their primary OLS specification below.

3. Empirical Methodology

To measure the effects of international trade on the incidence of social hostilities involving religion (SHI) and to maintain continuity with Somasundram et al. (2017), I use country-level data in 2001, 2003, 2005, and 2007-2016 within a panel regression framework where SHI is the dependent variable and the list of explanatory variables includes one proxy for trade, primarily aggregate imports.[36] The evaluation of the unidirectional effect of trade on SHI through OLS is deemed problematic due to the reverse, negative impact that SHI is expected to have on population size and, in turn, trade. As such, an important contribution of this chapter is

[36] Data in 2002, 2004, and 2006 are not analyzed due to the unavailability of SHI and GRI data for those years.

to address the apparent simultaneity bias on trade through an instrumental variables (IV) approach.

3.1 Data

Tables B.1 and B.2 contain the definitions of the variables in the dataset and descriptive statistics. Given that the present chapter aims to empirically identify the factors that lead to greater religious hostilities, I use the Pew Research Center's (2018) index of social hostilities involving religion (SHI) measure as my dependent variable.[37] In fact, considering the complete lack of similar measures that are available to the public, there was not much of a choice but to use this annual country-level measure of religious hostilities. More specifically, the SHI measure ranges from 0-10, with higher scores reflecting a higher incidence of concrete, hostile actions, which are both directed towards religious individuals or groups and carried out by private individuals, organizations, or social groups.

3.1.1 Aggregate Imports

To measure international trade, I use the World Bank's merchandise imports measure expressed in Purchasing Power Parity (PPP). Admittedly, a measure of imports alone is not the standard variable for trade in cross-country models of conflict. Instead, trade openness (i.e. the sum of a country's exports and imports divided by its GDP) is more frequently used.[38] Yet, meaningful problems arise with trade openness measures, as Fujii (2018) suggests, where this measure mostly captures variations in GDP, rather than variations in the sum total of exports and

[37] Due to the Pew Research Center (2018) first measuring the Social Hostilities involving Religion Index in 2007, the data from the three prior years are based on ARDA' (2001, 2003, 2005) comparable index of social regulation of religion, which is consistent with the approach of Somasundram et al. (2017).

[38] Tables B.3, B.4, B.6, and B.7 report the results when "Trade (% of GDP)" is the proxy for international trade. But, importantly, the validity of the instrumental variables that I use for trade are less clear under these specifications.

imports. Furthermore, there is no way to distinguish between the consequences of imports versus exports while using this trade measure. With this in mind, the use of imports (as opposed to exports) would appear to be the more effective strategy for my research question because imports more directly link to the level of "resentment towards out-groups" in a trading country.[39] To be sure, greater import competition from foreign firms likely reduces the price for which domestic firms sell their product. Hence, domestic firms and those employed by domestic firms are generally identified as the clear losers of rising import levels. In fact, the main function of certain government agencies is to determine (and possibly compensate) the industries which specifically lose from import competition,[40] suggesting that the groups associated with at least one domestic industry will become more resentful towards out-groups as a result of rising import levels. In contrast, the use of exports (as opposed to imports) better captures the positive link running from trade to various forms of conflict. For instance, if a country is known for exporting highly valued and contestable natural resources such as diamonds or gemstones, then it stands to reason that this attribute will (on average) create more conflict, which could specifically manifest into greater religious hostilities. In any case, because the correlation coefficient between the World Bank's merchandise import and export measures are nearly (positive) one, the use of either of these measures should produce very similar results.[41] To help illuminate this point, consider that trade liberalization policies generally increase both imports and exports, and protectionist policies typically decrease both imports and exports. Consequently, a measure of aggregate imports is used, but a measure of aggregate exports could have easily been used in its

[39] Section 2 discusses in greater detail the positive link running from "resentment towards outsiders" to SHI.
[40] For example, one of the most important functions of the United States International Trade Commission (USITC) is to identify the U.S. industries which have been "materially injured or threatened with material injury by reason of the imports under investigation" (2019).
[41] Tables B.5 and B.6 present the correlation matrix between the trade proxies and other related variables.

place.[42]

3.1.2 Instrumental Variables for Aggregate Imports

An important contribution of this chapter is to mitigate concerns over reverse causality running from SHI to international trade. To this end, I use three bilateral measures from Mayer and Zignago (2011) to generate three instrumental variables for aggregate trade. But, since the bilateral data of Mayer and Zignago (2011) do not vary over time, I make use of the calculation procedure proposed by Wei (1996), which effectively converts time-invariant bilateral variables (namely, distance) into country-level data that can, in fact, vary over time.[43] First, the product of a time-varying "GDP weight" with respect to country *j* and the "colony (ever)" dummy is calculated for every (*i*,*j*) dyad. Each GDP-weighted bilateral distance value is, then, aggregated across all of the possible dyads for country *i*, which generates a time-varying country-level measure reflecting the extent to which a country was ever in a colonial relationship with countries that (in the current year) are among the largest economies of the world. This colony measure is, then, normalized between 0 and 10 using min-max normalization. In addition to this, similar calculations are performed to generate two other country-level indices reflecting the extent to which a country shares a border and an official language with the countries that (in the current year) are among the largest economies of the world. The same calculation for the "colony" index is used for the "border" and "language" indices, except that the product of a time-varying "GDP weight" with respect to country *j* and the "contiguous" and "same official language" dummies respectively are calculated for every (*i*,*j*) dyad prior to the aforementioned

[42] In fact, the measure of aggregate exports is instead used as the trade proxy in this paper, where the empirical results are, naturally, very similar to that of using imports, as reported in Tables B.3 and B.4.

[43] Consequently, I reasonably assume that the bilateral data for the "colony (ever)," "contiguous," and "common official language" dummies in Mayer and Zignago (2011) are identical for each year between 2001 and 2016.

aggregation and max-min normalization processes being performed.[44]

3.1.3 Other Covariates

In addition to an economic variable (specifically, aggregate imports), I also control for several other factors including government restrictions on religion, religious homogeneity, level of democracy, percent Christian, and percent Muslim. The use of each of these control variables coheres with the model of Somasundram et al. (2017) and are motivated more in Section 2. First, I control for government restrictions on religion and religious diversity with the Pew Research Center's (2018) annual Government Restrictions Index (GRI) and the Pew Research Center's (2014) Religious Diversity Index respectively. Second, I control for the level of democracy with the Freedom House's (2019) civil liberties variable. Third, I also control for percent Christian and percent Muslim, both of which are derived from the ARDA (2011). Last, since the size of a country's population is believed to be an important control variable to include in a model of religious hostilities, I also control for the World Bank's total population measure.[45]

3.2 Empirical Specification

Before discussing the instrumental variables (IV) approach in greater detail, it is useful to first present the basis for the ordinary least squares (OLS) results, which are derived from the following empirical model:

$$SHI_{i,t} = \alpha + \beta_1 \ln(M_{i,t-1}) + \beta_2 \ln(Pop_{i,t}) + \beta_3 SHI_{i,t-1} + \beta_4 GRI_{i,t} + \beta_5 LoCL_{i,t} + \beta_6 RDI_i + \beta_7 \%Christian_i + \beta_8 \%Muslim_i + \beta_9 Region_{i,t} + \varepsilon_{i,t} \quad (1)$$

[44] The formulaic basis of the instrumental variable indices is presented in section 3.2.

[45] Because Table B.5 shows that the correlation coefficient between the aggregate import/export measures and population is high (above .47), I also report results when only the population measure is excluded from the model.

where $SHI_{i,t}$ and $GRI_{i,t}$ respectively represent an index of social hostilities involving religion and an index of government restrictions on religion for country *i* in year *t*; $M_{i,t-1}$ represents real aggregate merchandise imports that flows from any other country in the world to country *i* in year *t-1*;[46] $Pop_{i,t}$ is the total population of country *i* in year *t*; $LoCL_{i,t}$ is the Lack of Civil Liberties score for country *i* in year *t*; RDI_i is the Religious Diversity Index for country *i*; $\%Christian_i$ denotes the percentage of the population in country *i* that is Christian; $\%Muslim_i$ denotes the percentage of the population in country *i* that is Muslim; $Region_{i,t}$ represents the regional-year dummy for country *i* in year *t*; ln*x* denotes the natural logarithm of variable *x* ; α is a constant term; β is a coefficient; and ε is the error term.

It is also important to justify certain aspects of equation (1) before transitioning to the IV approach. Firstly, since the instrumental variables of this chapter reasonably have less predictive power for the "Trade (% of GDP)" measure, I purposely use aggregate imports as my primary proxy for international trade. At the same time, I control for population size because this measure is expected to closely (but far from perfectly) reflect the size of a country's overall economy.[47] Secondly, the inclusion of a lagged dependent variable is in line with the empirical model of Somasundram et al. (2017) and reflects my attempt to control for the unobserved factors that equation (1) would otherwise fail to account for. Hence, the use of a lagged dependent variable is especially helpful in mitigating issues of omitted variables. Thirdly, regional-year fixed effects are employed to control for the interaction effects between each year and the religious civilization in which a country is located. The five regional groups are derived from the same source as the dependent variable (SHI), namely from the Pew Research Center

[46] By "real," I mean that each datapoint is expressed in terms of Purchasing Power Parity (PPP).
[47] Table B.5 reports the correlation coefficient between GDP and population size, which is above .56. Tables B.3, B.4, B.7 and B.8 also report results when I instead use "Trade (% of GDP)" as the proxy for international trade.

(2018). By employing regional-year fixed effects, I am essentially controlling for civilizational divide for each year, which mimics both Grim and Finke (2007) and Somasundram et al. (2017).[48] Of course, this decision makes it difficult to evaluate the hypothesis that civilizational divide has a positive effect on SHI. But, because the use of regional-year fixed effects reasonably does more to control for civilizational divide compared to a time-invariant measure of the extent to which a country is composed of multiple civilizations,[49] this strategy seemingly puts one in a better position to accurately estimate the effect of trade on SHI—which is the foremost goal of this chapter. Fourthly, the measure of aggregate imports enters equation (1) logarithmically but the dependent variable (SHI) does not because one unit increases in aggregate imports are expected to increase SHI at a decreasing rate.[50] After all, a one unit increase in import levels will have a larger impact on countries with relatively low import levels because this rise in imports represents a greater percentage increase for low-import countries. For this reason, equivalent increases in aggregate imports reflect a larger (smaller) step toward globalization for countries with smaller (larger) import volumes. Hence, the positive effect of aggregate imports on SHI is expected to be larger the smaller is a country's volume of imports. With these points in mind, a semi-log functional form is justified with respect to the control variable of interest (aggregate imports) and the dependent variable (SHI). A similar rationale motivates the use of a semi-log functional form with respect to the population variable as well.

3.2.1 Details of the instrumental variables (IV) approach

To mitigate issues of reverse causality running from SHI to trade, I use the existing

[48] The rationale for why this is an important control variable in an empirical model of religious hostilities is captured by Huntington's (1996) "Clash of Civilizations" thesis, which is discussed more in Section 2.
[49] See footnote 31 for a more detailed discussion of this "multiple civilizations" measure.
[50] See, e.g. Studenmund (2016), for a thorough discussion of why this semi-log form is appropriate for situations in which a one-unit increase in the independent variable increases the dependent variable at a decreasing rate.

literature to help identify valid instruments for trade in a model of SHI. The validity of my IV approach can, in part, be gleaned by noticing that colony, contiguous, and same language variables are not expected to be weak instruments for trade (i.e. they satisfy the relevance condition). As a matter of fact, efforts within the gravity model of bilateral trade literature not only consistently show that trade between two countries rises whenever they share a colonial past (see, e.g., Anderson and Marcouiller, 2002) or lack language barriers (see, e.g., Lohmann, 2011), but the idea that bilateral trade decreases with distance (or increases with shared borders) is one of the most well-established empirical results in international economics.[51] For example, in examining the effect of an index of language barriers on trade, Lohmann (2011) consistently shows that "ever colony," "common border," and "common language" dummies each have explanatory power for bilateral trade. By extension, it stands to reason that indices reflecting the extent to which a country shares a border, an official language, or a colonial past with large-income countries in year *t* will each have significantly positive effects on aggregate trade.

In addition to this, colony, border, and language variables must neither be impacted by social hostilities involving religion (SHI) nor by other factors, such as government restrictions on religion, that affect SHI. And, while one cannot be absolutely certain that this identifying assumption holds, it would seem to be a rather difficult task to try and come up with a compelling story that suggests how (say) a rise in SHI would have an important impact on a country's colony, border, or language index scores. Alternatively, insofar as these indices meaningfully impact SHI, it is much easier to argue that this will occur through the (benign) channel of aggregate trade. As such, country-level versions of the "colony (ever),"

[51] Referring to a particularly influential meta-analysis study, Disdier and Head (2008) examines the 1467 distance estimates from 103 papers to offer systematic evidence of distance having a significant and persistently high negative impact on trade, "even after controlling for many important differences in samples and methods" (p. 37).

"contiguous," and "same language" dummies are (in tandem) expected to be valid instruments for aggregate trade in a model of SHI,[52] which would mitigate the reverse (and negative) causal process running from SHI to trade. Accordingly, the first stage results of the instrumental variables two-stage least squares (IV 2sls) regression (i.e. a model in which the above three measures are used to instrument for aggregate imports) are based on the following equation: [53]

$$\begin{aligned}\ln(M_{i,t-1}) = {} & \mu + \pi_1 Colony_{i,t-1} + \pi_2 Border_{i,t-1} + \pi_3 Language_{i,t-1} + \pi_4 \ln(Pop_{i,t}) \\ & + \pi_5 SHI_{i,t-1} + \pi_6 GRI_{i,t} + \pi_7 LoCL_{i,t} + \pi_8 RDI_i + \pi_9 \%Christian_i \\ & + \pi_{10} \%Muslim_i + \pi_{11} Region_{i,t} + v_{i,t} \end{aligned} \quad (2)$$

where $Colony_{i,t-1}$, $Border_{i,t-1}$, and $Language_{i,t-1}$ are indices that respectively reflect the extent to which a country shares a colonial history, border, and official language with large-income countries in year *t-1*;[54] the colony/border/language indices each range between 0 and 10 and are equal to the min-max normalized version of the following equations:

$$Colony_{i,t-1} = \sum_{i \neq j} w_{j,t-1} ColEver_{i,j}$$

$$Border_{i,t-1} = \sum_{i \neq j} w_{j,t-1} Contig_{i,j}$$

$$Language_{i,t-1} = \sum_{i \neq j} w_{j,t-1} SmLang_{i,j}$$

[52] A discussion of the aggregation process for the three instrumental variables are discussed further in Section 3.1, specifically under the "Instrumental Variables for Aggregate Imports" heading as well as in Table B.1.
[53] While equation (2) is an empirical model of trade, it hardly reflects the standard gravity model of trade. In essence, equation (2) represents a typical first stage regression of an IV 2sls model. More specifically, the dependent variable of equation (2) is the variable to be instrumented for (i.e. aggregate imports) and the list of control variables include: the three instruments for aggregate trade in addition to all of the control variables from equation (1), which includes the lagged dependent variable and excludes the measure being instrumented for.
[54] The lagged colony/border/language index scores are used since they instrument for a lagged measure of trade.

$ColEver_{i,j}$ is a dummy of whether countries *i* and *j* have ever engaged in a colonial relationship; $Contig_{i,j}$ is a dummy of whether countries *i* and *j* share a border; $SmLang_{i,j}$ is a dummy of whether countries *i* and *j* share an official language; $w_{j,t-1}$ is the "GDP-weight" placed on country *j* (i.e. each possible trading partner of country *i*) in year *t-1* and is equal to:

$$w_{j,t-1} = \frac{GDP_{j,t-1}}{\sum_{j \neq k} GDP_{k,t-1}}$$

$GDP_{j,t}$ is the Real GDP of country *j* in year *t*; μ is a constant term; π is a coefficient; and v is the error term. Additionally, I follow the gravity model of trade literature in that the trade variable enters equation (2) logarithmically but the colony/border/language variables do not. In summary, equations (2) and (1) respectively represent the first and second stages of the IV 2sls regression.

4. Empirical Results

Table 3.1 first reports results based on a model that is comparable to the existing literature. In particular, specification (1) includes all the explanatory variables included in the empirical model of Somasundram et al. (2017), which was itself an extension of Grim and Finke (2007). These studies, though, use Real GDP per capita as their economic control variable, which necessarily precludes the additional use of other economic variables such as trade from their model. As such, specification (1) facilitates direct comparison with results reported in the existing literature based on empirical models that do not account for the specific effects that international trade could have on social hostilities involving religion (SHI). Like Somasundram et al. (2017), I find that government restrictions on religion and civil rights have a statistically significant and positive effect on SHI in addition to finding religious homogeneity (or,

alternatively, religious diversity) and percentage of Christians to have no effect on SHI. I also find that population size is strongly and significantly associated with SHI. In contrast to both Grim and Finke (2007) and Somasundram et al. (2017) who find percentage of Muslims to have a positive effect on SHI, I find there to be no association between these two variables. Additionally, unlike Somasundram et al. (2017) and Grim and Finke (2007) who both find no significant relationship between GDP per Capita and SHI, my estimates suggest there is a negative association between GDP per capita and SHI and that this association is significant at the 1% level. This particular result is noteworthy because it suggests that GDP per capita could decrease the incidence of social hostilities involving religion.

In addition to remaining consistent with Somasundram et al. (2017), the appearance of a lagged dependent variable for each specification also reflects an attempt to mitigate the bias created by the omission of unobserved factors. While I control for other factors that are hypothesized to be relevant for explaining social hostilities involving religion (SHI), the uniform use of a lagged dependent variable helps ensure that the results are not misleading, which is especially important given the miniscule amount of research that has been devoted to developing an empirical model of religious hostilities. The results indicate that the coefficient on the lagged dependent variable is positive, statistically significant at the 1% level, and between .66 and .74 in magnitude for each specification. Thus, the effect of the lagged dependent variable on SHI is both positive and very strong, suggesting that the inclusion of this variable will indeed do much of the work in controlling for the myriad unobserved factors that I fail to account for with the other explanatory variables.

Compared with specification (1), specification (2) is generated by substituting Real GDP per capita for aggregate imports. Beyond this, the primary function of Table 3.1 is to contrast the

OLS and IV 2sls estimation results for the empirical model of this chapter. In this regard, specifications (2) and (3) are respectively based on an OLS and an overidentified IV 2sls

Table 3.1 - OLS and IV 2sls of aggregate imports on religious hostilities
(using 2001, 2003, 2005, and 2007-2016 data)

Dependent variable: Social hostilities involving religion [t]					
	(1)	(2)	(3)	(4)	(5)
(Specification Notes)	(OLS)	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])
Trade proxies:					
ln(Real aggregate imports [t-1])		**-.1330*** (.0310)**	**.2376** (.1031)**	**.0637*** (.0219)**	**.1988*** (.0545)**
Economic strength variable:					
ln(Real GDP per capita) [t]	-.1132*** (.0281)				
Other explanatory variables:					
ln(Population, total) [t]	.2069*** (.0257)	.3290*** (.0367)	.0042 (.0936)		
Lagged dependent variable [t-1]	.6711*** (.0195)	.6620*** (.0199)	.7215*** (.0252)	.7396*** (.0171)	.7280*** (.0180)
Gov't restrictions on religion [t]	.1431*** (.0261)	.1587*** (.0264)	.1119*** (.0309)	.1508*** (.0266)	.1180*** (.0300)
Lack of civil liberties [t]	-.1429*** (.0315)	-.1647*** (.0327)	.0278 (.0626)	-.0830*** (.0319)	.0019 (.0458)
Religious diversity index	-.0110 (.0187)	-.0078 (.0186)	-.0754*** (.0275)	-.0487*** (.0189)	-.0709*** (.0217)
Percent Christian	.0023 (.0020)	.0022 (.0019)	-.0011 (.0021)	-.0008 (.0019)	-.0012 (.0020)
Percent Muslim	.0013 (.0018)	.0005 (.0018)	.0006 (.0019)	-.0005 (.0018)	.0002 (.0019)
Kleibergen-Paap rk Wald F-statistic			**89.695**		**130.141**
Critical value of IV relative bias			13.91		13.91
Critical value of IV size			22.30		22.30
[P-value (↓)]	-	-	**[.2009]**	-	**[.3030]**
Hansen J Statistic	-	-	3.210	-	2.388
Regional-year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of regional-year groups	60	60	60	60	60
Number of observations	1,841	1,833	1,833	1,838	1,838

[1]: Real aggregate imports [t-1] is instrumented by $Colony_{i,t-1}$ and $Border_{i,t-1}$ and $Language_{i,t-1}$

Notes: A "Real" variable implies that the variable is expressed in terms of Purchasing Power Parity (PPP). Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. The critical value of both the IV relative bias and IV size are provided by Stock and Yogo (2005) and are respectively based on a 5% maximal IV relative bias and a 10% maximal IV size. For the lagged dependent variable, I substitute [t-2] data for [t-1] data for (t=2003, 2005, and 2007) due to [t-1] data constraints. The Regional groups are derived from the five regional categories of the Pew Research Center (2018), which include South-Saharan Africa, Asia-Pacific, Middle East-North Africa, Europe, and Americas. In each specification, 60 regional-year dummies (5 regions times the 12 years of available data) are controlled for. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

model.[55] Similarly, but with the important exception of the population measure being excluded, specifications (4) and (5) are also respectively based on an OLS and an overidentified IV 2sls model. But, before discussing the IV 2sls results, it is instructive to first discuss the OLS results. As is the case for specification (1), specification (2) indicates that the coefficient on the economic variable (here, trade) has a negative sign and is statistically significant at the 1% level. For purposes of interpretation, it is important to note that the control variable of interest enters logarithmically but the dependent variable (SHI) does not. In this regard, the OLS result in specification (2) indicates that a 1% increase in aggregate imports for a country *decreases* SHI by .1330/100=.001330. Alternatively, specification (4) demonstrates that the exclusion of a population control variable matters greatly. Not only does the OLS result in specification (4) indicate that a 1% increase in aggregate imports for a country *increases* SHI by .0637/100=.000637, but the coefficient on trade remains statistically significant at the 1% level. Of course, Table A.5 reveals that the correlation coefficient between population size and aggregate imports is nearly .48, suggesting that one cannot rule out the possibility that the omission of population size is driving the results on the trade coefficient in specification (4).

To set the stage for a discussion of the IV 2sls results, consider that the inclusion of a lagged dependent variable appears to substantially mitigate the issue of omitted variables. Consequently, the impact of reverse causality problems can conceivably be evaluated by comparing the ordinary least squares (OLS) results to that of instrumental variables two-stage least squares (IV 2sls). Crucially, however, reliable distinctions can only be made between OLS and IV 2sls if the instrumental variables are valid (i.e. both the relevance and exogeneity

[55] By "overidentified," I mean that, in the IV 2sls model under consideration, at least two measures are used to instrument for the (one) endogenous regressor, which is necessary to produce a Hansen J statistic.

conditions are satisfied). First, because I use heteroskedasticity-robust standard errors in the IV 2sls models, I primarily test the relevance condition for my instrumental variables by examining the Kleibergen-Paap Wald rk F-statistic. Using the critical values provided by Stock and Yogo (2005), I then compare this statistic to that of the two critical values for both the 5% maximal IV relative bias and the 10% maximal IV size.[56] Essentially, the magnitude of the Kleibergen-Paap Wald rk F-statistic must exceed both of the relevant critical values in order for me to confidently reject the null hypothesis that my set of instruments is weak. And, encouragingly, the Kleibergen-Paap Wald rk F-statistic in specifications (3) and (5) is 89.7 and 130.1 respectively, which eclipses both relevant critical values at least fourfold. Hence, in addition to suggesting that the bias associated with my IV 2sls trade coefficient is no greater than 5% of OLS, the resulting value for this F-statistic suggests that the potential increase in the size of the rejection region on the trade coefficient due to instrumental variables is not excessively large. On a related note, Table 3.2 shows that, in each first stage regression, at least two of the instruments for trade are statistically significant at the 1% level in addition to displaying a positive sign,[57] which further underscores confidence that my set of instrumental variables satisfy the relevance condition. Secondly, the Hansen J statistic p-value must minimally exceed that of .05, where my confidence in the exogeneity condition being satisfied would seem to rise with the size of this p-value. As shown in Table 3.1, my three instrumental variables easily pass the test of exogeneity in both specifications (3) and (5), uniformly yielding a Hansen J statistic p-value above .2. The relevance and exogeneity tests results taken together suggest that my set of three instrumental

[56] The critical value associated with the lowest-available percentage for the "maximal IV relative bias" and "maximal IV size" is 5% and 10% respectively. Incidentally, in order to use the "maximal IV relative bias" critical value, at least three instrumental variables for the (one) endogenous regressor in the model must be used.
[57] See section 3.2, specifically under the "Details of the instrumental variables (IV) approach" heading, for a justification of the realized signs on the instrumental variable coefficients reported in Table3.2 being positive.

variables do, indeed, serve as valid instruments for aggregate imports in a model of SHI. Consequently, the IV 2sls results sufficiently account for issues of reverse causality whereas the OLS results, in contrast, are subject to simultaneity bias.

Table 3.2 - First stages of IV 2sls regressions from Table 3.1
(using 2001, 2003, 2005, and 2007-2016 data)

Dependent variable: Natural log of real aggregate imports [t-1]		
	(1)	(2)
(Specification notes)	(OLS)	(OLS)
Instrumental Variables:		
Colony index [t-1]	**-.0189**	**.1609*** **
	(.0146)	**(.0176)**
Border index [t-1]	**.1758*** **	**.2940*** **
	(.0129)	**(.0185)**
Language index [t-1]	**.0445*** **	**.0135**
	(.0099)	**(.0128)**
Other explanatory variables:		
ln(Population, total) [t]	.8533***	
	(.0219)	
Social hostilities involving religion [t-1]	-.1714***	.0579***
	(.0138)	(.0175)
Government restrictions on religion [t]	.1083***	.2139***
	(.0205)	(.0291)
Lack of civil liberties [t]	-.4758***	-.5491***
	(.0249)	(.0329)
Religious diversity index	.1717***	.1428***
	(.0145)	(.0194)
Percent Christian	.0089***	.0030
	(.0016)	(.0026)
Percent Muslim	-.0008	-.0043*
	(.0015)	(.0024)
Regional-year fixed effects	Yes	Yes
Number of regional-year groups	60	60
Number of observations	1,833	1,838

Notes: Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. For the lagged dependent variable, I substitute [t-2] data for [t-1] data for 2003, 2005, and 2007 due to [t-1] data constraints. The Regional groups are derived from the five regional categories of the Pew Research Center (2018), which include South-Saharan Africa, Asia-Pacific, Middle East-North Africa, Europe, and Americas. In each specification, 60 regional-year dummies (5 regions times the 12 years of available data) are controlled for in each model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Given that the IV 2sls model is, *ceteris paribus*, reasonably more credible than that of OLS, I can now discuss the IV 2sls results. First, specification (3) in Table 3.1 indicates that a

1% increase in a country's level of aggregate imports increases social hostilities involving religion by .2376/100=.002376, which actually flips the sign from negative to positive compared with the OLS trade coefficient in specification (2). The IV 2sls trade coefficient in specification (3) is also statistically significant at the 5% level, where the positive effect of population size on SHI from specification (2) is nullified. This last result of specification (3) suggests that this population measure serves primarily as an intervening variable between the colony/border/language indices and trade.[58] Second, specification (5) indicates that a 1% increase in a country's level of aggregate imports increases social hostilities involving religion by .1988/100=.001988, which is more than three times stronger than what is indicated by the OLS trade coefficient in specification (4). Both the OLS and IV 2sls trade coefficients in specification (4) and (5) are also statistically significant at the 1% level. As suggested earlier, the present chapter's results provide reason to think that the OLS model will suffer from issues of reverse causality, where I find that the OLS coefficient on trade is consistently biased down.[59] Hence, the failure to account for reverse causality not only leads to an underestimation of the strength of the positive association between trade and SHI but it also could lead researchers to wrongly conclude that measures of economic strength have no effect on SHI (Grim and Finke, 2007; Somasundram et al., 2017) or, perhaps even, a negative effect on SHI.

5. Conclusion

The political psychology and economics of religion literature both suggest separate

[58] In other words, an increase in a country's colony/border/language index is expected to increase the size of their population and, in turn, the amount of imports that are demanded (or amount of exports that are produced).
[59] See Tables B.3, B.4, B.7 and B.8 for further empirical evidence that the OLS trade coefficient is biased down, where this result appears to hold when aggregate imports are replaced with other trade proxies, including both Trade(% of GDP) and exports, and when proxies for "social insurance" and median wages" are also controlled for.

channels through which international trade can have a positive effect on religiosity, namely through an "identity construction," a "social insurance," or an "outside economic opportunities" channel. However, to date, there is no empirical documentation of the effects of international trade on religiosity and, by extension, on the incidence of social hostilities involving religion (SHI) at the country-level. This chapter addresses this lacuna, both by replacing GDP per capita with aggregate trade in an existing model of SHI and by accounting for the reverse (and uniformly negative) causal relationship running from SHI to population size to aggregate trade with instrumental variables. The panel data analysis using 161 countries over 13 total years (2001, 2003, 2005, and 2007-2016) yielded two-stage least squares estimates of the effects of aggregate imports on SHI that are statistically significant, positive, and stronger than OLS. As such, this chapter empirically validates the theoretical conclusions suggested by the political psychology and economics of religion literatures.

Other fruitful avenues for future research exist, namely, to determine if the "outside economic opportunities" or "social insurance" channels are actually reflected in the data. First, the "outside economic opportunities" channel is influenced by the wage environment of a country or region, whereby a low or decreasing wage environment can increase the extent to which religious communities are strict. Hence, it stands to reason that the wage environment has a negative impact on SHI. Second, the "social insurance" channel suggests that the failure of a government to provide a credible social safety net for the public can create a need that religious organizations could, then, fill for a citizenry, thereby increasing religiosity in a country and SHI as well. Hence, it stands to reason that the extent to which a country fails to deliver social or public services, effectively and equitably, should have a positive impact on SHI. Neither of these hypotheses have been subject to empirical testing with specific respect to the Pew Research

Center's SHI measure, suggesting that an important gap in the literature remains to identify the empirical channels through which trade is most likely to increase SHI. Naturally, median wage and social safety net measures are expected to be highly correlated with aggregate trade, which is why an examination of possible transmission channels should primarily use "Trade (% of GDP)" as their trade proxy.[60] Additionally, it is important to note that the most direct median wage and social safety net data only seem to be available for the 36 OECD countries, suggesting that future work would need to instead make use of variables that are both similar in spirit to these ideal measures and available for a more expansive set of countries and years. Specifically, it could be argued that Real GDP per capita and the Fund for Peace's "public services fragility" measure could respectively serve as suitable proxies for the wage environment and the effectiveness of social safety net programs respectively. Morales et al. (2018) also provides a justification for why the Transparency International's Corruption Perceptions Index could serve as a suitable proxy for social insurance. Basically, in the context of credibly helping a citizenry overcome negative economic shocks, an increase in perceived corruption would seemingly push citizens to place more of their trust in religious or otherworldly organizations (as opposed to the state or non-religious organizations).[61] In any case, if suitable proxies for a country's wage environment or the state's ability to deliver social insurance can be identified, then we will be one step closer to evaluating the validity of the "outside economic opportunities" or "social insurance" channels.

Interestingly, the results of this chapter also motivate a further investigation into whether religious hostilities are uniquely influenced by trade. For instance, if certain individuals and

[60] Table B.6 reports the correlation coefficient between two trade proxies and the proxies for both the wage environment and social insurance discussed later in this section.

[61] The inclusion of each of the stated proxies for either the wage environment or social insurance are reported in Tables B.7 and B.8. Importantly, the expected sign is not only produced on the estimate of each proxy but the qualitative results on the Trade (% of GDP) measure are preserved under these specifications.

groups become more attached to their religious identity in the wake of losing an identity based on economic status, then they are also likely to become more attached to other fundamental characteristics such as their national or racial identities. So, as indicated earlier, the same logic of the "identity construction" channel would suggest that increased trade could spark additional hostilities based on national and racial identities. What's more, trade's effect on either national or racial hostilities not only describes an untapped empirical question but knowing whether the buck stops with religion could have important policy implications. For, if the effects of trade on tribalism can be successfully contained (e.g. compensating those who are negatively affected by trade), then countries could avoid the pitfalls of international trade and, in so doing, benefit from the additional wealth created by more trade while simultaneously making improvements to national security. The importance of this topic notwithstanding, I unfortunately could not find any existing national or racial hostility data that is comparable to the country-level index on social hostilities involving religion. Hence, if future work can confirm the nonexistence of such data, this chapter would minimally serve as motivation for the development of new measures that separately capture national and racial hostilities at the country level.

SUMMARY AND CONCLUSION

In the first chapter, I tackle the simultaneity bias embedded in OLS investigations of the effects of terrorism on trade. More specifically, I exploit previous empirical findings, which effectively claim that three distinct measures are both significant predictors of terrorism and excludable from trade models, to better understand the extent to which terrorism negatively impacts trade. And, with a yearly country-pair dataset between 2007 and 2016, *government* restrictions on religion, *societal* restrictions on religion, and tensions between groups are found to perform, in concert, strongly, as exogenous and relevant instruments for terrorism within an augmented gravity model of trade. Moreover, when year and country-pair dummies are included, both sets of instrumental variables easily pass both the relevance and exogeneity tests in addition to reporting a negative and statistically significant relationship between terrorism and trade, which is more than seventy times stronger than that of OLS. Results are also robust to the use of first-difference data. Therefore, in the first chapter, I conclude that OLS estimates systematically downplay the negative impact of terrorism on trade, which provides suggestive evidence in favor of the possibility that trade actually has a positive effect on terrorism.

Naturally, the second chapter attempts to confirm or deny the evidence suggested by the first. For example, the successful undermining of larger markets confers greater visibility for terrorist organizations, meaning that the benefits of terrorism in a country could conceivably rise with the size of its economy. Using data from 161 countries between 2001 and 2017 in addition to three time-varying instruments for trade, the two-stage least squares estimates of the effects of

aggregate imports on future terrorism are significant, positive, and stronger than OLS. Therefore, trade is indeed found to have an amplifying effect on terrorism.

Relatedly, the premise of the third chapter is that extant theories in political psychology and the economics of religion literature separately suggest channels through which economic globalization can increase the religious intensity of certain domestic communities. And, because the most prominent religions lay the groundwork for intolerance against those who do not share their beliefs, an uptick in religious intensity is expected to increase the incidence of religious hostilities inside a trading country. The third chapter uses data from 161 countries between 2001 and 2016 in addition to three time-varying instruments for trade (which are similar but not identical to those used in the second chapter). The two-stage least squares estimate of the effects of aggregate imports on future social hostilities involving religion are significant, positive, and stronger than OLS. Therefore, the third chapter provides evidence that trade does foster religious hostilities, which could very well constitute a transmission channel from trade to terrorism.

In summary, this dissertation consistently finds OLS estimates to be especially misleading in terms of reporting the true impact of terrorism on trade (Chapter 1), trade on terrorism (Chapter 2), and trade on religious hostilities (Chapter 3). Each of these misleading results emerge from the inability of OLS to account for issues of reverse causality, which is a notoriously difficult task to overcome. Nonetheless, my dissertation uses distinct instrumental variables approaches to overcome issues of reverse causality for three understudied causal relationships. As a result, by outlining a useful procedure for identifying relevant and exogenous instruments for both peace (e.g. terrorism) and economic (e.g. trade) regressors, I hope to imbue the dismal science community with some measure of optimism when it comes to identifying suitable instrumental variables in related future work.

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APPENDIX A: CHAPTER 2

Table A.1 - Descriptive statistics: Trade proxy and its associated instrumental variables

Variable	Definition	Obs	Mean	S.D.	Min	Max
Trade proxy:						
Real aggregate imports ($M_{i,t-1}$)	The World Bank's merchandise imports measure, expressed in terms of PPP.	2,595	69,158	215,252	16.951	2.41e07
Real aggregate exports ($X_{i,t-1}$)	The World Bank's merchandise exports measure, expressed in terms of PPP.	2,595	67,079	189,064	.8348	1.74e07
Trade openness ($\%Trade_{i,t-1}$)	The World Bank's trade (% of GDP) measure.	2,578	82.360	44.758	.1674	437.327
Instrumental variables for Trade Proxy:						
No. of "same currency" pairs ($Currency_{i,t-2}$)	Among all the 229 country pairs provided by de Sousa (2012), sum of the one-valued "currency union" pair dummies in year *t-2*.	2,576	3.4429	7.2835	0	28
Remoteness index ($Remote_{i,t-1}$)	Remoteness measure proposed by Wei (1996), which is then normalized between 0 and 10 using min-max normalization. Bilateral distance and the "GDP weights" in year *t-1* are respectively based on Mayer and Zignago (2011) and the World Bank.	2,576	5.461	2.1805	0	10
Colony index ($Colony_{i,t-1}$)	Remoteness measure proposed by Wei (1996), except bilateral distance is replaced with the "colony" dummy from Mayer and Zignago (2011).	2,576	.9632	1.4528	0	10

Note: A variable that is "Real" implies it is expressed in terms of Purchasing Power Parity (PPP). The World Bank's "Price level ratio of PPP conversion factor (GDP) to market exchange rate" measure is used to convert a $US variable into PPP. Bilateral distance, the colony dummy, and the "Relative GDP weight" of country *j* (i.e. each possible trading partner) are each based on the 226 possible "country pairs" provided by Mayer and Zignago (2011). Yugoslavia's set of "country pairs" in Mayer and Zignago (2011) are used to determine the bilateral variables for Serbia, Montenegro, and Kosovo. Moreover, for the dyads involving two of these countries, the distance value is dictated by the Yugoslavia/Yugoslavia dyad (i.e. the distance between the two most populous cities in former Yugoslavia) and the "colony" dummy is valued at zero (since none of these countries ever colonized the other). The Min-max normalizations, that are used to calculate both the remoteness and same country indices, are based only on the 161 countries included in Table A.6. Each trade proxy and instrumental variable describes country *i* during a particular year.

Table A.2 - Descriptive statistics: Dependent and other control variables

Variable	Definition	Obs	Mean	S.D.	Min	Max
Dependent Variable:						
Impact of terrorism ($IT_{i,t}$)	The Institute of Economics and Peace's (2018) Global Terrorism Index.	2,730	1.969	2.333	0	10
Other control variables:						
Real GDP per capita ($GDP_{i,t}$ /$Pop_{i,t}$)	The World Bank's Real GDP measure divided by population, total.	2,638	9,744.18	19,535.9	17.290	163,031
Population, total ($Pop_{i,t}$)	The World Bank's Population, total measure.	2,714	4.24e07	1.46e08	2.85e05	1.39e09
Lack of political rights (Lo$PR_{i,t}$)	Freedom House's (2019) Political Rights indicator.	2,702	3.639	2.141	1	7
Language fractionalization ($Lang_i$)	Alesina et al. (2003) Language Frac. indicator.	2,584	.4077	.2844	.0021	.9227
Ethnic fractionalization ($Ethnic_i$)	Alesina et al. (2003) Ethnic Frac. Indicator.	2,669	.4645	.2518	.002	.9302
Religion fractionalization ($Relig_i$)	Alesina et al. (2003) Religion Frac. Indicator.	2,652	.4307	.2334	.0023	.8603
Country area ($Area_i$)	Nunn and Puga's (2012) Land area indicator in 10 million hectares.	2,720	8.066	19.963	.0067	163.81
Elevation differences ($Elev_i$)	Nunn and Puga's (2012) Terrain Ruggedness Index.	2,720	1.342	1.228	.0165	6.740
Tropical area ($\%Trop_i$)	Nunn and Puga's (2012) % Tropical climate indicator.	2,720	33.501	42.700	0	100

Note: A "Real" variable implies that the variable is expressed in terms of Purchasing Power Parity (PPP). The World Bank's "Price level ratio of PPP conversion factor (GDP) to market exchange rate" measure is used to convert a variable from $US into PPP.

Table A.3 - OLS and IV 2sls of other trade proxies on terrorism
(using 2001-2017 data)

Dependent Variable: Impact of terrorism [t]						
	(1)	(2)	(3)	(4)	(5)	(6)
(Specification Notes)	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])
Trade proxy:						
Trade volume (as % of GDP) [t-1]	**-.0003**	**.0018**				
	(.0004)	**(.0040)**				
ln(Real aggregate exports [t-1])			**-.0154**	**.1041***	**.0184****	**.1311****
			(.0108)	**(.0546)**	**(.0094)**	**(.0519)**
Other explanatory variables:						
ln(Population, total) [t]	.0794***	.1056**	.0908***	-.0005		
	(.0152)	(.0517)	(.0164)	(.0442)		
Lagged dependent variable [t-1]	.9212***	.9252***	.9205***	.9306***	.9449***	.9256***
	(.0095)	(.0121)	(.0093)	(.0103)	(.0073)	(.0119)
Lack of political rights [t]	.0409	.0217	.0294	.1628**	.0795	.1962***
	(.0396)	(.0534)	(.0390)	(.0713)	(.0395)	(.0666)
Lack of political rights (squared) [t]	-.0409	-.0015	-.0024	-.0160**	-.0083	-.0196***
	(.0396)	(.0062)	(.0050)	(.0079)	(.0051)	(.0073)
Language fractionalization	.1131	.1117	.1042	.1364	.1203	.1381
	(.0910)	(.0906)	(.0910)	(.0941)	(.0911)	(.0936)
Ethnic fractionalization	-.0556	-.0532	-.0478	.0543	-.0372	.0794
	(.1014)	(.1013)	(.1012)	(.1165)	(.1007)	(.1220)
Religious fractionalization	.0051	-.0392	-.0240	-.1317	-.0220	-.1638
	(.0805)	(.1163)	(.0761)	(.1031)	(.0764)	(.1008)
Country area	-.0001	.0003	.0003	-.0009	.0014*	-.0015
	(.0008)	(.0011)	(.0008)	(.0009)	(.0008)	(.0016)
Elevation differences	-.0052	.0048	-.0072	.0264	-.0093	.0361
	(.0133)	(.0215)	(.0129)	(.0183)	(.0130)	(.0223)
% Tropical area	-.0004	-.0007	-.0006	-.0003	-.0005	-.0001
	(.0006)	(.0008)	(.0006)	(.0006)	(.0006)	(.0006)
Kleibergen-Paap rk Wald F statistic		**14.775**		**34.346**		**33.993**
Critical value of IV relative bias		13.91		13.91		13.91
Critical value of IV size		22.30		22.30		22.30
[P-value (↓)]	-	**[.0890]**	-	**[.2837]**	-	**[.3742]**
Hansen J statistic	-	4.838	-	2.519	-	1.966
Regional-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of regional-year groups	144	144	144	144	144	144
Number of observations	2,284	2,284	2,343	2,343	2,349	2,349

[1]: Trade proxy is instrumented by $Currency_{i,t-2}$ and $Remote_{i,t-1}$ and $Colony_{i,t-1}$

Notes: A "Real" variable implies that the variable is expressed in terms of Purchasing Power Parity (PPP). Trade volume is the sum of imports and exports. The Imports, Exports, and GDP measures are all expressed in real terms. Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. The critical value of both the IV relative bias and IV size are provided by Stock and Yogo (2005) and are respectively based on a 5% maximal IV relative bias and 10% maximal IV size. The Regional groups are derived from the nine regional categories of the Institute of Economics and Peace's (2018) Global Terrorism Index: South Asia, North America, Middle East and North Africa, Sub-Saharan Africa, South America, Asia-Pacific, Russia and Eurasia, Europe, Central America and the Caribbean. In each "lagged-dependent variable" specification, 144 regional-year dummies (9 regions times the 16 years of available data) are controlled for in each model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table A.4 - First stages of IV 2sls regressions in Table A.3
(using 2001-2017 data)

Dependent variable for specification (1): Trade volume (as % of GDP) [t-1]			
Dependent variable for specifications (2) and (3): Natural log of real aggregate exports [t-1]			
	(1)	(2)	(3)
(Specification Notes)	(OLS)	(OLS)	(OLS)
Instrumental Variables:			
Number of "same currency" relationships [t-2]	**.3404*** **	**.0447*** **	**.0457*** **
	(.1163)	**(.0045)**	**(.0055)**
Remoteness index [t-1]	**-1.531** **	**.0451**	**-.0450**
	(.7725)	**(.0293)**	**(.0396)**
Colony index [t-1]	**-1.968*** **	**-.0118**	**.0234**
	(.3116)	**(.0127)**	**(.0165)**
Other explanatory variables:			
ln(Population, total) [t]	-12.580***	.7618***	
	(.8601)	(.0276)	
Impact of terrorism [t-1]	-1.600***	-.0842***	.1583***
	(.4292)	(.0179)	(.0187)
Lack of political rights [t]	9.937***	-.9652***	-.8624***
	(2.861)	(.0894)	(.1000)
Lack of political rights (squared) [t]	-1.001***	.0954***	.0801***
	(.3384)	(.0113)	(.0125)
Language fractionalization	-.4823	-.4362***	-.3079*
	(4.062)	(.1411)	(.1668)
Ethnic fractionalization	-2.071	-.7199***	-.8995***
	(4.706)	(.1693)	(.1888)
Religious fractionalization	24.685***	1.389***	1.396***
	(4.559)	(.1382)	(.1818)
Country area	-.1909***	.0096***	.0269***
	(.0438)	(.0014)	(.0015)
Elevation differences	-4.639***	-.2758***	-.3788***
	(.8961)	(.0278)	(.0299)
% Tropical area	.1061	-.0030***	-.0047***
	(.0288)	(.0011)	(.0013)
Regional-year fixed effects	Yes	Yes	Yes
Number of regional-year groups	144	144	144
Number of observations	2,284	2,343	2,349

Notes: Trade volume is the sum of imports and exports. The Imports, Exports, and GDP measures are all expressed in real terms. Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. The Regional groups are derived from the nine regional categories of the Institute of Economics and Peace's (2018) Global Terrorism Index: South Asia; North America; Middle East and North Africa; Sub-Saharan Africa; South America; Asia-Pacific; Russia and Eurasia; Europe; Central America and the Caribbean. In each specification, 144 regional-year dummies (9 regions times the 16 years of available data) are controlled for in each model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table A.5 - Correlation matrix between trade proxies and related variables
(using 2001-2017 data)

(n = 2,493)	Imports	Exports	Trade vol.	GDP	GDP/Pop	Population
ln(Real aggregate Imports)	1.0000					
ln(Real aggregate Exports)	0.9695	1.0000				
Trade volume (as % of GDP)	0.0848	0.1064	1.0000			
ln(Real GDP)	0.9731	0.9578	-0.0721	1.0000		
ln(Real GDP/Pop)	0.8128	0.8037	0.2252	0.7820	1.0000	
ln(Population, total)	0.4798	0.4674	-0.4137	0.5638	-0.0740	1.0000

Note: Shorthand names are used for the column variables, but they are identical to the row variables that are listed. The lag (t-1) of each of the trade variables (Imports, Exports, and Trade vol.) are used, but year (t) is used for the other variables (GDP, GDP/Pop, Population).

Table A.6 - List of countries (161 in total)

Afghanistan	Albania	Algeria	Angola
Argentina	Armenia	Australia	Austria
Azerbaijan	Bahrain	Bangladesh	Belarus
Belgium	Benin	Bhutan	Bolivia
Bosnia and Herzegovina	Botswana	Brazil	Bulgaria
Burkina Faso	Burma	Burundi	Cambodia
Cameroon	Canada	Central African Republic	Chad
Chile	China	Colombia	Congo
Congo (Dem. Rep. of)	Costa Rica	Croatia	Cuba
Cyprus	Czech Republic	Denmark	Djibouti
Dominican Republic	Ecuador	Egypt	El Salvador
Equatorial Guinea	Eritrea	Estonia	Ethiopia
Finland	France	Gabon	Gambia
Georgia	Germany	Ghana	Greece
Guatemala	Guinea	Guinea-Bissau	Guyana
Haiti	Honduras	Hungary	Iceland
India	Indonesia	Iran	Iraq
Ireland	Israel	Italy	Ivory Coast
Jamaica	Japan	Jordan	Kazakhstan
Kenya	Korea	Kosovo	Kuwait
Kyrgyzstan	Laos	Latvia	Lebanon
Lesotho	Liberia	Libya	Lithuania
Macedonia	Madagascar	Malawi	Malaysia
Mali	Mauritania	Mauritius	Mexico
Moldova	Mongolia	Montenegro	Morocco
Mozambique	Namibia	Nepal	Netherlands
New Zealand	Nicaragua	Niger	Nigeria
Norway	Oman	Pakistan	Palestine
Panama	Papua New Guinea	Paraguay	Peru
Philippines	Poland	Portugal	Qatar
Romania	Russia	Rwanda	Saudi Arabia
Senegal	Serbia	Sierra Leone	Singapore
Slovakia	Slovenia	Somalia	South Africa
Spain	Sri Lanka	Sudan	Swaziland
Sweden	Switzerland	Syria	Taiwan
Tajikistan	Tanzania	Thailand	Timor-Leste
Togo	Trinidad and Tobago	Tunisia	Turkey
Turkmenistan	Uganda	Ukraine	United Arab Emirates
United Kingdom	United States	Uruguay	Uzbekistan
Venezuela	Vietnam	Yemen	Zambia
Zimbabwe			

APPENDIX B: CHAPTER 3

Table B.1 Descriptive statistics: Trade proxies and the associated instrumental variables

Variable	Definition	Obs	Mean	S.D.	Min	Max
Trade proxy:						
Real aggregate imports ($M_{i,t-1}$)	The World Bank's merchandise imports measure, expressed in terms of PPP.	1,990	74,747	226,210	16.951	2.41e07
Real aggregate exports ($X_{i,t-1}$)	The World Bank's merchandise imports measure, expressed in terms of PPP.	1,990	72,698	199,923	2.3763	1.74e07
Trade Openness ($\%Trade_{i,t-1}$)	The World Bank's trade (% of GDP) measure.	1,971	83.315	45.008	.1674	437.327
Instrumental variables for Trade Proxy:						
Colony index ($Colony_{i,t-1}$)	Remoteness measure proposed by Wei (1996), except bilateral distance is replaced with the "colony" dummy from Mayer and Zignago (2011).	1,932	.99539	1.438	0	10
Border index ($Border_{i,t-1}$)	Remoteness measure proposed by Wei (1996), except bilateral distance is replaced with the "contiguous" dummy from Mayer and Zignago	1,932	.62154	1.356	0	10
Language index ($Language_{i,t-1}$)	Remoteness measure proposed by Wei (1996), except bilateral distance is replaced with the "official language" dummy from Mayer and Zignago (2011).	1,932	2.4198	3.4333	0	10

Note: A variable that is "real" is one expressed in terms of Purchasing Power Parity (PPP). The World Bank's "Price level ratio of PPP conversion factor (GDP) to market exchange rate" measure is used to convert a $US variable into PPP. To generate the instruments, the World Banks' measure of GDP (expressed in PPP terms) is used. The 226 possible "country pairs" provided by Mayer and Zignago (2011) are used to calculate the colony, contiguous, and same language dummies along with the "GDP weight" of country *j* (i.e. each possible trading partner). Yugoslavia's individual set of "country pairs" in Mayer and Zignago (2011) are used to determine the bilateral variables for Serbia, Montenegro, and Kosovo. Moreover, for the dyads involving two of these countries, the "colony" dummy is valued at zero (since none of these countries ever colonized the other) whereas the "contiguous" and "official language" dummies are each valued at one (since all of these countries share a border and Serbian as an official language). The Min-max normalizations, that are used to normalize the colony, border, and language measures, are based on the 161 countries included in Table A.9.

Table B.2 - Descriptive statistics: Dependent and other control variables

Variable	Definition	Obs	Mean	S.D.	Min	Max
Dependent Variable:						
Societal hostilities involving religion ($SHI_{i,t}$)	The Pew Research Center's (2018) Social Hostilities Index for 2007-2016; and ARDA' (2001, 2003, 2005) Social Regulations on Religion indicator.	2,086	2.999	2.684	0	10
Other control variables:						
Government restrictions on religion ($GRI_{i,t}$)	The Pew Research Center's (2018) Government Restrictions Index for 2007-2016; and ARDA' (2001, 2003, 2005) Government Regulations on Religion indicator.	2,086	3.358	2.478	0	10
Real GDP per capita ($GDP_{i,t}/Pop_{i,t}$)	The World Bank's Real GDP measure divided by population, total.	2,018	10,380	20,535	18.155	163,031
Population, total ($Pop_{i,t}$)	The World Bank's Population, total measure.	2,075	4.27e07	1.46e08	284,968	1.38e09
Lack of civil rights ($LoCL_{i,t}$)	Freedom House's (2019) Civil Rights indicator.	2,069	3.5404	1.8176	1	7
Religious Diversity Index (RDI_i)	The Pew Research Center's (2014) measure.	2,093	3.2565	2.212	0	9
Percentage of Christians ($\%Christ_i$)	ARDA' (2011) Percent Christian indicator.	2,093	51.913	37.544	0	99.6
Percentage of Muslims ($\%Muslim_i$)	ARDA' (2011) Percent Muslim indicator.	2,093	29.013	38.204	0	99.9
Lack of Corruption ($LoCRP_{i,t}$)	Transparency International's (2019) Corruption Perceptions Index.	1,961	3.6845	2.5241	0	10
Public Services Fragility ($PSF_{i,t}$)	"Public Services" measure found in the Fund for Peace's (2019) Fragile State Index.	1,580	5.7709	2.4076	1	10

Note: Since the scale of the Corruption Perceptions Index is different before and after 2012, each year of data for this measure is normalized between 0 and 10 using max-min normalization. The Fragile State Index was first calculated in 2006, so the use of the "public services" measure implies that only the 2007-2016 data can be used.

Table B.3 - OLS and IV 2sls of other trade proxies on religious hostilities
(using 2001, 2003, 2005, and 2007-2016 data)

Dependent variable: Social hostilities involving religion [t]						
	(1)	(2)	(3)	(4)	(5)	(6)
(Specification Notes)	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])
Trade proxy:						
Trade (% of GDP) [t-1]	**-.0015**	**.0105****				
	(.0009)	**(.0050)**				
ln(Real aggregate exports [t-1])			**-.1048*****	**.1795***	**.0301***	**.1661*****
			(.0214)	**(.0967)**	**(.0167)**	**(.0500)**
Other explanatory variables:						
ln(Population, total) [t]	.1990***	.3410***	.3171***	.0333		
	(.0286)	(.0676)	(.0335)	(.0997)		
Lagged dependent variable [t-1]	.6749***	.7145***	.6578***	.7271***	.7439***	.7387***
	(.0196)	(.0241)	(.0201)	(.0305)	(.0170)	(.0175)
Gov't restrictions on religion [t]	.1353***	.0972***	.1593***	.1121***	.1574***	.1171***
	(.0265)	(.0315)	(.0265)	(.0328)	(.0264)	(.0311)
Lack of civil liberties [t]	-.0932***	-.0760**	-.1487***	-.0047	-.1040***	-.0182
	(.0269)	(.0298)	(.0301)	(.0587)	(.0303)	(.0439)
Religious diversity index	-.0344	-.0561***	-.0093	-.0710**	-.0441**	-.0706***
	(.0186)	(.0215)	(.0185)	(.0291)	(.0189)	(.0222)
Percent Christian	.0004	.0011	.0019	-.0005	-.0007	-.0008
	(.0020)	(.0022)	(.0019)	(.0021)	(.0019)	(.0020)
Percent Muslim	.0006	.0021	.0003	.0009	-.0006	.0005
	(.0019)	(.0022)	(.0018)	(.0019)	(.0018)	(.0019)
Kleibergen-Paap rk Wald F-statistic		**21.016**		**66.601**		**109.794**
Critical value of IV relative bias		13.91		13.91		13.91
Critical value of IV size		22.30		22.30		22.30
[P-value (↓)]	**-**	**[.8506]**	**-**	**[.1047]**	**-**	**[.1580]**
Hansen J Statistic	-	.324	-	4.513	-	3.691
Regional-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of regional-year groups	60	60	60	60	60	60
Number of observations	1,797	1,797	1,833	1,833	1,838	1,838

[1]: Trade proxy is instrumented by $Colony_{i,t-1}$ and $Border_{i,t-1}$ and $Language_{i,t-1}$

Notes: A "Real" variable implies that the variable is expressed in terms of Purchasing Power Parity (PPP). Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. The critical value of both the IV relative bias and IV size are provided by Stock and Yogo (2005) and are respectively based on a 5% maximal IV relative bias and a 10% maximal IV size. For the lagged dependent variable, I substitute [t-2] data for [t-1] data for (t=2003, 2005, and 2007) due to [t-1] data constraints. The Regional groups are derived from the five regional categories of the Pew Research Center (2018), which include South-Saharan Africa, Asia-Pacific, Middle East-North Africa, Europe, and Americas. In each specification, 60 regional-year dummies (5 regions times the 12 years of available data) are controlled for. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table B.4 - First Stages of IV 2sls regressions in Table A.3
(using 2001, 2003, 2005, and 2007-2016 data)

Dependent variable for specification (1): Trade (% of GDP) [t-1]			
Dependent variable for specifications (2) and (3): Natural log of real aggregate exports [t-1]			
	(1)	(2)	(3)
(Specification notes)	(OLS)	(OLS)	(OLS)
Instrumental Variables:			
Colony index [t-1]	**-2.522*** **	**-.0324***	**.1727*** **
	(.4301)	**(.0166)**	**(.0205)**
Border index [t-1]	**1.203****	**.2071*** **	**.3422*** **
	(.5510)	**(.0168)**	**(.0228)**
Language index [t-1]	**2.393*** **	**.0271****	**-.0090**
	(.6246)	**(.0141)**	**(.0168)**
Other explanatory variables:			
ln(Population, total) [t]	-11.615***	.9734***	
	(.7650)	(.0296)	
Social hostilities involving religion [t-1]	-3.718***	-.2529***	.0100
	(.6034)	(.0198)	(.0233)
Government restrictions on religion [t]	2.886***	.1436***	.2618***
	(.7953)	(.0309)	(.0402)
Lack of civil liberties [t]	-.3600**	-.4701***	-.5549***
	(.6783)	(.0336)	(.0419)
Religious diversity index	1.671**	.2105***	.1766***
	(.7471)	(.0200)	(.0250)
Percent Christian	-.0863	.0091***	.0023
	(.0560)	(.0022)	(.0034)
Percent Muslim	-.1507***	-.0008	-.0066**
	(.0493)	(.0020)	(.0030)
Regional-year fixed effects	Yes	Yes	Yes
Number of regional-year groups	60	60	60
Number of observations	1,797	1,833	1,838

Notes: A "Real" variable implies that the variable is expressed in terms of Purchasing Power Parity (PPP). Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. For the lagged dependent variable, I substitute [t-2] data for [t-1] data for 2003, 2005, and 2007 due to [t-1] data constraints. The Regional groups are derived from the five regional categories of the Pew Research Center (2018), which include South-Saharan Africa, Asia-Pacific, Middle East-North Africa, Europe, and Americas. In each specification, 60 regional-year dummies (5 regions times the 12 years of available data) are controlled for in each model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table B.5 - Correlation matrix between trade proxies and related variables
(using 2001, 2003, 2005, and 2007-2016 data)

(n = 1,910)	Imports	Exports	%Trade	GDP	GDP/Pop	Population
ln(Real aggregate Imports)	1.0000					
ln(Real aggregate Exports)	0.9688	1.0000				
Trade (% of GDP)	0.0817	0.1022	1.0000			
ln(Real GDP)	0.9715	0.9566	-0.0776	1.0000		
ln(Real GDP/Pop)	0.8071	0.8002	0.2251	0.7763	1.0000	
ln(Population, total)	0.4844	0.4700	-0.4164	0.5696	-0.0758	1.0000

Note: Shorthand names are used for the column variables, but they are identical to the row variables that are listed. The lag (t-1) of each of the trade variables (Imports, Exports, and %Trade) are used, but year (t) is used for the other variables (GDP, GDP/Pop, Population).

Table B.6 - Correlation matrix between trade proxies and proposed "transmission" variables (using 2007-2016 data)

(n = 1,472)	%Trade	Imports	Population	GDP/Pop	Public Serv	Corruption
Trade (% of GDP)	1.0000					
ln(Real aggregate Imports)	0.0729	1.0000				
ln(Population, total)	-0.4261	0.5007	1.0000			
ln(Real GDP/Pop)	0.2335	0.7877	-0.0871	1.0000		
Public Services Fragility	-0.2773	-0.7202	0.0941	-0.8814	1.0000	
Lack of Corruption	0.2474	0.6218	-0.1214	0.7862	-0.7937	1.0000

Note: Shorthand names are used for the column variables, but they are identical to the row variables that are listed. The lag (t-1) of each of the trade variables (Imports and %Trade) are used, but year (t) is used for the other variables (Population, GDP/Pop, Public Serv, and Corruption). The Fragile State Index was first calculated in 2006, so the use of the public services indicator implies that only the 2007-2016 data can be used when this variable is included in the analysis.

Table B.7 - OLS and IV 2sls controlling for possible transmission channels
(using 2001, 2003, 2005, and 2007-2016 data)

Dependent variable: Social hostilities involving religion [t]						
	(1)	(2)	(3)	(4)	(5)	(6)
(Specification Notes)	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])	(OLS)	(IV 2sls[1])
Trade proxy:						
Trade (% of GDP) [t-1]	**-.0013***	**.0118****	**-.0013***	**.0195*****	**-.0014**	**.0127****
	(.0008)	**(.0052)**	**(.0009)**	**(.0065)**	**(.0009)**	**(.0056)**
Proxy for social insurance:						
Public services fragility [t]	.0985***	.1339***				
	(.0234)	(.0288)				
Lack of corruption [t]			-.0554***	-.0901***		
			(.0197)	(.0281)		
Proxy for wage environment:						
ln(Real GDP per capita) [t]					-.1012***	-.1373
					(.0286)	(.0331)
Other explanatory variables:						
ln(Population, total) [t]	.1803***	.3376***	.2152***	.4662***	.1996***	.3686***
	(.0254)	(.0702)	(.0281)	(.0851)	(.0286)	(.0744)
Lagged dependent variable [t-1]	.7209***	.7567***	.6716***	.7331***	.6620***	.7065***
	(.0186)	(.0224)	(.0201)	(.0267)	(.0203)	(.0257)
Gov't restrictions on religion [t]	.0931***	.0540*	.1220***	.0549*	.1496***	.1042***
	(.0232)	(.0293)	(.0268)	(.0356)	(.0269)	(.0328)
Lack of civil liberties [t]	-.1413***	-.1449***	-.1299***	-.1335***	-.1437***	-.1477***
	(.0294)	(.0321)	(.0349)	(.0420)	(.0317)	(.0338)
Religious diversity index	-.0055	-.0072	-.0158	-.0435*	-.0116	-.0368
	(.0177)	(.0202)	(.0184)	(.0230)	(.0191)	(.0226)
Percent Christian	.0012	.0031	.0016	.0034	.0018	.0030
	(.0018)	(.0021)	(.0019)	(.0024)	(.0020)	(.0023)
Percent Muslim	-.0002	.0027	.0005	.0044*	.0003	.0029
	(.0016)	(.0021)	(.0018)	(.0025)	(.0019)	(.0023)
Kleibergen-Paap rk Wald F-statistic		**15.666**		**18.884**		**16.563**
Critical value of IV relative bias		13.91		13.91		13.91
Critical value of IV size		12.83		12.83		12.83
[P-value (↓)]	-	**[.0606]**	-	**[.3738]**	-	**[.3477]**
Hansen J Statistic	-	5.606	-	1.968	-	2.113
Regional-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of regional-year groups	50	50	60	60	60	60
Number of observations	1,495	1,495	1,760	1,760	1,773	1,773

[1]: Trade (% of GDP) is instrumented by $Colony_{i,t-1}$ and $Border_{i,t-1}$ and $Language_{i,t-1}$

Notes: A "Real" variable implies that the variable is expressed in terms of Purchasing Power Parity (PPP). Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. The critical value of both the IV relative bias and IV size are provided by Stock and Yogo (2005) and are respectively based on a 5% maximal IV relative bias and a 15% maximal IV size. For the lagged dependent variable, I substitute [t-2] data for [t-1] data for (t=2003, 2005, and 2007) due to [t-1] data constraints. The Regional groups are derived from the five regional categories of the Pew Research Center (2018), which include South-Saharan Africa, Asia-Pacific, Middle East-North Africa, Europe, and Americas. In each specification, regional-year dummies are controlled for. The Fragile State Index was first calculated in 2006, so the controlling of the public services indicator implies that only the 2007-2016 data can be used to generate results. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table B.8 - First Stages of IV 2sls regressions in Table A.7
(using 2001, 2003, 2005, and 2007-2016 data)

Dependent variable: Trade (% of GDP) [t-1]			
	(1)	(2)	(3)
(Specification notes)	(OLS)	(OLS)	(OLS)
Instrumental Variables:			
Colony index [t-1]	**-2.749*** (.4824)**	**-2.424*** (.4436)**	**-2.493*** (.4410)**
Border index [t-1]	**.7208 (.7011)**	**1.113* (.6440)**	**.6460 (.6317)**
Language index [t-1]	**2.274*** (.6833)**	**2.184*** (.5702)**	**2.288*** (.6010)**
Proxy for social insurance:			
Public services failure [t]	**-1.914** (.9397)**		
Lack of corruption [t]		**.6943 (.8143)**	
Proxy for wage environment:			
ln(Real GDP per capita) [t]			**1.951** (.9283)**
Other explanatory variables:			
ln(Population, total) [t]	-11.539*** (.8970)	-11.852*** (.7846)	-11.624*** (.7810)
Social hostilities involving religion [t-1]	-3.345*** (.6430)	-3.528*** (.5534)	-3.593*** (.5709)
Government restrictions on religion [t]	2.754*** (.9823)	3.084*** (.7945)	3.008*** (.7876)
Lack of civil liberties [t]	-.7329 (.9955)	.2152 (1.015)	.9552 (.7689)
Religious diversity index	1.036 (.7672)	1.468** (.6725)	1.774** (.7252)
Percent Christian	-.1582** (.0633)	-.1039* (.0572)	-.1059* (.0589)
Percent Muslim	-.1967*** (.0516)	-.1712*** (.0503)	-.1669*** (.0492)
Regional-year fixed effects	Yes	Yes	Yes
Number of regional-year groups	50	60	60
Number of observations	1,495	1,760	1,773

Notes: A "Real" variable implies that the variable is expressed in terms of Purchasing Power Parity (PPP). Heteroskedasticity-robust standard errors are shown in parentheses which is the basis for all statistical inferences. For the lagged dependent variable, I substitute [t-2] data for [t-1] data for 2003, 2005, and 2007 due to [t-1] data constraints. The Regional groups are derived from the five regional categories of the Pew Research Center (2018), which include South-Saharan Africa, Asia-Pacific, Middle East-North Africa, Europe, and Americas. In each specification, 60 regional-year dummies (5 regions times the 12 years of available data) are controlled for in each model. * indicates statistical significance at the 10% level. ** indicates statistical significance at the 5% level. *** indicates statistical significance at the 1% level.

Table B.9 - List of countries (161 in total)

Afghanistan	Albania	Algeria	Angola
Argentina	Armenia	Australia	Austria
Azerbaijan	Bahrain	Bangladesh	Belarus
Belgium	Benin	Bhutan	Bolivia
Bosnia and Herzegovina	Botswana	Brazil	Bulgaria
Burkina Faso	Burma	Burundi	Cambodia
Cameroon	Canada	Central African Republic	Chad
Chile	China	Colombia	Congo
Congo (Dem. Rep. of)	Costa Rica	Croatia	Cuba
Cyprus	Czech Republic	Denmark	Djibouti
Dominican Republic	Ecuador	Egypt	El Salvador
Equatorial Guinea	Eritrea	Estonia	Ethiopia
Finland	France	Gabon	Gambia
Georgia	Germany	Ghana	Greece
Guatemala	Guinea	Guinea-Bissau	Guyana
Haiti	Honduras	Hungary	Iceland
India	Indonesia	Iran	Iraq
Ireland	Israel	Italy	Ivory Coast
Jamaica	Japan	Jordan	Kazakhstan
Kenya	Korea	Kosovo	Kuwait
Kyrgyzstan	Laos	Latvia	Lebanon
Lesotho	Liberia	Libya	Lithuania
Macedonia	Madagascar	Malawi	Malaysia
Mali	Mauritania	Mauritius	Mexico
Moldova	Mongolia	Montenegro	Morocco
Mozambique	Namibia	Nepal	Netherlands
New Zealand	Nicaragua	Niger	Nigeria
Norway	Oman	Pakistan	Palestine
Panama	Papua New Guinea	Paraguay	Peru
Philippines	Poland	Portugal	Qatar
Romania	Russia	Rwanda	Saudi Arabia
Senegal	Serbia	Sierra Leone	Singapore
Slovakia	Slovenia	Somalia	South Africa
Spain	Sri Lanka	Sudan	Swaziland
Sweden	Switzerland	Syria	Taiwan
Tajikistan	Tanzania	Thailand	Timor-Leste
Togo	Trinidad and Tobago	Tunisia	Turkey
Turkmenistan	Uganda	Ukraine	United Arab Emirates
United Kingdom	United States	Uruguay	Uzbekistan
Venezuela	Vietnam	Yemen	Zambia
Zimbabwe			