Embargoes and the Regional Small Arms Trade

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Abstract

The black market trade in arms is often cited as a potential source of failure for arms embargoes that are imposed by the international community as a means of mitigating political violence. Sanctioned actors are suspected of gaining access to arms through illicit channels and are therefore able to offset the costs of an embargo. However, a lack of reliable data makes it difficult to study the black market arms trade and its effects in a systematic way. Most of what is known about this phenomenon comes from cases of discovered or intercepted black market trades. If diversion of black market arms is common when demand is high and supply is otherwise limited, this could account for the lackluster success of most arms embargoes. While black market trade is, almost by definition, impossible to collect reliable data on, most black market arms begin as legally-traded arms. Using data on the legal trade of small arms and light weapons, this study finds evidence that the diversion of arms to embargoed states is a systematic and substantial phenomenon.

1 Introduction

Small arms and light weapons (SALW) are defined as those weapons that can be wielded by an individual or a small group of individuals (UN General Assembly, 1997). These weapons, while less destructive on their own than major conventional weapons (MCW) or unconventional weapons, have proliferated widely and account for the majority of casualties in political conflict today. Krause and Mutimer (2005) estimate that SALW are responsible for between 60 and 90 percent of all conflict deaths. The mass violence sometimes facilitated by these tools has also led to a recent proliferation in arms embargoes, prohibitions on the sale of SALW to certain target actors, regimes, or states. Embargoes can be either unilateral or multilateral. The UN and EU, in particular, leverage embargoes as a tool of international persuasion. The first UN arms embargo was adopted in 1963 and targeted South Africa. This voluntary resolution cited apartheid, racial conflict, and arms accumulation as motivating factors (UN Security Council, 1963). Since then, the UN has adopted 29 more multilateral arms embargoes including a mandatory embargo on South Africa in 1977 (SIPRI, 2013*a*). Of these 30 embargoes, all but five are mandatory for all member states of the UN. ECOWAS, the African Union, and several other international governmental organizations have also adopted multilateral arms embargoes on occasion.

Despite their frequent use over the past few decades, arms embargoes have a poor reputation in academia for failing to deliver on their stated objectives. A number of reasons for this have been posited in the literature, including poor sender compliance (Erickson, 2013),[1] conflicting strategic interests (Moore, 2010), spoilers, and the illicit arms trade (Rogers, 1996). While the illicit trade in SALW is frequently cited, no large-N studies have attempted to link arms embargoes themselves to the size of these markets. Most of what is known about the black market trade in arms comes from case studies and examples of intercepted illegal arms

[1]Though it should be noted that Erickson finds generally positive sender compliance.

transfers. However, multilateral arms embargoes create conditions that should be conducive to the existence of a robust illicit arms trade. Arms embargoes restrict the legal supply of arms to areas where the demand for those arms is high. This creates a profit motive for actors in nearby states to leverage their comparative advantage in proximity and shared borders to supply arms to embargoed actors. Arms embargoes could therefore be counter-productive in that they encourage the diversion of legally-traded and recorded arms onto the black market and create conditions conducive to the growth of criminal organizations.

This study proceeds by first reviewing the existing literature on economic sanctions and arms embargoes. Next, the role of arms embargoes in promoting regional illicit arms markets is discussed and a testable implication is identified. Other determinants for SALW imports are discussed as well. The data and methods used to test this implication are described and the results of a large-N statistical analysis are presented. Finally, policy implications are identified.

2 Economic Sanctions and Arms Embargoes

The overall efficacy of economic sanctions is subject to much debate.[2] While sanctions are an increasingly common tool of the international community to signal dissatisfaction with a target state or with which to manipulate target state behavior, scholars are divided in assessments of their overall efficacy. In *Economic Sanctions Reconsidered*, originally published in 1985 and updated in 1990 and 2007, Hufbauer, Schott, and Elliot (HSE) conclude that sanctions are "at least partially successful in 34 percent of cases." The narrower the goal of the sanctions, they find, the more likely the sanctions are to succeed. However, sanctions with objectives of military impairment and disruption of military adventures experience relatively less success with 31 and 21 percent success rates respectively (Hufbauer, Schott and

[2]For an overview of literature on the efficacy of economic sanctions, see Kaempfer and Lowenberg (2007).

Elliott, 2007). These are the goals toward which arms embargoes are typically aimed. When compared with other studies, these results stand out as optimistic estimates of the utility of sanctions as a tool of international persuasion.

Using the HSE data, Pape (1997) comes to a very different conclusion about the efficacy of economic sanctions. In particular, Pape argues that 18 cases of economic sanctions deemed successes by HSE should instead be considered failures as military force was also necessary in each of these cases to affect change. Had the sanctions themselves been effective, he argues, this would not have been the case. In fact, after recoding the HSE data to account for military action and to reassess the concessions made by target states, Pape concludes that economic sanctions are successful less than 5 percent of the time. States are resilient to many forms of sanctions as they can conserve, substitute, and reallocate resources. Even if sanctions are more effective in the post-Cold War era than they were during the Cold War as HSE expect, Pape believes that the efficacy of sanctions must increase substantially before they can begin to replace the use of military force.

Rogers (1996), writing one year before Pape, disagrees. Through a case study of sanctions imposed on Iraq, Haiti, and Yugoslavia, Rogers finds that properly implemented sanctions can indeed compel policy change. The cases chosen represent well-implemented post-Cold War sanctions and therefore best predict the potential for success of future sanction efforts. All three, she argues, achieved success and bode well for future efforts that follow their example. Beyond compellence, Rogers points out that estimates of sanction efficacy suffer from failure to properly incorporate cases of deterrence. The overt or implied threat of sanctions can impact state behavior but these cases rarely if ever find their way into analyses.[3] Finally, Rogers notes that critiques of sanctions in the literature set different standards

[3]Drezner (2003) conducts an empirical test of this proposition and finds that there is indeed selection bias in the set of observed sanctions. In fact, the threat of sanctions has a substantially higher success rate than imposed sanctions do. This results in the systematic underestimation of success rates. Lacy and Niou (2004) also come to a similar conclusion: those sanctions that are most likely to succeed never need to be imposed as the threat of these sanctions is enough to modify target behavior.

by which to gauge efficacy and compare sanctions to disparate counterfactuals. While a 34 percent success rate sounds poor when compared to outcomes from military force, for instance, their relative costs to sender states are much lower. Baldwin (1999) emphasizes the importance of contextualizing judgements of sanction efficacy. Alternative policy options must be considered in order to determine the relative utility of sanctions. When all policies are doomed to failure, or when the alternatives are costly, sanctions may result in the most desirable outcome possible despite not achieving their stated goals.

While the literature continues to be divided over the efficacy of economic sanctions, and even over the metrics employed to assess sanction performance, there is less disagreement over the efficacy of arms embargoes in particular. Arms embargoes endeavor to restrict the supply of weapons and ammunition to a target state or actor. One example, Security Council Resolution 918, reads in part:

> ...all States shall prevent the sale or supply to Rwanda by their nationals or from their territories or using their flag vessels or aircraft of arms and related materiel of all types, including weapons and ammunition, military vehicles and equipment, paramilitary police equipment and spare parts. (UN Security Council, 1994)

Language like this is common to multilateral arms embargoes. Many arms embargoes demand cessation of ongoing hostilities. However, despite UNSCR 918's adoption in 1995, violence continued in Rwanda until 2002. The embargo was only lifted in 2008. Liberia has been the subject of numerous consecutive arms embargoes since 1992. Nonetheless, Liberia faced a civil war from 2000 through 2003, surpassing the 1,000 casualty UCDP PRIO threshold in 2003. Despite their popularity, arms embargoes maintain a poor reputation in academic circles. As Brzoska and Lopez (2009) write, "arms and supplies have been getting through to violent actors in most cases [of arms embargoes] and combating forces seldom seem to need to stop fighting for lack of supplies." Arms embargoes, they conclude, sometimes result in a reduced flow of arms to target states but that "clandestine and circuitous re-supply" often mitigate the effectiveness of embargo measures. Tierney (2005) agrees: "the record of UN arms embargoes in terms of restraining access to weapons is largely one of failure." A study that related economic sanctions to civil war duration found that some types of economic sanctions negatively impact the duration of civil war but that arms embargoes exhibited no effect. Furthermore, arms embargoes decrease the likelihood of military victory in civil conflicts but do not increase the likelihood of negotiated settlement. This suggests that arms embargoes may actually be counter-productive (Escriba-Folch, 2010). Many factors have been identified that could confound the implementation of arms embargoes. Sender compliance and cooperation, in particular, have been suspect (Wallensteen, Staibano and Eriksson, 2003; Boucher and Holt, 2009). However, recent research reveals that sender compliance is not universally poor.

Moore (2010), in a survey of UN arms embargoes and transfers of major conventional weapons, finds that many embargoes exhibited no reported violations on behalf of sender states. Of 872 embargo dyad years, only 89 had recorded transfers of MCW between a sender and the embargoed state. Of course, the exclusive focus on MCW may undermine confidence in the finding that senders often comply with embargoes. Embargoes are frequently placed on states embroiled in civil wars, conflicts that rely heavily on the use of SALW. Furthermore, SALW can be transferred covertly while transfers of MCW are harder to conceal. So, while data on MCW transfers from SIPRI (2013*b*) may reliably measure the MCW trade, analyses that rely on this data likely overestimate the effect of arms embargoes. Erickson (2013) uses a new dataset on arms embargoes from 1981-2004 to assess sender compliance with respect to both MCW and SALW. The data on SALW come from the Norwegian Initiative on Small Arms Transfers (NISAT). Erickson's analysis supports Moore's finding; embargoes inhibit senders from transferring both MCW and SALW to embargoed states. Variations in embargo success may be better explained, she suggests,through an improved understanding of the illicit trade in weapons and by looking to the ex-ante conditions conducive to success.

3 Illicit Arms Trade

This study takes an initial step in this direction by looking for indirect evidence of the expansion of regional illicit arms trade networks in data on legal arms transfers. It does not seek to determine whether arms embargoes achieve full or partial success. Instead, this study takes for granted the common position that arms embargoes are a particularly ineffective form of economic sanction and seeks evidence to explain why arms embargoes exhibit limited, if any, impact on target behavior. Actors in states that border embargoed states have a comparative advantage in arms distribution. In particular, arms from a neighboring state can cross the border into an embargoed state without transiting through third parties and risking detection. Attention to these covert arms transfers has generally been focused on criminal networks and rebel groups where demand for arms is high but suppliers are limited. However, there has been less work done to identify the role that covert arms sales play in the circumvention of arms embargoes. This is due in large part to the lack of data on illicit arms transfers.[4]

Many researchers have pointed to illicit or otherwise covert arms markets as a potential source for embargo failure. Rogers (1996) calls this trade "leakage," as arms tend to leak across porous borders. This is distinct, she argues, from the embargo-breaking spoilers that adopt an explicit policy of providing arms to the target state. In particular, the magnitude of leakage is not, she suspects, great enough to undercut embargoes while spoilers, or "black knights," can effectively replace lost supply lines. Dreyfus and Marsh (2006) examine evidence of leakage in the case of Brazil. While Brazil has never been the subject of a multilateral arms embargo, self-imposed import restrictions on SALW facilitate the study

[4]One major exception to this lack of data is a study undertaken by the Small Arms Survey. The first phase of this effort sought data on illicit arms recovered in Iraq, Afghanistan, and Somalia. These three cases provide some evidence that international efforts to curb the illicit trafficking of advanced small arms have seen some success in recent years. Armed groups in these states seem to have few if any of the most recent generation of anti-aircraft weapons and rocket propelled grenades (Schroeder and King, 2012).

of illegally imported arms. The report finds many cases of arms diversion from neighboring states to criminal organizations in Brazil. The authors conclude by stating that:

> there is little or no evidence of weapons whose source was likely to be directly from outside of South America. This shows that, at least in the case of Brazil, global trafficking networks [sic] are legally transported to the region and then diverted to illicit markets.

Klare and Andersen (1996) identify two types of covert arms distribution: black market sales motivated by profit and covert transfers facilitated by governments in support of political objectives. Unfortunately, these types of trade are difficult to study, the authors concede, because those involved take care to keep their activities hidden. Nonetheless, they provide a number of examples to illustrate how guns are diverted from intended recipients and find their way into the hands of rebels, criminals, and other covert organizations. Most of the examples involve the covert arming of rebel groups and none involve actors targeted by multilateral embargoes. However, these cases do shed light on the channels through which illicit weapons could be expected flow into embargoed states. For instance, Fidel Castro orchestrated the shipment of arms to Sandinistas in Nicaragua via Panama and Nicaragua's neighbor Costa Rica. The United States engaged in similar behavior when it secretly armed another rebel group in Nicaragua, the contras. In both cases, arms were routed through border states prior to arriving at their true destination. The authors also explain that non-government actors can use falsified end-user certificates to purchase weapons from abroad. These certificates are intended to guarantee to sender states that arms will be delivered to the end-user and not subsequently transferred without a new end-user certificate. Falsified certificates, sometimes signed by corrupt government officials, facilitate the sale and transit of arms from a point of origin and through intermediary countries. Sales of this type appear legitimate but the weapons are diverted to criminal organizations or actors in foreign countries rather than remaining at their stated destination. Vines (2005) provides examples of falsified end-user certificates being used to purchase arms for Liberia, the target of a multilateral arms embargo, and Pakistan. In the late 1990s and early 2000s, military supplies were found in Liberia that were originally authorized for transfer based on end-user certificates from Burkina Faso, Guinea, Cote d'Ivoire, and Nigeria. In other cases, soldiers of neighboring countries sold their own weapons to rebels for profit. Rebels in El Salvador purchased arms from troops in Honduras while rebels in Colombia acquired many of their AK-47s from troops in Venezuela. While these examples do not all refer to embargoed states, Wallensteen, Staibano and Eriksson (2003) recognize that similar processes could also contribute to embargo failure in their *Final Report of the Stockholm Process on the Implementation of Targeted Sanctions*. In it, they note that end-user certificate fraud represents a typical problem for embargo implementation. Neighboring state compliance and assessment of porous borders through which arms could leak are also identified as areas for improvement of embargo implementation.

In an overview of the global illicit arms trade, Haug (2001) distinguishes between two types of covert arms distribution. The gray market consists of covert trade, often government-sanctioned, that exploits loopholes to circumvent national or international laws. The black market consists of trade that is clearly illegal and operates without government consent. The author estimates that these two markets, often overlapping and not completely distinct from one another, account for between 10 and 20% of global trade in SALW, or no more than 1 billion USD annually. Black market is used in this paper to denote all covert trade.

4 Do Embargoes Affect Regional Demand?

This study takes an indirect approach to find evidence for the existence of particularly active illicit arms markets around embargoed states. Many black market arms begin as legally transferred arms that are diverted for resale en-route to or after arriving at their original

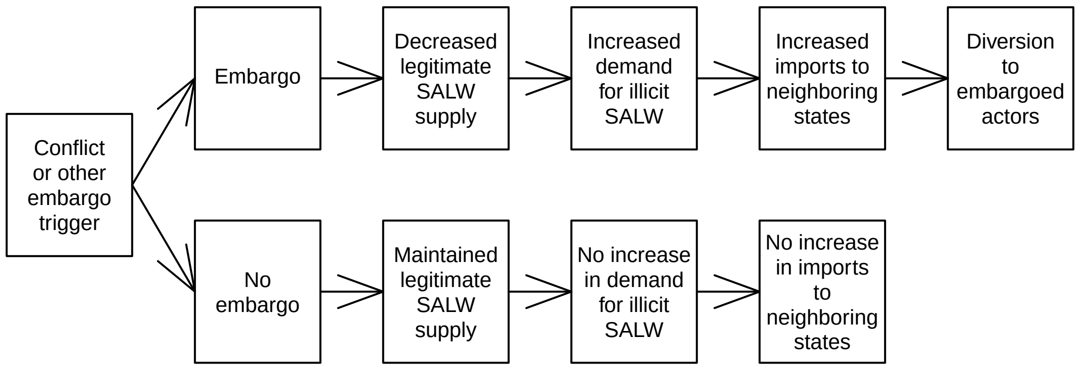

Figure 1: Hypothesized process linking arms embargoes to increased regional imports of SALW.

destination (Dreyfus and Marsh, 2006; de Soysa, Jackson and Ormhaug, 2009). Therefore, if arms embargoes create an incentive for actors in neighboring states to sell arms across the border, evidence for this may be found in records of licit arms trades.[5] This provides a testable implication that would indicate the existence of substantial illicit markets in SALW around embargoed states. All other things considered, the legal arms transfers to states neighboring embargoed states should increase if actors in those states are engaged in the illicit resale of arms to the embargoed state.

There are a number of potentially confounding variables that must also be considered in order to reduce the possibility that any relationship seen between neighbor state embargoes and arms imports is the result of omitted variable bias. The most obvious of these is the existence of a civil war in a neighboring state. Though long studied as largely intrastate events, there is a growing body of literature that portrays civil wars as international phenomena. Prominent among this line of research is the possibility of conflict contagion. Civil wars have been shown to cluster regionally but the cause of this clustering is still unclear. State characteristics that cluster regionally, such as poverty, could increase the probability of multiple

[5]This approach is similar to that taken by Dreyfus and Marsh (2006). Dreyfus and Marsh correlate legal arms transfers to states neighboring Brazil with subsequent seizures of similar weapons in Brazil.

states within that region experiencing civil war (Collier et al., 2005). Civil war contagion could just be an illusion caused by the frequent occurrence of civil wars in these regions. However, Buhaug and Gleditsch (2008) find evidence to indicate that civil war is contagious even when controlling for regionally-clustered structural variables. Ethnic linkages between states, they find, are the vectors by which civil war is transmitted across borders. Whether or not civil wars are in fact contagious, evidence suggests that policy makers fear that it is. Kathman (2011) argues that interested third parties choose to intervene in civil wars when they judge those wars to be regional contagion risks. Murdoch and Sandler (2004) find that civil wars have severe economic consequences regionally and that states will enact policies to counteract these effects. It is possible that states also adjust their arms imports in response to nearby civil wars. States could, for instance, import arms for subsequent transfer to rebel groups in neighboring states. A well-documented example of this comes from the "Iran-Contra" affair. While the United States coordinated the scheme to covertly supply arms to Nicaraguan rebels, states neighboring Nicaraqua were critical intermediaries for transporting the arms. Weapons from the United States were trafficked through Honduras and El Salvador into Nicaragua under the radar of US lawmakers and the international community (Klare and Andersen, 1996). Another possibility is that states will perceive nearby civil wars to be security threats. If civil wars are contagious, or if criminal activity associated with a weakened state or rebel group can cross the border, states may react by increasing their own security apparatuses. This could involve the increased importation of arms to bolster existing defensive capabilities. Both of these mechanisms point to the possibility that neighboring civil wars will result in increased arms imports. Because arms embargoes are frequently, but certainly not always, imposed on states suffering from civil conflict, it is important to distinguish the effect of neighboring civil wars on arms imports from the effect of neighboring embargoes on arms imports.

Another possible confounding variable is the level of arms imports in nearby states. A long history of arms race literature suggests that states are sensitive to and seek to match or surpass the military capabilities of states that pose security threats. Jervis (1978) delineates a theoretical foundation for the occurrence of arms races. Mutual insecurity and the inability of states to signal their true intentions can lead to spiralling dynamics and rapid arms buildups. For this reason, a weighted average of regional arms imports should help to predict the arms imports for a given country. Because arms embargoes, if successful, could impact the level of regional arms imports, we must control for regional arms imports in order accurately assess the affect of neighboring arms embargoes on arms imports.

Finally, the effect of domestic embargoes on the value of legal arms imports is of interest. Arms embargoes are expected to result in a decreased level of recorded arms imports as sender states consider their obligation not to arm the target of the embargo. Previous research has suggested that embargoes will indeed inhibit observed arms trade in SALW (Tierney, 2005; Brzoska, 2008; Erickson, 2013). Confirming this result will lend credence to the theory that regional illicit markets are substituting for a loss of trade in overt markets. If multilateral arms embargoes exhibit no effect on observed arms transfers, there is no loss of trade to be compensated for by local illicit trade. The inclusion of domestic arms embargoes also controls for a potential spurious relationship between neighboring arms embargoes and domestic arms imports. As can be seen in the maps in Figure 2, arms embargoes appear to cluster spatially. Therefore, if domestic arms embagoes are not considered, the unobserved impact of domestic arms embargoes may be mistaken for the effect of neighboring embargoes.

Multilateral arms embargoes are chosen for this study to minimize the influence of spoilers. Cases in which target states can offset the cost of an embargo by finding an alternate supplier are not expected to result in substantially higher demand for black market arms. Instead, this study seeks to find evidence that even ostensibly debilitating arms embargoes, those in which several of the major arms exporters participate, are circumvented by the illicit diversion of arms from neighboring states. Arms embargoes can be applied to either specific

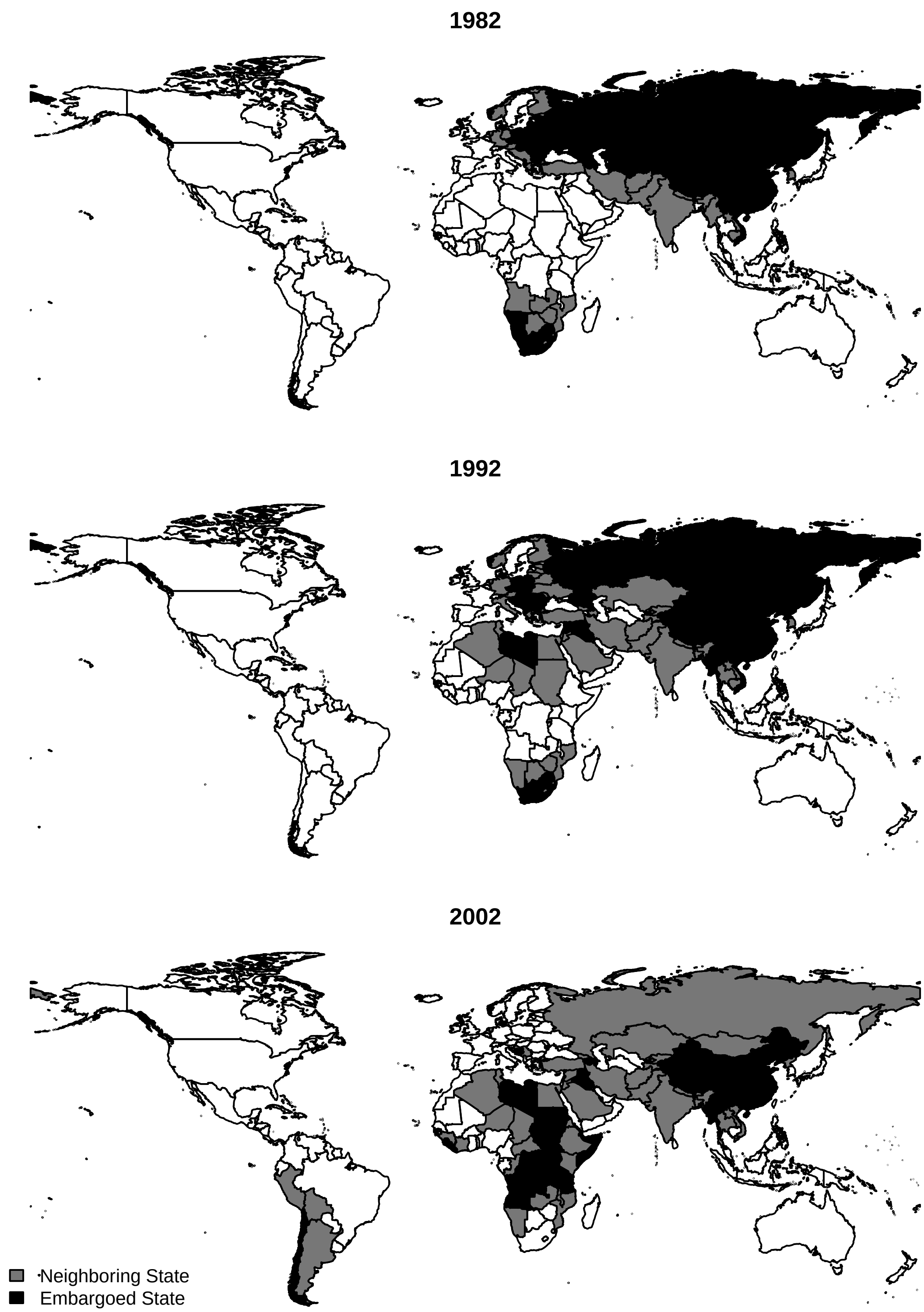

Figure 2: Embargoed states and neighboring states.

groups within a state (partial arms embargoes) or to all actors within a state (impartial arms embargoes). Both impartial and partial arms embargoes attempt to limit the supply of arms to a target area and therefore should both have the anticipated effect of forcing targets to seek arms from alternative sources. Therefore, no distinction is made in this study between the two types of embargoes.

5 Data and Methods

The unit of analysis for this study is the country year. The dataset is therefore time-series cross-sectional with 197 units observed over 24 years.[6] The only available source for data on the trade of SALW with the scope necessary for this research is the Norwegian Initiative on Small Arms Transfers (NISAT). The NISAT database compiles records of authorized arms trades for every country dyad from 1962 until 2011.[7] Much of these data come from what is reported to the United Nations Comtrade, the European Commission, and various national reporting bodies. NISAT identifies the types of arms involved in each transfer. Certain types of weapons have been excluded. These include replica weapons (for display purposes), sporting rifles, sporting shotguns, airguns, less than lethal weapons, and pyrotechnics. The remaining categories primarily cover ammunition, certain explosives, handguns, revolvers, machine guns, shotguns, rifles, and components of these weapons. These are the items in high demand in conflict zones that are likely to be subject to embargoes. As NISAT is constructed from self-reported records of imports and exports, it suffers from some self-selection bias. Some countries, for instance, have stricter reporting requirements than others. Some countries have their own interpretations of SALW that are more or less inclusive than others.[8] Furthermore, because NISAT is a trade database that measures instances of trade

[6] Not all 197 units are observed for all 24 years. Robustness tests conducted with SIPRI data cover 41 years.

[7] The NISAT database is available at http://legacy.prio.no/nisat.

[8] For more information on reporting issues in NISAT, see Dreyfus and Marsh (2006), p.29.

rather than annual aggregate trade, it is impossible to discern whether a lack of records for a country in a given year represents a true lack of trade in SALW for that year or a failure to report existing trade for that year. Despite these limitations of the dataset, it has become the standard for research in SALW trade (Erickson, 2013; de Soysa, Jackson and Ormhaug, 2010, 2009; Dreyfus and Marsh, 2006). Many steps have been taken to alleviate concerns of measurement error and missingness in the NISAT data.[9]

NISAT uses "mirror statistics" to capture each trade from both the exporter's and importer's perspective. In other words, exports from state A to state B should be recorded twice - once as an import and once as an export. This allows researchers to fill in gaps that result from a trading partner's failure to report an import or an export. In order to prevent double-counting trades for which there are two records, the total value in constant 2000 US dollars of annual arms imports for each country is calculated independently using both export records and import records. This results in two estimates for the total value of arms imports per country year. Because duplicate records from multiple reporting bodies may inflate estimates of arms trades, the lower annual estimate for arms imports is chosen. Summary statistics indicate that this is consistent with the approach of other researchers.[10] Robustness tests utilizing the high annual value for arms imports are also estimated and included in the appendix. The mean value of annual SALW imports per country is just under 14 million constant 2000 USD. The median is much lower at approximately 500 thousand

[9]Despite these steps, some irregularities remain in the data. I am in the process of identifying the sources of these strange observations but further work will be required to arrive at a data aggregation rule that preserves the most data while avoiding double-counting or erroneous observations. Most inspected data conforms to general expectations based on other estimates found from a variety of public sources. However, two observations for the UAE and another for East Germany are substantially higher than expected. These do not appear as outliers in the overall distribution of the dependent variable.

[10]Using the minimum estimate per country year, the average value in constant 2000 USD of SALW trade per year between 1992 and 2003 is just above 2.5 billion dollars. This precisely matches the estimate derived by de Soysa, Jackson and Ormhaug (2009). The estimate derived from using the higher value per year is nearly three times this amount and is just a bit higher than the estimated overall annual value of arms trade cited by Haug (2001). Dreyfus and Marsh (2006) use the high annual estimate derived from NISAT mirrored statistics. Robustness tests are estimated with both values, as well as a third estimate derived from both the high and low values, and are available in the appendix.

constant USD. The United States is a consistently high importer with typical values over 100 million USD that twice surpass one billion USD.

After aggregation of the NISAT data, approximately 12% of all country years are missing; that is, no trade in SALW is recorded for these observations. This is a common problem in trade data and three general approaches have been suggested for addressing it.[11] The first method is to assume that missing values are in fact accurate representations of no trade. Under this assumption, all country years for which no trades are recorded would be assigned a value of zero USD in arms imports. This approach does not allow the researcher to incorporate uncertainty about missing values and therefore results in artificially low standard errors. A second approach suggests listwise deletion to remove cases for which data is missing. In addition to biasing results (unless the missing data is missing completely at random, which is unlikely), the second method is impractical because it would complicate the creation of spatial variables that will be necessary for this analysis. Finally, multiple imputation can be used to estimate missing values using a variable's conditional posterior predictive distribution. This approach has the benefit of incorporating uncertainty about the missing values into the standard errors of the models run on the imputed datasets. The imputation process used in this paper is described in Honaker and King (2010). Five imputed datasets are created using the Amelia II package for R (Honaker, King and Blackwell, 2011). The models are then estimated five times each, once per imputed dataset, and combined to arrive at a final model.

In addition to the use of multiple imputation to fill in missing values, a similar procedure called "multiple overimputation" is utilized to help address concerns over measurement error in the dependent variable. Multiple overimputation, described by Blackwell, Honaker and King (2011), allows the researcher to specify a covariate for which measurement error

[11] For more details on the merits of each approach for handling missing data, see Gelpi and Grieco (2008) and Boehmer, Jungblut and Stoll (2011).

is suspected and an alternative proxy variable for that covariate. Then, as with multiple imputation, multiple overimputation will draw values from the conditional posterior predictive distribution of the covariate in question. However, rather than only drawing values for the missing data, multiple overimputation imputes values for every observation of the variable. In this way, uncertainty about the true value of each observation is incorporated in the five imputed datasets and then, ultimately, in the error terms of the statistical models. The Amelia II software package is used to multiply overimpute the value of arms imports for every country year.[12] The results for each model are presented using both the multiply imputed data and the multiply overimputed data. Because the imputation process requires that the data be distributed normally, the natural logarithm of arms imports is used.

Data on multilateral arms embargoes come from Erickson (2013). Erickson states that the primary source of information on arms embargoes for this dataset is the Stockholm International Peace Research Institute's (SIPRI) Arms Embargoes Database. Erickson then identifies additional embargoes from various other sources.[13] This dataset covers all country years from 1981 through 2004. The number of states targeted by multilateral arms embargoes in both the Erickson and SIPRI data is presented in Figure 3.

From the embargo variables, a neighboring arms embargo variable is created. This variable takes a value of one for every state that is bordering an embargoed state and zero otherwise. States that share a border are determined using the CShapes package in R (Weidmann and Gleditsch, 2010). Because of measurement error that sometimes results in a

[12] Multiple overimputation requires that a covariate be specified for which there is measurement error and imputed values will be estimated. In this case, the chosen covariate is the high estimate for annual arms imports. An alternative proxy for this covariate must also be specified. The low estimate for annual arms imports is chosen. A prior distribution is established using these values.

[13] Because there are some differences between Erickson's data and the SIPRI data, I have re-coded the SIPRI database myself and estimated the models presented in this paper using both sources. The models that use the original SIPRI data are included in the appendix. Following Erickson's procedure, embargoes are coded as beginning in their first full year after entry into force and end in their last full year before being lifted. While the SIPRI data includes fewer multilateral arms embargoes than Erickson's data, the period of time covered is greater: 1963-present. Due to coding rules and the limitations of other covariates, the models estimated using the SIPRI data begin in 1964 and end in 2004.

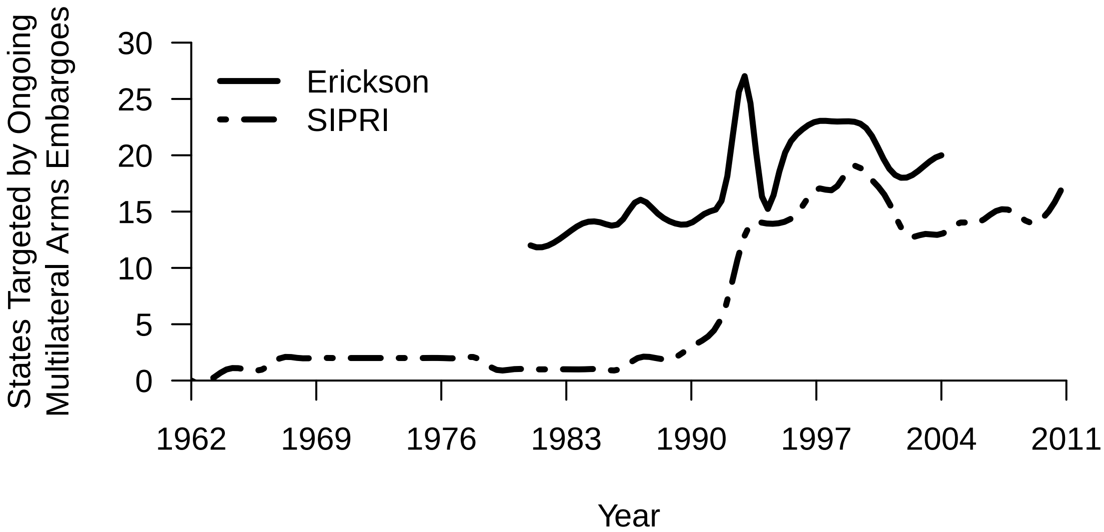

Figure 3: The number of states targeted by partial or impartial multilateral arms embargoes in the Erickson and SIPRI data. The spike in arms embargoes in the early 1990s is accounted for by several new embargoes that targeted Eastern European and African states.

greater than zero minimum distance estimate between states that actually share a border, I consider states separated by less than 20km to be "neighboring" states. Figure 4 plots the proportion of states in the dataset that border at least one embargoed state over time. The greatest number of states bordering embargoed states is 75 in 1993.

These same data on shared borders are used to determine whether or not a country borders at least one ongoing civil war in any given year. The civil war variable comes from the UCDP PRIO Armed Conflict Dataset (Gleditsch et al., 2002; Themner and Wallensteen, 2013). This dataset identifies two levels of conflict intensity, high and low. High intensity conflicts are those that result in over 1,000 battle deaths per year. Low intensity conflicts must only pass a 25 annual battle deaths threshold. The 1,000 battle deaths threshold is chosen for this study as it is these high intensity conflicts that are most likely to create insecurity in nearby states and high demand for illicit arms by rebel groups or state actors.

The spatial arms imports variable is also created with CShapes following the procedure

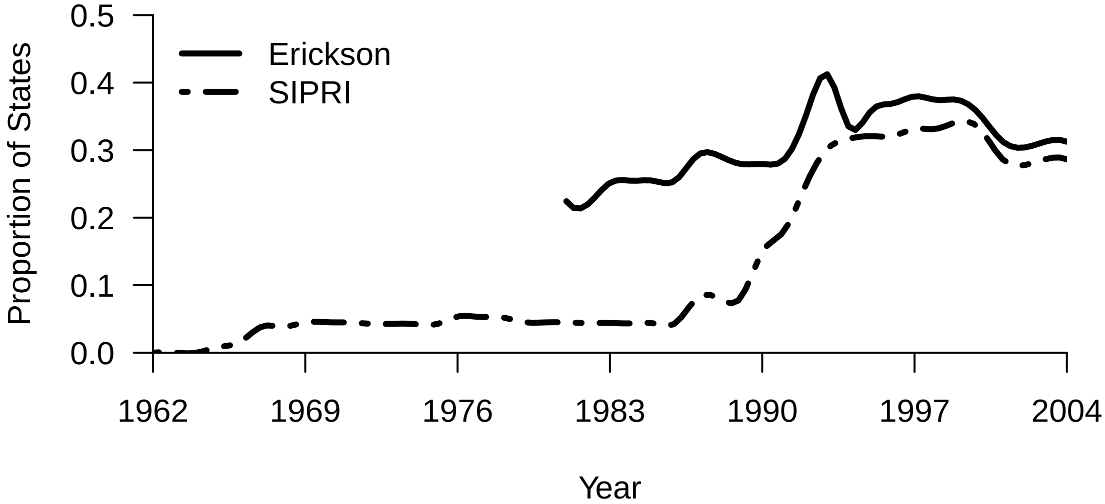

Figure 4: The proportion of states in the dataset bordering at least one target state of a multilateral arms embargo.

outlined by Buhaug and Gleditsch (2008). The spatial arms imports variable is a sort of weighted average of the arms imports of all other states where the nearest states are weighted more heavily than those further away. First, a distance matrix d is created so that each element d_{ij} of d is the distance between state i and state j. Then the inverse of each element of this matrix is calculated so that states nearer one another are weighted more highly than states further away from one another. Next, the rows are standardized; each element of row i is divided by the sum of all the elements in row i. This results in W, the weights matrix.[14] Each weights matrix, one per year, is then matrix multiplied with the corresponding arms import variable for that year. Every observation's spatial arms imports variable is unique and determined by the distance from that state to all other states. The area or region directly surrounding each state in the dataset is the most influential in this measurement while the arms imports of states further away carry less weight. This variable

[14] $W_{ij} = \frac{1/d_{ij}}{\sum_{j=1}^{n} a/d_{ij}}$

is logged for consistency with the dependent variable and to facilitate multiple imputation.

A number of control variables are included to help reduce the risk of omitted variable bias. Polity is included for its potentially confounding effect on both the dependent variable and the likelihood that a state is embargoed. Lektzian and Souva (2007) argue that sanctions are most likely between democratic senders and nondemocratic targets. On the other hand, de Soysa, Jackson and Ormhaug (2010) finds that democracies tend to import more SALW than nondemocracies. The polity variable is provided by Marshall, Jaggers and Gurr (2011). Polity 2 is coded on a 21 point scale, from -10 (most autocratic) to 10 (most democratic). Cases of foreign interruption that do not fall meaningfully on this scale are recoded as zero and an indicator variable is introduced to control for them.

Data on the logged GDP per capita in constant 2000 USD are from Gleditsch et al. (2002). Previous research has shown GDP per capita to be an important predictor of SALW imports. Wealthier states tend to import larger amounts of these weapons. However, wealth likely also plays a role in the likelihood of being targeted by a multilateral arms embargo. For instance, many of the wealthiest states today are members of the UN Security Council or close allies with members. As the UN is the most prolific institution in terms of multilateral arms embargoes, it is possible that many of these wealthy states are at a decreased risk of being targeted by embargoes of this nature.

The final three control variables are all measures of involvement in interstate and intrastate conflict. These data come from the UCDP PRIO Armed Conflict Dataset. All conflict involvement is based on the 1,000 battle-related deaths per year threshold. First, involvement in a civil war is considered. Many multilateral arms embargoes cite persistent or especially violent domestic conflict as a motivating factor. Furthermore, de Soysa et al. find that states engaged in civil war tend to import higher levels of SALW. Therefore, involvement in a civil war should be considered for its potentially confounding effect. Involvement in an international war is also controlled for.[15] This is considered in two parts: primary involvement and secondary involvement. Primary actors are those that have stated incompatible positions that lead to armed conflict. Secondary actors are those states that enter a conflict in support of a primary actor by contributing an active troop presence. Both primary and secondary parties to international conflict are expected to, if anything, import more small arms and light weapons than states not involved in international conflict. Involvement in these conflicts may also increase a state's likelihood of being embargoed.[16]

All control variables except for domestic embargoes are lagged by one year. This allows policy-makers time to adapt their arms imports/exports in response to changing conditions and follows the precedent set by Erickson (2013). Domestic embargoes are not lagged as their effects can be anticipated in advance and because they are already coded from beginning in their first full year of implementation.

Fixed effects and random effects models are two approaches commonly taken by researchers working with time-series cross-sectional data.[17] Both of these models allow for higher-level grouping of related observations. Fixed effects models control for all variation across a higher-level unit of analysis, typically the country in applications such as this. However, by controlling for all variation between countries, estimated coefficients can only be interpreted within a country over time. Furthermore, the effects of structural variables that do not change over time cannot be estimated at all (Beck and Katz, 2001). Random effects models, on the other hand, allow hierarchies within the data to be specified and modelled

[15] International wars are operationalized as extrasystemic armed conflicts, interstate armed conflicts, and internationalized civil wars as determined by the UCDP PRIO data.

[16] For example, UN embargoes have targeted Al-Qaeda and the Taliban in Afghanistan for their involvement in extrasytemic or otherwise international conflicts.

[17] The Hausman test is commonly used to help researchers decide whether a random effects model is appropriate. Specifically, the Hausman test determines whether there is heterogeneity bias arising from the between and within unit effects of the covariates. The procedure adopted in this paper models the between and within effects of each covariate explicitly. Therefore, the Hausman test is inappropriate for model selection here. Furthermore, Clark and Linzer (2012) show that the Hausman test oversimplifies the decision between random effects and fixed effects modeling.

explicitly without controlling for all of the variation between members of those hierarchies. Observations from within each higher-level group (such as the country or the year) are assumed to be drawn from a group-level distribution. This approach allows for the full range of variation in each independent variable to be used for estimation of the model. This has the unfortunate characteristic of estimating the effects of variation across space and variation over time simultaneously for each independent variable. Interpretation of these coefficients as a "one unit change over time" or a "one unit change from country to country" is therefore inappropriate. To address this problem, I follow the procedure outlined by Bell and Jones (2013).

The approach described by Bell and Jones supposes that each covariate actually represents two processes, a "between effect" and a "within effect," and should therefore be partitioned into these two parts. This is done by including in the model the average value for each covariate for each country over time. This time-invariant component, or between effect, represents the average value of a given variable for each country and facilitates cross-country comparisons of that variable. The time-varying component, or within effect, of each independent variable is determined by subtracting the mean for a given variable from its actual value. This results in a component that is centered at zero for each country but varies over time. This process produces more precise coefficient estimates by distinguishing the cross-sectional and time-series components of each covariate. Finally, to address concerns over balance in the dataset, a matching procedure is used and the models are re-estimated with the more evenly-balanced results.

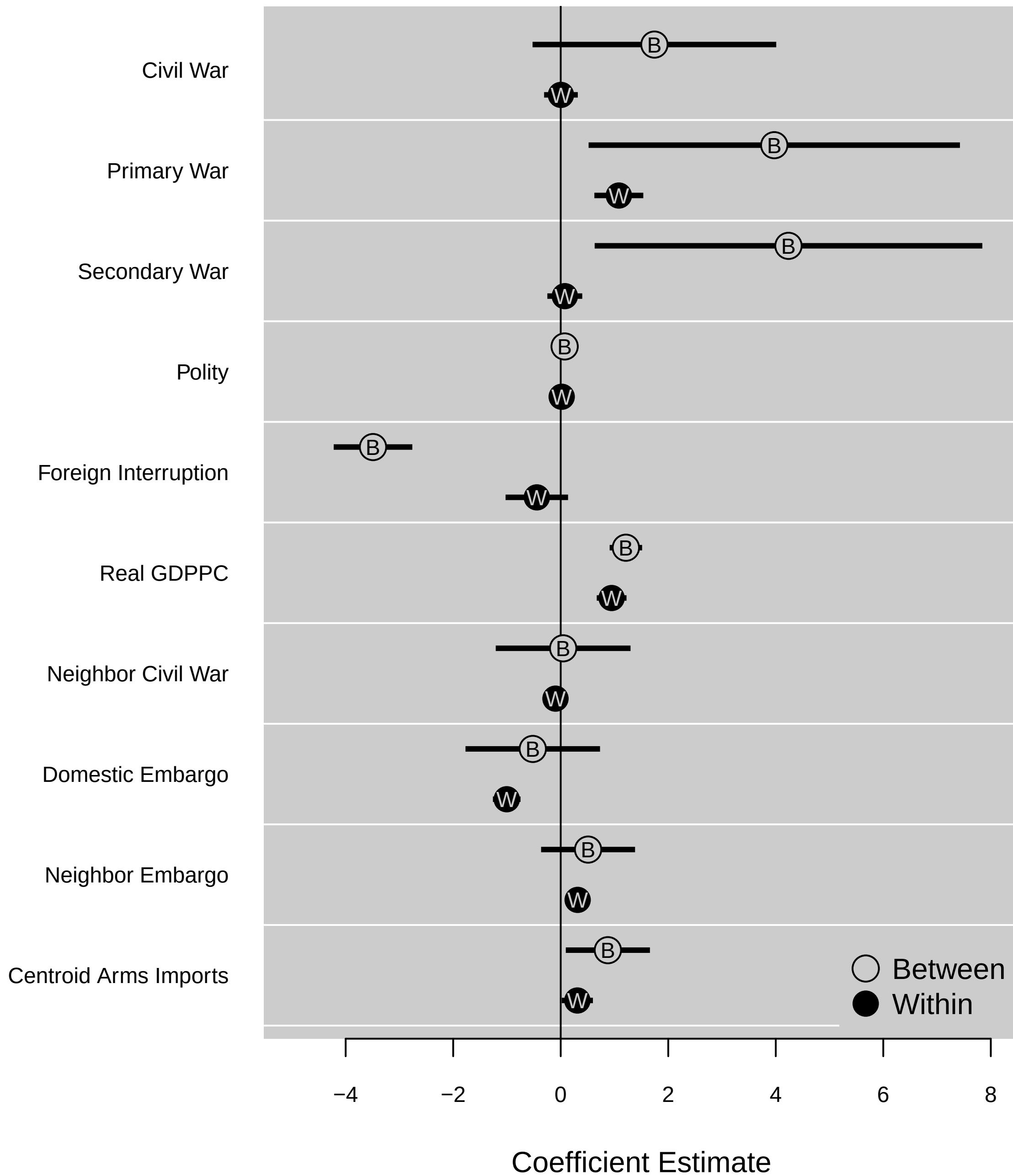

Figure 5: Coefficient plot of fully-specified random effects model. Dependent variable is logged values of multiply imputed arms imports in constant 2000 USD. Embargo data from Erickson (2013). Intercept and random effects not shown. Bold lines indicate 95% confidence intervals around coefficient estimates. Corresponding model details can be found in Table A.1.

6 Results

Figure 5 presents the results of the fully-specified random effects model of annual SALW imports.[18] The dependent variable in this model is the multiply imputed low estimate for annual arms imports in constant 2000 USD. Multilateral arms embargoes are from Erickson (2013). This plot visually represents the coefficient estimates and associated 95% confidence intervals for each covariate. Between and within country effects are included. The corresponding model details are available in Table A.1.

Domestic arms embargoes are negatively associated with the average annual value of recorded imports of SALW. The between term indicates that states targeted by multilateral arms embargoes for more years of the sample are predicted to import fewer small arms and light weapons on average than states targeted by arms embargoes for fewer years. However, the effect is indistinguishable from zero at standard levels of confidence. The within country effect of domestic arms embargoes indicates that a change within a state over time from no embargo to being the target of an embargo predicts a decrease in the value of reported imports of SALW. This relationship is significant at the 99% level.[19] These results are consistent with the expectation that arms embargoes inhibit the legal sale of arms to a targeted state.

The effect of multilateral arms embargoes on the predicted level of arms imports is substantial. Given a hypothetical country that is not targeted by an arms embargo and for

[18]The residuals from all five constituent models, one for each imputed dataset, have been tested for stationarity. Analysis of the residuals was undertaken with a panel covariate augmented Dickey-Fuller (panel CADF) test. Under this test, H_0 supposes the existence of a unit-root (non-stationarity). Rejection of H_0 indicates stationarity of the residuals and, therefore, proper cointegration of the model parameters. Appropriate lags were chosen for each panel using Akaike's information criterion as suggested by Liew (2004). The maximum lag (8) was chosen according to Schwert's rule of thumb $12 \times (T/100)^{1/4}$. Panel CADF tests were performed using *punitroots*, a package for R (Kleiber and Lupi, 2011). The procedure is further described by Lupi (2011). Cross-sectional dependence is also considered in this procedure. The p-value is consistently below 0.05 indicating high confidence in residual stationarity. Note that a small number of countries were not considered if they did not cover the full range of the time-series. Plotted residuals can be found in Figure A.1.

[19]All tests are two-tailed.

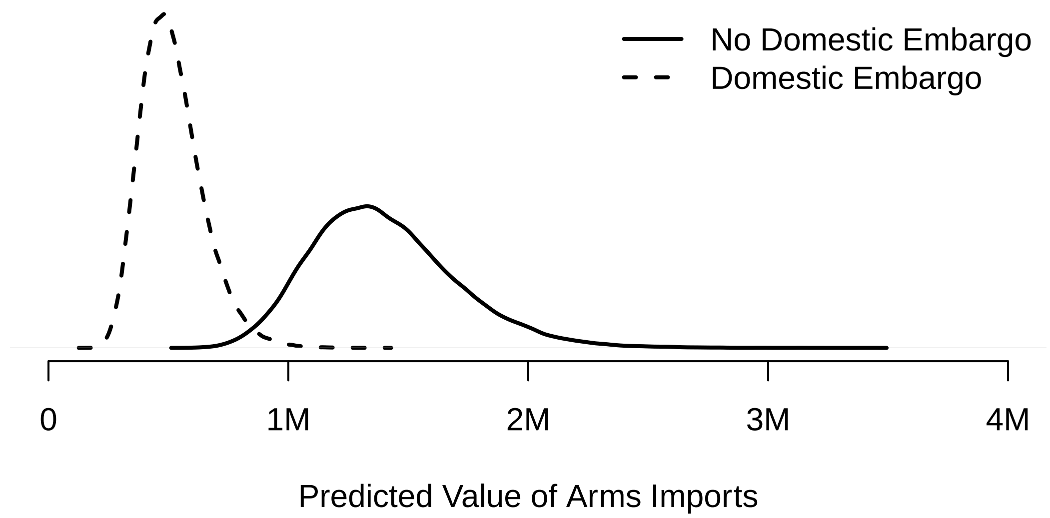

Figure 6: Predicted values for two hypothetical states, one targeted by a multilateral arms embargo and the other not. All other covariates set at the median value. Predicted values incorporate fixed effects uncertainty only.

which all other covariates are held at their median values, the annual mean predicted value of SALW imports is $1,399,725. If that same hypothetical country is instead the target of a multilateral arms embargo (such that the within term is changed from 0 to 1), the annual mean predicted value of SALW imports drops to $516,328.[20] Figure 6 plots the predicted values for these two scenarios side-by-side and includes the uncertainty associated with the fixed effects terms.[21]

Neighboring arms embargoes are also correlated with a state's SALW imports. While the between effect of this variable is indistinguishable from zero, the within term is positive and significant with a p-value less than 0.01. The coefficient estimate for neighboring embargo is 0.32. Therefore a change in status from no neighboring embargoes to at least one neighboring

[20]Because the dependent variable is log-transformed, the predicted values are calculated using the formula $e^{X\beta+\hat{\sigma}^2/2}$. This is the arithmetic mean.

[21]100,000 sets of coefficients are sampled from a multivariate random distribution defined by the coefficient estimates and variance-covariance matrices of the fixed effects portions of the models. 20,000 sets of sampled coefficients each are drawn from the five models based on the five imputed datasets. From these samples, 100,000 predicted values for each scenario are calculated and plotted.

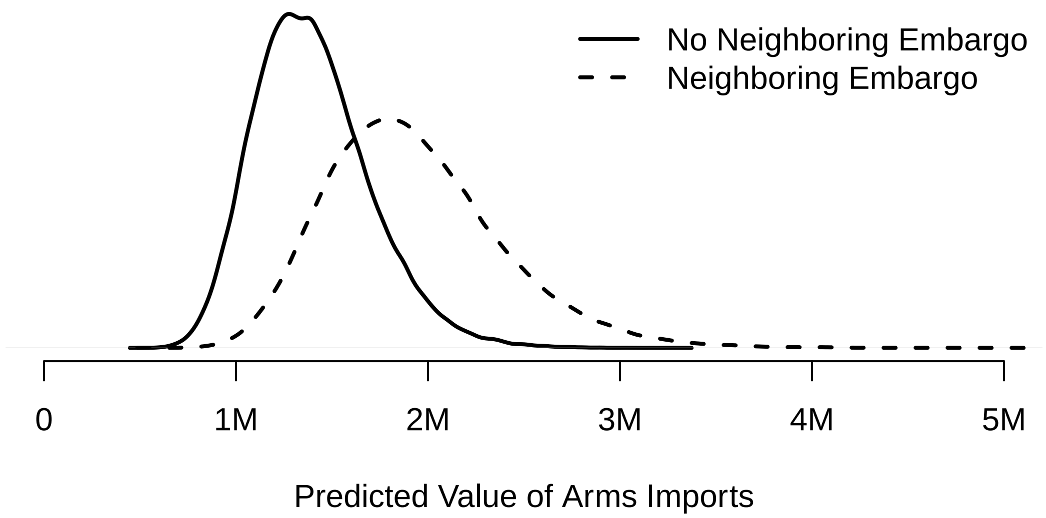

Figure 7: Predicted values for two hypothetical states, one neighboring an embargoed state and the other not. All other covariates set at the median value. Predicted values incorporate fixed effects uncertainty only.

embargo over time is associated with a 38% predicted average increase in the value of SALW imports.[22] For our hypothetical country with median values, the predicted average increase in the value of SALW imports is approximately \$526,234. Predicted import levels for these scenarios, along with associated uncertainty in the fixed effects estimates, are depicted in Figure 7.

This finding is consistent with the hypothesis that arms embargoes are undermined by illicit arms trade. States are shown to import substantially higher amounts of arms during those years when they border an embargoed state than in those years when they do not. It is important to note that this evidence does not necessarily indicate government complicity in such trade. Rather, arms are simply recorded as flowing to the unembargoed neighbors of embargoed states. These could have been destined for private or government interests, official or unofficial. Nonetheless, this indicates that arms embargoes may be counterproductive.

[22] $(e^{\beta} - 1) \times 100$ where $\beta = 0.32$.

While they do appear to stymie the legal flow of SALW to target states, they are also associated with an influx of these arms to neighboring states.

This result holds even when controlling for bordering civil wars. In fact, neither the predicted within nor between effects for neighboring civil war are distinguishable from zero at traditional levels of confidence. This signifies that the effect of neighboring embargoes is not the result of insecurity or other concerns raised by proximate conflict. The effect of domestic civil wars is also insignificant.

The spatially-weighted average of arms imports to other states, centroid arms imports, is positively associated with a state's imports of SALW. P-values for the between and within effects are 0.04 and 0.03 respetively. This indicates that there is a positive correlation between the level of arms imports in a state and the level of arms imports in the surrounding region. The effects of other control variables generally conform to expectations. Domestic involvement in international conflict is associated with a substantial increase in the value of SALW imports. Domestic civil wars, on the other hand, exhibit a positive but insignificant relationship with SALW imports. While it is difficult to see in Figure 5, the between term for polity is positive and significant with a p-value of 0.004. This can be taken to mean that states with higher average polity scores over time, meaning more democratic states, import higher levels of SALW on average than their less democratic counterparts. The marginal substantive effect is small, though. A one-unit increase in average polity score corresponds to an 8% increase in predicted average arms imports. The positive association between polity and SALW echoes the findings of de Soysa, Jackson and Ormhaug (2010).

Multiple overimputation incorporates uncertainty in our measurement of SALW into the regression models. A multiply-overimputed estimate of the value of SALW imports has been created using both the high and low estimates from NISAT. The results are shown in Table A.3. These are largely the same as those that use the low imports estimate in terms of both significance and substantive effect. Domestic arms embargoes correspond to a 63% predicted average decrease in the value of SALW imports within a country over time. Neighboring embargoes predict a 42% average increase the value of SALW imports to a country over time. These estimates closely match those presented above. Furthermore, estimates of the uncertainty around the fixed effects coefficient estimates do not change substantially.[23]

To address concerns about model dependence and the balance of the data, further analysis has been performed on a preprocessed dataset. Each treated observation in the dataset, that is observations which are "treated" by neighboring arms embargoes, has been matched with an untreated observation. This results in a balanced dataset that partially divorces the treatment from the effects of other covariates. A combination of stratification and propensity score matching is used to prune observations and derive balanced treatment and control groups. Prior to matching, all observations are stratified by year. This ensures that one panel cannot be matched with itself at an earlier or later time. This method of matching for time-series cross-sectional data was proposed by Young (2008).[24] Within each year, treatment and control cases are matched using propensity score matching (Ho et al., 2011). Pre and post-matching means are given for the control and treatment groups in Table 1. All covariates experience an improvement in balance after matching with the exception of the spatially-weighted arms imports variable which suffers a 128% decrease in balance. The difference in SALW import values between matched pairs is plotted over time in Figure 8. The median difference in arms imports between matched pairs is generally above zero (though it dips below zero on occasion). The fifty and ninety percent quantiles of the data are plotted annually as well.

[23] Further robustness tests are conducted and included in the appendix. Table A.1 includes the primary models discussed above. Table A.3 presents the same models using the multiply-overimputed dependent variable. Table A.2 presents the same models using the high estimate for SALW imports. Tables A.4 and A.5 both use the SIPRI database on multilateral arms embargoes rather than Erickson's data. The low and multiply-overimputed estimates of SALW imports are used in these tables, respectively.

[24] For a discussion of matching techniques with panel data, see Nielsen and Sheffield (2009).

	Pre-Matching			Post-Matching		
	Treated μ	Control μ	Difference	Treated μ	Control μ	Difference
Distance	0.451	0.236	0.214	0.451	0.336	0.115
Year	1994.288	1992.478	1.810	1994.288	1994.288	0.000
Domestic Embargo	0.249	0.036	0.212	0.249	0.073	0.176
Civil War	0.074	0.032	0.042	0.074	0.047	0.026
Foreign Interruption	0.068	0.182	-0.114	0.068	0.022	0.046
Primary War	0.057	0.006	0.051	0.057	0.010	0.047
Secondary War	0.044	0.029	0.015	0.044	0.034	0.010
Neighbor Civil War	0.391	0.130	0.261	0.391	0.261	0.130
Polity	-0.719	1.468	-2.188	-0.719	0.806	-1.525
Real GDP per cap.	7.974	8.553	-0.579	7.974	8.090	-0.116
Centroid Arms Imports	16.124	16.080	0.044	16.124	16.024	0.100
n	1251	2906		1251	1251	

Table 1: Summary of unmatched and matched datasets. Note that these are the results for one of five imputed datasets. Results for the other imputed datasets are consistent with these.

Two statistical tests are conducted on the matched datasets. First, a simple paired difference of means compares the mean annual logged value of SALW imports between the treated and control groups. Across all five imputed datasets, the average difference of means between treated and untreated units is 0.44. Associated 95% confidence intervals do not cover zero. In other words, a paired t-test confirms that the mean logged value of annual arms imports to states neighboring embargoed states is higher than the mean annual value of imports to states not neighboring embargoed states. Ho et al. (2007) suggest that datasets which have been matched using methods other than exact matching should be analyzed using standard parametric procedures. While nonparametric preprocessing can reduce bias by simulating a treatment and control group given observational data, samples matched via propensity score matching are nonetheless unbalanced. Failure to include appropriate covariates in a subsequent analysis could therefore result in omitted variable bias. For this reason, the same model that was estimated on the unmatched data is re-estimated on the matched data. The results are depicted in Figure 9.

The random effects model estimated with the matched dataset largely matches that es-

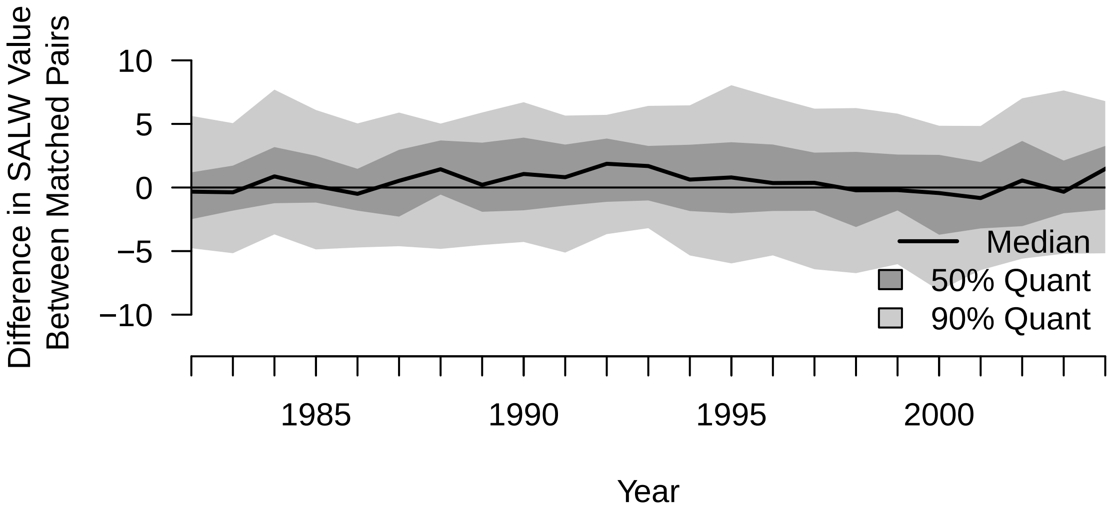

Figure 8: The difference in mean logged SALW import values between matched pairs. The median, 50% quantile, and 90% quantile are plotted. This plot represents one of five imputed datasets. Four more plots are included in Figure A.2, one per dataset.

timated with the raw data. In particular, the point estimates for the neighboring embargo variables are very similar, as are their standard errors. In the matched sample, a transition over time from no neighboring arms embargoes to at least one neighboring embargo is associated with a 51% average predicted increase in the level of SALW imports. This is slightly higher than the 38% average predicted increase estimated with the unmatched data.

Support for the hypothesis that arms embargoes promote the dramatic growth of regional SALW imports is found when using the raw data, the multiply-overimputed data, and the matched data. This finding is consistent with the expectation that arms embargoes encourage actors in neighboring states to acquire arms via legal channels for subsequent diversion to the embargoed actors. Furthermore, all data sets show that multilateral arms embargoes have a negative impact on the legal transfer of arms directly to embargoed actors.

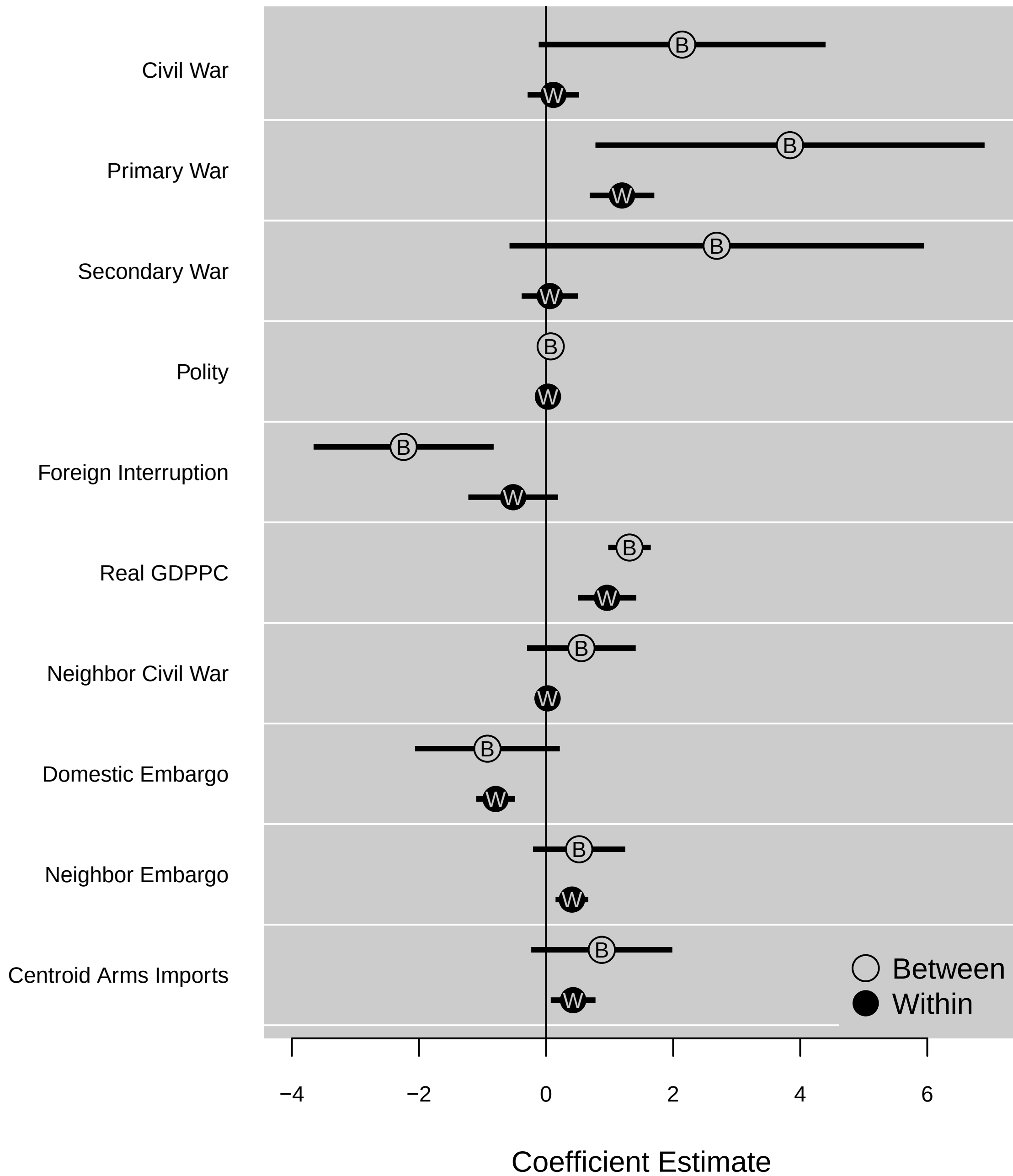

Figure 9: Random effects model with post-matching data.

7 Conclusion

Arms embargoes are a common form of economic sanction. Previous studies have focused on sender compliance (or the lack thereof) to explain their poor performance. However, recent work shows that sender compliance with multilateral arms embargoes is actually quite good. While another explanation, illicit arms markets in neighboring states, has been hypothesized and studied via critical examination of individual cases, no previous studies have tested this hypothesis on a large-N dataset of arms imports and embargoes. By examining the determinants of legal SALW imports, this study has found evidence to indicate that arms embargoes foster the growth of illicit arms markets in neighboring states. While controlling for a variety of alternative explanations, arms embargoes are associated with an increase in the level of arms imports to non-embargoed border states. Previous case studies lend support to the notion that this increase in arms is the result of the covert diversion of arms from intended recipients to embargoed actors.

While ostensibly hindering the flow of arms to targeted states, multilateral arms embargoes may in fact result in an influx of arms to the surrounding area. If these arms are in fact destined for targeted actors, this could have the effect of forcing more weapons that would otherwise be traded legally onto the black market. The results of this study suggest that it was wise to include language on the prevention of conventional arms diversion in the 2013 Arms Trade Treaty (UN General Assembly, 2013). This unprecedented treaty regulating the international trade of conventional weapons implores states to take measures to prevent the diversion of conventional arms to unauthorized recipients. These measures, if properly implemented, could improve the efficacy of arms embargoes by mitigating the risk that these embargoes will encourage substantial trade in illicit arms in the immediate region.

In addition to identifying a mechanism for embargo failure, this research points to unanticipated consequences of arms embargoes that deserve further study. In fostering the growth of regional arms markets, embargoes may in fact have long-lasting negative repercussions. Andreas (2005) notes that the criminal networks that emerge in the presence of economic sanctions persist even after the sanctions have been lifted. These networks simply transition to other profitable criminal enterprises. Wallensteen, Staibano and Eriksson (2003) also voice this concern. They warn that "the negative impacts of targeted sanctions include...increased incentives for criminal evasion, [and] increased civilian dependence on criminal economic activities," among others. This study lends weight to the importance of understanding how criminal networks thrive in the presence of embargoes and persist after their expiration.

International coalitions impose arms embargoes to prevent the acquisition of arms by actors typically engaged in mass violence or political conflict. However, an exclusive focus on the prevention of legal transfers to the target actor ignores the possibility that those actors can circumvent embargoes by relying on diversion through neighboring states. An effective embargo policy must consider not only the target actor but also the potential for criminal and political interests in the surrounding area that may act to subvert the embargo. The evidence presented here highlights the gravity of this problem by isolating the effect of embargoes on SALW imports in neighboring states.

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A Appendix

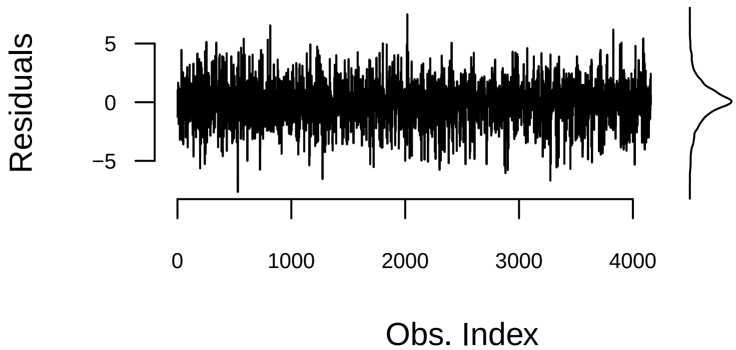

Figure A.1: Plotted residuals from fully specified model in Figure 5.

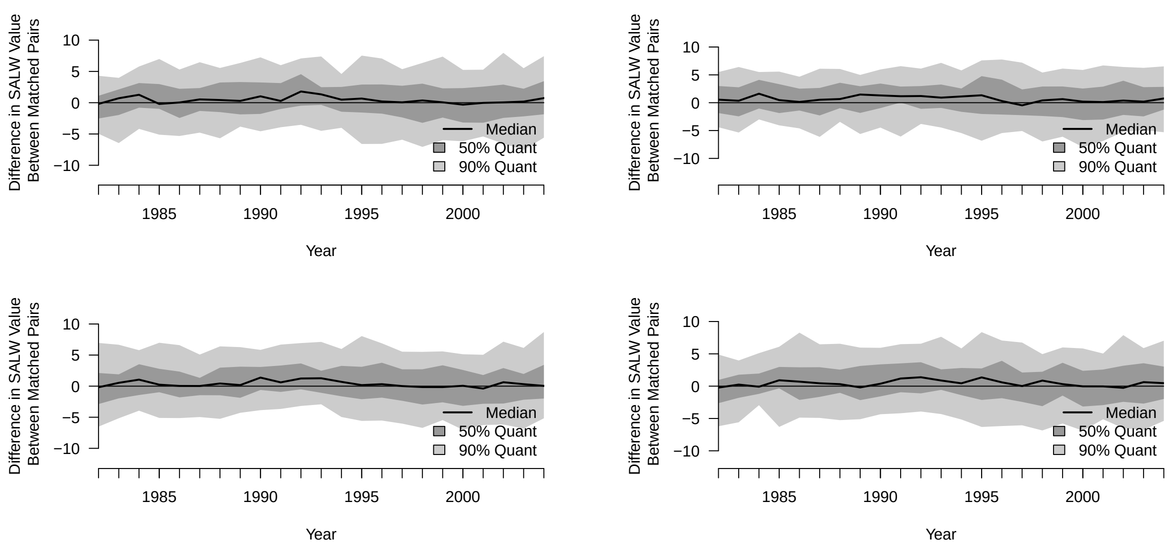

Figure A.2: Differences in logged SALW import values between matched pairs. Four imputed and matched datasets.

Table A.1: Erickson Embargoes Data (1981-2004), Multiple Imputation, Low Imports Estimate

	Full Model (S.E.)	Neighboring Embargo	Centroid Imports
(Intercept)	−11.34 (5.98)	1.48 (1.20)	−12.45 (5.87)
Within Country Effects			
Domestic embargo	−1.00 (0.13)	−0.99 (0.13)	−0.93 (0.13)
Neighbor Embargo	0.32 (0.09)	0.31 (0.10)	
Foreign Interruption	−0.44 (0.30)	−0.45 (0.30)	−0.44 (0.30)
Civil War	0.00 (0.16)	−0.00 (0.16)	0.00 (0.16)
Neighbor Civil War	−0.10 (0.09)	−0.10 (0.09)	−0.10 (0.09)
Primary War	1.08 (0.23)	1.08 (0.23)	1.14 (0.23)
Secondary War	0.08 (0.17)	0.08 (0.17)	0.07 (0.17)
Centroid Arms Imports	0.31 (0.15)		0.31 (0.15)
Polity	0.02 (0.01)	0.02 (0.01)	0.02 (0.01)
Real GDP per cap.	0.95 (0.14)	0.95 (0.14)	0.91 (0.14)
Between Country Effects			
Domestic Embargo	−0.52 (0.64)	−0.33 (0.64)	−0.31 (0.61)
Neighbor Embargo	0.51 (0.45)	0.69 (0.44)	
Foreign Interruption	−3.49 (0.37)	−3.47 (0.38)	−3.56 (0.37)
Civil War	1.74 (1.16)	1.69 (1.18)	1.84 (1.15)
Neighbor Civil War	0.05 (0.64)	0.01 (0.65)	0.30 (0.60)
Secondary War	3.97 (1.76)	3.96 (1.79)	4.20 (1.75)
Secondary War	4.24 (1.84)	3.89 (1.86)	4.28 (1.84)
Centroid Arms Imports	0.88 (0.40)		0.96 (0.39)
Polity	0.07 (0.03)	0.08 (0.03)	0.07 (0.02)
Real GDP per cap.	1.21 (0.15)	1.37 (0.14)	1.20 (0.15)
Random Effects			
Ccode	(1.67)	(1.70)	(1.67)
Year	(0.26)	(0.33)	(0.27)
Residual	(1.61)	(1.61)	(1.61)

$n = 4157$

Random effects represent the average standard deviation over the five imputed datasets.

Table A.2: Erickson Embargoes Data (1981-2004), Multiple Imputation, High Imports Estimate

	Full Model (S.E.)	Neighboring Embargo	Centroid Imports
(Intercept)	−10.74 (7.45)	0.66 (1.29)	−11.50 (7.31)
Within Country Effects			
Domestic embargo	−0.93 (0.15)	−0.91 (0.15)	−0.86 (0.15)
Neighbor Embargo	0.34 (0.10)	0.35 (0.10)	
Foreign Interruption	−0.40 (0.33)	−0.37 (0.33)	−0.40 (0.33)
Civil War	−0.19 (0.16)	−0.20 (0.16)	−0.20 (0.16)
Neighbor Civil War	−0.08 (0.10)	−0.08 (0.10)	−0.08 (0.10)
Primary War	1.14 (0.25)	1.11 (0.25)	1.20 (0.25)
Secondary War	0.19 (0.17)	0.18 (0.17)	0.18 (0.17)
Centroid Arms Imports	0.35 (0.24)		0.38 (0.25)
Polity	0.02 (0.01)	0.02 (0.01)	0.02 (0.01)
Real GDP per cap.	1.09 (0.15)	1.10 (0.15)	1.04 (0.15)
Between Country Effects			
Domestic Embargo	−0.91 (0.69)	−0.75 (0.69)	−0.79 (0.66)
Neighbor Embargo	0.29 (0.48)	0.43 (0.47)	
Foreign Interruption	−3.66 (0.40)	−3.68 (0.40)	−3.70 (0.40)
Civil War	2.74 (1.26)	2.65 (1.26)	2.80 (1.25)
Neighbor Civil War	0.51 (0.69)	0.51 (0.70)	0.65 (0.65)
Secondary War	3.05 (1.91)	3.06 (1.93)	3.18 (1.90)
Secondary War	3.86 (2.00)	3.62 (2.00)	3.89 (1.99)
Centroid Arms Imports	0.72 (0.47)		0.77 (0.46)
Polity	0.08 (0.03)	0.09 (0.03)	0.08 (0.03)
Real GDP per cap.	1.41 (0.17)	1.54 (0.15)	1.41 (0.17)
Random Effects			
Ccode	(1.82)	(1.83)	(1.81)
Year	(0.41)	(0.41)	(0.42)
Residual	(1.64)	(1.64)	(1.64)

$n = 4157$

Random effects represent the average standard deviation over the five imputed datasets.

Table A.3: Erickson Embargoes Data (1981-2004), Multiple Overimputation

	Full Model (S.E.)	Neighboring Embargo	Centroid Imports
(Intercept)	−7.04 (2.26)	1.52 (1.18)	−7.14 (2.19)
Within Country Effects			
Domestic embargo	−0.98 (0.15)	−0.98 (0.15)	−0.90 (0.14)
Neighbor Embargo	0.35 (0.11)	0.33 (0.11)	
Foreign Interruption	−0.53 (0.30)	−0.54 (0.30)	−0.53 (0.30)
Civil War	−0.08 (0.16)	−0.07 (0.16)	−0.08 (0.16)
Neighbor Civil War	−0.04 (0.09)	−0.04 (0.09)	−0.04 (0.09)
Primary War	1.12 (0.25)	1.16 (0.25)	1.19 (0.24)
Secondary War	0.10 (0.17)	0.13 (0.17)	0.10 (0.17)
Centroid Arms Imports	0.35 (0.14)		0.31 (0.14)
Polity	0.02 (0.01)	0.02 (0.01)	0.02 (0.01)
Real GDP per cap.	0.95 (0.13)	0.94 (0.13)	0.91 (0.13)
Between Country Effects			
Domestic Embargo	−0.69 (0.62)	−0.27 (0.64)	−0.65 (0.60)
Neighbor Embargo	0.11 (0.43)	0.64 (0.44)	
Foreign Interruption	−3.17 (0.36)	−3.45 (0.38)	−3.19 (0.36)
Civil War	1.78 (1.10)	1.72 (1.16)	1.81 (1.10)
Neighbor Civil War	0.20 (0.61)	0.03 (0.64)	0.25 (0.57)
Secondary War	4.06 (1.69)	4.12 (1.78)	4.10 (1.67)
Secondary War	4.87 (1.77)	3.98 (1.85)	4.89 (1.77)
Centroid Arms Imports	0.86 (0.20)		0.87 (0.19)
Polity	0.07 (0.02)	0.08 (0.03)	0.07 (0.02)
Real GDP per cap.	1.05 (0.15)	1.36 (0.14)	1.05 (0.15)
Random Effects			
Ccode	(1.59)	(1.68)	(1.59)
Year	(0.22)	(0.31)	(0.24)
Residual	(1.55)	(1.55)	(1.55)

$n = 4157$

Random effects represent the average standard deviation over the five imputed datasets.

Table A.4: SIPRI Embargoes Data (1962-2004), Multiple Imputation

	Full Model (S.E.)	Neighboring Embargo	Centroid Imports
(Intercept)	−3.81 (4.68)	1.47 (1.15)	−1.34 (4.65)
Within Country Effects			
Domestic Embargo	−0.75 (0.19)	−0.73 (0.19)	−0.56 (0.19)
Neighbor Embargo	0.58 (0.09)	0.61 (0.09)	
Foreign Interruption	−0.57 (0.22)	−0.55 (0.22)	−0.57 (0.22)
Civil War	−0.26 (0.15)	−0.28 (0.15)	−0.21 (0.15)
Neighbor Civil War	−0.08 (0.08)	−0.10 (0.08)	−0.04 (0.08)
Primary War	0.55 (0.23)	0.55 (0.23)	0.55 (0.23)
Secondary War	0.01 (0.14)	0.02 (0.14)	−0.01 (0.14)
Centroid Arms Imports	0.57 (0.09)		0.62 (0.09)
Polity	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
Real GDP per cap.	1.09 (0.09)	1.10 (0.10)	1.05 (0.09)
Between Country Effects			
Domestic Embargo	0.63 (0.90)	0.61 (0.92)	0.40 (0.91)
Neighbor Embargo	−1.25 (0.55)	−1.32 (0.55)	
Foreign Interruption	−3.61 (0.35)	−3.72 (0.35)	−3.52 (0.35)
Civil War	3.02 (1.35)	3.13 (1.38)	2.91 (1.37)
Neighbor Civil War	1.29 (0.63)	1.29 (0.64)	0.64 (0.57)
Secondary War	0.73 (1.46)	0.91 (1.50)	0.72 (1.48)
Secondary War	5.03 (1.51)	5.06 (1.50)	4.56 (1.51)
Centroid Arms Imports	0.38 (0.32)		0.19 (0.32)
Polity	0.07 (0.02)	0.07 (0.02)	0.08 (0.02)
Real GDP per cap.	1.28 (0.14)	1.35 (0.13)	1.31 (0.14)
Random Effects			
Ccode	(1.54)	(1.58)	(1.56)
Year	(0.39)	(0.68)	(0.41)
Residual	(1.65)	(1.65)	(1.66)

$n = 6466$

Random effects represent the average standard deviation over the five imputed datasets.

Table A.5: SIPRI Embargoes Data (1962-2004), Multiple Overimputation

	Full Model (S.E.)	Neighboring Embargo	Centroid Imports
(Intercept)	−5.64 (2.02)	1.42 (1.14)	−4.86 (2.06)
Within Country Effects			
Domestic Embargo	−0.68 (0.19)	−0.66 (0.19)	−0.48 (0.18)
Neighbor Embargo	0.61 (0.11)	0.63 (0.11)	
Foreign Interruption	−0.64 (0.23)	−0.58 (0.23)	−0.65 (0.23)
Civil War	−0.30 (0.14)	−0.29 (0.14)	−0.24 (0.14)
Neighbor Civil War	−0.08 (0.08)	−0.09 (0.08)	−0.04 (0.08)
Primary War	0.58 (0.19)	0.58 (0.19)	0.58 (0.19)
Secondary War	−0.01 (0.13)	0.00 (0.13)	−0.03 (0.13)
Centroid Arms Imports	0.46 (0.09)		0.51 (0.08)
Polity	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
Real GDP per cap.	1.07 (0.08)	1.07 (0.08)	1.02 (0.08)
Between Country Effects			
Domestic Embargo	0.47 (0.88)	0.70 (0.93)	0.19 (0.89)
Neighbor Embargo	−1.46 (0.54)	−1.27 (0.56)	
Foreign Interruption	−3.45 (0.34)	−3.72 (0.35)	−3.43 (0.34)
Civil War	3.26 (1.30)	3.23 (1.37)	3.20 (1.33)
Neighbor Civil War	1.22 (0.61)	1.30 (0.64)	0.45 (0.56)
Secondary War	0.23 (1.45)	0.79 (1.52)	0.30 (1.48)
Secondary War	5.52 (1.44)	4.90 (1.50)	5.09 (1.46)
Centroid Arms Imports	0.73 (0.18)		0.64 (0.18)
Polity	0.06 (0.02)	0.07 (0.02)	0.07 (0.02)
Real GDP per cap.	1.09 (0.14)	1.35 (0.13)	1.13 (0.14)
Random Effects			
Ccode	(1.49)	(1.58)	(1.52)
Year	(0.42)	(0.67)	(0.44)
Residual	(1.64)	(1.64)	(1.65)

$n = 6466$

Random effects represent the average standard deviation over the five imputed datasets.

Table A.6: Difference of Means Tests for Matched Pairs Dataset

Imputed Dataset	Paired t-test		Bootstrap mean difference	
	Mean Difference	95% CI	Mean Difference	95% CI
Dataset 1	0.40	$[0.20, 0.61]$	0.40	$[0.19, 0.61]$
Dataset 2	0.52	$[0.31, 0.73]$	0.52	$[0.31, 0.73]$
Dataset 3	0.42	$[0.21, 0.62]$	0.42	$[0.21, 0.62]$
Dataset 4	0.44	$[0.24, 0.65]$	0.44	$[0.24, 0.65]$
Dataset 5	0.42	$[0.21, 0.63]$	0.42	$[0.22, 0.63]$

$n = 2502$, $df = 1250$

Table A.7: Random effects model with matched dataset.

	Est.	S.E.
(Intercept)	-12.28	8.46
Within Country Effects		
Civil War	0.12	0.21
Primary War	1.20	0.26
Secondary War	0.06	0.23
Polity	0.03	0.01
Foreign Interruption	-0.52	0.36
Real GDP per cap.	0.96	0.24
Neighbor Civil War	0.02	0.08
Domestic Embargo	-0.79	0.16
Neighbor Embargo	0.41	0.13
Centroid Arms Imports	0.43	0.18
Between Country Effects		
Civil War	2.14	1.15
Secondary War	3.84	1.56
Secondary War	2.69	1.66
Polity	0.07	0.03
Foreign Interruption	-2.24	0.72
Real GDP per cap.	1.31	0.17
Neighbor Civil War	0.56	0.44
Domestic Embargo	-0.92	0.58
Neighbor Embargo	0.52	0.37
Centroid Arms Imports	0.88	0.57
Random Effects		
Ccode		1.67
Year		0.20
Residual		1.71

$n = 2502$