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Do Weapons Make Warfare? An Instrumental Variables Approach Towards Investigating the Relationship Between Small Arms Abundance, Civil Conflict Onset, and Civil Conflict Intensity

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Abstract

Scholars, journalists, and policymakers frequently attribute the intensity and onset of civil conflict to the abundance of small arms. However, the direction of causality has been difficult to assess due to a lack of data on the illicit small arms market and the plausibly endogenous relationship between the abundance of weapons and civil conflict. Using a new dataset of estimated small arms prices, I determine that a decrease in the price of small arms is significantly and negatively correlated with an increase in the intensity of conflict in the following year. I also determine that small arms prices increase in response to and in advance of the beginning of a civil conflict. In order to get unbiased estimates, I construct an instrumental variable, distance from disrupted stockpiles, and use it to estimate the specific effect of small arms prices in raising the risk of civil conflict onset. However, because the chosen instrument fails the exclusionary restriction the results do not provide conclusive proof of a negative causal relationship.

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1 Introduction

Robert Muggah and Jurgen Brauer once wrote that 'While conflict, aggression, and violence are not unique to humans, learning to manage and control these singularly are' (Brauer and Muggah 2006, 144). If so, a look at the last twenty years of civil war fatalities, presented in figure 1, suggests that we are in need of new management. In 2014 there were an estimated 100,000 fatalities from direct civil war violence. Most of the killing was clustered in a small number of conflicts. Over half of the fatalities, a little under 53,000, occurred from fighting in the Syrian Civil War (Lotta Themnér 2014, 540). 12,000 fatalities apiece were recorded in Afghanistan and Iraq (Lotta Themnér 2014, 540), while the remaining deaths were distributed amongst conflicts in Nigeria, Pakistan, South Sudan, Ukraine, and Somalia (Lotta Themnér 2014, 540-542). The fighting in all of these conflicts combined to make 2014 the bloodiest year in the last 20 years with respect to direct civil war violence (see figure 1).[1] The intense clustering of violence in a small number of countries is an ominous sign for those countries post-conflict future. Countries in which there are higher numbers of battle-related deaths tend to face significantly worse public health outcomes post-conflict Ghobarah et al. (2004).

The last time civil war violence rose above 40,000 battle-related deaths a year, the international community pushed a string of policy initiatives to curb the onset of civil war. Several of these initiatives targeted the abundance of small arms, the so-called "tools" of civil war (Boutwell and Klare 1999). Small arms are a category of military firearms known for their portable character, ease of use, and widespread availability. This category of weapons ranges from pistols to automatic rifles.[2] Before returning to the policies of decades past, it is valuable to review the role of small arms in instigating and intensifying civil conflict so that we know how much focus they deserve in efforts to stabilize these countries post-war.

[1] Battle-related deaths, or direct civil war violence, refers explicitly to deaths that occur in conflict events where fighting occurs between organized armed groups. This accounting of deaths does not include the deliberate massacre of civilians Gleditsch et al. (2002).

[2] There is no universally accepted definition of what a small arm is. But most scholars and policymakers agree that the category of weapons includes revolvers and self-loading pistols, rifles and carbines, assault rifles, sub-machine guns and light machine guns.

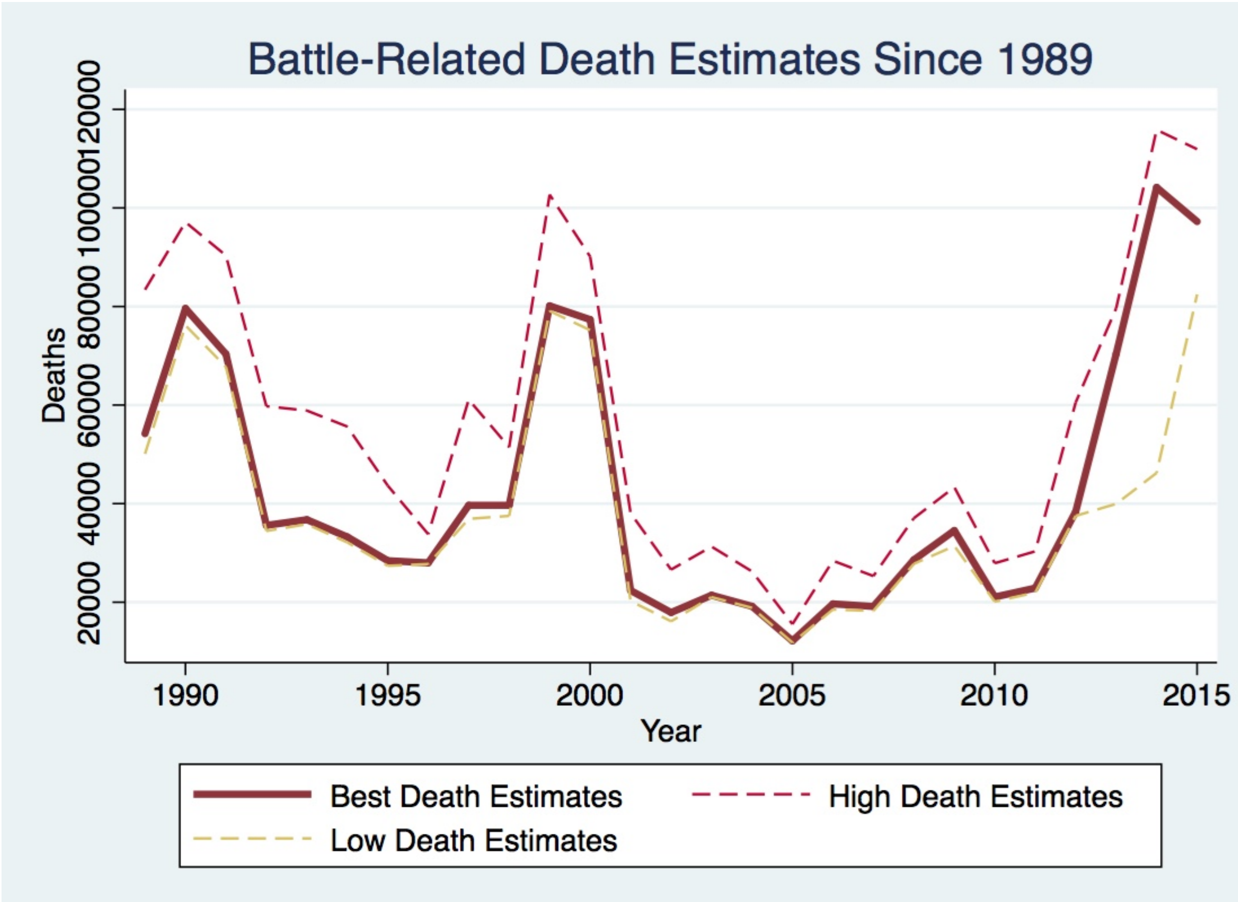

Figure 1: Deaths in Civil Wars (Gleditsch et al 2002)

Former U.N. Secretary General Kofi Annan once argued before the United Nations Security Council that small arms "survive from conflict to conflict, perpetuating the cycle of violence by their mere presence" (Vick 2001). However, even 16 years after Annan's speech his assumption, that the "mere presence" of small arms in a given region is a significant cause of conflict, is suspect. A number of case studies have noted the positive relationship between weapons abundance and civil conflict (Boutwell and Klare 1999) (Sisilin and Pearson 2001) (Strazzari and Tholens 2010) (Strazzari and Tholens 2014), but have been unable to concluded that the causality runs from the abundance of weapons to conflict.

It is difficult for case studies to establish that small arms abundance initiated a civil conflict because a civil conflict can create small arms abundance. Logically, if a conflict is ongoing then insurgents must have been able to obtain weapons. However, because the determinants of civil conflict create their own demand for small arms, market forces will often move to overcome the constraint on small arms availability. Theoretically, black market small arms dealers can anticipate a region's appetite for conflict and move stock of small arms into that region accordingly. Civil conflict creates an immense demand for weapons because it necessitates ownership of small arms either for civilian protection or so that a rebel group can participate in the conflict (Marsh 2011, 18). Countries that experience, or are about to experience, an internal conflict can thus select into a state of small arms abundance.

The militarization of the Karamojong tribe in Uganda is a case that illustrates the ambiguity of the relationship between small arms abundance and conflict.The Karamojong are a tribe of pastoral cattle-herders and foragers who live in Turkana, a district of northern Uganda that has historically been neglected by the national government. In 2011 94.3% of adults in Turkana lived below the poverty line compared to the national average of 45.9%, making it the most impoverished district in Kenya (Assesment 2014). In 1979 the president of Uganda, Idi Amin, was deposed by a rural insurgency lead by Yoweri Museveni. Amin's military forces deserted a base in Moroto leaving the armory's collection of AK-47s and ammunition to be looted by the Karamojong. It was the first time the Karamojong had access to firearms and the rapid shift from spears to guns occurred alongside a host of social changes. Elder leaders who specialized in herding were replaced by younger 'warlords' who were more interested rustling cattle from their neighbors, the Acholi. The regional insecurity raised demand for small arms and soon AK-47s flowed through the black market to both tribes. The conflict was primarily over cattle, but incidents of thievery would frequently degenerate into indiscriminate violence, specifically incidents of massacre, pillaging, and rape. Low intensity conflict between the two sides persists to this day (Mirzeler and Young 2000) (Mburu 2001).

While the conflict appears to have begun with a large shift in the abundance of weapons, understanding the influence of small arms requires accounting for each cause of conflict. The Karamojong lived in an impoverished area with few other opportunities to collect income. They experienced an episode of instability that not only provided opportunities to obtain firearms but also crippled the government's ability to control their possession and use of firearms. The primary resource in Moroto, cattle, is relatively profitable and easy to control. These two criteria, the potential for relative profits and the ease of possession, make cattle a 'lootable' resource that can transform conflict from a purely political act into a profitable venture (Grossman 1991). Finally, there were tensions built up over lifetimes of cattle-rustling between the two sides which were only interrupted by colonialism and the creation of the Ugandan state (Mburu 2001). These four factors -low income, the absence of security forces, the presence of a ‘lootable’ resource, and historical tensions- may have created both a motivation for conflict and a demand for small arms. That is why, even after the Karamojong looted the base weapons continued to flow into the region through informal channels in Sudan and Kenya. Given the number of variables at work, it is difficult to isolate the specific impact of additional small arms on the number of killings that occurred between the two sides.

The counter-factual question remains: What would have happened to the Karamojong had they never come across those first AK-47s? Would they have acquired arms and entered into a conflict with the Acholi? If the conflict was inevitable, would the fighting have been as intense if it was not for the initial cache of weapons?

We cannot compare the current state of violence to, as Robert Muggah and Jurgen Brauer put it, "a pre-gun state of fists, knives and spears" (Brauer and Muggah 2006, 144). Ideally we would asses the relative effects of there being more or less small arms on the informal market in a given geographical region. However, the black market keeps no records. Instead, I attempt to use the price of black market small arms, specifically assault rifles, as a proxy measure for how easily an individual, or an organization can acquire a weapon on the informal market. In this paper I attempt to answer two research questions. First, do lower small arms prices explain when armed groups choose to organize and enter a conflict? Second, do lower small arms prices explain when civil conflicts intensify?

In the following sections of this paper, I begin by reviewing the literature on civil conflict onset and civil conflict intensity. Next, I outline and justify the definitions that I choose to employ in coding civil conflict. Then, by using the existing literature on the role of small arms in starting and shaping civil conflict, I generate a set of hypotheses about the expected relationship between lower small arms prices, the risk of civil conflict onset, and the level of civil conflict intensity. I then test these hypotheses empirically using data on conflict onset from Kreutz (2016), data on conflict intensity from Sundberg and Melander (2013), and a new dataset of small arms prices provided graciously by Marsh and Mcdougal (2017). I find that lagged percentage changes in the price of assault rifles correlate with an increase in battle-related fatalities. Additionally, by using an instrumental variable I am able to observe a negative and significant relationship between the price of the assault rifle and the risk of civil conflict onset. I caution interpreting these results as conclusive proof of a negative causal relationship due to concerns that the instrument violates the exclusionary restriction. However, at the very least, the instrument offers a novel methodological approach that future researchers can use to asses the effects of small arms abundance.

2 The Usual Suspects, and a New One

I analyze the effect of small arms abundance on two outcomes: civil conflict onset and civil conflict intensity. Onset is the beginning of civil conflict. Studying onset means asking whether lower small arms prices make it more or less likely that a civil conflict will be initiated in a given year and in a given country. Intensity is the yearly fatality rate for people that are killed in direct combat. Studying intensity means asking why some people die more in particular conflict-years as opposed to others. Since the first cross-national empirical studies of civil war undertaken by Collier and Hoeffler (2001) and Fearon and Latin (2003), a large body of scholarship has iteratively tested and refined the suspected list of variables that raise the risk of a civil conflict onset. However, since the pioneering study of conflict intensity, Lacina (2006), only a handful of studies have sought to explain the variation in civil conflict intensity. In this section I review each literature separately and then move to a discussion on the methodology of past empirical studies that examined the effect of small arms abundance on conflict onset and intensity.

2.1 Conflict Onset

To date, the concepts that have been empirically tied to conflict onset are GDP per capita, population, resource rents, inequality, and geographic inaccessibility. Of these, population and GDP per capita, were found to be the strongest predictors of civil conflict onset while more recent scholarship has drawn attention to the role of resource rents, inequality, and geographic inaccessibility.

Population and GDP per capita were found to be strong predictors for civil conflict in the earliest studies of civil war. In the first cross-national study of civil conflict conducted by Collier and Hoeffler (2001) the authors argued that civil conflict was best explained by the existence of atypical 'opportunities to rebel,' specifically where rebellion would be more profitable (Collier and Hoeffler 2001, 564). They used data on civil conflict onset from the Correlates of War Project from

1960-1999 which coded a civil war as starting if there were more than 1,000 battle-related deaths in a calendar year. Using a logit model the authors found that civil conflict onset was associated with a low per capita GDP, a large population, and countries where the economy is reliant on natural resources, as measured in primary commodity exports (Collier and Hoeffler 2001, 586). Collier and Hoeffler (2001) explained the relationship between onset and income using a theory proposed by Grossman (1991), that a higher wage in the productive sector raises the opportunity cost that each individual faces when they choose to become a fighter (Collier and Hoeffler 2001, 564). Consequently, the incentives offered by a rebel group, and the costs they incur to offer those incentives, need to be stronger. Population, the authors theorized, raised the risk of civil conflict by giving insurgents a larger pool to recruit from.

There is still a substantial debate over the precise mechanism through which per capita GDP effects the risk of civil conflict onset. Fearon and Latin (2003) argued that GDP per capita is significant because it correlates with the capital and technology that a state has to repress rebellion and buy-off the opposition (Fearon and Latin 2003, 17). Their data came from an expanded dataset of civil conflicts that includes data from the Correlates of War Project, the State Failure Project, and the Institute for International Studies. While Fearon and Latin (2003) disagree with Collier and Hoeffler (2001), their study did confirm the significance of per capita GDP and population in raising the risk of civil conflict onset.

Another important idea to emerge out of Collier and Hoeffler (2001) is that the rents that armed groups obtain from natural resources provide a form of conflict financing (Collier and Hoeffler 2001, 565). The precise causal mechanism that relates natural resources to the risk of civil conflict onset is widely debated. Ross (2006) and Humphreys (2005) narrowed the measure of natural resources in Collier and Hoeffler (2001) to the rents obtained by extracting minerals and oil. They both found (Ross through case studies and Humphereys through an update to Fearon and Latin's model) that resource rents provide financing opportunities in the form of "booty shares" (Ross 2004, 57). Booty shares are the rights to the natural resources in question that rebel groups can sell pre-conflict to international companies in order to raise funds for the conflict. These rights can be sold even before an armed group controls the resources in question (Ross 2004, 57).

Humphereys (2005) also argues that natural resource rents lower the marginal cost of rebellion through the "structural change" they cause to a country's economy (Humphreys 2005, 512). When an economy is dependent upon natural resources, it suffers from what is known as "Dutch disease." Dutch disease occurs when a rising exchange rate weakens a country's manufacturing sector, lowers internal trade in the country, and raises the country's reliance on foreign imports (Humphreys 2005, 513). With a weaker manufacturing sector average incomes decline and it becomes cheaper for an insurgency to draw recruits away from formal employment.

Fearon and Latin (2003) also showed how countries with a larger share of geographically inaccessible territory are more likely to experience a civil conflict onset. The authors argued that it is easier for rebels to organize in geographically distant regions where it is harder for central government forces to reach. Fearon and Latin (2003) tested this hypothesis by using A.J Gerrard's dataset of the percentage of mountainous territory in each country. Their results showed that a larger share of mountainous territory is positively correlated with the risk of a civil conflict in a given country. Recently Schutte (2015) argued that territory which is hard for the state to access may also be culturally distinct from the rest of the country and may have a higher incentive to rebel (Schutte 2015, 1118). However, her empirical work focused more on the odds of victory and intensity, as opposed to the risk of civil conflict onset.

The final explanation for civil conflict onset is inequality. In theory, two types of inequality motivate the onset of a civil conflict. Vertical inequality, better known as income inequality, generally measures the ratio of the median income to the mean income. If a small subset of individuals earn a dramatically higher income than the average person, then income inequality is very high. In theory, the existence of income inequality incentivizes individuals to join a rebellion due to both the perceived injustice and because they stand to gain more should the incomes be more equitably distributed. However, Collier and Hoeffler (2001) and Fearon and Latin (2003) both found that income inequality, measured by the GINI coefficient, was an insignificant predictor of civil conflict onset. Their null findings have in part been ascribed to the lack of data on the GINI coefficient, so the theory has yet to be entirely dismissed (Fearon and Latin 2003, 26).

Horizontal inequality, unlike vertical inequality, looks at inequality distributed across group identities as opposed to individuals. The group identities studied tend to be oriented around ethnicity, religion, or national heritage. In theory, if one of these groups systematically has lower incomes compared to the average income level in the country, it is easier for the groups to mobilize. The groups can take advantage of a shared grievances and can mobilize around the group identity

(Buhaug et al. 2014, 421).

Buhaug et al. (2014) created a measure of horizontal inequality by combining two geocoded datasets, Ethnic Power Relations and G-Econ, in order to approximate the relative wealth and political status of ethnic groups in each country (Buhaug et al. 2014, 423). They then calculated negative horizontal inequality, or difference between the GDP controlled by the poorest ethnic group and the GDP controlled by the wealthiest ethnic group, and regressed it against civil conflict onset. Their results demonstrated that negative horizontal inequality is correlated with a higher risk of civl conflict onset (Buhaug et al. 2014).

2.2 The Intensity of Conflict

The intensity of conflict is the number of people who die yearly in combat incidents that occur between two armed groups. It is otherwise referred to as yearly battle-related deaths (Esteban et al. 2012, 1315). Only recently have developments in data collection, specifically the creation of the UCDP battle-related deaths dataset in 2010, permitted the use of this measure of conflict in empirical work. Past studies of battle-related deaths relied on measures that were conceptually similar, but imperfect.

Lacina (2006) wrote the first study that attempted to find the correlates of civil conflict intensity, and later studies have refined her work. Lacina (2006) examined the severity of conflict, which is the total number of battle-related deaths in a given conflict over the entire course of the conflict. As opposed to the measure of intensity that I use, deaths per year, severity takes into account the duration of conflict; thus, her theories will not perfectly map onto the work of this paper. Today, the agreed upon correlates of conflict intensity are GDP per capita (Chaudoin et al. 2017), regime type (Lacina 2006), foreign support (Lacina 2006), ethnic divisions (Esteban et al. 2012) (Buhaug et al. 2014), mineral and oil resources (Weinstein 2007) (Balestri 2012), foreign support (Lacina 2006; Weinstein 2007), and geographic accessibility (Schutte 2015).

Lacina (2006) and Chaudoin et al. (2017) theorize that population and per capita GDP affect the intensity of conflict the same way that they affect the risk of onset. Lacina (2006) first tested this theory and found a negative but insignificant relationship between GDP per capita and battle-related deaths. Chaudoin et al. (2017) later used a two-staged least squares model where per capita GDP was instrumented by a lag of itself in order to account for the endogeneity between conflict and diminished economic growth. The authors found a negative and statistically significant relationship between civil conflict intensity and GDP per capita.

One of the robust findings to emerge from Lacina (2006) was that civil conflicts in democracies are much less intense than those that occur in autocracies (Lacina 2006). Lacina (2006) theorized that there were three possible reasons for this finding. First, democratic leaders are under domestic pressure to avoid starting costly conflicts. Second, once conflict starts democratic norms pressure leaders to use restraint when capturing the enemy and controlling their territory. Finally, democracies are often better able to co-opt the opposition by giving them political power in the legislature so the effects of conflict are often less pronounced (Lacina 2006, 282-283).

Lacina (2006) tested a widespread theory about the role of ethnicity in conflicts: that conflicts in more ethnically divided countries are "zero-sum," causing the fighting to be more frequent, brutal, and sustained (Lacina 2006, 283-284). Surprising for Lacina (2006), the OLS model returned a negative and significant coefficient on the ethnolinguistic fractionalization index that she used to measure ethnic polarization (Lacina 2006, 286).

More recent scholarship has critiqued this finding as an artifact of a flawed measure of ethnic polarization, the ethnolinguistic fractionalization index (ELF) (Esteban et al. 2012, 1311). The ELF is the probability that two randomly selected people from a given country come from different ethnolinguistic backgrounds. It is therefore a better measure of diversity, than it is a measure of polarization, which implies that there are grievances between ethnic groups. Esteban et al. (2012) refined the work of Lacina (2006) with regards to ethnicity by controlling for the conflict type (either over public or private goods) and the cohesion of each ethnic group (Esteban et al. 2012, 1317-1318). Esteban et al. (2012) found that, as long as ethnic cohesion is high, ethnic fractionalization has a positive effect on intensity in conflicts over private goods, whereas ethnic polarization has a positive effect on intensity in conflicts over public goods (Esteban et al. 2012, 1319).

Weinstein (2007) argued through case studies of four separate conflicts that rebel access to "economic endowments" makes rebels more indiscriminate in their application of violence because they do not need to rely on civilians for recruits and economic support (Weinstein 2007). Weinstein's argument points to two relevant variables: natural resources and foreign support. Balestri (2012) validated Weinstein's argument in finding that fighting in Africa is significantly more intense near gold mines. The findings of Lacina (2006) also support the argument of Weinstein (2007) by pointing to a large effect of foreign support on conflict intensity.

Geographic inaccessibility is thought to have an opposite effect on conflict intensity than the effect it has on conflict onset. While people in geographically inaccessible areas are more likely to rebel than those closer military bases or centers of power, they are also less likely to have intense fighting simply because it is difficult for troops to find and confront insurgents. This hypothesis was confirmed by Schutte (2015) using a measure of geographic inaccessibility called the "Territorial Balance Indicator" (TBI) that measured the distance between population centers in a given country (Schutte 2015, 1113-1117). While the TBI would be the ideal measure of geographic inaccessibility, it is missing five years from the range of data I use. Instead I use road density, which is the percentage of territory covered by roads as used by Buhaug and Rød (2006).

2.3 Small Arms and Conflict Onset

Grossman (1991) was the first to assert that a lower cost of conflict capital would decrease the costs associated with civil conflict and raise the risk of occurrence (Grossman 1991). Collier and Hoeffler (2001), lacking data on the abundance of weaponry, used time since last conflict as a proxy for the availability of 'conflict specific capital' (Collier and Hoeffler 2001, 589). While the variable was a significant predictor of conflict onset, it provided no tangible measure of small arms abundance and failed to explain how weapons could become available in countries that had gone decades without experiencing a civil conflict.

Killicoat (2006) was the first, and so far only, scholar to take a quantitative approach to analyzing the relationship between small arms and conflict by using price data on the AK-47. A longstanding practice in conflict journalism has been to note the price of weapons as a way of assessing the regional appetite for conflict. Using over 9,000 articles collected by the Norwegian Institute of Small Arms Transfers noting black market small arms prices, Killicoat (2006) designed the first cross-country, time-series database of black market AK-47 prices with 335 price observations across 117 countries (Killicoat 2006, 6).

Using the database of small arms prices, Killicoat (2006) estimated three regression models of civil conflict onset using the logit model first used in Collier and Hoeffler (2001). In order to make the price data compatible with Collier and Hoeffler (2001), the observed prices were used to calculate five-year AK-47 price averages for given regions. A particular issue that Killicoat (2006) addressed was the countervailing effects of small arms supply and small arms demand. In the first year of conflict Killicoat (2006) noted that average weapons prices tend to rise because demand for small arms outstrips supply (Killicoat 2006, 61). In other words, when a civil war is looming, people rush the market for small arms, causing the black market price to rise. The reverse causality between the anticipation of civil conflict and small arms prices would cause coefficient estimates to be positive. An increase in the price of the AK-47 in a country-year is associated with an increase in the risk of conflict only because in that country-year the demand for weapons is acutely represented in the weapons price. It also constitutes an endogenous relationship that prevents an accurate estimation of the causal effect of weapons prices on the risk of civil conflict onset.

In order to isolate the effect of price on civil conflict onset, Killicoat (2006) estimated models using a lagged price of small arms and an instrumental variable. The instrumental variable estimated the price of small arms based on distance from Moscow, the existence of a civil war in an adjacent country, and the average military expenditure of neighboring countries, all of which Killicoat (2006) found to be correlated with the price of small arms in a given country (Killicoat 2006, 53). Those price estimates were then used in estimating the effect of small arms prices on civil conflict onset. In theory, these variables, unlike the price data, are exogenous to civil conflict onset, so the effect of the estimated prices on the risk of civil conflict onset, is also unbiased. Both the lag and the instrumental variables approach yielded negative and significant estimated coefficients. At the mean of all other control variables, Killicoat (2006) found that a \$100 decrease in the price of the AK-47 raised the risk of conflict onset by 0.5% in his dataset (Killicoat 2006, 64-65).

While pathbreaking, Killicoat (2006) suffers from flaws inherent to the dataset the paper inherited from Collier and Hoeffler (2001). The first issue is that the unit of analysis in Killicoat (2006), as it is in Collier and Hoeffler (2001) is country-five years. This broad choice of time period introduces additional layers of measurement error by obscuring potentially large swings in relevant explanatory variables within the five-year periods. For instance, in the data I use provided by Marsh and Mcdougal (2017) the median absolute value of the yearly percentage change in small arms prices is 32%. Since the variation in conflict onset is so often a question of when it occurs, it is important to use the most precise unit of time possible in order to ascertain its correlates.

The second issue is that Killicoat (2006) uses conflict onset data from the Correlates of War (COW). The COW has been accused of inconsistently applying of coding rules (Hegre and Sambanis

2006, 817-819), and uses a 1,000 battle-related deaths threshold to determine conflict onset. The threshold was used in the past to distinguish a civil war from a civil conflict. However the difference between the two levels of conflict has been shown to be arbitrary (Hegre and Sambanis 2006). As I argue in the next section, if we want to determine when a conflict is most likely to start, we need to use the most sensitive threshold: the first death. Otherwise it is difficult to distinguish between when conflicts start and when conflicts intensify.

2.4 Small Arms and Conflict Intensity

Research on the relationship between small arms and conflict intensity, on the other hand, is as Nicolas Marsh put it, "dominated by case studies and anecdotes" (Marsh 2011, 26). These case studies are informative and provide strong theoretical priors about how small arms availability might affect the intensity of civil conflict. Regretfully, because case studies rely on subjective observation of the events in the conflict, they cannot account for potential reverse causality as done in Killicoat (2006).

There are differing accounts of the relationship between price and battle-related deaths. Killicoat (2006) found that AK-47 prices prices tend to stabilize after the spike that occurs with conflict onset; however, Florquin (2014) found in observing ammunition prices in Lebanon that they tended to spike in proximity to battles between rebel groups in Syria (Florquin 2014). These two observations are not entirely incompatible because ammunition prices and small arms prices may follow different processes. Ammunition prices are naturally more sensitive to conflict dynamics than the price of the weapons themselves since ammunition needs to constantly be purchased and replenished in order to sustain fighting. Small arms, on the other hand, are theoretically a durable good in that, once bought, can be re-used for an extended period of time. Still, it is plausible that the appearance of conflict in a new part of the country could spike demand if enough residents did not previously acquire weapons.

There is also more than one mechanism for conflict intensity to affect arm prices. Nicolas Marsh (2011) argued the following:

"Groups acquire arms in order to implement a particular military strategy, but in doing so they effect their ability to acquire arms. For example if an insurgent group obtains extra arms supplies its military potential could be improved. It might then be able to capture more territory containing resources that can be exploited or people that can be taxed. The group then has access to more funds which can be used to purchase more weapons. It is therefore difficult to apply a distinction between a dependent and independent variable in the relationship between arms flows and conflict dynamics" (Marsh 2011, 25-26).

In essence, due to the number of different channels through which conflict and weapons influence each other, causality cannot be assumed as it is in case studies.

To my knowledge, there are no quantitative studies of small arms availability and conflict intensity. However, Moore (2012) used quantitative methods to study the impact of light weapons[3] on the intensity of conflicts. The author specifically tested a theory of Sisilin and Pearson (2001) that access to more advanced weaponry would have a escalation effect on the conflict by forcing government forces to increase their combat operations in response to rising insurgent capabilities (Moore 2012, 334). Moore (2012) used the SIPRI arms transfers database to test the effect of transfers on yearly battle-deaths. The effect of additional light weapons on conflict intensity could differ from the effect of additional small arms because light weapons are a more scarce commodity to insurgent groups. Nevertheless, his results suggest that increased supplies of light weapons does cause conflicts to escalate.

Like most studies of weapons and conflict, the results suffer from unresolved bias: civil wars that are more intense can plausibly select into external support and foreign arms transfers. Often support is only given to conflicts where one country's ally is losing, which implies a higher level of intensity (Lacina 2006). Moore (2012) argues that this endogenous relationship applies mostly to cases where arms transfers overlap with intervention. He attempts to reduce endogeneity by studying general arms transfers to states including those without domestic conflict (Moore 2012, 333). However, the same treatment cannot be applied to transfers to rebel groups, which are fundamentally always in a conflict with the state.

[3]Light weapons are a separate category of weapons. They tend to be heavier than small arms but are also portable. A useful heuristic I use to differentiate between the two is that generally a small arm can be carried in one arm but a light weapon requires two arms. A classic example of a light weapon is a hand held rocket launcher.

3 Operationalized Definitions of Onset and Intensity

One of the defining features of civil conflict research is the use of ad hoc coding rules and the absence of clear coding definitions. Before proceeding to the theory section, I first overview my working definition of civil conflict onset and civil conflict intensity so that it is clear how they relate to the theories and hypotheses.

3.1 When Does a Civil Conflict Start?

In overviewing the major civil conflict datasets and lists available at the time, Sambanis (2004) claims there are three important questions to answer when coding civil conflict:

> "What threshold of violence distinguishes civil war from other forms of internal armed conflict? How do we know when a civil war starts and ends? How can we distinguish between intrastate, interstate, and extrastate wars" (Sambanis 2004, 815).

For instance, the definition used by the armed conflict database is:

> "A contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in a calendar year" (Gleditsch et al. 2002).

Gleditsch et al. (2002) specifies this definition to cases of civil war by the nature of the "contested incompatibility." In the case of internal conflict the incompatibility is "concerning the status of a territory" that is considered to be juridically sovereign (Gleditsch et al. 2002, 3). I make one major revisions to this coding definition: I use the first death in a conflict to determine when the conflict starts.

My definition of civil conflict is the most useful for attempting to predict when an insurgent leader will choose to enter into and escalate a conflict. A good comparison is the work of Harff (2003). She chooses to depart from the definition of genocide used by human rights scholars to include the intent to commit genocide as crucial to distinguishing genocide from other forms of violence (Harff 2003, 60). She made this choice so that her definition would be connected to the choices of actors undertaking acts of genocide and would be better able to predict where and when genocide would occur. I take a similar set of considerations when it comes to the study of civil conflict. While past studies are attuned to the violence of conflict itself, I focus on the choices that relevant actors make which have implications for the direction of conflict.

In the case of civil conflict, that action we are trying to predict is an insurgent leader's decision to organize an armed group and enter into a conflict with the state. This is the choice that I try to evaluate when studying conflict onset. This decision involves weighing the costs and benefits of entering conflict, as well as the probability of victory. Once the decision is made, an insurgent leader will need to pay the upfront costs of organizing fighters, organizing supply networks, and purchasing weapons. Violence does not necessarily begin once the decision is made, and it certainly does not always begin with a significant intensity. Zedong (1989) says that, in the initial stages of conflict, a rebellion should be focused on getting recruits as opposed to fighting.

Naturally, the decision to enter into a conflict is not made transparently.[4] Given the data that is available, the best way to observe when the decision to begin a conflict is made, is to note when the fighting begins. Therefore, I use the most sensitive battle-related deaths threshold to judge when a civil conflict starts: the first recorded battle-related death.

3.2 Expanding the Definition of Intensity

I consider battle-related deaths from conflicts between non-state groups to be akin to battle-related deaths from conflicts between non-state groups and the state. The standard definition of conflict intensity only examines battle-related deaths in conflicts between rebel groups and the state (Gleditsch et al. 2002). The state in every case is the actor that is internationally recognized by the United Nations as having a 'monopoly' on the legitimate use of force for a defined territorial extent. These states are said to have 'juridical sovereignty' simply because other states have judged them to be sovereign (Krasner 1999, 14).

It is a poor measure of concept to use juridical sovereignty to define the exact actors that must be present in a conflict for it to be considered a civil conflict. In theory these states should be empirically sovereign and have 'the ability to defend and protect a defined territory, the existence of an established government, and the presence of a population' (Krasner 1999, 14). In practice, the only criteria they universally meet is that other countries thought it to be politically beneficial to

[4]However, many rebel groups publicly declared their opposition to the state. A dataset that coded conflict by the declaration of intent from rebel groups might provide a better level of specificity.

act as if they have empirical sovereignty (Krasner 1999, 15-17). Consequently, in many a juridically sovereign state there are non-state actors who play the same role as states by controlling the territory through military force whilst providing governance and basic services (Mampilly 2011).

These rebels often end up fighting other rebels. Modern examples of this phenomena are in the current civil war in Syria where the Islamic State is fighting not only the government of Syria but also a larger coalition of non-state armed groups such as Kurdish forces and Jaysh al-Fateh. The fighting that takes place between these non-state groups follows similar trends as the fighting between the Syrian state and the same non-state groups. The only systematic exception is that juridically sovereign states tend to have more resources due to foreign aid and access to the formal weapons market (Krasner 1999, 16).

As was the case when discussing the theoretically appropriate measure of onset, I chose my definition of civil conflict intensity here to make it more predictive of the number of battles-deaths that occur from organized fighting in a given conflict. Creating a model of civil conflict that only accounts for deaths that occur from fighting between organized armed groups and the state would not capture the full effect of a drop in small arms prices on the intensity of civil conflict. It would systematically eliminate the effect of a drop in price on the intensity of fighting between rebel groups. I adjust my definition of intensity to also account for the battle-related deaths that occur from fighting between organized non-state armed groups that espouse political goals for the control of territory.

Notably, this does not apply to my definition of civil conflict onset because, in theory, when each non-state armed group organizes, it enters into a conflict first and foremost with the state. If the armed group is indeed organized and claims control over territory, then the Uppsala/PRIO coding rules demarcate a conflict onset because there is an implicit incompatibility between the armed group's political position, and the position of the state (Gleditsch et al. 2002).

4 Theories of Weapons and Warfare

4.1 Theory of Small Arms and Conflict Onset

The past literature tends to explain the relationship between small arms availability in economic terms. As the small arms become more abundant, and their price on the second hand market decreases, civil conflict becomes cheaper for rebels to engage in and therefore more likely to occur. I refer to this as the "signal" effect based on the words of Killicoat (2006). Rebels forecast the affordability of conflict based on a change in small arms prices, but do not know the future prices. While I accept the hypothesis that economic theory provides, I take care to elaborate and evaluate its assumptions. In order to posit a positive effect of small arms abundance on the risk of conflict onset in all cases, we need to assume that the decision to rebel includes a cost-benefit calculation.

In instances of civil war the state is a priori assumed to have monopoly on the "legitimate use of physical force within a given territory" (Weber 1919). Sovereignty gives states direct access to the formal weapons market, and the public funds needed to purchase crucial weapons systems. In order to mount a challenge against the state, a rebel armed group needs access to weaponry that are at least "as good as the enemies" in order to compete for control of the state's territory (Young 1996, 13). If the high cost of obtaining such weaponry is impeding would-be-rebels from initiating a conflict with the state it would be what economists call a "barrier to entry" (Robert Pindyck 2013). Small arms help to circumvent this cost barrier by being relatively lightweight, easy to use, and available on the informal market (Boutwell and Klare 1999). Part of what makes them appealing to rebels is that they are lightweight and thus enable a lower cost mode of warfare called insurgency. Using insurgency rebels default to guerilla tactics in which they stay on the move and opt for brief raids and acts of sabotage instead of confrontations with other armed groups (Fearon and Latin 2003).

For the signaling effect to induce a civil conflict we need to assume that rebels are making cost-benefit calculation when they decide whether or not to initiate a conflict with the state. Grossman (1991) takes this assumption and models conflict as a contest between two groups over each other's resources. His model predicts that the diminished cost of "military technology" raises the risk that conflict will occur. This approach to civil conflict onset seems to treat a rebel armed group like a firm that decides to enter a market when it is profitable to do so. Logically, when the cost of inputs is cheaper for a given endeavor then the endeavor will be more profitable. Collier and Hoeffler (2001) captures this logic in asserting that if "conflict-specific capital" is unusually cheap, the prospect of civil conflict will increase because armed group are more able to turn a profit from control of natural resources as well as the ability to loot and tax (Collier and Hoeffler 2001, 569). However, one does not need to assume that all rebel are profit seekers in order to expect that cheaper small arms can instigate a civil conflict.

All that we need to assume is that insurgent leaders are strategic and attempt to maximize the chance of successfully challenging the state. No matter what outcome the leader seeks from a conflict, be it secession, control of the government, or the chance to control a gold mine, lower small arms prices will influence their decision to enter into a conflict if they wants to maximize the probability of success. It is useful to disaggregate the decision to enter into a conflict as one made by an insurgent leader. While recruits can come and go, insurgent leaders are innately associated with rebellion and thus carry the costs of failure post-conflict. Empirical evidence favors this assumption. Prorok (2015), for instance, shows that rebel leaders will extend the duration of conflict in order to avoid being punished afterwards.

As long as leaders want to achieve a rebellions desired outcome they will be concerned with the costs. What separates a rebellion from a riot, or an act of communal violence is the level of organization, or as Weinstein (2007) would put it, 'hierarchy.' In fact, I borrow the description of rebellion used by Weinstein (2007):

> "Individuals who share the goal of overthrowing an existing regime contract with one another to invest their time, energy, and resources in a military campaign. Mounting an effort requires negotiating access to ammunition, food, shelter, and intelligence from a host of outside parties. Organizing hierarchically reduces the cost of establishing all of these relationships to maintain the group" (Weinstein 2007, 49).

This logic promotes a similar conclusion as (Collier and Hoeffler 2001): an armed group enters into a conflict when its leaders perceive that the costs of organizing are either worth the gains or are not high enough for insurgent leaders to mount a successful rebellion. If incomes are lower, then leaders know that it will cost less to draw in recruits (Collier and Hoeffler 2001). If the leader's ethnic group is economically disadvantaged then communal ties can help mobilize funds from the diaspora to shoulder the initial costs (Buhaug et al. 2014). If a state is weak, the leaders know that each military operation can potentially claim more territory (Fearon and Latin 2003). If a country is openly hostile to rival armed groups, the state or otherwise, the leader might assume that a foreign patron would be willing to shoulder the costs of weapons (Florquin and Khakee 2005, 172). As Killicoat (2006) says, if the price of small arms is relatively cheaper, it acts as a 'signal' to potential leaders that conflict is more feasible (Killicoat 2006, 63).

In actuality, there are a number of instances in which rebellions began in circumstances of extreme small arms scarcity. In 2005, the Tuareg ethnic group began a rebellion in Mali with reportedly one AK-47 (Florquin and Khakee 2005). Similarly in Nepal in 1996 Maoist rebels began with only kukri machetes (Marsh 2007). In both instances the groups got by despite being unable to purchase small arms because they were highly organized and able to steal weapons from the state (Marsh 2007). These cases suggest that it is not necessary to purchase small arms on the informal market for a group to rebel. Lower small arms prices can still raise the risk of civil conflict onset on average, just not in every instance.

Hypothesis 1: If the price of small arms is lower then there is an increased risk of civil conflict onset.

4.2 Theories of Small Arms and Conflict Intensity

The literature on small arms abundance and conflict intensity describes roughly three different effects. First, the fueling effect replicates the same classical economics logic that was employed in the signal effect and applies it to conflict intensity. Next, the escalation effect describes how small arms abundance affects the strategic calculus of an ongoing conflict. Finally, the fragmentation effect explains how small arms abundance shapes the internal dynamics of armed groups.

The fueling effect is when a non-state armed group engages in more battles because weapons availability no longer constrains their ability to field fighters. This theory is described most aptly by Mao Zedong in his book on guerilla warfare. Zedong (1989) argues that insurgency operates in four distinct phases. In the first phase, after the onset of conflict the insurgency should focus on "persuading" people to its cause, while keeping its attacks on state forces to be acts of terrorism and sabotage (Zedong 1989, 20). In phase two these guerrilla style attacks should continue while the insurgents acquire the necessary weapons and equipment to escalate the conflict. After the acquisition of weapons, the conflict proceeds into phase three where combat intensifies and the insurgency is confronting government forces out in the open (Zedong 1989, 22-24).

Ironically this idea also follows from the analogy of a firm. Like a firm, an insurgent group increases its output when the specific inputs to conflict, fighters (labor) and weapons (capital) are cheaper. Some scholars note that there are diminishing returns for the fueling effect. According to Marsh (2011) "Once a group has crossed the excess threshold –arms in addition to those that can be employed by combatants 'weapons will not intensify the conflict" (Marsh 2011, 34). Weapons, after all, are wielded by fighters. Marsh argues that after some threshold an armed group will no longer have any additional fighters to arm, and any further increases in small arms abundance or decreases in the price of small arms simply allow the group to stockpile weapons. Still the prediction that this effect yields is that an increase in small arms abundance, or a decrease in the price of small arms, leads to an increase in conflict intensity.

The escalation effect makes the same prediction but by a different logic. Sisilin and Pearson (2001) argue that sudden shifts in arms abundance cause armed actors to escalate fighting in response to other actors acquiring weapons. The authors supported their claim with case studies of the civil conflicts in Rwanda, Bosnia, Angola, and Siri Lanka (Sisilin and Pearson 2001, 63). This theory explains the effect of an increase in small arms on conflicts where there is either very low intensity fighting or a ceasefire. Following the rise in small arms abundance, each armed group observes the other group's rapid acquisition of weapons and attempts to preemptively break the ceasefire. The dynamic is reminiscent of offensive realist depictions of the security dilemma. Every actor is insecure about both its own position as well as the future actions of other actors and consequently tries to compensate by acting offensively. Both the fueling effect and the escalation effect propose the following hypothesis:

Hypothesis 2: A decrease in small arms prices will lead to an increase in battle-related deaths.

The final effect, the fragmentation effect, proposes a mechanism through which small arms abundance could decrease the intensity of civil conflict. Marsh (2007) proposed that the level of arms availability determines the scale of rebellion in a given country. In Marsh's argument the mode of insurgency corresponds to the level in an insurgent's chain of command, which controls access to small arms.

When the weapons are hard to acquire, the insurrection will more likely feature one coordinated opposition to the state that is organized enough to seize weapons from the government (Marsh 2007, 63). Because in this instance the weapons are scarce and held by a highly organized group, weapons are managed by the highest level in the chain of command so that they can be most efficiently allocated. When weapons can be acquired, provided that an individual has access either to black market connections or to a foreign patron, weapons tend to be controlled by the regional commanders with the necessary connections. In this mode the insurrection is characterized by a decentralized power structure that Marsh (2007) refers to as "warlordism." When small arms are plentiful the weapons tend to be owned at the bottom of the chain of command, by individual fighters. These insurgencies tend to be characterized by "packs, often remarkably small ones, of criminals, bandits, and thugs" (Marsh 2007, 62). Marsh (2007) drew his theory from a case-study analysis of civil conflicts in Iraq and Siri Lanka, but other authors have extended his analysis and found it explaina conflicts in Nigeria (Duquet 2009), Gaza (Strazzari and Tholens 2010), and Libya (Strazzari and Tholens 2014).

Given that more organized insurrections tend to exclusively focus on control of the state (Weinstein 2007), Marsh‘s theory suggests that higher small arms prices are associated with a shorter more disciplined conflict and more intense conflict. Lower small arms prices are then associated with protracted low intensity conflict. Weinstein (2007) describes how the character of an armed group is often determined by its ability to discipline the use of violence by its recruits. As Marsh (2007) explained, weapons abundance makes it harder for insurgent leaders to control what their recruits use their weapons on. In a case study of four different rebel groups Weinstein (2007) found that armed groups that fail to restrain the use of violence by their recruits lose the trust of civilians and tend to be swamped by recruits who prioritize short term profit over long term political gain. These groups are less likely to confront the state forces because their main concern is the acquisition of short-term profits through looting, which implies that they direct their violence towards civilians.

What this theory suggests is circumstances of small arms abundance shape the internal structure of armed groups in such a way that they are no longer oriented towards political change, but toward short-term profit. This shift in the goals disincentives confrontations with military personnel and thus would decrease the yearly intensity of conflict.

Hypothesis 3: If the price of small arms decreases in a given conflict, then there will be a decrease in battle-related fatalities.

5 Data and Coding Criteria

My methodology differs from past empirical approaches towards studying small arms and civil conflict in four notable ways. First, I make use of a brand new dataset provided by Marsh and Mcdougal (2017) that gives country-year estimates for every year since 1960 (although my sample of conflicts begins in 1989). This dataset allows me to use country-year as my unit analysis. Killicoat (2006) was unable to refine his unit of analysis partly because there were not enough exact year observations of AK-47 prices that lined up with conflict onsets. Second, I am the first study to apply data on small arms to an empirical investigation of civil conflict intensity. Third, I clearly specify and justify my coding criteria between civil conflict onset and civil conflict intensity. Finally, I create an instrumental variable, distance from disrupted arms stockpiles, and attempt to use it to get an unbiased view on the effect of small arms prices on civil conflict onset.

5.1 Pre-Analysis Plan

I wrote a pre-analysis plan (PAP) in preparation for this thesis. In it, I specified my hypotheses, the data I intended to use, and the methods that I intended to use. The goal of a PAP is to be transparent about how my research design changes before and after I begin estimating regressions. The PAP provides assurances that I am not p-hacking and requires me to label all findings from models that were not pre-registered as inductive.

I made numerous changes to my models since submitting my PAP, so the bulk of my findings should be considered inductive. I few notable changes were made because of the knowledge of available data. For instance I moved from measuring onset as the 25th death to measuring onset as the 1st death when I learned about the UCDP Conflict Termination dataset (Kreutz 2016). Other changes were made because I learned that certain specifications did not made sense. For instance I dropped fixed effects from most of my models because they either could not be included, or because their introduction did not affect the coefficient of interest. For access to the PAP and the original model specification please e-mail my advisor, Professor Lisa Mueller at lmueller@macalester.edu.

5.2 Data on Civil Conflict Onset

I constructed my dataset of civil conflict onsets from the UCDP/PRIO Conflict Termination Database (CTD) (Kreutz 2016). Most of the previous literature uses the Armed Conflict Database (ACD). I prefer the conflict termination database because it includes the year where the first battle-related deaths was recorded unlike the ACD which only records the first year where researchers observed more than 25 battle-related deaths.

As discussed in my theory section I use the following modified version of the Uppsala/PRO armed conflict definition. A civil conflict is 'a contested incompatibility that concerns government and/or territory' of a juridically sovereign state and that begins with the first recorded battle-related death, ends only when one side is demobilized, and in which at least one party to the conflict is a non-state armed group (Gleditsch et al. 2002). Using the details provided in the CTD I coded onset as the year when the first death occurred in a conflict between an organized armed group and the state.

I use two conflict types; internal and internationalized internal which are conflict types 3 & 4 in the ACD codebook. An internal conflict is simply put, a civil war. An internationalized internal conflict is a civil war in which third party countries provide material support to the actors in the civil war. A country-year is assigned a 1 if a conflict began in it and a 0 if no conflict began in it. In the dataset there are 110 instances of a conflict being initiated and 4595 country-years where no conflict was initiated from 1989-2015 (Kreutz 2016). [5]

[5] An internal conflict is conflict type 3 in the database, an internationalized internal conflict is conflict type 4 in the database

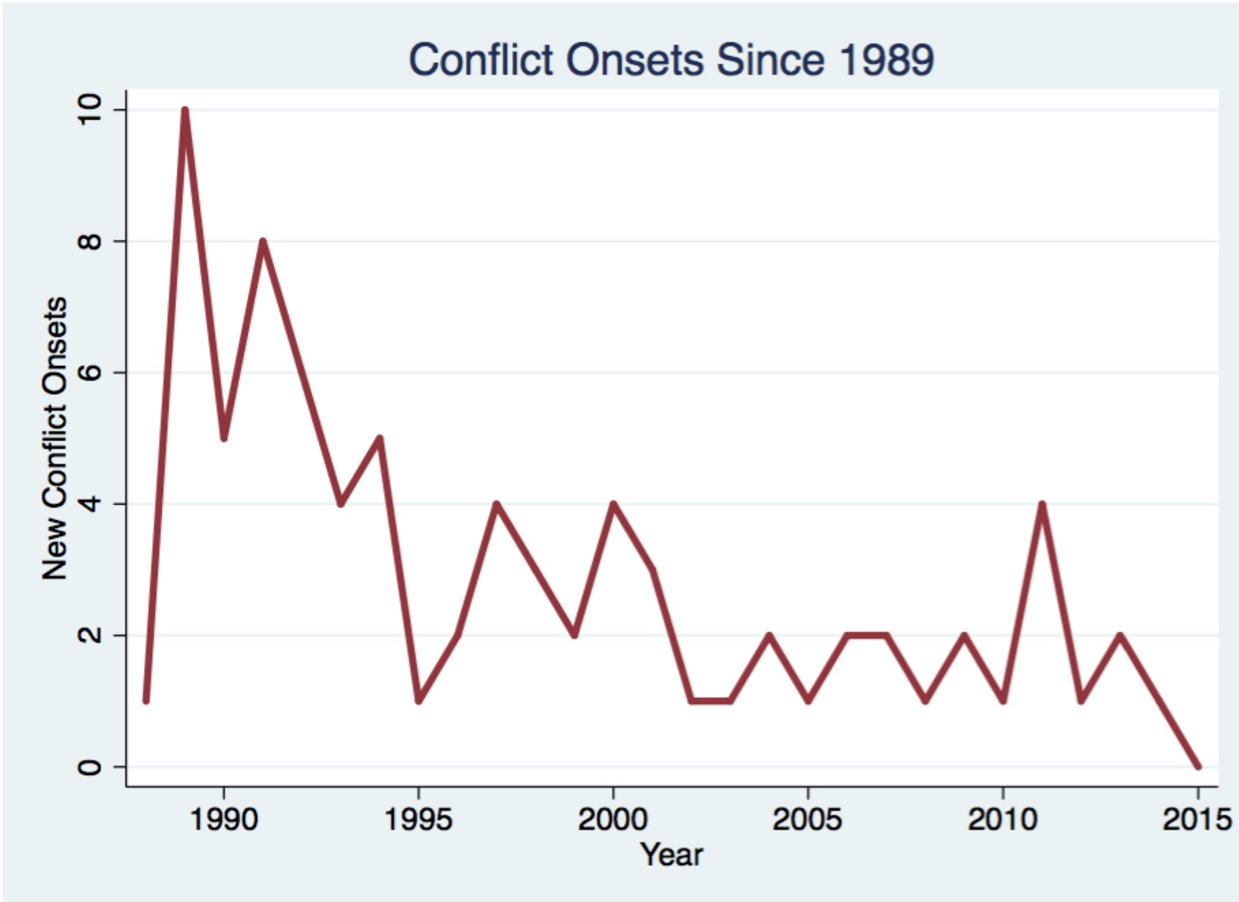

Figure 2: Data provided by Kreutz (2016)

Figure 3 displays the breakdown of civil conflict onset by year, where as figure 4 breaks it down by subregion. There has been a steady decline in the number of new civil wars since 1991. It is important to note that this does mean that there are fewer wars. Figures 3 and 4 display the number of starting conflicts; they say nothing about the general incidence of conflict. Figure 4 shows the spread of conflict geographically. The regions that experienced the most civil conflict onsets from 1988-2015 were Western Africa (21), Eastern Africa (14) and Southern Asia (12). Australia, New Zealand and Western Europe were the only regions that did not experience any civil conflict onsets in this time frame.

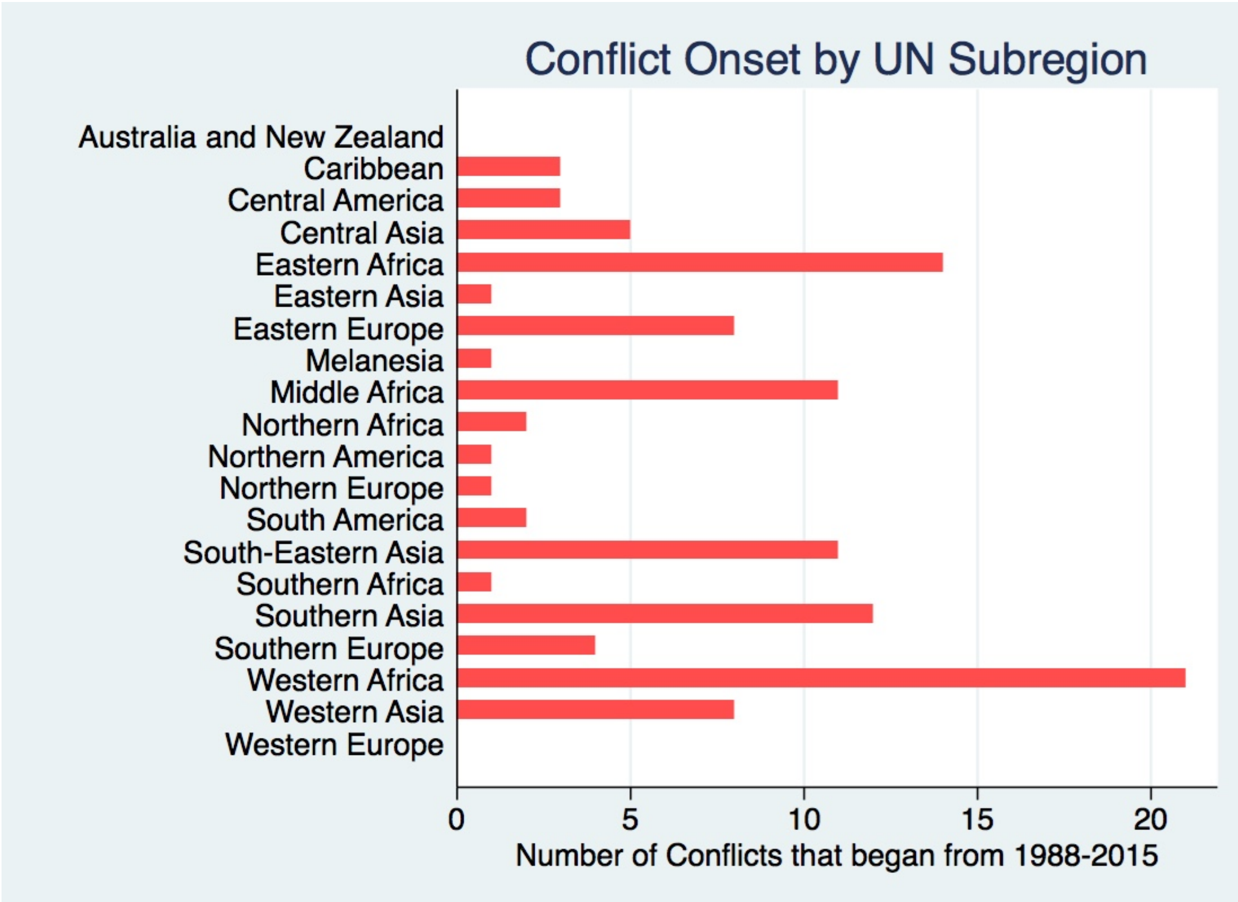

Figure 3: Data provided by Kreutz (2016)

5.3 Why Use Prices?

As I noted in the literature review, a perpetual issue in studying the small arms trade is how to assess small arms abundance. Ideally researchers would posses data on how many weapons were imported, sold, or even owned in a given country year. However, outside of the weapons purchased by states, the trade in small arms is largely illicit and conducted in secret. Most case study analyses are forced to use subjective measures of small arms availability, such as the 'acquisition' of weapons by rebel groups, or the general perception of how prevalent small arms are (Sisilin and Pearson 2001). However, there are two other way to quantify the small arms trade: weapons transfers and weapons seizures.

An approach used in scholarship and exemplified by Moore (2012) is to look at the effect of weapons transfers on a civil conflict. In theory, the ideal experiment assesses the impact of small arms abundance on civil conflict would be to airdrop crates full of small arms on randomly assigned countries. The geo-political equivalent of this experiment is to examine civil conflicts where a foreign patron provides small arms to a rebel group as a case where small arms are abundant and compare them to conflicts that lack such support. However, this approach suffers from concerns pertaining to both causality and nature of the specific relationship. Unlike the proposed experiment, no armed group receives military support randomly. Armed groups are selected to receive support in large part because they are more likely to engage in conflict in the first place. Additionally, military support on its own has been shown to have a strong effect on the intensity of a civil conflict simply by alleviating the resource constraint that armed groups traditionally face (Weinstein 2007).

The approach taken by the United Nations Program of Action to Reduce the Illicit Trade in Small Arms is to quantify the small arms trade by the number of trafficked weapons seized in each country (Uni 2013). This approach, however, has been thoroughly criticized as subject to both reporting bias amongst member countries, and a peculiar kind of reverse causality. As Marsh and Mcdougal (2017) note, countries with stronger regulatory frameworks will likely have seized more small arms; however, because they have robust regulatory frameworks these countries ironically will have lower stocks of small arms on the informal market (Marsh and Mcdougal 2017).

The difficulty in using prices, compared to the other discussed methods, is that they do not measure the stock of small arms present in a given country. The given small arms price in a particular region is a function of the total stock of weapons in that region, the demand for weapons in that region, and the resources that the average person possesses to permit them to acquire weapons (Brauer and Muggah 2006, 141). These factors are more conventionally understood in market context as supply, demand, and income. The advantage of using price data is that prices are, in theory, comparable across countries. In all countries the average small arms price 'directly influence[s] the purchasing power of one's resources' and they can easily be obtained through a simple survey of local vendors (Brauer and Muggah 2006, 140). The former point, however, is significant.

A supply side analysis of small arms presumes that all purchased small arms are utilized in conflict. But, as Brauer and Muggah (2006) argue, there are many 'cultural' and 'socially constructed' reasons for owning small arms outside of a military context (Brauer and Muggah 2006, 139). It is important to consider these cultural differences because they may constrain the ease with which armed groups can obtain small arms. Hypothetically speaking, small arms may be harder to acquire in certain regions because they are considered a status of wealth. There is added value in using the price of assault rifles as the main independent variable because I am able able to account for such factors.

5.4 Data on Small Arms Prices

For my primary independent variable, I use the Country Illicit Small Arms Prices Dataset provided by Marsh and Mcdougal (2017). The data set provides price estimates of an assault rifle from the AK-47 or AR-15 family for each country year from 1960-2015 (Marsh and Mcdougal 2017, 5). Marsh and Mcdougal (2017) calculate their estimates using the Transactional Illicit Small Arms Prices Dataset which is a compilation of reported weapons transaction with prices converted to 2010 USD (Marsh and Mcdougal 2017, 4). Marsh and Mcdougal (2017) find the average price of each category of weapon in a given country based on the price observations and they applied a weapons adjustment so that each transaction measures the cost of an assault-rifle. Where no transaction was used, the prices are estimated using lagged prices from previous years and adjacent countries.

Table 1: Estimated Small Arms Prices by UN Geographic Region

	N	Mean	Min	p25	Median	p75	Max	St. Deviation	Skewness
Australia and New Zealand	56	1677.309	324.2872	1209.212	1582.099	2032.698	6133.468	923.669	2.130558
Caribbean	196	1279.901	376.8979	905.7026	1232.779	1585.24	4229.041	532.6762	1.308602
Central America	224	1104.289	21.26265	736.0526	1144.251	1455.792	2148.096	463.5417	.0154168
Central Asia	140	1097.011	134.6875	731.6414	1051.653	1359.566	2194.966	446.274	.5750233
Eastern Africa	448	1012.015	36.70888	678.7733	985.4155	1238.335	2914.225	440.1501	.7423131
Eastern Asia	140	1069.428	236.2022	809.7951	1128.824	1263.569	2112.285	367.5182	.304942
Eastern Europe	275	1178.187	288.0923	735.946	1110.712	1437.1	3142.577	540.5283	.7706926
Melanesia	84	1299.505	151.2412	559.11	840.1112	1724.91	4891.445	1127.443	1.341028
Middle Africa	224	946.6218	47.78169	621.7503	976.7265	1203.552	2242.888	415.2371	.6478657
Northern Africa	168	1061.603	26.50827	646.3133	968.113	1281.36	4533.318	586.7069	1.815187
Northern America	56	1145.865	343.0683	842.7321	1077.672	1245.712	2824.518	491.3182	1.371583
Northern Europe	280	1280.319	318.3996	949.6633	1161.248	1721.222	2868.739	536.1945	.4275545
South America	336	1112.179	40.19688	653.2245	1026.562	1440.068	3608.991	591.3926	.7285021
South-Eastern Asia	308	1054.687	141.431	691.3727	1004.482	1283.74	4166.228	513.0925	1.439819
Southern Africa	140	912.7132	26.49004	624.7666	992.5607	1165.502	2086.761	410.026	.3879643
Southern Asia	224	969.3037	13.64493	626.8197	941.8828	1226.032	5298.485	554.1127	2.535876
Southern Europe	257	1417.353	371.4571	958.3691	1247.513	1793.161	4673.017	716.1334	1.155694
Western Africa	420	927.8659	56.27062	557.7794	888.1596	1240.793	2812.771	490.0841	.849471
Western Asia	474	1456.255	121.3888	849.736	1211.073	1921.39	6215.232	840.9984	1.500107
Western Europe	194	1278.147	178.5827	849.049	1171.623	1789.636	2960.727	607.3994	.3583539
Total	4644	1147.863	13.64493	685.3863	1073.073	1394.719	6215.232	611.7362	1.559454

Table 1 displays the breakdown of estimated assault riffle prices by UN geographic sub-region using the data from 1988-2015. In this subset of the dataset the average assault rifle costs $1147.86 with a standard deviation of $611.73. Overall, assault rifles are estimated to be the cheapest in Southern Africa and most expensive in Australia and New Zealand.

A notable trend in the data is that the price of the average assault rifle increases overtime. In figure 2, I aggregate the price estimates to the region level and demonstrated the change in prices yearly. In every region accept for Oceania there has been an upward trend in the price of small arms since 1988. Using OLS to estimate assault rifle prices as a function of the year, I found that on average the price of an assault rifle rose by $16 a year. A possible explanation could be depreciation in the stock of AK-47s. As discussed in Chivers (2010) the current state of small arms proliferation can be traced to policies first implemented in the Soviet Union in the 1960s in which AK-47s were mass produced and then distributed to allied countries where they were stockpiled until the regime's eventual collapse.

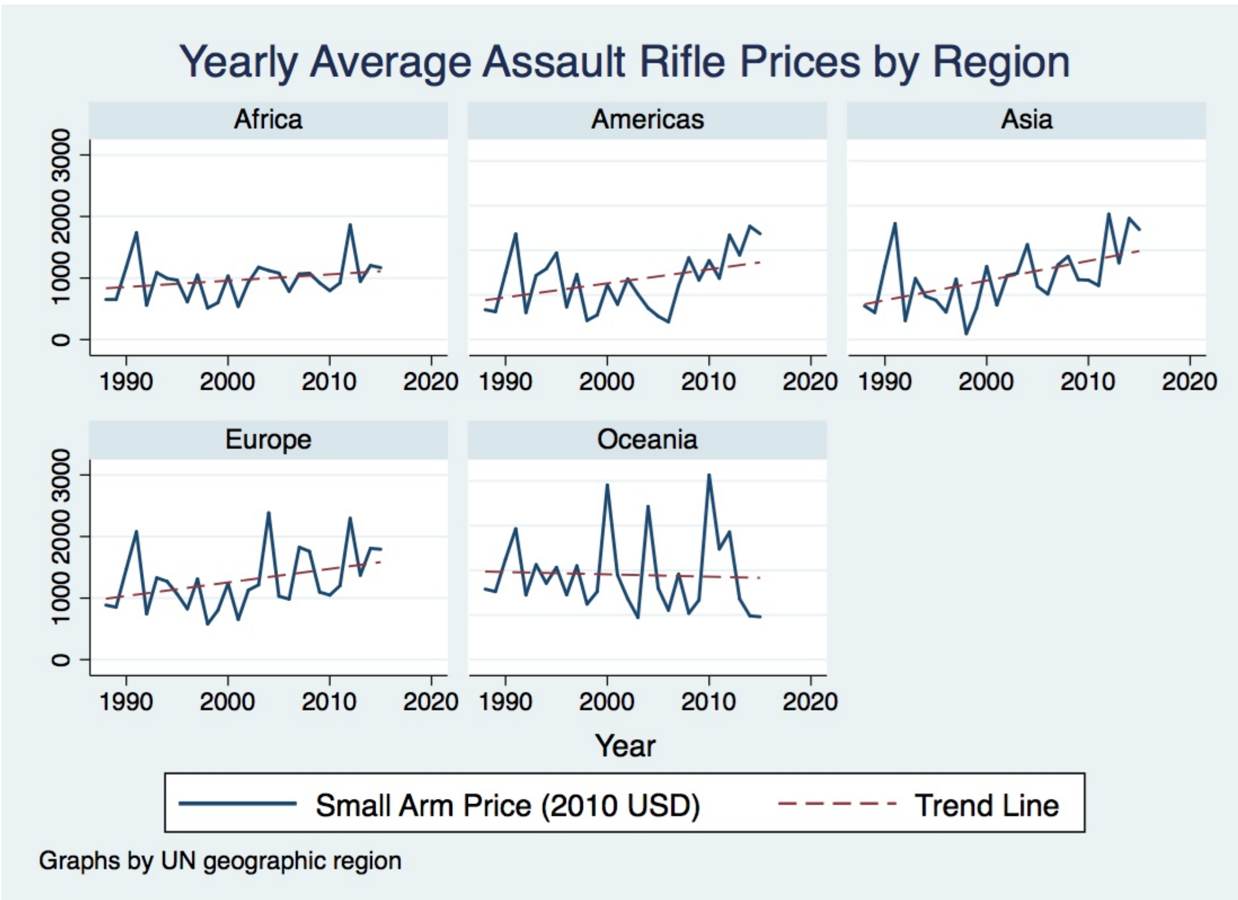

Figure 4: Estimated Small Arms Prices by Region (Marsh and McDougal 2017)

There is a a large degree of measurement error associated with using the Country Illicit Small Arms Prices Dataset. Only 6% of the price estimates are supported by a recorded transaction in that country-year. All other estimates are interpolated based on the price of small arms in adjacent countries and in past years. Consequentially there is a larger divergence from the "true" average assault rifle price in the country year and the recorded assault rifle price for that country year. An even smaller number of observations are based on observed transactions involving assault rifles and instead use transactions for other weapons (rocket launchers, pistols, ect.) to estimate the average price of an assault rifle.

That being said, the prices do appear to correlate with events in the real world that would shift the price of small arms. For instance, if you look at the change in small arms prices in the Americas, you will see a large drop in small arms prices around 2006 that does not exist in any other region. That dip could feasibly be explained by the expiration of the United States ban on selling assault rifles, which increased the supply of assault weapons on the continent. Marsh and Mcdougal (2017) have also noted that in the dataset prices rises in Colombia, Syria, Libya, Egypt, Yemen, Mexico and Sudan alongside heightened political instability (Marsh and Mcdougal 2017, 17). I am comfortable using the prices to examine causality and the general direction of the relationships but because the large amount of imputation involved in creating the dataset, I do not expect that it will yield accurate estimates of the correlations magnitude.

5.5 Reverse Causality

Conflict journalists note the rise of small arms prices prior to the onset of a conflict, suggesting that the relationship between small arms and conflict intensity is endogenous (Florquin 2014). This phenomenon is troublesome for scholars of small arms. The availability of weapons may have made it easier for conflict to start in a particular time and place but when the conflict appears imminent the price of weapons will spike. For instance, Marsh and Mcdougal (2017) have noted that the price of assault rifles in their dataset sharply increased in the Libya, Syria, Yemen and Egypt in response to the Arab Spring protests and the conflicts that followed them (17). At the start of Syria's civil war, one arms dealer in Lebanon reported that 'Prices [...] tripled in less than two months,' specifically that the price of an AK-47 went from $850 to $1450 as people responded to the rising fear of conflict (Alami 2011).

Killicoat (2006) showed that when regressing small arms prices against civil conflict onset the estimated coefficients are positive and suggest that the relationship is driven by small-arms demand. Field research suggests that the same problem exists when examining the relationship between small arms availability and conflict intensity. Florquin (2014) found the price of ammunition in Lebanon spiked following a battle in neighboring Syria's civil war. While ammunition is likely more susceptible to reverse causality, it is very plausible that a regional spike in fighting would cause both an increase in the number of battle-related deaths and an increase in the demand for assault rifles needed for self-defense.

To test for reverse causality, I present Granger Causality tests in Table 2. Granger causality tests are used when we have two variables X and Y that correlate with each other, but we do not know which variable causes the change in the other. Granger causality tests estimate each variable as a function of lags of itself and lags of the other variable. If the lags of X sufficiently predicts Y then we can say that X Granger causes Y. The Granger causality tests in Table 2 examine both the relationship between estimated small arms prices and onset, as well as estimated small arms prices and the intensity of conflict. The lag structure was chosen to be the most parsimonious while maximizing the regression's F-statistic.

Table 2: Granger Causality Tests

	Conflict->Weapons Prices (F-Stat)	Weapons Prices ->Conflict (F-Stat)	N	Lags
Onset & All Price Estimates	3.79***	0.33	4913	3
Intensity & All Price Estimates	0.50	1.98	619	1

The relationship between onset and assault rifle prices appears to be monocausal; civil conflict onset Granger causes the estimated rise in assault rifle prices. However, the F-stat on the lag of assault rifle prices is not significant, suggesting that assault rifle prices do not Granger cause civil conflict onset. There appears to be no causal relationship between civil conflict intensity and estimated small arms prices.

A tentative conclusion to be drawn from these tests is that small arms prices are caused by conflict onset, but, once a conflict begins, deaths do not affect the price of weapons themselves. That story fits with the findings of Killicoat (2006) and what is known about the dynamics of small arms prices during the course of conflict. Initially, the beginning of a conflict brings a demand shock as individuals start to buy weapons for self-defense and potential insurgents mobilize (Florquin 2014) (Killicoat 2006). Eventually, supply channels are established and increasing supply of small arms counteracts the shock to prices (Florquin and Khakee 2005).

Another possible explanation is that because the dataset of Marsh and Mcdougal (2017) is full of rough estimates of assault rifle prices, they may not respond to micro-level increase in fighting that would have a corresponding spike in the demand for assault rifles. It is also possible that any demand spikes caused by increased fighting are confined to the local level and would not manifest in price spikes at the country level. However, the tests demonstrate that at the very least there is no endogenous relationship between deaths and assault rifle prices in the given dataset. I therefore proceed by assuming only that onset is endogenous with small arms prices.

I use two different adjustments to the small arms prices that attempt to obtain exogenous estimates of the affect of assault rifle prices on the risk of civil conflict onset and intensity. First, I lag the estimated assault rifle prices by one period (a year). Next, I find the percentage change in the assault rifle price from the prior period and lag it as well. Additionally, I use an exogenous variable, distance from disrupted arms stockpiles, to obtain unbiased estimates of small arms prices in the current period. The instrumental variable is used to estimate the relationship of small arms prices to civil conflict onset.

I lag the price estimates by a year 'to control for suspected endogeneity with the conflict process and the expectations of agents in anticipating war' (Killicoat 2006, 52). Endogeneity issues aside, the lagged prices might actually have more of an effect on the risk of conflict onset than the year of onset itself. From what is known about homicide, weapons acquired in period t-1 manifest themselves on the homicide rate in period t (Brauer and Muggah 2006, 144). A similar process may exist with conflict whereby potential insurgents begin acquiring weapons when prices are low and their acquisition alerts the rest of the population of an incoming conflict and leads to an inevitable spike in prices.

I also use the lagged percentage change in the price of small arms. There is a concern that the price of an assault rifle is only meaningful in one specific context: the country where it was purchased. All prices are presented in 2010 USD; however, there remain concerns about how precisely they represent the universal purchasing power of insurgents because, to my knowledge, they remain unadjusted for purchasing power parity.[6] One particular concern is that prices do not take account of the relatively different costs of living. If food is more expensive in Thailand than in South Sudan, then, even if an AK-47 costs the same amount in both countries, insurgents in Sudan will have more flexibility to devote their resources towards weapons. To control for the different weights that small arms prices have in different contexts, I calculate the percentage change in the assault rifle prices

[6] The dataset designed bb Marsh and Mcdougal (2017) does provide a wealth-adjusted price index however that variable was not found in the provided stata files.

year to year. I then lag the percentage change in prices to avoid the aforementioned endogeneity with conflict in the current period.

Finally I use a third exogenous variable, distance from disrupted stockpiles, to obtain unbiased estimates of assault rifle prices prices in the current period. This approach is discussed in more detail in a later section.

6 Onset Estimation Strategy and Results

6.1 Estimation Equations

I estimate the relationship between civil conflict onset and small arms prices using a probit model with standard errors clustered at the country level. While logit is the default in past literature, probit is easier for estimating a two stage with instrumental variables. Later I take an instrumental variables approach to try and determine causality, so I chose to use a probit model in order to make the models more comparable. My basic estimation equation is as follows:

$$ConflictStart_{it} = \beta_0 + \beta_1 \alpha_{\textit{it-1}} + X_{it} + \gamma_{it-1} + \epsilon_{it} \tag{1}$$

The dependent variable is Conflict Start, a dichotomous variable that indicates whether an armed group entered into conflict with the state in a given country-year. I am most interested in the coefficient β_1 on my main independent variable, $\alpha_{\textit{it-1}}$ which is the estimated assault rifle price in country i in period (year) t-1. γ_{it-1} is a dummy variable that indicates whether a civil conflict was ongoing in country i in period t-1. This control is present in every model, however I am unconcerned with the coefficient on it and do not present it in my tables. The purpose of this control is to make sure that country-years that are already experiencing a civil conflict are not being compared to country-years that are not experiencing a civil conflict. X is the vector of controls selected based on the findings in the literature. The vector includes measures of resource rents, average incomes, inequality, geographic accessibility, and population. For a breakdown of the included controls, the data sources I choose to use for each one, and the notable variables which I choose not to control for, see Appendix A. β_0 is the intercept and ϵ_{it} is the error term.

I then re-estimate the relationship and replace $\alpha_{it\text{-}1}$ with $\lambda_{it\text{-}1}$ which is the percentage change in the assault rifle price lagged by a single period (year). All else remains the same.

$$ConflictStart_{it} = \beta_0 + \beta_1 \lambda_{it\text{-}1} + X_{it} + \gamma_{it-1} + \epsilon_{it} \tag{2}$$

Notably, country and time fixed effects are absent from these equations despite the use of panel data. Instead, the probit model is treated like a cross sectional. Fixed effects bias the coefficient estimates in a probit model by raising the degrees of freedom and reducing the number of observed conflict onsets. As a result they could not be included. However, I provide the same equations run with country fixed effects as a logit model in (see Appendix B). Although using fixed effects does change a number of control variables, it has no effect on the coefficient of interest, β_1.

6.2 Onset Results

Lagged small arms prices have no affect on the risk of civil conflict onset, but the lagged percentage change in small arms prices is consistently positive, which suggests that there is persisting endogeneity in these models. Table 3 presents the models described in equation 1, and Table 5 presents the models described by equation 2. Since the coefficients provided in a probit model are devoid of meaning, I also provide the marginal effects in Table 4 and Table 6 for equations 1 and 2, respectively. Starting in model 1, Table 3, the sign on the lagged assault rifle price is positive but the marginal effect, and coefficient size, indicates that the effect is really zero. In models 1-7 in Table 3 lagged assault rifle prices have no relationship statistically different from zero with the risk of civil conflict onset.

Table 3: Marginal Effects of a Lagged Increase in Small Arms Availability on Conflict Onset

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Marginal Effect (dy/dx)	1.01e-06	1.45e-06	3.47e-06	5.67e-06	4.64e-06	-3.85e-06	3.74e-06
Standard Error	4.06e-06	5.17e-06	4.07e-06	3.07e-06	3.77e-06	6.59e-06	4.72e-06

In each successive model, controls are introduced but none of them have any bearing on the main coefficient of interest. In the regression tables, I first introduce the most trimmed model that uses only the two controls that are the strongest predictors of civil conflict in Fearon and Latin (2003): the natural log of GDP per capita and the natural log of population. Each successive model, with the exception of model 4, has only one additional control variable. Introducing controls iteratively shows changes to the coefficient on the variable of small arms abundance while maximizing the number of observed conflict onsets in each sample. Model 7 is then the full model. Imputed measures of Road Density and the GINI coefficient had to be used in model 7 in order to observe enough conflict onsets to estimate the model. Those two variables are largely static over time, so the amount of bias changes minimally.

In model 2 I introduce the natural log of oil rents which as expected are positive and significant. In model 3 I introduce the natural log of mineral rents which is also positive, but insignificant. In general the small number of observed onsets makes it difficult to obtain significance for the control variables beyond the natural logs of population and GDP per capita. In model 4 two controls are introduced at once: the natural log of both military expenditure per capita and the natural log of military personnel as a percentage of the labor force. As Ross (2000) argues, the coefficient on military expenditures capture is meant to capture the repression effect of improved military technology. However, it will instead capture the effect of total spending on soldiers unless the size of the military is also accounted for (Ross 2000, 27). While the coefficients on both were expected to be negative, as they measure the ability of the state to repress conflict, the natural log of military expenditure per capita came back positive. That suggests that countries with looming civil conflicts preemptively raise their military spending.

In model 5 I introduce road density as a measure of geographic accessibility. Road density is measured as the percentage of a country's territory covered by roads. In model 5 the sign on road density is negative, as predicted, but insignificant. In model 6 I introduce the GINI coefficient which has a negative sign despite the prediction of the literature. This finding is not unexpected as numerous past studies have failed to get a significant sign on the GINI that matches theory (Collier and Hoeffler 2001)(Fearon and Latin 2003). Model 7 includes all of the aforementioned control variables. While the inclusion of every control at once has no discernible effect on the marginal effect of an increase in lagged assault rifle prices, it did flip the sign on the natural log of mineral rents per capita from positive to negative and significant. My explanation for this shift is that the shift in N from model 3 where there were 2470 observed country-years with 54 conflict onsets to 995 observed country-years with 25 conflict onsets allowed a few individual cases to dominate the estimated effects.

Table 3 also includes the portion of country-years that were correctly classified as either witnessing a conflict onset or no conflict onset. Every model correctly classifies above 90% of country-years as experiencing because, with the exception of model 7, they all default to the naive rule of always guessing that there is no conflict onset. Figure 5 displays the predicted risk of civil conflict onset for each country year according to model 7 against the lagged price of small arms. Notably, only one country-year has a risk of civil conflict onset above 50%. This result is the consequence of having such an unbalanced dataset. The percentage of country-years that are correctly classified varies only based on the percentage of country-years where a conflict onset actually occurred.

In contrast to the lagged price of small arms, the lagged percentage change in the price of small arms appears to have a positive, albeit insignificant, relationship with the risk of civil conflict onset. Table 5 displays the models calculated from equation 2. The only change from equation 1 is that the main independent variable is now the lagged percentage change in small arms prices. The marginal effects of these coefficient estimates are displayed in Table 6. The change in independent variables has no meaningful effect on the control variables. With the exception of model 6, all models predict that if the average assault rifle price went up last year, then there is a higher risk of a civil conflict. However the estimated effect is small. The full model, model 7, suggests that if the price of the average assault rifle doubled the year before, or increases by 50%, than the risk of a civil conflict increased by about 0.045%.

Table 4: Probit Model: 1st Death Onset and Lagged Assault Rifle Prices

	(1) Onset as the 1st Death	(2) Onset as the 1st Death	(3) Onset as the 1st Death	(4) Onset as the 1st Death	(5) Onset as the 1st Death	(6) Onset as the 1st Death	(7) Onset as the 1st Death
Lagged Assault Rifle Price	0.0000202	0.0000286	0.0000749	0.000154*	0.0000996	-0.000109	0.0000857
	(0.0000811)	(0.000102)	(0.0000889)	(0.0000831)	(0.0000798)	(0.000180)	(0.000109)
Log of Population	0.124***	0.178***	0.221***	0.258***	0.112*	0.337***	0.434***
	(0.0456)	(0.0652)	(0.0543)	(0.0647)	(0.0643)	(0.104)	(0.0830)
Natural Log of GDP per Capita	-0.260***	-0.318***	-0.295***	-0.214***	-0.261***	-0.238***	-0.388***
	(0.0429)	(0.0818)	(0.0558)	(0.0603)	(0.0582)	(0.0780)	(0.110)
Natural Log of Oil Rents per Capita		0.0789**					0.226***
		(0.0402)					(0.0536)
Natural Log Mineral Rents per Capita			0.0141				-0.100***
			(0.0255)				(0.0342)
Natural Log of Military Expenditure per Capita				0.109			0.481*
				(0.0788)			(0.254)
Natural Log of Army Personel (% of the Labor Force)				-0.127			-0.381***
				(0.109)			(0.142)
Road Density (% per square mile)					-0.0683		
					(0.0680)		
GINI						-0.00841	
						(0.0119)	
Imputed Road Density							-0.477^{+}
							(0.319)
Imputed GINI							-0.00846
							(0.0145)
Observations	4079	2207	2470	2126	1865	1110	995
Pseudo R^2	0.097	0.122	0.132	0.193	0.134	0.148	0.292
Wald Chi	60.81	32.51	63.75	96.83	53.31	22.52	117.63
Correctly Classified	97.70%	97.60%	97.81%	98.21%	97.80%	98.38%	97.59%
Observed Onsets	94	53	54	38	41	18	25

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Probit Model: 1st Death Onset and Lagged Assault Rifle Prices

	(1) Onset as the 1st Death	(2) Onset as the 1st Death	(3) Onset as the 1st Death	(4) Onset as the 1st Death	(5) Onset as the 1st Death	(6) Onset as the 1st Death	(7) Onset as the 1st Death
Lagged Assault Rifle Price	0.0193	0.00545	0.00451	0.0524	0.000640	-0.0295	0.0184
	(0.0292)	(0.0248)	(0.0228)	(0.0543)	(0.0380)	(0.0277)	(0.0278)
Natural Log of Population	0.139^{***}	0.168^{**}	0.214^{***}	0.253^{***}	0.110^{*}	0.407^{***}	0.438^{***}
	(0.0449)	(0.0658)	(0.0532)	(0.0624)	(0.0634)	(0.105)	(0.0833)
Natural Log of GDP per Capita	-0.260^{***}	-0.312^{***}	-0.277^{***}	-0.198^{***}	-0.249^{***}	-0.267^{***}	-0.379^{***}
	(0.0442)	(0.0835)	(0.0542)	(0.0575)	(0.0563)	(0.0826)	(0.106)
Natural Log of Oil Rents per Capita		0.0792^{*}					0.226^{***}
		(0.0407)					(0.0522)
Natural Log Mineral Rents per Capita			0.00371				-0.105^{***}
			(0.0268)				(0.0343)
Natural Log of Military Expenditure per Capita				0.102			0.461^{*}
				(0.0742)			(0.250)
Natural Log of Army Personel (% of the Labor Force)				-0.121			-0.373^{***}
				(0.108)			(0.139)
Road Density (% per square mile)					-0.0673		
					(0.0686)		
GINI						-0.0118	
						(0.0107)	
Imputed Road Density							-0.480^{+}
							(0.321)
Imputed GINI							-0.00816
							(0.0144)
Observations	3950	2138	2390	2123	1862	1095	995
Pseudo R^2	0.103	0.115	0.128	0.191	0.133	0.190	0.292
Wald Chi	63.58	32.43	71.42	99.93	46.61	30.94	111.37
Correctly Classified	97.85%	97.66%	97.91%	98.21%	97.80%	98.54%	97.49%
Observed Onsets	85	50	50	38	41	16	25

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Marginal Effects of a Lagged Percentage Increased in Small Arms Availability on Conflict Onset

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Marginal Effect	.0009102	.000272	.0002023	.0019329	.0000299	-.0009144	.0008008
Standard Error	.0013822	.0018933	.0010228	.0019695	.0017744	.000888	.0012202

A plausible explanation for these results is that people anticipate the outbreak of a civil conflict a year in advance and the demand for weapons rises accordingly. I used granger causality tests, displayed in Table 7, to test this assumption. The results suggest that onset has no causal effect on the change in prices from the past period and that the change in prices has no causal effect on the risk of civil conflict.

Table 7: Granger Causality Test of Lagged Percentage Price Change

	Conflict->Weapons Prices (F-Stat)	Weapons Prices ->Conflict (F-Stat)	N	Lags
Onset & Lagged Percentage Price Change	0.28	0.19	4193	1

7 Intensity Estimation Strategy and Results

7.1 Data On Civil Conflict Intensity

Data on battle-related deaths comes from the Uppsala/PRIO Georeferenced Events Dataset (GED) compiled by Sundberg and Melander (2013). I used data from the GED to construct a panel data set that displays the number of battle-related deaths for each country year that is experiencing a civil conflict. The GED is a dataset of conflict events, defined by Sundberg and Melander (2013) as "incident[s] where armed force was by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (2).

Most studies that perform analysis on the country level prefer the ACD dataset, which estimates the number of deaths on each side of the conflict based on news reports. I prefer to use the GED because it assigns the deaths to locations as opposed to conflicts. The distinction is small, but significant. There are instances in the battle-related deaths dataset where the deaths occurred in a country not listed as being the location of the conflict. For instance the Afghanistan insurgency against U.S occupation often stretched into northern Pakistan. The ACD, because it focuses on conflicts and not conflict locations, does not nuance these deaths and lists them as having occurred in Afghanistan. The GED geocodes every event with an estimated number of fatalities, so I can be confident that the deaths occurred in the listed county year.

Not all studies measure intensity as the year-to-year fatality rate in a country. Lacina (2006) used a cross sectional dataset where the unit of analysis was an entire conflict, start to finish, and the total number of deaths that transpired over the course of that conflict. I chose to use the year-by-year fatality rate because it closely resembles the choices made by actors in the conflict. The trade-off between conflict-year and country year is the ability to control for insurgent specific characteristics. However a conflict-year dataset also requires using the ACD, which as I mentioned before, does not assign deaths to locations but rather to sides. Since there were no conflict-specific characteristics that I wanted to control for, I went with country-year. As I argue in the theory section, the decision to engage in combat operations is made in some cases hour-by-hour and reflects responses by the actor to real time shifts in the amount of recruits, the strength of the state, and access to weapons. Each of these vary temporally and geographically, so I formatted the data as a country-year panel dataset in order to capture as much variation as possible.

While the GED does not distinguish between interstate, intrastate, and extrastate conflict each event was assigned a Conflict ID that corresponded to a conflict in the ACD. I was able to use the conflict IDs in the ACD that corresponded to interstate conflict and internationalized interstate conflict to isolate the events that made up these conflicts. I then collapsed the dataset by county-year in order to make it compatible with the data provided by Marsh and Mcdougal (2017). I used a similar strategy to include deaths that occurred from fighting between non-state organized armed groups. The Non-Sate Conflict Database (NCD) complied by Sundberg et al. (2012) compiles deaths that occur in non-state conflicts and codes the conflicts according to the armed groups level of organization. Like the ACD, the NCD also assigns a conflict ID that matches the events in the GED. I used the conflict ID that corresponded to ‘formally organized armed groups,’ or "rebel groups and other organized groups that have a high enough level of organization so as to be possible to include in the state-based armed conflict category" (3-4). I then collapsed the events in the GED that corresponded to these conflict IDs and added the estimated fatalities from both categories of conflict together.

I made two other fixes to my collapsed dataset. First, I dropped all years that had recorded battle-related deaths despite their not being an ongoing conflict (according to my definition of conflict). Because the deaths are recorded in events there are some ambiguous events that got coded as being part of a civil conflict. A notable example is the sole death recorded in a reported 'civil war' incident in Germany in 1990 that likely related to tensions following the fall of the Berlin wall. Because these deaths occur outside of a civil conflict, I am comfortable excluding them from my analysis. The other fix is for country-years where a conflict is ongoing but where no deaths were recorded. Because the GED only reports on events, not the total yearly fatalities, using it will by default exclude country-years where civil war intensity is 0.

Table 8: Conflict Intensity by UN Subregion

	Count	Mean	Min	P25	Medeian	P75	Max	St. Deviation	Skewness
Caribbean	17	26.70588	0	0	0	0	207	61.3135	2.233964
Central America	18	395.6111	0	37	69.5	88	4924	1150.261	3.672463
Central Asia	18	533.3889	0	17	73.5	368	3763	1047.454	2.312468
Eastern Africa	159	966.3145	0	0	57	611	49698	4514.604	8.98703
Eastern Asia	19	3.052632	0	0	0	0	36	9.43677	2.85834
Eastern Europe	30	954.9	0	191	445.5	1151	5854	1352.365	2.312194
Melanesia	8	35.625	12	22	31	44.5	78	21.33366	.9987545
Middle Africa	89	861.9101	0	6	135	948	12054	1855.928	4.103333
Northern Africa	87	907.0575	0	0	256	1210	5198	1314.227	1.701496
Northern America	15	15.53333	0	0	0	0	233	60.16034	3.474396
Northern Europe	5	26.2	0	26	29	35	41	15.73849	-1.006603
South America	53	543.3396	0	50	231	994	2851	632.7364	1.384697
South-Eastern Asia	116	326.7759	0	49	212.5	451.5	1571	366.906	1.605814
Southern Africa	2	34	0	0	34	68	68	48.08326	0
Southern Asia	145	2030.897	0	47	930	3190	17807	2834.729	2.225702
Southern Europe	16	1302.188	0	31.5	585.5	2114.5	5295	1666.646	1.176587
Western Africa	114	276.1579	0	0	34.5	217	4327	680.7765	3.970827
Western Asia	207	532.8454	0	0	36	409	12213	1395.143	5.303989
Total	1118	786.8202	0	2	95.5	668	49698	2279.631	11.31619

Table 1 displays summary statistics for the yearly battle-related deaths by UN subregion. The data are immensely skewed. Notably the mean conflict intensity, at around 786 deaths per year, is well above the 75th percentile of 668 deaths per year let alone median of 95 deaths per year. Part of the discrepancy are two particular outliers: 1989 and 1990 in Ethiopia. In those years 24,333 and then 49,698 people respectively died. I use a robust regression to correct for the presence of outliers (see Appendix C). The outliers do not change my results; however, using the less efficient model very slightly changes the significance of my results.

7.2 Intensity Estimation Strategy

For intensity, the main model is a fixed effects regression run with Driscoll–Kraay standard errors.[7] I use the following structural equation:

$$BattleDeaths_{it} = \beta_0 + \beta_1 \alpha_{it\text{-}1} + X_{it} + \epsilon_{it} \tag{3}$$

For intensity the dependent variable is *BattleDeaths*, a continuous variable of the number of battle-related deaths in country i and year t. Battle-Deaths is a function of $\alpha_{it\text{-}1}$, which is, the price of the average assault rifle in country i for year t-1. β_1 is the coefficient of interest, which expresses the relationship between $\alpha_{it\text{-}1}$ and *BattleDeaths*. X is the vector of controls, which includes measures of participating external patrons, resource rents, average incomes, horizontal inequality, state power, geographic inaccessibility, regime type, vertical inequality and population (See Appendix A).

$$BattleDeaths_{it} = \beta_0 + \beta_{1\,it\text{-}1} + X_{it} + \epsilon_{it} \tag{4}$$

Equations 3 and 4 were estimated using a random effects model with Driscoll–Kraay standard

[7] These errors account for clustering at the country level and the existence of spatial dependence between cases. For more information see stata help xtscc

errors. I used Hausman Tests to compare the coefficient estimates of equation 1 with the two most important control variables; population and GDP per capita. A Hausman test is used to compare between an estimator that is consistent but inefficient (fixed effects) and an estimator that is efficient but possibly inconsistent (random effects). It calculates the differences in the coefficients to see if they are significantly different. In this case it produced a wald-chi score of 3.72 when comparing between estimators for equation 1 and a wald-chi score of 2.14 when comparing between estimators for equation 2. Neither scores are sufficient to reject the null that there are significant differences in the coefficient estimates between the random effects estimator and the fixed effects estimator. Consequently, I default to the random effects estimator. It is worth noting that while the coefficients do not significantly change when using fixed effects, the sign on all coefficients that measures small arms abundance are insignificant.[8]

7.3 Intensity Results

The lagged percentage change in assault rifle prices has a robustly negative relationship with civil conflict intensity that stays significant up to the 10% level with the addition of controls. Table 9 displays the estimates from equation 3 in which the variable used to proxy small arms abundance is the lagged assault rifle price. The lagged assault rifle price has no relationship with the natural log of battle-related deaths in models 1-8 of Table 9. The signs on all other control variables match theory except for the GINI coefficient, the natural log of army personnel as a percentage of the labor force (model 8), and the Democracy dummy (models 4-8). Although each of the three has been featured in past work, none are considered paramount in understanding conflict intensity. Part of the departure from expectations could be the small sample size in model 8 which is makes the results more extreme.

Table 10 presents the models estimated based on equation 4, where the measure of small arms abundance is the lagged percentage change in the average assault rifle price. The coefficient on the lagged percentage change in small arms pricing in Table 10 is strikingly significant when compared to the models estimated in Table 9. For the first five estimated models, the coefficient remains in

[8] I very deliberately performed a hausman on the most trimmed set of variables. Using the full model the hausman test produces a wald-chi score of 46.23 suggesting that there a significant differences in the coefficient estimates between the two estimators. However, my only coefficient interest is β_1, the coefficient on my variable of small arms abundance. I am willing to sacrifice the accuracy of other coefficient estimates in order to decrease the probability of a type II error in assessing β_1.

the range of -0.06 to -0.09 and is significant at the 1% level. It declines in significance only when N drops below one half of the starting number of observations (N=270) but is significant at the 1% level in model 8, likely due to the small number of observations.

Because the dependent variable is logged, the coefficients can be interpreted as a percentage change in the dependent variable associated with a 1 unit change in the independent variable. Taking the estimates from model 2 a percentage increase in the price of the assault rifle last year is associated with 0.084% fewer deaths this year. To make this more interpretable, I consider what happens if the assault rifle price doubles (an increase of 100%). That would imply an 8.4% decrease in battle-related deaths in this year.

Figure 6 displays the predicted versus actual values in the regression. In most country-years the lagged percentage change in assault rifle prices is centered around -1% to 1%. The mean % However in the country-years where there were dramatic price shifts, say a drop of -5% or more, the magnitude of battle-related deaths tended to be higher. A close look at figure 6 demonstrates that a small number of conflicts are driving the entire relationship. These cases, which are far outside the 95% range of the data share one trait in common: they are defined by actual price observations. Large percentage changes in small arms prices during a conflict are likely not as rare as the graph suggests. Instead, because most prices are estimated, the only large shifts that the dataset picks up are when the prices are actually observed in the conflict. This relationship could be better explored with precise small arms pricing data on a single conflict. Notably, this relationship is driven entirely by drops in the price of small arms; there are no spikes during conflict where the price of weapons jumps above 5%. That means that these shifts in prices are likely due to supply side shifts in the abundance of small arms.

This finding supports hypothesis 2, increased availability of small arms is associated with an increase in battle-related deaths. However, the radically different results between the estimated models that used prices and Table 11 which shows percentage changes in prices requires some inductive explanation. Why would absolute price levels have no relationship with battle-related deaths, where a percentage increase in prices would? The answer is that the prices themselves, while inflation adjusted, do not account for regional variation in the cost of living that constrain the ability of armed groups to purchase small arms. The price provided by Marsh and Mcdougal

Table 9: Random Effects GLS Model with Driscoll-Kraay Standard Errors :Battle-Related Deaths and. Lagged Assault Rifle Prices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths
Lagged Price of Small Arms	0.00000985	-0.0000725	-0.0000405	-0.0000402	0.00000587	0.000152	0.000103	0.000200
	(0.000124)	(0.000119)	(0.000127)	(0.000130)	(0.0000926)	(0.000124)	(0.000228)	(0.000208)
Log of GDP per Capita	-0.227^{***}	-0.220^{***}	-0.169^{*}	-0.341^{**}	-0.385^{**}	-0.595^{***}	-0.765^{**}	-0.447^{+}
	(0.0726)	(0.0671)	(0.0839)	(0.157)	(0.181)	(0.173)	(0.355)	(0.294)
Log of Population	0.269^{***}	0.308^{***}	0.339^{***}	0.355^{***}	0.317^{**}	0.320^{***}	0.408^{*}	0.332^{**}
	(0.0560)	(0.0547)	(0.0493)	(0.0924)	(0.130)	(0.111)	(0.199)	(0.136)
Number of Supporting Countries		0.612^{***}	0.625^{***}	0.749^{***}	0.677^{***}	0.565^{***}	0.688^{***}	0.945^{***}
		(0.0800)	(0.0810)	(0.115)	(0.105)	(0.123)	(0.122)	(0.246)
Democracy			-0.300^{**}	0.174	0.307	0.420	0.0813	0.367
			(0.120)	(0.225)	(0.267)	(0.310)	(0.373)	(0.364)
Natural Log of Oil Rents per Capita				0.156^{***}	0.113^{**}	0.0942^{+}	0.0792	0.163^{**}
				(0.0389)	(0.0439)	(0.0628)	(0.0967)	(0.0750)
Natural Log Mineral Rents per Capita					0.125^{**}	0.201^{***}	0.220^{**}	0.311^{***}
					(0.0457)	(0.0514)	(0.102)	(0.100)
Natural Log of Army Personel (% of the Labor Force)						0.326^{**}	0.473^{**}	-0.0233
						(0.140)	(0.209)	(0.228)
Natural Log of Military Expenditure per Capita						0.684^{***}	0.723^{*}	0.691
						(0.207)	(0.351)	(0.512)
Road Density (% per square mile)							0.443	
							(0.696)	
GINI								-0.0922^{***}
								(0.0217)
Observations	778	778	744	489	399	270	146	99
R^2	0.050	0.176	0.173	0.228	0.235	0.301	0.407	0.433
F-Statistic	26.55	39.34	31.93	34.36	73.53	52.80	65.55	10.70

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Random Effects GLS Model with Driscoll-Kraay Standard Errors: Battle Related Deaths and Lagged Percentage Small Arms Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths
Lagged Percentage Price Change	-0.0967***	-0.0844***	-0.0752**	-0.108***	-0.0979***	-0.140*	-0.343*	-0.497**
	(0.0272)	(0.0262)	(0.0304)	(0.0368)	(0.0316)	(0.0695)	(0.189)	(0.180)
Log of GDP per Capita	-0.212***	-0.226***	-0.165*	-0.329**	-0.355*	-0.544***	-0.713**	-0.384
	(0.0671)	(0.0717)	(0.0841)	(0.154)	(0.182)	(0.166)	(0.324)	(0.273)
Log of Population	0.269***	0.313***	0.346***	0.373***	0.328**	0.290**	0.419**	0.246*
	(0.0573)	(0.0533)	(0.0465)	(0.0869)	(0.128)	(0.114)	(0.184)	(0.131)
Number of Supporting Countries		0.595***	0.612***	0.755***	0.679***	0.590***	0.701***	0.894***
		(0.0779)	(0.0829)	(0.120)	(0.106)	(0.129)	(0.140)	(0.232)
Democracy			-0.321**	0.132	0.273	0.444^{+}	0.130	0.512
			(0.120)	(0.205)	(0.260)	(0.291)	(0.384)	(0.373)
Natural Log of Oil Rents per Capita				0.150***	0.104**	0.0955^{+}	0.0877	0.180**
				(0.0413)	(0.0464)	(0.0591)	(0.0889)	(0.0644)
Natural Log Mineral Rents per Capita					0.119**	0.190***	0.202**	0.303***
					(0.0467)	(0.0519)	(0.0971)	(0.0966)
Natural Log of Army Personel (% of the Labor Force)						0.319**	0.507**	0.0684
						(0.143)	(0.201)	(0.217)
Natural Log of Military Expenditure per Capita						0.677***	0.718*	0.735
						(0.212)	(0.362)	(0.520)
Road Density (% per square mile)							0.472	
							(0.702)	
GINI								-0.0942***
								(0.0208)
Observations	746	746	712	472	388	270	146	99
R^2	0.054	0.172	0.168	0.238	0.240	0.305	0.416	0.467
F-Statistic	26.29	32.31	30.40	37.84	90.36	76.04	44.45	7.65

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

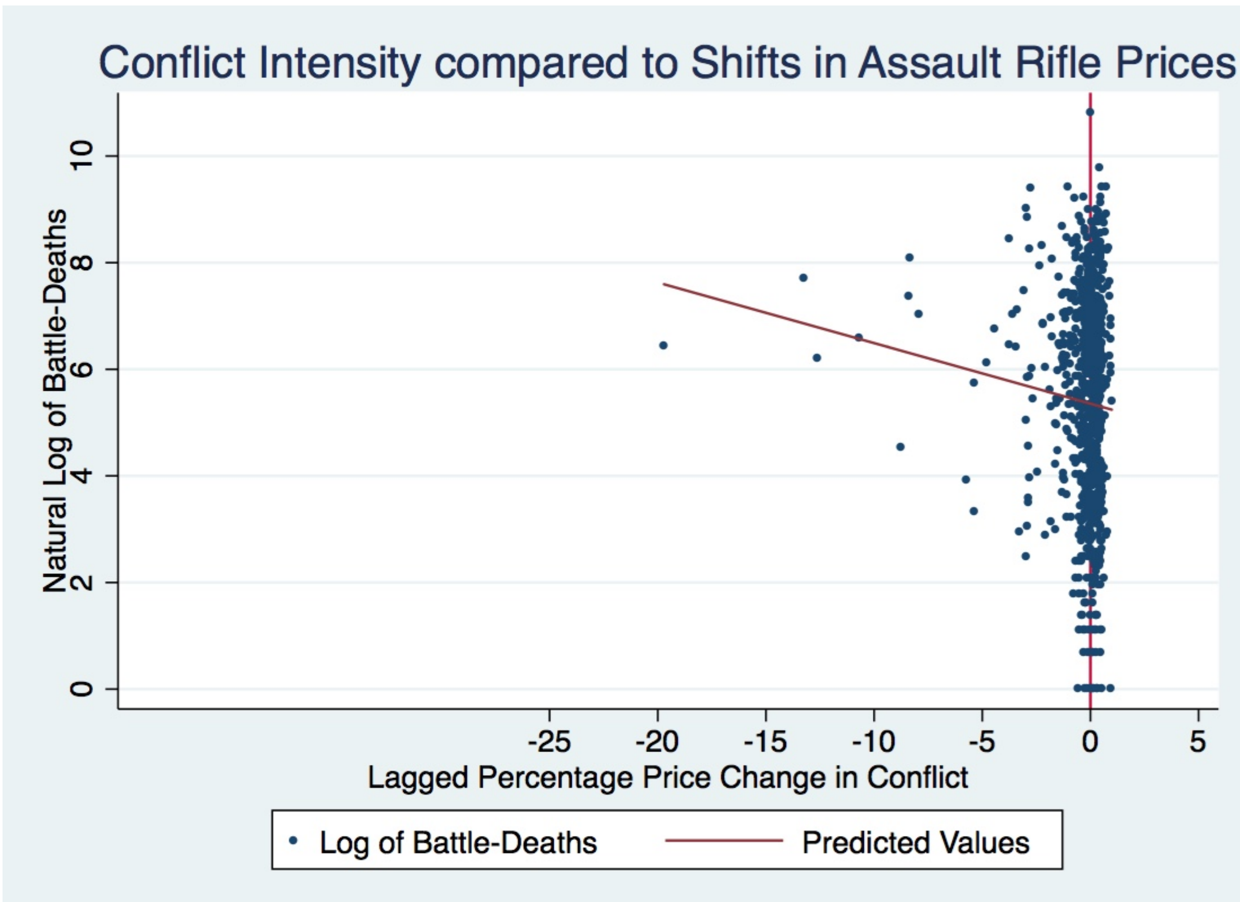

Figure 5: The Natural Log of Battle-related Deaths and Percentage Changes in Assault Rifle Prices Last Year

(2017) are not, to my knowledge, PPP adjusted.[9]

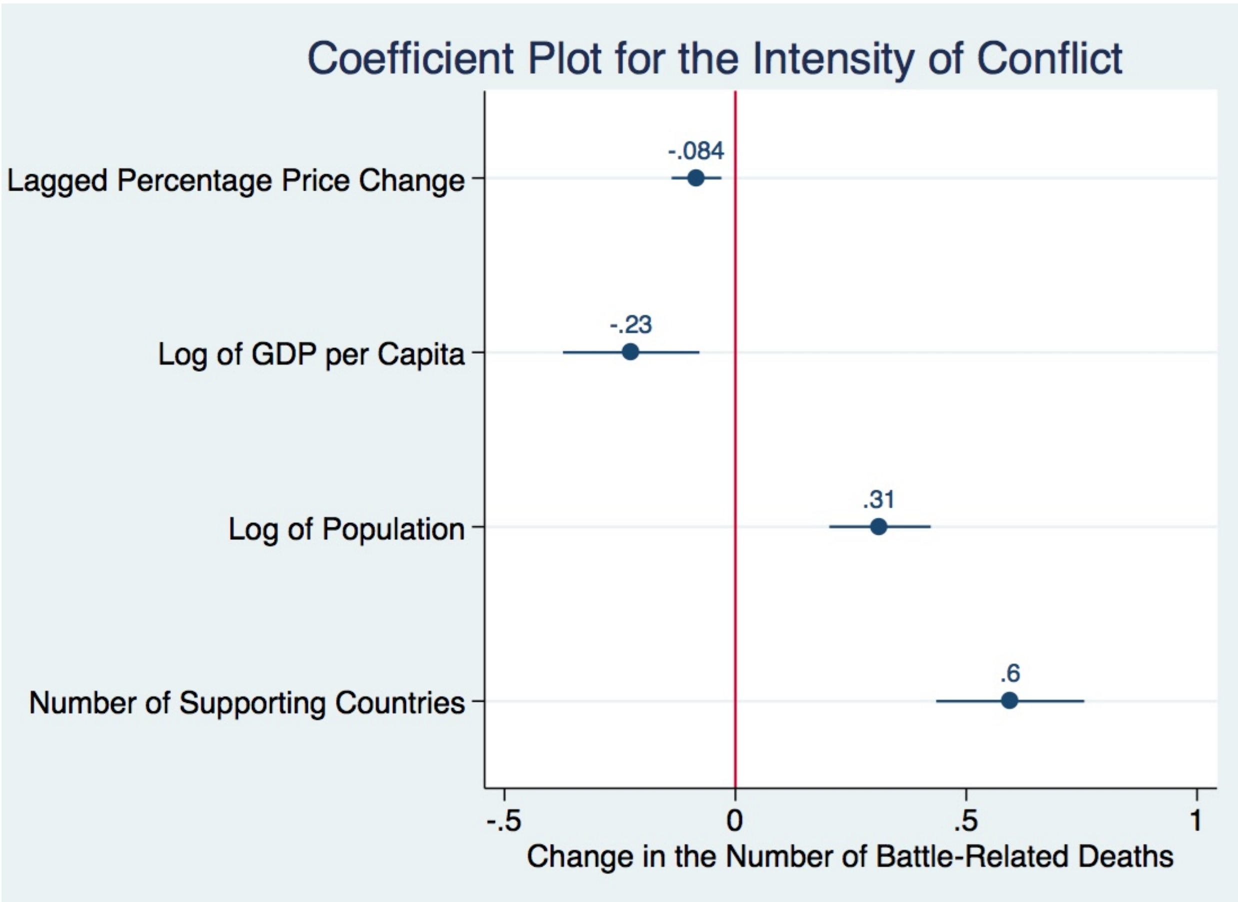

Figure 6: Coefficient Estimates for Table 10, Model 2

8 Two Stage Estimator

8.1 Distance From Disrupted Stockpiles

How can we estimate the effect of assault rifle prices on the risk of civil conflict onset separate from the effect of small arms demand on the prices? I use an instrumental variables estimator. Instrumental variables are variables that explain variation in our independent variable of interest (assault rifle prices) but are exogenous to the dependent variable of interest (civil conflict). By using the values of the independent variable that the instrumental variable predicts, we can theoretically view the casual effect of a change in X on Y and be confident that there is no reverse causality.

[9]While there is mention of a wealth-adjusted price index in their codebook I was unable to find it in either the small arms transfers transactional dataset and the small arms transfers country dataset.

Instrumental variables tend to be derived from nature so that their assignment to the variable of interest is random. In this case there are no natural conditions that makes small arms more or less abundant, but there are quasi-random unnatural conditions. Most small arms originate in large industrialized countries where their production is organized either by companies with private government contracts or the state.[10] In short, they originate on the formal market.

After production weapons are sold either to the government who contracted their production or to an allied country. There are three common stories often told and re-told in the small arms literature that explain how bulk quantities of weapons that were initially produced for use by formally recognized states and their appendages end up on the black market. In the first, weapons were sent as part of a strategic calculation to support a non-state group for use a civil conflict.[11] In the second story, insurgents capture a government facility and seize the weapons.

The final story starts with a government employee, perhaps a military officer or security guard, who works in a country with weak institutions and surplus stock of small arms. This employee contracts with a middleman for a bulk quantity of small arms to be quietly removed from a government warehouse and smuggled to a willing buyer (Feinstein 2011). Chivers (2010) describes how, beginning in the 1960s, the command-style of the Soviet Union economy caused the state to produce the AK-47 at a level far beyond those demanded of its armed forces. During the Cold War the USSR frequently moved weapons surplus to its allies. But even after the Union's collapse in 1991 in East Germany, Ukraine, and what is now the Russian Federation, there were warehouses of AK-47's that were just waiting for the right middleman to move them (Chivers 2010). The last two stories provide an exogenous channel that I can use to asses the abundance of small arms in a given region. Small arms are more likely to move onto the informal market through countries that have a large surplus stock of small arms and either weak institutions or where state weapons could be seized by insurgents.

I take this framework in designing my instrumental variable: distance from disrupted stockpiles. A stockpile is a location where surplus weapons have been hoarded by an organization with access to the formal small arms market. When these organizations experience a large shock, say a foreign

[10] In many conflicts there are handcrafted small arms but they are a small share of the total stock of small arms in any given conflict. (Florquin and Khakee 2005)

[11] Most prominently, during the cold war the United States and USSR routinely directed surplus military stock to back their preferred sides in internal conflicts.

invasion, the formal institutions meant regulate control of the weapons dissolve. The small arms are then pilfered by civilians and former government officials after which they are bought and sold on the informal market. Distance from these disrupted stockpiles measures how available the weapons are to a given population by proxying the transaction cost that any middleman would have to overcome to move them to a given region.

8.2 Locating Disrupted Arms Stockpiles

I first created a list of countries where governments and rebels spent years accumulating small arms only to lose control of them. I then calculated the distances between the capitals of these countries and every other country's capital to get a measure of small arms availability. I chose to focus on distance from capital cities because they are both the location where military surplus is likely to be located and because transportation infrastructure in each country, particularly airports, are likely to bring any weapons transfer through them.

I used three datasets to construct my list of disrupted arms stockpiles: the GWF dataset on autocratic regime change (Geddes et al. 2014), the UCDP/PRIO dataset on conflict termination (Kreutz 2016), and the UCDP/PRIO external support dataset (Croicu et al. 2011). I used these datasets to identify governments and rebel groups that received military support, precisely in the form of weapons transfers, from the Soviet Union (Russia), the United States of America, and China. These three countries are in the top two tiers of arms exporters, each having reported arms exports valued at over 100 million dollars from 2000-2013. The United States reported the most at $500 million; however, it is estimated that China and Russia also export an equivalent if not greater volume of small arms. The reason that their exports are not as easily quantified because they often lack transparency in their arms dealings (Holtom and Rigual 2015). It has been reported that Canada, Israel, Norway, South Korea, Spain, Switzerland,Turkey, and the United Kingdom also export over 100 million in small arms. However they get placed in the first two tiers of small arms exporters largely because they report all of their exports. Additionally, those governments are not as widely recognized as sponsors of insurgency as the big 3.

I began with the UCDP/PRIO external support dataset by removing all recorded instances of support that was not from the three aforementioned countries or was not weapons. I then combined the dataset with two others that identify shocks. For shocks to governments, I used the

GWF autocratic regime change database (Geddes et al. 2014). I used only regime changes I deemed drastic enough to cause the state to lose control of weapons. Ideally, I would have data on all regime changes for all regime types, but there are benefits to only using autocratic regimes. Autocracies are more likely to hoard excess weapons surplus simply because they have the highest military spending of all regime types (Albalate et al. 2012).

I selected only those regime changes that I considered drastic enough to cause the state to lose control of military facilities. In theory, regime transitions that are more chaotic create better opportunities for state arms stockpiles to be pilfered. The best way to pick regime changes that are sufficiently chaotic is to choose the most violent types of regime changes. To that end I only counted on three types of regime change: civil war defeat, foreign invasion, and "ceasing to exist" (Geddes et al. 2014). The GWF regime change dataset only goes until 2010, so I tried to compensate for six years of missed regime changes by using the UCDP/PRIO conflict termination dataset to look for additional instances where the state was defeated in a civil war (Kreutz 2016).[12]

I also identified non-state groups that received support from the United States, Russia, and China. Because these groups aimed to take control of the state, I coded their demise as either from defeat, a peace agreement, or the group disbanding (Kreutz 2016). Each of these actions implies that the organizational structures of the group ceased to exist and excludes the possibility that they conquered the state.

Using these three data sets, I composed a list of 12 unique countries which likely stockpiled weapons and lost control of their weapons. For each country I also noted the year in which the shock occurred. Two countries appeared as having multiple shocks. First, El Salvador whose U.S-backed regime was overthrown in 1982 but was also home to the FLMN, which was backed by the Soviet Union and demobilized in 1991 (Kreutz 2016) (Geddes et al. 2014). The second country is Afghanistan, whose Soviet-backed government was overthrown in 1992 but later was invaded by the United States in 2001 (Kreutz 2016) (Geddes et al. 2014).

In both cases I used what I knew about each conflict in designating the date of the shock. In El Salvador I chose 1991 as the shock date because the regime backed by the United States only received military support for 3 years. The Soviet Union, on the other hand, supported the FMLN for 11 years. In Afghanistan I chose 1992 as the shock date because foreign support for Afghanistan

[12]This modification added the Libyan revolution of 2011 to my list of regime changes

Table 11: Locations of Stockpiles and Years of the Shock

Country	Year Shock	Actor	Shock Type	Weapons Supplier
Namibia	1988	SWAPO	Demobilized in Peace Agreement	China & USSR
Chad	1990	Government	Overthrown by Insurgency	USA
Somalia	1991	Government	Overthrown by Insurgency	USSR
El Salvador	1991	FMLN	Demobilized in Peace Agreement	USSR
Soviet Union	1991	Government	Collapse	Itself
Ethiopia	1991	Government	Overthrown by Insurgency	USSR
Afghanistan	1992	Government	Overthrown by Insurgency	USSR
DRC	1997	Government	Overthrown by Insurgency	China
Cambodia	1998	Khamer Rouge	Defeated in Civil War	China
Angola	2002	UNITA	Defeated in Civil War	USSR
Iraq	2003	Government	Foreign Invasion	USSR & USA
Libya	2011	Government	Overthrown by Insurgency	USSR & USA

dried up after that year (Croicu et al. 2011). The Soviet Union was added to the list because it lost significant portions of its small arms stock during the collapse (Chivers 2010). The final list of stockpiles can be seen below in Table 11.

For each country year, I used ArcGIS to find the logged distance between the capital of the country in question and the capital cities of the countries with disrupted arms stockpiles in both the given region and all adjacent regions. Distance is logged because I expect that the effect of proximity to the stockpile varies at different levels of magnitude. In other words, I suspect that being five miles away from a stockpile has a much higher effect per unit of distance than being 50 miles away from a stockpile. The log also helps me handle stockpiles that are so far away from a given country year that they should not have an effect on the small arms market in that country. I would not want to overweight the suspected effect of a stockpile in El Salvador on battle-related deaths in Madagascar. Country-years where a given stockpile has not been disrupted are assigned a value of zero so that these stockpiles are not counted as effecting the price of small arms until the shock occurs. All the logged distances are then added up into one variable which measures the total logged distance from a country to all of the arms stockpiles.The unit of distance measurement is decimal degrees

8.3 Stockpile Stories

A number of the shocks listed in that table have already been singled out as regional sources of small arms. Libya is a prominent example given its recency. The regime of Mommar Ghaddafi spent more than four decades stockpiling weapons since it's beginning in 1969. Libya received weapons first from the USSR during the cold war and then later from European manufacturers after it agreed to cease its production of nuclear weapons. According to exporter reports to the UN Comtrade, Libya received at least 15 million USD in small arms imports from 2000-2010 from 18 different countries, not including unreported arms imports from Russia (Holtom and Rigual 2015). When Tripoli was captured in the summer of 201,1 these small arms were secured by various revolutionary brigades (McQuinn 2012). C.J. Chivers, the New York Times War Correspondent, witnessed the huge shift in the Libyan small arms market from conflict to post-conflict.

> "In Libya, arms researchers watched the swift arc from arms scarcity to oversupply among the opposition forces. From late winter into early summer, weapons were in short enough supply that many Libyan men went to battle without them, ready to pick up the weapon of a fallen fighter, while hoping to capture the weapons of slain Qaddafi troops. At that time, Kalashnikovs could cost $2,000 or more, just as they do for Syrian fighters now. By last fall, after the struggle for the country ebbed, many fighters possessed several rifles, along with machine guns or rocket-propelled grenades. Prices were plummeting, with reports of Kalashnikovs for sale at less than $500. Post-Qaddafi Libya, which for months had inhaled weapons, had become a black-market exporter, with all manner of arms being reported traveling out" (Chivers 2012).

In Libya a craigslist style market for small arms boomed in which AK-47s and pistols could be purchased over Facebook. In 2014 United Nations reported that weapons that used to be a part of Gaddafi's stockpiles were found in 14 different civil conflicts across North Africa, the Sahel, and the Middle East (of Experts 2013). The collapse of the stockpile can even be connected to a specific conflict onset. The year after the regime collapse, members of the Tuareg ethnic group in Libya, who fought against Gaddafi in the revolution, used weapons they pilfered from old regime supply depots to help their co-ethnics seize control of northern Mali (Nossiter 2012).

The civil war in El Salvador is a example of how weapons can be stockpiled even if they are sent to non-state groups. The 12 year civil war was influenced by a proxy conflict between the USSR and the United States in which government send weapons an aid to a different side in the civil conflict. The Soviet Union supported a coalition of leftist guerrillas called the Farabundo Marttional Liberation Front (FMLN in spanish). Meanwhile the United States backed the militarist government. In 1992 the FMLN agreed to a peace agreement and began demobilizing their armed forces; however, old stockpiles of weapons lingered in the area in case the FMLN should wish to re-mobilize. Later it was discovered that the FMLN had hidden as many as 109 caches of weapons in nearby countries (Laurance and Godnick 2001, 7). Weapons from the El Salvadoran civil war later would later make their way to Colombia, where they empowered the FARC guerilla movement (Florquin and Khakee 2005).

No section on stockpiles would be complete without mentioning the role of the cold war, specifically the weapons manufacturing and exporting policies of the Soviet Union, in creating the current state of small arms proliferation.[13] Chivers (2010) argues that the command style economy of the Soviet Union created the circumstances for weapons to stockpile. The command economy mass-produced the AK-47 without concern for its use and then distributed amongst its allies in the Eastern Bloc and beyond. When the Soviet Union collapsed in 1991, the old warehouses of weapons got looted and entered the informal market in greater quantities then before.[14] The map below uses data from Killicoat (2006) to map the average AK-47 price for set distances around Moscow for 1990-1995. While there the innermost buffer has by far the lowest price the differences between buffers diminishes as one goes farther from Moscow. The outer two buffers are roughly equivalent depending on how the buffers are drawn.

[13] I do not have a control variable for the cold war and post-cold war period even though during the cold war civil conflict was a much more common phenomenon due the the availability of foreign funding. I made this decision because only two years of my dataset (1989-1990) come before the collapse of the Soviet Union.

[14] One of the findings of Killicoat (2006) was that the fall of the Soviet Union had no worldwide impact on AK-47 prices. However distance from Moscow has a positive relationship with AK-47 prices. Killicoat (2006) explains the relationship between distance from Moscow and the price of the AK-47 as a product of the Soviet Union's policy of sending arms to allied regimes. I prefer to take the approach offered by Bourne (2007), that all small arms markets are local and geographically constrained. I view the collapse of the Soviet Union as being relevant only for the informal small arms markets in Eastern Europe, Western Russia, and Central Asia.

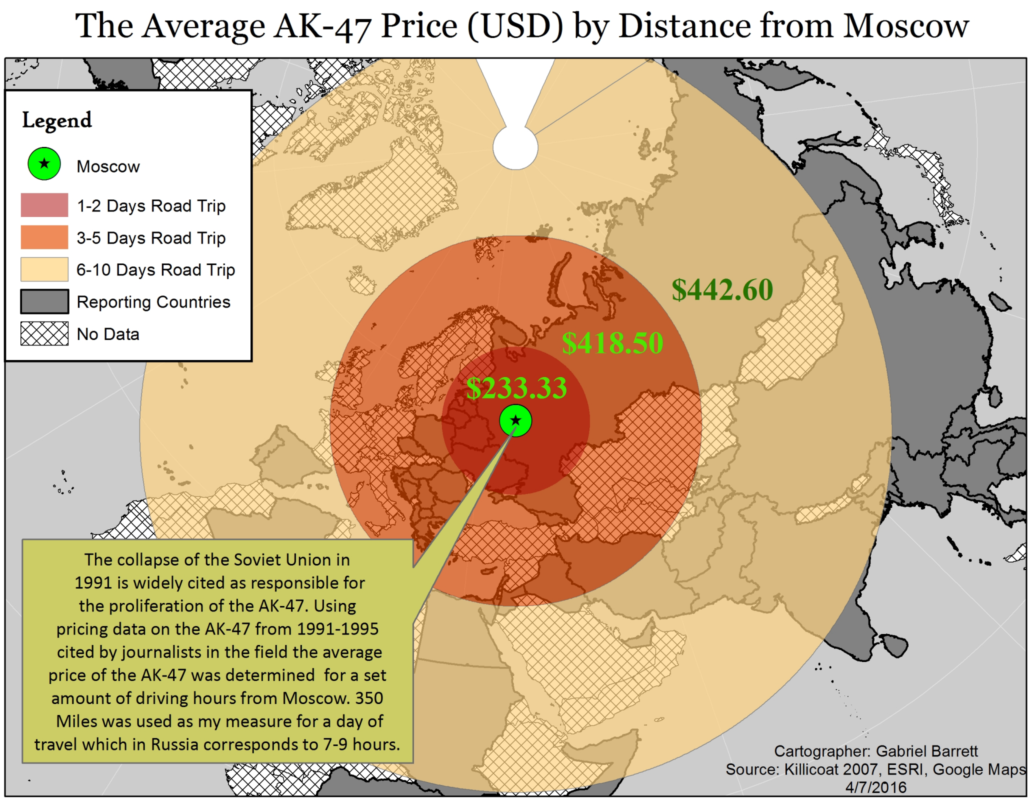

Figure 7: Mean AK-47 Prices Mapped by Different Distances to Moscow

9 Estimation Strategy

The map also demonstrates an important quality of my instrumental variable: it can explain variation in the price of small arms. I estimate the following equation, the first stage, to demonstrate that assault rifle prices do increase with the logged distance from the listed stockpiles.

$$D_{it} = \beta_0 + \beta_1 Z_{it} + X_{it} + \epsilon_{it} \tag{5}$$

Equation 5 is run using OLS to evaluate the first stage. In it, D_{it} is the estimated assault rifle price in country i at year t. Z_{it} is the total of country i's logged distance to each disrupted stockpile that experienced a shock after year t. X_{it} is the vector of country specific controls used to estimate the risk of onset. These need to be included in the first stage in order to get unbiased standard errors in the second stage. ϵ_{it} is the error term. Results in the first-stage, presented in Table 12, suggest a strong causal relationship between distance from disrupted stockpiles and the estimated assault rifle price. However, the relationship is sensitive to the inclusion of particular control variables and changes in the samples size.

In Table 12 I estimated the first stage for four different models. Distance from disrupted stockpiles in Table 12 has a positive and significant effect on the estimated assault rifle price in models 1,2, and 4. Those same models also have F-statistics above 10, suggesting that distance to disrupted stockpiles is a strong instrument. In model 1 no country-controls are included. In model 2 the most essential country controls are included. Those are the natural log of population, GDP per capita, and a dichotomous variable that indicates whether a conflict ongoing in the prior year.

Table 12: First Stage Regressions (OLS): Assault Rifle Prices and Distance from Disrupted Stockpiles

	(1)	(2)	(3)	(4)
	Estimated Small Arms Prices	Estimated Small Arms Prices	Estimated Small Arms Prices	Estimated Small Arms Prices
Distance	9.696***	5.654***	-1.832	4.537**
	(0.788)	(0.977)	(2.913)	(1.811)
Natural Log of Population		-13.18**	-50.46**	-23.54*
		(6.501)	(19.66)	(12.43)
Natural Log of GDP per Capita		92.74***	113.7***	104.7***
		(6.282)	(21.33)	(15.07)
Active Conflict Last Year		113.0***	221.8***	163.1***
		(25.54)	(60.63)	(41.68)
Natural Log of Oil Rents per Capita			14.43+	11.24+
			(9.366)	(6.875)
Natural Log Mineral Rents per Capita			13.25+	13.85**
			(9.013)	(6.368)
Natural Log of Military Expenditure per Capita			-92.28*	
			(48.12)	
Natural Log of Army Personel (% of the Labor Force)			-9.599	
			(33.73)	
Imputed Road Density			-13.98	0.951
			(59.74)	(39.12)
Imputed GINI			-3.156	-2.909+
			(2.501)	(1.799)
Observations	4471	3887	975	1550
Adjusted R^2	0.033	0.076	0.071	0.078
F-Statistic	151.45	80.52	8.40	17.41

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In model 3 the full vector of controls is included and the coefficient on distance has a negative, albeit insignificant, relationship with assault rifle prices. This result is puzzling because, looking at the reduced form of the two stage regression in Table 13, the sign on distance to disrupted stockpiles is negative like the other ones. The discrepancy is caused by mild multicollinearity and coefficient sharing between the instrument and the control variables. I calculated the variance inflation factors for the model 3 in the first stage and none was above 5, suggesting the absence of strong multicollinearity (See Appendix D). However, there was strong reason to suspect a systematically collinear relationship between distance to disrupted stockpiles and the measures of state power. [15]

Both the natural log or army personnel as a percentage of the labor force and the natural log of military expenditures per capita measure the amount of resources the state devotes to the military. Because most of the shocks described in Table 11 come in the form of a drastic civil war outcome it is logical to expect that countries that are of a closer proximity to the disrupted stockpiles would respond to the regional instability by raising their military spending. In these cases distance from these stockpiles can measure how concerned a country would be after seeing a revolution occur nearby. I confirmed this theory by regressing both measures of state power against distance to disrupted stockpiles. Distance from the stockpiles has a significant and negative relationship with military expenditures and the percentage of the labor force in the army.

The relationship between the measures of state power and distance to disrupted stockpiles represents a violation of the second requirement for an instrumental variables. That requirement states that the instrumental variable must be independent of the control variables used in the second stage. The reason that there is a negative sign on distance in the first stage is because the two measures of state power are explaining some of the same variation in assault rifle prices as distance to the stockpiles. In model 4 the first stage no longer captures the effect of distance from stockpiles on the price of small arms; instead that effect is captured by the coefficients on the two control variables. It is important to note here that there is no evidence that the effect of being closer to a stockpile is that the country preemptively increases military spending and diminishes the prospects for a civil conflict. If that were true than the sign on distance would be positive in the reduced form.

The available remedy in this instance is to drop the control variables that measure state power, which I do in model 4 of Table 12. Dropping these measures introduces the possibility of omitted variable bias in the 2SLS model. In return we can better observe the causal effect of being closer to one of these stockpiles on the risk of civil conflict onset. Additionally, while we have theoretical

[15] I only use imputed measures of the GINI coefficient and road density when for the models utilizing instrumental variables. Both of these variables have a reporting bias that slants the available data towards countries on the European and American continents. While neither variable is correlated with distance from disrupted stockpiles this reporting bias diminishes the instruments predictive power. I used an OLS regression to test if being closer to a disrupted stockpile made the country less likely to report the GINI coefficient, road density, oil rents, and mineral rents. There was a negative and significant relationship between being closer to a stockpile and failing to report for all four variables. Using imputed measures for the variables that are largely static is one way I can circumvent the effect of the reporting bias on the estimated relationship between assault rifle prices and distance to disrupted arms stockpiles

priors that the two measures of state power have a negative effect on the risk of civil conflict, the relationship has yet to be robustly demonstrated in the literature.[16] In Table 5 model 4 the coefficients on both are negative and the sign on military expenditure defies theory. In this instance I am willing to accept the risk of omitted variable bias by treating model 4 of Table 12 as the full model. When the two measures of state power are dropped in model 4, the coefficient on distance from disrupted stockpiles becomes positive and significant.

The scatter plot below demonstrates how distance from these stockpiles has a positive relationship with the estimated assault rifle prices when it is from all stockpiles.

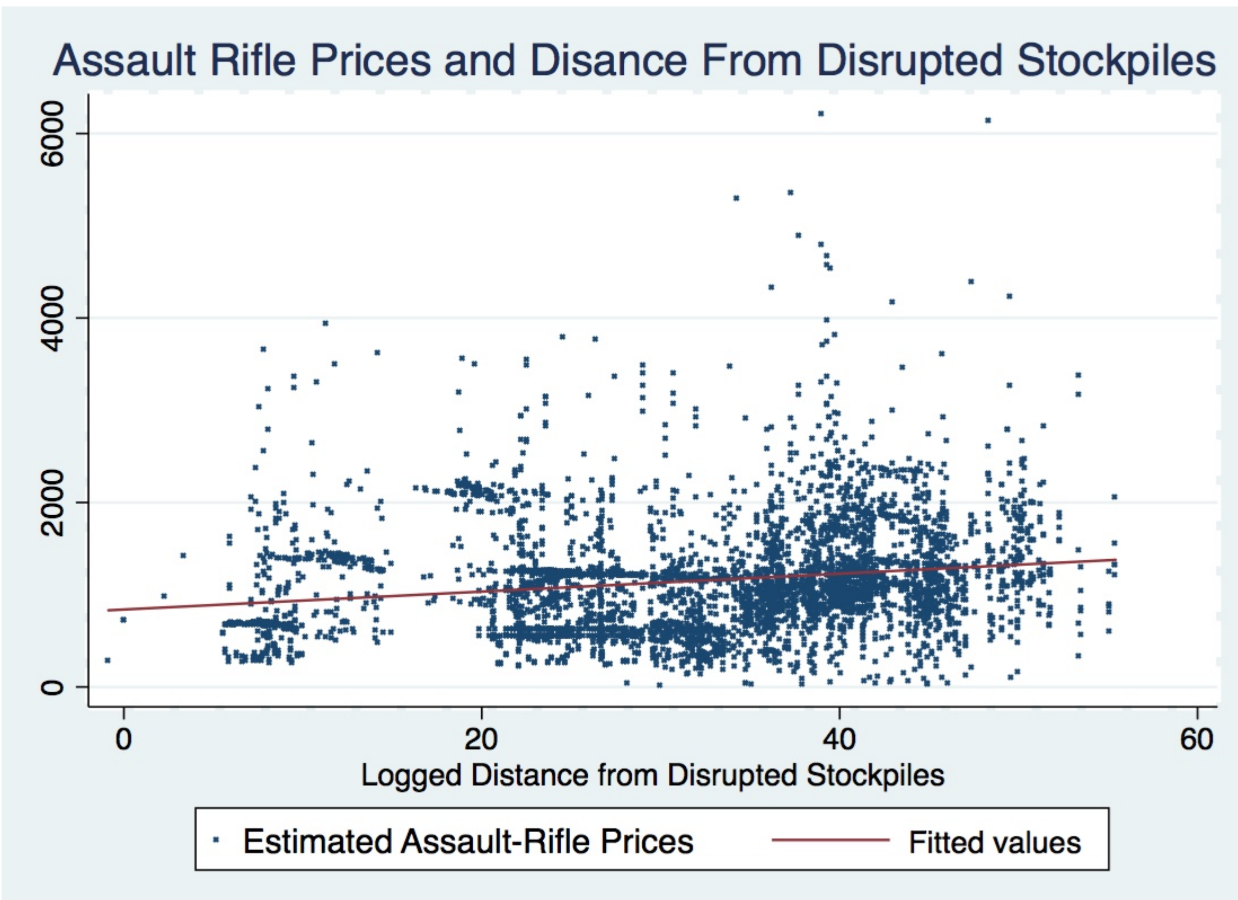

Figure 8: First Stage Relationship: Distance to Stockpiles and Assault Rifle Prices (Marsh and Mcdougal 2017)

There is a concern that my list of disrupted stockpiles might just constitute a convoluted Africa dummy variable given that 7 out of the 12 stockpiles are on the African continent. The assault rifle

[16] Collier and Hoeffler (2002) has found evidence that on average military expenditures tend to rise in response to conflict and suggests that military expenditures are a poor measure of state power.

price summary statistics in Table 1 do show that African regions have the lowest average assault rifle prices. It is plausible that the first stage coefficients are significant only because they explain the lower price of small arms on the African continent when compared to the global average. I show that this is not the case in the set of scatter plots below. Distance from disrupted stockpiles has a positive relationship with assault rifle prices estimates across all regions except for Oceania. It makes sense that there would be no relationship with estimates in Oceania because there are no stockpiles in Oceania.

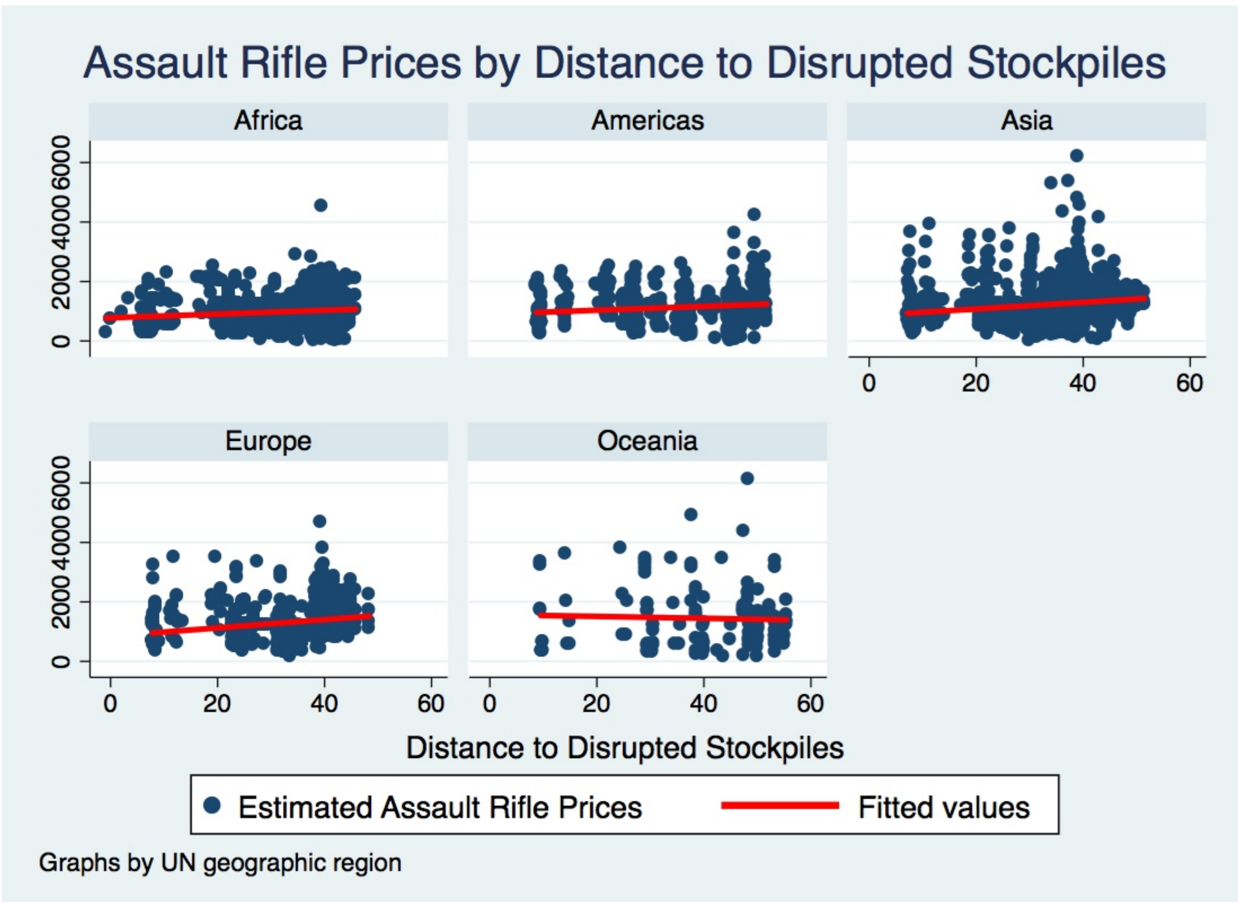

Figure 9: First Stage Relationship by UN Region (Marsh and Mcdougal 2017)

To further demonstrate that this relationship is robust to shifts in region, I used an OLS estimator to regress assault rifle prices against distance to disrupted stockpiles by region (See Appendix E). There is a significant relationship between distance from disrupted stockpiles and the estimated assault rifle prices on each region except for the American continents where there is only one stockpile.

The reduced form of the two staged regression, presented in Table 13, demonstrates that there is a negative relationship between distance from disrupted stockpiles and the risk of civil conflict onset. The relationship is significant at the 1% level in both the bivariate model, model 1, and in model 2 when the essential country controls are included. Those controls are the natural log of population, the natural log of GDP per capita and a dichotomous variables indicating whether or not a civil conflict is ongoing. However, the relationship is not robust to the inclusion of additional controls in models 3 and 4. In those latter models, the sign on distance from disrupted stockpiles remains negative, however the coefficient is cut in half. [17]

[17] In proceeding to the two staged instrumental variable regression, I only test models 1,2, and 4. Model 3 could not be tested because using the full vector of controls in a two staged regression reduces the number of observed onsets to zero. I included it in the first stage and reduced form to demonstrate a flaw with my instrument.

Table 13: Reduced Form (OLS): Civil Conflict Onset and Distance from Disrupted Stockpiles

	(1)	(2)	(3)	(4)
	Onset as the 1st Death	Onset as the 1st Death	Onset as the 1st Death	Onset as the 1st Death
Distance from Disrupted Stockpiles	-0.00107***	-0.00101***	-0.000349	-0.000470
	(0.000191)	(0.000238)	(0.000619)	(0.000422)
Log of Population		0.00582***	0.0171***	0.0139***
		(0.00158)	(0.00418)	(0.00290)
Log of GDP per Capita		-0.00866***	-0.0114**	-0.0172***
		(0.00153)	(0.00453)	(0.00352)
Active Conflict Last Year		0.00103	-0.0246*	-0.0129
		(0.00621)	(0.0129)	(0.00972)
Log of Oil Rents per Capita			0.00207	0.00298*
			(0.00199)	(0.00160)
Log Mineral Rents per Capita			-0.00522***	-0.000175
			(0.00192)	(0.00148)
Log of Military Expenditure per Capita			0.0165+	
			(0.0102)	
Log of Army Personel (% of the Labor Force)			-0.0216***	
			(0.00717)	
Imputed Road Density			-0.0162	0.000321
			(0.0127)	(0.00912)
Imputed GINI			-0.00106**	-0.00113***
			(0.000532)	(0.000420)
Observations	4471	3887	975	1550
Adjusted R^2	0.007	0.022	0.060	0.037

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

9.1 2nd Stage Results

The following structural equation (5) represents the second stage of the instrumental variable regression. In it the fitted values from the first stage $\hat{D}$ are used to estimate the effect of assault rifle prices on the risk of civil conflict onset. X is the vector of controls and ϵ_{it} is the error term.[18]

$$ConflictStart_{it} = \beta_0 + \beta_1 \hat{D}_{it} + X_{it} + \epsilon_{it} \tag{6}$$

Two different estimators are used to estimate this equation (5): IV-2SLS and IV-probit.[19] Both use standard errors clustered at the country level. These are the preferred estimators for models with endogenous independent variables when the dependent variable is a dichotomous measure of civil conflict (Miguel et al. 2004, 738-744) (Killicoat 2006, 50-56). An incredibly simplified explanation of the differences between these two estimators is that, in IV-2SLS the second stage is an OLS regression and in IV-Pobit the second stage is a probit model.[20] Using a least squares estimator (IV-2SLS) with a dichotomous dependent variable comes with inferential drawbacks since the predicted values are not bounded between 0 and 1. Consequentially, I default to using the marginal effects estimates from IV-Probit. Fixed effects were not included because they cannot be used with a probit estimator.

The results for the two-stage regressions with instrumental variables are presented in Tables 14 and 15. The ordering of the models is same as in the first stage and reduced form regressions. In both the IV-Probit and IV-2SLS estimators, and across the models, the price of assault rifles has a negative relationship with the risk of conflict onset. The relationship is significant at the 1% level for the first two models but in neither estimator is the relationship robust to the full vector of controls in model 3. The insignificance of model 3 is likely the consequence of a large drop off in the observed number of conflict onsets (from 84 to 36). Notably, the estimated coefficients on model 3, for both estimators, are very similar to the estimated coefficients in model 2. In IV-Probit the coefficient changes from -0.00147 to -0.00113 and in the 2SLS estimator the coefficient changes from -0.000179 to -0.000104. The similarity in coefficient estimates leads me to conclude that the lack of significance in model 3 is due to the change in the degrees of freedom.

[18]For simplicities sake the control for an active conflict last year was included in the vector of controls.

[19]The corresponding stata commands are ivregress 2sls and ivprobit.

[20]In actuality IV Probit is not even a formal two-stage regression, it is a control regression that works backwards from the second stage. For more information see ivprobit help in stata.

Table 14: IV Probit: Civil Conflict Onset and Assault Rifle Prices

	(1)	(2)	(3)
	Onset as the 1st Death	Onset as the 1st Death	Onset as the 1st Death
Estimated Small Arms Prices	-0.00127***	-0.00147***	-0.00113
	(0.0000980)	(0.000125)	(0.000791)
Log of Population		0.0364	0.144
		(0.0313)	(0.144)
Log of GDP per Capita		0.0326	-0.164
		(0.0610)	(0.317)
Active Conflict Last Year		0.165	-0.0644
		(0.133)	(0.390)
Log of Oil Rents per Capita			0.0831+
			(0.0549)
Log Mineral Rents per Capita			0.0119
			(0.0390)
Imputed Road Density			-0.0740
			(0.131)
Imputed GINI			-0.0181+
			(0.0120)
Observations	4471	3887	1550
Correctly Classified	97.34%	89.79%	97.29%
Observed Onsets	98	84	36

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: 2SLS-IV: Civil Conflict Onset and Assault Rifle Prices

	(1) Onset as the 1st Death	(2) Onset as the 1st Death	(3) Onset as the 1st Death
Estimated Small Arms Prices	-0.000110***	-0.000179***	-0.000104
	(0.0000252)	(0.0000687)	(0.000124)
Log of Population		0.00346	0.0115**
		(0.00338)	(0.00555)
Log of GDP per Capita		0.00793	-0.00639
		(0.00786)	(0.0155)
Active Conflict Last Year		0.0212	0.00397
		(0.0186)	(0.0285)
Log of Oil Rents per Capita			0.00415*
			(0.00225)
Log Mineral Rents per Capita			0.00126
			(0.00399)
Imputed Road Density			0.000419
			(0.0110)
Imputed GINI			-0.00143*
			(0.000835)
Observations	4471	3887	1550
Observed Onsets	98	84	36

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A troubling shift in both models from the reduced form in Table 13 to the two-stage regressions in Tables 14 and 15 is the change on the significance of population and GDP per capita (see model 2 in Tables 13, 14, and 15). While both variables are significant in the reduced form, they become insignificant in the second stage and the sign on GDP per capita becomes negative. This result is more concerning in model 2 than in model 3 simply because in model 3 the lack of significance can be attributed to the small sample of onsets. This shift in the sign could be a sign of a deeper flaw in the models. My current explanation is that because population and GDP per capita were highly related to small arms prices in the first stage, the predicted values used in the second stage capture variation in GDP per capita and population. Consequently, I view these results as presenting a biased view of how GDP per capita and population affect the risk of civil conflict onset.

Marginal effects and elasticities are presented in Table 14. The marginal effects between the models vary by an order of magnitude. However the effects disappear as we move to elasticities. Overall a 10% decrease in the price of the assault rifle corresponds to an increased risk of civil conflict by 1.2-3.2%. The estimated elasticities and marginal effects in model 3 are likely closest to the 'true' relationship of all the models given that models 1 and 2 likely suffer from omitted variable bias. The elasticities in model 3 for both estimators suggest a 1.2% increase in the risk of conflict onset for every 10% decrease in the assault rifle price.[21] Overall the estimated effects are fairly consistent across models regardless of the selected vector of controls and the estimator used.

These marginal effects are of a much higher magnitude than the marginal effects obtained by the last study of small arms prices and civil conflict onset, Killicoat (2006). Killicoat (2006) found that a 10% decrease in the price of the assault rifle would raise the risk of civil conflict by approximately 0.5%. One notable reason for this discrepancy is that I use a much lower deaths threshold. My threshold for a civil conflict onset is the first battle-related deaths where as Killicoat (2006) uses 1000 deaths. I view this difference as evidence in support of a lower deaths threshold for defining civil conflict. These results suggest that the initiation of a conflict is a phenomena that can be affected by a reduction in small arms abundance. However, if we look at conflict as only being relevant after the 1000 death then any disarmament policy would appear far more inconsequential.

There are other explanations for the discrepancy between the marginal effects. Killicoat (2006) used a much smaller dataset of small arms prices, and his dataset was of observed prices while

[21] Because the outcome in this case is a probability of conflict I calculate elasticities as a unit change in Y for a % change in X

significant portions of the data from Marsh and Mcdougal (2017) are interpolated. It is important to reiterate here that these marginal effects are likely very far from the true marginal effect of a decrease in small arms prices because of how far the used assault rifle prices are from the actual prices. In fact the true marginal effect could easily better resemble the results from Killicoat (2006). The key takeaway is not the magnitude of the effect, but that the relationship is negative, significant, and appears to begin with a change in small arms abundance.

Table 16: Marginal Effects and Elasticities: Assault Rifle Prices (X) and the Risk of Conflict Onset (Y)

	Model 1	Model 2	Model 3
Marginal Effects (dy/dx) for IV Probit	-.0002482 (0.0000605)	-.0003606 (.0000931)	-.0001514 (.0003233)
Elasticities (dy/ex) for IV Probit	-.2038528 (.055543)	-.3153721 (.1025132)	-.1220737 (.2714988)
Marginal Effects (dy/dx) for IV-2SLS	-0.000110 (0.0000252)	-0.000179 (0.0000687)	-0.000104 (0.000124)
Elasticities (dy/ex) for IV-2SLS	-.1268999 (.0291206)	-.2085352 (.0800576)	-.1227129 (.1463511)

*Standard errors in parentheses

* Elasticities were calculated as dy/ex because the outcome is already a change in the probability of civil conflict onset.

The models in Table 15 have a very high classification rate because, like the initial onset models, they classify all country-years as experiencing no conflict onset. The one exception to this trend is in model 2 and it is likely because of the positive sign on GDP. In model 3 the sign goes back to negative and the model estimates no-conflict onset for all country-years in the sample. Figure 10 shows the predicted probabilities of civil conflict onset assuming that all independent variables are at their mean other than assault rifle prices. The predicted probabilities were calculated using the IV-Probit estimates from model 3 in Table 15 and are plotted against the estimated assault rifle prices. Notably, not one observation was assigned a predicted probability above 0.5. However, it does show a declining risk of civil conflict onset as assault rifle prices increase. For the actual price estimates overlaid on the curve see appendix F. Figure 10 also shows the range of small arms prices at which a civil conflict is feasible. Model 3 in Table 15 predicts that there is a 0.00% chance of civil conflict when the price of an assault rifle is above $2000.

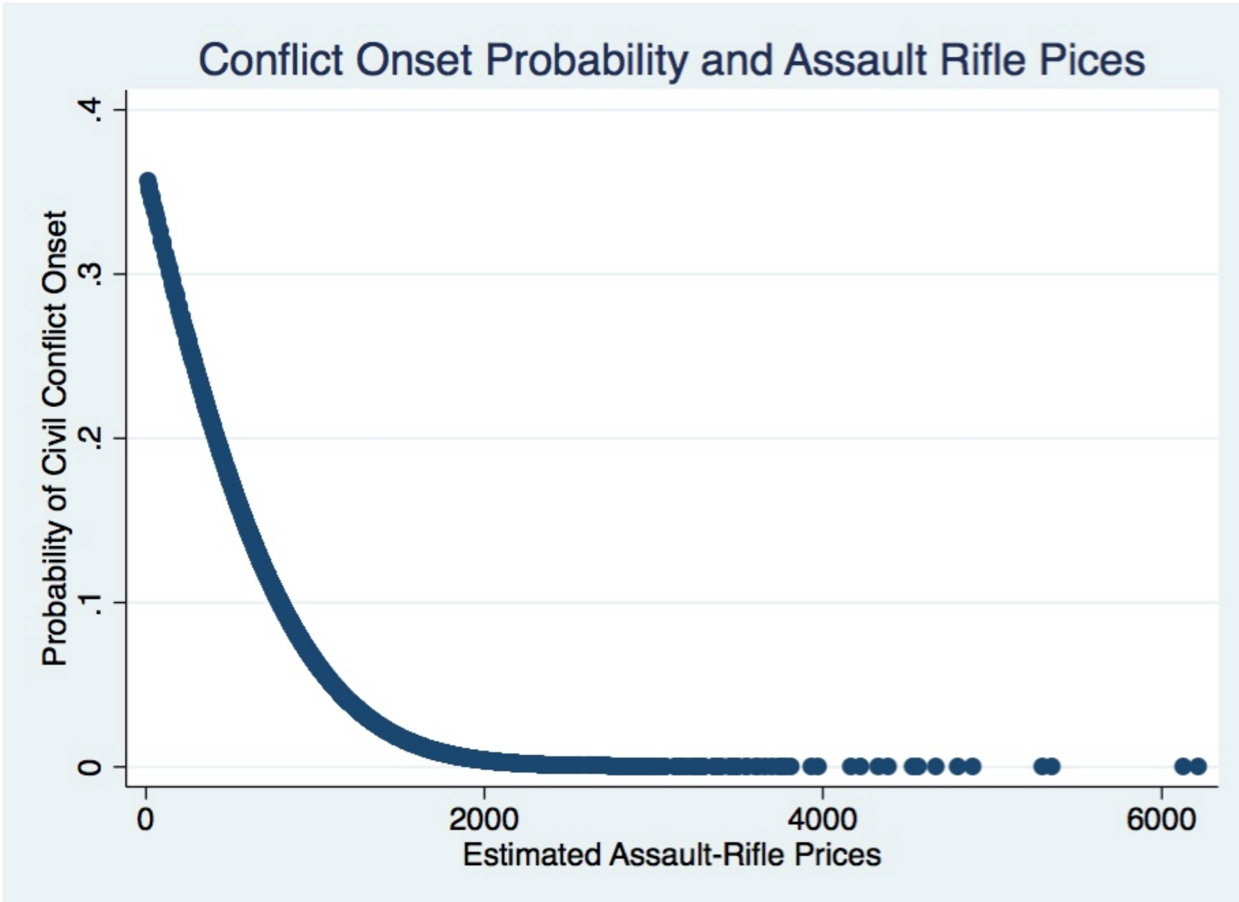

Figure 10: Predictions were made using Model 3 in Table 15

9.2 Exclusionary Restriction Concerns

The regression analysis above does not present conclusive proof of a negative causal relationship between small arms abundance and conflict onset because distance from disrupted stockpiles potentially violates of the exclusionary restriction. Instrumental variables need to meet three requirements for us to assume that they present an unbiased causal effect. The variable must have a causal effect on the independent variable (assault rifle prices), be independent of variables that might be controlled for (GDP per capita, Military Expenditures), and should only effect the dependent variable of interest (civil conflict) through its effect on the independent variable of interest (assault rifle prices) (Angrist and Pischke 2014, 106). Distance from disrupted stockpiles has a significant relationship with the price of assault rifles and is robust to multiple regions as well as the amended vector of control variables. However, I cannot be certain that the only way distance from disrupted stockpiles influences the risk of civil conflict onset is through its effect on the price of small arms. This last requirement is referred to as the exclusion restriction.

The total distance from 12 disrupted stockpiles also includes the total distance from 11 war-torn countries. This overlap is problematic because civil conflict in one country is often said to have a "neighborhood" effect that raises the risk of civil war in adjacent countries (Buhaug and Gleditsch 2008). The exist of a general trend is not problematic because my proposed mechanism might explain why civil wars tend to cluster geographically. But, there are a number of specific mechanisms that increase risk of conflict onset in countries adjacent to a civil conflict.

Notably, the countries that are the closest to disrupted stockpiles tend to neighbor at least one of the stockpiling countries. This fact suggests that my instrument could be picking up neighborhood effects which are following the same correlation as small arms abundance and distance to disrupted stockpiles. In both cases there is a negative relationship between distance and the risk of civil conflict onset. Neighborhood effects have the strongest impact on the risk of civil conflict onset when the distance to disrupted stockpiles is lowest. Consequentially, the neighborhood effects could be the source of the negative relationship between the price of an assault rifle and the probability of civil conflict that is presented in the second stage (Tables 14-15).

The most well defined neighborhood effects are cross-border ethnic connections (Buhaug and Gleditsch 2008), refugees (Salehyan and Gleditsch 2006), and regional economic shocks (Murdoch and Sandler 2004). Buhaug and Gleditsch (2008) found that civil war in a neighboring country raises the risk of civil war in the current country when both countries share members of the same ethnic group. The authors explain that the mobilization of ethnic groups in one country encourages the mobilization of ethnic groups in the neighboring country (Buhaug and Gleditsch 2008, 220). Buhaug and Gleditsch (2008) also argued that cross-border linkages exacerbate the refugee problem. Salehyan and Gleditsch (2006) found that the number of refugees in a country correlated with its risk of conflict onset. The author speculated that this effect could be related to the economic strain that refugees place on the economy, the potential for former fighters to use refugee networks for shelter, and the rise of tensions within the country related to the presence of refugees. Finally, Murdoch and Sandler (2004) showed that a country's proximity to a civil conflict correlates with diminished economic growth. These economic shifts can in turn reduce the opportunity cost of joining an insurgency and make it easier to recruit (Collier and Hoeffler 2001).

The existence of these mechanisms does not perfectly impeach my instrument. Distance from disrupted stockpiles does not measure perfectly how close a country is to an ongoing conflict. Rather, it measures how close a country is to a set of countries that experienced a civil conflict within the last 10-37 years. Additionally, because distance from the stockpiles is not accounted for until the year when the shock happened, which in most cases is when the conflict ended, there is less overlap with the neighborhood effects of concurrent conflicts.

These mechanisms are also not mutually exclusive from the effect of small arms abundance. In fact small arms abundance likely complements each other mechanism to the degree that it is difficult to determine the other mechanisms true effect. For instance, Salehyan and Gleditsch (2006) suggests that one of the mechanisms through which refugees raise the risk of conflict is because they bring weapons from the neighboring civil conflict. The case of the Tuareg in Libya illustrates that the looting of small arms from state depots can explain how co-ethnics separated by a border can empower each other (Nossiter 2012). Finally, Murdoch and Sandler (2004) suggests that causality might run from civil conflict to diminished economic growth. Murdoch and Sandler (2004) could be observing the effect of small arms abundance raising the risk civil conflict onset and then the effect of civil conflict onset on debilitating economic growth. Because each causal mechanism is connect to each other causal mechanism, the exact make-up of neighborhood effects cannot be perfectly decomposed. The effect of small arms abundance in raising the risk of civil conflict is, therefore, just as likely as any of the other neighborhood effects.

Numerous other mechanisms describe how distance from disrupted stockpiles could affect the risk of civil conflict onset. The first stage estimates demonstrate that distance from disrupted arms stockpiles explains variation in assault rifle prices; but, we cannot be sure that it is the effect of distance from disrupted stockpiles on the price of small arms that is producing the negative relationship in the second stage. The coefficient estimates from the two stage estimator do offer evidence in support of a causal and negative relationship between assault rifle prices and the risk of civil conflict onset. However, it is prudent to treat these results with suspicion due to the existence of multiple causal mechanisms.

10 Robustness Checks

I tested the sensitivity of my results to changes in the threshold of deaths used to define civil conflict onset and the exclusion of non-state violence from the count of battle-related deaths. Tables 17-19 re-run the essential regressions with the threshold of 25 battle-related deaths instead of 1 battle-related death. In both tables 17 and 18 the dependent variable is civil conflict onset. In Table 17 I use two primary independent variables: the lagged percentage change in small arms prices and lagged small arms prices. For comparison, I am essentially re-estimating models 1 and 7 of tables 2 and 3. The magnitudes and significance of the coefficient estimates does not change at all. In models 1 and 2 in table 17, where the independent variable is the lagged price of small arms, the estimated coefficient is essentially 0. When the measure is shifted to the lagged percentage change in the price of small arms the estimated coefficient is still insignificant and the size of the coefficients seems to only increase by around 0.008.

In Table 18 I re-estimate the IV-Probit models in Table 9 using the 25 battle-related death threshold. In both models, the coefficient estimates for the measures of small arms abundance do not differ substantially from previous estimates. Once again all coefficient estimates are around the same magnitude. However, one notable difference is that the coefficient estimate for estimated small arms prices in model 3 is now significant at the 10% level.

Next, I re-estimate the models of civil conflict intensity but I change the dependent variable exclude the battle-related deaths that occurred from fighting between non-state armed groups. Those results are presented in Tables 20 and 21. Those results do not differ from the initial estimation in significance or in coefficient size. The coefficients on the lagged price of small arms remain at approximately 0, and the marginal effect of a 1% increase in the price of small arms last year is between -0.34 and -0.5 depending on which full model is used.

Table 17: Robustness Check (Probit): 25 Death Onset the Lagged Price of Small Arms and the Lagged Percentage Change

	(1)	(2)	(3)	(4)
	Onset as the 25th Death	Onset as the 25th Death	Onset as the 25th Death	Onset as the 25th Death
Lagged Price of Small Arms	-0.00000863	0.000106		
	(0.0000700)	(0.0000866)		
Lagged Percentage Price Change			0.0261	0.0257
			(0.0251)	(0.0293)
Natural Log of Population	0.109*	0.157**	0.131**	0.224***
	(0.0644)	(0.0717)	(0.0627)	(0.0654)
Natural Log of GDP per Capita	-0.201***	-0.290***	-0.196***	-0.316***
	(0.0393)	(0.0595)	(0.0402)	(0.0651)
Active Conflict Last Year	0.632***	0.565**	0.665***	
	(0.116)	(0.226)	(0.122)	
Natural Log of Oil Rents per Capita		0.0606+		0.0671+
		(0.0384)		(0.0410)
Natural Log Mineral Rents per Capita		0.0134		0.00416
		(0.0314)		(0.0310)
Imputed Road Density		0.218		0.207
		(0.187)		(0.169)
Imputed GINI		-0.00864		-0.00328
		(0.0112)		(0.00981)
Observations	4079	995	3950	995
Pseudo R^2	0.153	0.276	0.165	0.255
Observed Onsets	178	63	168	63
Correctly Classified	95.64%	93.57%	95.75%	93.77%

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: Robustness Check (IV-Probt): 25 Death Onset and Estimated Assault Rifle Prices

	(1)	(2)	(3)
	Onset as the 25th Death	Onset as the 25th Death	Onset as the 25th Death
Estimated Small Arms Prices	-0.00104***	-0.00135***	-0.00109*
	(0.000125)	(0.000193)	(0.000605)
Log of Population		0.0420	0.0418
		(0.0386)	(0.0686)
Log of GDP per Capita		0.0471	-0.0488
		(0.0603)	(0.176)
Active Conflict Last Year		0.663***	0.864***
		(0.167)	(0.320)
Log of Oil Rents per Capita			0.0431
			(0.0315)
Log Mineral Rents per Capita			0.0337+
			(0.0221)
Imputed Road Density			0.154
			(0.165)
Imputed GINI			-0.0106
			(0.0101)
Observations	4471	3887	1550
Observed Onsets	181	163	86
Correctly Classified	95.95%	91.41%	92.39%

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19: Robustness Check(GLS with Driscoll-Kaay Standard Errors): Intensity With Omitted Years and Each Measure of Small Arms Abundance

	(1) Log of Batte-Deaths	(2) Log of Batte-Deaths	(3) Log of Batte-Deaths	(4) Log of Batte-Deaths
Lagged Price of Small Arms	0.00000985	0.000136		
	(0.000124)	(0.000137)		
Lagged Percentage Price Change			-0.0967***	-0.157**
			(0.0272)	(0.0574)
Log of Population	0.269***	0.304**	0.269***	0.274*
	(0.0560)	(0.136)	(0.0573)	(0.136)
Log of GDP per Capita	-0.227***	-0.420**	-0.212***	-0.366**
	(0.0726)	(0.158)	(0.0671)	(0.156)
Number of Supporters		0.581***		0.611***
		(0.0967)		(0.103)
Democracy		0.375		0.415
		(0.341)		(0.312)
Log of Oil Rents per Capita		0.0346		0.0348
		(0.0729)		(0.0696)
Log Mineral Rents per Capita		0.231***		0.221***
		(0.0554)		(0.0558)
Natural Log of Army Personel (as a % of the Labor Force)		0.0734		0.0612
		(0.140)		(0.143)
Natural Law of Military Expenditure per Capita		0.226 ***		0.217*** ***
		(0.0592)		(0.0582)
Imputed GINI		-0.0695***		-0.0710***
		(0.0150)		(0.0151)
Imputed Road Density		-0.993***		-1.022***
		(0.195)		(0.199)
Observations	778	270	746	270
R^2	0.050	0.359	0.054	0.365
F-Statistic	26.55	80.76	26.29	92.38

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: Random Effects GLS model with Driscoll-Kraay Standard Errors: Battle Related Deaths from State-Based Conflicts and. Lagged Percentage Small Arms Change

	(1) Natural Log of Battle-Deaths	(2) Natural Log of Battle-Deaths	(3) Natural Log of Battle-Deaths	(4) Natural Log of Battle-Deaths	(5) Natural Log of Battle-Deaths	(6) Natural Log of Battle-Deaths	(7) Natural Log of Battle-Deaths	(8) Natural Log of Battle-Deaths
Lagged Percentage Price Change	-0.0919***	-0.0867***	-0.0767***	-0.0971***	-0.0841***	-0.142*	-0.343*	-0.508**
	(0.0219)	(0.0178)	(0.0192)	(0.0162)	(0.0133)	(0.0705)	(0.177)	(0.226)
Natural Log of GDP per Capita	-0.323***	-0.315***	-0.217***	-0.429***	-0.468***	-0.600***	-0.539*	-0.684***
	(0.0622)	(0.0580)	(0.0628)	(0.103)	(0.128)	(0.0819)	(0.297)	(0.175)
Natural Log of Population	0.234***	0.290***	0.355***	0.381***	0.403***	0.279**	0.545**	0.547**
	(0.0685)	(0.0629)	(0.0499)	(0.0890)	(0.134)	(0.118)	(0.232)	(0.229)
Number of Supporting Countries		0.689***	0.730***	0.902***	0.767***	0.616***	0.747***	0.965***
		(0.0683)	(0.0727)	(0.105)	(0.0968)	(0.108)	(0.119)	(0.177)
Democracy			-0.643***	-0.0946	-0.0681	0.355	-0.157	0.361
			(0.142)	(0.246)	(0.292)	(0.311)	(0.475)	(0.319)
Natural Log of Oil Rents per Capita				0.182***	0.0949**	0.0867*	0.0253	0.179***
				(0.0302)	(0.0369)	(0.0442)	(0.0704)	(0.0512)
Natural Log Mineral Rents per Capita					0.120**	0.222***	0.210**	0.370***
					(0.0440)	(0.0469)	(0.0800)	(0.0748)
Natural Log of Army Personel (% of the Labor Force)						0.287**	0.329+	0.298
						(0.134)	(0.210)	(0.452)
Natural Log of Military Expenditure per Capita						1.084***	1.099**	1.097**
						(0.183)	(0.491)	(0.490)
Road Density (% per square mile)							-0.122	
							(0.809)	
GINI								-0.0697***
								(0.0225)
Observations	791	791	757	499	397	275	155	98
R^2	0.062	0.198	0.204	0.286	0.287	0.402	0.543	0.541

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: Random Effects GLS Model with Driscoll-Kraay Standard Errors: Battle-Related Deaths from State Based Conflicts and Lagged Assault Rifle Prices

	(1) Natural Log of Battle-Deaths	(2) Natural Log of Battle-Deaths	(3) Natural Log of Battle-Deaths	(4) Natural Log of Battle-Deaths	(5) Natural Log of Battle-Deaths	(6) Natural Log of Battle-Deaths	(7) Natural Log of Battle-Deaths	(8) Natural Log of Battle-Deaths
Lagged Assault Rifle Prices	-0.00000482	-0.0000907	-0.0000448	-0.0000419	0.0000208	0.000162^{+}	0.000119	0.000298^{+}
	(0.0000948)	(0.000103)	(0.000110)	(0.000104)	(0.0000785)	(0.000108)	(0.000220)	(0.000189)
Natural Log of GDP per Capita	-0.330^{***}	-0.302^{***}	-0.216^{***}	-0.438^{***}	-0.491^{***}	-0.634^{***}	-0.570^{*}	-0.764^{***}
	(0.0634)	(0.0523)	(0.0600)	(0.104)	(0.125)	(0.0823)	(0.302)	(0.179)
Natural Log of Population	0.232^{***}	0.283^{***}	0.345^{***}	0.358^{***}	0.388^{***}	0.322^{***}	0.525^{**}	0.716^{***}
	(0.0646)	(0.0627)	(0.0515)	(0.0939)	(0.137)	(0.112)	(0.244)	(0.144)
Number of Supporting Countries		0.700^{***}	0.734^{***}	0.888^{***}	0.756^{***}	0.585^{***}	0.719^{***}	0.995^{***}
		(0.0707)	(0.0704)	(0.104)	(0.0968)	(0.102)	(0.0985)	(0.195)
Democracy			-0.610^{***}	-0.0511	-0.0267	0.316	-0.223	0.125
			(0.153)	(0.266)	(0.306)	(0.336)	(0.483)	(0.328)
Natural Log of Oil Rents per Capita				0.186^{***}	0.102^{***}	0.0776^{+}	0.00667	0.145^{**}
				(0.0285)	(0.0351)	(0.0459)	(0.0693)	(0.0620)
Natural Log Mineral Rents per Capita					0.125^{***}	0.231^{***}	0.220^{**}	0.387^{***}
					(0.0441)	(0.0477)	(0.0854)	(0.0706)
Natural Log of Army Personel (% of the Labor Force)						0.293^{**}	0.264	0.158
						(0.130)	(0.239)	(0.452)
Natural Log of Military Expenditure per Capita						1.082^{***}	1.090^{**}	1.048^{*}
						(0.177)	(0.469)	(0.525)
Road Density (% per square mile)							-0.143	
							(0.769)	
GINI								-0.0605^{**}
								(0.0222)
Observations	823	823	789	516	408	275	155	98
R^2	0.058	0.200	0.205	0.273	0.278	0.399	0.535	0.519

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

11 Discussion and Conclusion

This paper attempts to evaluate the effect of small arms abundance on to the risk and intensity of civil conflict. It differs from previous work by using a new dataset of small arms prices, a more precise definition of conflict, and the use of a new instrumental variable. The contributions that this paper makes are best subdivided into theories and knowledge of civil conflict, policy implications, and lessons for future research.

11.1 Civil Conflict

In regards to what is know about civil conflict as a phenomenon, this paper's biggest contribution is to what we know about civil conflict intensity. I find that lagged percentage changes in assault prices are negatively correlated with the intensity of civil conflict in the current period. Furthermore, the relationship is robust to the omission of outliers, and changes to the threshold used to define civil conflict. Using the results from model 2 in Table 10, if the price of an assault rifle dropped by 50% in the prior period we would expect an 4.2% increase in the number of battle-related deaths. While that effect on intensity is large, due to the amount of measurement error on both sides, particularly the use of estimated small arms prices, those changes are likely very far from the true marginal effect. Instead of focusing on the relative effects, the important story is that there is a negative and a significant relationship.

The relationship also implies, but does not prove, that causality runs from small arms abundance to increased civil conflict intensity. If we were to assume that causality runs from violence in the current period to the change in prices last period these results would suggest that rebels, or civilians, are anticipating violence and then deciding that they do not need an additional assault rifle. Additionally, we can be confident that price shifts are largely due to supply side variation because most price shifts during conflict are negative. In theory, demand for small arms over the course of a conflict should be relatively constant (Florquin 2014) (Killicoat 2007). Also it is unintuitive to suggest that a large decline in small arms prices occurred because of a demand-side shift, implying that people are less concerned with self-defense, when the decline in prices is shown to be followed by an increase in battle-related deaths the following year.

One drawback of my approach is that I was unable to identify the precise causal mechanisms at work. This finding provides evidence in support of both the escalation effect discussed by Sisilin and Pearson (2001) and the fuel effect. However, these findings do not disprove the fragmentation effect since it could just be overwhelmed by the other two effects. There are some methodological approaches that can be taken in the future to distinguish among the competing effects. The escalation effect is likely to be observed in conflicts divided along ethnic lines. In ethnic conflict there is additional fear that when weapons become available opposing ethnic groups could use them to attack civilian co-ethnics (Sisilin and Pearson 2001). Controlling for the type of conflict and providing an interaction term between ethnicity and the lagged percentage change in small arms would help identify if the increase in fighting is an escalatory reaction, or simply the enabling of further violence.

Interestingly, if we assumed that the effect being observed was the fueling effect, and that it operated in Mao's distinct four phases, we would expect to see percentage increases in small arms prices over the course of the conflict. In Mao's theory insurgents are more concerned with arms acquisition once they have obtained enough recruits (Zedong 1989). Yet, percentage shifts in small arms prices are largely negative and no positive shift in the price of small arms is greater than 5%. This implies that demand for weapons at least remains constant over the course of a conflict. However one reason we might not be observing an increase in arms prices is that the demand for small arms from the civilian market keeps prices high over the course of the conflict.

While the estimation results pertaining to civil conflict onset are compromised by the existence of multiple civil conflict "neighborhood" effects, they do imply that small arms availability might be one of those effects. Disregarding second stage results, small arms prices are noticeably lower as distance from disrupted stockpiles increases and the relationship is robust across regions (See Appendix E). If small arms abundance does increase the risk of civil conflict this would help explain the "clustering" of civil wars in specific regions. These findings are affirmation that conflict is more likely in areas where small arms are more abundant. However, they do not provide evidence of causality between small arms and conflict onset.

11.2 Policy

There are three findings of this paper relevant for policy-makers: decreases in small arms prices likely intensify conflict, proximity to disrupted stockpiles explains the price of small arms, and that there is much less certainty as to whether small arms abundance initiates civil conflict. One potential policy recommendation suggested by these findings is the use of a buy-back program during a conflict in order to keep small arms prices high. However, these findings also suggest that the effect of buy-back programs post-conflict, may not be as effective.

A cursory contemplation of a buy-back program used during the course of conflict suggests that it is fraught with ethical and empirical concerns. Such a program would make it more difficult for civilians to obtain weapons for self-defense. Additionally, an increase in small arms prices may not actually decrease the intensity of conflict. We do not know if the negative linear relationship between lagged percentage changes in small arms prices and the intensity of civil conflict continues on the right side of the y-axis because there are so few instances of an increase in assault rifle prices. These results do not imply that if small arms prices go up their will be less conflict, only that if they go down their will be more. A buy back programs during conflict is therefore only worth considering as a way dampen the effects of large negative shifts in the prices of assault rifles.

My research also provides further evidence for the blowback effects of stockpiling weapons in unstable countries. An empirical finding of this paper is that the cold war era weapons exporting policies used by the Soviet Union, the United States, and China increased the abundance of weapons in countries other than the countries they exported to. Therefore, policies that intend to use weapons shipments to bolster unstable regimes, or unsure rebellions, should account for the likely implications the policies may have on the political stability of neighboring countries.

While I do provide additional evidence that small arms abundance raises the risk of civil conflict onset, the net effect of this paper should be to introduce more uncertainty to the claim that small arms abundance makes conflict more likely. The last piece of empirical evidence for a causal relationship, Killicoat (2006), suffers from the same methodological errors that I do. One of the components of the instrumental variable used in Killicoat (2006) was a dummy variable that indicated if a neighboring country experienced a civil war. This variable also had the highest F-stat and was the only piece of the instrumental variable that was significant above the 5% level (Killicoat 2006, 53). On balance, this paper suggests that there is no empirical evidence that can assuredly conclude that a causal relationship exists.

Consequently, there should be a little more skepticism of post-conflict disarmament programs. Practitioners have for years been moving away from the understanding that disarmament is a policy that definitively reduces the risk of conflict (Muggah 2005). Given the methodological difficulties present in assessing the relationship between weapons and conflict, I would argue that the change in thinking is prudent.

11.3 Implications for Research

The first methodological contribution of this paper is the use of lagged percentage changes in small arms prices to proxy small arms abundance. There are radically different estimates of the effect of weapons abundance on the intensity of conflict when using lagged percentage changes in small arms prices as opposed to the prices themselves. I hypothesize that this is because the estimated prices does not take account of cost of living differences. A broader explanation is that the prices only have meaning in their specific context (in this case the country) and are not comparable outside of that context. Using the percentage change in small arms prices, researchers are able to see how prices vary within their specific country and see how that effects the dependent variable of interest.

The other methodological contribution of my paper was the construction of a list of disrupted stockpiles that can be used to instrument the price of small arms, so long as the dependent variable of interest is uncorrelated with civil war. Future research could refine this list of stockpiles and use conflict data that goes back to 1960 so that they can comfortably drop all countries that are adjacent to the stockpiling countries from the regression analysis.

There are several changes that could be made to this list which would improve its use for further research. A number of stockpiles on the list are puzzling. For instance, nowhere in the literature is Chad discussed as source of black market small arms. These aberrations are due to the procedures that I took to make sure my selection of stockpiles was disconnected from my prior biases about where small arms tend to be clustered geographically. A rigorous exploration of the small arms literature could produce a more fine-grained coding procedure about what characteristics of a country make it more inclined to have a large small arms market. A more parsimonious list would then allow researchers to exclude adjacent countries without substantially reducing the observed number of civil conflict onsets.

Another potential improvement is to take into account the effect of time. When identifying the country-years where distance from disrupted stockpiles did not adequately explain the average assault rifle price, the largest residuals were generally in years after 2003. That is because I did not allow for the effect of distance to diminish over time. In theory, the distance from a disrupted stockpile should matter less and less as the years go by. Simply put, the weapons have more time to move from point A to point B and it is less likely that weapons abundance will remain clustered in the same location. Investigating the appropriate lag would help improve the ability of the instrument to capture price variation.

Finally, researchers could depart from the country-year analysis and use the dataset of geocoded small arms transactions with the PRIO GRID, and geocoded battle events data provided by (Sundberg and Melander 2013). This approach would reduce the unit of analysis down to a geographic cell, provide a greater level of variation to explore with pricing data, and it would decrease the use of interpolated data.

11.4 Concluding Notes

The relationship between small arms abundance and civil conflict onset appears much more ambiguous than at the start of this paper. Weapons prices, lagged weapons prices, and instrumented weapons prices, are endogenous in every approach I have taken to ascertain their relationship with civil conflict onset. The biggest contributions that this paper makes are to offer a plausible methodological approach that future researchers could use to ascertain the strength and direction of the relationship, and support for a negative and causal relationship between changes in small arms prices and the intensity of conflict. In conclusion, I do not know if weapons make warfare, but I am fairly certain that they make warfare worse.

A The Vector of Controls

My vector of controls encompasses the theorized and tested variables that correlate with the risk of civl war onset and the conflict's intensity. I included different controls in different models. The full vector of controls for onset and intensity includes, the resource rents per capita of oil, the resource rents per capita of minerals, GDP per capita, the GINI coefficient, a dummy variable for regime type, the natural log of population, a dummy variable for whether or not an active conflict is present, and road density. I chose not to control for ethnic diversity or ethnic grievance.

A.1 Resource Rents

I use data from the World Bank to measure two types of resource rents: mineral rents and oil rents. According to Ross (2004) oil, minerals, and drugs are the natural resources that are tied to the onset of civil conflict (Ross 2004, 37-38). A resource rent is defined as the difference between the resources value given at current international prices minus the cost of extraction multiplied by the estimated quantity of natural resources in the country. I use rents per capita in line with the methodology of Ross (2006); Humphreys (2005) and then apply logs to normalize the distribution and correct for skewness. I predict a positive sign on the coefficient for resource rents for the risk of conflict onset and for conflict intensity.

A.2 GDP Per Capita

I take the natural log of GDP per capita as provided by the World Bank. The natural log is used to normalize the distribution and prevent higher magnitude per capita GDPs from the developing world from biasing the results. Logged GDP per capita should have a negative effect on the risk of conflict onset.

A.3 Territorial Accessibility

I measure territorial accessibility using data on road density per square mile that was provided by the international federation of road statistics (IRF 2015). I prefer to use road density as opposed to mountainous territory because the absence of roads correlates with other geographical causes of inaccessibility like dense jungles, swamps, or regions that are not well culturally connected to the rest of the country. Roads are a particularly apt measure of accessibility because they correlate with higher levels of inter-country trade and a faster response time of government troops to potential rebellions. Road density is a measure of geographic accessibility, so I expect a negative relationship with civil conflict onset and positive relationship with civil conflict intensity. I only have 17 years of data on road density and 348 values, so the model will be run with it and without it so that relative effects can be observed (IRF 2015).

A.4 Population

Fearon and Latin (2003) and Collier and Hoeffler (2009) both find that population is correlated with an increased risk of civil conflict onset. Gleditsch et al (2002) similarly find that more populated countries experience more severe civil conflicts. Theoretically, the more people there are, the easier it is to recruit fighters and the larger the armed groups will be. I control for population by including in my models the natural log of World Bank country population estimates. The predicted sign on this control is positive.

A.5 State Power

Fearon and Latin (2003) asserted that GDP per capita is significant as a measure of state power, however I believe a better approach would be to control for the amount of resources allocated to the military as done in Lacina (2006). I use two measures of state power, first I control for how much the military is crowding the labor market using the world bank's measure of army personnel as a % of the labor force. Next, I control for the resources allocated to the military by including a measure of military expenditure as a % of GDP. I take the natural log of both measures to correct for skewness in their distribution. Both measures are expected to have a negative relationship with conflict onset and a positive relationship with conflict intensity.

A.6 Regime Type

One of the significant findings of Lacina (2006) is that Democracies have civil wars that are less severe than autocracies. However, Lacina (2006) measured severity by the total battle-related deaths over the course of the entire conflict, as opposed to the annual fatality rate. Consequently, the theory presented in Lacina (2006) may not completely translate to estimating my measure of intensity. There is reason to suspect that first mechanism proposed by Gleditsch et al. (2002) may still apply: democracies might be gentler to their rebelling civilians than autocracies. Following the methodology of Lacina (2006), I coded a democracy as a 1 if the country has a polity score above 6 and a 0 for below. This variable is predicted to have a negative relationship with the intensity of civil conflict.

A.7 Inequality

I attempted to control for both vertical and horizontal inequality. To account for horizontal inequality I tried to use a variant of negative horizontal inequality (NHI) calculated by Buhaug et al (2014) with the Ethnic Power Relations Dataset (Wimmer et al. 2009). However, the NHI could only be calculated for 88 countries and the number shrank even further when controlling for other variables.

The only measure of inequality I include is vertical income inequality as measured by the GINI coefficient. A substantial problem with the GINI coefficient is the lack of data available for various country-years. I attempted to address the reporting bias by adding imputed measures of the GINI coefficient. My imputation strategy was to assume that the coefficient was the same as the last time the coefficient was reported. This strategy is less than ideal. While the GINI is largely time invariant, as a researcher it is suboptimal to assume no variation. However, given the other large number of controls, the only alternative option is to drop all measures of vertical inequality.

A.8 Ongoing Conflict

It is not uncommon for more than one civil conflict to occur at a time in a given country. Naturally each conflict competes for some share of conflict resources such as weapons and fighters. A common reason why a conflict does not start in a given country year, is because there is already one ongoing. To address this phenomena, I followed the methodology of Fearon and Latin (2003) and added a dummy variable which indicates whether a country is experiencing an active civil conflict.

A.9 Ethnicity

I chose not to control for any measure of ethnic diversity, or the existence of ethnic grievances. Measures of ethnic diversity have long been used in models of civil conflict onset and civil conflict intensity. However, in most models ethnic diversity, particularly the ethnic fractionalization index, has no relationship with either the risk of civil conflict onset or civil conflict intensity (Collier and Hoeffler 2001) (Fearon and Latin 2003). Buhaug et al. (2014) found that by combining ethnicity with inequality created a strong predictor of conflict. Unfortunately due to the small number of countries covered by the Ethnic Power Relations dataset their measure was not available for enough countries in my dataset. Esteban et al. (2012) found that ethnic fractionalization has a positive effect on intensity in conflicts over private goods, whereas ethnic polarization has a positive effect on intensity in conflicts over public goods (Esteban et al. 2012, 1319). This result was only obtained after controlling for conflict type.

I chose not to control for any measure of ethnic diversity or grievance because so called "off the shelf" measures often cannot predict conflict without adding additional controls and further reducing the degrees of freedom. I admit that excluding ethnicity introduces the possibility of omitted variable bias; however, based on past work I believe that it will be miniscule.

B Fixed Effects for Initial Onset Models

Table 22: Logit Model: 1st Death Onset vs. Lagged Small Arms Prices with Country Fixed Effects

	(1) Onset as the 1st Death	(2) Onset as the 1st Death	(3) Onset as the 1st Death	(4) Onset as the 1st Death	(5) Onset as the 1st Death	(6) Onset as the 1st Death	(7) Onset as the 1st Death
Lagged Percentage Price Change	0.0463	0.00499	0.0284	0.0637	-0.00511	-0.337	0.0383
	(0.124)	(0.101)	(0.105)	(0.207)	(0.118)	(0.372)	(0.162)
Natural Log of Population	0.0722	0.308	1.478	-4.772^{+}	1.108	-4.025	-15.17^{***}
	(1.170)	(2.080)	(1.771)	(3.198)	(2.763)	(2.829)	(5.682)
Natural Log of GDP per Capita	-0.686^{**}	-1.413^{**}	-0.927^{+}	-0.985^{+}	-0.952^{+}	-0.201	0.0553
	(0.337)	(0.571)	(0.574)	(0.626)	(0.586)	(0.654)	(1.344)
Natural Log of Oil Rents per Capita		0.500					-0.613
		(0.361)					(0.873)
Natural Log Mineral Rents per Capita			0.103				-0.0957
			(0.143)				(0.332)
Natural Log of Military Expenditure per Capita				-2.386^{*}			-2.703^{+}
				(1.321)			(1.861)
Natural Log of Army Personel (% of the Labor Force)				-3.299^{***}			-3.469^{*}
				(1.235)			(2.089)
Road Density (% per square mile)					0.247		
					(2.302)		
GINI						-0.210^{*}	
						(0.117)	
Imputed Road Density							7.455^{***}
							(2.884)
Imputed GINI							0.0633
							(0.164)
Observations	907	487	451	232	189	76	182
Pseudo R^2	0.086	0.105	0.063	0.235	0.159	0.129	0.303

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 23: Logit Model: 1st Death Onset vs. Lagged Small Arms Prices With Fixed Effects

	(1) Onset as the 1st Death	(2) Onset as the 1st Death	(3) Onset as the 1st Death	(4) Onset as the 1st Death	(5) Onset as the 1st Death	(6) Onset as the 1st Death	(7) Onset as the 1st Death
Lagged Price of Small Arms	-0.0000696	0.0000416	0.0000982	-0.0000341	0.000108	-0.00128^{+}	-0.000172
	(0.000323)	(0.000414)	(0.000353)	(0.000562)	(0.000495)	(0.000809)	(0.000656)
Natural Log of Population	-0.561	-0.0505	1.081	-4.919^{+}	1.293	-6.271^{*}	-15.56^{***}
	(1.087)	(1.956)	(1.610)	(3.200)	(2.729)	(3.239)	(5.775)
Natural Log of GDP per Capita	-0.846^{**}	-1.556^{***}	-1.109^{**}	-0.977^{+}	-1.005^{*}	0.00154	0.234
	(0.336)	(0.561)	(0.534)	(0.648)	(0.599)	(0.667)	(1.443)
Natural Log of Oil Rents per Capita		0.589^{+}					-0.667
		(0.362)					(0.891)
Natural Log Mineral Rents per Capita			0.148				-0.110
			(0.139)				(0.332)
Natural Log of Military Expenditure per Capita				-2.441^{*}			-2.666^{+}
				(1.323)			(1.841)
Natural Log of Army Personel (% of the Labor Force)				-3.354^{***}			-3.604^{*}
				(1.242)			(2.153)
Road Density (% per square mile)					0.224		
					(2.305)		
GINI						-0.253^{**}	
						(0.118)	
Imputed Road Density							7.527^{***}
							(2.915)
Imputed GINI							0.0530
							(0.166)
Observations	1122	551	541	233	190	115	182
Pseudo R^2	0.096	0.108	0.077	0.235	0.159	0.191	0.303

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C Robust Regression for Initial Intensity Models

Table 24: Robust Regression: Battle Related Deaths vs. Lagged Percentage Small Arms Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths
Lagged Percentage Price Change	-0.0872**	-0.0842**	-0.0718*	-0.0889**	-0.0775*	-0.143*	-0.421**	-0.516**
	(0.0440)	(0.0406)	(0.0414)	(0.0446)	(0.0440)	(0.0845)	(0.199)	(0.224)
Natural Log of GDP per Capita	-0.353***	-0.352***	-0.261***	-0.450***	-0.544***	-0.653***	-0.484**	-0.713***
	(0.0563)	(0.0520)	(0.0561)	(0.0813)	(0.0970)	(0.119)	(0.197)	(0.235)
Natural Log of Population	0.234***	0.289***	0.371***	0.414***	0.417***	0.326***	0.638***	0.553*
	(0.0564)	(0.0523)	(0.0549)	(0.0759)	(0.0834)	(0.103)	(0.160)	(0.296)
Number of Supporting Countries		0.680***	0.725***	0.914***	0.759***	0.616***	0.763***	0.996***
		(0.0619)	(0.0637)	(0.0806)	(0.0918)	(0.0998)	(0.125)	(0.210)
Democracy			-0.674***	-0.183	-0.0543	0.368	0.0745	0.430
			(0.168)	(0.239)	(0.257)	(0.273)	(0.378)	(0.416)
Natural Log of Oil Rents per Capita				0.174***	0.100**	0.0987*	0.0557	0.192*
				(0.0361)	(0.0449)	(0.0529)	(0.0717)	(0.102)
Natural Log Mineral Rents per Capita					0.136***	0.206***	0.168***	0.352***
					(0.0360)	(0.0385)	(0.0541)	(0.0762)
Natural Log of Army Personel (% of the Labor Force)						0.362**	0.505*	0.422
						(0.174)	(0.267)	(0.366)
Natural Log of Military Expenditure per Capita						0.963***	1.129***	1.017*
						(0.290)	(0.366)	(0.571)
Road Density (% per square mile)							-0.282	
							(0.542)	
GINI								-0.0669**
								(0.0326)
Observations	812	812	778	513	408	285	159	100
R^2	0.064	0.188	0.202	0.275	0.288	0.399	0.529	0.494

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 25: Robust Regression: Battle-Related Deaths vs. Lagged Small Arms Prices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths	Natural Log of Battle-Deaths
Lagged Price of Small Arms	-0.0000444	-0.000146	-0.0000857	-0.0000785	-0.0000488	0.0000877	0.0000262	0.000268
	(0.000113)	(0.000104)	(0.000104)	(0.000117)	(0.000118)	(0.000126)	(0.000170)	(0.000236)
Natural Log of GDP per Capita	-0.356***	-0.332***	-0.256***	-0.456***	-0.559***	-0.678***	-0.511**	-0.787***
	(0.0596)	(0.0547)	(0.0585)	(0.0837)	(0.0985)	(0.122)	(0.204)	(0.244)
Natural Log of Population	0.234***	0.281***	0.357***	0.389***	0.395***	0.360***	0.606***	0.708**
	(0.0555)	(0.0511)	(0.0539)	(0.0757)	(0.0831)	(0.106)	(0.163)	(0.297)
Number of Supporting Countries		0.696***	0.732***	0.901***	0.745***	0.593***	0.745***	1.044***
		(0.0602)	(0.0617)	(0.0800)	(0.0908)	(0.101)	(0.131)	(0.214)
Democracy			-0.633***	-0.137	-0.00761	0.340	-0.0318	0.206
			(0.165)	(0.235)	(0.250)	(0.276)	(0.386)	(0.415)
Natural Log of Oil Rents per Capita				0.179***	0.108**	0.0929*	0.0315	0.166+
				(0.0354)	(0.0439)	(0.0534)	(0.0731)	(0.104)
Natural Log Mineral Rents per Capita					0.141***	0.209***	0.167***	0.366***
					(0.0357)	(0.0389)	(0.0563)	(0.0778)
Natural Log of Army Personel (% of the Labor Force)						0.375**	0.438+	0.278
						(0.176)	(0.282)	(0.369)
Natural Log of Military Expenditure per Capita						0.943***	1.097***	0.996*
						(0.293)	(0.375)	(0.579)
Road Density (% per square mile)							-0.314	
							(0.556)	
GINI								-0.0600*
								(0.0333)
Observations	845	845	811	530	419	285	159	100
R^2	0.061	0.193	0.205	0.265	0.282	0.391	0.508	0.478

Standard errors in parentheses
$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D Variance Inflation Factor

Table 26: Variance Inflation Factor for Model 2 in Table 12

Variable	**VIF**	**1/VIF**
Imputed Road Density	2.44	0.410503
Natural Log of GDP per Capita	1.98	0.504482
Natural Log of Military Expenditures	1.88	0.532358
Natural Log of Population	1.57	0.635904
Active Conflict	1.57	0.635904
Natural Log of Army Personel	1.54	0.649831
Natural Log of Mineral Rents	1.53	0.651847
Distance to Disrupted Stockpiles	1.44	0.694056
Imputed GINI	1.29	0.776288
Natural Log of Oil Rents	1.28	0.782796

E Effect of Distance from Stockpiles by UN Region

I checked to ensure that distance from disrupted stockpiles explained variation in assault rifle prices within four out of the five UN subregions[22] I also re-calculated the sum of logged distances in each sub-region using only the stockpiles in the region. Europe was the one exception. Since Europe has no armed stockpiles I used the closest stockpiles in Libya and the Soviet Union. I then re-estimated the coefficients using generalized least squares model that incorporated random effects. The regressions were done using both the full dataset of price estimates and the smaller subset of prices that were actually observed.

[22] Oceania was excluded from this table. As discussed earlier, there are no stockpiles in Oceania or remotely close to Oceania. Consequentially, there is no way to assess the in-region effect of being farther away from a disrupted stockpile

Table 27: Total Logged Distance from Stockpiles Globally and by Region

Price Variable (Y)	**Distance Variable (X)**	**Coefficient**	**P-Value**	**R squared**	**N**
Reported Prices	All Stockpiles	23.81307	0.080	0.0212	261
Price Estimates	All Stockpiles	10.65753	0.000	0.0359	4728
Reported Prices Europe	Europe Stockpiles	160.6106	0.047	0.0211	20
Price Estimates Europe	Europe Stockpiles	144.6069	0.000	0.0904	900
Reported Prices Asia	Asian Stockpiles	54.17265	0.266	0.0143	108
Price Estimates Asia	Asian Stockpiles	30.26023	0.000	0.0210	1146
Reported Prices Americas	American Stockpiles	8.038809	94	0.0199	50
Prices Estimates Americas	American Stockpiles	1.200587	0.705	0.0017	725
Reported Prices Africa	African Stockpiles	9.332561	0.792	0.0025	66
Price Estimates Africa	African Stockpiles	26.53346	0.000	0.0410	1400

In every model the distance from the disrupted arms stockpiles had a negative relationship with the average assault rifle price. The significance of the relationship varies based on the chosen of a price variable. The price estimates have 100 times more cases than the observed prices, so naturally they obtain significance with greater ease. Distance from disrupted stockpiles is a significant determinant of the estimated assault rifle prices in three of the sub-regions but is an insignificant predictor of assault rifle prices on the American continents. The relationship is likely insignificant when looking at the assault rifle prices in the Americas because only one stockpile, El Salvador, was identified on the two continents. In general, the fact that being farther away from a stockpile increases the average assault rifle price, regardless of region, confirms the theory that went into constructing the instrument.

F Predicted Conflict Onsets and Actual Conflict Onsets

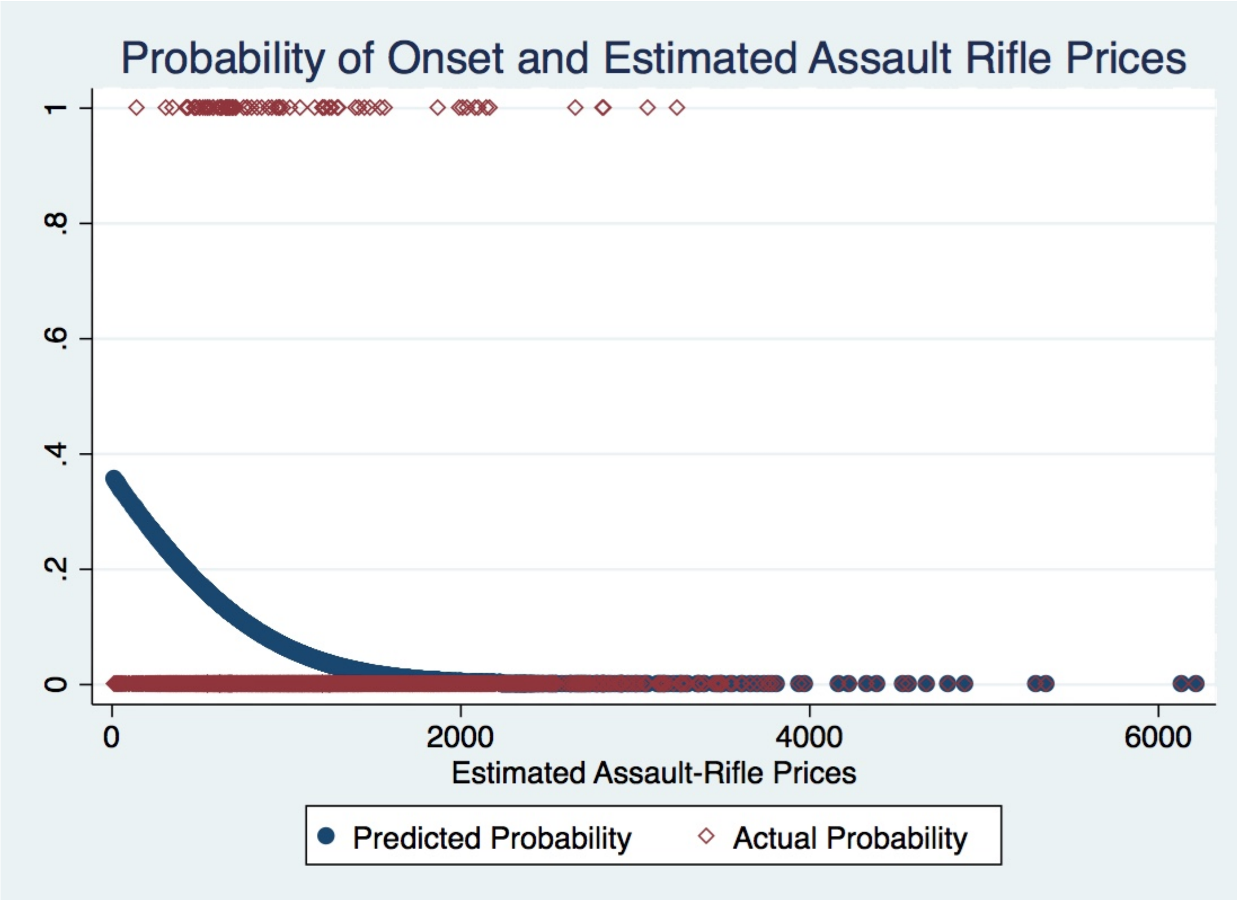

Figure 11: Predictions were made using Model 3 in Table 15

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