Is Happiness Really a Warm Gun?

The Consequences of U.S. Weapons Sales

for Political Violence[*]

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Abstract

We exploit exogenous shifts in the cost of purchasing commercial weapons from the U.S. to uncover the causal effect of U.S. weapons purchases on political violence. We find that weapons purchases reduce the likelihood of political repression but increase the likelihood of onset of civil war in purchasing countries. The results suggest that state investment in military capability incites civil war in countries where state repression of an aggrieved opposition would have otherwise prevailed.

1. Introduction

The United States is the largest international arms supplier (SIPRI, 2011). Over the last 40 years, an average of 130 countries – representing 68 percent of the world's sovereign states – purchased weapons from the U.S. each year. In 2005 alone, 40 billion dollars worth of U.S. weaponry and defense services were delivered to 172 countries, and these do not even include excess defense articles that were transferred in the form of military aid.[1] Many buyers of U.S. weapons, over the same time horizon, experienced episodes of political violence with sometimes devastating consequences.

In this paper we study the causal effect of U.S. commercial weapons on political violence in purchasing countries. Following Besley and Persson (2009) and Besley and Persson (2011), we distinguish between political repression (one-sided violence) and civil war (two-sided violence), and study the effects of weapons on each form of political violence separately. U.S. weapons purchases represent an improvement in state military capability, which could in principle increase or decrease the propensity for political violence of either type. Weapons change the incentives of both the state and its opponents. For example increased military capability could *deter* opponents of the state from mobilizing, if the perceived chances of defeating an armed state are too small. In this case weapons may reduce the incidence of political violence. But on the other hand it may *incite* opponents of the state to mobilize, if the future prospects of a toothless opposition in a heavily militarized state are grim enough. From the perspective of the state, enhanced military capability could *facilitate* the elimination of political opposition, increasing the incidence of political violence. In general, the interaction of incentives of the state and its opposition creates an ambiguous equilibrium outcome concerning violence, as weapons could affect the propensity for violence as well as the type of violence that occurs.

History is rife with examples of political uprisings and state responses to them, but perhaps most interesting is the variety of forms the uprisings and responses can take, in particular with

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[1]Figures based on archival data on U.S. weapons transfers from the Defense Security Cooperation Agency, Department of Defense.

respect to the use of violence. Popular uprisings vary greatly in the degree of violence employed, not to mention their motivation. While peaceful protest is the chosen method of airing a grievance in some cases, in others it is guerilla warfare. State responses are just as varied. In some cases, the state responds with repressive tactics and the situation does not escalate into war, while of course in many other cases the situation does culminate in civil war. Examples include leftist uprisings in Latin America, Islamist uprisings in the Middle East and ethnic grievances in Africa, Chechnya, Sri Lanka and Turkey to name only a few. In many cases, the government responded to political grievances using repressive tactics such as torture, enforced disappearance and extra judicial killings. Several of these countries have been accused by non-governmental organizations of committing these acts in response to political grievance or insurgent threat. Often times, when the state responds to insurgency more violently, the situation escalates into civil war. Prominent examples include the civil war in Syria in the late 1970s between Islamist insurgents and the Ba'ath regime, the post-1983 ethnic conflict in Sri Lanka, and the civil conflict between the state and leftist insurgents (Shining Path) in Peru. The role of military capability in how these conflict episodes played out is particularly interesting, but is rarely addressed in the existing empirical literature.

The main difficulty in identifying the causal effect of weapons sales on political violence is that buyers perhaps anticipate the onset of violence and procure weapons for an episode of violence that would have occurred in any case. Similarly, countries engaged in violence may anticipate the onset of peace and stop purchasing weapons in advance. Either of these cases will result in a positive anticipation bias. To identify the causal effect of weapons sales on conflict, we exploit exogenous variation in the cost of purchasing U.S. weapons. It is known in the U.S. defence industry that in periods characterized by higher-than-usual domestic inflation, the foreign demand for U.S. weapons increases as the relative price for foreign buyers is lower (DISAM, 2012). We then use lagged U.S. domestic inflation in combination with cross-sectional variation in a country's historical frequency of purchases as an instrument for contemporaneous weapons purchases.

Our identification strategy relies on the assumption that the interaction of a country's historical frequency of purchases and lagged U.S. domestic inflation only affects the likelihood of political

violence through procured weapons. Concerns about the exclusion restriction may arise if U.S. domestic inflation is correlated with U.S. political cycles that may also affect political violence differently across frequent and infrequent buyers of U.S. weapons. In addition, frequent buyers of U.S. weapons may also be more likely to receive U.S. economic or military aid, both of which are known to affect conflict (Dube and Naidu, 2014; Crost, Felter, and Johnston, 2014; Nunn and Qian, 2014). To address these concerns, we allow for purchasing country specific and region-year specific unobserved heterogeneity, as well as a comprehensive set of controls in order to capture the nature of the relationship between the U.S. and the purchasing country.

We find that the purchase of U.S. weapons reduces political repression (one-sided violence), but increases the likelihood of civil war (two-sided violence) in purchasing countries. Viewing repression and civil war as ordered states of political violence, our results imply that increased state military capability does not uniformly increase or decrease political violence, but rather that it changes the type of violence that states use to respond to domestic political threats. This suggests that U.S. weapons purchases change the way grievances between a state and its opponents are resolved. Whereas in the absence of arms there may have been a peaceful resolution or repression from the government, arms make civil war more likely.

While the link between access to cheaper weaponry and civil war is relatively straightforward, it is not immediately obvious why weaponry, much of which is not useful as an input into repression, causes a decrease in repression. To this end, we rationalize these results by using a simple model in which weapons purchases, which need not be useful inputs for repression, serve instead as a signal of the state's willingness to employ violence against the opposition, and hence shifts the equilibrium from repression to civil war.

To further understand the mechanisms through which weapons increase the likelihood of civil war, we also separately examine the *onset* and the *duration* of civil war. That is, we study whether U.S. weapons increases the likelihood of initiating civil wars (for countries not in a state of war) and whether U.S. weapons prolong existing civil wars (for countries already at war). If we find that weapons increase the *onset* of civil war, we can infer that states that would have dealt with

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domestic political threat through less violent means are now more likely to go into civil war with political opponents once they buy weapons. Alternatively, if weapons cause an increase in the *duration* of civil war, we can infer that countries that would have transited to a less violent state, by either choosing repression or reaching a peace agreement, instead continue in a state of civil war.

We find that weapons increase the *onset* of war but have no effect on its duration. That is, weapons make countries that are not currently in civil conflict more likely to be so in the following year, but does not change the likelihood that a country currently in civil conflict exits in the following year. This is further evidence that military capability affects the *type* of violence that ensues in well armed countries. Countries that may have avoided civil war through either peaceful or repressive means are more likely to fall into civil conflict, but countries already in civil conflict are unaffected.

It is very likely that the causal effect of weapons on political violence is heterogenous across countries. In this case, we identify a local average treatment effect (LATE) of weapons on political violence, that is, we identify the effect of weapons on political violence for the set of countries whose demand for weapons increases when the price of weapons decreases – the "compliers", in the language of Imbens and Angrist (1994).[2] While we expect that many countries are price responsive and that therefore we are identifying an average effect for a meaningful subset of the population, our estimates cannot be attributed to countries who buy weapons regardless of the price ("always takers") and countries who do not buy weapons regardless of the price ("never takers"). As such, it is possible that we are identifying a particularly high LATE if, for example, political violence in the countries who buy more weapons in response to a price shock is more dependant on access to weaponry than in countries whose weapons purchases are not dependant on price. While this may be viewed as a limitation of our study, the fact that changes in the instrument represent changes in price make our results policy relevant, as our findings suggest

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[2]Importantly, to be identifying a LATE requires the absence of "defiers" – countries who buy less weapons when prices go down. It is hard to imagine there are such cases, so the monotonicity condition is likely satisfied.

that those countries whose demand for weapons is elastic are precisely the ones who should have their weapons purchases restricted.

Given our focus on weapons purchases from the United States, it is fair to ask whether our findings apply to purchases from other countries. That is, are we identifying the effect of weapons purchases in general or the effect of weapons purchases from the U.S. in particular. For example, one reason the effects may differ is that arms sold by the U.S. are different from arms sold by other countries. We note that our study is likely not possible in the context of arms sales from countries other than the U.S. In addition to the fact that U.S. weapons sales data are the only reliable weapons sales data we know of, it is the features of the weapons sales process in the U.S. itself that gives us the exogenous variation necessary to identify the causal effect of U.S. weapons sales on political outcomes. The fact that U.S. weapons sales covaries with U.S. inflation, plausibly exogenous to recipient country outcomes, gives our results internal validity, and in this sense our study is U.S. specific by design.

This paper contributes to several literatures. Chassang and Padró i Miquel (2010) theoretically study the effect of changes in weapons stock on conflict in a setting where players have strategic uncertainty - uncertainty not only about the state of the world, but also about each other's beliefs about the state of the world. They show that in such a setting, though greater asymmetry in military capability increases the incentive for *predatory* behavior on the part of the stronger party, it can decrease the incentive for *preemptive* behavior on the part of either party. Taken in the light of Chassang and Padró i Miquel (2010), our key result, that increased asymmetry in military capability increases the likelihood of civil war, suggests that on average U.S. weapons sales do not reduce the incentive for preemptive behavior enough to offset the increased incentive for predatory behavior.

We also contribute to the empirical literature on the causes and consequences of armed conflict such as Miguel, Satyanath, and Sergenti (2004), Dube and Naidu (2014), Brückner and Ciccone (2011), Dube and Vargas (2013), and Nunn and Qian (2014) [see Blattman and Miguel (2010) for a review]. In particular, the availability of weapons is rarely discussed as a plausible determinant

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of war and our paper represents one of the first empirical tests for the role of conflict technology. Relatedly, our paper also speaks to the political science literature investigating the relationship between arms races and war [see, for example, Sample (1997) and Diehl and Crescenzi (1998)]. Our findings suggest that unilateral arms proliferation can by itself affect the likelihood of war.

Our paper also speaks to a growing literature examining the political impact of superpower interventions. Easterly, Satyanath, and Berger (2008) and Berger, Easterly, Nunn, and Satyanath (2013), for example, find that covert CIA operations decrease democracy significantly, but increase U.S. exports in selected sectors. Dube and Naidu (2014) also find that U.S. military aid to Columbia leads to increases in attacks by paramilitaries who collude with the military. Nunn and Qian (2014), who use a similar identification strategy to ours, find that U.S. food aid increases the duration of civil conflict in recipient countries. Albornoz and Hauk (2014) proposed that civil wars around the world are more likely to happen under Republican governments and when the U.S. presidential approval ratings are low. Our finding that U.S. weapons sales are associated with political violence therefore adds another dimension to this literature.

Finally, and perhaps most importantly, our paper joins a recent literature looking at the determinants of political violence more broadly, in particular, Besley and Persson (2009) and Besley and Persson (2011). In the Besley-Persson framework, an incumbent government and opposition make investments in violence in pursuit of political power and as the authors illustrate, under some conditions equilibrium political violence is ordered in the spoils of war. When the spoils are small, peace is most likely, but as the spoils increase the equilibrium outcome is repressive violence or in the worst case civil war. A key component of their framework is a conflict technology which translates incumbent and opposition investments in violence into whether the incumbent retains political power. As we discuss in more detail below, our study complements this strand of literature by shedding light on this process and the role of weapons in determining the equilibrium outcome of violence.

The rest of this paper is structured as follows. Section 2 provides a description of U.S. weapons sales and the data that we use. Our empirical strategy and results are presented in Sections 3 and

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4 respectively. We conclude in Section 5.

2. Background and Data

2.1 U.S. Weapons Sales

The global arms trade is a massive business. In 2005, the top 100 arms manufacturers in the world sold a staggering 290 billion dollars worth of weapons and defense services (SIPRI, 2011). In this business, the U.S. is undoubtedly a major global player. In fact, 40 of the top 100 arms manufacturers are based in the U.S., of which 7 are in the top 10 (SIPRI, 2011).[3] Typically, U.S. arms manufacturers sell major weaponry to foreign countries through one of two non-aid channels: Direct Commercial Sales (DCS) and Foreign Military Sales (FMS). DCS are negotiated directly between the buyer and the weapons manufacturer, with approval (i.e. an export license) from the Department of State, while FMS are government-to-government sales with the Pentagon acting as the purchasing agent for the buyer (see Appendix A1 for more details). U.S. weapons may also be transferred to other countries via military aid.[4]

In what follows, we focus exclusively on DCS which we refer to as U.S. weapons sales. There are three main reasons for this. First, DCS are more typical of a market transaction than FMS. In DCS, potential buyers approach weapons manufacturers directly, and contracts on price and quantity are drawn up based on private negotiations. This suggests that the demand for commercial weapons will likely respond to prices, and it is this price responsiveness that we capture in our instrument involving U.S. domestic inflation: when inflation is high, the relatively-weak U.S. dollar makes prices more favorable to buyers, so demand for commercial weapons increase, other things being equal. Conversely, the U.S. government's heavy involvement in FMS implies that weapons sales through FMS have less resemblance to market transactions.[5] Second, the time taken between order and delivery (or lead time) is shorter and much more certain in DCS. This is

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[3]The top 10 largest arms manufacturers in 2005 are: Boeing, Lockheed Martin, BAE Systems, Northrop Grumman, Raytheon, General Dynamics, EADS, L-3 Communications, Finmeccanica, and Thales.

[4]Specifically, the (now-defunct) Military Assistance Program (MAP) provides defense articles and services to recipients on a grant basis, while the Excess Defense Articles (EDA) program allows the U.S. government to transfer surplus military equipment to recipients for free or at greatly reduced prices. For practical purposes, we consider the sum of arms transfers though these two programs as military aid, which we use later on as a control variable.

[5]This is supported, for example, by the fact that we find lagged U.S. domestic inflation to be strongly correlated with DCS but not FMS.

important, because our instrument relies on lagged inflation, so we need to have a good idea about when the weapons are actually delivered to buyers i.e. when the "treatment" occurs. Third, the U.S. government frequently uses FMS as strategic maneuvers in foreign policy, and openly cites bilateral defense relations and interoperability with allies as objectives achieved through FMS (Cohen, 2000). Focusing solely on DCS, therefore, allows us to avoid the complications that strategic motives introduce to weapons sales, so that we can isolate the effect of putting weapons in the hands of the average purchasing country.

2.2 Weapons Sales Data

Our weapons sales data are obtained from the Defense Security Cooperation Agency of the U.S. Department of Defense, and contain the annual value of U.S. weaponry and defense services delivered to every country during the period 1970-2008. We extracted the value of weapons sales through DCS from this data set, and dropped international organizations, colonies, dependent territories, and non-sovereign entities that are typically excluded from cross-country analyses.[6] Importantly, although in principle any foreign "private buyer" is eligible to purchase weapons through the DCS channel, in practice we do not observe any non-government buyers in the data provided to us.[7] What remains for subsequent analyses is a sample of 7,416 observations representing 191 recipient countries as listed in Table 1.[8]

To get a snapshot of U.S. weapons transfers through DCS, we present some summary statistics in Table 2 (see the appendix for a detailed comparison between sales through DCS and weapons acquisitions through other channels). The annual value of weapons sales via DCS is approximately 29.57 million dollars (in thousands of constant U.S. dollars in 2005) on average. This makes up roughly 30 percent of all weapons transfers (the other major channels being FMS and military aid). Importantly, every country in the sample made at least one DCS purchase over the sample

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[6]Empirically, these excluded entities make little difference as weapons receipts are rare and typically low in value, with the exception of Hong Kong and French Guiana.

[7]The only exceptions are weapons purchases made by international organizations, but these are extremely rare in the data; in any case, we omit them from our analyses.

[8]Our sample represents an unbalanced panel as it includes several countries that only came into existence later in the period.

Table 1: Countries in the U.S. Weapons Sales Data

Country	Country	Country	Country
Afghanistan	Dominican Republic	Liberia	St Vincent & the Grenadines
Albania	East Timor	Libya	Samoa
Algeria	Ecuador	Liechtenstein	San Marino
Andorra	Egypt	Lithuania	Sao Tome & Principe
Angola	El Salvador	Luxembourg	Saudi Arabia
Antigua & Barbuda	Equatorial Guinea	Macedonia	Senegal
Argentina	Eritrea	Madagascar	Serbia
Armenia	Estonia	Malawi	Seychelles
Australia	Ethiopia	Malaysia	Sierra Leone
Austria	Fiji	Maldives	Singapore
Azerbaijan	Finland	Mali	Slovakia
Bahamas	France	Malta	Slovenia
Bahrain	Gabon	Marshall Islands	Solomon Islands
Bangladesh	Gambia	Mauritania	Somalia
Barbados	Georgia	Mauritius	South Africa
Belgium	Germany	Mexico	Spain
Belize	Ghana	Micronesia	Sri Lanka
Benin	Greece	Moldova	Sudan
Bhutan	Grenada	Monaco	Suriname
Bolivia	Guatemala	Mongolia	Swaziland
Bosnia & Herzegovina	Guinea	Morocco	Sweden
Botswana	Guinea-Bissau	Mozambique	Switzerland
Brazil	Guyana	Myanmar	Syria
Brunei	Haiti	Namibia	Taiwan
Bulgaria	Honduras	Nauru	Tajikistan
Burkina Faso	Hungary	Nepal	Tanzania
Burundi	Iceland	Netherlands	Thailand
Cambodia	India	New Zealand	Togo
Cameroon	Indonesia	Nicaragua	Tonga
Canada	Iran	Niger	Trinidad & Tobago
Cape Verde	Iraq	Nigeria	Tunisia
Central African Rep.	Ireland	Norway	Turkey
Chad	Israel	Oman	Turkmenistan
Chile	Italy	Pakistan	Tuvalu
China	Ivory Coast	Palau	United Arab Emirates
Colombia	Jamaica	Panama	Uganda
Comoros	Japan	Papua New Guinea	Ukraine
Rep. of Congo	Jordan	Paraguay	United Kingdom
Dem. Rep. of Congo	Kazakhstan	Peru	Uruguay
Costa Rica	Kenya	Philippines	Uzbekistan
Croatia	Kiribati	Poland	Vanuatu
Cuba	Korea Rep.	Portugal	Venezuela
Cyprus	Kuwait	Qatar	Vietnam
Czechoslovakia	Kyrgyzstan	Romania	Yemen
Czech Republic	Laos	Russia	Yugoslavia
Denmark	Latvia	Rwanda	Zambia
Djibouti	Lebanon	St. Kitts & Nevis	Zimbabwe
Dominica	Lesotho	St. Lucia	

Notes: We dropped 31 colonies, territories, and entities: Norfolk Island (Australia); Hong Kong and Macau (China); Faroe Islands, Greenland, and Svalbard & Jan Mayen (Denmark); French Guiana, French Indochina, French Polynesia, Guadeloupe, Martinique, New Caledonia, Réunion, and Saint Pierre & Miquelon (France); Aruba and Netherlands Antilles (Netherlands); Niue and Tokelau (New Zealand); Anguilla, Bermuda, British Indian Ocean Territory, British Virgin Islands, Canton & Enderbury Islands, Cayman Islands, Falkland Islands, Gibraltar, Montserrat, Pitcairn Islands, Saint Helena, and Turks & Caicos Islands (United Kingdom); Western Sahara.

period.[9]

There are several points worth nothing from Panel A. Most glaringly, high income and democratic countries, as well as allies and countries with strong civil freedom, purchase more weapons via DCS on average. This is unsurprising given that human rights practices are important considerations in U.S. weapons sales (Cingranelli and Pasquarello, 1985; Meernik, Krueger, and Poe, 1998), and that civil freedom tends to be correlated with income, democratic institutions, and U.S. alliance. That being said, the amount of weapons sold through DCS to OECD countries is markedly higher than average, which indicates that the demand side of the weapons market is also a significant factor in play; for example, in Panel B where we present the breakdown by top and bottom three buyers, we can see that the biggest spenders in this category are Japan, Germany, and United Kingdom – all of whom are notable allies with substantial demand for major weaponry. Finally, we note that there is a significant upward trend in the amount of U.S. weapons sold through DCS over time, with post-Cold War sales roughly 30 percent higher.

There are inherent sample selection issues that remain. The most obvious one is that countries that never purchased any weapons through DCS in this period, such as North Korea, are omitted from the data. Our empirical results thus cannot be externally valid for these countries. Even within the sample, countries may not always be eligible for weapons purchases. This could be due to the UN arms embargo – on countries such as Iran, Iraq, Libya, and Sudan – or sales restrictions imposed on countries sponsoring terrorist activity, such as Afghanistan and Cuba.[10] Omitting instances of non-eligibility will likely bias our estimates downwards, since these countries may be experiencing episodes of violence in those years where they receive no U.S. weapons. Similarly, unofficial "under-the-table" transactions that may occur during arms embargoes will probably also bias our estimates downwards.

2.3 Political Violence Data

[9]On the other hand, over the same time period, all but 23 countries made at least one FMS purchase, and slightly more than half the countries received zero military aid.

[10]We identify a set of country-year observations that are affected by (mandatory and non-mandatory) UN arms embargoes and other multilateral embargoes from the SIPRI Arms Embargoes Database.

Table 2: Summary Statistics – Average Annual DCS

Panel A:	DCS	Obs.
Full sample	29,572	7,416
	(208,038)	
High income	44,104	4,583
	(257,325)	
Democratic	59,943	2,894
	(318,305)	
Civil freedom	41,457	4,103
	(265,337)	
Ally	42,545	4,554
	(257,874)	
OECD	127,325	1,287
	(462,358)	
Post-Cold War	38,225	3,236
	(292,286)	

Panel B:	Country	DCS
Top three:	Japan	1,051,956
	Germany	514,606
	United Kingdom	443,932
Bottom three:	Tuvalu	0.409
	Cuba	0.218
	Equatorial Guinea	0.050

Notes: Figures refer to average annual DCS in 1970-2008, reported in thousands of constant U.S. dollars in 2005. Standard deviation in parentheses. Measures of income, democracy, civil freedom, and alliance are described in Appendix A2. High income, civil freedom, and ally, denote observations that are above the annual median in the respective measures.

We consider two states of political violence – repression and civil war. To measure repression, we use the Physical Integrity Rights Index from the CIRI Human Rights Data Project.[11] This is an additive index constructed from four components – torture, extrajudicial killing, political imprisonment, and disappearance. Each component represents a violation of physical integrity rights, which can be viewed as instrumentally rational behavior employed by incumbents to achieve political goals. In the raw data, each component takes a score of 0 (frequent occurrence), 1 (occasional occurrence), or 2 (no occurrence); therefore the additive index ranges from 0 to 8, where 0 represents no respect for physical integrity rights. We construct an indicator variable for repression, treating instances where the index is below 4 as repressive regimes.[12] Our repression data are available from 1981 onwards, which slightly constrain our sample size. Within this sample period, approximately 27 percent of country-year observations are in the repression state according to our indicator.

Our civil war variables are based on the Uppsala Conflict Data Program/Peace Research Institute Oslo (UCDP/PRIO) Armed Conflict Dataset, which are widely used as episodic measures of conflict and thus most appropriate for our purpose. Here, an armed conflict is defined as "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". Not all countries are included in the data, so we assume that excluded countries are peaceful. Approximately 14 percent of country-year observations are in the civil war state according to the data.[13] We also construct variables that represent civil war onset and offset. The

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[11]Details on the construction of the index can be found in Cingranelli and Richards (1999). An alternative to this index is a measure from Banks and Wilson (2013) that records the number of political purges – systematic elimination of political opponents by jailing or execution – which is used in Besley and Persson (2011). However, most of the variation in purges stems from the front end of the series, so there are too few purges in the period that we are looking at. Specifically, since 1970, only three percent of all country-years experience purges, and less than one percent have more than a single purge. Another alternative is to use the Political Terror Scale which is based on human rights reports by the U.S. State Department and Amnesty International. That measure, however, is known to suffer from reporting bias (Qian and Yanagizawa, 2009).

[12]We construct this indicator to facilitate the interpretation of our empirical findings, and note that the results are qualitatively robust to using the additive index (0 to 8) as the measure of repression.

[13]Like Besley and Persson (2011), we find that repression and civil war are not necessarily mutually exclusive states in the data. Unlike Besley and Persson (2011), however, we do not conduct our analyses by using ordered states of violence, but instead consider effects on repression and on civil war separately. Hence, we do not make adjustments to the data as they did. What concerns us is that there are 146 cases in which a country is not repressive according to the

state-dependent nature of onset/offset variables raises estimation issues which we discuss later on. For the sake of testing robustness, we also later on examine other war measures that reflect more major episodes of conflict, from the Correlates of War (COW) project, and from the Major Episodes of Political Violence (MEPV) list provided by the Center for Systemic Peace.

3. Empirical Strategy

We first present the econometric model that we use to study the relationship between U.S. weapons sales and the incidence of political violence in purchasing countries. The main estimating equation is given by:

$$y_{irt} = \beta \, w_{irt} + \mathbf{x}'_{irt}\gamma + \psi_{ir} + \delta_{rt} + u_{irt} \tag{1}$$

where y_{irt} is a binary variable indicating either political repression or civil war in country i of region r in year t. w_{irt} measures the log of U.S. weapons sales to i in year t.[14] $\mathbf{x}_{irt}$ is a vector of exogenous control variables which we describe in more detail below. We explicitly allow the unobservable component of the regression equation to be correlated across years within a country and across countries within a region in any given year, by including country fixed effects ψ_{ir} and region-specific year fixed effects δ_{rt} respectively.[15] Region-specific year effects are especially important in accounting for regional political instability that may occur in certain periods, which may be correlated with both violence and weapons.

Our causal effect of interest is β. However, we expect that the OLS estimation of equation (1) does not provide a consistent estimate of β because $cov(w_{irt}, u_{irt}) \neq 0$. That is, we expect that unobserved factors that help explain whether country i experiences political violence in year t

Physical Integrity Rights Index but is in civil war according to the UCDP/PRIO Armed Conflict Dataset. To address the suspicion that these 146 observations are in fact in a repression state, we drop them in our repression analysis and find that our empirical results are qualitatively similar whether we include them or not.

[14]The distribution of DCS is heavily skewed towards zero, with only 15 percent of all country-year observations taking values above the mean. Therefore, we use the log of weapons sales in subsequent analyses. To avoid dropping country-year observations with no weapons sales from the data, we add one dollar before taking logs.

[15]The regions are: Australasia, Middle East, Africa, Europe, and South America. We use this definition of regions to control for unobserved permanent differences across regions because this is the region classification reported in the weapons sales data, and is therefore the relevant categorization from the perspective of weapons sales, our treatment variable. We note that our results are robust to using the World Bank's regional classification system, which is a finer measure as there are seven regions using this categorization.

also explain country $i's$ weapons purchases in year t. In particular, we can think of two reasons for why this is the case.

First, weapons buyers are (perhaps privately) aware of imminent violence, and they purchase weapons in preparation. Conversely, countries engaged in violence may anticipate peace and thus terminate weapons purchases in advance. In this case, we expect an upward *anticipation bias* on the estimate of the effect of weapons purchases on political violence. Second, allies of the U.S. are more likely to have access to the purchase of U.S. weapons. If allies are systematically more (less) likely to be engaged in political violence, then we expect an upward (downward) *ally bias* on the estimate of the effect of weapons purchases on political violence.[16]

To address these issues, we develop an identification strategy that relies on differential demand response of countries to plausibly exogenous variation in the purchasing cost of U.S. weapons. First, we exploit time variation in weapons purchases resulting from changes in U.S. domestic inflation. It is known in the U.S. defence industry that in periods characterized by higher-than-usual domestic inflation, the foreign demand for U.S. weapons increases as the relative price for foreign buyers is lower (DISAM, 2012). This industry assumption is borne out by the data, as we find that a one percent increase in U.S. inflation is roughly associated with a 10 percent decrease in the value of the U.S. dollar, which is a very significant shock to the price of U.S. weapons.[17] We postulate, therefore, that potential buyers are more likely to procure U.S. weapons in years of high U.S. domestic inflation.[18] Given that congressional approval is required for weapons purchases,

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[16]According to the summary statistics in Table 2, the same case can also be made for high income and democratic countries. For simplicity, we refer to biases that may arise collectively as *ally biases*.

[17]Specifically, we examine the relationship between U.S. inflation and a trade-weighted U.S. dollar index from the Federal Reserve. See Table A2 where we present results from two separate regressions: in the first we consider the relationship between lagged inflation and the value of the U.S. dollar, while in the second we consider the same regression but with contemporaneous inflation. The U.S. dollar index, beginning in 1973, is a weighted average of the foreign exchange values of the U.S. dollar against the currencies of a large group of major U.S. trading partners. The index weights, which change over time, are derived from U.S. export shares and from U.S. and foreign import shares. We thank Martin Bodenstein for suggesting this index.

[18]Alternatively, we could use bilateral foreign exchange which would directly capture the price differential effects that we are after, and at the same time, does not require the construction of an interaction instrument to predict contemporaneous weapons sales. However, bilateral foreign exchange rates is partially determined by local economic and political conditions which makes it more likely to be endogenous (to political violence) than U.S. domestic inflation. Another option is to use lagged U.S. domestic inflation alone to instrument for weapons sales, albeit at the cost of being unable to flexibly control for year effects (or even region-specific year effects), which we think are crucial given that U.S. foreign policies are often time-varying.

the actual delivery of purchases will not occur before the following year, so we use variation in lagged inflation rather than contemporaneous inflation. Next, we exploit cross-sectional variation in a country's historical frequency of purchases. This captures the differential purchasing response of buyers to prices – frequent buyers adhere to a regime of military expenditure are thus sensitive to price changes, while infrequent buyers make unforeseen purchases (perhaps out of necessity) and are thus less price-elastic.

Specifically, we use the interaction of lagged U.S. domestic inflation with cross-sectional variation in a country's historical frequency of purchases as an instrument for contemporaneous weapons sales.[19] The first-stage equation in the instrumental variables approach is given by:

$$w_{irt} = \alpha(\pi_{t-1} \times \bar{q}_{ir}) + \mathbf{x}'_{irt}\gamma + \psi_{ir} + \delta_{rt} + u_{irt} \tag{2}$$

where π_{t-1} is the U.S. domestic inflation in year $t-1$, and $\bar{q}_{ir}$ is the country $i's$ historical frequency of weapons purchases over the sample period. For example, if a country purchased U.S. weapons in 10 out of 39 years, then its $\bar{q}_i \approx 0.256$. Figures 1A and 1B illustrates the intuition of our identification strategy. Here, we group weapons buyers into frequent (those who buy more than the sample average of $\bar{q}_{ir}$, which is 0.6) and infrequent (otherwise). Generally speaking, weapons sales by purchasing countries is positively related to lagged U.S. domestic inflation, and importantly, frequent buyers are much more price responsive when compared with infrequent buyers. The stark difference in price responsiveness between these two groups of buyers is evident in the figures.

Our identification strategy relies on the assumption that the interaction term only affects the

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[19]In principle, one could use historical average weapons purchases instead of historical frequency of purchases, as they arguably measure similar quantities and are positively correlated. We prefer to use frequency of purchases in the interaction because we want to separate regular buyers of weapons from irregular buyers of weapons, as we expect that moderate price movements shift weapons purchases on the intensive margin (quantity conditional on buying) more than purchases on the extensive margin (buy or not). In this sense, our instrument is more powerful if we are able to separate regular buyers from irregular buyers. Average weapons sales is not as good a variable for separating these types of buyers, as under this measure, a one-time buyer of a very large amount of weapons may appear more like a "regular" buyer than a country that buys a smaller amount of weapons relatively frequently. We believe this intuition also underlies the identification strategy in Nunn and Qian (2014).

likelihood of political violence through procured weapons. Formally:

$$E\left[\pi_{t-1} \times \bar{q}_{ir} \times u_{irt}\right] = 0 \tag{3}$$

Our instrument combines a plausibly exogenous component in lagged U.S. domestic inflation π_{t-1} with the propensity to purchase weapons $\bar{q}_{ir}$. Since we allow for (region-specific) time fixed effects and country-specific fixed effects, by construction, we have:

$$E[\pi_{t-1} u_{irt}] = 0$$
$$E[\bar{q}_{ir} u_{irt}] = 0$$

Notice that country fixed effects are useful because they account for any permanent differences across countries that may cause them to have systematically-different values of the instrument. Additionally, region-specific time effects account for global and regional economic and political conditions that vary over time. This is important especially since U.S. inflation reflects many changes in the U.S. and world economy. Given that inflation shifts the exchange rate (our identification argument hinges on this), it could in turn affect trade more generally as U.S. imports and exports move the market price, output, and real incomes of many countries. Furthermore, inflation is a consequence of economic conditions in the U.S., which could also affect the demand and supply of foreign goods, again shaping the market prices, output and real incomes in the rest of the world.

However these conditions holding are not sufficient for the identification assumption (3) to hold; even if lagged inflation and weapons purchase propensity are each individually uncorrelated with the unobservable, their interaction may still be correlated with it. This may be the case if, for example, U.S. domestic inflation is correlated with U.S. political cycles that affect political violence differently across frequent and infrequent buyers of U.S. weapons. Specifically, recent inflation may be correlated with the type of U.S. government in power (Democrat or Republican)

and this in turn may be related to conflict in purchasing countries differentially. To address this, we include the interaction of $\bar{q}_{ir}$ with an indicator that equals one in years where the U.S. president is a Democrat, and region-specific year effects. Similarly, U.S. domestic inflation may affect political violence in purchasing countries due to changes in economic conditions. If countries with a higher (or lower) propensity to buy weapons depend on the health of the U.S. economy to a greater extent, then the interaction of lagged U.S. inflation and weapons propensity may be endogenous. For example, a U.S. economic downturn could translate into negative economic shocks for countries with latent violence, which in turn could ignite political violence. We address this by allowing for region-specific year effects.

We should also be concerned that economic conditions in the U.S. affect its willingness to intervene with economic and military aid in conflict prone countries. If, for example, during periods of high inflation, the U.S. is less willing to spend aid to reduce conflict abroad, and that this (un)willingness depends on some permanent unobservable feature of the recipient country, our identification may be compromised. To deal with this potential issue, we also control for U.S. strategic and military interest by allowing for interactions between country averages in per capita (i) U.S. economic aid, (ii) U.S. military aid, and (iii) U.S. troops, interacted with year fixed effects. Again, this is an interaction between a permanent variable, exogenous by construction, and a time fixed effect.[20]

Ultimately, identification in our setting relies on an untestable assumption [equation (3)]. In particular, even if our instrument does not cause conflict through some channel that we have not controlled for, one could still be concerned about selection of the instrument on unobservable variables. In particular, if for some unobservable time varying reason, countries with a higher propensity for conflict have systematically different interactions between propensity to purchase weapons and U.S. inflation. In sections 4.4 and 4.5 we consider a battery of checks for selection on unobservables as well as alternative checks of the exclusion restriction. The evidence is strongly

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[20]U.S. economic aid data are taken from the U.S. Overseas Loans and Grants (Greenbook), while U.S. military aid and troops data are provided by the U.S. Department of Defense. Here, we cannot directly control for time-varying U.S. economic aid, U.S. military aid, or U.S. troops, as they may be endogenously determined by U.S. weapons sales.

in favour of the causal channel that we propose.

4. Results

4.1 Repression and Civil War

We present the results for repression and civil war sequentially. Additionally, in our examination of the effects on civil war, we follow Nunn and Qian (2014) by first looking at incidence and then at mechanisms (onset and duration, separately). Table 3 shows the results for repression. In column (1), we see that the OLS estimate is -0.010, which indicates that the log of weapons sales is negatively correlated with repression in purchasing countries. The magnitude of the estimated effect is robust to the inclusion of country and region-year fixed effects, as presented in column (2). As discussed previously, we do not believe these estimates have a causal interpretation. Governments may purchase weapons anticipating political violence. As such, we instrument for the log of weapons sales in columns (3)-(4), where the latter includes control variables (explained in the previous section). The first stage estimates from estimating equation (2) show that our instrument is positively correlated with the log of weapons sales, which is in line with our expectations: demand responds positively to U.S. inflation, and more so for frequent buyers. The Cragg-Donald F-statistics are larger than the Stock-Yogo critical values for 15 percent maximal bias in size, so our instrument is reasonably strong. Our IV estimates show that the effect of log of weapons sales on repression is rather large. In column (4), for example, the IV estimate is -0.075, statistically significant at the five percent level.

Before discussing the interpretation of these findings, we first present the results for the civil war outcome in Table 4. Here, the OLS and fixed effects estimates in columns (1)-(2) show that the log of weapons sales have no statistically-significant effect on civil war. However, once we instrument for the log of weapons sales, the effect becomes positive and economically large. In column (4), for example, where we also include control variables, the IV estimate is 0.075, statistically significant at the 10 percent level. For both repression and civil war, we find that the IV estimates are much larger in magnitude than their OLS and FE counterparts, which suggests that the anticipation and ally biases may be quite substantial and/or that the OLS and FE estimates

Table 3: Repression and U.S. Weapons

Dependent variable: Repression	OLS(1)	FE(2)	IV(3)	IV(4)
log(DCS)	-0.010***	-0.010***	-0.123**	-0.075**
	(0.003)	(0.003)	(0.059)	(0.043)
First-stage:			log(DCS)	log(DCS)
Lagged inflation × DCS history			40.030**	52.878***
			(17.561)	(20.346)
Cragg-Donald F-statistic			10.89	14.29
Country & region-year FE	No	Yes	Yes	Yes
Democrat × DCS history	No	No	No	Yes
Avg. U.S. economic aid p.c. × year FE	No	No	No	Yes
Avg. U.S. military aid p.c. × year FE	No	No	No	Yes
Avg. U.S. troops p.c. × year FE	No	No	No	Yes
N	4,241	4,241	4,241	3,618

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

suffer from attenuation bias due to measurement error in weapons purchases.

Taken together, the results suggest that U.S. weapons sales decrease the incidence of repression but increase the incidence of civil war. This suggests that purchases of U.S. weapons cause countries that would have been at peace or repressive but not in a state of war to enter into civil war. Conceptualizing repression and civil war as ordered states of violence, the results raise an interesting question: why do U.S. weapons have an asymmetric effect on the two types of violence?

We find the Besley-Persson (2009) framework a useful starting point for rationalizing our findings. In their framework, an incumbent government and an opposition choose how much to invest in violence, and they establish conditions under which the equilibrium level of political violence is increasing in the spoils of war. When spoils are small, neither party invests in violence and "peace" ensues. When the spoils are at some intermediate level only the incumbent government invests in violence and "repression" ensues, and when the spoils are large both sides invest and "civil war" ensues. That is, there are two threshold levels of spoils: the smaller one defining whether a country is in peace or repression, depending on whether the incumbent chooses a positive level of violence, and the larger one defining whether a country is repressive or at war,

Table 4: Civil War and U.S. Weapons

Dependent variable: Civil war	OLS(1)	FE(2)	IV(3)	IV(4)
log(DCS)	0.002	0.000	0.056*	0.075*
	(0.002)	(0.024)	(0.032)	(0.045)
First-stage:			log(DCS)	log(DCS)
Lagged inflation × DCS history			32.096***	33.796**
			(10.816)	(15.656)
Cragg-Donald F-statistic			26.93	19.88
Country & region-year FE	No	Yes	Yes	Yes
Democrat × DCS history	No	No	No	Yes
Avg. U.S. economic aid p.c. × year FE	No	No	No	Yes
Avg. U.S. military aid p.c. × year FE	No	No	No	Yes
Avg. U.S. troops p.c. × year FE	No	No	No	Yes
N	7,416	7,416	7,416	5,694

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

depending on whether the opposition chooses to respond to incumbent violence with a positive violence investment.[21]

A key component in the Besley-Persson framework is a conflict technology which determines, for given investments in political violence by the incumbent and opposition, the likelihood of the opposition defeating the incumbent. This technology is modeled by a contest function, on which the authors place minimal assumptions and is for the most part left as a black box. As incumbent military capacity almost certainly enters this contest function, our study potentially sheds some light on this aspect of their framework. Interpreting our findings strictly within the confines of their framework, increased state military capacity alters the contest function in such a way that the peace-repression threshold remains unchanged, while the repression-civil war threshold shifts

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[21]If we interpret violence as an ordered variable (repression is worse than peace and civil war is worse than repression), an ordered outcome framework such as that employed in Besley and Persson (2011) may seem more appropriate for studying the causal effect of interest. However, given the importance of unobserved heterogeneity in studies using cross country panel data, in addition to the complications associated with instrumental variables methods in non-linear models, standard ordered outcome methods are not desirable. In any case, we formally tested whether political violence is ordered in the log of weapons sales. We define two binary variables, v_{irt}^{L} and v_{irt}^{H} where the former indicates whether *either* repression or civil war occurred, while the latter indicates whether civil war occurred. Our logit estimates indicate that the effect of the log of weapons sales on v_{irt}^{L} is zero (economically and statistically) while the effect of the log of weapons sales on v_{irt}^{H} is positive and statistically significant.

downward. For this to be the case, more weapons in the hands of the state must incite the opposition to use violence in airing its grievance (which leads to civil war) as opposed to choosing non-violence when faced with the prospect of state repression.

This then begs the question: why would the threshold between peace and repression remain fixed while the threshold between repression and war shifts downward? Recent studies in political science have established that when states use violence as a means of subduing perceived or real insurgent threat, the insurgency gains popular support, especially when civilians are part of the "collateral damage" (Condra, Felter, Iyengar, and Shapiro, 2010; Condra and Shapiro, 2012). Insurgent movements that anticipate state violence are more likely to use violence themselves when they enjoy popular support, and they are more likely to enjoy popular support when the state is in possession of heavy artillery that may inflict civilian casualties.

While this is certainly a possibility and allows us to rationalize our findings without leaving the confines of the Besley-Persson framework, alternative explanations are possible once one is willing to consider alternative assumptions. In Appendix A3 we present a simple model very much in the spirit of the Besley-Persson framework with one key modification: we allow for the possibility that before the state and its opponent decide on investments in violence, the state makes a choice, observable to all, about costly investment in military capacity. We also assume that there is asymmetric information about the state's willingness to use arms in carrying out political violence. Using weaponry increases the likelihood of civilian casualties as well as other undesirable outcomes, and the opposition is unsure about how costly the state finds these outcomes. In a separating equilibrium of the model, only states that are willing to use weapons in repression make the costly investment in weapons, and as long the opposition finds the prospect of repression by an armed state sufficiently unattractive, it mounts a violent insurgency in response to a militarized state which then leads to civil war, even if in the absence of the state investment in weapons the opposition would have accepted the prospect of repression and not instigated a civil war. The theory then predicts that weapons can serve as a signal of the state's intentions. If the state's intentions are to use violence augmented by weaponry, and if the repressed opposition gets an

especially small share of resources when the state is armed, the opposition chooses to mount a violent insurgency and civil war ensues. In this way, an exogenous decrease in the cost of weapons can change the equilibrium from repression to civil war, rationalizing our empirical findings.

4.2 Mechanisms

To further understand the mechanisms through which U.S. weapons sales increase the incidence of civil war, we study whether it is the *onset* or *duration* of war that increases as a result of weapons sales.[22] If we find that weapons increase the *onset* of war, we can infer that states that would have dealt with domestic political threat through less violent means are now more likely to enter into civil war with political opponents once they receive weapons. Alternatively, if weapons cause an increase in the *duration* of a civil war, we can infer that countries that would have transited to a less violent state, by either choosing repression or reaching a peace agreement, instead continue in a state of war. We present estimates of the effect of weapons sales on civil war onset and duration in Table 5.

With regards to war onset, we try two approaches. One is to exclude periods of ongoing conflict, following Collier and Hoeffler (2004). An alternative approach by Fearon and Laitin (2003) is to include those observations but control for the occurrence of conflict in the preceding period. The second approach preserves the full sample while accounting for the mechanical relationship between the onset of war and the occurrence of war in the last period. In columns (1)-(2), we try the first approach, and in columns (3)-(4) we try the second. Both approaches offer qualitatively similar conclusions, that the log of weapons sales increases the likelihood of civil war onset. The magnitudes of IV estimates (conditional on controls) range from 0.021-0.026.

Next, we examine the effects on war offset (or duration) by estimating a discrete hazard model, where the hazard is assumed to be logistically distributed. In this case, the sample size is very much reduced because we exclude all periods of peace except when peace is first achieved following periods of war. In other words, the sample comprises only countries that "survive" by

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[22]Modeling onset and duration separately also has the added advantage of addressing conflict dynamics, since running on split samples has been shown to be mathematically equivalent to introducing lagged conflict to the model (Beck and Katz, 2011; Bazzi and Blattman, 2014). We thank an anonymous referee for this suggestion.

Table 5: War Onset/Offset and U.S. Weapons

Dependent variable:	Civil war onset				Civil war offset	
	IV(1)	IV(2)	IV(3)	IV(4)	Logit(5)	Logit(6)
log(DCS)	0.021**	0.025**	0.022*	0.026*	-0.021	-0.034
	(0.009)	(0.011)	(0.012)	(0.016)	(0.034)	(0.041)
First-stage:	log(DCS)	log(DCS)	log(DCS)	log(DCS)		
Lagged inflation × DCS history	35.809***	43.851***	32.339***	34.291**		
	(11.053)	(15.873)	(10.711)	(15.439)		
Cragg-Donald F-statistic	31.99	30.76	27.25	20.36		
Lagged civil war	No	No	Yes	Yes	-	-
Country & region-year FE	Yes	Yes	Yes	Yes	-	-
Democrat × DCS history	No	Yes	No	Yes	-	-
Avg. U.S. economic aid p.c. × year FE	No	Yes	No	Yes	-	-
Avg. U.S. military aid p.c. × year FE	No	Yes	No	Yes	-	-
Avg. U.S. troops p.c. × year FE	No	Yes	No	Yes	-	-
Region FE	-	-	-	-	Yes	Yes
Third-degree polynomial in duration	-	-	-	-	Yes	Yes
Time-invariant controls	-	-	-	-	No	Yes
N	6,567	4,853	7,416	5,694	1,091	1,072

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

continuing to be at war and those that achieve peace (in its first year). We specify a third degree polynomial of duration to flexibly model the effect of duration. We also control for region fixed effects and time-invariant controls: U.S. economic aid, U.S. military aid, and U.S. troops per capita. We instrument for the log of weapons sales by applying a control function in a two-stage approach, where first-stage residuals enter the second-stage logit regression as a control function. Results from columns (5)-(6) indicate that there is no effect on war offset.

Taken together, our results say the following: U.S. weapons sales decrease political repression and increase the incidence of civil war by affecting war onset (but not duration) in purchasing countries. For a country purchasing the average amount of 29 million dollars through DCS, every additional 10 million dollars of weapons will decrease repression risk by 2.2 percentage points, and increase the incidence of civil war by 2.2 percentage points.[23] To use a concrete example, con-

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[23]The sample average of DCS is 29.572 million dollars, so an exogenous increase of 10 million dollars is equivalent to a change in the log of weapons sales of $\ln(39572) - \ln(29572) \approx 0.291$. We multiply this by the coefficient β in each case to obtain the implied effect.

sider two countries with roughly equal purchasing power (in terms of per capita income) – Oman and Turkey. Turkey is a major arms buyer averaging 106 million dollars in DCS annually, while Oman averages just 9 million dollars. Raising Oman's U.S. weapons purchases to that of Turkey's average will increase Oman's civil war propensity by approximately 18 percentage points.

4.3 Weapons Stock

Perhaps the most serious concern with respect to our identification strategy is that countries that anticipate imminent war purchase weapons hurriedly, contaminating the effect of weapons on political violence that we identify. This is important since we have so far only examined current weapons purchases which may be strongly influenced by anticipation of war. Of course, for this to invalidate our results it must be the case that the anticipatory behavior is more prevalent among frequent buyers in times of high U.S. inflation, which we find unlikely. Nevertheless, we provide some robustness results by studying the effect of the accumulated *stock* of U.S. commercial weapons, which is arguably less affected by anticipatory purchases than current weapons purchases, on political violence. In particular we replace current weapons with the previous three years' worth of purchases in our main equation of interest. To instrument for 3-year weapons stock, we use a set of three instruments: inflation in $t-1$, difference in inflation between $t-1$ and $t-2$, and difference in inflation between $t-2$ and $t-3$, each interacted with DCS history.

In Tables 6-8 we present the estimated causal effect of weapons stock on political violence. All our previous results continue to hold (qualitatively) when we replace current weapons purchases with accumulated weapons stock. In particular, weapons stock causes a decrease in repressive behavior, an increase in the incidence of civil war which is explained fully by an increase in the onset of war from a state of non war.

Table 6: Repression and 3-Year U.S. Weapons Stock

Dependent variable: Repression	OLS(1)	FE(2)	IV(3)	IV(4)
log(DCS stock)	-0.009***	-0.010***	-0.078**	-0.048**
	(0.003)	(0.003)	(0.034)	(0.022)
First-stage:			log(DCS stock)	log(DCS stock)
$t-1$ inflation $\times$ DCS history			18.285	21.375
			(27.247)	(33.612)
$\Delta(t-2)$ inflation $\times$ DCS history			63.746**	142.491***
			(25.391)	(29.986)
$\Delta(t-3)$ inflation $\times$ DCS history			87.842**	80.944**
			(35.272)	(40.536)
Cragg-Donald F-statistic			8.96	16.14
Country & region-year FE	No	Yes	Yes	Yes
Democrat $\times$ DCS history	No	No	No	Yes
Avg. U.S. economic aid p.c. $\times$ year FE	No	No	No	Yes
Avg. U.S. military aid p.c. $\times$ year FE	No	No	No	Yes
Avg. U.S. troops p.c. $\times$ year FE	No	No	No	Yes
N	4,239	4,239	4,239	3,618

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 12.83 and 22.30 respectively.

Table 7: Civil War and 3-Year U.S. Weapons Stock

Dependent variable: Civil war	OLS(1)	FE(2)	IV(3)	IV(4)
log(DCS stock)	0.003	-0.001	0.017*	0.016*
	(0.002)	(0.002)	(0.010)	(0.010)
First-stage:			log(DCS stock)	log(DCS stock)
$t-1$ inflation $\times$ DCS history			55.224***	58.292***
			(17.210)	(24.207)
$\Delta(t-2)$ inflation $\times$ DCS history			62.236***	91.635***
			(9.094)	(9.799)
$\Delta(t-3)$ inflation $\times$ DCS history			96.197***	94.252***
			(12.362)	(16.606)
Cragg-Donald F-statistic			36.55	31.25
Country & region-year FE	No	Yes	Yes	Yes
Democrat $\times$ DCS history	No	No	No	Yes
Avg. U.S. economic aid p.c. $\times$ year FE	No	No	No	Yes
Avg. U.S. military aid p.c. $\times$ year FE	No	No	No	Yes
Avg. U.S. troops p.c. $\times$ year FE	No	No	No	Yes
N	7,414	7,414	7,414	5,694

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 12.83 and 22.30 respectively.

Table 8: War Onset / Offset and 3-Year U.S. Weapons Stock

Dependent variable:	Civil war onset				Civil war offset	
	IV(1)	IV(2)	IV(3)	IV(4)	Logit(5)	Logit(6)
log(DCS stock)	0.007***	0.009**	0.005	0.004	-0.022	-0.031
	(0.003)	(0.004)	(0.004)	(0.004)	(0.034)	(0.041)
First-stage:	log(DCS stock)	log(DCS stock)	log(DCS stock)	log(DCS stock)		
$t-1$ inflation × DCS history	64.823***	78.802***	55.628***	59.019***		
	(16.952)	(24.032)	(17.155)	(24.078)		
$\Delta(t-2)$ inflation × DCS history	58.096***	86.949***	62.410***	91.562***		
	(9.427)	(10.706)	(9.135)	(9.797)		
$\Delta(t-3)$ inflation × DCS history	94.036***	99.219***	96.293***	94.638***		
	(12.708)	(17.946)	(12.379)	(16.641)		
Cragg-Donald F-statistic	40.24	38.89	36.78	31.52		
Lagged civil war	No	No	Yes	Yes	-	-
Country & region-year FE	Yes	Yes	Yes	Yes	-	-
Democrat × DCS history	No	Yes	No	Yes	-	-
Avg. U.S. economic aid p.c. × year FE	No	Yes	No	Yes	-	-
Avg. U.S. military aid p.c. × year FE	No	Yes	No	Yes	-	-
Avg. U.S. troops p.c. × year FE	No	Yes	No	Yes	-	-
Region FE	-	-	-	-	Yes	Yes
Third-degree polynomial in duration	-	-	-	-	Yes	Yes
Time-invariant controls	-	-	-	-	No	Yes
N	6,565	4,853	7,414	5,694	1,091	1,072

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 12.83 and 22.30 respectively.

4.4 Falsification Tests

While the exclusion restriction i.e. the assumption that $E\left[\pi_{t-1} \times \bar{q}_{ir} \times u_{irt}\right] = 0$ is not directly testable, we propose three sets of falsification tests that may inform us about whether the assumption is likely to hold.[24]

First, we run our first-stage regressions on a sample of country-years where the local currency was pegged to the U.S. dollar.[25] If our instrument ($\pi_{t-1} \times \bar{q}_{ir}$) is indeed picking up price responsiveness due to changes in U.S. domestic inflation (and hence changes in foreign exchange), then in country-years where the foreign exchange is fixed we should observe no impact on the demand for U.S. weapons, and that is is exactly what we find in columns (1) and (3) of Table 9. Moreover, for this sample, there is no statistically-significant relationship between the instrument and political violence [columns (2) and (4) of Table 9], which confirms that our instrument, in cases where it could not influence the demand for weapons, does not explain the variation in political violence. Moreover, to check that the lack of statistically-significant effects is not driven by this particular set of countries, we also run the same regressions for the country-years in which there were no currency pegs [columns (5)-(8)] and find that there the effects are always statistically significant.

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[24]These tests are inspired by Angrist (1990), who examined the effect of the draft (using a lottery on draft eligibility as instrument) on earnings. He tested for the lack of draft eligibility effects on earnings among non-draftee cohorts, as well as the lack of draft eligibility effects on pre-draft-lottery earnings. Nunn and Qian (2014) also produced similar tests by checking that their instrument predicts future food aid but is uncorrelated with past food aid.

[25]The pegged exchange cases that coincide with our weapons data are: Antigua and Barbuda (1976-), Bahamas (1966-), Bahrain (2001-), Barbados (1975-), Belize (1978-), Cambodia (1980-), Dijibouti (1973-), Dominica (1976-), East Timor (2000-), Ecuador (2000-), El Salvador (2001-), Eritrea (2005-), Micronesia (1944-), Grenada (1976-), Haiti (1912-1989), Iraq (1959-1971, 1971-1989), Jordan (1995-), Lebanon (1997-), Liberia (1944-), Marshall Islands (1944-), Oman (1986-), Palau (1944-), Panama (1904-), Qatar (2001-), Saint Kitts and Nevis (1976-), Saint Lucia (1976-), Saint Vincent and the Grenadines (1976-), Saudi Arabia (2003-), United Arab Emirates (1997-).

Table 9: Falsification Tests I - Pegged Currency

Dependent variable:	Pegged				Not Pegged			
	log(DCS) FE(1)	Repression FE(2)	log(DCS) FE(3)	Civil war FE(4)	log(DCS) FE(5)	Repression FE(6)	log(DCS) FE(7)	Civil war FE(8)
Lagged inflation × DCS history	-1261.200	-69.985	-1746.759	-168.693	48.580**	-4.534**	34.619**	2.625**
	(2292.310)	(82.861)	(2292.366)	(169.962)	(21.872)	(2.261)	(15.938)	(1.279)
Country & region-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Democrat × DCS history	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg. U.S. economic aid p.c. × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg. U.S. military aid p.c. × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg. U.S. troops p.c. × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	195	210	287	287	3,423	3,550	5,407	5,407

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. This sample comprises only country-years where the local currency was pegged to the U.S. dollar. Furthermore, columns (1)-(2) is limited to years where we have data on repression.

Table 10: Falsification Tests II - Zero Weapons Purchases

Dependent variable:	Repression FE(1)	Civil war FE(2)	Repression FE(3)	Civil war FE(4)	Civil war FE(5)
Lagged inflation × DCS history	-4.093	5.894	-121.342	-39.744	0.187
	(7.315)	(4.065)	(152.839)	(107.143)	(0.469)
Country & region-year FE	Yes	Yes	Yes	Yes	Yes
Democrat × DCS history	Yes	Yes	Yes	Yes	Yes
Avg. U.S. economic aid p.c. × year FE	Yes	Yes	Yes	Yes	Yes
Avg. U.S. military aid p.c. × year FE	Yes	Yes	Yes	Yes	Yes
Avg. U.S. troops p.c. × year FE	Yes	Yes	Yes	Yes	Yes
N	1,004	2,052	105	152	3,066

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. Columns (1)-(2) use only country-years where there were no weapons purchases, and columns (3)-(4) additionally require that those country-years coincided with an arms embargo. Column (5) uses the pre-1970 sample.

Second, we check if there exist an association between political violence (repression or civil war) and the instrument – essentially the reduced form regression – in country-years where there were no weapons purchases. If the only reason for the instrument to affect political violence is via weapons purchases, then the effect of the instrument on political violence should be zero when there are in fact no weapons purchases. In this regard, it is comforting to see that the instrument has no statistically-significant relationship with repression or civil war in country-years where there were no weapons purchases [columns (1)-(2) of Table 10]. Furthermore, we confirm that the lack of association between political violence and the instrument continues to hold in cases where zero weapons purchases were observed for exogenous reasons, either due to arms embargoes [columns (3)-(4) of Table 2] or in the pre-1970 period before DCS became available [column (5), Table 10].[26]

Third, we check if the first-stage estimates could be confounded by spurious trends between U.S. inflation and weapons purchases. We do this by estimating "placebo" first-stage regressions where the instrument is used to predict past rather than future weapons purchases. In Table 11, the falsification results indicate that neither contemporaneous weapons purchases ($w_{ir,t-1}$) nor past weapons purchases ($w_{ir,t-2}$ and $w_{ir,t-3}$) are associated with the instrument. These results

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[26]Note that for the pre-1970 sample, we can only examine the relationship between civil war and the instrument because our repression data does not begin until 1981.

Table 11: Falsification Tests III - Spurious Trends

Dependent variable:	$t-1$ log(DCS) FE(1)	$t-2$ log(DCS) FE(2)	$t-3$ log(DCS) FE(3)	$t-1$ log(DCS) FE(4)	$t-2$ log(DCS) FE(5)	$t-3$ log(DCS) FE(6)
Lagged inflation $\times$ DCS history	36.669	26.689	30.787	14.870	-5.048	-16.759
	(23.561)	(26.532)	(30.669)	(16.975)	(18.343)	(19.076)
Country & region-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Democrat $\times$ DCS history	Yes	Yes	Yes	Yes	Yes	Yes
Avg. U.S. economic aid p.c. $\times$ year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg. U.S. military aid p.c. $\times$ year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg. U.S. troops p.c. $\times$ year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	3,760	3,760	3,760	5,694	5,694	5,694

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. Columns (1)-(3) is limited to years where we have data on repression.

are reassuring as they tell us that the instrument only predicts future weapons purchases but not contemporaneous or past weapons purchases, so our earlier first-stage estimates are not picking up any possible spurious relationships between U.S. inflation and weapons purchases.

4.5   Further Robustness Checks

While our falsification tests provide significant evidence in support of our key findings, we consider several other checks to establish the robustness of our results. First is the issue of selection. Even if our instrument does not *cause* political violence through a channel other than weapons, we must be concerned about the possibility that countries with a high (or low) propensity for political violence having systematically-different values of the instrument. We know this is unlikely to a certain degree as our instrument is the interaction between a variable that is fixed at the country level over time, thus necessarily satisfying the exclusion restriction given that we difference away unobservable permanent heterogeneity, with another variable that varies over time but is common across countries, thus also satisfying the exclusion restriction given that we include time effects in the regression. As such, the exclusion restriction is not violated even if, for example, countries that are especially prone to political violence tend to be more frequent buyers of U.S. weapons.

However, to be sure that our results are not driven by selection on unobservables, we use a simple method proposed by Altonji, Elder, and Taber (2005). As they point out, the amount

Table 12: Reduced-Form Results

Dependent variable:	Repression		Civil war	
	FE(1)	FE(2)	FE(3)	FE(4)
Lagged inflation × DCS history	-4.912**	-3.978*	2.516**	2.522**
	(2.016)	(2.071)	(1.175)	(1.196)
Country & region-year FE	Yes	Yes	Yes	Yes
Democrat × DCS history	No	Yes	No	Yes
Avg. U.S. economic aid p.c. × year FE	No	Yes	No	Yes
Avg. U.S. military aid p.c. × year FE	No	Yes	No	Yes
Avg. U.S. troops p.c. × year FE	No	Yes	No	Yes
N	3,618	3,618	5,694	5,694

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level.

of selection on *observable* variables, which can be estimated, is informative about the selection on *unobservables* that may be present. More precisely, we can quantify the amount of selection on unobservables relative to selection on observables that would need to be present in order to reduce our results to a null effect. To do so, we compare the reduced form regressions with and without observable controls.[27] The results are in Table 12.

First, consider columns (1) and (2), where we examine the reduced-form effect of the instrument on repression. In column (1) we only include country and region-year fixed effects, and in column (2) we introduce our full set of control variables, which, as we have previously explained, is an exhaustive list of observable variables that may be correlated with the instrument and political violence. We see that as we go from column (1) to (2), the coefficient estimate drops by almost 20 percent. This means that there would have to be four times as much selection on *unobservables* as there is selection on *observables* in order for us to be obtaining the estimate we do when the population effect of the instrument on repression is zero. This is an implausibly large amount of selection on unobservables given the controls we are considering. The comparison between columns (3) and (4) is even more stark in that, if the selection on unobservables works in the same direction as the selection on observables, than we are *underestimating* the effect of weapons on conflict. In particular, when we add controls, the estimated reduced-form effect of the instrument on

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[27]Our approach here is one of coefficient stability which follows from Bellows and Miguel (2009).

civil war increases, which means that if relevant unobservables have the same effect, we should only expect the coefficient estimate to increase if we were able to control for them.

The finding from above that we are potentially *underestimating* the effect of weapons on conflict raises another question – are weapons flows from the U.S. correlated with flows from other countries? In other words, are we spuriously attributing the effect of global weapons flows to U.S. weapons flows? To check this, we reconsider our first-stage regression by replacing U.S. weapons with total arms imports (see Table A3).[28] The idea is to check whether our instrument shifts other weapon flows besides the flows from the U.S.. Interestingly, the results suggest that, if anything, there is a negative effect of our instrument on global weapons flows, though this is far from being statistically significant. This suggests that countries perhaps substitute towards U.S. weapons when they become cheaper relative to arms supplied by the rest of the world, though we cannot make conclusive statements on this.[29]

Another concern is conflict dynamics. In particular, conflict is persistent, so one would expect that a predictor of conflict is past conflict. For this to break our identification strategy, it must be the case that countries with a high propensity to buy weapons are systematically more (or less) likely to be in conflict during periods of high inflation in the U.S.. The obvious way to check whether our results are robust to this possibility is to include lagged civil war as a control variable in our civil war regressions. However, as Beck and Katz (2011) and Bazzi and Blattman (2014) have shown, a more flexible method that accounts for conflict dynamics just as well is to separately examine the onset and offset of civil wars as we do above. We found there that weapons increase the probability of new wars starting (onset) but do not have an effect existing ones (offset). For completeness, we also consider conflict regressions with lagged conflict included as a control in Table 13. As we see in the table, including lagged conflict reduces the magnitude of the coefficient significantly, though

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[28]The arms imports data are taken from the SIPRI Arms Transfers Database.

[29]One further concern is that as U.S. weapons become cheaper for importing countries, they become more accessible to insurgent/opposition groups as well. While we cannot formally test this possibility due to data limitations (our U.S. weapons data only cover arms transfers to the state, not to opposition groups), we note that most U.S. weapons that go directly to opposition groups (such as sales to armed opposition groups in Soviet allied countries during the Cold War) are a consequence of geopolitical and not market factors. The weapons channel we study, and importantly, our instrument, should not be systematically related to these non-market transactions.

Table 13: Civil War and U.S. Weapons - Including Lagged Civil War)

Dependent variable: Civil war	OLS(1)	FE(2)	IV(3)	IV(4)
log(DCS)	0.001	0.000	0.022*	0.026
	(0.000)	(0.001)	(0.012)	(0.016)
First-stage:			log(DCS)	log(DCS)
Lagged inflation × DCS history			32.339***	34.291**
			(10.711)	(15.439)
Cragg-Donald F-statistic			27.25	20.36
Lagged civil war	Yes	Yes	Yes	Yes
Country & region-year FE	No	Yes	Yes	Yes
Democrat × DCS history	No	No	No	Yes
Avg. U.S. economic aid p.c. × year FE	No	No	No	Yes
Avg. U.S. military aid p.c. × year FE	No	No	No	Yes
Avg. U.S. troops p.c. × year FE	No	No	No	Yes
N	7,416	7,416	7,416	5,694

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

the sign remains the same and it is significant or near significance [the coefficient in column (4) is statistically significant at the 11 percent level]. The result is not surprising given that when we account for conflict dynamics in a more flexible way (studying onset and offset separately) we get that one coefficient is very significant (onset) and one is not (offset). The estimate in Table 13 can be interpreted as a weighted average of those estimates.

Finally, we look into the issue of conflict scale. Our empirical results suggest that weapons purchasers transit from repression to civil war, but one might ask: how could states transit so distinctly and abruptly from a repressive regime to full-blown civil war by simply buying more weapons? Indeed, while the effects that we found so far are substantial, they do not necessarily imply that weapons purchasers enter into *major* civil conflict. To see this, we consider alternative measures of civil war that reflect only major episodes of conflict. First, we use two episodic measures that count only wars that exceed 1,000 battle-related combatant fatalities: a major civil war indicator from the UCDP/PRIO dataset, and an indicator from the Correlates of War (COW) project. Second, we use a civil war indicator that include only wars that have at least 500 "directly-

related" fatalities from the Major Episodes of Political Violence (MEPV) list provided by the Center for Systemic Peace. In contrast, our current measure includes civil wars with at least 25 battle-related deaths. In Table A4, we present the results from using these measures of major civil war, where column (1) essentially uses a higher civil war threshold from the same UCDP/PRIO dataset that we employed earlier. We see that while the effect of weapons purchases on all three alternative civil war measures are positive, they are not statistically significant. This suggests that the transitions from repression to civil war may be more subdued in nature, as they do not necessarily lead to large-scale warfare.

5. Conclusions

In this paper we study the causal effect of commercial weapons purchases from the U.S. on political violence. We exploit exogenous shifts in the cost of purchasing commercial weapons from the U.S. to identify these effects. We find that weapons purchases reduce the incidence of political repression but increase the incidence of civil war in purchasing countries, and that the increased incidence of civil war is due entirely to an increase in *onset*, and not *duration*. Interpreting political violence as an ordered variable as in the existing literature, our results suggest that state militarization induces the state opposition to mount an insurgency, which the state responds to with violence. We argue that our empirical findings can be rationalized by a simple model where state militarization signals to the opposition the state's willingness to employ armed violence in addressing an opposition grievance.

We make two caveats with respect to the external validity of our findings. First, the weapons purchases in our analyses correspond to commercial market weapons transactions, and thus we do not (and cannot) similarly conclude for other types of weapons transfers that may be more strategically-motivated. As such, our results should be interpreted cautiously: the causal effect that we identify here relates only to the types of weapons sold through commercial channels. Second, it is possible that weapons sold by other countries have different consequences for political violence, and in this case our results can not be applied to transactions that do not involve the U.S. In both cases, however, we note that it is the very fact that weapons are commercially sold by

firms in the U.S. that provides us with the exogenous variation necessary to identify the causal effect of interest. In particular, the fact that commercial U.S. weapons sales move with U.S. inflation, plausibly exogenous to recipient country outcomes, gives our results internal validity, and in this sense our study is U.S.-specific by design.

Finally, while we expect that many countries are price responsive and that therefore we are identifying an average effect for a meaningful subset of the population, it is possible that we are identifying a particularly high LATE from "compliers" if price responsiveness and the impact of weapons on political violence are highly correlated. In spite of this potential limitation, the fact that changes in the instrument represent changes in price make our results policy relevant, as our findings suggest that those countries whose demand for weapons is elastic are precisely the ones who should have their weapons purchases restricted.

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Appendix A1. Comparing DCS and FMS

In general, DCS and FMS are subject to the same process involving congressional review and approval, technology release approval, end-use monitoring, and re-transfer agreement. However, there are also stark differences, First, DCS allows buyers to acquire non-standard, tailor-made defense articles. Second, weapons sold through FMS are priced at cost while those sold through DCS have prices determined by direct negotiations between the buyer and the manufacturer. However, this does not necessarily mean that weapons sold through FMS are cheaper, as it is difficult to compare prices between DCS and FMS in practice.[30] Third, DCS prices may be lower in instances where two or more manufacturers are competing for business. Third, the U.S. Department of Defense is heavily involved in FMS. The Pentagon acts as a middleman between buyers and weapons manufacturers, provides purchase consolidation, contract administration services, and even manages military financing loans or grants that may be used to pay for weapons. Finally, DCS do not include after-sales maintenance or service, and are less transparent in reporting requirements.

In Table A1 we present some statistics to illustrate the differences between DCS and FMS, as well as military aid. Average annual sales through FMS are substantially larger than sales through DCS, and it is also clear that the frequent buyers through the two channels are very different. Traditional allies of the US in areas of strategic importance are the most frequent buyers through FMS, while buyer income and other political characteristics are more important for determining the magnitude of sales through DCS. For further details on DCS and FMS, we refer readers to DISAM (2012).

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[30]DCS and FMS prices are difficult to compare for three reasons. First, purchases through DCS do not incur fees for U.S. government administrative services. Second, weapons procured through DCS often comprise non-standard defense articles which could not be obtained through FMS.

Table A1: Summary Statistics – Average Annual U.S. Weapons Transfers

Panel A:		DCS	FMS	Military aid	Total	Obs.
Full sample		29,572	66,083	9,974	105,628	7,416
		(208,038)	(312,037)	(226,191)	(464,559)	
High income		44,104	95,276	4,541	143,922	4,583
		(257,325)	(381,391)	(64,877)	(502,299)	
Democratic		59,943	91,774	4,420	156,137	2,894
		(318,305)	(282,233)	(69,091)	(476,899)	
Civil freedom		41,457	64,690	13,760	119,908	4,103
		(265,337)	(232,183)	(299,079)	(494,141)	
Ally		42,545	82,605	14,778	139,928	4,554
		(257,874)	(311,148)	(286,995)	(527,216)	
OECD		127,325	184,123	12,783	324,231	1,287
		(462,358)	(318,762)	(118,353)	(627,819)	
Post-Cold War		38,225	70,861	268	109,355	3,236
		(292,286)	(317,048)	(3,633)	(460,555)	

Panel B:	Country	DCS	FMS	Military aid	Total
Top three:	Japan	1,051,956			
	Germany	514,606			
	United Kingdom	443,932			
	Saudi Arabia		2,348,879		
	Israel		1,115,907		
	Taiwan		926,957		
	Vietnam			1,047,830	
	South Korea			246,005	
	Cambodia			99,605	
	Saudi Arabia				2,457,320
	Israel				1,504,490
	Japan				1,482,982
Bottom three:	Tuvalu	0.409			
	Cuba	0.218			
	Equatorial Guinea	0.050			
	Rep. of Congo		0.466		
	Lesotho		0.257		
	Angola		0.024		
	Benin			0.061	
	St. Kitts & Nevis			0.061	
	St. Lucia			0.060	
	Vanuatu				1.006
	Tuvalu				0.409
	Cuba				0.218

Notes: Figures refer to average annual transfers in 1970-2008, reported in thousands of constant U.S. dollars in 2005. Standard deviation in parentheses. Measures of income, democracy, civil freedom, and alliance are described in Appendix A2. High income, civil freedom, and ally, denote observations that are above the annual median in the respective measures. All but 23 countries made at least one FMS purchase during this period, and more than half the countries received zero military aid. The bottom three countries are those with the least positive transfers.

Appendix A2. Other Data

Apart from data on U.S. weapons sales and political violence, we use several other data which we explain here. First, we use per capita GDP figures from Maddison's Historical Statistics. These figures are adjusted to 2005 constant U.S. dollars (in thousands), using consumer price indices published by the U.S. Bureau of Labor Statistics.[31]

We also use the Polity2 variable from the Polity IV Project to measure the degree of democracy. Polity2 is computed by subtracting an autocracy index from a democracy index, and is reported on a scale of -10 to +10, with -10 being strongly autocratic and +10 being strongly democratic. Periods of interregnum are recoded zero (neutral) while a step function is used to recode cases of transition and foreign interruption.[32] For interpretational convenience, we transform the Polity2 to a score between 0 and 1, where scores above 0.5 denote democracies.

We use the civil liberties index from Freedom House to gauge civil freedom. This index is based on 15 questions regarding "the opportunity for individuals to act spontaneously in a variety of fields outside the control of government and other centers of potential domination", and is representative of the degree to which civil liberties are respected. Civil liberties generally refers to particular forms of expression, and should not be confused with physical integrity rights which we use to measure political repression. We transform the raw index to a score between 0 and 1, where 1 indicates the highest degree of freedom.[33] The transformed score can be categorized as $0 - \frac{1}{3}$: "Not Free", $\frac{1}{3} - \frac{2}{3}$: "Partly Free", and $\frac{2}{3} - 1$: "Free".

Finally, we construct a measure of U.S. alliance using UN General Assembly voting data compiled by Voeten (2004).[34] Specifically, we look for vote matches in UN resolutions between the U.S.

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[31]Maddison's GDP figures are also adjusted by purchasing power parity (Geary-Khamis dollars), which is not accounted for by the deflator. Therefore, GDP is not directly comparable to weapons sales, so it is only used as a relative measure of national income across countries and years.

[32]Where foreign interruptions are bounded by a missing Polity2, we recode the entire period to zero.

[33]The raw index takes value between 1 and 7, where 1 indicates the highest degree of freedom. Our transformation involves the following: we first revert the index by subtracting 8 and taking its absolute value. Then we normalize it by subtracting 1 and dividing by 6.

[34]Our measure is similar to that of Qian and Yanagizawa (2009) except that they were specifically interested in whether countries voted with the U.S. or the Soviet Union during the Cold War. We also thought of alternative alliance measures. For instance, the annual World Factbook contains a set of CIA country reports that measures U.S. perception of a country; however, these reports are hardly objective, and do not necessarily represent the views of the Pentagon. There are also measures of political or leadership volatility, such as the International Country Risk Guide;

and a given country, to determine how aligned the country's policies are with those of the U.S., or simply, how much of an U.S. ally the country is. We first construct a dummy for each country-resolution pair that equals one if it matches the corresponding U.S. vote. Although votes could take one of three possible forms ("yes", "abstain", and "no"), we remain agnostic about whether an "abstain" vote represents lower resistance to "yes" than a "no" vote.[35] In other words, we count matches without taking into account the degree of a match. Then, we average these dummy variables over all resolutions in a given year for each country. Our measure contains plenty of heterogeneity across countries and time, suggesting that allies – as defined by UN General Assembly vote matches – are potentially fickle.

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however, these would arguably represent only a subset of U.S. criteria for alliance, and are well-known to be highly correlated with Polity2.

[35]In any case, the overwhelming majority of resolutions are adopted, as voting is highly strategic and countries could well undertake pre-assembly vote-buying, so voting against the resolution is uncommon. A similar pattern can be found in the UN Security Council (Kuziemko and Werker, 2006).

Appendix A3. Simple Model

In this appendix, we propose a simple model in the spirit of Besley and Persson (2009) to rationalize our key empirical finding, that an increase in weapons increases the likelihood of civil war but decreases the likelihood of repression. The model is very "reduced form" in nature, as we leave important micro foundations unspecified, and instead use the theoretical framework to illustrate the class of micro founded models that can rationalize our empirical findings. Specifically, we model the interaction between a state and an aggrieved opposition. The model includes the key elements of Besley and Persson (2009) while introducing asymmetric information about the state's preference for armed violence. The opposition has the option of airing its grievance violently or through peaceful means, and the state can in turn address the grievance of the opposition through violent or peaceful means. The choices of the state and the opposition determine how the grievance is resolved – who gets what share of the pie being fought over. The resolution also depends on state military capability, which augments the state's ability to use political violence in addressing the opposition's grievance. We provide conditions under which an increase in state military capability makes civil war (two-sided violence) more likely where state repression (one-sided violence) would have prevailed otherwise.

The formal model is as follows. There are two players, the "state" S and the "opposition" O. The common value of the subject of grievance is $R > 0$. This can be thought of, for example, as the value of land in a conflict between separatists and the state. The state moves first, and chooses whether to arm or not $\omega \in \{0, 1\}$. Arms come at a cost $c_\omega > 0$. This cost need not just measure the financial cost of purchasing weapons. It may, for example, include "sovereignty" costs a state pays when it buys weapons from the United States. The choice of whether to arm or not is observed by the opposition, who then decides whether to air its grievance through violent or peaceful means. We consider a violent uprising by the opposition to be an "insurgency". Insurgency comes at a cost $m(\omega) > 0$, as insurgents must mobilize an army. We explicitly allow this reduced form cost of mobilization to be a function of state military capacity. In particular we assume that $m(0) \geq m(1)$: it is more difficult for the opposition to mobilize an army when the state has not invested in

weapons.

Once the opposition has made its decision to mount an insurgency or not, the state decides whether to respond with violence. We assume that states that have invested in military capacity pay an additional penalty α when employing violence. This is due to the fact that, for example, civilian casualties are higher and the state and/or the international community values civilian life.[36] We assume that α is unobserved by the opposition at the time of its decision, but known by the state from the beginning of the game.[37] The distribution of α is given by F, and this is common knowledge among the players. We assume the support of α is given by $[0, \alpha_{max}]$

The sharing of R is determined jointly by the choices of the state and opposition of whether to use violence, $v_S \in \{0, 1\}$ and $v_O \in \{0, 1\}$, together with state military capability through the contest function $P(v_S, v_O, \omega)$, which is the share of R that the state obtains, with the opposition obtaining the complementary share.

The payoff functions of the state and opposition are respectively given by:

$$\pi_S(v_S, v_O, \omega) = P(v_S, v_O, \omega)R - \omega c_\omega - \omega v_S \alpha$$

$$\pi_O(v_S, v_O, \omega) = (1 - P(v_S, v_O, \omega))R - v_O m(\omega)$$

We make the following assumptions on the contest function:

$$P(0, v_O, \omega) \leq P(1, v_O, \omega) \tag{4}$$

$$P(v_S, 0, \omega) \geq P(v_S, 1, \omega) \tag{5}$$

That is, the share received by the state is weakly larger (smaller) when the state (opposition) in-

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[36]Of course, the state does not need to use weapons. Our assumption then amounts to requiring that a state which purchases weapons commits to using them when choosing violent response, perhaps because the state cannot perfectly control the use of weapons once violence has been initiated. For example, in some countries, the decision to employ violence or not, and the decisions associated with how to carry out the violence are made by different branches of the state (government and military), and these branches are not perfectly coordinated and have different interests and objectives. Alternatively, we could allow for an armed state to choose among types of violence (with weapons or without). This complicates the model without changing the main message.

[37]Formally, nature chooses the state type α at the start of the game, and nature's choice is observed only by the state.

vests in violence for any choice of the opposition (state) and arms decision. We also make the additional assumption:

$$P(1,1,1)R - \alpha_{max} \geq P(0,1,1)R \tag{6}$$

Assumptions (4) and (6) together ensure that the state always optimally responds to an insurgency with violence – there are no undefended insurgencies. This is consistent with the framework of Besley and Persson (2009). Given that the outcome of undefended insurgency is ruled out, we label the remaining possible outcomes as in Besley and Persson (2009):

- If both sides choose non-violence, the outcome is *peace*.

- If the opposition chooses non-violence and the state replies with violence, the outcome is *repression* (one-sided violence).

- If both sides choose violence, the outcome is *civil war* (two-sided violence).

We now provide conditions under which the model predicts a Separating Perfect Bayesian Nash Equilibrium of the game that is consistent with the empirical findings presented in the paper.

Define two threshold penalties for employing armed violence:

$$\bar{\alpha}_1 \equiv [P(1,1,1) - P(1,0,0)]R - c_\omega \tag{7}$$

$$\bar{\alpha}_2 \equiv [P(1,0,1) - P(0,0,1)]R \tag{8}$$

The first threshold $\bar{\alpha}_1$ determines whether the state prefers armed violence to unarmed repression, while the second threshold $\bar{\alpha}_2$ determines whether the state prefers armed repression to

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armed peace. We further assume that:

$$P(1,1,0) + \frac{m(0)}{R} > P(1,0,0) \tag{9}$$

$$\frac{m(1)}{R} < P(1,0,1) - P(1,1,1) \tag{10}$$

$$P(1,0,0) \geq P(0,0,1) \tag{11}$$

Assumptions (9) and (10) govern the impact of state arming on the mobilization costs faced by the opposition. Together, they imply that the opposition would rather not employ violence if the state represses without arms (because it is more difficult for the opposition to mobilize an insurgent army when the state is unarmed), but would mount a violent insurgency when the state employs armed violence (because the opposition is likely to enjoy popular support in this case). Finally, assumption (11) says that the state receives at least as large a share in unarmed repression as in armed peace; in other words, weapons are not very useful unless violence is employed.

Under the assumptions (9), (10), and (11), there exists a separating PBNE where: a state of type $\alpha < \bar{\alpha}_1$ buys weapons and other types do not.[38] The opposition mounts an insurgency if and only if the state buys weapons, and the state replies with violence regardless of the choice of the opposition.

Thus, when the state does not buy weapons the outcome is repression (one-sided violence) and when the state buys weapons the outcome is civil war (two-sided violence). The intuition that underlies this equilibrium is that only states with the intention of using weapons in facilitating violence against the opposition buy them, so that when the opposition observes the purchase of weapons it is able to infer the state's intentions. Moreover, the prospect of being repressed by an armed state is so unappealing that when the opposition observes the purchase of weapons it mounts an insurgency which leads to civil war. On the other hand, when the state does not purchase weapons, state type plays no role and the parameters of the model are such that (unarmed)

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[38]In particular, assumption (11) necessarily implies that $0 \leq \bar{\alpha}_1 < \bar{\alpha}_2$. Notice that while $\bar{\alpha}_1 \leq \bar{\alpha}_2$ is sufficient for this type of separating equilibrium given the other assumptions, it is not necessary. It simply guarantees that when the state purchases weapons it represses a peaceful uprising: $P(\alpha \leq \bar{\alpha}_2 | \omega = 1) = 1$. We do not need this to be the case, so long as $\bar{\alpha}_2$ is not too small relative to $\bar{\alpha}_1$.

repression is the outcome.

Thus while our model is in many ways quite similar to Besley and Persson (2009), the key departure from their framework that we make – uncertainty about the willingness of an armed state to use weapons in repression, and the role of weapons purchases in signalling willingness to use weapons – is what drives our result. Our other departure from Besley and Persson (2009), that there is a mobilization cost of insurgency, decreasing (in a reduced-form way) in the weapons purchases of the state, is not necessary for there to be a subset of the parameter space where the separating equilibrium we have described to exist, but it makes it more likely, and is a possible channel through which weapons increase the likelihood of war.

Our model predicts that an exogenous decrease in the cost of weapons c_ω decreases the likelihood of repression and increases the likelihood of civil war by making armed civil war more attractive to the state than unarmed repression of the opposition. There is a caveat here, however: that the larger c_ω is the more informative the weapons purchase is as a signal of state type to the opposition, as a larger c_ω means that $\bar{\alpha}_1$ is more likely to be smaller than $\bar{\alpha}_2$. However, our exogenous shifter of c_ω is likely not to cause too drastic a change in the relative values of $\bar{\alpha}_1$ and $\bar{\alpha}_2$, as it only shifts the financial cost of acquiring weapons, not country-specific sovereignty costs which are likely much larger.

Table A2: Foreign Exchange and Inflation

Dependent variable: Trade-weighted U.S. dollar	OLS(1)	OLS(2)
Lagged inflation	-10.390***	
	(1.332)	
Contemporaneous inflation		-10.500***
		(1.276)
N	36	36

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level.

Table A3: Weapons other than DCS

Dependent variable:	log(Total arms import)
Lagged inflation × DCS history	-12.301
	(35.059)
Country & region-year FE	Yes
Democrat × DCS history	Yes
Avg. U.S. economic aid p.c. × year FE	Yes
Avg. U.S. military aid p.c. × year FE	Yes
Avg. U.S. troops p.c. × year FE	Yes
N	5,694

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level.

Table A4: Major Civil War and U.S. Weapons

Dependent variable:	UCDP/PRIO IV(1)	COW IV(2)	MEPV IV(3)
log(DCS)	0.024	0.028	0.011
	(0.028)	(0.038)	(0.085)
First-stage:	log(DCS)	log(DCS)	log(DCS)
Lagged inflation × DCS history	33.796**	33.796**	33.796**
	(15.656)	(15.656)	(15.656)
Cragg-Donald F-statistic	19.88	19.88	19.88
Country & region-year FE	Yes	Yes	Yes
Democrat × DCS history	Yes	Yes	Yes
Avg. U.S. economic aid p.c. × year FE	Yes	Yes	Yes
Avg. U.S. military aid p.c. × year FE	Yes	Yes	Yes
Avg. U.S. troops p.c. × year FE	Yes	Yes	Yes
N	5,694	5,694	5,694

Notes: Standard errors are clustered at the country level. * Indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, *** indicates statistical significance at the 1% level. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.