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# Can arms breed peace? The consequence of arms imports from the US on civil wars

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ABSTRACT

While little evidence sheds light on the positive role of arms, it is still arguably possible for arms imports to reduce conflicts. In this study, we examine whether and how arms imports from the US affect internal conflicts in 135 non-OECD countries. Leveraging a two-way interacted instrumental variable, we exploit a time-series variation of arms supply that arises from the political component of the US Congress and a cross-country variation of arms demand measured as the propensity of purchasing arms. Our analysis reveals that importing US arms exerts a significantly negative impact on the incidence of civil war, particularly in recipients with extreme climate conditions, scarce natural resources, or less diversified socio-demographic structures. We further provide explanations for such a positive role of arms imports by examining the existence of a deterrent effect, the enhancement of public confidence, and the strengthening of state capacity.

## 1. Introduction

According to Fearon and Laitin (2003), in 1999 alone, at least 127 civil wars claimed the lives of 16.2 million people, five times the ongoing interstate toll. What fuels these civil conflicts? The determinants of civil conflicts have been the subject of substantial and expanding economic research and have shown to be important for economic development. Examples include income growth that shifts the potential labor force to violent activities (Besley and Persson, 2009), natural resources from which armed groups can easily extract revenues (Berman et al., 2017), military aid that inadvertently feeds conflicts (Berger et al., 2013; Crost et al., 2014), diversified ethnic groups (Corvalan and Vargas, 2015), fragile organizational structures (Moscona et al., 2020), and historical legacies (Jones and Olken, 2022). These studies concentrate on the perspective of armed groups while neglecting the official government, whose actions may influence the opportunity cost of initiating conflicts. In this paper, we attempt to advance the understanding of the effect of arms inflow on civil conflict by including government-to-government arms transfer.

While it is often the case that military weapons are actually transferred to or sabotaged by local rebels and thus induce more intensive battles, it is still theoretically possible that weapon stocks lead to the deterrence of war under a complete information environment (Chassang and Padró i Miquel, 2010). To explore the effect of arms imports, we look into the US arms officially imported into 135 non-OECD countries between 1972 and 2020. Focusing on the official arms transaction enables us to include the large block of arms trades between governments. There are two main benefits encompassing such governmental arms transactions in a study of arms sales. First, the prevailing political implications involved in arms deals render government-to-government sales a leading channel of arms transfer. It is thus insufficient to focus solely on the effect of the commercial arms trade. Second, unlike

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commercial sales and military aid, government-to-government transactions are less susceptible to potential sabotage or smuggling, consequently bolstering the deterrence power of governments.

A natural strategy to start the analysis is using OLS estimation to examine the relationship between arms imports from the US and civil conflict. However, potential endogenous concerns, such as measurement error and reverse causality, arise with identifying the causal effect using OLS. To overcome these identification issues, we employ an instrumental variable strategy that exploits two sources of variations. First, given that the political stand of the Republicans is inclined towards supporting military sales, we exploit a time-varying Republican seat ratio in the US Congress to proxy for arms supply in the following year. The exogenous nature of the Republican seat ratio mainly relies on the fact that US citizens almost base their voting decisions on domestic affairs, considering the salience of domestic issues during election campaigns and the confidential nature of arms transactions documents (Albornoz and Hauk, 2014). To mitigate the concern that Republican preference may affect civil conflict through channels other than arms trade, such as military aid or political alignment, we control for these variables and find no influence on our conclusion. Second, similar to Nunn and Qian (2014), we introduce country-specific arms demand measured as the proportion of years in which a country purchases US arms. By combining both the supply and demand variations, we are able to estimate the causal effect of arms imports on the incidence of civil war.

We assemble a rich dataset from various sources to construct our variables of interest. The dependent variable, the incidence of civil conflict, is an indicator that equals one if a country in a year experiences one or more civil conflicts that cause at least 25 battle deaths and equals zero otherwise. Our key independent variable is the annual imports of the US arms claimed by local customs of a non-OECD country to the UN Comtrade from 1972 to 2020. We find that countries importing more arms were subsequently less likely to experience civil wars. The baseline estimate is statistically significant and economically meaningful. For a country that imports arms at the sample mean (i.e., 4,677,361 US dollars), a 1% increase in arms imports would lead to an 8.7% decrease in the incidence of civil war. The baseline result remains stable when we adopt alternative measurements, specifications, and sample selections. To extend the baseline result to more scenarios, we perform numerous heterogeneity analyses. The results suggest that the effect of the US arms majorly stems from countries with extreme climate conditions, scarce natural resources, and less diversified ethnic and religious populations. Additional evidence suggests that global involvement, such as international trade and political alignment, induces a larger decline in civil war.

To understand how arms imports from the US can reduce the incidence of internal conflicts in under-developing nations, we propose and verify three hypothesized channels motivated by prior studies. First, considering that major conventional weapons (MCW) account for the major portion of US arms transactions, importing US arms by local authorities generates a strong deterrent effect on conflict groups. Second, arms imports significantly enhance public confidence in official armed forces and military capabilities. In addition, the imports of US arms entail the transfer of intelligence and technology in weapon manufacturing and operation, leading to the improved provision of public goods, enhanced economic development, and reduced incentive of opposing groups to initiate conflicts.

Our findings contribute to a better understanding of the military-related motives of conflict. While most studies find an unpleasant effect of military infusion on receiving countries' stability (e.g., Dube and Naidu, 2015; Crost et al., 2014; Magesan and Swee, 2018; Pamp et al., 2018; Garfinkel et al., 2020; Auer and Meierrieks, 2021), such findings are commonly confined to specific scenarios. In specific to arms imports, a recent and closely related study by Magesan and Swee (2018) investigates the association between commercial weapon purchases and political violence in purchasing countries. Our findings complement this line of research by taking into account government-to-government sales to eliminate the potential concerns on smuggling problems in commercial arms sales, which result in biasing the real effect of arms transactions.

One step further, in comparison to Magesan and Swee (2018) and Pamp et al. (2018), our study offers alternative explanations for the effect of arms imports on civil conflicts. Except for the typical analysis of arms' influence on the outbreak, the duration, and the offset of civil conflicts, we explore and emphasize the deterrent and signaling effect induced by arms imports, particularly the imports of major conventional weapons. Our findings also leverage the positive spillovers from a series of post-sales maintenance and service following government-to-government sales, which could strengthen recipient countries' state capacity, and in turn, alleviates the presence of weak institutions and reduce the likelihood of conflicts.

Our study also speaks to the established research regarding the effect of foreign political influence on civil conflict (e.g., Anderton, 1995; Gleditsch, 2007; Easterly et al., 2008; Aidt and Albornoz, 2011; Antràs and Padró i Miquel, 2011; Nielsen et al., 2011; Berger et al., 2013; Nunn and Qian, 2014; Albornoz and Hauk, 2014). While these studies show a considerable impact of political intervention and foreign aid in under-developing countries, they pay little attention to the role of arms transactions as a foreign intervention. Our analyses shed light on the role of arms sales as a pathway to cast foreign political influence by linking the change in the US ruling party to its arms sales.

The remainder of this paper proceeds as follows. Section 2 provides a brief description of the background. Section 3 describes the data and the empirical strategy. Section 4 presents the estimation results. Section 5 presents a further analysis of the mechanisms and heterogeneity. The final section concludes and discusses the implications.

## 2. Background

### 2.1. The US arms sales

The share of US arms sales in the global market has been increasing since the 90s (Anderton, 1995). Most recently, the average market share of US arms reached nearly 40.0% from 2018 to 2022, showing a 14% increase compared with the average in the last

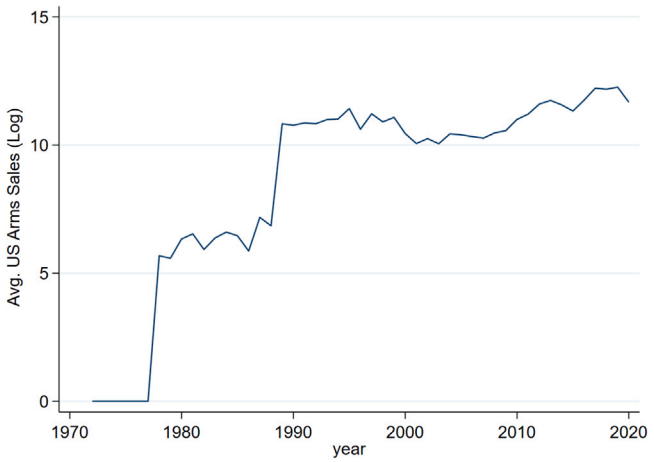


**Fig. 1.** Growth of US arms sales. *Note:* This figure plots the annual US arms sales (in logarithm) to the sample countries between 1972 and 2020.

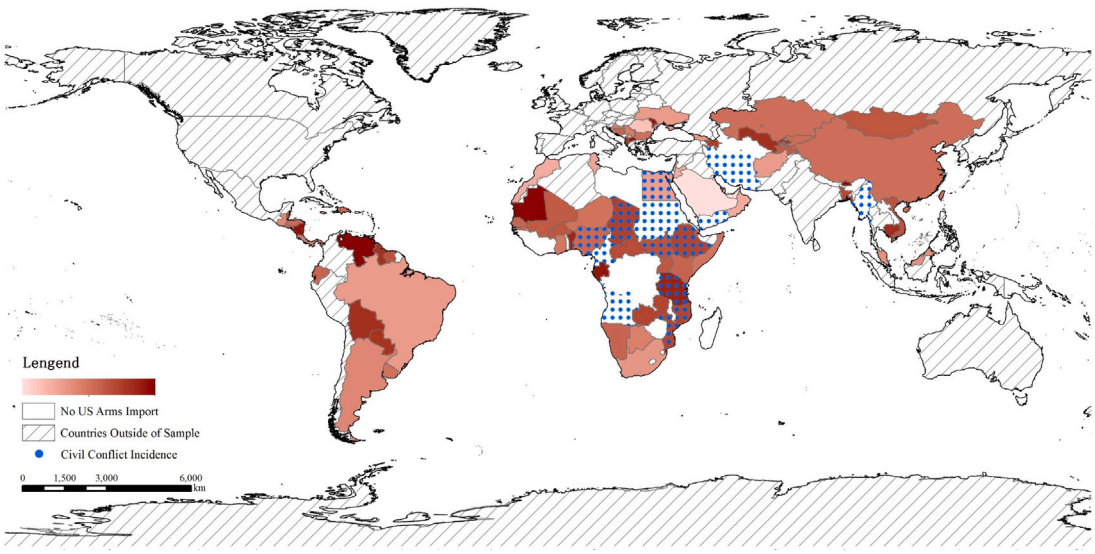


**Fig. 2.** Geographical distribution of arms imports from the US and civil conflict. *Note:* This figure plots the worldwide distribution of arms imports from the US and civil conflict, with a darker color indicating a larger amount of arms imports and the blue dot indicating whether there are one or more incidences of civil conflict in the corresponding country. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

five years (Wezeman et al., 2023). Undoubtedly, with an essential rise over the past 40 years, the United States is the top influential arms exporter in the global market. In general, US arms sales experienced an increasing trend with a drastic upward jump in the 90s because of the Gulf War, as illustrated in Fig. 1. Between 1972 and 2020, an average country purchased weapons totaling up to 170 million USD, with 18.9% of these imports from the US.[1] The recipients are mainly distributed in East Asia, Africa, and South America, as depicted in Fig. 2. In our sample, the top three recipients of US weapons are Saudi Arabia, the United Arab Emirates, and Egypt (in descending order of arms imports from 1972 to 2020), accounting for 68.6% of total transaction volumes. Fig. 2 further shows that more arms imported from the US are linked to fewer disputes and violence. However, as revealed in the figure as well, governments might anticipate domestic clashes and subsequently intend to increase arms imports. Therefore, reverse causality is one of the biggest concerns we have to address when estimating the effect of arms imports on civil conflicts.

[1] A summary of US arms sales in the global market can be found in Table A1. As a major type of US arms, the major conventional arms imported from the US account for 18.8%.

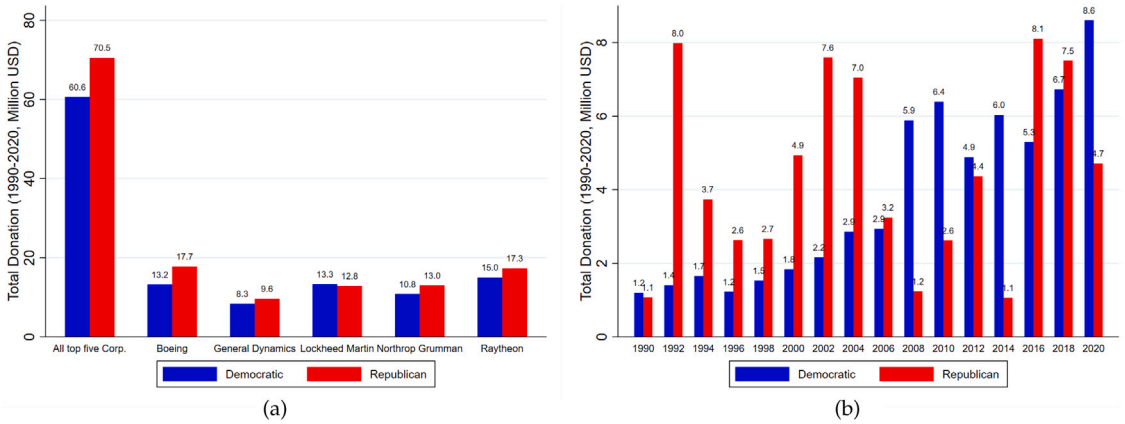


**Fig. 3.** Political donations of Top 5 arms merchants. *Note:* This figure plots the political donation of the top 5 arms merchants. Fig. 3(a) plots the total donation of each top 5 arms merchants to either Democratic or Republican from 1990 to 2020, shown by the blue and red bars, respectively. Fig. 3(b) plots the total donations from the top 5 arms merchants to either Democratic and Republican each year, shown by a blue bar and red bar, respectively. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

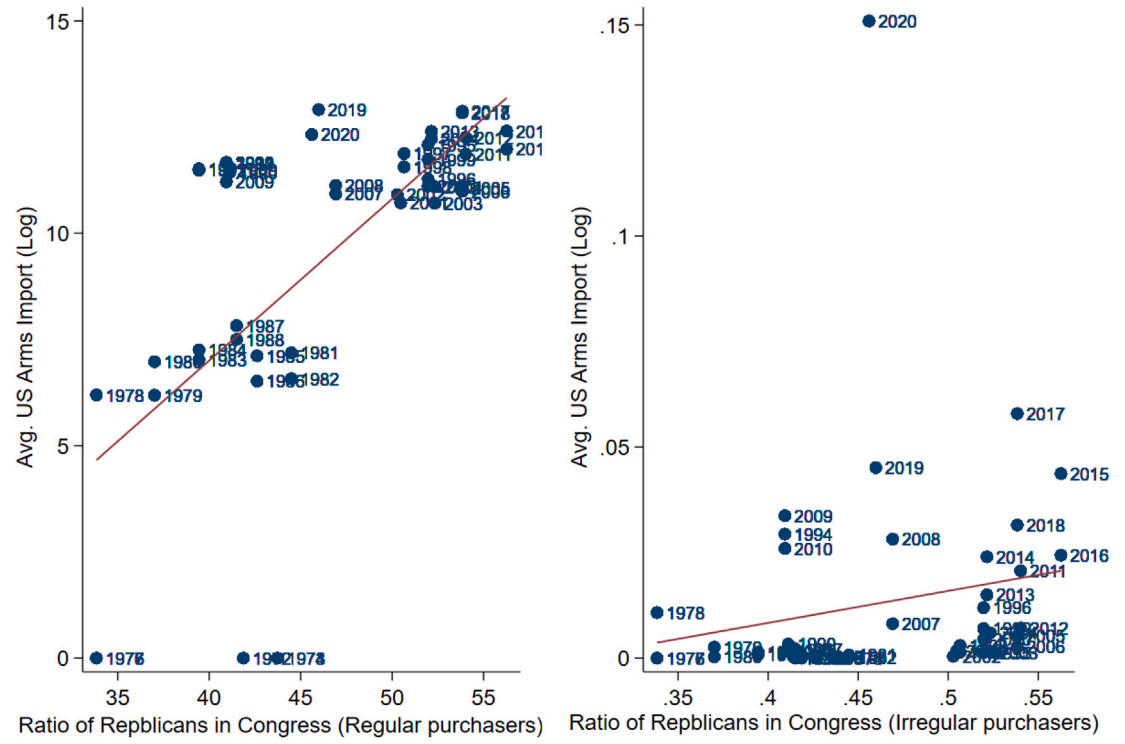


**Fig. 4.** Arms imports from the US and the seat ratio of Republicans in Congress. *Note:* This figure plots the correlation between the arms imports from the US and the ratio of Republicans in Congress. The left-hand-side figure plots the correlation for regular buyers whose average number of years importing US arms is above the sample mean. The right-hand-side figures plot the correlation for irregular buyers whose average number of years importing US arms is below the sample mean.

The US arms are purchased by other foreign countries through either foreign military sales (FMS) or direct commercial sales (DCS).[2] As displayed in Table A2 listing the composition of arms imported from the US, a considerable proportion of arms imports from the US are major conventional weapons (MCW) (78.2%), which are primarily transferred through FMS (Fearon and Laitin,

[2] According to the Department of Defense, FMS is the US government's program for transferring defense articles to other nations and international organizations, while DCS refers to the direct sale of defense articles between the US industry and foreign buyers, which is not administered through Foreign Military Sales

2003). Instead, small and light weapons (SALW), as well as cold weapons, are mainly transferred via commercial sales (Pamp et al., 2018).[3] While commercial military weapons face a high risk of being transferred to or sabotaged by local rebels and thus complicate the evaluation of arms' effect, FMS is relatively less vulnerable to such risks (Dube and Naidu, 2015). Additionally, the inclusion of FMS indicates that our analyses primarily capture the effect of MCW, a major category of weapons that are most capable of improving governments' military deployment in wars. Therefore, unlike the widespread view that military transfer drives conflicts, government-to-government arms sales offer potentially distinct mechanisms for the effect of US arms inflow on civil conflict.

### 2.2. Congress's role in US arms sales

Upon the enactment of the Arms Export Control Act (AECA), US Congress secured its role in the process of arms transfers to foreign countries. Initially, the role of the US Congress in arms sales pertained to the right of information notification. Specifically, any proposals of FMS or DCS were required to be formally notified to Congress 30 calendar days before the Administration can issue a formal offer or an export license if the value of an arms transaction exceeds a particular threshold (Pierre, 1982).[4] Along with the rise of US arms sales globally and a lack of efficiency of the executive branch in dealing with the decision-making of arms sales, Congress became increasingly interested in scrutinizing arms transfers and imposing more prohibitions on arms sales that violate human rights (Pierre, 1982). The power of Congress includes not only the use of a joint resolution of disapproval but also the opportunity to explicitly oppose a certain arms transaction so as to affect the timing and composition of arms sales or dissuade the President from continuing to propose the transfer. During the resolution procedure, both House and Senate would hold committee hearings to vote for the arms sales proposal submitted by opponents. If a majority of the House or the Senate support the joint resolution of disapproval, efforts would be made to seek additional consideration for the arms sales proposals.[5]

### 2.3. The ideology difference in arms sales

Given the important role of Congress in determining US arms sales, decisions on military sales could become intricately intertwined with the partisan difference in Congress. The US Congress itself consists of the House and the Senate, where proposed bills are voted to be passed. The party that dominates the Congress seats can be plausibly more decisive for where the proposed bills end. Table A3 provides historical records of the majority party in the House and the Senate, as well as the corresponding arms sales during our sample period. As expected, the Democrats and Republicans become the majority in Congress in different periods, corresponding with fluctuations in arms sales over the years. Then a natural question is why there is an association between partisanship and arms sales. It is widely acknowledged that the two-party system is characterized by a distinction in the ideology of being Liberal and Conservative. Generally speaking, liberals advocate for progressive reforms that aim to achieve greater social equality, a stance reflecting the political attitude of most Democrats. Conversely, the conservatives tend to support private ownership, free enterprise, and limited changes in traditional values, traits that commonly shape the majority of Republicans (Delaet and Scott, 2006; Dueck, 2010; Barber and Pope, 2018).

Such a partisan difference in ideology has been closely linked to arms sales. Relative to the Democrats, the Republicans are more inclined to support businesses involving weapon production and exports. This partisan difference in ideology could be traced back to the 1980s. The attitudes of the Democrats towards arms sales were anchored with the beginning of Carter's policy (Pierre, 1982). As a representative of the Democrats, President Cater exploited arms sales to other nations as an "exceptional" instrument of foreign policy and formally issued a pledge to curtail arms sales in case the proliferation of arms causes human rights abuses abroad. The Republican doctrine, in contrast, is distinctive in emphasizing its military might in foreign policy and "hawkish and intense American nationalism" (Dueck, 2010). The Republican President Reagan, as a typical example, saw the transfer of conventional arms as an essential element of the US global defense posture and an approach to safeguard the party's economic interests, marking the explosion of Republicans' permissive attitudes towards arms sales. The Republicans' ambition to expand the US arms export, to a great extent, also reflects the positions of the major arms merchants who share the same ideology regarding weaponizing the country with the Republicans (Pierre, 1982). Maintaining Republican seats in the US Congress as much as possible secures the odds of expanding the use of arms sales as an instrument to achieve the goal of militarized ideology.

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procedures but required an issued license. Both types of arms sales are reported by national customs, but FMS dominates the transaction volume of the US arms trade.

[3] MCW encompasses various equipment, including bombs, grenades, mines, missiles, and other armored combat vehicles. SALW includes pistols, rifles, gas guns, assault rifles, and other forms of guns. Small arms are intended for individual use, while light weapons are designed for a crew of around two people.

[4] The notification requirement is applied to major defense equipment valued above $14 million, defense articles or services valued above $50 million, or design and construction services valued above $200 million.

[5] Details for this procedure could be found in CRS (2022).

### 2.4. The Republicans' economic ties with the defense industry

The Republican philosophy of favoring military capability has been further reinforced by the growing ties between capitalized military industry and the Republicans due to personal economic interests (Weingast and Marshall, 1988; Delaet and Scott, 2006). Anecdotal evidence shows that members of Congress could be economically connected with defense companies through stock shares. As of 2021, 36 House members hold stock in military contractors such as Lockheed Martin and Raytheon, with 20 being Republicans and 16 being Democrats. The Republicans in House own defense stocks worth over $1 million more than their Democratic counterparts.[6] Meanwhile, the arms industry tends to skew towards the Republican party as arms merchants spend millions on lobbying and donating to the political campaigns of Republican politicians. We track political donations made by the top 5 US arms merchants between 1990 and 2020 and plot the total amount given to the Democrats and Republicans, respectively, as shown in Panel (a) of Fig. 3. Four out of five arms merchants financially contribute more to the Republicans, showing that they share more common ground with Republicans regarding arms transactions. Panel (b) of Fig. 3 further displays a time trend of total donations made by these top 5 merchants between 1990 to 2020, indicating that these leading arms merchants hold a stronger political preference for Republicans than Democrats.[7]

The underlying alignment between Republicans and US arms industry regarding economic concerns can subtly sway Republicans' decision in Congress towards backing up arms transactions.[8] Therefore, with a high ratio of Republican members in the US Congress, a bill favored by the Republicans is more likely to receive support in Congress, eventually serving the interest of arms merchants and Republicans themselves (Feinstein, 2011; Farah and Braun, 2007).

## 3. Data and empirical strategy

### 3.1. Data

Data on our main outcome of interest, the incidence of civil conflict, is collected from the UCDP/PRIO Armed Conflict Dataset. This dataset offers annual records for conflict episodes from 1946 to 2020. In this study, we limit our attention to civil war, a type of political violence that has been widely studied.[9] The specific measure for the dependent variable is a dummy which equals one if a country in a given year experiences one or more civil conflicts and equals zero otherwise.[10] After excluding countries that stay in peace over the years, we obtain 6,615 country-year observations in total. On average, the frequency of civil wars reaches 9.5% with a standard deviation of 0.293. For further analysis of underlying mechanisms, we also extract information on the onset of civil conflict and the peace duration between any two war episodes of conflict. Moreover, we supplement our conflict data with The Armed Conflict Location & Event Data Project (ACLED), which provides an alternative sample to test the robustness of our results.

The data on arms imports from the US are compiled from the UN Comtrade database, which provides transaction records that are officially reported by local customs to the UN Comtrade. We construct our independent variable by adding up the values of arms purchased by each country in each year using the HS92 code (93).[11] From the same database, we also obtain data on other products imported from the US.

We collect data on the seat ratio of two parties in the US Congress from Nick Hillman's Party Control data to construct the measures for the instrument variable in the baseline and robustness checks. The control variables, including GDP per capita, population, primary education attainment, monthly average temperature, and precipitation, are collected from World Bank. The data on military aid is exclusively collected from the US Agency of International Development. We also gather data on the degree of democracy in each country from Freedom House, the political alignment with the US from the Erik Voten Dataset, and ethnic and religious-related metrics from the Ethnographic Atlas and World Religion Review.[12]

To provide a formal sense of the data, Table 1 presents a summary of statistics for the key variables we cover. The average logarithmic value of arms imports from the US of our sample is 3.468, with a standard deviation of 4.304. A typical country in our sample made official purchases of US weapons for a period of more than 20 years between 1972 and 2020. Notably, some countries, like Eritrea, have never purchased any US arms, whereas others, such as Saudi Arabia, Argentina, Egypt, and the United Arab Emirates, have made positive purchases of US arms for at least 43 years.

[6] See https://disclosures-clerk.house.gov/PublicDisclosure/FinancialDisclosure.

[7] The data of political contribution of the five major US arms merchants are manually collected and compiled from https://www.opensecrets.org/.

[8] Additional worries arise that it might be Congress members' economic concerns of their representative states or districts rather than their party connection with the arms industry that drives the US arms sales. To further examine this possibility, we provide placebo tests in Section 4.2.3.

[9] By definition, civil war refers to battles between the government and an opposite domestic group that results in at least 25 deaths per battle in a year.

[10] Summary statistics show that among countries experiencing at least one civil conflict, only 20.5% of them experience more than one civil war, while the share of countries with only one civil war approaches 79.5%. Therefore, the concern that a binary denotation would lose many original variations is negligible.

[11] The values of arms imports are deflated to 2010 constant USD and transformed to logarithm forms. In Section 4.3.1, we also explore alternative measures of the independent variable including the IHS-transformed arms imports and arms imports weighted with population and GDP.

[12] Detailed information on definitions and data sources for each variable can be found in Table A4.

**Table 1**
Summary statistics for main variables.

| | count | mean | sd | min | max |
|---|---|---|---|---|---|
| ***Panel A: Main variables*** | | | | | |
| Civil Conflict Incidence | 6615 | 0.095 | 0.293 | 0 | 1 |
| Arms Imports from the US (log) | 6615 | 3.468 | 4.304 | 0 | 16 |
| The Propensity of Arms Import | 6615 | 0.439 | 0.283 | 0 | 1 |
| The Ratio of Republican Seats | 6615 | 0.457 | 0.066 | 0 | 1 |
| ***Panel B: Other variables*** | | | | | |
| GDP Per Capita (log) | 6615 | 3.225 | 1.217 | 0 | 7 |
| Population Size (log) | 6615 | 14.996 | 2.034 | 9 | 21 |
| Military Aid (log) | 6615 | 9.171 | 2.576 | 3 | 19 |
| Other Imports from the US (log) | 6615 | 11.861 | 5.694 | 0 | 21 |
| Monthly Average Temperature | 6615 | 21.808 | 6.606 | −1 | 29 |
| Monthly Average Precipitation | 6615 | 105.075 | 76.522 | 1 | 453 |
| Democracy Level | 6615 | 0.461 | 0.351 | 0 | 1 |
| Primary School Attainment (%) | 6615 | 21.408 | 21.937 | 0 | 100 |

*Note:* This table provides summary statistics for our main variables. The unit of observation is at the country-year level. The time window is from 1972 to 2020. Panel A summarizes the main independent and dependent variables. "Civil Conflict Incidence" is a dummy that equals one if a civil conflict occurred in a given country in a year. "Arms Imports from the US" is measured by the logarithm of arms imports from the US (in a million USD). "The Propensity of Arms Imports" is measured by the average number of years a country imports arms from the US. "The Ratio of Republican Seats" is measured by the proportion of Republican seats in the US Congress over the years. Panel B summarizes the control variables. "GDP Per Capita (log)" is the logarithm of GDP per capita. "Population Size (log)" is the logarithm of the population scale. "Military Aid (log)" is the US military aid amount. "Other Imports from the US (log)" is measured by the total amount of imported goods from the US except arms. "Monthly Average Temperature" and "Monthly Average Precipitation" are measured by the average temperature and precipitation in a given country yearly. "Democracy Level" indicates the level of citizens' freedom in a country. "Primary School Attainment (%)" is the primary schooling attainment for the age group over 25 (%).

### 3.2. Empirical strategy

We first examine the correlations between arms imports from the US and the incidence of civil war in recipient countries using OLS.

$$y_{ct} = \beta Arms_{ct} + X'_{ct}\gamma + \delta_c + \lambda_t + \epsilon_{ct} \tag{1}$$

where $y_{ct}$ is a dummy variable indicating whether there is one or more civil war in country $c$ in year $t$. The key independent variable $Arms_{ct}$ measures the (log) volumes of arms imported from the US. $X_{ct}$, a vector of country characteristics, control for the (log) GDP per capita, (log) population size, (log) military aid, (log) other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary education attainment.[13] By including year-fixed and country-fixed effects, we further control for country-specific while time-invariant characteristics, as well as time-variant features common to all countries.

The key coefficient of our interest in specification (1) is $\beta$. Without any identification, specification (1) shows whether importing US arms correlates with the incidence of civil war. Since our aim is to establish a causal relationship between the above two, we have to address the potential endogeneity of arms imports that undermines the causal explanation. That is, there exist unobserved factors related to arms that also explain parts of the civil war in the country $c$. Therefore, the estimated $\beta$ will not be asymptotically consistent. In addition, the measurement error may result in an under-reported value of arms sales, thus driving the absolute value of the estimate downward.[14] Except for underlying factors that researchers cannot observe and measurement error of arms sales, the estimation in specification (1) can also be biased because of reverse causality. Multiple waves of civil wars raise a government's demand for arms over the years, which drives the absolute value of the estimate upward. All the above endogenous issues in OLS estimation may bias the OLS estimate in directions that are ambiguous ex ante.

To obtain a consistent estimation of the effect of arms imports, we apply an identification strategy that utilizes a two-way interaction term as the instrumental variable for endogenous arms imports. Specifically, we exploit the one-year-lagged Republican seat ratio in the US Congress to measure the arms supply directly or indirectly regulated by the US government.[15] The demand for arms, on the other hand, is captured by a country's propensity of purchasing US arms during sample periods.[16]

Based on the above instrumental estimation strategy, the two-stage equation is given as follows:

$$Arms_{ct} = \alpha(Seat_{t-1} \times D_c) + X'_{ct}\gamma + \delta_c + \lambda_t + \mu_{ct} \tag{2}$$

[13] The variable democracy is measured as an index ranging from 1 to 7, in which a higher value of the variable corresponds to a greater level of democracy.
[14] For instance, the arms import data submitted by countries' customs to the UN Comtrade may not precisely capture the true value of arms sales.
[15] The use of the one-year lagged seat ratio is based on the consideration that it takes time for Congress to make a decision on arms sales.
[16] We employ different possible proxies to construct instrumental variables in the robustness check. For instance, military expenditure can also be one of the possible measurements for arms demand (Bove and Gavrilova, 2017). We report the result using these alternative instruments in Section 4.2 and the effect of arms imports is consistent with the baseline estimate.

**Table 2**
Baseline results.

| | (1) OLS | (2) OLS | (3) OLS (lasso) | (4) 2SLS | (5) 2SLS |
|---|---|---|---|---|---|
| Arms Imports from the US (log) | −0.006*** | −0.009*** | −0.009*** | −0.026*** | −0.025*** |
| | (0.002) | (0.003) | (0.003) | (0.010) | (0.009) |
| GDP Per Capita (log) | | | −0.022* | | −0.002 |
| | | | (0.013) | | (0.015) |
| Population Size (log) | | | 0.037 | | 0.058* |
| | | | (0.028) | | (0.032) |
| Military Aid (log) | | | 0.002 | | 0.004 |
| | | | (0.004) | | (0.004) |
| Other Imports from the US (log) | | | 0.001 | | 0.002 |
| | | | (0.002) | | (0.002) |
| Monthly Average Precipitation | | | 0.015 | | 0.023** |
| | | | (0.011) | | (0.012) |
| Monthly Average Temperature | | | 0.000 | | 0.000 |
| | | | (0.000) | | (0.000) |
| Democracy Level | | | −0.046 | | −0.030 |
| | | | (0.035) | | (0.036) |
| Primary School Attainment (%) | | | −0.001 | | −0.001* |
| | | | (0.001) | | (0.001) |
| Observations | 6615 | 6615 | 6615 | 6615 | 6615 |
| Kleibergen-Paap F test | | | | 218.097 | 192.856 |
| Olea-Pflueger test | | | | 218.097 | 192.856 |
| Durbin-Wu-Hausman test | | | | 17.762 | 14.928 |
| Year fixed effect | No | Yes | Yes | Yes | Yes |
| Country fixed effect | No | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. The dependent variable across all columns is a dummy that equals one if a country experiencing a civil conflict in a given year. The key independent variable is the logarithm of a country's arms imports from the US. In column 4 and column 5, the instrument for arms imports from the US is an interaction term between a country's propensity to import US arms (time-invariant) and the one-year lagged seat ratio of Republicans in Congress. The controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table 3**
First stage of IV estimation and reduced form estimation.

| | First stage | | Reduced form | |
|---|---|---|---|---|
| | (1) Arms imports | (2) Arms imports | (3) Civil conflict | (4) Civil conflict |
| Seat * D | 37.699*** | 37.306*** | −0.964*** | −0.936*** |
| | (2.508) | (2.637) | (0.358) | (0.333) |
| Observations | 6615 | 6615 | 6615 | 6615 |
| Baseline controls | No | Yes | No | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. Column 1 and column 2 provide the first-stage estimates, while column 3 and column 4 present the reduced form estimates. The dependent variable in column 1 and column 2 is the logarithm of a country's arms imports from the US. The dependent variable in column 3 and column 4 is the incidence of civil conflict in a given country in a given year. "Seat * D" is an interaction term between a country's propensity to import arms from the US (time-invariant) and the one-year lagged seat ratio of Republicans in Congress. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

$$y_{ct} = \beta A\hat{rms}_{ct} + X'_{ct}\gamma + \delta_c + \lambda_t + \epsilon_{ct} \tag{3}$$

$$D_c = \frac{1}{49}\sum_{t=1972}^{2020}(D_{ct}) \tag{4}$$

where Eq. (2) is the first stage estimation. $Seat_{t-1}$ is the ratio of Republican seats in the US Congress in the year $t-1$. $D_c$ is the average frequency of arms imports by country $c$ over the sample period, in which $D_{ct}$ is an indicator that equals one if a country $c$ imports arms from the US in year $t$ and zero otherwise. Similar to a difference-in-difference (DD) estimation strategy, the instrumental variable, $Seat_{t-1} \times D_c$ captures the differential effect that the Republican seat ratio had on the US arms sales in recipients with a high demand for the US arms relative to recipients with a low demand. Fig. 4 explains our estimation strategy by showing the differentiated relationship between the Republican seat ratio and arms imports in two groups with different levels of arms demands. To this end, we can eliminate confounding factors that mix the baseline estimation.

The way of constructing an instrumental variable for arms imports from the US underlies the identification strategy of various research in the arms sales and conflict literature. For instance, to investigate the impact of commercial arms sales on civil conflict, Magesan and Swee (2018) use an interaction between a plausibly exogenous shift in the US inflation rate and a country's propensity to purchase US arms as the instrument for US commercial arms sales. A similar instrumental design is also applied by Ahmed (2016), who uses the interaction between a plausible exogenous variation in party fragmentation and a country's tendency to receive aid from the US as the instrument of US foreign aid. Both Magesan and Swee (2018) and Ahmed (2016) control for either the interaction between the propensity component and some time-varying factors or the interaction between the shift component and the country fixed effects to alleviate concerns on the exclusion restriction of the instrument.[17]

Overall, our IV strategy uses a Bartik-type instrument that relies on two prior assumptions. First, the instrument should be significantly connected with the endogenous independent variable (i.e., the relevance condition). The seat ratio of Republicans in the US Congress is strongly correlated with the arms imports from the US as shown in Figure A1.[18] We adopt the Stock-Yogo test and Olea-Pflueger test to check whether our instrument explains a significant portion of the endogenous independent variable in Section 4.1. Meanwhile, we run a Monte Carlo simulation developed by Christian and Barrett (2017) and undertake an ADF test to exclude the potential that the instrument suffers from spurious correlation with the civil conflict incidence.

Second, our instrument should affect the dependent variable only through the arms imports conditional on the baseline controls $X_{ct}$ rather than the unobservable $\epsilon_{ct}$ in specification (1) (i.e., the exclusion restriction). The exclusive nature of our instrument is implied by the features of the US political system. For one thing, the independence of Congress has been protected by the US Constitution (The U.S. Constitution Intro 6.2.2). For another, when making voting decisions for seats in Congress, US citizens tend to give all their priority to domestic issues while leaving none to issues in other countries.[19] Hence, the time-varying seat ratio of Republican in Congress are exogenous to changes in other countries. Yet, using endogenous arms imports to construct the share component would still cast endogenous concerns on the instrument. Following Goldsmith-Pinkham et al. (2020), Adão et al. (2019), and Magesan and Swee (2018), we employ a series of placebo tests and examinations to support the validity of instrumental estimation in Section 4.2.

## 4. Empirical results

### 4.1. Baseline results

#### 4.1.1. OLS estimates

We start with our analysis by showing a simple OLS estimation. Column 1 of Table 2 shows a negative correlation between arms imports and the incidence of civil war. In column 2 and column 3, we consider country-fixed and year-fixed effects and a series of time-variant country characteristics. Regarding the choice of country characteristics, we adopt a machine learning technique, least absolute shrinkage, and selection operator (LASSO) to mitigate the omitted variable problem and optimize the set of covariates correlated with civil conflict.[20] Comparing the estimates across column 1 to column 3, the estimated coefficient only increases slightly. The estimates of column 2 and column 3 indicate that the statistically negative relationship between arms imports and civil war is not affected by the inclusion of additional controls.

[17] The estimation with these additional controls would not undermine our main conclusion. These estimation results are upon request.

[18] Note that a country's propensity to purchase US arms is computed using arms imports data and thus by nature correlated with arms imports from the US.

[19] Previous studies suggest that non-government citizens are struggling to access official documents or reports on arms transfers made by executive branches (Echols, 2022). Lewis-Beck et al. (2008) also highlights the importance of domestic issues in determining the voting behavior of Americans.

[20] Specifically, we select the potential covariates in three steps based on a double-LASSO estimation. First, we fit a lasso regression predicting the incidence of civil conflict and keeping track of all the possible covariates with non-zero estimated coefficients. Second, we fit a lasso regression predicting the focal arms imports and keeping track of the above covariates with non-zero estimated coefficients. Finally, we fit an OLS regression of the civil war on the focal arms imports with a penalty based on a "plug-in" iterative formula, including all the covariates selected in the above procedures (Belloni et al., 2014; Urminsky et al., 2016).

#### 4.1.2. 2SLS estimates

Column 4 to column 5 of Table 2 display the outcomes using 2SLS estimation. The key coefficient, $\beta$, is significantly negative at the 1% level. According to the estimate using all sets of controls in column 5, a 1% increase in arms imports from the US decreases the likelihood of civil war in receiving countries by 2.5%.[21] Comparing the OLS estimate in column 3 with the 2SLS estimate in column 5, the absolute value of the key coefficient increases from 0.009 to 0.025, suggesting a downward bias in the OLS estimate.[22] Following Nunn and Qian (2014), we further assess the magnitude of this result by using the mean of arms imports from the US in logarithm (3.468) and the estimate from column 5. We arrive at a magnitude that for a country without any US arms purchases, an average increase of arms imports from the US leads to a reduction in its civil conflict incidence by 8.7%(0.025*3.468).[23]

The contradictory estimated effect of arms imports is noteworthy given that a study by Magesan and Swee (2018) finds the effect of US arms on civil conflict to be opposite to ours. For one thing, the estimated effect partially relies on the rationales for constructing the instrument. Our instrument exploits a partisan affiliation in the US Congress and captures the demand variation and supply shift in US arms sales. Magesan and Swee (2018), however, focus on demand variations by using the market inflation rate.[24] For another, the estimated effect could be different due to the arms transferring channel and type. Our study takes into account the major portion of US arms that are transferred via the FMS, which offers a series of post-sale services. Moreover, the MCW is majorly transferred through government-to-government sales, whose influence is commonly characterized by a strong deterrence and signaling effect. Both of these advantages in FMS lead to a decline in civil conflicts in recipients, as summarized by Pamp et al. (2018).

#### 4.1.3. First-stage and reduced-form estimates

We rely on estimates from the first stage and two additional tests to assess whether the instrument we use to isolate the exogenous variation in arms imports can explain major changes in this key variable of interest. The corresponding results can be found in column 1 and column 2 of Table 3. The estimates on the instrument across all specifications are statistically positive and significant at the 1% level, showing a strong positive correlation between the seat ratio of Republicans in Congress and arms imports. Recall the weak instrument test proposed by Stock and Yogo (2005), the Kleibergen-Paap F statistics in all specifications significantly pass the threshold value when allowing 10% bias in our 2SLS estimates, as shown in Table 2. The Olea-Pflueger test statistics (218.097; 192.297) are significantly (1% level) larger than the critical value.[25] The time-varying seat ratio is also confirmed to be non-stationary using the standard unit root test at the 5% level. We further provide corresponding reduced-form effects of our instrument on the outcome of interest in column 4 and column 5, showing that the effect of the instrumental variable on civil war is negative and significant at the 1% level. Overall, our instrument is immune to the weak instrument problem.

Two concerns may blur the exclusive restriction of our instrument. First, allies of the US tend to import relatively more arms from the US no matter if the Republican is in control of Congress or not; Second, we may unjustifiably ascribe the effect of arms from other countries on reducing civil war to the effect of US arms (Magesan and Swee, 2018). To mitigate the first concern, we calculate the fraction of votes from the arms-purchasing countries that are in line with those of the US in the UNGA and include it in the estimation as a proxy for political alignment with the US, as shown in column 3 of Table A6. We further control for arms imports from China and Russia, the other two major players in the global arms market, in column 4 of Table A6. Neither of the two results weakens the main effect identified by our instrumental estimation.

### 4.2. Potential concerns with the validity of the instrument

#### 4.2.1. Concerns with the exogeneity of the share component

In this section, we discuss the validity of our instrument in light of recent studies that focus on the shift-share approach of instrumental variables (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2020). Given that we use an endogenous variable to construct the "share" component, we must carefully tackle the potential concerns arising from the correlation between this "share" and the unobservables, which could ultimately bias the estimate. Specifically, we provide evidence on the validity of our share component by offering a placebo test and supplementary evidence.

In the placebo test, similar to Magesan and Swee (2018), we use different periods to construct the share component and civil conflict incidence to examine if the "share" component can affect the civil conflict through some underlying confounders that cannot

[21] Given that advanced railway systems make it more convenient for armed groups to geographically connect (Sequeira et al., 2020), and natural resource revenues can contribute to civil wars in ambiguous directions (e.g., Cotet and Tsui, 2013; Berman et al., 2017; Angrist and Kugler, 2021), we further control for the logarithm of railway length (km) and the logarithm of natural resource rents in column 1 and column 2 of Table A6, respectively. The estimated outcomes remain nearly identical to the baseline result.

[22] To corroborate the necessity of applying the 2SLS strategy, we adopt the Durbin-Wu-Hauman test and report the corresponding F statistics in Table 2. The result indicates that we can reject the null hypothesis that there is no considerable difference in estimation outcomes between the OLS and 2SLS strategies.

[23] Note that it may take time for arms imports from the US to affect civil conflict. We check this conjecture by delaying arms imports from the US for one, two, three, four, and five years and re-estimate using the baseline specification. The results in Table A5 suggest that the effect of US arms can be rather long-lasting.

[24] We replicate Magesan and Swee (2018) with our sample, and the findings on the effect of SALW on repression are consistent with that of Magesan and Swee (2018). However, when we adopt an instrumental variable strategy, we do not find an adverse effect of US arms inflow on the incidence of civil war. A major reason could be that we exclude the sales of MCW transferred via commercial channels in the estimation. The detailed procedures are upon request.

[25] The O-P test is implemented when relaxing the strict assumptions of homoscedasticity and serial uncorrelation on the asymptotic distribution of the 2SLS (Stock and Yogo, 2005). The critical value is set when the estimator's approximate asymptotic bias exceeds a fraction of 5% of the "worst-case" benchmark (Olea and Pflueger, 2013).

**Table 4**
Placebo tests.

| | Reduced form | | | | First stage | |
|---|---|---|---|---|---|---|
| | (1) Civil conflict 1946–1971 | (2) Civil conflict embargo | (3) Civil conflict 1972–1987 | (4) Civil conflict 1988–2003 | (5) Arms imports 1972–1987 | (6) Arms imports 1988–2003 |
| Seat * (1972–1997) D | 0.370 | | | | | |
| | (0.238) | | | | | |
| Seat * (w/o embargo) D | | 1.681 | | | | |
| | | (2.749) | | | | |
| Seat * D (2004–2019) | | | −0.001 | −0.005 | 0.020 | −0.088*** |
| | | | (0.003) | (0.005) | (0.025) | (0.029) |
| Observations | 3510 | 226 | 2160 | 2160 | 2160 | 2160 |
| Baseline controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The variable "Seat * (1972–1997) D" is an interaction between a country's propensity to import arms from the US between 1972 and 1997 and one-year lagged seat ratio of Republicans in Congress from 1946 to 1971; the variable "Seat * (w/o embargo) D" is an interaction between a country's propensity to import arms from the US when there is no arms embargo and one-year lagged seat ratio of Republicans in Congress during the embargo period; the variable "Seat * D (2004–2019)" is an interaction between a country's propensity to import arms from the US and the one-year lagged seat ratio of the Republicans in Congress both from 2004 to 2019; The dependent variables from column 1 to column 4 are the civil conflict incidence during the period of 1946–1971, the period of arms embargo, the period of 1972–1987 and the period of 1988–2003, respectively; the dependent variables in column 5 and column 6 are arms imports from the US during the period of 1972–1987 and the period of 1988–2003, respectively. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01.

**Table 5**
Alternative measurements.

| | ACLED (1) | Civil conflict ratio (2) | IHS transformation (3) | Weighted by population (4) | Weighted by GDP (5) |
|---|---|---|---|---|---|
| Arms Imports from the US | −0.025*** | −0.068*** | −0.024*** | −0.030*** | −0.028*** |
| | (0.009) | (0.016) | (0.009) | (0.011) | (0.010) |
| Observations | 6615 | 6615 | 6615 | 6615 | 6615 |
| Kleibergen-Paap F test | 192.856 | 192.856 | 182.079 | 225.005 | 202.040 |
| Baseline controls | Yes | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. The instrument is an interaction between a country's propensity to import arms from the US and the one-year lagged Republican seat ratio in Congress. The dependent variable in column 1 is constructed using an alternative conflict data source, the Armed Conflict Location and Event Dataset (ACLED). The dependent variable in column 2 is the ratio of civil conflict among all types of conflicts. Across columns 3 to 5, the dependent variable is civil conflict incidence. The independent variables are the logarithm of arms imports from the US in columns 1 and column 2. The dependent variables are transformed by the IHS method or weighted by population and GDP from columns 3 to column 5, respectively. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01.

be completely captured in the estimation. In column 1 of Table 4, we undertake a placebo test by re-constructing an instrument with the share component averaged between 1972 and 1977, whereas the shift component varied from 1946 to 1971. We regress the civil conflict incidence between 1946 and 1971 on the aforementioned instrument. In column 2, we construct the share component by using the sample periods without arms embargoes while leaving the constructions of the shift component, the endogenous arms imports, and the civil conflict to sample periods with arms embargoes. If the share component can indirectly affect conflict incidence via unobservables in the error term, then the estimated coefficient of interest should still be significant in the above two placebo tests. Nevertheless, it is comforting to see that our instrument is not significantly correlated with the conflict incidence in both cases.

As supplementary evidence, we explore the correlation between the share component and location features that could vary in response to demand shocks of the arms market suggested by Goldsmith-Pinkham et al. (2020). Specifically, we regress the share

**Table 6**
Alternative specifications.

| | Logistic (1) | Ordered Probit (2) | Zero Ordered Probit (3) | Past Civil War (4) |
|---|---|---|---|---|
| Arms Imports from the US | −0.052** | −0.023** | −0.070** | −0.012*** |
| | (0.021) | (0.009) | (0.027) | (0.004) |
| Observations | 3577 | 6615 | 6480 | 6480 |
| Kleibergen-Paap F test | | | | 180.719 |
| Baseline controls | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | | Yes |
| Country fixed effect | Yes | Yes | | Yes |
| Region fixed effect | | | Yes | |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. The instrument is an interaction between a country's propensity to import arms from the US and the one-year lagged Republican seat ratio in Congress. The dependent variable in column 1 is the civil conflict incidence. The dependent variable in column 2 is a multi-value discrete variable that equals two if more than 1000 people died in the conflict, one of fewer than 1000 died in the conflict, and 0 if there is no civil conflict. The dependent variable in column 3 is the same as in column 2, except for the change of estimation method to deal with zero observations in the dependent variables. The dependent variable in column 4 is the civil conflict incidence, with a one-year-lagged civil conflict as a control variable. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
*p < 0.1, **p < 0.05, ***p < 0.01.

component, a country's propensity of purchasing US arms, on a set of averaged baseline controls.[26] The corresponding result is reported in column 1 of Table A7. We further provide an additional robust estimation in column 2, where we remove outliers from the baseline sample according to Cook's distance. Both values of R-squares suggest that more than 70% of the variations in the share component can be explained via baseline controls.[27]

Moreover, to systematically examine whether countries with varied arms demand are indifferent in civil conflict under placebo shocks, we implement a randomization test developed by Adão et al. (2019). Specifically, we interact the share component with placebo supply shocks drawn from a random normal distribution to construct the instrument.[28] We then apply this new instrument to regress the baseline estimation. Finally, we iterate these procedures 800 times and document the percentage of times when the results present significant effects at either 5% or 1% levels. As displayed in Figure A2, the coefficients of the placebo instruments are significant in 11.9% of the estimations at the 5% level and in 5.5% of the estimations at the 1% level. These randomization results suggest that the countries with varied arms demand present no significant differences in civil conflict under placebo shocks.

#### *4.2.2. Concerns with a spurious correlation*

Regarding the shift component, bias may arise from spurious trends between the shift component and the outcome variables. Following Christian and Barrett (2017), we plot the annual variation of the Republican seat ratio and the annual variations of arms imports from the US and civil conflict in different quantiles of countries' purchasing propensity in Figure A3. On the one hand, the yearly trends in arms imports and the occurrence of civil conflict are broadly parallel across all quantiles. On the other hand, the annual trend of the Republican seat ratio neither displays a discernible difference in correlation with that of arms imports or civil conflict nor a long-term trend similar to that of civil conflict.[29]

Additionally, we apply a Monte-Carlo simulation developed by Christian and Barrett (2017). In practice, for a specific country with strictly positive US arms purchases, we first randomly draw a yearly series of arms imports different from the true arms imports from a uniform distribution.[30] This procedure is repeated for each specific country, and the randomized arms imports are subsequently merged together. Next, we regress the incidence of civil conflict on the randomized arms imports while holding other variables unchanged, including the Republican seat ratio, the identity of the countries that purchase US arms, the conflict incidence, and other country characteristics. Finally, we repeat the above entire procedures 1000 times and plot the resulting distribution of 2SLS estimation in Figure A4. We observe that precisely 83.1% of the coefficients obtained from the simulations exceed the baseline coefficient, as indicated by the red dot line. This suggests that the estimation could only be ascribed to the actual arms imports rather than other confounders.[31]

[26] Unlike the conventional way that fixes the share to an initial period, the share component of our instrument is built as an average value over the entire sample period (1972–2020). Hence, the location characteristics in our context are correspondingly averaged over the sample years.

[27] Note that only three of the coefficients are significant at the 5% level. These significant estimates could be of less concern because the negligible movements in points estimates after controlling for this series of controls indicate little problem about omitting unobservables (Altonji et al., 2005; Oster, 2019).

[28] For simplicity, we use the same mean and variance (0 and 5) as Adão et al. (2019).

[29] The result of a unit root test on the shift component (−2.298), which rejects the null hypothesis that the shift component is non-stationary at the 5% level, further corroborates the stationary nature of the shift component.

[30] Note that the upper bound of arms imports between 1972 and 2020 serves as the maximum parameter of the uniform distribution, while the minimum is set to be the lower bound of arms imports during the same period.

[31] In the worst situation where the exclusion restriction of the instrument is not satisfied, we replace the exact exclusion restriction with an assumption related to the instrumental coefficient's distribution and thus permit the failure of the exclusion restriction within a specific range (Conley et al., 2012). By doing so,

#### 4.2.3. Other concerns with the identification

In addition to the above-mentioned tests, similar to Nunn and Qian (2014) and Autor et al. (2013), we construct both share and shift components of the instruments using the data between 2004 and 2019 and then regress the arms imports between 1972 and 1987 or between 1988 and 2003 on this newly constructed instrument conditional on baseline controls.[32] Conceptually, we should not observe a significantly positive effect of these placebo instruments. Otherwise, the construction of our instrument is not reliable. As displayed in column 3 and column 4 of Table 4, the future instruments in both cases are not significantly and positively associated with past arms imports. Column 5 and column 6 provide the corresponding first-stage estimation, showing no significant positive correlation with arms imports.[33]

One potential concern regarding the aforementioned test is that the result is likely to be driven by the subjective choice of sample years. To address this sample selection issue, we adopt a randomization test suggested by Ferrara et al. (2012). Specifically, we randomly select different cutoffs that divide the sample period into two exclusive ones—the early period for the construction of arms imports and the latter period for the construction of "spurious" instruments and repeatedly run the first-stage estimation 1000 times. We then plot the density of the estimated coefficients in Panel (a) of Figure A6. As expected, the "spurious estimates" are centered on 0 and the baseline estimate denoted by the vertical red line lies outside the density distribution of these "spurious estimates". This simulation exercise gives us more confidence that the previous placebo test is solid to support the validity of the 2SLS strategy.[34]

One may also worry that the supply of US arms is driven by the ruling power of major arms-exporting states or districts in Congress rather than partisan preference. To eliminate this concern, we undertake a placebo test by constructing a shift component with the seat ratio of Congress members whose representative states are major arms exporters.[35] Suppose that Congress members' economic concerns for representative states, if any, would dominate political decisions, we should expect a plausibly strong correlation between arms imports from the US and this newly constructed instrument. In practice, we first use the sample median of arms exports to distinguish electoral states and then count the proportion of Congress members from states with above-median arms exports. The corresponding results of the first stage and reduced form estimation, as shown in column 1 and column 2 of 2 of Table A8, reveal that this placebo instrument is statistically irrelevant to endogenous arms imports and has no significant effect on civil wars. A similar test is applied to a district-year panel and reaches the same conclusion, as shown in column 3 and column 4. Overall, there is no significant evidence showing that arms imports affect civil conflict once the shift component captures the variation of a congressman's economic preference towards their representative state or district.

### 4.3. Robustness checks

Though we apply an instrumental estimation to establish a causal relationship between arms imports from the US and civil war, there can be other underlying concerns weakening our results. To facilitate the robustness of the baseline estimation, we present a series of supportive evidence that the effect of the US arms we identified is not coincidental.

#### 4.3.1. Alternatives measurements and specifications

*Measurement Issues*— In this section, we test for the robustness of the baseline result by exploring alternative measurements of both the dependent variable and the independent variable. In column 1 of Table 5, we quantify the dependent variable as a ratio of civil conflict among all conflicts. In column 2, we use another conflict data source, the Armed Conflict Location and Event Dataset (ACLED; Raleigh and Dowd, 2016; Berman et al., 2017), to construct the dependent variable. Following the variable construction in the baseline estimation, the dependent variable is similarly defined as a dummy that equals one if there is one or more conflicts between the two forces and zero otherwise.[36] The estimation suggests a significant and negative effect of arms imports from the US, the magnitude of which is close to the baseline estimation. Column 3 to column 5 provides results with alternative measurements for arms imports. In column 3, we use the inverse hyperbolic sin transformation(IHS) to alleviate potential biases caused by the logarithm transformation of the arms imports. In column 4 and column 5, we use country-year-specific GDP and population size to weigh arms imports, respectively. In all cases, the coefficients of our interest remain significantly negative with a modest change in magnitude. Therefore, measurement errors in key variables should not bias our baseline estimates.

*Specification Issues*— We further test for the robustness of baseline results in different specifications, as shown in Table 6. In column 1, given the discrete nature of our dependent variable, we report the estimation results using a Logistic regression with the

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we substantiate that a significant effect of arms imports still exists even with the relaxation of the exclusion restriction. We provide this evidence in Figure A5, and estimations are provided upon request.

[32] Note that the period for constructing our instrument is restricted to 16 years, and as a result, it is necessary to match it with the outcome variable that spans the same length of time. Therefore, we compile two samples of the outcome variable, one from 1972 to 1987 and the other from 1988 to 2003.

[33] Although the result in column 6 presents a significantly negative correlation between the future instrument and past arms imports, it contradicts the facts that Republicans advocate for the expansion of the US arms sales.

[34] In Panel (b) of Figure A6, we conduct a similar exercise by regressing past civil conflict on future instruments. The procedure is identical, except that we construct the civil conflict incidence using the previous periods and the instrument using the latter periods. We then plot the density of the key estimate and observe that the real estimate also falls outside the distribution of spurious estimates.

[35] We collect the data on state-specific and district-specific Congress membership from Congress.Gov (https://www.congress.gov/).

[36] Civil conflict in the ACLED includes conflicts between state forces and rebel actors, clashes between state forces and unidentified armed groups, violence between military and civilians, violence between rebels and pro-government militia, as well as civil conflicts that result in less than 25 deaths.

**Table 7**
Alternative instruments.

| | Seat ratio(House) (1) | Seat ratio(Senate) (2) | Bills enacted (3) | Seat ratio (t − 2) (4) | Military spend in GDP (5) | Detrended IV (6) | Multiple IV by LIML (7) | Multiple IV by GMM (8) |
|---|---|---|---|---|---|---|---|---|
| Arms Imports from the US | −0.025*** | −0.025*** | −0.046* | −0.036*** | −0.016** | −0.021*** | −0.031*** | −0.032*** |
| | (0.009) | (0.008) | (0.025) | (0.012) | (0.007) | (0.007) | (0.004) | (0.004) |
| Observations | 6615 | 6615 | 5805 | 6615 | 6615 | 6615 | 5805 | 5805 |
| Kleibergen-Paap F test | 176.903 | 258.481 | 36.525 | 157.451 | 17.373 | 277.177 | | |
| Overidentification test | | | | | | | 4.021 | 7.170 |
| Baseline controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. In column 1 and column 2, the instrument is constructed by interacting the arms purchasing propensity with the Republicans seat ratio in the House and in the Senate, respectively. In column 3, the instrument is an interaction between arms purchasing propensity and the percentage of arms-related bills in all bills. In column 4, the instrument is an interaction between arms purchasing propensity and the two-year lagged seat ratio of Republicans in Congress. In column 5, the instrument is an interaction of Republican seat ratio with a country's military expenditure in GDP. In column 6, the instrument is an interaction between a detrended Republican seat ratio in Congress with arms purchasing propensity. Across columns 7 to 8, we use the instruments from column 1 to column 5 as well as the baseline instrument to predict arms imports from the US. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
*p < 0.1, **p < 0.05, ***p < 0.01.

definition of the dependent variable unchanged. In column 2, we employ a multi-value discrete measure as the dependent variable, which equals two if a conflict claims more than 1000 deaths, equals one if it claims 1000 death, and equals zero if there is no civil conflict. We apply the Ordered Probit Model to estimate the causal effect in column 2 since this estimation method fits the ordered multi-value dependent variable better.[37] In column 3, we also check the robustness of our baseline estimate to the excessive number of zero observations in the dependent variable (Harris and Zhao, 2007).[38] The estimation is the same as that in column 2 whereas we use a Zero-Ordered Probit Model in the second stage.[39] The estimated coefficients of interest across the three columns are statistically negative and significant at the 1% level (−0.052; −0.023; −0.070) though the magnitudes vary compared with the baseline estimate. Finally, to alleviate the underlying concern that our baseline specification does not capture the persistent effect of civil war, we control for the one-year-lagged civil conflict incidence to account for the dynamics of conflict. The estimated result in column 4 of Table 6 shows that the significantly positive effect of US arms still exists.

#### *4.3.2. Alternative formulations of the instrument*

We next check the robustness of our baseline estimation to using alternative instruments that isolate the exogenous variations in arms imports. Recall that in the baseline estimation, we exploit the time-varying seat ratio of Republicans in Congress to proxy for the US arms supply. A straightforward way to check its robustness is to apply alternative measurements of the Republican seat ratio. Since the House and the Senate differ in responsibilities for political decisions, we use the share of Republican seats in either the House or the Senate to measure arms supply respectively, as shown in column 1 and column 2 of Table 7.[40] In addition, we consider the share of enacted bills related to arms export among all bills passed by Congress and signed by the president as another proxy for arms supply. The corresponding result is shown in column 3.[41] Across column 1 to column 3, despite slight drops in magnitudes, the estimated effects of US arms are still significantly negative.

Additional worry arises in that membership in the US Congress is reserved for at least two years, our baseline instrument could bias the estimate as it does not account for the variations right after the re-election of Congress members.[42] To address this issue, we use the two-year lagged seat ratio of Republicans in Congress to approximately measure the variation of US arms supply that possibly comes from the re-election procedure. The corresponding result in column 4 is qualitatively similar to the baseline estimate.

[37] Given that the estimated models in column 1 and column 2 are nonlinear, we use a control function to address the endogeneity in arms imports (Wooldridge, 2015). First, We obtain residual $\hat{\lambda}$ by regressing arms imports on the instrument with a full set of baseline controls using OLS. Then in the second stage, we control for the residual $\hat{\lambda}$ and $(\hat{\lambda})^2$ using the Logistic or Ordered Probit Model.

[38] Recall that the incidence of civil conflict equals zero when there is no civil conflict or the conflict caused less than 25 deaths, resulting in a relatively high share of zeros in the outcome variable.

[39] Due to the limited capacity of the traditional Ordered Probit Model in explaining the preponderance of zero observations, the Zero-Ordered Probit Model can account for the possibility that zero observations arise from two different sources: no existence or lower value.

[40] We also use two different dummy variables and a House fragmentation (Ahmed, 2016) to proxy for arms supply. The first dummy measurement equals one if the seat ratio of Republican to Democratic exceeds one. The second dummy measurement equals one if the incumbent president is a Republican. The fragmentation, $1 - \frac{|Democrat_t - Republican_t|}{435}$, captures the degree of fragmentation in the US House, with a higher value indicating a higher seat ratio for Republicans relative to Democrats. All results remain robust. These estimation results are upon request.

[41] Data on seat ratio of Republicans in the House and Senate and which party the incumbent president belongs to are collected from Nick Hillman's Party Control in Congress and State Legislatures. Data on enacted bills related to arms export are collected from GovTrack.

[42] One-third of Senators and all members in the House are reelected every two years.

**Table 8**
Onset, duration, and escalation.

| | Collier and Hoefler (2004) | Deterrent effect | Logistic discrete-time Hazard model | Peace duration | Conflicts escalation |
|---|---|---|---|---|---|
| | (1) Onset | (2) Onset | (3) Offset | (4) Duration | (5) Escalation |
| Arms Imports from the US | −0.006*** | | 0.047 | 1.320*** | −0.165* |
| | (0.002) | | (0.045) | (0.350) | (0.096) |
| L.Arms Imports from the US | | −0.006*** | | | |
| | | (0.002) | | | |
| Observations | 6615 | 6480 | 749 | 6615 | 6615 |
| Kleibergen-Paap F test | 192.856 | 191.232 | | 192.856 | |
| Baseline controls | Yes | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. In column 1, the dependent variable is the onset of civil conflict, which equals one if a period lies in the beginning year of a civil conflict and equals zero otherwise. In column 2, the dependent variable is constructed the same as that in column 1 but is lagged one year. In column 3, we adopt a discrete hazard model and limit the sample to episodes of that country's first transit into a peaceful state. The "survival probability", the probability that a country stays in a state of conflict, follows a logistic distribution. In column 4, the dependent variable is the number of peaceful years between two civil conflicts. In column 5, the dependent variable is the escalation of conflict, which equals one if a civil conflict between two unchanged groups induced more than 1000 casualties and equals zero otherwise. The regression in column 5 employs a Probit estimation. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
*p < 0.1, **p < 0.05, ***p < 0.01.

**Table 9**
Public confidence.

| | (1) Armed forces | (2) Local policy | (3) Government capacity |
|---|---|---|---|
| Arms Imports from the US | 0.010*** | 0.067** | 0.063* |
| | (0.004) | (0.032) | (0.038) |
| Observations | 106 429 | 106 429 | 106 429 |
| Kleibergen-Paap F test | 8.705 | 8.446 | 5.562 |
| Individual characteristics | Yes | Yes | Yes |
| Baseline controls | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes |

*Note:* Observations are at the individual-year level. The periods of the observations consist of five survey years, including 1995, 1998, 2006, 2013, and 2018. The sample size is limited to 50 countries with valid observations. Across column 1 to column 3, the dependent variable is a rating index ranging from 0 to 4, with a higher value indicating the stronger public trust in armed forces, local police, and government capacity, respectively. The individual characteristics includes gender, age, income class, and the highest education an individual receives. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
*p < 0.1, **p < 0.05, ***p < 0.01.

Moreover, we exploit military expenditure to gauge the demand for US arms. Given that military expenditure mainly consists of arms purchases and thus can be closely associated with arms demand, we use the share of military spending in GDP to measure a country's arms demand and report the corresponding result in column 5 of Table 7.[43] Though the magnitude of the coefficient is relatively smaller than the baseline estimate, the effect of the US arms is still significantly negative at the 10% level.

Finally, to address the concern that the linear trend of the Republican seat ratio in the US Congress could be confounded by underlying factors with similar trends, following Bluhm et al. (2020), we construct our instrument with a detrended Republican seat ratio. The corresponding estimate using this new instrument in column 6 shows that our main conclusion is unaffected. To further ensure that there is no presence of misspecification, we include all forms of instruments from column 1 to column 5 of Table 7 along with the baseline instrument and adopt alternative estimators that are more efficient with multiple instruments—the Limited Information Maximum Likelihood (LIML) estimator and the General Method of Moments(GMM) estimator. Column 7 to column 8 report the estimation results, which remain negative and significant. The overidentification tests show that the estimated effects are statistically indistinguishable and robust to misspecifications.

[43] The data on military expenditure in GDP is collected from the World Bank Indicator.

Overall, our baseline story is robust to using different formulations of the instrument or incorporating multiple instruments to predict the US arms sales.

#### 4.3.3. Additional robustness checks

We also explore a bunch of other robustness checks to support our baseline results. In this section, we briefly describe these additional estimations.

First, our baseline estimate might be biased due to the inclusion of data with poor quality in early periods.[44] We restrict our sample to the period from 1980 to 2020 and re-estimate the effect of arms imports using specification (1). The estimation for the key independent variable is still statistically significant (at the 1% level) but marginally decreases in magnitude, as shown in column 1 of of Table A9.[45] Moreover, we examine whether the significance of our baseline estimate is driven by a small number of influential observations. In column 2, we simply focus on countries that have purchased US arms for at least one year and less than ten years (Bove and Gavrilova, 2017). To further corroborate this check, we remove the top 1% and 5% of US arms recipients in column 3 and column 4, respectively. Following Nunn et al. (2018), we also check the results by dropping observations according to Cook's distance, as shown in column 5.[46] Across all these columns, we find that the baseline effect is robust to the exclusion of the influential outliers.[47]

Finally, our core result in Table 2 is robust to an exhaustive list of other potential channels through which the instrument could influence the civil conflict. Similar to Ahmed (2016), we control for the interaction between the Republican seat ratio and a vector of country characteristics (e.g., oil exporter and former colony dummy), the interaction between a propensity to purchase US arms and annual crude oil prices, the interaction between the share component and year fixed effects, the regional year trends as well as testing for a potentially possible channel through consistent foreign policy.[48]

## 5. Additional results

### 5.1. Mechanism

In the previous sections, we have established the causal relationship between arms imports and the occurrence of civil war, showing that arms imports from the US reduce civil conflicts to some extent. Why does the effect exist? In this section, we proceed to examine the potential mechanisms that could explain the above findings.

#### 5.1.1. Onset, duration, and escalation

The first direction to explain our baseline result is to see whether importing more US arms can prevent the initiation of new wars, curtail the existing wars, prolong the duration of peaceful periods, and restrain the escalation of ongoing wars. Particularly, we assess the deterrent effect of arms imports from the US on the outbreaks of civil conflict. The deterrence power of arms inflow mainly comes from the central role of MCW in manifesting the military capability of resisting violent activities (Pamp et al., 2018). Our estimations on the effects of different weapons in Table A10 also account for this conjecture to some extent. Clearly, the estimate of MCW is relatively more prominent than the other two types of weapons, suggesting that MCW is more accountable for the conflict-reducing effect of US arms.

We start by examining the effect of arms imports on the onset of civil conflict. To achieve this estimation, we reconstruct the sample by removing periods of continued conflict and only keeping periods of no conflict or the first year of a conflict (Collier and Hoeffler, 2004). By this construction, the onset of a civil conflict is defined as a dummy variable that equals one if $t$ is the first period of a civil conflict and zero for periods without civil conflict. Then we estimate the effect of arms imports on the onset of civil conflict using specification (1). The corresponding estimation result is presented in column 1 of Table 8. In column 2, we particularly investigate the deterrent effect of arms imports on the onset of civil conflict using one-year-lagged arms imports as the key independent variable. The existence of the deterrent effect is effectively explained by the expected result that one-year prior arms imports would lead to a reduction in the occurrence of civil conflict. Both estimates in column 1 and column 2 provide compelling evidence that arms imports reduce the likelihood of civil conflict through a powerful deterrent effect, which holds statistically significant in our estimation. We examine the effect of US arms on the offset of a civil conflict by including observations that a country transit into a peaceful state for the first time and then adopting a discrete hazard model.[49] The estimation result, as shown in column 3 of Table 8, indicates that arms imports increase the probability of offset, though the effect is not significant at the conventional level. We revisit the arms' effect on the offset by looking into the duration of peace between war episodes. The result

[44] For instance, before 1992, arms imports data are coded by SITC 1, and thus we have to manually convert these imports data in every four-digit department to the HS92 Version.

[45] Other key results also remain unaffected if we use the sample from 1980 to 2020.

[46] We first compute the influence of each observation using Cook's distance. Then we omit observations with a distance greater than $4/n$, where $n$ is the number of observations in the sample.

[47] We offer additional evidence on the influence of outliers, in which we randomize the removal of influential observations. In practice, we randomly drop a country from the baseline sample each time and re-estimate specification (1). We repeat this procedure 135 times and plot the estimated key coefficients in Figure A7. In most cases, the coefficient rarely changes, lending support for the reliability of our estimation across different sub-samples.

[48] These estimation results are upon request.

[49] In the discrete hazard model, the "survival probability"—the state of being in conflict—is denoted as a probability function following a logistic distribution. Additionally, we include a third-degree polynomial of duration to control for the varying effects of duration on offset.

in column 4 shows that a 1% increase in arms imports from the US is associated with an extended period of peace by 1.320 years, which is considerably salient in terms of saving millions of lives.

Finally, unlike Magesan and Swee (2018) who focus on the onset and offset, we provide additional evidence on whether arms imports hold back the escalation of ongoing wars. We define the escalation of civil conflict as a dummy variable that equals one if a civil conflict involves the same opposing actors and results in more than 1,000 casualties. The estimated result in column 5 confirms our expectation that importing more arms from the US could restrain the escalation of civil wars.

#### 5.1.2. Public confidence

Another potential explanation for our baseline result is that arms imports from the US affect public confidence in governments. Under a complete information environment where fake arms stock cannot constitute a successful deterrence (Neumayer and Plümper, 2009), official arms purchases, particularly the procurement of MCW, send a strong signal of the government's willingness to deploy its military forces (Kreps and Wilson, 1982). This signaling of the government's resolution reflects a credible commitment to future peace, influencing the public trust in governments. The extent of trust in governments could further affect individuals' decisions to participate in civil violence (Blattman and Miguel, 2010). Hence, it is reasonable to hypothesize that increasing arms imports decreases the likelihood of civil war by offering the general public a credible commitment to peace and strengthening their confidence in the government's capability of safeguarding.

The major challenge for testing the above assumption is to appropriately construct individuals' confidence in national armies, police forces, and government capacity. Similar to Nunn et al. (2018), we resort to three related questions in five waves of the World Value Survey (1995, 1998, 2006, 2013, 2018), from which we are able to measure the public confidence in national armies, local policing, and government capacity.[50] After deleting invalid answers, we obtain 106,429 observations across 50 countries in all.[51] We also control for gender as a dummy, age, highest education the person attained, and income level in our estimation. Column 1 to column 3 of Table 9 report the corresponding estimation results. The estimates of our interest are significantly positive, implying that importing arms leads to a significant shift in the distribution of public confidence. This strengthened confidence restrains people from joining violence and crushes the initiation of internal battles.

#### 5.1.3. State capacity

Our third explanation for the baseline result is related to the improved availability of public goods facilitated by the spillover effect of military infusion. For one thing, as an indispensable component of public goods, the military capabilities (e.g., troop size) could be directly enhanced with the purchase of arms, thereby decreasing the likelihood of civil conflict (Fearon and Laitin, 2003; Pamp et al., 2018). For another thing, arms sales involve more than a transfer of weapons. They also encompass the transfer of intelligence in weapons manufacturing and operations (Yakovlev, 2007; Fearon and Hansen, 2018). To maintain these imported weapons and initiate joint arms production, various infrastructure projects have to get involved. As a major proportion of US arms sales, the FMS provides administrative support for international partners, including access to joint training in arms operations, technical assistance required to maintain a weapon system, and cooperative arms development programs that may not be accessible in the private sector. To fully realize the benefits of all these US post-sale services, arms recipients must continue to invest in public goods provision and education training. Consequently, the US arms inflow is expected to increase the provision of social infrastructure and public services.

The ripple effect of arms imports may extend to even broader socio-economic dimensions. For example, previous studies suggest that arms imports could potentially enable the government to extract natural resources and impose taxes on its populace more efficiently (Levi, 1988; Cheibub, 1998) or improve the working productivity of individuals transitioning from the military to civilian employment (Dunne, 2005). Overall, we assume that the likelihood of a civil war is diminished as arms imports from the US improve the provision of public goods and thus engender a rising opportunity cost of involving in violence.

To measure the provision of public goods, we obtain the data on peacekeeping commitment from the Peace Keeping website and the data on other public goods, including paved road length (in logarithm, km), hospital beds (per 1000 people), and secondary education attainment among the population over the age of 25.[52] We then repeat the 2SLS estimation procedure by substituting the dependent variable in specification (1) with a series of variables measuring the provision of public goods. Estimation results are presented in column 1 to column 4 of Panel A of Table 10.[53] Column 1 reports a positive effect of US arms on troop size, though the effect is not significant at the conventional level. Column 2 to column 4 provides the estimated effect of the US arms on paved road length, hospital beds, and secondary education attainment, respectively. The estimates of our interest across these columns are all significantly positive, suggesting that the US arms inflow improves the state capacity in terms of public goods provision.[54]

[50] The three related questions are "How much confidence do you have in armed forces?", "How much confidence do you have in the police?", and "How much confidence do you have in the government?". The first question is adopted for measuring individuals' confidence in national armies, the second one for public confidence in local policing, and the last one for confidence in government capacity. The available answers to these three questions include "1 (a great), 2 (quite a lot), 3 (not very much), and 4 (none at all)". To have a direct sense of estimated results, we reverse the survey answers so that a larger value indicates more confidence.

[51] The sample coverage is somehow limited if we adopt the scores of the answers from the World Value Survey, spanning only 50 countries over five survey waves. However, both the distributions of the sample countries and years are representative. We cautiously interpret these estimation results as suggestive evidence.

[52] Peacekeeping commitment is measured by the logarithm of the number of militia soldiers. The proxies for other public goods are obtained from the World Bank Development Indicators.

[53] Note that we could not include all of our baseline observations in the specifications of the above measurements due to missing values in the raw data.

[54] Considering that it may take time for the increased arms imports to affect the provision of public goods, we also employ one-year, two-year, and three-year lagged arms imports to test the above conjecture. These results are upon request.

**Table 10**
State capacity.

| *Panel A: Provision of public goods* | | | | |
|---|---|---|---|---|
| | (1) Troop size | (2) Paved road length | (3) Hospital capacity | (4) Secondary education |
| Arms Imports from the US | 0.017 | 0.010* | 0.041** | 0.062** |
| | (0.033) | (0.006) | (0.021) | (0.024) |
| Observations | 6615 | 5488 | 2013 | 6615 |
| Kleibergen-Paap F test | 192.856 | 163.246 | 19.746 | 192.850 |
| Baseline controls | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes |

| *Panel B: Institution* | | | |
|---|---|---|---|
| | (1) Corruption control | (2) Rule of law | (3) Trade uncertainty |
| Arms Imports from the US | −0.003 | 0.004 | −0.212 |
| | (0.017) | (0.096) | (0.133) |
| Observations | 6615 | 6566 | 6365 |
| Kleibergen-Paap F test | 192.856 | 195.905 | 142.484 |
| Baseline controls | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes |

| *Panel C: Economic development* | | |
|---|---|---|
| | (1) Poverty rate | (2) Employment rate |
| Arms Imports from the US | −0.003 | 0.007*** |
| | (0.010) | (0.002) |
| Observations | 5537 | 6566 |
| Kleibergen-Paap F test | 113.336 | 192.664 |
| Baseline controls | Yes | Yes |
| Year fixed effect | Yes | Yes |
| Country fixed effect | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. The number of observations varies across regressions because of missing values in the outcome variables. Panel A reports the estimates on public goods provision. In column 1, the dependent variable is civilian troop size (logarithm of the number of militia soldiers). In column 2, the dependent variable is the paved road length (logarithm, km). In column 3, the dependent variable is the hospital beds per 1000 people. In column 4, the dependent variable is the secondary education attainment among the population aged over 25. Panel B reports the estimations on institutional outcomes. In column 1, the dependent variable is the corruption control index. It quantifies the extent to which public power is exercised for private gain, including petty and grand forms of corruption and the "capture" of the state by elites and personal interests. A higher score for this index indicates lower corruption. In column 2, the dependent variable is a rule of law index. It captures perceptions of the extent to which agents have trust in and adhere to established social rules. It considers the quality of contract enforcement, property rights, the police, courts, and the likelihood of crime and violence. A higher value on this index denotes a more advanced law system. In column 3, the dependent variable is a uncertainty proxy for trade policy. Panel C reports the estimations on economic development. In column 1, the dependent variable is the percentage of the population earning less than $2.15 a day adjusted by purchasing power-adjusted prices. In column 2, the dependent variable is the employment rate. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
*p < 0.1, **p < 0.05, ***p < 0.01.

Next, we attempt to provide more evidence by exploring the potential effects of US arms inflow on institutional outcomes, including corruption, the rule of law, and trade policy. To gauge the level of corruption and the rule of law, we use an index of corruption control and a rule of law index acquired from World Bank Government Indicators.[55] A higher score for the corruption control index indicates a lower degree of corruption, and a higher value on the rule of law index denotes a more advanced law system. Regarding trade policy, we rely on the measurement of trade policy uncertainty developed by Handley and Limão (2017).[56] The corresponding estimation results for these institutional outcomes are displayed in Table 10. Across all the columns, we do not

[55] The corruption control index aims to quantify the extent to which public power is exercised for private gain, including petty and grand forms of corruption and the "capture" of the state by elites and personal interests. The rule of law index captures perceptions of the extent to which agents have trust in and adhere to established social rules. Particularly, it considers the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence.

[56] Concisely, the trade policy uncertainty is constructed by $log(Tariff_{it}^{t} - Tariff_{i}^{1971})$, where $Tariff_{it}^{t}$ denotes the average tariff rate of country $i$ in year $t$, and $Tariff_{i}^{1971}$ denotes the average tariff rate of country $i$ in the year 1971. The data on tariffs are obtained from the World Bank Indicators.

**Table 11**
Heterogeneity by natural conditions.

| | (1) Civil conflict | (2) Civil conflict | (3) Civil conflict | (4) Civil conflict |
|---|---|---|---|---|
| Arms Imports from the US | −0.011*** | −0.028*** | −0.063*** | −0.012*** |
| | (0.004) | (0.010) | (0.022) | (0.005) |
| Arms Imports from the US ∗ Adverse Rainfall Shock | −0.063* | | | |
| | (0.032) | | | |
| Arms Imports from the US ∗ Excessive Rainfall Shock | | −0.021*** | | |
| | | (0.008) | | |
| Arms Imports from the US ∗ Mineral Production | | | 0.020** | |
| | | | (0.010) | |
| Arms Imports from the US ∗ Energy Production | | | | 0.004** |
| | | | | (0.002) |
| Observations | 6615 | 6517 | 6615 | 6615 |
| Kleibergen-Paap F test | 60.119 | 55.102 | 37.676 | 19.745 |
| Baseline controls | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. In column 1, the variable "Rainfall Shock" measures a country-specific degree of rainfall instability. For each country, we first calculate a deviation to the mean monthly precipitation (mm) level within each year and then take a yearly average using the country's land area as the weight. A higher value of this adverse rainfall shock at the country level indicates less rainfall, resulting in a higher probability of drought. In column 2, extreme rainfall is calculated by the yearly average number of days with precipitation over 50mm (i.e., stormy weather) weighted by a country's land area. In column 3, the variable "Resource Production" is the total quantity of 18 kinds of crucial minerals in a country, including aluminum, coal, copper, fluorspar, gold, iron, lead, lithium, chrome, manganese, phosphate rock, silver, platinum, titanium, uranium, and zinc. In column 4, the variable "Resource Production" is the total quantity of 4 energy resources, including coal, gas, petroleum, and uranium. The instruments in columns 1–4 are the interaction term between a country's propensity to import arms from the US and the one-year lagged seat ratio of Republicans in Congress, the interaction term between the above-mentioned natural conditions and one-year lagged seat ratio of Republicans in Congress, and the triple interaction among a country's propensity to import arms from the US, the one-year lagged seat ratio of Republican in Congress, and the above-mentioned natural conditions. The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table 12**
Heterogeneity by demography and political relations.

| | (1) Civil conflict | (2) Civil conflict | (3) Civil conflict | (4) Civil conflict |
|---|---|---|---|---|
| Arms Imports from the US | −0.015*** | −0.071*** | −0.021*** | −0.024*** |
| | (0.005) | (0.022) | (0.007) | (0.008) |
| Arms Imports from the US ∗ High Ethnic Diversity | 0.009** | | | |
| | (0.004) | | | |
| Arms Imports from the US ∗ Large Religious Population | | 0.027** | | |
| | | (0.013) | | |
| Arms Imports from the US ∗ Trade Volume with the US | | | −0.025* | |
| | | | (0.014) | |
| Arms Imports from the US ∗ Political Alignment with the US | | | | −0.016* |
| | | | | (0.008) |
| Observations | 5733 | 6517 | 6615 | 6517 |
| Kleibergen-Paap F test | 22.410 | 185.169 | 11.715 | 11.953 |
| Baseline controls | Yes | Yes | Yes | Yes |
| Year fixed effect | Yes | Yes | Yes | Yes |
| Country fixed effect | Yes | Yes | Yes | Yes |

*Note:* Observations are at the country-year level. The time window of the regression is from 1972 to 2020. In column 1, the variable "High Ethnic Diversity" is a dummy that equals one if the share of the ethnic group population in a country is above the median level in all countries and equals zero otherwise. In column 2, the variable "Large Religious Population" is a dummy that equals one if the share of the main religious population in a country is above the median in all countries. In column 3, the variable "Trade Volume with the US" is measured by the trade value per capita (deflated to 2010 US dollar) between each country and the US. In column 4, the variable "Political Alignment with the US" is the share of a country's votes in the UNGA consistent with the US. The instruments used across columns 1 to column 6 are the interaction term between a country's propensity to import arms from the US and the one-year lagged seat ratio of Republicans in Congress, the interaction term between the above-mentioned demography (or political relations) and the one-year lagged seat ratio of Republicans in Congress, and the triple interaction among a country's propensity to import arms from the US, the one-year lagged seat ratio of Republicans in Congress, and above-mentioned demography (or political relations). The baseline controls include the logarithm of GDP per capita, the logarithm of population size, the logarithm of military aid, the logarithm of other imports from the US, monthly average temperature, monthly average precipitation, democracy level, and primary schooling attainment for the age group over 25 (%). Standard errors, clustered at the country level, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

observe any significantly unpleasant effect of arms imports from the US on corruption, the rule of law, and trade policy. In light of this evidence, our finding that arms imports from the US can bring peace to purchasing countries is much more credible.

Lastly, we supplement our analysis by examining the effect of arms imports on economic development. Specifically, we examine whether arms imports promote economic activities, foster local employment, and mitigate poverty.[57] We replace the baseline dependent variable with the above-mentioned measurements and re-estimate specification (1). As shown in Panel B of Table 10, importing US arms significantly increases employment and decreases poverty, though its effect on poverty is not significant at the conventional level.

### 5.2. Heterogeneity

In the final section of our empirical analysis, we provide suggestive evidence on whether the effect of arms imports from the US differs depending on natural conditions and characteristics of socio-demographic conditions. The specific heterogeneity effect of arms imports we consider covers the frequency of extreme weather, the endowment of natural resources, and a series of socio-demographic features, including a country's ethnic diversity, religious population, and relationship with the US.

#### 5.2.1. Heterogeneity by natural conditions

Motivated by previous studies on the relationship between rainfall and conflict (Miguel et al., 2004; Aragón and Rud, 2013; Rogall, 2021; König et al., 2017; Angrist and Kugler, 2021), we first investigate how the effect of arms inflow varies by the differentiated climate conditions in recipient countries. In less developed countries where revenue extracted from the agriculture sector accounts for the majority of state income, an increase in rainfall shocks reduces the gross gain from the agriculture sector, leading to the unemployed labor force being absorbed by conflict activities. In such cases, frequent rainfall disruptions result in more domestic conflict and thus dampen the benign role of US arms. Nevertheless, it is entirely possible that in countries with more such shocks, the governments may anticipate the social instability resulting from climate shocks and thus stock more arms to deter violence, which further strengthens the positive role of the US arms. Therefore, how rainfall shocks differentiate the arms' effect across countries is theoretically undetermined.

We employ two measurements for rainfall shock. First, following Moscona et al. (2020), we construct a variable $Adverse\ Rainfall\ Shock$ to measure the average level of rainfall shortage for a given country. Specifically, we calculate the average precipitation (mm) experienced by each country in each year of our sample period. We then subtract the monthly rainfall from the above yearly average precipitation. To obtain a cross-country variation of adverse rainfall shock, we further take an average of the above difference over years for each country and normalize the final value by a country's land area. A higher value of this variable indicates an adverse rainfall shock, resulting in a serious drought. The second measurement for rainfall shock, $Excessive\ Rainfall\ Shock$, is measured by the number of days with precipitation over 50 mm in a given country in a given year. We further take an average of this value over the years for each country and normalize it by a country's land area. The value of this measure is higher when more excessive rainfall episodes occur and thus cause flooding.[58]

To empirically examine the heterogeneous effects, we include an interaction term between arms imports and the country-specific characteristics described above. The estimation specification changes to:

$$y_{ct} = \beta_1 \hat{Arms}_{ct} + \beta_2(\hat{Arms}_{ct} \times R_c) + X'_{ct}\gamma + \delta_c + \lambda_t + \epsilon_{ct} \tag{5}$$

where $R_c$ denotes the country-specific rainfall shock whose direct effect is absorbed by country fixed effects. Following Nunn and Qian (2014), the endogenous variable $Arms_{ct}$ and $Arms_{ct} \times R_c$ are instrumented with $Seat_{t-1} \times D_c$, $Seat_{t-1} \times D_c \times R_c$, and $Seat_{t-1} \times R_c$. The corresponding first-stage equation for $Arms_{ct}$ is:

$$Arms_{ct} = \alpha_1(Seat_{t-1} \times D_c) + \alpha_2(Seat_{t-1} \times D_c \times R_c) + \alpha_3(Seat_{t-1} \times R_c) + X'_{ct}\gamma + \delta_c + \lambda_t + \mu_{ct} \tag{6}$$

Note that the interaction term $R_c \times D_c$ is time-invariant country-specific variation and thus is also absorbed by country fixed effects.

The coefficients of interest are $\beta_1$ and $\beta_2$. $\beta_1$ measures the effect of the US arms on civil conflict in countries with fewer rainfall shocks while $\beta_1 + \beta_2$ together measures the effect of the US arms in countries experiencing larger rainfall shocks. The estimation results are reported in column 1 and column 2 of Table 11. The estimates indicate that the role of the US arms in reducing civil conflict remains significantly positive (−0.011; −0.028) while the positive role of arms in crushing civil wars is considerably stronger in countries with more occurrences of extremely low/high rainfall (−0.011–0.058 = −0.069; −0.028–0.020 = −0.048). This result is consistent with the prior conjecture that less developing countries with undesirable climate conditions are more vigilant on social unrest. Hence, governments in these areas tend to procure more military weapons to resist armed groups who are tempted to use "worse weather" as an excuse for conflict recruitment. Another relevant natural condition subject to our scrutiny is mineral and energy endowment. Previous research associates natural riches with fueling armed fights (Berman et al., 2017), particularly the resources that are capital-intensive such as oil, gas, and minerals (Dube and Vargas, 2013). Therefore, we carry out the second heterogeneity test by focusing on two kinds of capital-intensive resources, energy, and mineral. Specifically, we utilize the production

[57] We obtain the data on the economic activities, the poverty rate, and the employment rate from the World Bank Indicators. The poverty rate is defined as the percentage of the population who own less than $2.15 a day, adjusted by purchasing power-adjusted prices in 2017. The employment rate is defined as the percentage of the population who are employed.

[58] Both two variables for rainfall shock is calculated using the monthly precipitation data from the World Bank Climate Change Knowledge Portal.

data on energy and mineral to construct a variable $Resource\ Production_c$, defined as the total amount of energy/mineral production normalized by a country's land area.[59]

Likewise, we interact the production of these two kinds of natural resources with the key independent variable in the baseline estimation and re-estimate the specification (4). Column 3 and column 4 of Table 11 report the corresponding results. The estimated $\beta_2$ suggests that the violence-reducing effect of US arms is much dampened in countries with more endowments of these natural resources. It is worth noting that in mineral-abundant countries, the impact of arms inflow on civil conflict is much more offset by mineral-driven social unrest compared with that in energy-abundant countries. This is consistent with the prior evidence that exposure to a sizeable distribution of small mining spreads violence by making it easier for armed groups to extract revenues and thus enhancing their financial capacities (Berman et al., 2017).

#### 5.2.2. Heterogeneity by social demography

Given that the diversity of social demography appears to affect within-country disputes (Connor, 1994; Huntington, 1996; Moscona et al., 2020), we consider two features of social demography, ethnic diversity and the share of the religious population, to understand the relationship between arms imports from the US and civil conflict.

The first examination is whether the effect of the US arms on internal conflict differs in the ethnic diversity measured by the population share of ethnic groups for each country.[60] Specifically, the variable $High\ Ethnic\ Diversity$ equals one if the population share of ethnic groups in a country exceeds the median of all and zero otherwise. Next, we examine the role of the religious population measured by a dummy that equals one if the population share of the main religion in one country is larger than the median of all and equals zero otherwise.[61]

Similarly, we interact these two measures of demographic diversity with the key independent variable in the baseline and re-estimate specification (4). Column 1 and column 2 of Table 12 display the heterogeneous effect of arms concerning the above two features. The estimates of interest are significant at least at the 10% level, suggesting that the effect of US arms is weaker in countries with higher ethnic diversity or larger religious composition. A possible explanation for these findings is that multi-ethnic diversity and ideological clashes between various religions are conditions that favor instability (Fearon and Laitin, 2003; Neumayer and Plümper, 2009).[62]

In addition, we examine the differentiated effect of US arms by looking into the economic and political relationship between recipients and the US. Specifically, we include population-weighted trade volumes to measure the economic tie and the fraction of a country's votes in the UNGA that are consistent with those of the US to measure the political alignment. We hypothesize that countries that are more economically and politically aligned with the US experience a larger conflict-reducing effect of US arms. The hypothesis is mostly based on the fact that the US is more inclined to trade high-tech arms with its economic partners or political allies, and these high-tech arms are in turn one of the most effective tools to deter and restrain violent activities. The corresponding results in column 3 and column 4 of Table 12 confirm that either economic or political accordance with the US strengthens the conflict-reducing effect of the US arms.

## 6. Conclusion

There have been widespread concerns that arms transactions instigate social and political conflicts throughout the world. The noted phrase "Arms fuel conflicts" is a justifiable inference drawn from many previous works. However, most studies that arrive at this conclusion focus on the effect of commercial arms sales or military aid, which is susceptible to being diverted to rebel groups, while neglecting the conflict-reducing effect stemming from arms transactions under official manipulations. Our study aims to make progress in understanding the positive role of arms inflow, emphasizing the conflict-reducing effect of arms transactions between governments.

We establish a rigorous causal relationship between arms imports from the US and the incidence of civil war. The identification method we employ is an instrumental variable strategy, in which we exploit two exogenous but relevant sources of variations. Specifically, we construct an instrument, an interaction between a time-varying seat ratio of Republicans in the US Congress as a proxy for arms supply and the across-country propensity of purchasing US arms as a proxy for arms demand. We find the existence of a beneficial role of US arms in reducing the incidence of civil conflict, which survives numerous sensitive tests on instrumental validity and remains robust to various measurements and specifications. Interestingly, the conflict-reducing effect of the MCW inflow is more prominent than other types of weapons. Moreover, we provide more evidence to confirm that such a beneficial effect of US arms is most likely via three mechanisms. On the one hand, purchasing more arms, especially the MCW, promotes public confidence and imposes a deterrent effect on rebels by equipping the national armies and local police. On the other hand, an increasing inflow

[59] We include eighteen types of mineral resources (aluminum, coal, copper, fluorspar, gold, iron, lead, lithium, chrome, manganese, phosphate rock, silver, platinum, titanium, uranium, and zinc) and four types of natural energy resources (coal, gas, petroleum, and uranium). The production data of these natural resources are collected from the British Mineral Statistics.

[60] We collect data on ethnic diversity from the Ethnographic Atlas. The dataset provides cultural practices for global societies with perfect coverage of Africa and Western North America.

[61] We collect data on the composition of the religious population from the World Religion Review. The main religions in our sample include Christianity, Muslim, Hinduism, Buddhism, Judaism, and other major folk religions.

[62] We also check the effect of social flexibility and social structure in differentiating the impact of the US arms on civil conflict. These results are upon request.

of weapons strengthens the government's capability to maintain and increase the provision of public goods and even promotes economic development, resulting in a high opportunity cost of involving in conflict.

Our findings on the beneficial role of arms transfer provide a sound rationale for attaching importance to how arms are transferred and where arms are finally used. Compared with commercial arms transactions, the government-to-government arms transfer equips the recipient government with military capacity more efficiently due to the regulated scopes of end-users and transparent post-sales maintenance. Such a beneficial role of government-to-government arms imports suggests that a more careful investigation of the weapon recipients should be carried out, especially in cases where the arms are transferred commercially so that global arms transactions could be potentially beneficial rather than jeopardizing regime stability. In addition, our discussions on the heterogeneity of the arms effects offer further insights into considering different local conditions when scrutinizing and approving the arms transfer by exporting countries. It is worth noting that arms imports are more helpful for those countries caught in extreme climate conditions but can be discounted in countries with abundant capital-intensive resources and a diversified social structure. Such findings speak to humanity-related concerns on the role of multi-ethnic resource-based countries in the future implementation of global arms regulations.

While the pleasant role of arms imports is limited to certain scenarios with specific arms composition and transfer channels, the finding of our study is, to some extent, encouraging in releasing the growing concerns on regional political stability due to block transactions of military weapons. Meanwhile, our study provides insights for future policy adjustments on potentially excessive regulations and war compensation regarding global arms control.

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## Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jce.2023.06.011.

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