The Build-up of Coercive Capacities: Arms Imports and the Outbreak of Violent Intrastate Conflicts

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Abstract

Do governments' military build-ups foster the outbreak of intrastate violence? This article investigates the impact of governments' arms imports on the onset of intrastate conflicts. There is scant empirical research on the role of the external acquisition of coercive technologies, and even fewer studies explore the respective causal mechanisms of their consequences. We argue that the existing literature has not adequately considered the potential simultaneity between conflict initiation and arms purchases. In contrast, our study explicitly takes into account that weapon inflows may not only causally induce conflicts but may themselves be caused by conflict anticipation. Following a review of applicable theoretical models to derive our empirical expectations, we offer two innovative approaches to surmount this serious endogeneity problem. First, we employ a simultaneous equations model that allows us to estimate the concurrent effects of both arms imports on conflict onsets and conflict onsets on imports. Second, we are the first to use an instrumental variable approach that uses the import of weapon types not suitable for intrastate conflict as instruments for weapon imports that are relevant for fighting in civil wars. Relying on arms transfer data provided by the Stockholm International Peace Research Institute for the period 1949-2013, we provide estimates for the effect of imports on

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civil war onset. Our empirical results clearly show that while arms imports are not a genuine cause of intrastate conflicts, they significantly increase the probability of an onset in countries where conditions are notoriously conducive to conflict. In such situations, arms are not an effective deterrent but rather spark conflict escalation.

Keywords: arms trade, civil war, simultaneous equations model, instrumental variable approach

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Introduction

Weapons constitute the non-sufficient but necessary input technology for conflict (see Hirshleifer, 2000). The coercive capacity of incumbent governments and their effective control over a state's territory against internal and external challengers relies essentially on the availability of arms. This is an important aspect, which the extensive literature on intrastate conflict onsets (recent reviews include Dixon, 2009; Blattman & Miguel, 2010) has largely neglected so far.[1] As only few countries are able to produce modern conventional weapons systems (see Thurner et al., 2016), most countries have to import them. This raises serious questions. What is the role of governments' external arms acquisition for intrastate crisis bargaining and escalation? Do they play a causal role in intrastate conflicts?

Looking selectively at historical examples does not provide conclusive answers. There are cases where large arms imports preceded the outbreak of a conflict (e.g. Angola: 1975 and 1998) but also cases where only small or no inflows of arms occurred before a conflict onset (e.g. Liberia: 1989 and 2000). At the same time, there are instances where we observe large arms imports but no civil war (e.g. Libya: 1974-1983, United Arab Emirates: 2004-2013). Therefore, a fresh look at the consequences of international weapon flows is of practical relevance. The creation of the United Nation's Arms Trade Treaty in 2013 shows that the international community is well aware of the potential risks of an uncontrolled international arms trade. Knowledge of whether the availability of major conventional weapon systems to incumbent governments increases or decreases the risk of intrastate conflict would be therefore of major importance for conflict prevention.

Quantitative empirical research has been almost silent on the impact of arms imports. The (very) few studies that do exist offer no clear theoretical account on how weapon deliveries could affect the initiation of intrastate conflicts, and they arrive at contradicting

[1] Note that there is a more recent literature which relates conflict technologies and external support of rebel groups to conflict intensity and duration, see Balcells & Kalyvas (2014); Sawyer, Gallagher Cunningham & Reed (2015).

empirical conclusions. We argue that this is most likely due to the fact that when studying the relationship between conflicts and the acquisition of arms, one is faced with the problem of a potential simultaneity between the emergence of conflict and arms procurement: weapon imports (just as military expenditures) may not only have a causal influence on conflict probability, but perceived and anticipated conflict itself could causally influence governments' inclinations to purchase arms. The resulting endogeneity makes any theoretical as well as statistical analysis challenging. In our view, this problem has not been appropriately dealt with in previous research.

Previous research on arms and intrastate conflicts has also not offered many theoretical rationales for why there should be a causal link between weapon transfers and conflict onset. We accentuate the fundamental insight that scholars of interstate wars have long recognized (see Diehl, 1983): conflicts do not predominantly arise over arms, they arise first and foremost over contested issues. We argue that only the proper control of the established conflict incentives and opportunities allows us to identify whether arms imports have a causal influence on conflict. Bearing this caveat in mind, we explore several relevant approaches. In particular, we look at (i) arguments about how the availability of arms may change policy makers willingness to use them (e.g. Fordham, 2004); (ii) how arms inflows could be perceived as signals about a government's resolve or it's military capabilities in the framework of well-established crisis bargaining models (e.g. Powell, 2006; Walter, 2009b,a), and (iii) how investment in arms may affect different types of state capacities and thus make conflict more or less likely (e.g. Fearon & Laitin, 2003; Besley & Persson, 2011). While arguments about deterrence and signalling might suggest a negative relationship between imports and conflict probability, a number of other mechanisms point in the opposite direction. Under high conflict-risk conditions, governments and opposition groups have strong incentives to initiate violence due to various preemptive motives or to solve collective action problems in situations of highly asymmetric military endowments. Furthermore, a lack of state capacity may also make conflict more likely.

In this article, we analyze major conventional weapons (MCW) and their import by governments. This makes sense as MCWs are regularly deployed by the state in civil wars (see Kalyvas & Balcells, 2010). There are two objections that could be raised against this particular focus, however. First, small arms and light weapons (SALW) are also an essential conflict technology in civil wars. Second, looking only at governments neglects the fact that rebel groups may also import arms which could in turn affect intrastate conflicts. With respect to the first point, it would indeed be optimal to simultaneously analyze both MCW and SALW transfers. However, existing data series on SALW transfers are much shorter (starting only in the 1990s) and less consistent than for MCW trades. Furthermore, there is no reason to believe a priori that conflict dynamics induced by both weapon classes are similar. Governments often have a monopoly on MCW before a conflict outbreak. Thus, they are a potentially visible signal of a state's resolve and military capabilities. Hence, it does make sense to analyze the effects of MCW and SALW separately. Regarding the second objection, transfers to rebel groups are generally much more secretive than government purchases and probably only a highly biased subset of these transactions appears in our data. As a result, we have much less reliable data on exports to rebels and expect systematic measurement errors to be a huge problem.[2] Moreover, governments are the main recipient of major conventional weapons (MCW) prior to a conflict outbreak. Observable arms flows are therefore predominantly going to governments (although this often changes after a civil war begins).

We are able to draw on an extensive data set of 137 countries for the period 1949-2013. Our empirical analysis finds a clear positive effect of MCW imports on the probability of intrastate conflict onset. This effect is particularly large for high-risk countries, that is, for countries in which conflict-inducing conditions, as outlined by the literature, are present. This leads us to conclude that arms imports in these situations do not serve as an effective deterrent by governments but contribute to conflict escalation and thus the outbreak of civil war.

[2]Note that the existing external support data by Cunningham & Salehyan (2009) focuses exclusively on conflict events and provides only discrete measures of rebel military capacity.

In the following, we will first provide a short overview of the sparse quantitative literature investigating the correlation between weapons imports and intrastate conflict. We then outline our theoretical arguments for deriving our empirical expectations. Before proceeding with our statistical analysis, we discuss how our econometric structural equation and instrumental variable approaches help surmount the endogeneity problem. This is followed by a discussion of our empirical results and a number of robustness tests. The final section offers a brief summary and concluding remarks.

The role of arms imports in the literature

Research on arms transfers and domestic conflicts is scarce. A comprehensive review of empirical findings for about 200 variables by Dixon (2009) mentions arms trade only briefly as belonging to the group of negligible, non-significant factors. The few publications that do investigate the role of arms trade on intrastate war arrive at diverging conclusions. Some studies indicate that arms imports have no effect on the outbreak of internal conflicts. Craft (1999) investigates the link between arms and inter- and intrastate wars at the systemic level between 1945 and 1992. He finds a positive relationship between the total value of transferred MCWs and the number of ongoing conflicts, but no meaningful correlation between arms transfers and the outbreak of conflicts. Durch (2000) investigates interstate and intrastate wars separately and analyzes the role of arms deliveries for 106 developing states between 1970 - 1995. His results suggest that arms transfers may facilitate the outbreak of external wars but do not affect the risk of internal wars. The most distinct rejection of a link between arms transfers and the outbreak of civil conflict comes from Suzuki (2007). He regresses the outbreak of civil and ethnic conflicts on arms imports in a pooled regression covering 100 states between 1956 to 1998 and finds no statistically significant links.

Other studies, by contrast, suggest that arms contribute to the outbreak of civil conflicts. Based on a pooled regression analysis, Craft & Smaldone (2002) find that arms imports are a significant predictor of civil war incidence in sub-Saharan Africa (1967 to 1997). Blanton (1999) reports a robust link between country-level arms imports and political repression and infers: "Arms acquisitions appear to contribute to repression by making violent political acts more feasible"(Blanton, 1999: 241). Sislin & Pearson (2001) investigate the role of arms transfers in ethno-political conflicts. Their main finding from three in-depth case studies is that arms acquisition leads to increased violence during conflicts.[3] However, an additional comparison of 133 ethno-political groups in the 1990s suggests that access to arms may also trigger rebellion and conflict.

The empirical literature leaves us with contradictory results, which may suggest that there simply is no systematic link between arms imports and civil wars. However, a definite assessment requires additional theoretical and empirical work. Most papers discuss appeasement vs. instability in general terms but do not disentangle the motivations and signals set by arms transfers (e.g. Craft, 1999; Craft & Smaldone, 2002; Suzuki, 2007). We seek to contribute to the debate with a more detailed discussion of the specific causal implications of governmental arms imports. Our research is further motivated by improvements regarding the documentation of arms transfers over recent years. Some previous publications relied on less reliable data sources and used more restricted sample sizes and observation periods. Finally, a crucial empirical challenge is the problem of endogeneity and reverse causality. Conflicts may be a consequence of arms acquisition, yet arms acquisition may also be the result of the anticipation of conflicts or conflict risks. This problem has not been sufficiently taken into account by previous research. Our study therefore proposes the use of both instrumental variables and simultaneous equation modelling.

[3]See also Moore (2012).

Exploring theoretical links between intrastate conflict onset and the import of arms

The existing empirical studies offer almost no theoretical rationale for their empirical hypotheses. This should come as no surprise since there are, to our knowledge, no theoretical models that explicitly consider the causal influence of arms imports on the onset of violent intrastate conflicts. To overcome this shortcoming, one can draw on the rationalist approaches developed for the explanation of wars (for overviews see Powell, 1999; Levy & Thompson, 2010; Bueno de Mesquita, 2014). This includes a growing theoretical literature on militarization and armament processes in interstate conflicts (e.g. Kydd, 2000; Baliga & Sjöström, 2004; Meirowitz & Sartori, 2008; Jackson & Morelli, 2009). Its assumptions on the distribution of capabilities and information, commitment problems and the credibility of signals, as well as the costs of deterrence and war have been discussed as major mechanisms also in the domestic context (for an overview see Lichbach, 2012). However, systematic, context-specific theoretical transfers of insights from interstate war models to domestic conflicts have been rare. The reason is that intrastate conflicts usually involve a state and one or more domestic groups. Classical notions of arms races and strategic mutual armament developed for interstate dynamics do not always apply here. This is all the more the case when considering major conventional weapons. Prior to the outbreak of violence, it is usually the government that is in control of a country's MCW imports. Even if rebel groups prepare for civil war by illegally obtaining weapons, these are in most cases small arms and light weapons, which are technically less demanding and easier to acquire. Rebels often gain access to MCWs only after conflicts start, mostly through external support by third countries or looting (Jackson, 2010).

The following theoretical discussion does not attempt to develop a new model but rather draws on insights from the literature on armament, interstate wars and domestic conflicts. Our basic premise is that conflicts do not arise over arms, they arise over certain issues that seed the sow for possible conflicts (Fearon & Laitin, 2003; Collier & Hoeffler, 2004). If the conditions that create violent conflicts are not present, importing arms should not provoke any violence. Throughout, we assume the existence of a central government and one or more opposition groups under conflict-inducing conditions.

A government can decide to build up military capabilities by importing MCWs. This can either be in response to foreign or domestic threats, but it always has to be balanced against the opportunity costs of not spending limited resources for other ends (Powell, 1993; Besley & Persson, 2011). For intrastate conflicts, only weapons that can be used against internal challengers are relevant. The aim of such a purely domestically-oriented armament process can either be to prepare for a future conflict onset or to deter domestic challengers by signaling the government's resolve and its capabilities to act militarily. The decision to increase military capabilities could be made in anticipation of a certain conflict because the government expects violent challenges by rebels or intends to use force to remove the potential for any future challenges (Powell, 2013). In this case, weapon deliveries are positively associated with the origin of intrastate conflict but are not a causal factor. This point highlights why taking the enodgeneity of arms imports seriously is critical in our empirical analysis. The government is therefore always faced with the strategic decision of whether to buy arms and whether to initiate a violent conflict (for an overview see Kydd, 2015: chapter 7). Domestic groups (potential rebels), on the other hand, can observe arms imports and need to decide whether to initiate a violent challenge to the government. They cannot observe the true resolve of a government to respond militarily to a challenge and they may or may not be able to observe the state's true capabilities. This situation can thus be conceptualized as a repeated strategic game. In each period, the government determines whether to import arms and each side decides whether to initiate a conflict. Since acquiring weapons is costly, the government needs to weigh its benefits (successful deterrence or prevailing in future conflicts) against its opportunity costs (Powell, 1993). Given this simplified set up, we discuss how a government's decision to import MCW affects the probability of conflict onset.

Arms imports and the feasibility of force

This argument was initially proposed by Most & Starr (1989) who distinguish between "opportunity" and "willingness". Having military capabilities creates the opportunity to resolve latent conflicts through the use of force. But having a military option available does not necessarily mean that a government is willing to use it. Only if arms imports affect "willingness" should we observe a link between arms imports and civil war onsets. Hence, any theoretical account needs to explain why "willingness" could increase with arms inflows. Analyzing the use of force in U.S. foreign policy, Fordham (2004: 635) refers to different arguments why higher military capabilities may increase policy makers' "willingness". In particular, increases in the expected utility of fighting or the strengthening of hardliners in the government are offered as possible explanations. His empirical analysis of U.S. foreign policy finds that military capabilities indeed increased the frequency with which force was used (Fordham, 2004: 649-652). In a similar vein, Sislin & Pearson (2001: 59) argue that in domestic conflicts arms increase tensions levels and threat perceptions, thus making violence more feasible. In sum, these arguments suggest that arms imports increase the willingness of governments to use force, which makes conflict onset more likely.

Signaling and deterrence

The situation of a government facing one or more potential rebel groups contesting over a certain issue can be linked to an entry deterrence game in economics, where repeated play and private information about one's resolve creates strong incentives for reputation building (Kreps & Wilson, 1982). To deter rebel groups from posing a violent challenge, the government has an incentive to gain a reputation to be resolute and thus willing to meet any challenge to its authority with the use of force. If successful, such a signal should discourage challengers and make conflict less likely. Rebel groups are unable to observe the true resolve of the government and make inferences based on the signals they receive.

There are, however, two important qualifications that cast serious doubts on this hypothesized negative relationship. First, arms inflow may be interpreted as a signal of the unwillingness of the government to compromise. This could accelerate conflict onset if rebels expect their chance of winning to decrease further in the future. Second, even if winning the conflict is unlikely, rebels may still wish to fight. As Walter (2009b: 35) points out, we cannot only distinguish resolute and irresolute governments but there are also different levels of commitment by rebel groups. In particular, there could be groups that are not prepared to back down from their demands even if a violent conflict is very unlikely to be won. While in our view this distinction between resolute and irresolute rebels is not satisfactory to explain a seemingly irrational wish to fight a losing battle, an alternative explanation could lead to the same outcome. Rebel groups may initiate conflict (or low-level skirmishes such as terrorist attacks) to provoke a government reaction and then use this reaction to recruit support for their cause (see Lichbach, 1998). This would in turn increase their chance of winning a conflict. Attempts to organize and overcome collective action problems could therefore explain why, despite strong signals of resolve by the government, rebels may still choose violent conflict. Thus, onset probability would increase as a peaceful solution is deemed more unlikely with rising arms imports.

So far, we have considered arms imports as signals about a government's resolve. This should not be conflated with signals about military capabilities (Slantchev, 2011: 14). Purchasing weapons could also be an attempt to signal military strength and the potential to increase this strength through imports. This again is aimed to deter challenges and therefore, if successful, should reduce conflict probability (Huth, 1988; Jervis, 1989). Again, the opposite effect is also possible. Observing the ongoing armament of the government may spur rebel groups to initiate conflict as quickly as possible as weapon imports may lead to a shift in the distribution of power. As Powell (2006: 195) has argued, a rapidly shifting distribution of power may lead to conflict because an initially weak state cannot commit to other domestic groups that it will not use it's increased capabilities in the future to renege on existing peaceful settlements. Furthermore, interstate models of endogenous armament adapted to the intrastate context suggest that rebels may decide to fight when the costs of violent conflict are perceived to be lower relative to the costs resulting from the power shift towards the government. If, on the other hand, the state is strong and there is a large asymmetry in military capabilities, then there could be predatory motives by the state to initiate military action. As the model by Chassang & Padró i Miquel (2010) suggests, in this situation conflict is very likely under either complete information or even under incomplete information if the state has a complete monopoly on violence. Finally, the government may consider deterrence as too costly in the long term and thus decides to start a violent conflict in order to eliminate the opposition (Kydd, 2015). In this case, rising arms imports are temporary and a preparation for domestic war. Note that this may also lead to cases where, due to incomplete information, rebels preempt the government's decision to fight if they cannot distinguish attempts at deterrence from a government's preparation to strike first.[4]

Arms imports and state capacities

Fearon & Laitin (2003) have argued that it is the weakness of governments that strongly influences the onset of civil war. States have different types of capacities to peacefully solve domestic conflicts or deal with challenges militarily (Hendrix, 2010). A very narrow and simple approach would be to equate state capacity with military and police capabilities (Fearon & Laitin, 2003; Kalyvas, 2006). Arms imports increase this type of capacity and should obviously reduce the probability of violent domestic conflict.

The effect of arms imports is less clear-cut when considering other dimensions of state capacity. Azam (2001) points to the importance of redistributive capacity. Developing a game-theoretical model, he argues that in African cases it is the ability of states to

[4] As an anonymous referee has correctly pointed out, rebels may also misinterpret arms imports aimed at foreign enemies to be directed at them. This could either lead to a sort of accidental deterrence or to intrastate conflict because rebels may initiate violence because they erroneously think that the government is preparing for domestic war.

effectively redistribute resources among different groups and their elites that reduces the probability of violent conflicts. A more prominent dimension of capacity is fiscal or extractive capacity (Tilly, 1985; Levi, 1988; Cheibub, 1998). This describes the ability of countries to efficiently tax their citizens and to extract natural resources. A high capacity is expected to lower the potential for violent conflicts. Empirical analyses have only yielded mixed results, failing to establish a clear-cut inverse relationship between fiscal capacity and onset probability (Thies, 2010, 2015). Besley & Persson (2011) have developed a formal model in which governments make decisions to invest in future state capacity (fiscal and legal) and in military capabilities. They show that if political institutions are non-inclusive, decisions by the government to invest in military capabilities results in either one-sided political violence (see also Blanton, 1999) or civil war. Resources spent on the military reduce investment in future state capacity which lowers wages and thus makes war more likely. Intuitively and in more general terms, resources spent on arms imports lower investment in conflict reducing (redistributive, fiscal and legal) capacity-building. It follows that arms imports should increase the probability of violent intrastate conflict.

Summary of theoretical expectations

Endogeneous armament processes and violent intrastate conflict are still not fully understood in the domestic context. Adapting existing theoretical approaches to account for possible arms inflows yields no clear-cut predictions. Arguments from crisis bargaining that stress the importance of signaling to deter challengers as well as theories highlighting the importance of military state capacity suggest an inverse relationship between arms transfers and onset probability. In contrast, a positive effect of weapon imports on onset probabilities can be deduced from arguments about the feasibility of force, crisis bargaining with highly committed challengers, numerous incentives for both sides to initiate preemptive attacks and trade-offs between investing in arms and investing in redistributive, fiscal and legal state capacity. We provide a synopsis of these theoretical arguments in Table I.

< Table I about here >

In the following sections, we aim to conduct a thorough econometric analysis that adequately deals with the problem of the endogeneity of arms imports. While this is not intended to be a test of all the models and arguments introduced in this section, it will allow us to isolate the aggregate effect of weapon imports.

Data and research design

Data Sources & operationalizations

We use an unbalanced panel data set that covers 137 countries for the years 1949 to 2013. We measure the beginning of an intrastate conflict as a dummy variable (*onset*) that takes the value 1 if two conditions are met: (i) in a given year t there are at least 25 deaths related to an intrastate (ethnic or civil) conflict, and (ii) there has been no outbreak of the same conflict in $t-1$ and $t-2$.[5] Data on intrastate conflicts are from UCDP/PRIO's Armed Conflict Dataset 4-2015 (http://ucdp.uu.se/downloads/, see Gleditsch et al. (2002); Melander, Pettersson & Themnér (2016)).

A particular issue arises with respect to the coding of ongoing intrastate conflicts. The majority of the empirical literature sets ongoing conflict years to zero (McGrath, 2015: 535). As McGrath (2015) shows using Monte Carlo experiments, this can lead to biased estimates and wrong substantive inferences. We decided therefore to set ongoing conflict years to missing.

Data on arms imports are provided by the Stockholm International Peace Research Institute (SIPRI). They cover the transfer of major conventional weapons (MCWs), which do not include weapons of mass destruction and small arms and light weapons (SALWs). Arms imports are measured in trend indicator values (TIV) that represent the known, inflation-adjusted production costs of core weapon systems measured in millions

[5]Note there are instances where there is more than one intrastate conflict in a country in a given year.

of constant-1990 US dollars. Hence, what is supposed to be measured is not the financial value of arms but the volume of military capabilities that is being transferred. MCWs cover a wide range of weapon systems, from armoured vehicles to aircraft to satellites to components such as engines.[6] We exploit the fact that not all of these weapon systems are deployable for intra-state conflicts. For our analysis, we therefore construct different versions of MCW import variables: *logimports* measures the annual import of arms types which are presumably of high importance in intrastate wars (aircraft, armoured vehicles, artillery and missiles); *logimports_unrelated* captures the annual import of arms that are unrelated to intrastate conflicts (air-defense systems, anti-submarine weapons, satellites and ships).[7] Note that only arms deals where the TIV of the traded weapons is at least 0.5 (i.e., 500,000 US-$) are recorded by SIPRI. For all of our import variables we have multiplied this TIV value by 100 and then taken the natural logarithm.

The drawback of looking at annual figures is that while SIPRI reports TIV values for the date of delivery, the time of ordering weapons might already constitute a signal to rebel groups. To smooth the temporal differences between order and delivery dates, we created two variables, *logimports_avg*5 and *logimports_avg*10, which are 5-year and 10-year backward moving averages of arms imports.

Conflict onsets and arms imports: Addressing the endogeneity problem

Analyzing the impact of arms deliveries on the onset of intrastate conflicts, we are confronted with a problem of co-determination: Arms imports may not only affect conflict but could themselves be caused by its outbreak or the anticipation of such an event in the immediate future. This is all the more apparent if we conceptualize conflict onset as a latent, continuous variable. Other than a binary conflict indicator might suggest, onset is the consequence of a development in which tensions rise until a certain threshold

[6] See http://www.sipri.org/databases/armstransfers/background/coverage for more details.

[7] "Engines", "Sensors" and the category "Other" were not included in either variable.

is reached. In our case, this threshold is marked by a certain number of conflict-related deaths. As a result, a positive correlation between arms imports and onset could either indicate a causal effect of weapon deliveries on conflict onset or be a consequence of the looming outbreak which raises the demand for weapons. The few empirical studies that do exist on arms imports and intrastate conflict have, in our view, not paid sufficient attention to this problem.

We apply a twofold strategy: On the one hand, we propose an instrument for civil war related weapons. On the other hand, we employ a simultaneous equation model (SEM) as developed by Amemiya (1978), Heckman (1978) and Maddala (1983).The instrumental variable framework proceeds in two steps: first, we predict weapon imports with an instrumental variable that is related to the import decision (first-stage relevance), but not to civil war onset (the exclusion restriction). Second, we use the exogenous variation induced by the instrument to assess the endogenous variation[8] between imports and civil war (second stage). We propose to use variation in non-civil war related weapons as a suitable instrument for civil-war related weapons. With rare exceptions (e.g. the naval warfare in the Sri Lankan civil war), these weapons are plausibly targeted at external defense and interstate wars. We argue that the purchase of these weapons types is due to regional arms races, security concerns and external threats, but also because of corruption, military greed and prestige (e.g. Ball, 1993). At the same time, in order to sustain a balanced military portfolio, governments tend to purchase other major conventional weapons that are deployable in a civil-war. This latter part of our argument is testable: The correlation between both types is strong. We report the first stage regression results in Online Appendix Table A.3: an increase in *logimports_unrelated* leads to a substantial and significant increase in the related imports variable (*logimports*), with and without inclusion of a broad range of control variables. The correlation is, as expected, even stronger

[8]As indicated by the bottom row of Table II, an approach taking endogeneity into account is warranted: a Wald test on exogeneity (see Wooldridge, 2002: 472-5) does not reject the null of no correlation between the reduced form residuals and the dependent variable for several models.

with 5-year or even 10-year moving averages. The instrument is thus clearly relevant and passes weak-instruments tests. The exogeneity of the instrument, on the other hand, can only be assessed theoretically. We deem it highly plausible, however, that in anticipation of a civil war, rational military and political leaders would replenish and expand stocks of civil-war related weapons and not specifically purchase the types included in *logimports_unrelated*. As a test of this assumption, we show that *logimports_unrelated* is not correlated with civil war onset when controlling for (SEM-instrumented) *logimports* (compare Appendix Section A.2.2).

The IV-probit set-up then takes the following form:

$$Y_1 = \alpha_1 + \beta_1 R_1 + \gamma_1' \mathbf{Z}_{on} + \epsilon_1 \tag{1}$$

$$Y_2^* = \alpha_2 + \beta_2 \hat{Y_1} + \gamma_2' \mathbf{Z}_{on} + \epsilon_2, \tag{2}$$

where Y_1 measures arms imports and is instrumented by civil-war unrelated arms imports R. Equation 1 controls also for variables later used in the second stage probit ($\mathbf{Z}_{on}'$). The second stage, Equation 2, then draws on predicted arms imports ($\hat{Y_1}$) as dependent variable to explain binary conflict onset Y_2^*. To our knowledge, we are the first in proposing this instrument and think there is a good case that we go beyond the existing empirical literature when addressing the endogeneity problem in this way.

Since one can never fully prove that an instrument works, we therefore, as a second strategy, apply a simultaneous equations model (SEM). A number of studies have utilized this approach to deal with simultaneity in studying, for instance, the reciprocal relationships between trade in interstate disputes (Keshk, Pollins & Reuveny, 2004), democracy and war (Reuveny & Li, 2003), state capacity and intrastate conflict onset (Thies, 2010), and between fiscal capacity and state failure (Thies, 2015).

The SEM takes the following general form:

$$Y_1 = \beta_1 Y_2^* + \gamma_1' \boldsymbol{Z_{im}} + u_1 \tag{3}$$

$$Y_2^* = \beta_2 Y_1 + \gamma_2' \boldsymbol{Z_{on}} + u_2, \tag{4}$$

where Y_1 measures arms imports and Y_2^* is our binary conflict onset variable. The vectors $\boldsymbol{Z_{im}}$ and $\boldsymbol{Z_{on}}$ capture the set of variables for the second stage import and onset equations. In the first stage, both equations are estimated using the *full* set of variables (Z_{im} and Z_{on}). The fitted values of the two dependent variables are then plugged into the second stage to replace the endogenous regressors using the specific sets of independent variables. The continuous dependent variable equation is then estimated by OLS and the binary dependent variable equation is a probit model. This allows us to simultaneously estimate the effect of weapon deliveries on conflict onsets and the effect of onsets arms imports.

While technically relying on a similar exclusion restriction compared to an instrumental variable approach, both the import and the export equation are predominantly theoretically justified. For the import equation, we use insights from Smith & Tasiran (2005) who estimate demand functions for arms imports as well as results from Pamp & Thurner (forthcoming) who analyze the relationship between arms and military expenditures. The second-stage import equation is thus as follows:

$$ln(import) = \beta_0 + \beta_1 \hat{onset} + \boldsymbol{\gamma_1} \begin{pmatrix} logimports\_unrelated \\ logmilex \\ logexport \\ loggdppc \\ capacity \\ region\_dummies \end{pmatrix} + \epsilon_{1,t} \quad (5)$$

Similar to the instrumental variable approach, we use the import of unrelated weapons. This complements the use of log military expenditures (*logmilex*), which are measured in millions of 2010 constant US dollars. One may suspect that arms imports and military spending are part of an accounting identity but results by Pamp & Thurner (forthcoming) clearly show that this is not the case. We also include the log of arms exports (*logexport*). The rationale for doing so is that the international arms trade is in large part an intra-industry trade (Thies & Peterson, 2015). In addition, we control for GDP per capita, which should be positively related to arms imports due to likely income effects of weapon demand (*loggdppc*). As different levels of state capacity might influence not only onset directly but also the feasibility of arms imports, we include the Relative Political Capacity index (*capacity*) created by Kugler & Tammen (2010). Finally, we introduce five regional dummies ($west$, $eastern\ europe$, $latin\ america$, $subsaharan\ africa$, $asia$) with the reference category being the region of Northern Africa and the Middle East. This is motivated by the fact that some countries have faced security-dilemma-type situations which have led to arms races with other countries (Richardson & Rashevsky, 1960).

Turning next to the second-stage onset equation, the choice of covariates largely follows the seminal contribution by Fearon & Laitin (2003). We improved on some of their variables, especially with respect to measuring ethnic heterogeneity. Furthermore, we expand their specification by taking the insights of some of the more recent studies into account:

$$onset = \beta_0 + \beta_2 ln(\hat{import}) + \boldsymbol{\gamma_2} \begin{pmatrix} onset\_last5y \\ loggdppc, logpop, redistr \\ mid, noncontiguous \\ capacity \\ anoc, instability \\ excl\_pop \\ logmountain \\ t, t^2, t^3 \end{pmatrix} + \epsilon_{2,t} \qquad (6)$$

We created a dummy variable , which is 1 if there was an onset during the period $t-1$ to $t-5$. We also include the log of GDP per capita (*loggdppc*) and the log of population size (*logpop*). In addition, we created an indicator ($redistr$) which captures the amount of income redistribution by the state. It is obtained by calculating the difference between the Gini coefficients of market and disposable incomes. The underlying intuition is that more redistribution should reduce the incentives for challenging the government and thus reduce conflict (see Buhaug, Cederman & Gleditsch, 2014).

To control for interstate conflict, we included a variable (*mid*) which measures whether a country is involved in a military interstate dispute and, if so, its intensity (Marshall, 2016). As proposed by Fearon & Laitin (2003), we include a dummy (*noncontiguous*) indicating whether a country is a noncontiguous state. Moreover, we also expect state capacity of the government to influence the probability of conflict onsets.

The literature has assigned an important role to the type and stability of the political regime in a country (e.g. Fearon & Laitin, 2003; Vreeland, 2008; Sunde & Cervellati, 2013). First, *instability* is a variable that captures whether there was a change in the Polity IV index (Marshall, Gurr & Jaggers, 2016) of three or higher in the previous three years. Second, *anoc* is a dummy which indicates whether a country is an anocracy. Previous research has found countries with these regimes to be particularly prone to intrastate conflict. Furthermore, the share of population that is excluded from political power and participation (*excl_pop*), be it for ethnic, religious or other reasons, and the amount of mountainous terrain (*logmountain*) in a country are expected to increase the probability of an intrastate conflict (e.g. Fearon & Laitin, 2003).

Finally, to deal with possible temporal dependence (Beck, Katz & Tucker, 1998), we included linear, quadratic and cubic polynomials of time (t, t^2, t^3) in our onset equation, as proposed by Carter & Signorino (2010). An overview of all our variables and data sources can be found in Table C.1 in the Online Appendix, where we also offer a more extensive discussion on variable selections, operationalizations and methodological issues. We tested extensively if our results depend on the inclusion of any one of these variables in the import or onset equations.

Discussion of the empirical results

In the following, we present our results on the relation between weapons imports and intrastate conflict. Table II reports second stage results from an instrumental variable probit regression. The main independent variable are arms imports measured as logs of yearly inflows (Models 1 and 2), 5-year moving averages (Models 3 and 4) and 10-year moving averages (Models 5 and 6). The results clearly show that (instrumented) imports have a strong and statistically significant influence on the probability of an onset. The effect is stronger when controlling for the conflict-inducing factors. Effects are more precisely measured when drawing on 5-year or 10-year moving averages, which is plausible as spikes in annual imports are evened out. Assuming the validity of the instrument, the results give a strong empirical support for the hypothesis that arms imports have a positive causal influence on the onset probability of civil war. This effect clearly goes beyond any effect that (latent) intratstate conflict may have on decisions to import weapons. Most control variables show the expected signs. However, *logpop*, *logmountain*, *noncontiguous* and *capacity* are not statistically significant. Interestingly, the share of the excluded population (*excl_pop*) has a strong, positive effect that is highly significant. This corroborates similar findings by Wimmer, Cederman & Min (2009), Cederman, Wimmer & Min (2010) and Wimmer (2013).

< Table II about here >

Drawing on the simultaneous equation approach, a similar picture emerges from our second stage results for conflict onset (Table III). Arms imports have a substantially and statistically very similar influence as in Models 2 and 4 of Table II. Our control variables mostly show the expected signs, while *logmountain*, *noncontigouus*, and *capacity* remain statistically insignificant. Although comparisons to the results obtained by Craft & Smaldone (2002) and Suzuki (2007) are difficult due to very different samples, variable selections and empirical approaches, we find, in general, our findings more in line with the former.

< Table III about here >

Note that these results are average effects of a non-linear model. As Hanmer & Kalkan (2012) have shown, calculating marginal effects on the basis of average variable values creates a weak connection with our hypotheses. We therefore create meaningful cases to make causal inferences. As laid out in our theoretical approach above, we expect imports to matter most when a country is in a situation where conditions for a domestic conflict are present, thus raising the probability of an onset. To examine this possibility, we calculate marginal effects for a high-risk scenario and a low-risk scenario. We define as high-risk, countries with an anocratic regime, a recent conflict onset, at least one bordering country, a change in the Polity IV index of three or higher in the previous three years, an interstate conflict value of four and values for mountainous terrain and share of excluded population that put a country in the 90th percentile of the sample and a low amount of income redistribution, state capacity and GDP per capita (bottom 10% of the sample). The reverse of these values defines low-risk.[9] For further illustrative purposes, we also include the case of Angola, a country that has experienced a number of civil war onsets between 1975 and 2002. Looking at the mean values for 1973-2013, Angola resembles the high-risk case for most independent variables.

< Table IV about here >

As column 1 in Table IV shows, arms imports increase onset probability in the high-risk scenario and the Angolan case. The effect is extremely small in the low-risk case. In quantitative terms, assuming the sample average for arms imports and transforming log-values back into TIVs, we find that an increase in arms imports by 2.7 million TIV raises the onset probability by 0.023 percentage points under the high-risk scenario, whereas the marginal effects for Angola are about 0.013 percentage points. To put this into perspective, the sample mean for arms imports is 195.45 million TIV, which is somewhat lower than civil war torn Angola's average of 267.81 million TIVs. Note that arms imports can change quite strongly over time – upward or downward movements by several hundred million TIVs are not rare. In 1987, for instance, imports to Angola increased by almost 500 million TIVs. When comparing a situation with no imports to the case with the

[9]These values correspond, for example, to Denmark 1991-2013.

sample maximum of imports, we find that for the high-risk case the predicted probability of conflict onset increases from 59%to 80 percent (see Figure 1). In the low-risk case, on the other hand, the marginal effects are much smaller. A change from zero to maximum imports increases onset probability from 0.09 to 0.6%. This finding emphasizes the fact that arms imports to countries in which conditions for civil conflict are not present have almost no impact on onset probability. In other words, arms imports in and of themselves do not cause intrastate conflicts. Note that the marginal effects of the 5- and 10-year backward averages of arms imports are almost identical.

< Figure 1 about here >

With respect to the second stage estimations for the import equation, we provide the results and a more detailed discussion in Online Appendix Table A.1. At this point, a brief summary shall suffice given that our focus is on conflict onsets. What the estimations for the import equation clearly show is that anticipation effects are sizable. Conflict onset in year t significantly increases the amount of weapons imports, thus underlining the simultaneity of imports and conflicts. Ignoring this endogeneity would therefore lead to highly biased inferences.

We are now in a position to relate our empirical findings to our theoretical expectations. The results show that there is indeed a positive, causal effect of arms imports on the probability of violent conflict onsets. Given our estimation strategies, we can rule out that this is simply due to a conflict anticipation effect. In line with our theoretical thinking, weapon inflows strongly influence conflict probability in a high-risk scenario, i.e. in a situation where other conditions make conflict more likely. In the low-risk case, however, all the marginal effects remained extremely small. These results suggests that arms inflows to countries where conflict-inducing conditions are present have an escalating effect and do not discourage potential challengers. In these situations effective deterrence does not seem to be taking place. Rather, conflict initiation becomes more likely. Whether this is due to preemptive motives by the government or rebel groups is beyond the scope of our analysis.

Robustness and validity of the instruments

We test the robustness of our models in a number of ways: We apply alternative estimators (two stage least squares), show results with different assumptions on standard errors, employ alternative definitions for the main dependent variable (*logimports*), and for the conflict onset variable. Overall, we can confirm all of our conclusions. The robustness tests are summarized in more detail in the Online Appendix, which also reports tests on the exogeneity assumption of the IV- and SEM-model. First of all, we test whether *logimports_unrelated* has an independent influence on civil war onset in a SEM model that excludes *logimports_unrelated* in the Z_{im} vector. While all other coefficients, and notably our arms imports coefficient, change only marginally, unrelated weapons show neither a substantive nor a statistical relation with conflict onset (see Table A.4). Secondly, for the SEM-model, we allowed variables only contained in $Z_{im/on}$ and most susceptible to potential endogeneity concerns to influence both the import and the onset equation. Again, our conclusions hold, which makes us confident that both approaches provide internally valid results for the effect of arms imports on conflict onset.

Summary and conclusions

This article systematically analyzed the relationship between the import of major conventional weapons and the onset of intrastate conflict. We focused on weapon transfers to governments as primary recipients of MCWs. Our point of departure was the conceptual and econometric challenge that governments may import arms in anticipating a future conflict outbreak, which would mean that a positive relationship between arms and civil war onset does not imply causation. This inherent endogeneity is the key problem when empirically exploring the relationship between weapons and conflicts. Previous studies have not appropriately dealt with this issue.

Based on theoretical ideas developed initially in the interstate war literature, we first explored several theoretical mechanism relating imports to conflicts. We argued that the availability of arms may increase governments willingness to use them in order to resolve a conflict by violent means. We showed that some crisis bargaining models suggest that arms imports could function as deterrence against potential challengers. However, we maintained that imports could also lead to escalation and thus conflict due to predatory incentives by governments or preemptive motives by rebels. Hence, arms inflows affect the actors' decision to fight because they change the underlying distribution of military capabilities and exacerbate commitment problems. Finally, past arms imports may have reduced investment in state capacities, thus reducing states' abilities to resolve conflicts peacefully. We therefore expected that when controlling properly for the inherent endogeneity, we would still find a positive effect on intrastate conflict onsets.

Testing our theoretical expectations, we found that increases in imports do indeed increase the probability of an intrastate conflict. This suggests that they fuel tensions in a latent conflict and make its outbreak more probable. This effect is quite substantial, but only in high-risk cases, i.e. in situations where conflict-inducing conditions are present. In other words, for arms to have a destabilizing effect, there must be issues of contention between the government and other societal groups. Our results indicate that in such situations, arms imports are not an effective deterrent for governments against potential challengers. From a policy perspective, we would therefore caution against the idea to use weapon exports as a means of stabilizing a country facing a domestic conflict. Based on our results, escalation and violent conflict become more likely. However, note that our econometric approach focuses on the effect of changes in civil-war-related weapon imports induced by our instruments - this local average treatment effect does not necessarily imply similar consequences for variation induced in arms imports through other factors. Particularly, we show that government imports due to motives unrelated to civil war onset make outbreaks more likely. Future research could study whether arms imported with the explicit aim of deterring domestic rivals can achieve this goal.

While we are quite confident in the robustness of our results, we are acutely aware that this can only be the beginning of our research. We only looked at major conventional weapons, which begs the question what role small arms and light weapons could play. We also limited ourselves to conflict onsets, thus ignoring the effect of weapon deliveries on ongoing conflicts. Do they prolong or shorten them? Do they intensify them, leading to more deaths? Providing answers to these questions would bring us a step closer to fully understand the role of the international arms trade in intrastate conflicts.

Data replication

The dataset, codebook, and do-files for the empirical analysis in this article can be found at http://www.prio.org/jpr/datasets.

All analyses were conducted using Stata 14.2.

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Table I. Theoretical Links between Arms Imports and Intrastate Conflict Onset

Incentive	Causal mechanism	Reference	Probablity of onset
	Arms imports and feasibility of force		
opportunity and willingness	opportunity increases willingness	Fordham (2004)	+
	Signalling and deterrence		
signalling resolve	deterrence of challengers	Kreps & Wilson (1982)	-
signalling resolve	preemptive attack by challengers	Lichbach (1998); Walter (2009)	+
signalling capability	deterrence of challengers	Huth (1988); Jervis (1989)	-
signalling capability	preemptive attack by challengers	Powell (2006)	+
large asymmetry in capabilities	predatory motive for government to attack	Chassang & Padró i Miquel (2010)	+
deterrence too costly	preemptive attack by government	Kydd (2015)	+
	Arms imports and state capacities		
military state capacity	deterrence of challengers	Fearon & Laitin (2003)	-
fiscal, legal, redistributive capacity	trade-off arms imports and capacity building	Besley & Persson (2011)	+

Table II. Instrumental Variable Probit Regression: Weapon Imports and Onsets

	(1) onset	(2) onset	(3) onset	(4) onset	(5) onset	(6) onset
logimports	0.054** (0.012)	0.090* (0.043)				
logimports_avg5			0.048** (0.0098)	0.083* (0.034)		
logimports_avg10					0.046** (0.0094)	0.078* (0.032)
onset_last5y		0.63** (0.095)		0.63** (0.094)		0.63** (0.094)
loggdppc		-0.19* (0.077)		-0.18** (0.066)		-0.17** (0.063)
logpop		0.040 (0.063)		0.045 (0.054)		0.052 (0.050)
redistr		-0.042** (0.012)		-0.044** (0.012)		-0.044** (0.012)
mid		0.15** (0.050)		0.15** (0.049)		0.15** (0.049)
anoc		0.26** (0.082)		0.26** (0.082)		0.25** (0.082)
logmountain		0.0038 (0.029)		0.0034 (0.029)		0.0033 (0.029)
noncontiguous		-0.20 (0.18)		-0.18 (0.17)		-0.17 (0.17)
instability		0.35** (0.093)		0.34** (0.093)		0.33** (0.093)
capacity		-0.100 (0.098)		-0.092 (0.097)		-0.091 (0.097)
excl_pop		0.32* (0.15)		0.32* (0.15)		0.34* (0.15)
N	7219	4645	7070	4645	7070	4645
Pseudo-$R2$	0.022	0.29	0.0011	0.14	0.000092	0.12

Instrumental variable probit regression (twostep procedure), logimports (logimports_avg5) instrumented by logimports (logimports_avg5) of civil-war-unrelated major conventional weapons. Standard errors in parentheses. Constant and coefficients for t, t^2 and t^3 not shown.
* (†,**) indicates p < 0.05 (0.1, 0.01)

Table III. Second Stage Results of SEM: Onsets

	(1) onset	(2) onset	(3) onset
logimports	0.075*		
	(0.030)		
logimports_avg5		0.072*	
		(0.029)	
logimports_avg10			0.064*
			(0.028)
onset_last5y	0.66**	0.66**	0.66**
	(0.10)	(0.10)	(0.10)
loggdppc	-0.20**	-0.20**	-0.18**
	(0.066)	(0.064)	(0.062)
logpop	0.11*	0.11*	0.12**
	(0.049)	(0.049)	(0.048)
redistr	-0.042**	-0.043**	-0.043**
	(0.013)	(0.013)	(0.013)
mid	0.22**	0.22**	0.22**
	(0.059)	(0.059)	(0.059)
anoc	0.21*	0.20*	0.19*
	(0.092)	(0.091)	(0.091)
logmountain	0.029	0.029	0.029
	(0.033)	(0.033)	(0.033)
noncontiguous	-0.15	-0.14	-0.12
	(0.19)	(0.19)	(0.19)
instability	0.31**	0.29**	0.28**
	(0.10)	(0.10)	(0.10)
capacity	0.035	0.019	0.014
	(0.11)	(0.11)	(0.11)
excl_pop	0.34*	0.34†	0.35*
	(0.17)	(0.17)	(0.17)
N	3998	3998	3998
Pseudo-R^2	0.18	0.18	0.18

Simultaneous equations model according to Equations 5 and 6. Standard errors in parentheses. Constant and coefficients for t, t^2 and t^3 not shown.
* (†,**) indicates p < 0.05 (0.1, 0.01)

Table IV. Marginal Effects of Arms Imports on Conflict Onset in Different Scenarios

	logimports	*logimports_avg*5	*logimports_avg*10
High-risk	0.02279 (0.00963)	0.0222 (0.0094)	0.02194 (0.00991)
Low-risk	0.00031 (0.00016)	0.00030 (0.00016)	0.00030 (0.00016)
Angola	0.01343 (0.00595)	0.01265 (0.00557)	0.01227 (0.00576)

Note: Marginal effects with standard errors in parentheses. High-risk: *onset_last*5*y*=1, *anoc*=1, *instability*=1, *noncontiguous*=0, *logmountain* =3.985273, *exclpop*=0.458, *redistr*=0.9306297, *capacity*=, 0.478, *loggdppc*=11.33362, *mid*=4, all other variables=mean; Low-Risk: *onset_last*5*y*=0, *anoc*=0, *instability*=0, *noncontiguous* =1, *logmountain* =0, *exclpop*=0, *redistr*=16.62126, *capacity*=1.495, *loggdppc*=14.60336, *mid*=0, all other variables=mean; Angola: *onset_last*5*y*=0.5, *anoc*=0.5897, *instability*=0.111, *noncontiguous*=1, *logmountain* =2.370244, *exclpop*=0.62, *redistr*=2.805, *capacity*=1.782, *loggdppc*=12.60593, *mid*=0, all other variables=mean .

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A Online Appendix for 'The Build-up of Coercive Capacities: Arms Imports and the Outbreak of Violent Intrastate Conflicts'

A.1 Second Stage of the Import Regression

In the following we provide results of the second stage of the import regression: As outlined in the Research Design section of the main text, onset and imports are likely co-determined. Appendix Table A.1 reports the relationship between (binary) conflict onset and the different specifications for the import variables we defined in the main text, given the exclusion restriction underlying our estimation strategy. As can be seen from Models 1-3 of Appendix Table A.1, onset is positively and significantly related to *logimports* (Model 1). The relationship is slightly, though not significantly, weaker with the five year (Model 2) or ten year (Model 3) backward moving averages of *logimports* as dependent variable. This would be expected, as especially short term imports of civil war related weapons should be related to civil war onset. Overall, these results are in line with our theory and plausible given a demand logic, where conflict outbreak increases demand for military equipment, which is then (at least in part) purchased abroad. This relationship is not obvious, as e.g. weapons exporters might limit the export of major conventional weapons to conflict zones. Germany as the fourth largest weapons exporter in 2014 (SIPRI, 2015) e.g. has an explicit policy of embargoing in this regard. At the same time, it has been shown that arms embargoes (with ending civil war being prime stated objective of embargoes (see e.g. data in Brzoska, 2008)) indeed restrain sending states' arms exports (Erickson, 2013).

As expected, the import of unrelated weapons is as well strongly related to weapon imports, and even stronger with the 5 and 10 year moving averages. The other control variables as well behave as expected.

Table A.1. Second Stage Results of SEM: Weapon Imports

	(1) logimports	(2) logimports_avg5	(3) logimports_avg10
onset	0.86**	0.79**	0.71**
	(0.16)	(0.13)	(0.11)
logimports_unrelated	0.20**		
	(0.016)		
logimports_unrelated_avg5		0.33**	
		(0.017)	
logimports_unrelated_avg10			0.42**
			(0.018)
logmilex	0.56**	0.47**	0.40**
	(0.038)	(0.028)	(0.025)
logexport	0.065**	0.062**	0.073**
	(0.019)	(0.015)	(0.013)
loggdppc	0.59**	0.48**	0.41**
	(0.093)	(0.071)	(0.063)
capacity	-0.30†	-0.10	-0.088
	(0.16)	(0.12)	(0.11)
west	-0.97**	-0.42†	-0.35
	(0.32)	(0.24)	(0.22)
eastern europe	-3.15**	-2.70**	-2.54**
	(0.27)	(0.21)	(0.19)
latin america	-1.88**	-1.53**	-1.45**
	(0.23)	(0.17)	(0.15)
subsaharan africa	-2.23**	-1.95**	-1.91**
	(0.22)	(0.17)	(0.15)
asia	-1.24**	-1.12**	-1.08**
	(0.23)	(0.17)	(0.15)
N	3998	3998	3998
Adj R^2	0.52	0.70	0.76

Simultaneous equations model according to equations 5 and 6. Standard errors in parentheses. Constant not shown.
* (†,**) indicates p < 0.05 (0.1, 0.01)

A.2 Robustness and Validity of Instruments

In the following, we describe several tests for the validity of the instruments and robustness tests for our main specifications (Model 4 of Table II and Model 2 of Table III in the main text, and Model 2 of Appendix Table A.1). We begin with presenting a short summary. Subsequently, we describe and present our robustness tests in more detail: First of all, we provide first stage results for the IV-probit and show a test on the exogeneity assumption. Second, we apply less restrictive assumptions on the import and onset equations used in our simultaneous equation model. Third, we demonstrate that alternative modeling choices and assumptions on the structure of error terms lead to similar results. Fourth, we show that our results do not depend on the operationalization of our dependent and independent variables. Results of these tests are summarized in Appendix Table A.2.

A.2.1 Summary of Robustness Tests and Validity of Instruments

We tested the robustness of our results in a number of ways. We examined the exclusion restriction of the probit-model, tested alternative estimators, checked the sensitivity of our SEM approach to different exclusion restrictions and employed different definitions for the onset variable. In all these cases we found our results to be in general very robust. In the following, we summarize the tests[1] we conducted to assess the robustness of our main specifications. These draw specifically on the effects of the *logimports_avg*5 variable (Model 4 of Table II and Model 2 of Table III).

The instrumental variable probit approach we rely on crucially rests on the exogeneity assumption, i.e. that weapons imports of the 'unrelated' type (ships, missiles, anti-aircraft, satellites) affect civil war only through civil-war-related weapons imports. One quantitative test we conducted that supports the exogeneity assumption of the IV approach is reported in Appendix Table A.4: We use the simultaneous equation approach, excluding unrelated weapons in the Z_{im} vector in Model 1. Our substantive findings are unchanged with this approach, indicating that the SEM models works with and without the unrelated weapons variable. Now, in Model 2, we include *logimports_unrelated* as a control variable for civil war onset, i.e. in the Z_{on} vector of the SEM model. As can be seen, all other coefficients, and notably our arms imports coefficient, change only marginally. Importantly, the unrelated weapons variable does not show a substantive or statistical relation with conflict onset in this set-up. This supports our case of an exogenous instrument.

Similarly, as detailed out in Appendix Section A.2.4, the core assumption of the simultaneous equation model implies an exclusion restriction (Maddala, 1983): Variables that only enter the import equation (vector Z_{im}) may statistically not influence the (perceived) likelihood of conflict onset, *other than through increases in weapons inflow*, and vice versa (for vector Z_{on}). As highlighted by Goenner (2011), this is a critical assumption that needs to be justified, similar to a standard instrumental variables approach. In line with Reuveny & Keshk (2013), we predominantly rely on a theoretical justification for our im-

[1] These are fully reported in the following subsections.

port and the onset equations; still, the instruments used may themselves be endogenous. As a robustness test, we therefore allowed variables that are only contained in $Z_{im/on}$ and are most likely susceptible to endogeneity concerns to influence both the import and the onset equation.[2] As can be seen from Panel B of Table A.2, the reported coefficients are close to our main specification, though the standard errors increase slightly with different $Z_{im/on}$ vectors.

Additionally, we report results in Appendix Section A.2.4 and Panel C for a SEM model where our dependent variable contains all imports (related and unrelated) or a broader definition of related weapons. Again, results are very similar, the coefficient is slightly (not significantly) smaller for the 'all MCW' definition.

As reported in Appendix Section A.2.5, a linear probability model would require less modeling assumptions (Angrist & Pischke, 2009; Goenner, 2011) compared to Maddala's SEM or even an instrumental variable probit model. As can be seen from Panel D of Table A.2, results are very similar, as both the SEM, IV probit and IV 2SLS approaches in the end invoke similar assumptions for identifying the average effects of imports. With the 2SLS approach, we as well test different assumption on standard errors (normal, robust, clustered) and do not find that these influence our conclusions. We as well report results from a within estimation using fixed effects logit, and are able to recover our average effects for the *logimports*-variable.

Finally, we show in Panel E that alternative operationalizations of our dependent variable do not change our results: Especially, we recode conflict onset whenever at least one peaceful year preceded the last incidence of this conflict (not two years as in our main specification). Alternatively, we recode ongoing conflicts as zeros (not missings as in our main specification). Results are substantively very similar.

[2] E.g., military expenditure impacted conflict only through imports, military intrastate dispute impacted imports only through conflict in the main specification above. With this robustness test, these variables are allowed to directly influence both import and conflict.

Table A.2. Summary of Robustness Tests

	IV probit models			SEM models		
	β: Imp.→Ons.	SE	Table	β: Imp.→Ons.	SE	Table
A. Main specification						
	0.083	0.034	II-4	0.072	0.029	III-2
B. Alternative exclusion restrictions						
Diff. $Z_{im/on}$ vectors				0.11	0.040	A.5-7
C. Alternative operationalizations of weapon imports						
Broader category of imp.				0.076	0.029	A.6-2
All arms imports				0.066	0.032	A.6-3
D. Different modeling assumptions						
2SLS	0.0082	0.0027	A.7-2	0.0066	0.0025	A.7-4
2SLS (Country-clustered SE's)	0.0082	0.0051	A.8-6			
FE logit	0.14	0.063	A.9-4			
E. Different operationalization of dependent variable						
DV: 1 year pause of conflict	0.085	0.033	A.10-3	0.076	0.028	A.10-5
DV: . → 0	0.078	0.031	A.10-4	0.054	0.027	A.10-6

Estimated coefficients and standard errors for variations of the IV probit models (left panel) and the SEM models (right panel) as fully reported in the respective (appendix) table models (the table column indicates (Appendix.)Table-Model). Panel A reports the core results of our main specification. Panel B reports results from a SEM model estimated with more restrictive Z and less restrictive X vectors (compared to panel A, variables now in X instead of $Z_1/2$ include anoc, mid, logmountain, noncontigouus, instability, excl_pop, logmilex, the region dummies, and logexport). Panel C reports results from for SEM models where the logimport variable is recoded to encompass all MCWs besides anti-submarine missiles, air defense and ships and, respectively, includes all MCWs altogether (civil-war-related and unrelated weapons). Panel D reports results of a linear two-stage least squares modeling strategy (as well with country clustered standard errors) and a fixed effects logit modeling strategy (without instrumentation). Panel E reports results with different operationalizations of the dependent variable (conflict onset is coded whenever at least one peaceful year lies between two conflict years (not two as in our main specification); subsequent conflict-years are coded as zeros (not missings as in our main specification)).

A.2.2 Validity of Instrument in IV Probit

In the following, we first of all report first stage results for the IV-model (Appendix Table A.3), showing that imports of unrelated weapons have a substantively large and statistically strong impact on the import of civil-war related weapons. This influence is independent of control variables. Additionally, it is larger with the 5-year and 10-year moving averages.

Still, the instrumental variable probit approach we rely on crucially rests on the exogeneity assumption, i.e. that weapons imports of the 'unrelated' type (ships, missiles, anti-aircraft, satellites) affect civil war only through civil-war-related weapons imports. One quantitative test we conducted that supports the exogeneity assumption of the IV approach is reported in Appendix Table A.4: We use the simultaneous equation approach, excluding unrelated weapons in the Z_{im} vector in Model 1. Our substantive findings are unchanged with this approach, indicating that the SEM models works with and without the unrelated weapons variable. Now, in Model 2, we include *logimports_unrelated* as a control variable for civil war onset, i.e. in the Z_{on} vector of the SEM model. As can be seen, all other coefficients, and notably our arms imports coefficient, change only marginally. Importantly, the unrelated weapons variable does not show a substantive or statistical relation with conflict onset in this set-up. This supports our case of an exogenous instrument.

Table A.3. First stage results: Influence of unrelated on civil-war-related major conventional weapons imports

	logimports			logimports 5 year			logimports 10 year		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
logimports_unrelated	0.54**	0.25**	0.17**						
	(0.0076)	(0.011)	(0.010)						
logimports_unrelated_avg5				0.81**	0.43**	0.30**			
				(0.0072)	(0.012)	(0.011)			
logimports_unrelated_avg10							0.91**	0.52**	0.38**
							(0.0073)	(0.012)	(0.012)
onset_last5y		0.42**	0.26*		0.24**	0.18*		0.040	0.051
		(0.12)	(0.12)		(0.086)	(0.084)		(0.074)	(0.072)
loggdppc		1.15**	0.76**		0.94**	0.59**		0.82**	0.47**
		(0.049)	(0.078)		(0.036)	(0.056)		(0.033)	(0.047)
logpop		1.06**	0.68**		0.85**	0.58**		0.73**	0.51**
		(0.035)	(0.057)		(0.029)	(0.042)		(0.027)	(0.036)
redistr		0.057**	0.043**		0.077**	0.057**		0.083**	0.065**
		(0.0093)	(0.012)		(0.0064)	(0.0080)		(0.0055)	(0.0069)
mid		0.29**	0.041		0.22**	-0.0016		0.16**	-0.052
		(0.078)	(0.084)		(0.043)	(0.042)		(0.042)	(0.036)
anoc		-0.083	-0.30**		-0.036	-0.26**		-0.018	-0.19**
		(0.10)	(0.11)		(0.072)	(0.071)		(0.062)	(0.060)
logmountain		0.11**	0.0098		0.13**	0.021		0.14**	0.045*
		(0.030)	(0.033)		(0.020)	(0.021)		(0.017)	(0.018)
noncontiguous		1.31**	1.11**		1.31**	1.12**		1.30**	1.10**
		(0.14)	(0.15)		(0.094)	(0.096)		(0.074)	(0.078)
instability		-0.13	-0.22†		0.13	0.17*		0.18*	0.24**
		(0.12)	(0.13)		(0.081)	(0.084)		(0.070)	(0.072)
capacity		0.18†	-0.24*		0.16*	0.031		0.11†	0.051
		(0.11)	(0.12)		(0.071)	(0.073)		(0.062)	(0.063)
excl_pop		1.18**	1.33**		1.24**	1.47**		1.12**	1.43**
		(0.19)	(0.21)		(0.14)	(0.14)		(0.12)	(0.11)
t		0.48**	0.44**		0.33**	0.31**		0.19**	0.18**
		(0.062)	(0.062)		(0.045)	(0.042)		(0.040)	(0.038)
t2		-0.014**	-0.013**		-0.0090**	-0.0087**		-0.0046**	-0.0043**
		(0.0018)	(0.0017)		(0.0013)	(0.0012)		(0.0011)	(0.0011)
t3		0.12**	0.11**		0.067**	0.068**		0.027**	0.027**
		(0.015)	(0.015)		(0.011)	(0.010)		(0.0096)	(0.0091)
logmilex			0.42**			0.37**			0.33**
			(0.047)			(0.038)			(0.033)
logexport			-0.033*			-0.027**			-0.013
			(0.013)			(0.0098)			(0.0088)
west			-1.76**			-1.29**			-1.13**
			(0.21)			(0.15)			(0.12)
eastern europe			-2.80**			-2.61**			-2.60**
			(0.23)			(0.16)			(0.14)
latin america			-1.96**			-1.62**			-1.48**
			(0.16)			(0.12)			(0.093)
subsaharan africa			-1.97**			-1.82**			-1.81**
			(0.18)			(0.12)			(0.10)
asia			-1.16**			-1.07**			-1.08**
			(0.17)			(0.12)			(0.10)
Constant	4.31**	-30.3**	-19.2**	3.60**	-24.3**	-15.6**	3.33**	-20.3**	-12.4**
	(0.052)	(0.99)	(1.47)	(0.042)	(0.78)	(1.07)	(0.038)	(0.73)	(0.91)
N	8580	5614	4833	8415	5614	4833	8415	5614	4833
Adj R^2	0.26	0.50	0.55	0.47	0.68	0.74	0.54	0.74	0.79

First stage regression of unrelated on civil war related conventional weapons import (logged) with different control variable sets (none in Model 1; control variables for onset (Model 2); determinants of import in SEM-approach (Model 3)). Standard errors in parentheses.
* (†,**) indicates p < 0.05 (0.1, 0.01)

Table A.4. SEM model with and without control for civil-war-unrelated arms

	(1) onset	(2) onset
logimports	0.071*	0.068†
	(0.032)	(0.038)
logimports_unrelated		0.0038
		(0.014)
onset_last5y	0.66**	0.66**
	(0.10)	(0.10)
loggdppc	-0.20**	-0.20**
	(0.067)	(0.068)
logpop	0.12*	0.12*
	(0.050)	(0.050)
redistr	-0.042**	-0.041**
	(0.013)	(0.013)
mid	0.22**	0.22**
	(0.059)	(0.059)
anoc	0.20*	0.20*
	(0.092)	(0.092)
logmountain	0.030	0.030
	(0.033)	(0.033)
noncontiguous	-0.14	-0.14
	(0.19)	(0.19)
instability	0.32**	0.31**
	(0.10)	(0.10)
capacity	0.033	0.034
	(0.11)	(0.11)
excl_pop	0.34*	0.35*
	(0.17)	(0.18)
t	-0.0070	-0.011
	(0.071)	(0.071)
t2	0.00062	0.00073
	(0.0020)	(0.0020)
t3	-0.0076	-0.0086
	(0.017)	(0.017)
_cons	-1.61	-1.49
	(1.56)	(1.58)
N	3998	3998
Pseudo R^2	0.18	0.18

SEM model as in Table III, excluding unrelated weapons as predictive variable from $\boldsymbol{Z_{im}}$ (Model 1 and 2) and including these as additional control variable in $\boldsymbol{Z_{on}}$ (Model 2). Standard errors in parentheses.
* (†,**) indicates p < 0.05 (0.1, 0.01)

A.2.3 Alternative Specifications of the SEM

One of the core assumption of the simultaneous equation model we use is the implied exclusion restriction: Variables that only enter the import equation (vector Z_{im}) may statistically not influence the (latent) likelihood of conflict onset, *other than through increases in weapons inflow*, and vice versa (for vector Z_{on}). This is a critical assumption that needs to be justified, similar to a standard instrumental variables approach (as e.g. highlighted by Goenner, 2011). In line with Reuveny & Keshk (2013), we predominantly rely on a theoretical justification for our import and onset equations; still, the instruments used may themselves be endogenous.

Appendix Table A.5 displays results for various tests on this assumption. Model 1 and 2 contain the baseline specification for logimports (5 year average) and conflict onset. Model 3 and 4 report results with the variable *logimports_unrelated* dropped from the import equation. This does not change results.

As further robustness test, we define a new variable vector X that contains all variables that enter both the import and the onset equation; thus, these variables are allowed to have a direct impact on *logimports* (*onsets*) besides their indirect influence through increases in onset likelihood (*logimports*). In our main specification this is the case for *loggdppc* and *capacity*.

$$Y_1 = \delta_1 Y_2^* + \beta_1' \boldsymbol{Z_{im}} + \gamma_1 \boldsymbol{X} + u_1 \quad (1)$$

$$Y_2^* = \delta_2 Y_1 + \beta_2' \boldsymbol{Z_{on}} + \gamma_2 \boldsymbol{X} + u_2 \quad (2)$$

In a first step, we shifted those variables from $Z_{im/on}$ to X that are most likely susceptible to endogeneity concerns for our main specification. These are *logmilex* from $\boldsymbol{Z_{im}}$ and *ucdp_last*5*y*, *mid* and *instability* from $\boldsymbol{Z_{on}}$.[3] As reported in Model 3 and 4 of Appendix Table A.5, including these variables in X does not change our substantive findings for the onset and import equations: Imports are positively and significantly (although on the 10%-level) related to the likelihood of conflict onset, and vice versa. Note that the first stage equations do not change, as still the *full* set of variables is used there, before plugging in the fitted values in the second stage (with now different control vectors).

In a second step, we explored the boundaries of our approach with additional modifications of the import/onset equation: Regarding the import side, our results hold as long as *logmilex* and/or the *logimports_unrelated_avg*5 are contained in Z_{im}. In other

[3] A variable contained only in the import equation (Z_{im}) that might be linked to conflict onset through channels other than weapons imports is especially military expenditure (recognizing e.g. that larger expenditure might similarly signal resolve and thus influence conflict onset directly). Additionally, variables contained in the onset equation (Z_{on}) that might be linked to weapons imports through channels other than the (perceived) likelihood of conflict are in particular past onset (e.g. via a logic of replenishing weapons stock), military interstate disputes (recognizing that external conflict might drive imports even when controlling for military expenditure (Dunne & Perlo-Freeman, 2003)), and instability (recognizing e.g. that unstable regimes might especially need to appease the military).

words, we assume that logged military expenditure and/or the unrelated imports influence conflict only through imports. Regarding the onset side, our results hold as long as we are correct in assuming that *logpop* and *redistr* are contained only in Z_{on}. Importantly, the direction of effects depends on log population size, so our findings concerning the import equation depend on whether and to what degree logged population size likely affects weapons imports through channels other than conflict likelihood.

A.2.4 Alternative Main Independent Variable

As a further robustness test, we use alternative specifications of our main independent variable. Model 2 of Appendix Table A.6 contains a specification where we consider the import of all MCWs, besides anti-submarine, air defence, ships and satellites (*logimports_alt*). This does not influence our results. We arrive at a similar conclusion with Model 3, where we report results for an import variable containing all MCW imports (all, as well civil-war unrelated, weapon types: *logimports_all*). Model 1 reports our baseline specification for relevant MCWs (only aircraft, armoured vehicles, artillery, missiles: *logimports*).

Table A.5. SEM with less restrictive specifications

	Baseline		SEM-Variant 1		SEM-Variant 2		SEM-Variant 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	onset	logimports_avg5	onset	logimports_avg5	onset	logimports_avg5	onset	logimports_avg5
logimports_avg5	0.072*		0.065*		0.061†		0.11**	
	(0.029)		(0.033)		(0.036)		(0.040)	
onset		0.79**		1.30**		1.01**		1.68**
		(0.13)		(0.17)		(0.23)		(0.46)
logimports_unrelated_avg5		0.33**				0.33**		0.27**
		(0.017)				(0.022)		(0.037)
onset_last5y	0.66**		0.66**		0.66**	-0.29	0.62**	-0.78*
	(0.10)		(0.10)		(0.10)	(0.22)	(0.10)	(0.38)
loggdppc	-0.20**	0.48**	-0.18**	0.58**	-0.20**	0.52**	-0.15†	0.76**
	(0.064)	(0.071)	(0.066)	(0.100)	(0.064)	(0.088)	(0.081)	(0.14)
capacity	0.019	-0.10	0.019	0.011	0.022	-0.069	-0.080	0.056
	(0.11)	(0.12)	(0.11)	(0.17)	(0.11)	(0.14)	(0.11)	(0.21)
logpop	0.11*		0.12*		0.11*		0.093	
	(0.049)		(0.052)		(0.049)		(0.062)	
redistr	-0.043**		-0.043**		-0.043**		-0.033*	
	(0.013)		(0.013)		(0.013)		(0.015)	
mid	0.22**		0.23**		0.22**	-0.14	0.23**	-0.41**
	(0.059)		(0.059)		(0.059)	(0.10)	(0.060)	(0.16)
anoc	0.20*		0.20*		0.20*		0.20*	-0.65**
	(0.091)		(0.092)		(0.091)		(0.093)	(0.18)
logmountain	0.029		0.030		0.028		0.043	-0.12†
	(0.033)		(0.033)		(0.033)		(0.035)	(0.067)
noncontiguous	-0.14		-0.12		-0.14		-0.21	1.49**
	(0.19)		(0.19)		(0.19)		(0.20)	(0.33)
instability	0.29**		0.29**		0.29**	-0.11	0.27**	-0.36
	(0.10)		(0.10)		(0.10)	(0.15)	(0.10)	(0.23)
excl_pop	0.34†		0.34*		0.36*		0.22	0.76*
	(0.17)		(0.17)		(0.18)		(0.18)	(0.38)
logmilex		0.47**		0.62**	0.017	0.45**		0.41**
		(0.028)		(0.039)	(0.029)	(0.035)		(0.056)
logexport		0.062**		0.051*		0.059**	-0.0016	0.029
		(0.015)		(0.021)		(0.017)	(0.016)	(0.027)
west		-0.42†		-0.50		-0.23	-0.38	0.40
		(0.24)		(0.35)		(0.32)	(0.27)	(0.52)
eastern europe		-2.70**		-3.47**		-2.68**	0.42†	-2.28**
		(0.21)		(0.29)		(0.25)	(0.25)	(0.38)
latin america		-1.53**		-1.89**		-1.48**	-0.053	-1.21**
		(0.17)		(0.24)		(0.20)	(0.19)	(0.30)
subsaharan africa		-1.95**		-2.67**		-1.97**	0.33†	-1.92**
		(0.17)		(0.23)		(0.20)	(0.19)	(0.30)
asia		-1.12**		-1.11**		-1.13**	0.085	-0.76**
		(0.17)		(0.23)		(0.19)	(0.17)	(0.29)
N	3998	3998	3998	3998	3998	3998	3998	3998
Pseudo R^2	0.18		0.18		0.18		0.19	
Adjusted R^2		0.70		0.64		0.70		0.73

Simultaneous equations model with less restrictive sets of variables for import and onset equations. Standard errors in parentheses. Constant not shown.

* (†,**) indicates p < 0.05 (0.1, 0.01)

Table A.6. SEM with different weapons types

	(1) onset	(2) onset	(3) onset
logimports_avg5	0.072* (0.029)		
logimports_alt_avg5		0.076* (0.029)	
logimports_all_avg5			0.066* (0.032)
onset_last5y	0.66** (0.10)	0.66** (0.10)	0.66** (0.10)
loggdppc	-0.20** (0.064)	-0.20** (0.066)	-0.19** (0.067)
logpop	0.11* (0.049)	0.11* (0.050)	0.12* (0.051)
redistr	-0.043** (0.013)	-0.044** (0.013)	-0.043** (0.013)
mid	0.22** (0.059)	0.22** (0.059)	0.22** (0.059)
anoc	0.20* (0.091)	0.21* (0.092)	0.20* (0.091)
logmountain	0.029 (0.033)	0.028 (0.033)	0.031 (0.033)
noncontiguous	-0.14 (0.19)	-0.15 (0.19)	-0.11 (0.19)
instability	0.29** (0.10)	0.29** (0.10)	0.29** (0.10)
capacity	0.019 (0.11)	0.013 (0.11)	0.0089 (0.11)
excl_pop	0.34† (0.17)	0.33† (0.17)	0.34* (0.17)
N	3998	3998	4015
Pseudo R^2	0.18	0.18	0.18

Simultaneous equations model of Equations 5 and 6, using different weapons types for measuring imports. Standard errors in parentheses. Constant not shown. * (†,**) indicates p < 0.05 (0.1, 0.01)

A.2.5 Alternative Instrumental Variables Approach, Model Misspecification and Assumptions for Standard Errors

As addressed above, the main econometric issue involved is the endogeneity between weapons trade and conflict, especially the potentially simultaneous relationship between weapon trade and (latent) conflict onset. We expect this to be less a problem given our proposed instrument and the simultaneous modeling approach, with a strong theoretical justification for both the import as well as the conflict equation. Other issues involved concern the modeling choices.

Given the trade-off in adequately addressing the simultaneity problem and the modeling assumptions necessary, we chose both an instrumental variables probit approach (as argued for e.g. by Goenner, 2011) and the SEM approach of Maddala (1983). Alternatives to the IV probit would have been a two stage estimation of a linear probability model (which is less dependent on functional form assumptions (Angrist & Pischke, 2009: 198ff.)). This approach is econometrically very similar to the IV probit and the second stage of onset equation of the SEM approach.

As reported in Eq.5 in the Research design section in the main text, we explain logged arms import by the log of unrelated weapon imports, logged military expenditure, weapons exports, GDP per capita, and region dummies. Appendix Table A.7 shows results for an instrumental variable approach with two stage least squares where either this full set or only the logged unrelated weapon imports explain imports in the first stage and the predicted import values are then used for the second stage onset explanation. Depending on the specification we include additional controls for the variables in the SEM onset equation (Eq.6 of the main text).

The results for the import-variable have to be interpreted as a local average treatment effect: For an average complier, i.e. for an average state that increases its 5-year average import of civil war related weapons due to the import of civil war unrelated arms, the probability of onset increases by 0.22% to 0.82% (depending on specification) for every logged million of TIV.

Additionally, assumptions on the correlation of error terms might influence the interpretation of results. Our main results for the IV probit draws on Newey's two-step estimator, which does not allow for robust or clustered standard errors. For the two-stage least squares approach above, we therefore report results with normal, robust and country-clustered standard errors in order to assess to what extent the significance of our results might be influenced by assumptions on the error structure. Appendix Table A.8 reports results. Only for the 5-year arms imports specification in the country-clustered standard error case does the import-coefficient loose its significant influence on conflict onset.

Finally, we show results of a fixed effects logit regression in reporting the correlation of weapons trade and conflict drawing on the onset regression of the SEM above and without correction for the potential simultaneity involved (see Appendix Table A.9). Both for the yearly and 5-year-average imports variable, the coefficient is substantively and statistically

Table A.7. Instrumental Variable 2SLS Regression: Weapon Imports and Onsets

	Instrument: Unrelated Weapons		Instrument: SEM import variables	
	(1)	(2)	(3)	(4)
	onset	onset	onset	onset
logimport_avg5	0.0045**	0.0082**	0.0022*	0.0069**
	(0.00089)	(0.0027)	(0.0011)	(0.0025)
onset_last5y		0.11**		0.12**
		(0.011)		(0.012)
loggdppc		-0.017**		-0.018**
		(0.0051)		(0.0050)
logpop		0.0046		0.011*
		(0.0043)		(0.0042)
redistr		-0.0017*		-0.0014†
		(0.00070)		(0.00072)
mid		0.023**		0.037**
		(0.0063)		(0.0076)
anoc		0.019*		0.010
		(0.0076)		(0.0081)
logmountain		-0.00045		0.00094
		(0.0022)		(0.0023)
noncontiguous		-0.015		-0.011
		(0.012)		(0.012)
instability		0.038**		0.029**
		(0.0094)		(0.010)
capacity		-0.0064		0.0030
		(0.0078)		(0.0085)
excl_pop		0.026†		0.026
		(0.014)		(0.016)
t		0.00030		-0.00034
		(0.0046)		(0.0050)
t2		0.000022		0.000040
		(0.00013)		(0.00014)
t3		-0.00028		-0.00044
		(0.0011)		(0.0012)
Constant	0.014**	0.14	0.032**	0.063
	(0.0054)	(0.12)	(0.0072)	(0.12)
N	7070	4645	4234	3998
Adjusted R^2	.	0.061	0.0021	0.074
p-value for Durbin χ^2 statistic	0.00030	0.060	0.27	0.11
First stage adjusted R^2	0.46	0.70	0.69	0.75
First stage partial R^2	0.46	0.22	0.69	0.32

Instrumental variable 2SLS regression. Logimports_avg5 are instrumented by civil-war-unrelated logimports_avg5 in Models 1 and 2. Im Model 3 and 4 logimports_avg5 are instrumented additionally by all variables included in the SEM import equation. Models include controls as indicated. Standard errors in parentheses.
* (†,**) indicates p < 0.05 (0.1, 0.01)

Table A.8. Instrumental Variable 2SLS Regression: Weapon Imports and Onsets

	Normal se's		Robust se's		Country cloustered se's	
	(1)	(2)	(3)	(4)	(5)	(6)
	onset	onset	onset	onset	onset	onset
logimports	0.0087*		0.0087*		0.0087†	
	(0.0034)		(0.0036)		(0.0049)	
logimports_avg5		0.0082**		0.0082**		0.0082
		(0.0027)		(0.0029)		(0.0051)
onset_last5y	0.11**	0.11**	0.11**	0.11**	0.11**	0.11**
	(0.011)	(0.011)	(0.019)	(0.019)	(0.032)	(0.031)
loggdppc	-0.019**	-0.017**	-0.019**	-0.017**	-0.019*	-0.017†
	(0.0060)	(0.0051)	(0.0062)	(0.0051)	(0.0093)	(0.0090)
logpop	0.0039	0.0046	0.0039	0.0046	0.0039	0.0046
	(0.0050)	(0.0043)	(0.0052)	(0.0042)	(0.0068)	(0.0063)
redistr	-0.0016*	-0.0017*	-0.0016*	-0.0017*	-0.0016	-0.0017
	(0.00070)	(0.00070)	(0.00064)	(0.00065)	(0.00100)	(0.0011)
mid	0.023**	0.023**	0.023*	0.023*	0.023	0.023
	(0.0063)	(0.0063)	(0.012)	(0.012)	(0.019)	(0.019)
anoc	0.019*	0.019*	0.019*	0.019*	0.019†	0.019†
	(0.0076)	(0.0076)	(0.0094)	(0.0094)	(0.011)	(0.011)
logmountain	-0.00054	-0.00045	-0.00054	-0.00045	-0.00054	-0.00045
	(0.0022)	(0.0022)	(0.0020)	(0.0019)	(0.0028)	(0.0029)
noncontiguous	-0.017	-0.015	-0.017	-0.015	-0.017	-0.015
	(0.012)	(0.012)	(0.010)	(0.0094)	(0.017)	(0.016)
instability	0.039**	0.038**	0.039**	0.038**	0.039**	0.038*
	(0.0094)	(0.0094)	(0.013)	(0.013)	(0.015)	(0.015)
capacity	-0.0060	-0.0064	-0.0060	-0.0064	-0.0060	-0.0064
	(0.0078)	(0.0078)	(0.0083)	(0.0083)	(0.011)	(0.011)
excl_pop	0.026†	0.026†	0.026	0.026	0.026	0.026
	(0.014)	(0.014)	(0.018)	(0.018)	(0.028)	(0.029)
t	-0.0013	0.00030	-0.0013	0.00030	-0.0013	0.00030
	(0.0048)	(0.0046)	(0.0045)	(0.0043)	(0.0051)	(0.0048)
t2	0.000074	0.000022	0.000074	0.000022	0.000074	0.000022
	(0.00014)	(0.00013)	(0.00013)	(0.00012)	(0.00015)	(0.00014)
t3	-0.00077	-0.00028	-0.00077	-0.00028	-0.00077	-0.00028
	(0.0012)	(0.0011)	(0.0011)	(0.0011)	(0.0013)	(0.0012)
Constant	0.17	0.14	0.17	0.14	0.17	0.14
	(0.14)	(0.12)	(0.14)	(0.11)	(0.17)	(0.15)
N	4645	4645	4645	4645	4645	4645
Adjusted R^2	0.060	0.061	0.060	0.061	0.060	0.061

Instrumental variable 2SLS regression, logimports (logimports_avg5) instrumented by logimports (logimports_avg5) of civil-war-unrelated major conventional weapons. Standard errors in parentheses corrected as indicated.
* (†,**) indicates p < 0.05 (0.1, 0.01)

similar to the IV probit or SEM estimates. As the fixed effects estimator draws on the variation within cases, we expect omitted variables to be less a problem in this setup. Even when additionally controlling for military expenditure, although substantively smaller and just not significant on the 10% level for the yearly variable, our results hold. This might indicate that our results could be interpreted more broadly, beyond the variation in the *logimports* variable induced by the instruments, or that a similar type of bias affects both the SEM, the IV and fixed-effects approaches for the relationship between *logimports* and *onset*.

Table A.9. Fixed Effects Logit: Weapon Imports and Onsets

	(1) onset	(2) onset	(3) onset	(4) onset
logimports	0.11**	0.076*		
	(0.033)	(0.038)		
logimports_avg5			0.092†	0.14*
			(0.052)	(0.063)
logmilex		0.34†		0.31
		(0.20)		(0.20)
onset_last5y	0.16	0.11	0.15	0.11
	(0.22)	(0.24)	(0.22)	(0.24)
loggdppc	0.075	-0.22	0.15	-0.22
	(0.29)	(0.34)	(0.29)	(0.34)
logpop	0.29	0.17	0.29	0.19
	(0.87)	(0.99)	(0.87)	(1.00)
redistr	-0.065	-0.037	-0.066	-0.034
	(0.061)	(0.063)	(0.061)	(0.063)
mid	0.61**	0.56**	0.60**	0.56**
	(0.16)	(0.18)	(0.16)	(0.18)
anoc	0.65**	0.56*	0.67**	0.58*
	(0.23)	(0.27)	(0.23)	(0.27)
noncontiguous	-1.74	-1.95	-1.82	-1.92
	(1.55)	(1.61)	(1.55)	(1.61)
instability	0.81**	0.64*	0.79**	0.61*
	(0.22)	(0.25)	(0.22)	(0.25)
capacity	-0.20	-0.21	-0.22	-0.25
	(0.34)	(0.39)	(0.33)	(0.38)
excl_pop	-0.72	-1.03	-0.77	-1.15
	(0.57)	(0.72)	(0.57)	(0.74)
t	0.060	0.043	0.079	0.012
	(0.15)	(0.17)	(0.15)	(0.17)
t2	-0.00021	-0.00015	-0.00083	0.00058
	(0.0042)	(0.0047)	(0.0042)	(0.0047)
t3	-0.0075	-0.0072	-0.0019	-0.013
	(0.036)	(0.041)	(0.036)	(0.041)
N	2738	2023	2738	2023
Pseudo R^2	0.075	0.060	0.066	0.061

Fixed effects logit estimation for logimports (logimports_avg5) of civil-war-related major conventional weapons on conflict onset. Standard errors in parentheses.
* (†,**) indicates p < 0.05 (0.1, 0.01)

A.2.6 Alternative Operationalizations

Finally, we show that alternative operationalizations of our dependent variable do not change our results. Model 1-4 of Appendix Table A.10 report equivalent estimates for Table II, Models 3-4, in the main text with different coding of onset. Respectively, Model 5 and 6 report results for the main SEM specification Table III. Onset is either coded as one whenever at least one peaceful year lies between two conflict-years (not two as in our main specification) in Model 1, 2 and 5 or ongoing conflict-years are coded as zero (not missing as in our main specification) (in Model 3, 4, 6). Results as substantively very similar, with the 'ongoing conflict coded as zero'-variant substantively slightly smaller – as expected, when ongoing conflict years, with different dynamics for weapon imports, potentially confound the control condition of 'no onset'.

Table A.10. Alternative Operationalizations of Conflict Onset

	IV probit				SEM	
	(1)	(2)	(3)	(4)	(5)	(6)
	onset_alt	onset_alt	onset_zeros	onset_zeros	onset_alt	onset_zeros
logimports_avg5	0.048**	0.085*	0.042**	0.078*	0.076**	0.054*
	(0.0093)	(0.033)	(0.0097)	(0.031)	(0.028)	(0.027)
onset_last5y		0.86**		0.14†	0.91**	0.16†
		(0.087)		(0.082)	(0.094)	(0.088)
loggdppc		-0.20**		-0.18**	-0.23**	-0.18**
		(0.064)		(0.059)	(0.061)	(0.057)
logpop		0.047		0.028	0.10*	0.10*
		(0.052)		(0.050)	(0.047)	(0.045)
redistr		-0.043**		-0.035**	-0.041**	-0.031**
		(0.011)		(0.010)	(0.012)	(0.011)
mid		0.14**		0.076†	0.23**	0.100*
		(0.049)		(0.043)	(0.059)	(0.047)
anoc		0.29**		0.18*	0.25**	0.14†
		(0.078)		(0.076)	(0.086)	(0.084)
logmountain		0.011		-0.018	0.033	0.0073
		(0.028)		(0.027)	(0.031)	(0.031)
noncontiguous		-0.30†		-0.062	-0.28	0.011
		(0.16)		(0.16)	(0.17)	(0.17)
instability		0.35**		0.23**	0.31**	0.21*
		(0.089)		(0.085)	(0.097)	(0.093)
capacity		-0.11		-0.072	-0.018	0.014
		(0.093)		(0.088)	(0.10)	(0.10)
excl_pop		0.31*		0.14	0.32*	0.18
		(0.15)		(0.14)	(0.16)	(0.16)
_cons	-1.97**	-0.030	-2.09**	-0.25	-0.38	-1.32
	(0.061)	(1.49)	(0.065)	(1.42)	(1.45)	(1.37)
N	7083	4657	8315	5614	4010	4833
p-value of Wald exogeneity test	0.00054	0.16	0.00073	0.031		
Pseudo R^2					0.23	0.10

Instrumental variable probit regression using twostep procedure (Model 1-4). SEM (Model 5-6). Standard errors in parentheses.
* (†,**) indicates p < 0.05 (0.1, 0.01)

B General notes on variable selections, operationalizations and methodological issues

B.1 Choice of variables in import and onset equations of the SEM models

We chose the variables in the two SEM equations based on theoretical rationales and previous empirical research. In particular, our onset model is based on the seminal contribution by Fearon & Laitin (2003). We used some better measurements for some of their variables (especially with respect to ethnicity) and included some additional controls suggested by some more recent studies. Additionally we have tested some variables proposed by the literature. In aiming for a sparse model, we did not include them, however, if they had no explanatory power. We also show the results for our import variables without any controls in the specification. Note that in the first stage of the SEM the full set of variables is included in both equations (Equations 5 and 6 in the article refer to the second stage). We find that interstate conflict has no significant effect on imports if we include it in the import equation (probably because it is strongly related to military expenditures). In the robustness section, we show that including the variable in both second stage equations does not change our results overall.

B.2 Arms races and weapon types

We explicitly control for interstate conflicts in our empirical analyses and we also distinguish between MCW that are relevant for domestic conflict and those that are only relevant for interstate conflicts. Our empirical analysis confirms that this distinction is valid. Importing weapons not related to civil wars such as submarines and air defenses has no effect on onset probabilities. This could suggest that rebels are good in interpreting the purpose of weapon purchases by governments. This may lower the likelihood that they misread a government's intent.

B.3 Inclusion of regional effects

We tested different measures of conflict in neighboring countries using the MEPV data but found them to be irrelevant. The region dummies capture possible regional arms races. They are included in the first-stage onset equations, where only the dummy for western countries is significant. Previous research on intrastate conflict has not used region dummies to explain onsets, which is why we only included it in the second-stage import equation. We tested their inclusion in the second-stage onset equation and found none of the dummies significant at the 5%-level.

Table B.1. Descriptive statistics for intervention in domestic affairs mentioned in an alliance treaty

	1815-1945	1945-2013	Total
no mention	220	226	446
mutual non-intervention	12	268	280
under certain circumstances (e.g. protect against rebels)	4	0	4
one or more states can intervene, not reciprocally	8	2	10
one or more states, but not all, promise non-intervention	0	2	2
Total	244	498	742

B.4 Inclusion of arms exports

Arms imports may result from the fact that a country has a domestic arms industry and is therefore engaged in intra-industry trade with other countries. This potentially import driver is captured by including arms exports. Consequently, it also controls for the fact that some countries may have the ability to produce the desired weaponry themselves. With respect to the variable's explanatory weight, leaving this variable out has virtually no effect on the results. For example, in the SEM model for annual imports, the coefficient is still 0.75 with a similar statistical significance.

B.5 Further factors that could explain arms imports

1. Arms imports may be due to alliance obligations

   A variable potentially omitted in our analysis are interstate alliance structures. For our results to be biased, alliances would need to be related both to civil-war onset and to weapons imports. While the latter clearly matters, the former is less obvious. We cannot think of compelling reasons why this should be the case, as in most alliances there is no provision included to assist in cases of internal power struggles. We checked for this assumption in the extensive alliance-year data set on Alliance-Treaty-Obligations of (Leeds et al., 2002). Among the 498 alliances founded since 1945, only in rare cases is mutual or one-sided intervention allowed. Non-intervention became a dominant provision, however, compared to the pre-1945. Table B.1 provides the descriptive statistics for intervention in domestic affairs mentioned in an alliance treaty.

2. Arms imports may be due to corruption

Corruption may be a variable related to weapon imports as well as conflict onset. We checked whether its in- or exclusion changes our estimates and found this not to be the case.

3. Arms imports may serve to insure the loyalty of the military

   We see within-country military loyalty rather as an implied mechanism than an omitted variable. Weapons imports are increasing the military capacity of the state. Whether they do this via strengthening the military capacity directly (via firepower) or indirectly (via loyalty) is not directly addressed by our analysis.

B.6 Why not include the armarment of rebels?

Our theoretical discussion and the empirical analysis do not explicitly consider the armament of rebel groups. There is a theoretical and a practical reason for this decision. Governments are the main recipient of major conventional weapons (MCW) prior to a conflict outbreak. This is no surprise as they are normally in control of the border and large-scale weapon systems are much harder to smuggle than small arms. Observable arms flows are therefore predominantly going to governments (although this often changes after a civil war begins). This brings us to the second, practical point. Since arms transfers to rebels are usually illegal, they are much harder to detect. As a result, we have much less data on transfers to rebels and expect systematic measurement errors to be a huge problem. We often only have information on rebel groups that engaged in military conflicts. Given that we want to explain the outbreak of violence, this would risk massively overestimating the effect of (rebel) arms import on onsets.

We would have liked to include rebel group capacity as one of the factors affecting conflict risk. However, existing data only captures relative rebel strength in comparison to governments (see Cunningham & Salehyan, 2009). These are merely of an ordinal scale and cover only non-state actors that were engaged in military conflicts. Given that our study aims at estimating the average effects of weapon imports on conflict onset, using these data would likely lead to a serious selection-bias, since groups not involved in a violent conflict are not observed. Employing a (non-linear) instrumental variable set-up and a (non-linear) simultaneous equation model, we report an average positive effect of imports on onset. As we cannot condition our effect on (unobservable) rebel group capacity, we estimate marginal effects for countries at high-risk and low-risk levels for civil conflict. As expected, the effect of weapons import on conflict is small where conditions for conflict are non-existent, but large where conditions for conflict are met. We believe that our estimates provide convincing evidence that arms imports are indeed fueling conflict when conditions for conflict on the ground are met.

B.7 Why not also analyze small arms and light weapons?

Analyzing both MCW and SALW imports would be the optimal approach. However, there are practical reasons (especially data availability, as the data collected by NISAT on small arms covers a much shorter period and their work on rebel acquisition of small arms has not gotten very far yet) but, as we explain in the article, there are also theoretical reasons why we focus on MCW imports of governments.

B.8 Why not use military expenditure instead of arms imports?

There are two problems with military expenditures. First, as Pamp & Thurner (forthcoming) have shown empirically, increasing imports may not be reflected in corresponding changes in military budgets, especially in developing countries. This can be due to different financing modes (including debt finance and barter trade) and the fact that especially during the Cold War military aid played an important role. Second, military budgets often capture many other aspects such as health care and pension commitments. Furthermore, definitions of what is included in defense spending vary between countries and over time. We could therefore observe significant changes in military spending that do not reflect changes in underlying military capabilities. This justifies our approach focusing on arms imports.

B.9 Why not use military stocks instead of arms imports?

We would expect the (difficult to measure) level of military equipment to also have an effect on civil war. It is not clear how to get reliable figures for military stocks however, as we would need to have baseline levels of military goods and estimates of home production, in addition to estimates of imports and assumptions on depreciation. Moreover, military stock will likely be an endogenous function of perceived threat levels as well. We remain agnostic as to whether governments increase (or decrease) imports after a domestic threat has subsided. However, we address part of this issue implicitly by estimating effects for 5-year and 10-year moving averages of weapon imports. Especially the 10-year average (which by definition is one tenth of the sum of all weapon imports in the last 10 years) may provide insights into the effects of military stocks on civil war onset. As reported in main Table 2, this effect is significant and positive.

B.10 Predictive power of our models

With respect to the assessment of predictive power using classification tables, as is the case with all binary choice models (see Muchlinski et al. (2016) specifically with respect to intrastate conflict), they are very bad at predicting conflicts (in-sample as well as out-sample). While this results in a very low rate of false positives, it also results in a very low number of correctly predicted onsets. In our case, there is the additional complication that we use instruments/predicted values for our import variables. This necessarily reduces the predictive power of such a model further. We therefore think that predictive success is not an appropriate benchmark for our modeling approach and is potentially even misleading. If predictive success were the benchmark, then alternative approaches like Random Forest machine learning models would be required. However, our aim in this article is to identify whether there is truly a causal link between arms imports and conflict onsets when considering potential simultaneity.

C Variables and Sources

Table C.1 gives an overview on the operationalization of the variables used in our analysis and their sources.

Table C.1. Variable overview

Variable	Description	Source
onset	Onset dummy (more than two years since the last observation of the conflict dyad)	UCDP/PRIO
onset_1y	Onset dummy (more than one year since the last observation of the conflict dyad)	UCDP/PRIO
logimports	ln(Imports of civil-war related arms, i. e. aircraft, armoured vehicles, artillery and missiles, in TIV)	SIPRI
logimports_alt	ln(Alernative specification of imports of civil-war related arms, i. e. all MCWs besides anti-submarine, air defence, ships and satellites, in TIV)	SIPRI
logimports_unrelated	ln(Imports of civil-war unrelated arms, i. e. ships, anti-submarine weapons, air-defense and satellites, in TIV)	SIPRI
logimports_all	ln(Imports of MCW, in TIV)	SIPRI
import variable_*avg*5/10	average of the five/ten preceeding years of the respective import variable	SIPRI
onset_last5y	Onset of intrastate conflict in the last 5 years	
logexport	ln(Arms exports, in TIV)	SIPRI
loggdppc	ln(Real GDP per capita, 2005 prices)	Gleditsch/PWT
logpop	ln(Population)	Gleditsch/PWT
logmilex	ln(Military expenditure, constant PPP US$)	SIPRI
redistr	Market Gini - net Gini	SWIID/LIS
mid	sum of interstate violence and warfare intensity	MEPV
anoc	Anocracy based on Polity2, Polity IV	
instability	Change in Polity IV index of three or higher in previous three years	
capacity	Relative political capacity, using the general sample model of relative political extraction	RPC
excl_pop	ln (Share of excluded population relative to total population)	EPR
noncontiguous	Noncontiguous state	MEPV
logmountain	ln(Mountainous terrain)	EPR
west, *eastern_europe*, *latin_america*, *subsaharan_africa*, *asia*	Dummy for Northern America/Western Europe, Eastern Europe, Latin America, Subsaharan Africa, and Asia, resp.	EPR

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