Governments' and rebel groups' armaments and their impact on civil conflict

Dissertation

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Governments' and rebel groups' armaments and their impact on civil conflict

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Zusammenfassung

Deutschsprachige Zusammenfassung gemäß § 9 Abs. 3 Satz 3 der Promotionsordnung der Ludwig-Maximilians-Universität München für die Sozialwissenschaftliche Fakultät (2016)

Motivation und Überblick

Innerstaatliche Konflikte[1] sind weit häufiger als Konflikte zwischen Staaten (Davies u. a. 2023). Trotzdem konzentriert sich die Literatur zu Konsequenzen von militärischer Ausstattung in erster Linie auf zwischenstaatliche Kriege: Rüstungswettläufe, Aufrüstung und Abrüstung sind viel diskutierte Konzepte in der Forschung zu Konflikten zwischen Staaten (vgl. z. B. Brzoska und Pearson 1994; Glaser 2000). Dabei ist es auf Grund der hohen Anzahl innerstaatlicher Konflikte aber ebenso von hoher Relevanz, wie Rüstungsmechanismen und Bürgerkriege zusammenwirken. In der öffentlichen Diskussion z. B. in Deutschland werden oft Rüstungsexporte in fragile Staaten kritisch bewertet, weil sie dort mutmaßlich zu weiterer Instabilität, Verschlechterung der Menschenrechtslage und einem erhöhten Bürgerkriegsrisiko führen (vgl. Rudolph u. a. 2024).

Solche Ansichten stehen aber auf wissenschaftlich wackligem Fundament, da gesicherte Erkenntnisse zu den Auswirkungen von Waffenlieferungen auf innerstaatliche Konfliktlagen nur sehr beschränkt vorliegen. Diese Dissertation soll deshalb dazu beitragen, das Verständnis der Wirkmechanismen von Rüstung und Konflikten zu erweitern, und fundierte empirische Belege für diese Mechanismen vorlegen, insbesondere in Hinblick auf das Risiko eines Ausbruchs und die Dauer von Bürgerkriegen.

Drei übergreifende Problemstellungen standen bei den Arbeiten im Rahmen der Dissertation im Mittelpunkt:

1. Die Grundlage für jede empirische Untersuchung sollte in kohärenten, formalen Theorien bestehen. Die Literatur zu Rüstung und innerstaatlichen Konflik-

[1] Innerstaatliche Konflikte bezeichnen Auseinandersetzungen zwischen einem Staat und einem oder mehreren nicht-staatlichen Akteuren, die – der Definition des Uppsala Conflict Data Program folgend – zu mindestens 25 Toten in einem Kalenderjahr führen (vgl. Davies u. a. 2023). „Bürgerkrieg" und „Konflikt" werden in dieser Zusammenfassung synonym zum Terminus „innerstaatlicher Konflikt" verwendet.

ten hat darauf bisher wenig zurückgegriffen. Um Hypothesen zu entwickeln und ihre Konsistenz sicherzustellen, wird deshalb in dieser Dissertation sowohl auf Verhandlungsmodelle aus der Literatur zu zwischenstaatlichen Konflikten aufgebaut, die für den innerstaatlichen Kontext angepasst wurden, als auch auf Modelle zurückgegriffen, die spezifisch für innerstaatliche Konflikte entwickelt wurden.

2. Bei allen empirischen Analysen wird ein besonderes Augenmerk darauf gelegt, dass die gefundenen Zusammenhänge tatsächlich kausaler Natur sind, d. h. beobachtete Veränderungen von Bürgerkriegsrisiko oder -dauer auch tatsächlich durch staatliche Bewaffnung ausgelöst werden. Theoretisch ist es ebenso möglich, dass Regierungen sich für eine Aufstockung ihres Waffenarsenals entscheiden, um für einen sich abzeichnenden Konflikt gerüstet zu sein. Diese Endogenität von Rüstungsentscheidungen wird durch spezifische empirische Verfahren berücksichtigt.

3. Bei allen Forschungsansätzen zur Bewaffnung im Kontext innerstaatlicher Konflikte besteht ein grundsätzliches Datenproblem. Daten von guter und zuverlässiger Qualität liegen für den Waffenhandel zwischen und die Waffenausrüstung von Staaten vor – allerdings hauptsächlich für Großwaffen wie Panzer oder Flugzeuge. Im Gegensatz dazu gibt es bislang kaum Daten zur Bewaffnung von Rebellengruppen. Im Rahmen der Dissertation wurden deshalb Daten erhoben, um einen ersten grundlegenden Einblick in die Waffenausstattung von nichtstaatlichen Akteuren in Bürgerkriegen zu erhalten.

Die Dissertation gliedert sich in vier Kapitel: Das erste bietet einen allgemeinen Überblick über die Literatur und beleuchtet insbesondere theoretische Modelle näher sowie Methoden zur Überprüfung der Zusammenhänge von Bewaffnung und Konflikten auf Kausalität. Das zweite Kapitel untersucht die Auswirkungen speziell von Kleinwaffenimporten auf das Risiko eines Konfliktausbruchs. Im dritten Kapitel werden die Auswirkungen von Aufrüstung auf die Dauer der Konflikte analysiert. Abschließend wird im vierten Kapitel die Datenerhebung zu Waffenkapazitäten von Rebellengruppen vorgestellt. Die Einleitung ordnet die einzelnen Kapitel näher ein und stellt Schlussfolgerungen, Leerstellen und Ansatzpunkte für weitere Forschung dar.

Kapitel 1: Armament dynamics and civil conflict: State of the literature, theoretical frameworks and the quest for causality

Das erste Kapitel beginnt mit einem Überblick des bisherigen Forschungsstands zum Zusammenhang von Rüstung und innerstaatlichen Konflikten, mit Schwerpunkt auf die drei Konfliktdimensionen Ausbruchsrisiko, Dauer und Intensität. Dabei zeigt sich, dass – im Gegensatz zu weit verbreiteten Ansichten in der Öffentlichkeit – ein konfliktverschärfender Einfluss von Aufrüstung nicht als Konsens bisheriger Studien angesehen werden kann. Insbesondere in Hinblick auf Ausbruchsrisiko und Dauer ist die Studienlage wenig zufriedenstellend; hier sind weitere Untersuchungen notwendig, um fundiertere politische Schlussfolgerungen ableiten zu können.

Basierend auf diesem Stand der Literatur werden vier wesentliche Herausforderungen für die weitere Forschung identifiziert:

1. Grundsätzlich kann eine große Bandbreite an theoretischen Mechanismen, mittels derer Bewaffnung sich auf innerstaatliche Konflikte auswirkt, eine substantielle Rolle spielen. Diese Mechanismen, oftmals im Kontext zwischenstaatlicher Konflikte entwickelt, lassen sich auch auf innerstaatliche Konflikte anwenden. Das Kapitel gibt dazu einen Überblick und stellt ein einfaches theoretisches Modell vor, in dem diese Mechanismen kohärent abgebildet werden können. Zusätzlich dazu werden auch ausgewählte Modelle im Detail vorgestellt und auf die Fragestellungen der Dissertation angepasst. Basierend auf einem für diesen Zweck adaptierten Modell von Besley und Persson (2011) werden die spezifischen Effekte von Groß- und Kleinwaffen auf das Risiko eines Konfliktausbruchs analysiert. Zur Analyse der Konfliktdauer wird ein Modell entwickelt auf der Basis der sog. Contest-Success-Funktion (vgl. Hirshleifer 1988), die die militärischen Fähigkeiten der Regierung und der Rebellen zueinander ins Verhältnis setzt, und ein Modell von Powell (2012) angewendet, das auch als Grundlage der Ausbruchsrisiko-Analysen in Kapitel 2 dient.

2. Damit empirische Analysen tatsächlich kausal belastbare Aussagen zu den Effekten von Aufrüstung auf Konflikte treffen können, wurden in der Literatur verschiedene Ansätze angewendet, die in diesem Kapitel dargestellt werden. Insbesondere werden die verschiedenen Instrumentalvariablen, die für diesen Zweck in anderen Studien sowie in Kapitel 2 und 3 dieser Dissertation vorgeschlagen werden, ausführlich behandelt und hinsichtlich ihrer Stärken und Schwächen untersucht.

3. Die Verfügbarkeit von Daten zu Rüstung stellt eine große Herausforderung für die Forschung dar. Insbesondere Daten zu Rebellenbewaffnung sind kaum vorhanden. Im Rahmen der Dissertation wurden deshalb solche Daten erstmals gesammelt, wie Kapitel 4 näher darstellt.

4. Bisher wurde die Rolle von weiteren Staaten, die über Unterstützung von Regierungen oder Rebellengruppen etwa durch Waffenlieferungen aktiv in Konflikte eingreifen, zu wenig berücksichtigt. Das Kapitel gibt Anhaltspunkte, wie dieses Defizit in der weiteren Forschung behoben werden kann.

Kapitel 2: Arming for conflict, arming for peace? How small arms imports affect intrastate conflict risk

Im Gegensatz zur Menschenrechtslage (vgl. z. B. Blanton 1999), wurden die spezifischen Auswirkungen von Kleinwaffenimporten auf das Ausbruchsrisiko innerstaatlicher Konflikte bisher nicht näher beleuchtet. Der Artikel geht deshalb dieser Frage im Detail nach, da Kleinwaffen sich von Großwaffen substantiell unterscheiden, insbesondere im Hinblick auf die Einsatzszenarien – sie können z. B. auch von der Polizei zur Sicherstellung der öffentlichen Ordnung verwendet werden.

Diese Unterschiede werden im Rahmen eines angepassten Verhandlungsmodells von Powell (2012) theoretisch untersucht. Dabei zeigt sich, dass Kleinwaffen zu Machtverschiebungen hin zur Regierung führen können. Diese Machtverschiebungen können dann wiederum die Rebellen zur Entscheidung motivieren, einen Konflikt frühzeitig zu starten, um weitere Machtverschiebungen zu verhindern. Gleichzeitig können Kleinwaffen aber auch als Werkzeuge zur Erhöhung allgemeiner Staatskapazitäten dienen und so den Verhandlungsspielraum für friedliche Lösungen erweitern, sodass das Konfliktrisiko sinkt.

Um diese theoretischen Vorhersagen empirisch zu untersuchen, kommen verschiedene Methoden zum Einsatz, die im Vergleich zur bisherigen Literatur einen besonderen Schwerpunkt darauf legen, dass Konfliktausbrüche seltene Ereignisse sind. Zum einen haben Cook u. a. (2020) einen Ansatz entwickelt, um „fixed-effects"-Modelle schätzen zu können, ohne Länder ausschließen zu müssen, bei denen innerhalb des Beobachtungszeitraums keine Konflikte ausgebrochen sind. Zum anderen wird mit einem „Split-population-duration"-Ansatz von Beger u. a. (2017) unterschieden zwischen Staaten, bei denen keinerlei Risiko eines Konfliktausbruchs festgestellt wird, und solchen, die ein strikt positives Risiko haben, das aber nicht zwangsläufig zu einem Konfliktausbruch im Beobachtungszeitraum führen muss. Zudem wird in weiteren Modellen über eine Instrumentierung der Waffenimporte sichergestellt, dass die gefundenen Zusammenhänge tatsächlich kausale Effekte der Importe darstellen.

Die empirischen Analysen bestätigen Ergebnisse (vgl. Pamp u. a. 2018), dass Großwaffenimporte die Konfliktwahrscheinlichkeit erhöhen. Erstmals zeigt der Artikel nun, dass für Kleinwaffenimporte ein solcher Effekt nicht nachweisbar ist – im Gegenteil führen diese in einigen Modellen tatsächlich zu geringerem Konfliktrisiko. Die theoretisch vermuteten Unterschiede, wie sich zusätzliche Bewaffnung mit Klein- im Vergleich zu Großwaffen auswirkt, werden dementsprechend empirisch bestätigt.

In einem Addendum werden außerdem die Auswirkungen auf staatliche Repression untersucht. Der bisher in der Literatur festgestellte starke Zusammenhang zwischen Kleinwaffenimporten und verschlechterter Menschenrechtslage (Blanton 1999; de Soysa u. a. 2010; Brender und Pfaff 2018) kann mit den in dieser Dissertation verwendeten

Daten und modernen Methoden nur eingeschränkt bestätigt werden.

Kapitel 3: Duration of intrastate wars of attrition. The causal impact of military build-ups

Die vorhandenen Studien zu den Auswirkungen von Aufrüstung auf die Dauer von innerstaatlichen Konflikten geben kein einheitliches Bild ab (vgl. u. a. Moore 2012; Caverley und Sechser 2017; Magesan und Swee 2018). Der Artikel untersucht deshalb diesen Zusammenhang mit dem Ziel, drei Probleme der bisherigen Literatur anzugehen: teilweise inkohärente Theoriebildung; Betrachtungen auf der Konfliktebene statt auf der disaggregierten Ebene von Dyaden zwischen Regierung und einzelnen Rebellengruppen; und mögliche Endogenität der Importe.

Um konsistente Hypothesen zu entwickeln, wird ein angepasstes „War-of-attrition"-Modell („Abnutzungskrieg", Powell 2017) vorgestellt. Da Rebellengruppen fast immer die schwächere Seite einer Konfliktdyade sind (vgl. Cunningham u. a. 2009), führen Aufrüstungen der Regierungsseite zu noch stärkerer Asymmetrie. Im War-of-attrition-Modell führen stärkere Asymmetrien zu kürzeren Konflikten, sodass Aufrüstung, etwa in der Form von Großwaffen-Importen, zu kürzerer Konfliktdauer führen sollte – allerdings nur dann, wenn diese Waffen auch effektiv eingesetzt werden können, z. B. um Konflikte in entlegeneren Gegenden zu führen.

Zur empirischen Überprüfung setzt der Artikel auf einen dyadischen Datensatz, der es erlaubt, wie theoretisch impliziert, neben der absoluten Bewaffnung der Regierungsseite auch die relative Stärke der Regierung gegenüber der jeweiligen Rebellengruppe abzubilden. Um Kausalität nachweisen zu können, führt der Artikel einen Cox-Instrumentalvariablen-Ansatz aus der Gesundheitsökonomie in den politikwissenschaftlichen Kontext ein. Dieser Ansatz erlaubt es, Instrumentierung in einem Cox-Survival-Modell umzusetzen, das spezifisch für die Analyse von Konfliktdauer als abhängiger Variable geeignet ist.

Die Ergebnisse weisen darauf hin, dass insbesondere bei Konflikten, die in größerer Entfernung zur Hauptstadt gegen schwächere Rebellengruppen geführt werden, Großwaffen zu einer kürzeren Konfliktdauer beitragen. In einem Addendum im Rahmen dieser Dissertation wird außerdem gezeigt, dass Waffenimporte auch die Zahl der Konflikttoten, die bis zur Konfliktbeendigung zu beklagen sind, reduzieren können.

Kapitel 4: What we know about rebel groups' armament. Collecting evidence for a more comprehensive assessment

In den Untersuchungen zu Konfliktausbruch und -dauer in den Kapiteln 2 und 3 konnten die militärischen Kapazitäten der Rebellengruppen nur indirekt, etwa durch Indikatoren zur Truppenstärke, gemessen werden, weil entsprechende Daten nicht vorhanden waren. Im Rahmen dieser Dissertation wurden deshalb erstmals Daten zur Bewaffnung von Rebellengruppen, die in innerstaatlichen Konflikten zwischen 1991 und 2018 gekämpft haben, gesammelt.

Der Artikel beschreibt den bisherigen Forschungsstand in Hinblick auf die militärische Ausstattung von Rebellengruppen, stellt das Vorgehen der Datensammlung dar und gibt einen Überblick über die daraus gewonnenen Erkenntnisse zur Zusammensetzung der Rebellenbewaffnung. Rebellen erlangen ihre Waffen meist durch Erbeutung staatlicher Waffenvorräte und über Unterstützung von außen (vgl. Jackson 2010) – im Gegensatz zum Waffenhandel zwischen Staaten gibt es dazu (fast) keine offiziellen Daten. Deswegen musste für die Datenerhebung auf öffentliche Quellen und Berichterstattung zurückgegriffen werden, etwa die Dokumentensammlung der Norwegian Initiative on Small Arms Transfers, Webpages oder Agenturmeldungen.

In 10.665 einzelnen Einträgen wurden Informationen zu 270 Gruppen gesammelt. Der Hauptdatensatz aggregiert diese Informationen und gibt für jede dieser Gruppen eine ordinale Einschätzung der Bewaffnung in 14 spezifischen bzw. 4 allgemeinen Waffenkategorien. Dabei bestätigt sich, dass Rebellengruppen im Mittel insbesondere hohe Ausstattungen an Kleinwaffen und Sprengstoff und geringe Ausstattungen an Großwaffen besitzen, aber auch, dass ein hoher Anteil durchaus substantiell mit leichten Waffen, etwa Boden-Luft-Flugabwehrraketensystemen, ausgestattet ist – anders als in der Literatur oft vermutet. Insgesamt war es eine große Herausforderung, ausreichend Informationen zur Waffenausstattung von Rebellengruppen zu sammeln: Für 75 der 345 untersuchten Gruppen konnten keinerlei Belege gefunden werden, für viele andere Gruppen nur wenige. Auch wenn diese Datenerhebung deshalb nur ein unvollständiges Bild abgeben kann, bietet sie erstmalig systematische Einblicke in die Zusammensetzung der Bewaffnung von Rebellengruppen.

Potentiale für weitere Forschung

Einige Perspektiven für weitere Forschung wurden bereits in der Zusammenfassung zu Kapitel 1 näher dargestellt; Abschnitt 4 des Einleitungskapitels rückt weitere Ansätze für zukünftige Forschung in den Fokus:

- Die formalen Theorierahmen, auf die diese Dissertation aufbaut, erlauben es auch, militärische Unterstützung durch externe Dritte explizit zu modellieren – ein wichtiger Baustein für ein besseres Verständnis der Zusammenhänge von Bewaffnung und innerstaatlichen Konflikten.
- Die verschiedenen Wirkmechanismen von Rüstung überlagern sich zu einem Gesamteffekt, der sich dann in den empirischen Analysen manifestiert. Um die Güte der einzelnen Theorien festzustellen, sind empirische Ansätze vielverspre-

chend, die spezifisch einzelne Mechanismen testen, z. B. indem sie zur Untersuchung der Signalwirkung von Aufrüstung zwischen Effekten von Waffenimporten – also zusätzlichen Waffen, die eine solche Signalwirkung auslösen können – und vorhandenen Waffenbeständen unterscheiden.

- Die Heterogenität des Konzepts „Bewaffnung" wurde bisher in der Literatur wenig berücksichtigt. Mit der spezifischen Untersuchung von Kleinwaffen-Importen (Kapitel 2) wurde gezeigt, dass verschiedene Waffentypen durchaus signifikant unterschiedliche Auswirkungen haben können. Solche Differenzierungen sollten deshalb systematisch auch z. B. für Konfliktdauer oder -ausgang vorgenommen werden.
- Die Instrumentalvariablen-Ansätze in der Literatur und dieser Dissertation zeigen zwar nur geringe Hinweise auf Nachfrageendogenität von Waffenimporten. Eine explizite Modellierung dieser Nachfrage, etwa in Simultangleichungsmodellen (vgl. Pamp u. a. 2018), könnte trotzdem entsprechende Mechanismen offenlegen und sollte deshalb weiterverfolgt werden.
- Die Datenerhebung zur Rebellenbewaffnung im Rahmen dieser Dissertation war ein bedeutsamer erster Schritt. Weitere Schritte sind allerdings notwendig, um erstens die Datensammlung zu verstetigen und zweitens mehr Quellen automatisiert und damit umfangreicher nutzen zu können, wie es neuere technische Verfahren wie z. B. Large Language Models ermöglichen.

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Introduction

1 Motivation

While wars between states receive stark public attention, not least since the Russian invasion of Ukraine, intrastate conflicts still outnumber interstate ones by a large margin, with higher death tolls in total (Davies et al. 2023). Regardless of the type, all battles are fought by human forces employing mechanical tools, be it with rather "low-tech" arms like AK-47s and self-made explosives or computerized aircraft and tanks. In both academic literature and public opinion, it is widely argued that the build-up of military capacities and how they relate to a potential adversary's military capacities plays a pivotal part in the dynamics of international conflicts: arms races, armament and disarmament are essential and well-known concepts when trying to comprehend the dynamics of e.g. World War II or the Cold War (cf. e.g. Brzoska and Pearson 1994; Glaser 2000).

This role of armament receives much less attention in the literature on intrastate conflicts, i.e. conflicts fought between a state government and one or more "rebel" groups. None of the more prominent review articles summarizing the literature on intrastate conflicts explicitly mention the arms that are needed to fight such conflicts (e.g. Brubaker and Laitin 1998; Sambanis 2002; Dixon 2009; Blattman and Miguel 2010; Regan 2010; Rauta 2021; Walter et al. 2021), and comparatively few articles are concerned with the impact of arms trade and armament on civil conflicts. By no means does this imply that civil conflict as a field has suffered from too little interest; for many other questions, the literature has produced an impressive pile of theoretical comprehension and empirical evidence. We can be relatively confident that intrastate conflicts are more likely to break out in anocracies than in democracies, in poorer or oil-exporting countries, and when there was already a conflict; and they are more likely to last longer when rebels have access to natural resources, when the conflict is more distant from the government's administrative center, and in countries with larger populations, to name some well-established conditions (cf. Fearon and Laitin 2003; Fearon 2004; Collier et al. 2004; Buhaug et al. 2009; Cunningham et al. 2009; Dixon 2009).

However, there is no consensus on whether a government's arms imports lead to a higher likelihood of civil conflict (cf. e.g Suzuki 2007; Pamp et al. 2018). Furthermore, do they help to put down a rebellion faster? And how do the other side's fighting capabilities factor in? These questions are the key issues governing this thesis.

These are not only academic questions but bear a fundamental importance: arms exports are "actionable" policy instruments that can be and are used for exerting influence, be it as leverage to induce particular actions by the importing government or for changing the military power distribution to a hostile country's disadvantage. While the arms transfers might be intended to affect the power distribution between states, they might also have—possibly unintended—political consequences within a state. For instance, a minority fearing these arms to be used for repressing them could opt to launch an attack on the government preemptively to prevent the power imbalance deteriorating further in favor of the government. Also, a military empowered by arms imports could push a government into launching military attacks instead of seeking peaceful solutions to disputes.

In Germany, for instance, the general public is rather skeptical of export permits because of the feared disastrous impact when these arms are actually used (cf. Rudolph et al. 2024). In the end, substantive policy advice regarding arms exports has to rely on reliable knowledge about the likely impact of exported arms under specific conditions. Therefore, a primary motivation for this thesis is to shine more light on arms' impact on intrastate conflicts.

For some time, public discourse, especially in Germany, was most focused on so-called small arms—that is, weapons that can be carried and worked by one person, like rifles—because they are assumed to cause most deaths in civil conflicts (Wille and Krause 2005; Kreutz and Marsh 2011). Recently, Rudolph et al. (2024) provided survey evidence that, on average, people are actually more favorable to exports of small arms than major weapons like tanks or aircraft. Nevertheless, it is hard to find any roots of such preferences in a factual understanding of specific arms types' impacts, as research on differences in consequences between e.g. small arms' and major weapons' importation was and is scarce. Thus, one chapter of this thesis attempts to discern the potential impact of these specific arms types on the outbreak risk of intrastate conflict.

Similar public scrutiny is put on armament during civil conflicts—when the general objective might be to end a conflict as soon as possible. Again, there is no established scientific evidence on a prolonging or shortening impact of arms imports amid ongoing fighting, which would allow to evaluate potential export policies. One chapter of this thesis therefore analyzes the consequences of armament for civil conflict duration.

A major blind spot in all research is the equipment of the non-state actors in these conflicts. First, a large share of rebel groups' military equipment can be traced back to a state's inventories (Jackson 2010). Answering questions about the potential impact of arms exports thus always has to consider a potential leakage to non-state groups, with unintended effects. Second, how governments put their arms to use is greatly determined by the characteristics of a conflict, e.g. whether it is fought in large-scale, open-field battles or as a slow guerrilla war—and this, in turn, mainly depends on what kind of equipment the rebel groups have available (cf. Kalyvas and Balcells 2010). Unfortunately, information on rebels' armament is minimal, hindering in-depth research. To improve this situation, I present a new dataset on rebels' armament in the final chapter of this thesis.

2 Overarching motifs

The works compiled in this thesis encompass three overarching motifs: first, the introduction of armament into formal theories of intrastate conflicts, second, setting up research designs that are suited to checking causality (challenging because the relationship between arms trade and conflict is two-track, with each potentially influencing the other), and third, overcoming apparent issues with data availability.

2.1 Placing armament in intrastate conflict theory

Countless theories have been developed to better understand the outbreak and development of civil conflicts. Armament, as the build-up of military capacities, is however not prominently featured in most of them. On interstate conflicts, a significant body of literature has developed regarding armament and arms races, mainly employing game-theoretical models to capture the dynamics of bargaining and fighting (cf. e.g. Kydd 1997; Powell 2002; Slantchev 2005; Meirowitz and Sartori 2008; Jackson and Morelli 2009; Chassang and Miquel 2010). On the subject of intrastate conflicts, only a few attempts to incorporate arming in formal models exist (cf. e.g. Besley and Persson 2011; Powell 2012, 2017).

Armament, obviously, changes the relative military power of the fighting parties. Such changes affect the probability of winning the conflict and thus the expected payoff (cf. e.g. Hirshleifer 1988). It is, compared to the extreme case of no armament at all, even a necessary condition for starting an outright conflict (cf. Most and Starr 1989). Arming can also be understood as signaling to potential opponents, which can lead to deterrence, but also to the provocation of preemptive measures (cf. e.g. Chassang and Miquel 2010; Powell 2012): when rebel groups fear a further deterioration of their relative power due to the government arming, they might be motivated to start a conflict now rather than to wait. At the same time, buying arms comes with a fiscal price, as available financial resources for other goods are reduced (cf. e.g. Powell 1993; Besley and Persson 2011).

In the tradition of the interstate-war literature, this thesis is mainly concerned with formal game-theoretical frameworks. Such theoretical frameworks require well-founded theoretical mechanisms to be formalized, subsequently allowing for thorough evaluation of the verbatim-theorized effects. Building upon existing bargaining models of inter- and intrastate conflict, I closely examine the role of armament especially for onset risk and duration: chapter 1 gives a general overview of frameworks and presents selected models in detail, and chapters 2 and 3 make use of adapted bargaining models from Powell (2012) and Powell (2017) for analyzing onset risk and duration, respectively.

2.2 The quest for causality

Theoretical frameworks are indispensable for explaining and, in the best case, predicting the impacts of certain events and actions. However, only empirical tests can discern between unlikely and less trustworthy reasoning and theories of high credibility and utility, earned by explanatory power for observed facts and causal mechanisms in the real world.

Gaining knowledge about this thesis' key questions thus requires to take the theoretical predictions to empirical scrutiny. Armament as the main explanatory factor presents however a challenging issue for empirical research, due to reverse causality. In contrast to the occurrence of natural resources or earthquakes, armament is not a given fact, but a result of human decisions shaped by other relevant factors. When a powerful opponent threatens to challenge the government, the government can decide to boost arming. If a conflict breaks out in sequence, did arming contribute to its onset? Or was arming solely the result of tensions building up?

Satisfying insights will therefore only be provided by empirical analyses that explicitly account for the potential endogeneity of armament processes. My co-authors and I have made it our task to address this issue more thoroughly than preceding research has done so far. Starting with Pamp et al. (2018), we developed and employed instrumental variable (IV) approaches to control for potential reverse causality. I utilize these IV models to infer causality of the observed effects when examining the impact of (specific) arms imports on both conflict onset risk and duration, as is part of this thesis (chapters 2 and 3, resp.). For survival analysis, no such models are commonly used in Political Science. However, thankfully, epidemiologists developed a Cox Proportional Hazards IV model that I could adapt to the questions of conflict duration (cf. Martínez-Camblor et al. 2019).

For a valid IV analysis, valid instruments are needed. In general, this dissertation focuses on the armament and conflict processes within a focal country, which are of course affected by external impacts. Nevertheless, the arms trade in itself delivers valuable insights: its mechanisms and constellations exogenously can affect a government's buying decisions, fulfilling the necessary conditions for valid instruments. For instance, Pamp et al. (2018) and Mehltretter (2022/chapter 2) exploit the fact that imports of arms used to fight in civil conflict, like armored vehicles and tanks, and arms that are of no use in civil conflicts, like surface-to-air missiles, are highly correlated. As such weapons that are irrelevant for civil conflict are never imported because of a looming *civil* conflict, they are a valid, i.e. relevant and exogenous, instrument. Most instruments used and developed in the course of this thesis' research endeavor exploit such arms trade regularities. Chapter 1 provides a comprehensive overview of these and alternative approaches to establishing causality.

2.3 Combating data challenges

A major obstacle to research on armament and civil conflict is the mediocre availability of reliable data. Unsurprisingly, data availability mirrors the general research interest: regarding inter- as well as intrastate conflicts, reliable data on the general properties of conflicts exist, and efforts to collect those for contemporary conflicts are ongoing. In contrast, dependable data on armament is limited to specific types of armament and thus partly lacking.

Data on intrastate conflict has improved immensely over the last decades, primarily thanks to the work of the Uppsala Conflict Data Program (UCDP). For all conflicts since 1946, information on e.g. involved groups, start and end dates and conflict issues is available, and detailed fatality numbers, geographic information down to the singular conflict event as well as indications of support from external governments are available starting 1989.

In contrast, only some areas of armament have a comparable standard of data collection and availability, namely those arms data that are most relevant for conflicts between states: the Stockholm International Peace Research Institute (SIPRI) delivers a remarkable effort to provide datasets of all available intel on transfers of major weapons like tanks, ships and aircraft. On stocks of such large military equipment, the International Institute for Strategic Studies (IISS) delivers a meticulous yearly compendium on state's military stockpiles.[2]

At the same time, we know little about the contemporary trade in small arms and light weapons, even though these types of weapons play a large role in civil warfare (cf. Wille and Krause 2005). The UN Comtrade database traces transfers of small arms like rifles and machine guns, but the data is error-prone and incomplete (Marsh 2005; Holtom and Pavesi 2017). Therefore, the Norwegian Institute of Small Arms Transfers (NISAT) provided diligently corrected and validated data for the period of 1992

[2]The data was until very recently (cf. Gannon 2023) not widely available in an open machine-readable, digitized form and could therefore unfortunately not have been taken advantage of for this thesis.

to 2014, allowing for valuable insights into the small arms trade. However, the data will not receive any further updates, leaving the raw UN Comtrade data as the only source. Even less is known about the trade in team-operated light weapons like mortars and grenade launchers, where, aside from incomplete raw UN Comtrade entries, no compilation of data exists that could depict a dependable image of light weapons transfers.

If that was not dire enough, the data availability described above only applies to *governmental* arms trade. SIPRI records some major weapons transfers to non-state actors like LTTE and Hisbollah, but less than 4 per year on average, clearly not covering a sufficient part of arms transfers to rebel groups—while there is no comprehensive data at all on rebels' small arms and light weapons. Considering the importance of relative military strength in many theories, e.g. when incorporating a contest success function (Hirshleifer 1988), it is an unfortunate situation to only have data for one side of the conflict. To improve our knowledge about rebel groups' arms, I devised and aided in implementing a strategy to collect publicly available information on non-state actors' military equipment for groups fighting conflicts in the period 1991–2018. This thesis presents both the information gathering process and its results in chapter 4.

These three major motifs—rigid theoretical frameworks, causality of empirical analyses and data challenges—govern the thesis' endeavor to improve on the understanding of armament and civil conflict. These motifs are intensively reflected upon in chapter 1 and re-appear throughout the other chapters. The contents of the thesis' four chapters are layed out in the following.

3 Introducing the articles

3.1 Chapter 1: Armament dynamics and civil conflict: State of the literature, theoretical frameworks and the quest for causality[3]

The first chapter comprises a review of both theoretical and empirical approaches to questions relating armament and civil conflict and assesses the main challenges of the literature. Additionally, I provide some in-depth perspectives on specific theoretical frameworks and on how to deal with the potential endogeneity of armament.

For starters, the existing empirical evidence regarding armament's impact on civil conflict is laid out, focusing on the three conflict dimensions of onset risk, duration, and intensity. Contrary to public reasoning, negative impacts of governmental arms are

[3]This chapter is based on a version of the article "Armament Dynamics and Civil Conflict: What We Know and the Challenges Ahead" co-authored with Oliver Pamp and Paul W. Thurner, not yet published. I contributed the summaries of the empirical literature and relevant data as well as the theoretical overviews regarding conflict duration and intensity. For this thesis, I added in-depth analyses of specific frameworks on conflict onset and duration and provide a detailed exploration of available and self-developed approaches to armament endogeneity.

not consensus within the literature, with multiple studies finding e.g. no effect on conflict risk. Especially on the emergence and duration of conflict, no compelling overall picture has emerged from the literature so far, necessitating further research.

The chapter identifies four significant issues that have to be overcome for a more comprehensive understanding of armament and civil conflict: theoretical approaches have not been rooted sufficiently in rigorous (formal) frameworks; concerns of demand endogeneity have mostly not been taken into account; data availability, especially with regard to rebel groups' arms, is meager; and the role of third-party states in armament in such conflicts is widely overlooked.

Theories of conflict and armament Formal theorizing is central to developing stringent mechanisms of cause and effect. Unfortunately, the literature on armament and civil conflict often does not make use of formal models, in contrast to the interstate conflict literature. The chapter lays out relevant models from the interstate perspective, transferring the findings to the intrastate context.

Thereby, the range of potential theoretical mechanisms for how armament affects civil conflict is fully exposed: bargaining models can incorporate simple power-of-force effects—as captured by the contest success function relating the government's military capabilities to those of the rebel groups; they can implement the bread-vs.-butter logic, where spending on armament reduces general wealth or spending for other purposes; they might focus on dynamics of power shifts that occur when military buildups change the power distribution in favor of the government; and they can integrate potential failures of bargaining itself due to commitment problems or issue indivisibilities. To relate these effects to each other, the chapter provides a simple bargaining model that allows to reflect on all these concepts.

Additionally, the chapter offers more profound insights into a selection of frameworks for advancing the theoretical understanding of the emergence and dynamics of civil conflict. For onset risk, a closer look at a bargaining model from Besley and Persson (2011) shows how armament might actually deter rebel groups, as the bread-vs.-butter logic can reverse when there is less economic wealth to capture for a government's adversary. On conflict duration, I show that a relatively simple round-based approach utilizing the contest success function can give relevant insights into armament's impact on conflict length, especially in multi-dyadic conflicts. Furthermore, elaborating on Powell (2012), dynamics of power shifts during conflicts provide valuable insights into armament's effect on conflict duration as well. Both frameworks show that decisiveness of fighting plays a vital role in determining whether a conflict is terminated.

Endogeneity concerns Endogeneity of armament is a main concern for the causal interpretation of empirical analyses. The chapter therefore discusses possible identification approaches and different instruments that allow for extracting exogenous variation in armament. The approach that the analyses in this dissertation relied mostly on exploits related imports of different types of weapons that have no connection to internal conflicts, like air defense. In other literature, identification is based on economic reasoning for demand, e.g. due to price fluctuations. Advantages of and objections to the different approaches are discussed, sensitizing for their usage in the literature and the following chapters of this thesis.

Data issues Apart from demand endogeneity, data on armament is the major obstacle to empirical research. The chapter provides an overview of available data, their reliability and completeness, as well as the glaring gaps. In short, only for governments' trade in and stocks of major weapons, data is sufficiently reliable and complete. On the trade of small arms like rifles and machine guns, a dataset for a shorter time period exists, but is not updated anymore, with other sources only providing incomplete data; on light weapons like missile launchers, available data is even more sparse. The main issue, however, is data on rebel groups' arms, which did not exist at all so far. To brighten up this data blind spot, chapter 4 presents the approach and results of a first effort to collect rebel groups' arms data.

The missing role of external actors As the fourth challenge to the literature, the chapter identifies the missing role of external actors in most research approaches. Whereas this dissertation advances in overcoming the first three issues, this additional complexity of outside actors is not as rigorously addressed. Nevertheless, this chapter indicates why this would be a significant step for future research, as arms trade is not a liquid market where demand is always fulfilled. Instead, the exporters' interests play a significant role for whether a transfer materializes. In intrastate conflicts, this role of exporting countries' considerations is even more significant in terms of support for non-state actors, where security and political objectives are decisive. At the same time, economic gains from selling weapons to rebels are usually negligible. Third parties can treat such conflicts as proxy wars (cf. Berman et al. 2019) and might exert influence on civil conflicts *by granting or withholding arms exports*—which should be taken into account when analyzing armament and conflict.

3.2 Chapter 2: Arming for conflict, arming for peace? How small arms imports affect intrastate conflict risk[4]

Contrary to the repression literature (Blanton 1999; de Soysa et al. 2010; Brender and Pfaff 2018), small arms have not played a role in research on internal conflict risk so far, even though small arms are probably the primary type of weapons used in these conflicts and cause the most deaths (Wille and Krause 2005). Only Craft and Smaldone (2002) and Magesan and Swee (2018) include small arms in their analyses but do not distinguish between their effects and those of major conventional weapons.

Closing this research gap, the article examines the impact of small arms on conflict outbreaks. Focusing on the transfer of small arms separately seems justified both from an empirical and theoretical perspective. Small arms require minimal training and can be imported in the short term, whereas acquiring major weapons often takes years from order to delivery. Based on a game-theoretic framework by Powell (2012), the article works out two counteracting potential consequences of small arms imports: On the one hand, small arms can have an escalating effect like major weapons (cf. Pamp et al. 2018). On the other hand, arming with small arms can have a conflict-preventing effect. Civil war research has shown that both the police-administrative and economic capacities of states significantly reduce the risk of conflict (Fearon and Laitin 2003; Fjelde and de Soysa 2009). Small arms play a central role in strengthening these state capacities in peaceful times and also in the emergence of conflicts. Not only the military but also police forces must be equipped with small arms, as they are necessary tools for ensuring security and public order (cf. Bourne 2007; Karp 2018). These, in turn, are an essential prerequisite and also a consequence of the state monopoly on the use of force. Therefore, the overall effect of small arms imports on the risk of conflict is not necessarily negative, but depends on the strength of the opposing theoretical forces.

Methodologically, the article places emphasis on the fact that the outbreak of internal conflicts is a rare event, which poses additional challenges for empirical research. Thus, the article introduces two new estimation approaches into the conflict literature for better taking into account the rare-events characteristic of internal conflicts.

Firstly, it would be generally preferable to use models that account for unobserved heterogeneity between countries. However, standard models with fixed effects would lead to biased estimates since countries without an outbreak during the observation period, i.e. without variance in the dependent variable, must be excluded. Cook et al. (2020) therefore propose a penalized maximum likelihood estimator with fixed

[4] Article, published in *Conflict Management and Peace Science*, with an addendum on armament's effects on repression. Full citation of the article: Andreas Mehltretter (2022): "Arming for conflict, arming for peace? How small arms imports affect intrastate conflict risk". *Conflict Management and Peace Science* 39(6): 637–660.

effects. This estimator allows for correct control of unobserved differences between countries.

Secondly, *many* countries show no conflicts over the observed period or even since the beginning of their existence. Beger et al. (2014, 2017) develop a split-population duration estimator that distinguishes between such consolidated, i.e. "immune", and "endangered" countries, as they call them. The split-population model estimates an immunity equation, that determines which country-years are at risk of conflict, and a duration equation identifying factors that increase or decrease the risk of an outbreak if the country-year has at least a low probability of conflict.

Using these split-population and penalized fixed-effects logit models as innovative estimation methods for data with rare events, I find that importing small arms rather has risk-reducing effects in most model specifications. In comparison, these effects are not statistically significant in a standard probit model. However, an instrumental variable model is used here since endogeneity, i.e. a positive correlation between conflict risk and small arms imports, could hide an underlying negative effect of these types of imports. In fact, the negative effects found in IV models are much stronger and again statistically significant. Overall, the evidence points out clearly that small arms imports do not increase conflict risk, but might rather decrease it.

Additionally for this thesis, I provide a re-analysis of the often-discussed small arms-repression nexus. The literature so far (Blanton 1999; de Soysa et al. 2010; Brender and Pfaff 2018) has found a significant impact of small arms on repression, worsening the human rights situation in the importing country. However, with the NISAT data used in this chapter and appropriate statistical methods, I cannot reproduce these results: I find a statistically significant effect of small arms importation increasing repression, but of no substantial effect size.

3.3 Chapter 3: Duration of intrastate wars of attrition. The causal impact of military build-ups[5]

Empirical evidence on the effects of government's armament endowments on the duration of intrastate conflicts is sparse and ambiguous. Moore (2012) suggests that imports of major conventional weapons (MCW) correlate with longer-lasting conflicts. In contrast, findings from Caverley and Sechser (2017) indicate an opposite effect: by examining mechanization, which is the ratio of military equipment to troops, they demonstrate that enhanced mechanization in combined air and ground forces shortens conflict durations. As for the impact of US weapons sales, Magesan and Swee (2018) find no significant effects. Overall, the literature has provided inconclusive results on

[5]Article, co-authored with Paul W. Thurner, not yet published, with an addendum on a "casualties-as-time" analysis approach of the duration-intensity nexus. Paul W. Thurner contributed core ideas and advice, especially on the theoretical framework and methodological issues, and revised the manuscript.

whether governmental armament leads to longer or shorter civil conflicts.

This literature has not drawn upon coherent formal frameworks so far. Unfortunately, this approach can lead to inconsistencies in arguments. For instance, Buhaug et al. (2009) argue that stronger rebels should fight shorter conflicts, while a greater distance between government stronghold and conflict site is expected to lead to more protracted conflicts—although a greater distance quite clearly also effectively makes rebels relatively stronger. Therefore, it is essential to build a coherent theoretical framework.

Consequently, in the article, my co-author Paul W. Thurner and I adapt Powell's (2017) war-of-attrition model to derive consistent hypotheses on governmental military build-ups' effects on the duration of government-rebel conflict dyads. In accordance with the war-of-attrition framework, the article argues that in each separate dyad, governments and the respective rebel group strategically determine how long they are withstanding the costs of military fighting. Whenever one party quits, the conflict ends, and the other party wins. The decision on how long to fight depends on the valuation of winning and on the costs that arise when fighting, e.g. due to casualties. Using this framework, we arrive at the remarkable prediction that not symmetric (cf. Balcells and Kalyvas 2014), but especially asymmetric configurations of the government's and the rebels' military capabilities lead to shorter civil conflicts. This theoretical result revives older findings by Bennett and Stam (1996) on interstate war duration—and is in line with the simple contest-success-function duration model presented in chapter 1.

Since additional armament enables the government to impose costs on its adversaries, arms imports cause rebels to withdraw from the conflict earlier, thereby shortening its duration. This mechanism is especially powerful when major weapons can be effectively brought into combat, which is the case especially in conflicts at higher distances from the government stronghold. Then, MCW enable the government to project its power into more remote areas.

The empirical analyses on the disaggregated dyad level demonstrate these mechanisms to be effective: it is shown that higher imports of major weapons lead to shorter conflict durations, but only when the conflicts are located in more remote areas and the power distribution favors the government.

While most analyses using the Non-State Actors data (Cunningham et al. 2013) find that weaker rebels fight more prolonged conflicts (e.g. Cunningham et al. 2009; Buhaug et al. 2009), we expect stronger governments to be able to inflict higher costs onto such weaker rebel groups, causing them to quit the conflict earlier. In fact, the results provide clear empirical evidence—contradicting the findings derived from analyses employing the Non-State Actors data—that conflicts last longer with relatively stronger rebels when using the troop ratio as an alternative measure of relative military strength (cf. Hultquist 2013).

The overall logic of the war-of-attrition framework is further supported by the results regarding the effect of casualties on duration: higher total costs are expected to lead to shorter conflicts, as both the government and the rebel group would decide to withdraw sooner. Thus, it is predicted that larger numbers of casualties should reduce the duration of a conflict (cf. Brandt et al. 2008; Balch-Lindsay et al. 2008). In contrast to the existing literature, the article proposes operationalizing casualties as number of casualties per day to match the measurement unit of conflict duration. Using this measure, a higher intensity of conflicts robustly reduces duration.

Again, the article takes potential issues of demand endogeneity seriously: governments are likely boosting their military equipment importation if they foresee a drawn-out conflict, gearing up for enduring warfare. This anticipation of extended conflict can skew the analysis of how armaments affect conflict duration, leading to biased results. This article thus uses a two-stage residual-inclusion IV approach for Cox survival models (Martínez-Camblor et al. 2019), which is a first in the Political Science literature. This method effectively accounts for this endogeneity, corroborating causality of the effects.

In an addendum specifically for this thesis, I take the analysis to a slightly re-phrased question: does armament cause higher or lower total numbers of fatalities before a conflict can be put to an end? For instance, arms imports could lead to hefty, but short conflicts, leading to overall *lower* fatalities than would have occurred in a not as intense, but longer conflict. Neither standard duration nor standard intensity analysis is able to answer this question properly, and standard models using the total number of conflict deaths as the dependent variable cannot account for time-varying effects. To provide an analysis for this aim, I propose to use a "casualties-as-time" model where the number of fatalities occurring over the course of the conflict, with varying intensity, is the dependent variable in a survival model. Employing this empirical strategy, I find that under similar conditions as for duration, governmental armament can cause conflicts to end after fewer deaths.

3.4 Chapter 4: What we know about rebel groups' armament. Collecting evidence for a more comprehensive assessment[6]

One of the primary data deficiencies hindering quantitative civil conflict research is missing information on rebels' armament. Apart from vague notions of relative fighting capacity or strength and a dichotomous "arms procurement capacity" indicator, all provided by the invaluable Non-State Actors dataset (Cunningham 2013), knowledge about specific groups' endowment of arms is very limited.

[6]Single-authored version of the article, not yet published.

A major goal of this dissertation project was therefore to collect additional data for providing a more general and precise overview of the specific military capabilities of non-state actors and their procurement channels, for example through international arms transfers. The new dataset encompasses a wide range of information about the armament of rebel groups and offers various opportunities for quantitative and qualitative empirical research. It is the first dataset to provide comprehensive measurements of the armament of rebel groups, including detailed information about their equipment with specific categories of conventional large and small arms. All data entries also include information about the context in which the weapons were observed and, where possible, indicate the origin of the military equipment of the rebel groups.

While official records exist e.g. for weapons transfers to states, indications of the rebels' armaments only emerge under certain circumstances. Typically, rebels acquire arms through imports, seizing stockpiles, capturing military equipment and other illegal means (cf. Jackson 2010). Information about such equipment is only officially recorded when groups lose equipment, for example, through the confiscation of weapons by state security forces. Therefore, I developed a multi-stage process for gathering information from various sources in a comprehensible and transparent procedure, that my research assistants and I applied uniformly for each group that was involved in a civil conflict in the period 1991–2018.

In order to gather the necessary data, a variety of publicly available resources were utilized. Initially, information was sourced from the UCDP Conflict Encyclopedia and other conflict-centric databases. As the main step, an extensive review of documents in the NISAT document library was conducted. Additionally, extensive searches were carried out using Google for general web content, Google Scholar for academic literature and Nexis for news reports, employing a range of keywords related to both general armament terms and specific types or categories of arms. This comprehensive search strategy resulted in a total of 10,665 manually coded "raw" entries, consisting of observations or reports about the usage or possession of specific arms.

Aggregating these individual entries at the rebel-group level, the dataset is able to provide information on 14 different arms categories for 270 groups. Note that, although we examined a wide range of sources for each group, it turned out that for a significant portion of the groups, precisely 75 out of 345, no information could be found about their specific military capabilities, that is, at least some form of evidence for the equipment or use of a particular type of weapon. When examining these groups using data from other sources, I find that they were involved in comparatively small conflicts with a lower number of casualties and that they rarely received external support.

The group-level data show that many rebels have a substantial amount of small arms. It turned out that for most of the groups with any information recorded evidence of small arms was found. 108 of the 270 groups were classified in the second-highest equipment category of a 5-point ordinal scale, confirming the general assertion that rebel groups are typically well-equipped with small arms. Considering light weapons, the large amount of evidence for rocket and grenade launchers, especially MANPADS (Man-Portable Air-Defense Systems), notably relates not just to a few selected groups but is distributed across a more significant number of groups. In fact, the data collected shows that over 100 groups have at least a medium level of equipment with light weapons. Compared to small arms, the average stock of light weapons is significantly lower, and only a few groups have particularly high stocks. Slightly more than half of the groups studied used or possessed explosives. However, these typically had considerable quantities, with 76 groups reaching the second-highest level of equipment. In contrast to other categories, using or possessing larger conventional weapons such as tanks or aircraft is less common among the groups, with 225 groups having no major weapons recorded at all. Also, the quantities registered here are generally smaller than those for small or light weapons.

All in all, this dataset is the first to deliver a systematic, comprehensive overview of publicly available information on the arms of rebel groups. Whenever this data is used for further research, it has to be borne in mind that it does and cannot claim completeness. Rather, the data provides a lower threshold of rebels' armament, since additional arms might not have been detected by the research steps conducted for this data collection project. Still, it is the most exhaustive compilation of knowledge on rebels' armament to date.

4 What we have learned, and what we still do not know

This thesis contributes to the literature on civil conflict research by shedding light on the role of armament in conflicts' emergence and persistence. Despite this thesis' contributions, many questions remain open—some of which can be addressed by further research in the near future, and some that will most likely be left in the dark for far longer. In the following, I hint at insights to take away from this thesis, relating to both the gathered knowledge and the blind spots of this research area that need to be worked on.

4.1 Armament and game-theoretic models

We can learn a lot from adapting theoretical frameworks that were developed to comprehend interstate conflicts for intrastate conflict analysis, as laid out in chapter 1. Nevertheless, there are substantial differences that have to be taken into account. For one, according to all available data, rebel groups are almost always outnumbered and out-gunned at the beginning of fighting, meaning asymmetry between fighting parties is much higher than usually observed between most countries.

More importantly, active arms acquisition before a conflict breaks out is more one-sided than in interstate conflicts: in most cases, only the government has access to relevant supplies of arms. As the data collection on rebels' arms has shown, rebel groups usually only gain access to substantial armament when the conflict has started. Additionally, governments exert much-amplified influence about the rebel groups' status (subsuming economic and ideological factors) than one country usually is able to exert over another state without declaring full-on war.

Therefore, chapter 2, examining small arms' impact on conflict risk, finds its foundation of theoretical reasoning in a bargaining model developed explicitly for the intrastate context (cf. Powell 2012). This framework allows for implementing the specific roles of armament with small arms that can—apart from military use in battle situations—also be employed for internal repression and upholding the government's monopoly of force within the country, not especially relevant in theories of interstate conflict. On the duration of civil conflicts, chapter 3 as well uses a framework tailored for intrastate conflicts (cf. Powell 2017). In general, the core logic of the war-of-attrition model is not fundamentally different from models focused on interstate conflicts—because armament dynamics during civil conflicts are more similar to those in international conflict than dynamics before a conflict breaks out. Nonetheless, Powell's war-of-attrition framework provides a relatively tractable but powerful approach to reason about the duration of civil conflicts. Evaluating its theoretical predictions empirically lends substantial credibility to its underlying mechanisms, endorsing wider usage of this model.

The war-of-attrition framework also cues at a fundamental area for further theoretical developments, where this thesis is not able to expand on sufficiently: Powell's model additionally allows for third parties to weigh in on the conflict and provide support to one or both sides. Rebel groups are particularly heavily restrained by the availability of external arms supplies. The decision of whether a foreign government is supporting a rebel group with arms therefore bears great impact on an ongoing conflict. Thus, it will be valuable to examine the role of external support in the armament dynamic of conflicts both in theory and empirics more thoroughly.

4.2 Theoretical ambiguity and testing mechanisms

Implementing armament dynamics into theoretical models of conflict provides relevant insights into all the different effects arming can have on civil conflict. As described in chapter 1, armament can, for instance, lead to deterrence, but prospects of further armament can also lead to preemptive attacks, while acquiring additional arms can signal strength, but also weakness when the government is perceived to need further weapons to withstand potential attacks. In the end, it is reasonable to assume that all these potential armament effects can exist and do exist—under specific circumstances and/or to varying degrees, with multiple overlaying effects acting in opposite directions.

The analyses conducted for this thesis offer noteworthy results on the impact of armament dynamics on conflict risk and duration. These empirical findings describe the overall impact of these armament processes, providing clear indications for which of the effects—like signaling resulting in preemptive action or deterrence—are stronger because the overall effect points in a certain direction. Nevertheless, most analyses in this thesis unfortunately are not able to discern between different mechanisms working in the same direction or partially mutually cancelling each other out.

Overall, the results point toward effects from contest-success-function hypotheses being often stronger than signaling forces when they act in different directions. Small arms imports seem rather to deter conflict onset, as found in chapter 2, and major weapons allow for ending conflicts earlier under certain conditions, speaking to the "hard facts" of military power. At the same time, however, major weapons are consistently found to *increase* conflict risk, alluding to the complexity linking conflict and armament. This could be a sign that during conflict, concepts of (relative) military capabilities are most relevant, while bargaining mechanisms, e.g. regarding commitment problems, are less so. However, these concepts of military strength must necessarily be accompanied by bargaining concepts like signaling in phases before an outright escalation into conflict—otherwise, theoretical reasoning would fail to explain escalating effects of major weapons, for instance.

To deepen our understanding of this complexity, future research has to develop better tests of different mechanisms that channel armament's impact on conflict. On the role of signaling versus power mechanics, better data and exploitation of time differences between ordering, expecting, communicating and receiving arms imports could help uncovering how relevant each of these events and therefore the specific theoretical mechanisms are. Additionally, utilizing more recent data from Gannon (2023), differentiating the impacts of military *stocks* on the one side and *imports* on the other side can provide insights under which conditions power shifts—an argument made strongly by Powell's theories employed in this thesis—or absolute levels of power bear greater explanatory potential for the onset and dynamics of conflicts.

4.3 Taking advantage of heterogeneity in "armament" and "conflict"

Heterogeneity in the concepts of armament and conflict can considerably help to discern between theoretical mechanisms and evaluate their relative importance. As my results on the specific impact of small arms versus major weapons imports highlight, it makes a difference in conflict emergence what kinds of arms are added to the government's stock. However, these results provoke further questions: is the nature of the weapons the deciding factor, or is it who holds the imported weapons? Differentiating between small arms owned by military forces and small arms owned by police forces could help to find the more significant causal mechanism.

Even variance within categories of weapons might pose major chances for discerning between theoretical arguments: in chapter 3, power projection capabilities play an important role in the argument as to why arms imports can lead to shorter conflicts under certain circumstances. In the empirical analyses, it is found that imports of MCW reduce duration of conflicts against weaker rebel groups in more distant areas. With more detailed data on arms stocks, it should be possible to evaluate the theoretical power projection hypotheses more concretely, e.g. about the highlighted impact of aircraft in conflicts in mountainous terrain compared to tanks, for instance. At the same time, the interactions between armament and military personnel, by itself an important indicator of conflict duration (see chapter 3), should be explored in more depth than simple "mechanization" ratios employed in the literature so far.

Not only armament, but also the circumstances of "peace" and conflict contain considerable heterogeneity that can be exploited for more rigorous testing of differing hypotheses. Notably, the literature and also this dissertation are concerned with different concepts of conflicts in a broader sense, e.g. outright fighting, terrorism, or repression (see chapter 2). However, it might be a worthwhile research endeavor to evaluate these different aspects of violence in a more holistic approach, both in theory and especially in empirics, e.g. by modeling the multi-dimensionality of conflict in form of multiple dependent variables simultaneously. The impacts of armament might hinge crucially on the differing mutual conditions of different types of violence. In particular, the potential trade-offs between repression and civil conflict—as indicated by the potential impacts of small arms imports for conflict risk found in my research and by related research (cf. e.g. Rød and Weidmann 2023)—should be stressed to allow for a more nuanced discussion on armaments' overall impact.

4.4 Endogeneity of armament

One major concern for interpreting empirical results on armament and conflict has always been causality: while armament might affect conflict processes, conflict or its anticipation might affect armament decisions as well. It is a major leap in the conflict literature that attempts to address this issue have become more prominent (cf. Pamp et al. 2018; Magesan and Swee 2018; Gallea 2023).

Evidence on endogeneity is, however, mixed. Gallea (2023), for instance, only finds a positive impact of arms imports on conflict risk in IV models, not in standard OLS models. In chapter 2's analyses, major weapons' conflict-inducing effects are also larger in IV models than in non-instrumented models. Notably, these results do not fortify the argument of demand-endogeneity, i.e. that demand for arms rises in anticipation of conflict. With demand endogeneity, one would expect positive correlations of arms imports and onset risk to be *smaller* in instrumented analyses.

Throughout the analyses of this thesis, I did not find that controlling for endogeneity changed the results substantially compared to correlational analyses—contrary to my expectations at the beginning of this research endeavor. However, this does not necessarily imply that there is no armament endogeneity. It might as well just indicate that we do not understand the endogeneity sufficiently in theory to test it in empirics. Also, IV approaches might not be sufficient to capture the endogeneity dynamics: in Pamp et al. (2018), simultaneous equations models exhibit statistically significant reverse causality. More sophisticated modeling of the two-way relationship between armament and conflict could thus lead to deeper insights when we have to account for endogenous demand for armament.

4.5 Data on rebels' armament

Data availability is good regarding states' major weapons stocks and importation, with reliable datasets on hand for quantitative research. Regarding the states' small arms trade, there has been no reliable data since 2014, but data for the two-and-a-half preceding decades at least allows empirical analyses, as is carried out in chapter 2. In contrast, we did not know much about rebel groups' armament—fortunately, this thesis is able to offer a modest contribution by providing some information on rebels' arms.

Gathering evidence on such armament proves to be a complicated and laborious undertaking, as I describe in chapter 4. It certainly is worth the effort because we now can at least get a systematic glimpse of the military hardware of non-state actors. Nevertheless, the data needs to be taken with a great grain of salt. It is just a fragment of a larger picture—and we do not even know how large the whole picture is. After all, our data collection could only collect what was publicly available information.

Some shortcomings of this data collection effort can unquestionably be alleviated by attributing even more resources to it. Taking stock of large corpora of source texts could be achieved with the help of machine learning (ML) and large language models (LLM), thereby allowing to scour much more text and information than we were able for our data collection. ML and LLM can also tap into image and movie data, e.g. from social media, to gather evidence on arms' possessions and usage. I would highly commend an enduring, long-term effort to ensure a complete aggregation of all openly available information on rebels' armament.

Nevertheless, we will still have to bear with two fundamental issues of such data. First, most weapons find their way to rebel groups over illicit paths, without official (and not even unofficial) records. Thus, there exists a high likelihood that a significant portion of rebels' armament goes undetected in public sources, only ending when its usage or seizure by the military or authorities is recorded. Data on rebels' armament will therefore always be incomplete.

Second, all data collected from such public sources will always be challenging to work with in empirical research. The likelihood that a given stock or transfer of arms is revealed depends on the media attention, availability of social media, duration and intensity of the fighting. While the data can be nevertheless used for valuable analyses, research utilizing rebels' armament data has to account for the possibility that records of rebels' arms might endogenously correlate with the conflict variables of interest due to the inherent biases of the data collection.

In the end, what does all this imply for the motivational question whether arms exports are bad because they cause human suffering? This thesis gives some hints that the answer is more complicated than often claimed in public discourse. However, to provide more reliable evidence and guidance for politics and societies to evaluate their approaches to arms exports, Political Science still has a long road ahead. Nonetheless, I intend the work in this thesis to advance our understanding of armament and civil conflict and provide a modest contribution to better decisions preventing and ending conflicts and, that is the humble aspiration in the end, to a more peaceful world.

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Chapter 1

Armament dynamics and civil conflict: State of the literature, theoretical frameworks and the quest for causality

This chapter is based on a version of the article "Armament Dynamics and Civil Conflict: What We Know and the Challenges Ahead" co-authored with Oliver Pamp and Paul W. Thurner, not yet published. I contributed the summaries of the empirical literature and relevant data as well as the theoretical overviews regarding conflict duration and intensity. For this thesis, I added in-depth analyses of specific frameworks on conflict onset and duration and provide a detailed exploration of available and self-developed approaches to armament endogeneity.

1 Introduction

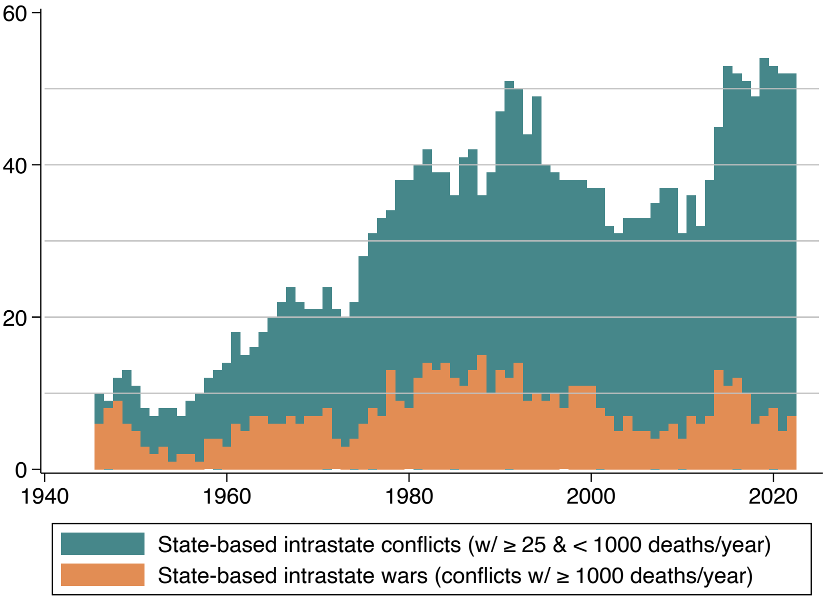
In his definition of the state, inspired by institutional economics and historical analyses, Douglas North defines it as "an organization with a comparative advantage in violence extending over a geographic area whose boundaries are determined by its power over its constituents" (North 1981: 21). The very technologies of projecting power and exercising violence are weapons. Weapons are the default instruments of the Leviathan in guaranteeing domestic tranquility and defending against external aggression. However, the endowment with weapons is a double-edged sword. Governments can also use them internally for repression, exclusion, autocratic governance and externally for military threats and aggression. Weapons are also sought by non-state groups, often supported by external powers, to challenge a government's monopoly of violence, contest state boundaries, conduct proxy wars or for criminal objectives like looting resources and trafficking drugs.

In the interstate context, military build-ups, especially in the form of mutually reinforcing arms races, have long been argued to lead to the military escalation of disputes (cf. e.g. Brzoska and Pearson 1994; Diehl 1983; Gibler et al. 2005; Glaser 2000; Wallace 1979). It is therefore noteworthy that comparatively less attention has been paid to armament dynamics and the role that the proliferation of weapons plays in civil conflicts, despite the fact that great strides have been made in exploring the onset, duration and intensity of civil violence. This is surprising given intrastate wars' much greater ubiquity. While international wars are a relatively rare phenomenon, armed state-based civil conflicts—commonly defined as violent disputes between governments and non-governmental groups[1] leading to at least 25 deaths in a calendar year, as proposed by the Uppsala Conflict Data Program (UCDP)—have continued to rise in importance over the last two decades. As shown in figure 1, the number of active civil conflicts rose from around 30 in the 1990s and 2000s to over 50 since 2019 (with 2022 being the latest data available at the moment of writing). According to UCDP data, intrastate conflicts led to over 185,000 fatalities in 2022 (Davies et al. 2023).

Military fighting between governments and armed non-state groups can take many forms, from large-scale military confrontations to asymmetric forms of conflict which include different types of guerrilla tactics and terrorist attacks (cf. Cook and Lounsbery 2017; Kalyvas and Balcells 2010). In order to understand what drives and shapes these conflicts and their dynamics, the literature has emphasized the underlying tensions and structural factors such as ethnic grievances, political oppression, economic opportunities, political instability, government weakness or poverty (for overviews, see Blattman and Miguel 2010; Brubaker and Laitin 1998; Cederman et al. 2013; Collier and Hoeffler 1998; Dixon 2009; Sambanis 2002; Wimmer 2013). However, the con-

[1] Throughout this thesis, the terms "non-state actors", "armed non-state groups" "rebel groups", "rebels" or "opposition group" are used interchangeably, without intending any normative connotation.

Figure 1: Intrastate conflicts over time



Source data: UCDP/Davies et al. (2023)

flict strategies chosen by the conflict parties and in particular the military technologies used are also of great importance (Balcells and Kalyvas 2014; Kalyvas and Balcells 2010; Lichbach 2012; Staniland 2014). This chapter, and the thesis as a whole, therefore addresses the important question of how the availability of weapons affects intrastate violence.

Anecdotal evidence suggests that arms build-ups and international arms flows may be an important factor in the evolvement of internal conflicts. Before the outbreak of the Syrian civil war in 2011, for instance, the government of Bashar al-Assad massively increased its arms imports. According to data from the Stockholm International Peace Research Institute (SIPRI), inflows of major conventional weapons (MCW) between 2008 and 2011 increased by a factor of 8 compared to the previous four year period 2004–2007. The main suppliers were Russia and Iran. As the conflict escalated, Syrian rebels also massively increased their arsenals using various sources such as illicit arms markets, Syrian government stockpiles and battlefield captures, but also the help of foreign governments like the United States and Turkey (Dick 2019). According to a new data set presented in chapter 4, they were able to acquire not only small arms, like rifles and machine guns, and light weapons, like missile launchers, but also MCW such as armored vehicles, tanks and artillery.

These findings do however not mean that every outbreak of violence is preceded by a clear pattern of arms inflows. In the case of the Ukrainian civil war[2] in the Donbas region and before the Russian invasion in 2022, we find virtually no transfers of MCW to the Ukrainian government or the separatists prior to the eruption of violence in 2014. Only after its outbreak did arms transfers occur to both the Ukrainian government (according to SIPRI data) and the separatists (Grove and Strobel 2014).

From a policy-making perspective, a better understanding of how armament processes and the (un-)availability of weapons affect both the outbreak and dynamics of intrastate conflict processes is vital. As will be argued in this chapter, research on the effects of external interventions in civil conflicts has so far neglected the essential role of armament dynamics. However, it is not a priori clear, for instance, whether sending arms to a government helps in successfully deterring a domestic challenge; whether it will lead to escalation; or whether providing military support to a rebel group will make them more belligerent, thus creating a moral hazard problem that undermines any chances for a negotiated outcome. Similarly, it is unclear if preventing the build-up of conflict parties' military capacities via arms embargoes really is an effective tool that can prevent violence, reduce its intensity and help bring about a peaceful settlement.

Answering these and related questions requires rigorous theoretical and empirical work that has to overcome several obstacles. This chapter highlights four major issues:

1. There is little theoretical research explaining the nexus between armament dynamics, different types of arms and intrastate conflicts. Consequently, empirical research has often lacked a stringent theoretical foundation that justifies the choice of hypotheses and guides the interpretation of the results.

2. Endogeneity is a major stumbling block that has often not been sufficiently addressed in earlier studies. The anticipation of a future conflict escalation will drive the demand for arms, making it thus much more challenging to isolate the causal effects that military build-ups and arms flows may have on intrastate violence. Reverse or reciprocal causality needs to be considered both theoretically and statistically.

3. While there is plenty of information on government military endowments and arms transfers to and from governments, at least concerning major conventional weapons, very little data exists for rebel groups. The situation is even worse for small arms and light weapons (SALW). There is only limited data on transfers and virtually none on rebel group arsenals.

4. No attempts have been made so far to link the domestic dynamics of armament

[2] Although heavily influenced by Russia, the separatists are categorized as non-state actors and thus the conflict as internationalized intrastate conflict by UCDP.

> processes with the role of external actors and interventions. However, in most cases, neither governments nor rebel groups have the capacities to produce arms themselves. They therefore have to rely on weapon transfers from external actors whose interests and behavior will also affect intrastate conflict dynamics.

While there are a number of literature surveys in the field of intrastate conflict research (e.g. Brubaker and Laitin 1998; Dixon 2009; Blattman and Miguel 2010; Regan 2010; Rohner 2018; Zürcher 2021; Rauta 2021b; Walter et al. 2021), none of them deal with the role of arms. Providing an overview will aid researchers in better evaluating results as new data and more advanced methods are employed.[3] This chapter therefore aims to assess the state of a growing literature that focuses on the issues of armament dynamics and civil conflicts. In doing so, we highlight the existing theoretical and empirical challenges and present ways to improve research further. In particular, we propose to adapt well-known crisis bargaining models to explain armament decisions and how they affect onset, intensity and duration of intrastate conflicts[4]. We also discuss the statistical and conceptual issues of dealing with endogeneity problems. Additionally, we emphasize essential links to the literature on external interventions and seek to connect the two research strands that have so far ignored each other.

Throughout this chapter and thesis, armament will be defined as the military hardware available. This includes major conventional weapons as well as small arms and light weapons. We deliberately do not consider other critical military dimensions such as weapons of mass destruction, cyber- and A.I. capabilities (for an overview, see Horowitz 2020), as well as matters related to combat personnel recruitment, resolve and training. Furthermore, the focus in this chapter is on militarized disputes: when talking about the onset or dynamics of civil conflicts, we mean conflicts that involve military fighting between a government and at least one non-state actor.

The chapter is structured as follows: The next section provides a comprehensive survey of the empirical literature, which analyzes the role of armaments concerning the three main areas of civil conflict research: onset, duration and intensity. Informed by this literature overview, we then propose a simple theoretical framework based on standard crisis bargaining models. Such a framework can help to derive hypotheses more rigorously and aid in interpreting empirical results.

Additionally for this thesis, I present a more in-depth analysis of selected formal theoretical frameworks that can advance the theoretical understanding of arming and civil conflict: a deeper dive into an adapted version of a dynamic armament model by Besley and Persson (2011), focusing on potentially different effects of heterogeneous

[3] As an example of why such an overview would help research, a recent study by Gallea (2023) in the Journal of Development Economics completely misses the existing Political Science literature.

[4] This is not to say that there are no other important dimensions such as non-violent dissent, repression, one-sided violence or different forms of low-level violence. We focus here on the issues most relevant to research on intrastate wars.

armament of small arms and major weapons; a framework based on the contest success function providing simple, but helpful perspectives on conflict duration; and an application of Powell's (2012) power shift model (which is laid out more detailed in chapter 2 in the context of onset risk) to armament's impact on duration.

Turning to approaches of empirically testing these theoretical mechanisms, the fourth section highlights the statistical and measurement challenges that still significantly hamper current research and briefly maps out possible solutions. For this thesis, I provide an additional discussion of strategies and instrumental variables to encounter potential demand endogeneity of armament. The chapter concludes by discussing the policy implications that can be drawn from the literature and charting a path for research in this critical area.

2 What we know so far about armament dynamics and intrastate violence – empirical findings

2.1 Empirical research on conflict onset and incidence

In the seminal article by Fearon and Laitin (2003), military capabilities are argued to be an essential building block of state capacity[5], which is in turn operationalized as GDP per capita. While higher GDP per capita is clearly associated with lower risks of civil conflict (Fearon and Laitin 2003; Collier and Hoeffler 2004), results for direct measures of military capabilities, like military expenditures and military personnel, are less conclusive. Hegre and Sambanis (2006) find a negative, deterring relationship of military personnel and civil conflict risk, while in Walter (2006), personnel numbers do not affect the chances of a government accommodating challengers.

For military expenditures, Henderson (2000), Krause and Suzuki (2005) and Collier and Hoeffler (2006) detect increased probabilities of conflict with higher military spending. Taydas and Peksen (2012), however, do not find a statistically significant relationship in their more recent analysis. Notably, only Collier and Hoeffler (2006) are concerned with potential endogeneity, employing an instrumental variable approach to rule out reverse causality due to the possibility that military expenditures might be driven by expectations of internal conflict. That military spending is driven by threat perception is clearly demonstrated by studies that find that the probability of militarized interstate disputes strongly affects the size of defense budgets (Nordhaus et al. 2012).

[5] Some have argued that states have different types of capacities—fiscal, administrative, redistributive, policing, military, etc.—to peacefully solve domestic conflicts or to quell challenges (cf. Fjelde and Soysa 2009; Hendrix 2010).

Military expenditures, however, are a rather crude measure of armament levels. They are also not always closely related to the amounts of arms imported (cf. Pamp and Thurner 2017). Arms trade is not solely based on economic terms, but is often funded by barter trade or conducted as military aid (Brzoska 2004; Pamp and Thurner 2017). Thus, a separate strand of literature developed that focused on how the international arms trade affects intrastate conflict onsets. Craft (1999) and Durch (2000) relied on the conflict definitions by the Correlates of War data project (COW) employing simple bivariate correlations. Using SIPRI's data on MCW trade, Craft (1999) finds a weakly positive relationship between aggregated global arms imports and the number of conflicts in the next year. On the national level, contemporaneous, lagged and moving-average imports also have a statistically significant positive association with conflict onset. However, both inter- and intrastate conflicts are pooled together in this analysis. Durch (2000) provides separate analyses, which again find a positive correlation between aggregated imports and interstate onsets, but no statistically significant relation for intrastate conflicts.

Beginning with Craft and Smaldone (2002, 2003) and Suzuki (2007), multivariate analyses were employed to control for important confounders like economic development and the political system. Craft and Smaldone (2002, 2003) use data from the UCDP/PRIO Armed Conflicts Database, which sets a substantially lower threshold of 25 deaths compared to COW. In Craft and Smaldone (2002), they build a combined indicator for both interstate as well as domestic conflict, whereas they focus only on intrastate conflicts in sub-Saharan Africa in Craft and Smaldone (2003). For measuring arms imports, the World Military Expenditures and Arms Transfers (WMEAT) database is used. Their empirical analyses show that arms imports and military spending have a statistically significant positive association with the probability of political violence. This effect is also clearly significant for sub-Saharan African countries. In contrast to these two studies, Suzuki (2007) does not find a statistically significant relationship between arms imports and intrastate conflict. He, however, uses conflict data from the State Failure Project and SIPRI data for MCW delivered by the five permanent members of the UN Security Council. Hess (2020) uses a "coercive capacity index" based on military expenditure, personnel and armored fighting vehicles, using data from the Military Balance+ database (see section 6.2). Re-analyzing models from Fearon and Laitin (2003) and Collier and Hoeffler (2004) with the coercive capacity index as explanatory variable, he does not find a statistically significant relationship with civil conflict onset.

Thus, the results of these non-causal analyses are as diverse as the different studies' operationalizations of conflict and armament, data sources, scope, and methods. However, findings of a positive impact of armament on intrastate conflict onset align with methodologically advanced recent studies by Pamp et al. (2018), Magesan and Swee

(2018) and Mehltretter (2022/chapter 2). These newer analyses use a wide-ranging sample of both developing and developed countries and apply advanced econometric techniques to not only establish correlations but also provide causal findings. When states import larger amounts of arms in preparation for a conflict, reverse causality might affect the empirical results. Focusing on MCW and covering 137 countries from 1953 to 2013 in Pamp et al. (2018), my co-authors and I therefore employ simultaneous equations and instrumental variable models to account for reverse causality. We present strong evidence of a positive causal effect of major weapons imports on the probability of civil conflict outbreaks. In conflict-prone settings, our in-sample predictions reveal an increased probability of conflict onset by 2.3 percentage points if average arms imports are increased by one percentage point. Similar conclusions can be drawn from Magesan and Swee (2018) who focus on US arms sales to 191 countries over the period of 1970 to 2008. Exploiting price effects due to US inflation, their instrumental variable models indicate that higher US sales lead to a higher risk of conflict in the buyer country.

In Mehltretter (2022/chapter 2), on the other hand, I focus on small arms as a distinct military technology. In contrast to major weapons, I find armament with small arms to have no or even a risk-reducing effect on intrastate conflict. The most recent study by Gallea (2023) also focuses on small arms and light weapon imports but is restricted in scope to Africa. He uses the weighted average of arms suppliers' war involvement outside of Africa as an instrument and finds some evidence that arms imports make particularly one-sided violence more likely. His results for conflict incidence are inconclusive.

An escalatory effect of arms imports has also been suggested for other types of intrastate violence that are sometimes related to onsets. Blanton (1999), for instance, finds that arms imports are associated with poor human rights conditions in developing countries. Concerning regime stability, Maniruzzaman (1992) provides evidence that weapon transfers make the occurrence of coup d'etats more likely. However, Choulis et al. (2023) find that increased military capabilities in form of higher mechanization decreases coup risk. A study by Boutton (2019) suggests that US military aid makes newly established democracies and personalist regimes more prone to anti-government violence. Overall, the empirical results so far indicate that at least under some circumstances, importation of major weapons may contribute to conflict escalation, while the effects can differ for other types of weapons like small arms, as chapter 2 in this thesis shows.

2.2 Empirical research on conflict duration

Only relatively few studies have empirically investigated the impact of the government's armament on conflict duration. In terms of the effect of the overall size of the government's army, research has mainly focused on conflict outcomes. The empirical results suggest that a larger army reduces the likelihood of a peaceful settlement and increases the chances of military victory for the government (DeRouen and Sobek 2004; Mason and Fett 1996; Mason et al. 1999). With respect to transfers of military equipment to governments, Sislin and Pearson (2001) find that total arms imports are usually higher in prolonged conflicts. However, they acknowledge that this may be driven by the endogenous demand for arms. Focusing exclusively on imports of major conventional weapons, Moore (2012) finds that increased volumes of such imports by the government are associated with longer conflict duration. Note, however, that Moore only uses one observation per conflict, not allowing for variations over time in the covariates and not distinguishing between different non-state adversaries involved in the same conflict. In Mehltretter and Thurner (2023/chapter 3), we resolve these shortcomings and embed imports of major weapons in a dyadic research design with yearly observations. To rule out effects of endogenous demand for arms, we adopt an instrumental variable survival model, using arms not related to civil conflict as an instrument (cf. Pamp et al. 2018). In contrast to Moore (2012), we find that increased arms imports can lead to shorter durations, especially in conflicts against weaker rebels located in remote areas. Major weapons are thus argued to be essential for projecting military power over larger distances.

While not directly investigating the temporal extent of conflicts, Lyall and Wilson (2009) argue that increased ground mechanization, i.e. the ratio of military equipment to troop size, leads to less direct contact with civilians and combatants, thereby losing access to valuable intelligence. Consequently, mechanization might have a prolonging effect, when governments would be less capable of ending conflicts earlier. Caverley and Sechser (2017) reach strikingly different conclusions with their focus on the mechanization of government forces, distinguishing between "ground" and "air" mechanization. They find that especially combined (i.e. interacted) mechanization leads to shorter conflicts, making ground and air mechanization strong complements. Taken together, Mehltretter and Thurner (2023/chapter 3) and Caverley and Sechser (2017) provide substantial empirical evidence that armament is in fact able to reduce conflict duration under specific circumstances.

Kalyvas and Balcells (2010) and Lockyer (2010) argue that rebels' specific warfare strategy is the direct consequence of the configuration of military power in a conflict: strong rebels and strong states fight conventional civil wars, weak rebels facing strong states employ guerrilla tactics, and when states and rebels both have limited capabilities, "symmetric non-conventional" warfare emerges (Kalyvas and Balcells 2010: 418). In turn, the type of warfare affects the duration of conflicts, as conventionally fought conflicts are assumed to bring forth faster resolution, while guerrilla and symmetric non-conventional tactics allow rebels to evade decisive battles and to survive longer

(Balcells and Kalyvas 2014).

In line with these arguments, Cunningham et al. (2009) suggest that greater rebel strength leads to shorter conflicts due to a higher capacity to target the government. Using the Non-State Actors dataset (Cunningham et al. 2013), they find empirical evidence that "rebels at parity" with and "rebels stronger" than the government fight relatively shorter conflicts. These results are corroborated in other studies using these data, e.g. Buhaug et al. (2009) and Wucherpfennig et al. (2012). Employing a different measure for the relative capabilities of rebels based on the troop ratio of government and rebels, Hultquist (2013) concludes that more symmetric capabilities distributions increase the probability of ceasefires or settlements. However, he finds no statistically significant relationship for government and rebel victories. Caverley and Sechser (2017) employ the Non-State Actors dataset in their analyses and do not identify significant effects of their rebel strength variables on duration either.

Only Moore (2012) measures the rebels' military capabilities more directly by using major conventional weapons transfers to rebels, for which SIPRI provides some data. However, this approach is highly problematic because SIPRI only records an extremely small number of those transfers to a minimal number of groups. Probably as a consequence, Moore's analysis exhibits no statistically significant effect of rebels' major weapons imports on conflict duration.

Rebels' military capabilities might also depend on the provision of external support. In general, interventions and military support are found to prolong conflicts due to complicating the bargaining processes and enabling rebels to pursue more enduring battles (Regan 2002; Balch-Lindsay and Enterline 2000; Brandt et al. 2008; Cunningham 2010; Cederman et al. 2013). However, Collier et al. (2004) find the opposite, namely a conflict-shortening effect of external support, whereas in Moore (2012) and Caverley and Sechser (2017), military assistance does not affect conflict duration. Using more granular data discerning between different types of support, Sawyer et al. (2017) show that more fungible support for rebels, like funding and weapons, prolongs conflicts, while direct intervention by sending troops leads to shorter conflicts.

2.3 Empirical research on conflict intensity

The empirical literature on conflict intensity has employed different indicators to analyze the effect of armaments on conflict intensity, usually measured by the number of battle-related deaths. One strategy is to use the ratio of military expenditures to military personnel as an indicator for military quality (Bennett and Stam 1996). Lacina (2006) uses this as a proxy for "counterinsurgency capabilities" (p. 284), but does not find a statistically significant relationship with conflict intensity. De Juan and Pierskalla (2015) look at regional variation in the manpower of security forces in the South

Sudan conflict but do not identify a significant effect.

More direct measures of governments' military capabilities produce differing results, indicating that higher capacities are not used to contain conflict, but to intensify fighting. Moore (2012) finds arms imports to increase intensity, but this effect loses statistical significance after correcting for a coding error (Mehrl and Thurner 2020). Employing an IV approach, Mehrl and Thurner (ibid.) analyze the separate effects of major weapons and small arms imports on conflict severity, distinguishing between rebels weaker than or at least as strong as the government. Additional armament does not translate into more battle-related deaths when fighting weaker rebels, as the additional capabilities are argued to be ineffective in irregular conflicts. However, against stronger adversaries, meaning more conventional battle situations, importation of both small arms and major weapons increases a conflict's intensity. Focusing on small arms, Gallea (2023) finds that an increase in their imports leads to deadlier conflicts. Using a different perspective, the results by Hultman and Peksen (2017) confirm these effects of weapon transfers. They analyze the impact of arms embargoes and show that these tend to lower conflict intensity.[6]

In Fritz et al. (2021), arms imports are used to forecast conflict fatalities in a highly disaggregated manner. Using a count data approach, they develop a hierarchical hurdle regression model to address the prediction challenge at a monthly PRIO-grid level. This allowed them to obtain predictions on whether casualties will be observed at the national and 50x50 km^2 grid-cell level and also enabled them to make predictions about the overall number of fatalities. Here, higher volumes of arms imports generally predict higher intensities. However, they also argue that a grid cell's remoteness moderates the effects of military build-ups. Fauconnet et al. (2019) find that exports of MCW in general increase battle intensity, while the specific exports of France decrease it. They attribute this to a selection effect of the countries France exports to and of the types of weapons which are argued to be more "defense-oriented".

In a case study on the Sri Lankan Civil War involving the rebel group of the Liberation Tigers of Tamil Eelam (LTTE), Sislin and Pearson (2006) follow armament and escalation patterns closely. They find that the government's arms acquisitions preceded phases of increased conflict severity. Besides, they also conclude that the government stepped up its arming efforts in reaction to escalations by LTTE, rendering armament endogenous to the conflict dynamics. Supporting the findings regarding direct effects of armament, Petersohn (2017) and Lees and Petersohn (2023) find that the support of private military companies, which can be interpreted as a form of indirect additions to the government's military capabilities, increases conflict severity.

Not only does the government's military capacity affect how conflicts are fought, but

[6] Notably, Klomp (2024) finds however that arms embargoes generally have only limited impact on hindering arms trade with the embargoed countries.

so do rebels' fighting capabilities and their interaction with the government's capabilities (Balcells and Kalyvas 2014; Mehrl and Thurner 2020). Directly measuring rebel strength with an ordinal indicator from the Non-State Actors data set (Cunningham et al. 2009) has yielded mixed evidence: Hultman et al. (2014) do not find a statistically significant relationship of rebel strength and intensity, while Heger and Salehyan (2007) and Lujala (2009) find that greater rebel strength is associated with more deadly conflicts. Finally, Mehrl and Thurner (2020) show that conflicts are more intense when insurgents are militarily at least as strong as arms-importing governments.

2.4 Summarizing the state of the literature

The state of the literature can be summed up as follows: most of the earlier work did not use a coherent theoretical framework and relied rather on ad-hoc hypothesizing. Furthermore, these earlier works differed considerably in their regional and temporal scope. They were thus neither comparable nor generalizable (e.g. Blanton 1999, Craft 1999, Craft and Smaldone 2002, Craft and Smaldone 2003, Durch 2000, Sislin and Pearson 2001, Sislin and Pearson 2006, Suzuki 2007). Some do not properly distinguish between small arms and major conventional weapons, even though different types of arms may have different effects on conflicts (cf. Mehltretter 2022/chapter 2). They all have scarce data on the military capabilities of rebel groups and mostly do not pay sufficient attention to endogeneity caused by reverse causality, which arises due to the demand for military equipment in anticipation of a conflict outbreak or a continuation of military fighting. Recent work tries to address some of these issues. Pamp et al. (2018), Magesan and Swee (2018), Mehrl and Thurner (2020), Mehltretter (2022/chapter 2) and Gallea (2023) use instrumental variable approaches to circumvent the endogeneity problems. Moore (2012) and Mehrl and Thurner (2020) take rebel armaments into account, but the former relies on a minimal number of data points, whereas the latter only use very coarse ordinal measures.

Moreover, most of the earlier studies focused at the national or, when analyzing ongoing conflicts, the conflict level. Only recently has work begun to take a more disaggregated approach by analyzing conflict dyads (Mehltretter and Thurner 2023/chapter 3) and even 50x50 km^2 PRIO-grid levels (Fritz et al. 2021). However, even most of the latter studies lack comparable, well-developed formal-theoretical approaches. Moreover, the literature has so far completely ignored how the conflict parties acquire their arms and how the incentives of outside actors providing military aid may affect conflict dynamics. After all, armament decisions are endogenous to external interventions, and third-party behavior may also be endogenous to armament dynamics. The following section outlines a simple theoretical framework and addresses these questions.

3 Theory overview and in-depth analysis of selected frameworks: Armament and conflict onset

3.1 Utilizing interstate crisis bargaining models

Theoretical work on militarization and armament processes in *interstate* wars has expanded considerably over the years. Military build-ups, especially in the form of mutually reinforcing arms races, have long been assumed to lead to the military escalation of disputes (cf. e.g. Wallace 1979; Diehl 1983; Intriligator and Brito 1984; Brzoska and Pearson 1994; Gibler et al. 2005). Most of the literature relies heavily on game-theoretic crisis bargaining models (e.g. Kydd 1997; Powell 2002; Slantchev 2005; Meirowitz and Sartori 2008; Jackson and Morelli 2009; Chassang and Padró i Miquel 2010; Slantchev 2011; Baliga and Sjöström 2020). However, unlike in the interstate literature, there is little formal theoretical research on how armament dynamics affect specifically intrastate conflicts.[7] Powell (2012) presents a model that analyzes how shifts in relative power between governments and challengers can lead to civil wars. Mehltretter (2022/chapter 2) extends this model to formally analyze how small arms and major weapons affect power shifts and thus conflict risks. Finally, Meirowitz et al. (2022) propose a model that illuminates how third-party interventions affect armament decisions and the probability of fighting.

Crisis bargaining models are therefore a natural starting point as there are strong similarities between inter- and intrastate conflicts. The main difference, of course, is that in civil conflicts, a government and (at least) one non-state actor within a state territory confront each other. Moreover, we often find a higher power asymmetry here than in interstate disputes. Our aim in the following sections is to provide a straightforward general theoretical framework and to show how this model can help derive more theoretically grounded hypotheses that take into account that the effect of arms on conflict risk could be highly conditional.

3.2 A simple theoretical framework

Violent intrastate conflicts usually do not simply arise because of the presence of weapons. Conflict research has identified a large number of possible factors that help explain intrastate violence. If the conditions opening pathways to violent conflict—like grievances and other tensions—are not present, then military build-ups should not trigger violence. Throughout our following theoretical considerations, we assume that the general preconditions for conflict exist between a central government and one or more rebel groups. However, when these conditions are present, armament impacts the dynamic of conflict emergence and fighting considerably.

[7] Note that other parts of the civil war literature have drawn more heavily on these rationalist approaches (cf. Walter 2009a,b).

Armament decisions can be understood as part of a strategic game between two or more parties. We first outline the interaction between the government and a challenger before discussing possible extensions that take procurement channels and external influences into account. From a game-theoretic perspective, civil conflict can be modeled as a particular form of a crisis bargaining game in which the actors must decide whether to use military force to resolve a dispute or choose a negotiated settlement. Moreover, both sides must decide whether and how much to invest in their military capabilities to prepare for potential future confrontations. The conflict can thus be represented as an adapted version of the sequential games of Kydd (1997, 2000) with the following stages:

1. First bargaining round: The government makes an offer to the rebel groups to settle the dispute. The rebels can decide whether or not to accept it. In addition, both sides must decide whether to launch a military attack.
2. Armament round: If the government's offer is rejected, both sides decide how much to increase their military capacities.
3. Second bargaining round: The government again makes an offer to the rebel groups to settle the conflict. It can decide whether it is the same offer, an improved offer or a worse offer. The rebels decide again whether or not to accept the offer.
4. Resolution round: If no peaceful solution is reached, both sides decide whether to try to resolve the conflict militarily.

The interests between the government and the rebels are often zero-sum, so the stake to be bargained over can be represented as a value x normalized to the range 0 to 1. For example, in the Ukrainian civil conflict before the invasion of Russia, x represented the share of the Donbas region under complete government control. Thus, in a possible agreement, the government receives x, and the rebels receive $1 - x$. In the armament phase, both sides can decide to spend some of their overall resources Y_i on the military. This results in M_g and M_r, the levels of military armament of the government g and the rebels r.

The often-found asymmetry in the power distribution between the government and the rebels is modeled in two ways. On the one hand, the bargaining rounds take the form of an ultimatum game, meaning that the government can propose a solution unilaterally, and the rebels can only agree to or reject it. Second, and this is a major difference to interstate conflicts, the government usually has a clear advantage in the armament round. In most cases, only a state has the resources and capabilities to acquire arms on a large scale. Thus, the armament phase could significantly worsen the rebels' position, which in turn will affect the government's offer in the second round. In general, military build-ups affect the probability p of emerging victorious in a military conflict and therefore also have a direct impact on the second bargaining round as the range of possible settlements is also determined by it. The probability that the government wins a military confrontation can be derived from a so-called contest success function (CSF) (cf. Hirshleifer 1989; Skaperdas 1996), i.e. from the ratio of the military capacities M of the two conflict parties $p_g = \frac{M_g}{M_g+M_r}$.

Power shifts in favor of the government will lead to a revised and worse offer to rebels in the second bargaining round. At the same time, however, it increases the rebels' willingness to accept an offer. A power shift can occur, for example, if the government increases its own military capabilities by a factor β in the armament round, thus increasing the probability of emerging victorious from a military conflict to $p'_g = \frac{\beta M_g}{\beta M_g+M_r}$ if the rebels themselves do not arm. However, the opportunity costs of armament entail that these resources cannot be spent for other purposes (Powell 1993; Besley and Persson 2011). Therefore, not every desired military build-up is possible. After the second bargaining round, in which x can be revised, both sides decide about whether to attack the other. Military fighting will entail costs for the government and the rebels of c_g and c_r, respectively.

This type of crisis bargaining model is common in the analysis of conflicts and allows us to formulate expectations about how military build-ups affect the probability of military fighting, i.e. the outbreak of a civil war, as well as its dynamics. It should be noted here that, as Fearon (1995) has shown, a military conflict is always inefficient ex-post. Given the interests and power distributions, there is always an ex-ante bargain that both sides prefer to a costly war. Therefore, the question arises as to when and under what conditions an arms build-up makes the realization of a peaceful settlement less likely.

Preceding conflict stages like dissent or military threat, repression and the resort to terrorism (potentially also occurring during conflict) also require an explanation (cf. e.g Auer and Meierrieks 2021). The simple game-theoretic framework presented here is one possible way to examine these issues coherently. For instance, in Meirowitz et al. (2022), only the rebels are assumed to arm, and their armament decision—which is binary, high or low—cannot be fully observed by the government. Section 3.4 additionally provides an in-depth analysis of an adapted version of a dynamic armament framework from Besley and Persson (2011).

3.3 Applying the simple framework in relation to the literature

Using this general framework, several mechanisms can be identified that explain how armaments affect the outbreak of military conflict: first, armaments change the relative balance of power p_g. If this shift cannot be perfectly observed by the two sides, and both sides have an incentive to disguise their true military strength and resolve, this will impede finding a bargaining solution in the second round of negotiations. The reason is that p_g directly affects the ability of both parties to prevail in negotiations. Military power contributes to bargaining strength, and the stronger side can claim a larger share of x. Therefore, both the government and the rebels have a strong incentive to conceal and overstate their true military capabilities. Direct communication between the conflicting parties cannot easily solve the information problem because the signals sent about their own strength are not credible due to these incentives. Thus, asymmetric and incomplete information can lead to a military conflict outbreak that serves to address the lack of information about the relative distribution of power ("fighting as learning"). At the same time, it can also be predicted that if sufficient information is available, armament can function as deterrence and may lead to a peaceful settlement that reflects the relative power distribution in the second bargaining round.

Second, military armament, especially if it is heavily one-sided, can lead to conflict escalation and thus military confrontation, even if there are no information asymmetries. If it is foreseeable that the government side can gain military superiority or increase its relative capabilities in the future through armament—meaning that p_g would increase sharply—then the other party might preemptively seek a military confrontation.

Thus, importing weapons in this situation leads to a substantial power shift, which would directly affect the outcome of the second round of negotiations. The weakened side could consider this outcome so bad that it prefers an immediate military confrontation to what it sees as a worse negotiated solution in the future. As a result, even the expectation of a massive build-up of arms by one side can lead directly to an outbreak of conflict after the first bargaining round. This aligns with the model by Powell (2012), which predicts that a rapid shift in power leads to an onset of fighting between rebels and the government (for details, see chapter 2). If, for example, the rebels assume that their chances of winning will continue to decline in the future, they will preemptively seek a military confrontation with the government. Moreover, military escalation could be a strategy for recruiting supporters (Lichbach 1998) or serve to mobilize external support. Attempts to organize and overcome collective action problems may therefore explain why rebels nevertheless opt for violent conflict despite government superiority.

Third, and closely related to the previous argument, armament can exacerbate the so-called commitment problem (cf. Fearon 1995; Walter 1997, 2009b). A negotiated settlement usually requires the rebels to reduce or completely give up their military capabilities. However, this carries the risk that the government side will take advantage of this post-conflict situation and will not implement the negotiated settlement. The disarmed rebels would then be in a worse position and have no means to prevent this violation of the agreement. Arming on the government side is therefore doubly problematic for the rebels. On the one hand, it strengthens the government's future negotiating position. On the other hand, it exacerbates the commitment problem, as the rebels are in an even weaker position to resist a breach of the agreement by the government. Therefore, the arms build-up could lead to either a failure to reach a peaceful settlement in the second round of negotiations and the rebels opting for military conflict or, anticipating this, the rebels opting for violent conflict already in the first round of negotiations.

Fourth, the core of the conflict between the rebels and the government might not allow for a negotiated settlement whenever compromise is impossible because the government's proposal can only be $x = 1$ or $x = 0$. Such a situation is referred to as „issue indivisibilities“, i.e. the good being disputed (e.g. a territory or who controls the government) cannot be meaningfully shared (Fearon 1995). This might be the case, for example, where the independence of rebel territory is at stake. Here, the risk of military escalation is particularly high. In this case, armament can only lead to successful deterrence if the government gains significant military superiority relative to the rebels.

3.4 A deeper dive into a dynamic armament model

So far, armament has been treated as a homogenous concept. Effects of different types of armament have mainly been unaccounted for in formal frameworks—for instance, if and how acquisitions of small arms versus major weapons impact conflict risk differently. To develop a better understanding of such a heterogeneous concept of armament, this section, which has been specifically added for this thesis, lays out a theoretical perspective on how to grasp the different effects of small arms and major weapons armament. In chapter 2, I detail a power-shifts framework based on Powell (2012) to analyze the differing impact of specific arms types. To provide an additional theoretical perspective, I adapt Besley and Persson's (2011) framework in the following section. Besley and Persson develop a model that formally captures the mechanisms of state capacities affecting civil conflict. In the following, I present the model setup (section 3.4.1), slightly modified and simplified where complexity is not needed for the purposes of this chapter, and the decisions to invest in the military (section 3.4.2). Then, I deviate from the original model and expand it to derive hypotheses on arms imports' effects on conflict risk and incorporate specific effects of small arms (section 3.4.3).

3.4.1 Model setup

There exist two groups $j \in \{I, O\}$, with I being the incumbent group (analogous to the government in our general framework's terminology) and O the opposition (analogous to the rebel group). In each period of time, one generation of both groups is born, inherits the incumbent or opposition status, respectively, and dies at the end of the period. Within any period, the incumbent can lose power to the opposition with probability $\gamma(M^O, M^I)$, which is a contest success function depending on the effective sizes of the opponent's and incumbent's military $M^{O,I}$.[8]

The timing for each period is as follows: the stochastic values of government revenue R and wage level w are realized, then both groups choose their levels of military investment. The configuration of military investments determines whether conflict occurs, described below in more detail, and consequently, the incumbent I either stays in power or gets overthrown by the opponent O. The new incumbent I' then determines public goods and transfers, and payoffs are realized.

Both groups' representative individuals receive utility u^j from public goods G and consumption c^j of private goods. The utility function takes the quasi-linear form

$$u^j = H(G) + c^j \tag{1}$$

with $H(\cdot)$ being an increasing and concave function determining the valuation of public goods.

Private consumption is financed by wage income w from labor and transfers r^j from the government. The government's exogenous revenue R is spent for public goods and the transfers to both groups. Additionally, the incumbent can pay their own military out of the government's budget. The total costs of investment in military strength wM^j are determined by the general wage level w and the effective size of the respective military. The government budget constraint is thus

$$R - G - wM^I - \sum_{j \in I,O} r^j \geq 0\,, \tag{2}$$

which can be assumed to bind and hold with equality. The incumbent has to transfer a fixed proportion σ of the transfer for their own group to the opposition group, $r^O = \sigma r^I$, due to institutional restrictions, which can be captured by $\theta = \frac{\sigma}{1+\sigma}$ ranging from completely exclusive institutions, $\theta = 0$, to cohesive institutions, $\theta = \frac{1}{2}$, where transfers are divided equally. Note that, in contrast to the general framework presented in section 3.2 above, this θ is exogenously given. Transfers are thus defined by

$$r^I = (1-\theta)(R - G - wM^I) \tag{3}$$
$$r^O = \theta(R - G - wM^I)\,. \tag{4}$$

[8]Note that in this section, superscripts instead of subscripts are used for attributing variables to the incumbent/government and opposition/rebel group, respectively, since subscripts are used to denote derivatives, with the objective of a clear presentation of equations.

The expected payoff for the incumbent is thus

$$\hat{v}^I = H(G) + w + [(1-\theta) - \gamma(M^O, M^I)(1-2\theta)][R - G - wM^I] \,. \tag{5}$$

When the incumbent stays in power, they get $1-\theta$ of the transferable budget $R - G - wM^I$. When the opposition takes over, the then former incumbent receives only the share θ. The opponent thus receives θ with certainty and gains the share $(1-\theta)-\theta$ *additionally* when they comes into power, giving them the expected payoff

$$\hat{v}^O = H(G) + w(1 - M^O) + [\theta + \gamma(M^O, M^I)(1-2\theta)][R - G - wM^I] \,. \tag{6}$$

While the state budget bears the costs for the incumbent's army M^I, the opponent's M^O has to be paid from their own income.

3.4.2 The decision to invest in the military

Both the incumbent and the opponent choose the investment in their military simultaneously, similarly to stage 2 in the general model presented above. As Besley and Persson (2011) show, there exists a unique Nash equilibrium in pure strategies that depends on the realization of the stochastic variables w and R as well as the valuation of public goods $H(\cdot)$. The first-order optimality condition for the choice of M^I then implicitly gives the chosen, optimal level of the incumbent's military

$$\hat{M}^I = \frac{1 - \theta - \gamma(\hat{M}^O, \hat{M}^I)(1-2\theta)}{\gamma_I(\hat{M}^O, \hat{M}^I)(1-2\theta)} + Z \geq 0 \tag{7}$$

with $\gamma_I(M^O, M^I)$ being the first derivative of γ w.r.t. M^I and $Z = \frac{R-G}{w}$, as proposed by Besley and Persson, to measure the privately available government resources, adjusted for the wage level. The opponent's military choice $\hat{M}^O$ is implicitly defined by the respective first-order condition:

$$\gamma_O(\hat{M}^O, M^I)(1-2\theta)[Z - M^I] \geq 1 \tag{8}$$

The left-hand side gives the marginal return of the investment in M^O, calculated as the marginal effect on the probability of taking over power times the additional transfer benefit. This is equated to the marginal cost, which is 1, as we adjusted the equation for the wage level.

As Besley and Persson (ibid.) prove, the model brings about three states, which they define as peace, repression and conflict. Thus, in contrast to our general framework, arming decisions directly define conflict occurrence, which is not a separate decision in Besley and Persson's model. In peace, neither the incumbent nor the opponent group invests in the military, $M^I = 0 \wedge M^O = 0$. Repression occurs when only the incumbent has a military, $M^I > 0 \wedge M^O = 0$. Conflict arises when both the incumbent and the opponent invest in their military capabilities, $M^I > 0 \wedge M^O > 0$. This unique ordering of states results from the different marginal costs the two players have to bear: only the opponent has to pay their own military fully. The three states of peace, repression and conflict give rise to two thresholds Z^I and Z^O: with $Z < Z^I$, peace occurs; with $Z > Z^I \wedge Z < Z^O$, repression; and with $Z > Z^O$, conflict.

The inclusiveness of political institutions, measured by θ, reduces the risk of conflict and repression. This relationship can easily be inferred from equations 7 and 8 which show a decreasing effect of θ on investment in the military due to reduced expected returns of taking over power. With completely cohesive institutions, $\theta = \frac{1}{2}$, there is never investment in the opponent's military and therefore no conflict.

To examine the effect of exogenous armament changes in our parameters, we have to define the two thresholds Z^I and Z^O. At Z^I, both M^O and M^I are 0, but the first-order condition for the incumbent holds with equality, as they are indifferent between investing and not investing into the military at this threshold. We can thus rearrange to define Z^I as

$$Z^I = \frac{1 - \theta - \gamma(0,0)(1-2\theta)}{\gamma_I(0,0)(1-2\theta)}. \tag{9}$$

In the same way, we can define the conflict threshold Z^O implicitly where the opponent's investment M^O is still 0, but their first-order condition holds with equality:

$$\gamma_O(0, \hat{M}^I(Z^O))(1-2\theta)[Z^O - \hat{M}^I(Z^O)] = 1 \tag{10}$$

3.4.3 The effect of exogenous changes in the incumbent's military capabilities

Modeling military capabilities Until now, we took the military capabilities of both the incumbent and opponent as completely endogenously determined choice variables. For the purpose of this thesis' analyses, it is however helpful to deviate from the original model in Besley and Persson (ibid.) by interpreting military capabilities also as a function of inputs: conflict technology ξ and military personnel L determine military capabilities M, i.e. $M^O(L^O, \xi^O)$ and $M^I(L^I, \xi^I; \rho)$. Since the incumbent has to use their military also for defending the state in international conflicts, their decision on the investment in military capabilities also depends on the exogenous parameter ρ measuring the external level of threat. As we expect military capabilities to increase in both inputs and the threat level, we can denote the respective capabilities as

$$M^I(\overset{+}{L^I}, \overset{+}{\xi^I}; \overset{+}{\rho}) \text{ for the incumbent and} \tag{11}$$

$$M^O(\overset{+}{L^O}, \overset{+}{\xi^O}) \text{ for the opponent.} \tag{12}$$

As all inputs affect $M^{I,O}$ positively, the effects we derive for the reduced expressions M^I and M^O in the next section can be interpreted implicitly as capturing the directions of the effects of L, ξ and ρ.

Effects of exogenous armament changes on conflict and repression threshold My theoretical ramifications aim to model the effects of arms imports on the risk of conflict onset and—as it is implemented in the Besley and Persson model—the level of repression. Therefore, we have to examine the impact of exogenous increases in governmental arms imports on the two thresholds Z^I and Z^O. With lower thresholds, we are more likely to encounter environments that produce conflict or repression, i.e. the risk of conflict and repression rises.

In this model, repression is directly defined in dependence of the military investment: whenever the government's investment is strictly positive, we experience repression. Thus, any increase in military capabilities results in a higher level of repression. The addendum to chapter 2 discusses the nexus of arming and human rights violations in more detail.

For the effects on conflict risk, we have to evaluate the effect of changes in M^I on Z^O. When we assume $\hat{M}^I(L^I, \xi^I; Z^O, \rho)$ to be marginally changed exogenously due to a rise in ρ, we can explicitly solve equation 10 for the new threshold Z'^O in the new environment and derive how it is affected by the exogenous change in M^I:

$$Z'^O = M^I + \frac{1}{\gamma_O(0, M^I)(1-2\theta)} \tag{13}$$

$$\frac{\partial Z'^O}{\partial M^I} = 1 - \frac{\gamma_{OI}(0, M^I)}{[\gamma_O(0, M^I)]^2(1-2\theta)} \tag{14}$$

The first term captures the direct reduction of state transfers due to higher investment in M^I, while the second term denotes how changes in M^I affect the marginal utility of the opponent's investment in the military. As $\gamma_O(0, M^I)^2(1-2\theta) > 0$ for incompletely inclusive institutions, the direction of the effect depends on the derivatives of the contest success function. To determine the direction, we have to assume a specific functional form of the contest success function $\gamma(\cdot)$, as described by Besley and Persson (2011: 1423). Using a simple form $\gamma(M^O, M^I) = \frac{M^O}{M^O+M^I}$ evaluated at $M^O = 0$, the expression simplifies to

$$\frac{\partial Z'^O}{\partial M^I} = 1 + \frac{1/{M^I}^2}{(1/{M^I}^2)(1-2\theta)} = 1 + \frac{1}{1-2\theta} > 0 \tag{15}$$

Thus, deterrence and the reduction of spoils that would be worth fighting over make investment in the opponent's military, and thus conflict, less attractive.

Incorporating small arms into the model Small arms feature different characteristics than MCW (see chapter 2 for more details), which have to be carefully considered in the theoretical modeling. Adapting Besley and Persson's framework, we can capture two different aspects of small arms: for one, we can assume small arms to be not only used to provide utility in conflict, but also in peaceful times as tools for police forces to establish security $S(M^I)$. Then, security can be seen as a public good that enters the utility functions as additional input for $H(G, S(M^I))$, the valuation of public goods. However, when we inspect equation 13, which determines when we see a conflict onset, it is clear that changes in $H(G, S(M^I))$ do not affect the opponent's decision when to fight: it is only the incumbent that determines $S(M^I)$, meaning the opponent has to take it as a given, independent utility. Apart from that, adding security as a public good will influence the incumbent's decision, thus affecting the risk of repression. Because military investment delivers additional utility via higher security, the incumbent will choose higher military investment levels—meaning higher repression, as defined in this model.

Focusing on another specific difference of small arms over MCW, we can introduce tax revenue $w\tau(M^I)$, where the tax rate $\tau(M^I)$ depends on the incumbent's taxing capacity, which is enhanced by small arms securing enforcing of the laws and general wealth-generating stability of the state. The conflict threshold (equation 13) can then be expressed as

$$Z^O = M^I + \frac{1}{\gamma_O(0, M^I)(1-2\theta)} - \tau(M^I)\,. \tag{16}$$

Here, $\tau(M^I)$ affects the decisions of the opponent because it entails additional financial means that can be captured in case of a power transition. The tax revenue thus also affects how the threshold Z^O changes with exogenous changes of M^I:

$$\frac{\partial Z'^O}{\partial M^I} = 1 + \frac{1}{1-2\theta} - \tau_I(M^I) \tag{17}$$

When taking the effect of small arms on taxing state capacity into account, we are not able to derive a conclusive hypothesis about the effect of small arms on the conflict threshold, as it depends on small arms' effectiveness in raising the taxing capacity τ.

Intuitively, the government can expect additional utility from small arms due to higher τ and larger financial resources, making investments in small arms more attractive. The increased spoils associated with overturning the government might also induce military investments of the challenger at lower Z^O, leading to a less negative or even positive effect of small arms imports on conflict risk.

All in all, this model shows how the impact of importing small arms can differ from importing major weapons because they affect different important factors: for both types, deterrence of adversaries is effective, but small arms might also enhance non-military capacities—in the case of the framework from Besley and Persson (2011), this creates larger spoils and incentives to attempt to capture the state by force. In contrast, in the model presented in chapter 2, based on the framework from Powell (2012), the mechanisms work differently, as power shifts due to increased armament might fuel conflict. However, small arms' impact on non-military capacities may reduce the impact of these power shifts—in contrast to Besley and Persson's framework, small arms could mitigate the generally conflict-inducing effect of armament, as the improved taxing capacity provides increased means to buy off the rebel group. Relating to the general framework presented in section 3.2, this would be equivalent to small arms allowing the government to present an improved offer at stage 3 due to a larger overall state wealth, thus also reducing conflict risk. Empirical results presented in chapter 2 lead credence to these mechanisms, with small arms notably not increasing, but rather decreasing conflict risk. Concerning the empirically found risk-increasing impact of MCW, the deterrence effect at the core of Besley and Persson's framework might play less of a role in the end, at least for MCW. Nevertheless, this framework highlights the ambivalent role of small arms armament when not only focusing on conflict outbreak, but also on repression.

4 Theory overview and in-depth analysis of selected frameworks: Armaments and conflict duration

4.1 Summarizing theoretical approaches in the literature

In the case of an ongoing conflict, the bargaining rounds are now no longer about preventing a military escalation, but about ending an ongoing military conflict. A build-up of military capacities that specifically increases the government's military capabilities M_g would usually result in a larger imbalance of power p_g in favor of the government. This in turn could result in shorter conflicts: chapter 3 presents a war-of-attrition framework (Powell 2017) in detail, where governmental arming raises the costs of fighting for the rebel group, increasing the likelihood that the rebels quit the conflict earlier. The following section gives an overview of theoretical approaches on relating conflict duration and armament, referring again to our general framework.

Then, in-depth analyses of a contest-success-function approach and a power shift duration model are presented.

The importation of weapons eliminates the deficiencies of existing military equipment in certain areas, such as ground transportation, reconnaissance, or air power. Governments will therefore choose to import a portfolio of weapons tailored to the conflict environment in which their military capabilities will be deployed, allowing them to benefit from the utility of specific weapon types in different conflict scenarios. Well-equipped ground forces and advanced air strategies can also be used against weak rebels employing guerrilla tactics (Caverley and Sechser 2017; Corum and Johnson 2003; Smith and Toronto 2010), thus contributing to terminating conflicts sooner. In this context, termination occurs either because the government defeats the rebels militarily or because the rebels accept the government's offer in the next round of negotiations. In light of the higher p_g, they are also willing to accept a worse offer compared to the first bargaining round.

Information is again a key factor here, as is the case in any crisis bargaining model. Information asymmetries arise because the warring parties have only limited information about the other party's military capabilities. In the model, settlements reflect the bargaining power distribution, which in turn is determined by the relative strength and therefore the probability of winning the conflict militarily (see also Licklider 1993). If both sides had complete information about the other side's capabilities and valuation of outcomes, an agreement could be quickly reached, since it were known ex-ante which side would eventually win. From this perspective, fighting can be interpreted as a means to gradually decrease uncertainties over the opponent's military capabilities, thus reducing information problems as the conflict continues (Powell 2006; Weisiger 2016). However, ongoing armament might lead to increased uncertainty over the government's capabilities because changing rates of additional military equipment prevent rebels from learning about the true level of government capabilities. In Shirkey (2016), such changes in military technologies are conceived to be introducing new private information. In a sequential model on interstate conflicts, Slantchev (2003) shows for instance that higher mutual uncertainty over the power distribution—as might be caused by ongoing armament—prolongs conflicts despite "nonmanipulable battlefield outcomes" (p. 621).

As section 4.4 lays out in more detail, Powell (2012) provides a different rationale for the persistence of fighting. Assume that the relative power shifts in favor of the government and the rebel group cannot be sufficiently compensated for this change in the distribution of power. Then, in this model, rebels can decide to forestall the power shift by fighting the government. Powell shows that the threat of further power shifts thus leads to continued fighting, while there is no conflict when the power distribution is relatively stable. Regarding conflict duration, ongoing arming of the government during conflicts can undoubtedly be considered to lead to power shifts in the sense of this model. Arms imports would be expected to prolong conflicts in this situation. Weisiger (2013) corroborates this argument empirically for interstate wars: conflicts that are started to prevent power shifts last longer, as the underlying commitment problems are not resolved throughout the conflict, but only dissolve with a decisive victory. At the same time, armament can also affect the decisiveness of fighting, leading to potentially shorter conflicts in Powell's model, as section 4.4 shows.

Regarding uncertainties over the government's capabilities, arms acquisitions might enable it to more clearly reveal its evident superiority by conducting large-scale attacks, especially in conventionally fought wars (cf. Hultquist 2013). Regan (2002) argues that relatively symmetrical power distributions actually increase the risk of misjudging the odds of victory, thus leading to longer conflicts. Usually, governments are the stronger side in civil conflicts (Lockyer 2010; Buhaug et al. 2009). Then, additional armament on the government side leads to an even more asymmetric power distribution, thus reducing the risk of misjudgment and thereby also decreasing conflict duration. However, at the same time, the government's ability to gain a reliable estimate of the rebel group's military strength might be hindered. While it is generally challenging to evaluate groups employing covert, e.g. guerrilla, tactics (cf. Walter 2009b), Lyall and Wilson (2009) argue that using highly equipped forces in civil war impedes obtaining relevant information from local population.

Commitment problems are the greatest obstacle to ending an intrastate conflict by an agreement. Both sides, the government and rebels, cannot be bound to agree that the other side will keep its promises to adhere to the agreement (Fearon 2004; Walter 1997, 2009b). Armament dynamics play an essential role here because they might undermine the government's ability to commit to a settlement credibly. Governments are generally in a relatively stronger position, which becomes even stronger with agreements that often include at least a partial disarming of rebels (Walter 1997). Rebels therefore have to trust the government not to overturn the deal (cf. Fearon 2004). At the same time, governmental military build-ups *additionally* strengthen the position of the government for the post-conflict time. As a consequence of the increasingly asymmetric distribution of power, rebels might fear that, after settlement, the government exploits its stronger position and aims at reneging on the deal or achieving a military victory after all (Walter 2009b).

Armament can also be interpreted as a costly signal of strength and resolve to the government's adversaries. While in conflict, arms acquisitions indicate that the government does not expect to reach a peaceful settlement and is resolved to continue the conflict militarily. Furthermore, military build-ups provide governments with an additional edge over the rebels, improving their bargaining position for possible negotiations—but rebels might want to avoid an agreement struck at a point in time when the power distribution is significantly shifted in favor of the government due to the imported arms, since they fear complete subjection and might hope for third party intervention in their favor. In contrast, Slantchev (2011) argues that armament can function as a costly signal to push rebel groups into negotiations, making conflict termination more likely. In addition, armaments might also serve as a signal of the government's military strength directed at third parties, preventing them from entering the conflict in support of the rebel side.

4.2 Applying the contest-success-function approach to conflict duration

One of the most prominent theoretical frameworks for analyzing intrastate conflict is the contest success function (CSF) developed by Tullock (1980) and first adapted for conflicts by Hirshleifer (1988) as a "combat power function". Buhaug et al. (2009) undertook a first attempt to derive expectations for the duration of intrastate conflicts from a contest success function, albeit without formally linking duration to the probability of winning given by the function. The issue with standard formulations of the CSF regarding their usefulness for analysis of duration, as noted by Jia (2012), is that it assigns a zero probability to the possibility of a conflict *not* ending, i.e. the probabilities of winning of each conflict party sum up to 1. Thus, conflicts in the standard CSF model only last one period, as continuation of the conflict is not a possible outcome. To make progress over this shortcoming, this section, specifically added for this thesis, presents a modified version of the CSF from Jia (ibid.) to formally implement a positive probability for the conflict continuing.

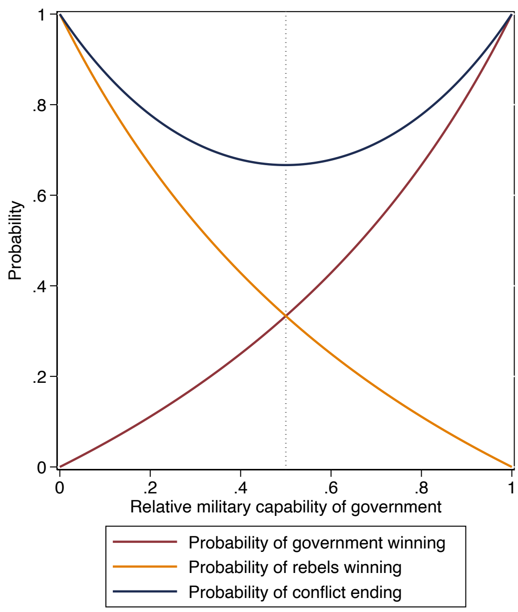
This CSF duration model is based on the general ratio form (cf. Tullock 1980; Hirshleifer 1989). In the two-sided case, it takes the following functional form:

$$\pi_i = \frac{M_i(\cdot)}{M_i(\cdot) + M_j(\cdot)} \tag{18}$$

with $i, j \in \{g, r\}, i \neq j$ denoting either the side of the government g or the rebels r, respectively, $\pi_{g,r}$ being the probability of winning for the government and rebel side and $M_{g,r}$ as their respective military capabilities as functions of military inputs. Due to the common denominator, the probabilities of the government or rebels winning sum up to 1, $\pi_g + \pi_r = 1$, without a possible outcome of continuing conflict. Technically, this CSF describes a conflict with a given duration of one period, rendering it unsuitable for the analysis of variance in duration.

Nevertheless, it is widely noted that the CSF is a fitting stylization of conflict processes (cf. Garfinkel and Skaperdas 2007), which is thus worth adapting to research questions of conflict duration. To analyze conflict duration in a CSF framework, we have to integrate the possible outcome of no side winning, i.e. continuing conflict. Jia (2012) introduces a "coarseness" factor into the CSF framework. This factor k, called

Figure 2: Contest success function with decisiveness parameter $k = 2$



"decisiveness" factor in our application, can be interpreted as a measure of how much stronger one side has to be than the other one to be able to win. In a stochastic environment, the probability of one side winning thus decreases the more symmetrical the distribution of power is and, at the same time, the probability of a conflict continuing increases.

Formally, the decisiveness factor $k > 1$ enters the CSF by raising the weight of the respective adversarial side's capabilities:

$$\pi_g = \frac{M_g(\cdot)}{M_g(\cdot) + kM_r(\cdot)} \tag{19}$$

$$\pi_r = \frac{M_r(\cdot)}{M_r(\cdot) + kM_g(\cdot)} \tag{20}$$

This functional form allows now to set up a multi-period game: in every period, fighting takes place, and the outcome is determined by the CSF. Either the conflict ends in favor of one side, which includes both total military victory and imposed settlements/agreements that reflect the distribution of power, with probabilities π_g and π_r, or the conflict continues into the next, analogously structured period, with probability $\pi_c = 1 - \pi_g - \pi_r$. Thus, this framework formally links the CSF with conflict duration through the probability of continuing conflict π_c.

Figure 2 shows how these probabilities evolve depending on the relative military capabilities (without loss of generality, we can define $M_g + M_r = 1$) for an exemplary value $k = 2$. At symmetrical relative military capabilities, the risk of longer conflict durations is highest. In contrast, the chance that the conflict ends within a given period increases with an increasingly asymmetrical power distribution.

Note that this theoretical result differs from often-stated assumptions that symmetrical power ratios lead to shorter and asymmetric ones, due to stronger rebels, to longer conflict (Cunningham et al. 2009; Buhaug et al. 2009). Such expectations are based on the different strategies of rebels observable in intrastate conflict, i.e. irregular, guerrilla warfare with strong asymmetry and conventional warfare with symmetry. However, when we take strategy into account by controlling e.g. for external support in addition to the troop ratio, the theoretical expectations derived from this CSF approach capture the "pure" effect of relative power. Practically, to end a conflict by winning, one side has to weaken the other side substantially, almost per the definition of "winning", meaning that the distribution of power has to be distinctly asymmetric at the moment the conflict ends. Furthermore, incentives for fighting are larger the higher the probability of winning, because the expected utility from fighting is larger. Thus, with symmetric power distributions, no party has a particularly high incentive to retract from fighting. The CSF duration framework captures these effects and thus predicts conflicts will last longer with more symmetric distributions of military capabilities.

We can use this framework to derive hypotheses how different military factors affect conflict durations. Essentially, the expectations on changes in relative military capabilities depend on which side of the conflict is stronger to begin with. In our case of intrastate conflict, almost always the government is the stronger side, as argued by Lockyer (2010: 97) and Buhaug et al. (2009: 550) and also indicated by our troop ratio measure from UCDP and COW data (see section 4.2.4 in chapter 3). Thus, these expectations are based on the fact that intrastate conflicts are in general best described by the right half of figure 2, where $M_g \geq M_r$.

The military capabilities $M_{g,r}(\cdot)$ can be defined as functions of military "inputs". The impact of different military factors on conflict duration is thus determined by their effect on the military capability functions and, in turn, how the change in relative military capabilities affects the expected duration. For the latter step, the marginal effect of military capabilities on the probability of continuing conflict determines how conflict durations change with changes in military capabilities. As can be graphically seen in figure 2, $\partial\pi_c/\partial M_g(\cdot) < 0$ when $M_g \geq M_r$ as argued above, meaning higher military capabilities of the government lead to shorter durations. Analogously, $\partial\pi_c/\partial M_r(\cdot) > 0$ when $M_g \geq M_r$, meaning higher military capabilities of the rebel side lead to longer durations. In general, the CSF duration model thus predicts a shortening effect of factors strengthening the government and a prolonging effect of factors strengthening the rebels.

Specifically, additional armament e.g. in form of arms imports should raise the government's military capabilities, as it is the main reason relevant military equipment is imported during an intrastate conflict. In the same way, a higher troop count, or, as it is implemented in the empirical analysis in chapter 3, a troop ratio more favorable to the government side, positively affects the government's capabilities. Distance between the government stronghold, i.e. the capital, and the conflict might reduce the effective military capabilities deployed at the fighting site (Buhaug et al. 2009).

With this framework, the simple logic of contest success functions can be consistently employed to derive hypotheses concerning conflict durations. In comparison to the war-of-attrition framework used in the main analyses in chapter 3, however, it has no inherent concept of time and therefore cannot incorporate intertemporal decision-making, as every stage of the game is independent from the next. Nevertheless, I find the framework valuable for understanding duration effects, particularly due to its simplicity—with empirical analysis, such as in chapter 3, corroborating its core predictions.

4.3 Taking the multiplayer nature of civil conflicts into account

The contest-success-function duration framework also allows to derive some straightforward insights into the multi-dyadic nature of many civil conflicts. In a simple understanding of such a conflict, the contest success function would just combine the military capabilities of the rebel groups, decreasing the relative power of the government in comparison to fighting only one rebel group with a given military strength:

$$\pi_g = \frac{M_G}{M_G + \sum_{i \neq G} M_i} \tag{21}$$

Then, additional simultaneous dyads would simply equate to the case of an increased M_r, leading to longer conflicts.

A more substantial case arises when each dyad is treated as nested within a conflict that has to be decided on independently. Then, it is possible to set up a model where the government faces a situation with e.g. two conflict dyads against two different rebel groups at the same time. The government has to split up its military resources and will do so in such a way that it equates the marginal effect on the utility of winning a respective dyad over all dyads. Ultimately, this leads to a situation comparable to a lower M_g in the mono-dyadic case, meaning conflicts are longer the more dyads they contain, given a specific M_r of each rebel group.

In a conflict against two rebel groups, the government has expected utility

$$U_g = \pi_{g,1}(M_{g,1}, M_{r,1})v_{g,1} + \pi_{g,2}(M_{g,2}, M_{r,2})v_{g,2} - (M_{g,1} + M_{g,2}) \; , \tag{22}$$

where $\pi_{g,i}$ is the probability of government victory against rebel group i, $M_{g,i}$ is the military capabilities of the government devoted to the conflict dyad against rebel group i, $M_{r,i}$ denote the military capabilities of the rebel group i, and $v_{g,i}$ is the valuation the government attributes to victory against rebel group i.

The military resources M_g are exogenously determined and can be allocated by the government to different conflict dyads:

$$M_g = M_{g,1} + M_{g,2} \tag{23}$$

Now, the government maximizes its utility:

$$\max_{M_{g,1},M_{g,2}} U_g = \pi_{g,1}(M_{g,1}, M_{r,1})v_{g,1} + \pi_{g,2}(M_{g,2}, M_{r,2})v_{g,2} - (M_{g,1} + M_{g,2}) \tag{24}$$

$$\text{s.t. } M_{g,1} + M_{g,2} \leq M_g, \tag{25}$$

leading to the first-order conditions:

$$\frac{\partial U_g}{\partial M_{g,1}} = \frac{\partial \pi_{g,1}(M_{g,1}, M_{r,1})}{\partial M_{g,1}} v_{g,1} = 1 \tag{26}$$

$$\frac{\partial U_g}{\partial M_{g,2}} = \frac{\partial \pi_{g,2}(M_{g,2}, M_{r,2})}{\partial M_{g,2}} v_{g,2} = 1 \tag{27}$$

$$M_{g,1} + M_{g,2} = M_g \tag{28}$$

Combining the first two yields:

$$\frac{\partial \pi_{g,1}(M_{g,1}, M_{r,1})}{\partial M_{g,1}} v_{g,1} = \frac{\partial \pi_{g,2}(M_{g,2}, M_{r,2})}{\partial M_{g,2}} v_{g,2} \tag{29}$$

This shows that marginal utility of increasing $M_{g,i}$ is the same over all conflicts. To evaluate the effects of the overall military capabilities M_g, this can be re-written using the third condition as

$$\frac{\partial \pi_{g,1}(M_{g,1}, M_{r,1})}{\partial M_{g,1}} v_{g,1} = \frac{\partial \pi_{g,2}(M_g - M_{g,1}, M_{r,2})}{\partial (M_g - M_{g,1})} v_{g,2}. \tag{30}$$

To derive a helpful conclusion, we assume that the probability of winning is increasing the military capabilities that the government devotes to this dyad, with decreasing marginal returns, and is decreasing in the rebel groups' military capabilities, i.e.

$$\frac{\partial \pi_{g,i}(M_{g,i}, M_{r,i})}{\partial M_{g,i}} > 0 \qquad \text{(positive marginal effect of } M_{g,i}\text{)} \tag{31}$$

$$\frac{\partial^2 \pi_{g,i}(M_{g,i}, M_{r,i})}{\partial M_{g,i}^2} < 0 \qquad \text{(decreasing marginal effect of } M_{g,i}\text{)} \tag{32}$$

$$\frac{\partial \pi_{g,i}(M_{g,i}, M_{r,i})}{\partial M_{r,i}} < 0 \qquad \text{(negative marginal effect of } M_{r,i}\text{).} \tag{33}$$

Then, it is clear that the higher the valuation $v_{g,i}$, the higher $M_{g,i}$. The more valuable victory is in a specific conflict dyad, the more military effort will be allocated to this dyad to equate the marginal utility of fighting over all dyads, and the higher the probability of winning in this conflict dyad $\pi_{g,i}$. By the same reasoning, stronger rebels with higher $M_{r,i}$ lead to higher $M_{g,i}$, with the government re-allocating resources from the fights against r_{-i} to r_i. Thus, a higher $M_{r,i}$ lowers the government's overall probability of winning $\pi_{g,i}$ for all dyads.

The same holds with additional conflict dyads, which the government needs military capabilities to designate for—thus, conflicts with an increased number of dyads last longer than mono-dyadic conflicts when each rebel group's capabilities are a given. This provides a straightforward formal underpinning for the reasoning and empirical results e.g. in Cunningham et al. (2009) finding that conflict dyads in conflicts consisting of two or more dyads last longer, all other things equal.

The opposite effects occur with governmental armament in this framework: with increased total military capabilities of the government M_g, the winning probability of each dyad $\pi_{g,i}$ rises, as each $M_{g,i}$ allocated to different conflict dyads rises. Thus, increased armament by the side of the government can end conflicts earlier, as established for the mono-dyadic case in the section above. While the contest-success-function approach to duration of *multi-dyadic* conflicts can provide valuable insights for analyzing the causes of prolonged conflicts in general, it does not alter the reasoning for analyzing the specific effects of governments' armament—in the CSF duration framework, the impact of armament in conflict with multiple dyads resembles the effects in the mono-dyadic case. However, the multi-dyadic framework's results strengthen the case for dyad-focused analysis, as the valuation as well as the effectiveness of the government's military equipment depend on the specific dyad. Therefore, chapter 3 bases its research approach on the conflict-dyad level.

4.4 A deeper dive into a power shift duration model

As an alternative framework, I want to elaborate on a bargaining model from Powell (2012). This framework is introduced in more detail in section 3.2 of chapter 2, where it is used to derive hypotheses regarding the effects of both major weapons and small arms importation on conflict *onset*. At the same time, the model extends not only to the beginning, but also the continuation and settlement of conflict, allowing to conceive conflict duration within the framework as well.

In this model, power shifts induced by arms imports are at the center of the analysis. Conflict occurs if these power shifts are too large, meaning the government cannot compensate the opposition sufficiently. Regarding conflict onset, both small arms and major weapons induce power shifts in favor of the government. Those increase the risk of conflict when the opponent sees conflict as a profitable strategy to prevent the government from consolidating its new position of strength. In contrast to major weapons, small arms imports also enhance specific state capacities, contributing to improved general security and economic development. In terms of the model, these state capacities, advanced by small arms importation, raise the expected future utility after the consolidation of the government has taken place. The formal model thus provides a valuable framework to reconcile the counteracting risk-inducing and risk-reducing mechanisms of small arms imports; therefore, it is the basis for the onset analysis conducted in chapter 2 (Mehltretter 2022).

Fighting prevents further deterioration of the balance of power when a power shift occurs. Thus, not only the start of a conflict is important, but "persistent fighting" is needed, as only continued conflict can avoid a further shift of power. In a dynamic environment, starting to fight today and continuing for multiple periods might be less costly than starting later on. Assume the government is able to buy off the rebel group at time t, but will not be able to do so at $t+1$. Then it hinges on the decisiveness of fighting in each of these periods whether fighting at t might be more attractive than accepting the government's offer. In fact, when fighting at t is substantially more decisive than at $t+1$, it might be more attractive to begin fighting today with a higher chance of a shorter and thus less costly conflict compared to starting the conflict later. Formally, the relevant condition whether starting at t is preferred to start fighting at $t+1$ is that $k_t >> k_{t+1}$, with k being the decisiveness factor, which measures how likely a conflict is terminating in a given period.[9]

In short, the more decisive a conflict can be fought, the higher the chance of the conflict ending early, i.e. the shorter the expected duration of the conflict. Formally, Powell (2012) shows that the probability that a conflict lasts $n \geq 1$ rounds is $k(1-k)^{n-1}$,

[9]Note that the measurement of decisiveness differs compared to the CSF model where $k > 1$ increased the weight of the adversary's military capability in the CSF. In Powell (2012), $k \in [0, 1]$ describes the probability that fighting stops in a given period.

meaning expected duration $\mathbb{E}(n)$ is[10]

$$\mathbb{E}(n) = \sum_{j=1}^{\infty} jk(1-k)^{j-1} = k\sum_{j=1}^{\infty} j(1-k)^{j-1} = k\frac{1}{k^2} = \frac{1}{k}. \tag{40}$$

The total cost of fighting n rounds, C_{tot}, is the discounted value of costs C, which occur in each round because fighting is inefficient, decreasing net payoffs to both government and rebels[11] (Powell 2012: 622):

$$C_{tot} = \sum_{j=0}^{n-1} \beta^j C = \frac{(1-\beta^n)}{1-\beta} C, \tag{41}$$

with β being the discount factor. Expected total costs then are

$$\mathbb{E}(C_{tot}) = \sum_{n=1}^{\infty} \frac{k(1-k)^{n-1}(1-\beta^n)}{1-\beta} C = \frac{C}{1-\beta(1-k)}. \tag{42}$$

Both expected duration and expected costs are thus dependent on k, but not on the individual probabilities of winning, conditional on reaching a decisive outcome. Expected duration is clearly decreasing in k:

$$\frac{\partial\,\mathbb{E}(n)}{\partial k} = \frac{\partial[1/k]}{\partial k} = -\frac{1}{k^2} < 0 \tag{43}$$

Thus, the more decisive the conflict is fought, the shorter it is, independent of relative power. In this model, armament impacts conflict duration due to its consequences on decisiveness. With equipment suited to forcing the battle into the open or landing a devastating strike, armament is the critical factor determining decisiveness and

[10] This is derived using the Cauchy product formula (with $l = j - 1$):

$$\sum_{l=0}^{\infty}(l+1)(1-k)^l = \sum_{l=0}^{\infty}(1-k)^l \sum_{i=0}^{l} 1 \tag{34}$$
$$= \sum_{l=0}^{\infty}\sum_{i=0}^{l}(1-k)^l \tag{35}$$
$$= \sum_{l=0}^{\infty}\sum_{i=0}^{l}(1-k)^{i+l-i} \tag{36}$$
$$= \sum_{l=0}^{\infty}\sum_{i=0}^{l}(1-k)^i(1-k)^{l-i} \tag{37}$$
$$= \left\{\sum_{l=0}^{\infty}(1-k)^l\right\}\left\{\sum_{i=0}^{\infty}(1-k)^i\right\} \tag{38}$$
$$= \frac{1}{1-(1-k)} \times \frac{1}{1-(1-k)} = \frac{1}{k^2} \tag{39}$$

[11] $C = 1 - f_G - f_R$ in terms of the full model described in chapter 2.

thus duration of a conflict. At the same time, this reasoning lays weight onto the specific composition of the military forces, which have to be apt for the circumstances of the conflict. For instance, as chapter 3 argues, major weapons are essential for ending conflicts at larger distances from the government stronghold—in light of this framework because they allow the government to increase the decisiveness of the fighting. This logic expands as a valuable foundation e.g. for the verbal arguments and empirical results of Balcells and Kalyvas (2014), finding irregular conflicts, i.e. battles with low decisiveness, lasting longer than conventional and symmetrical non-conventional conflicts.

In a more general version of Powell's framework that allows for consolidation of the state taking longer than one period and also *during* fighting, however, there exists a second mechanism counteracting the impact of increased decisiveness: large power shifts during conflict aggravate the very commitment problems that cause the fighting in the first place (see chapter 2 for more details), leading to continued fighting. After all, conflicts might come to an end not only after a decisive military outcome, but also when the shift in power is slow enough to enable reaching a negotiated settlement. This implies a trade-off of governmental arming regarding conflict duration: additional armament might enhance decisiveness of fighting, shortening the conflict, but sufficiently restrained arming could also allow for a settlement offer to be accepted by the rebels.

Notably, the role and impact of fighting decisiveness in this framework are mirroring the CSF framework's result in the section above. In contrast to this power shift model, in the war-of-attrition framework presented in chapter 3, the expected duration is directly affected by the relative fighting power of the government and rebel side—as is also the case in the CSF model. While decisiveness and relative strength are two distinct and independent concepts in Powell (2012), both are of course related. When a government builds up its military capabilities, it enhances its relative strength but, at the same time, attempts to increase its capacities for decisive fighting. In contrast to the war-of-attrition as well as the CSF framework introduced above, the general version of the power shift model from Powell (ibid.) naturally allows for negotiated settlements to arise, incorporating an important fact of many conflicts ending with bargaining agreements—as does our general framework presented in section 3.2 above.

5 Theory overview and in-depth analysis of selected frameworks: Armament and conflict intensity

When applying our general framework to conflict intensity, the bargaining process–like in the duration case above–occurs amid ongoing military confrontations. There are two major mechanisms through which armament processes can affect intensity, usually measured by the number of battle-related casualties.[12] First, they represent a costly signal of one's capabilities and resolve to escalate the conflict. Second, they determine the military technologies and strategies employed in a conflict, which in turn determine the intensity of the fighting.

Regarding the first mechanism, the level of casualties a side is able to inflict and willing to endure can be interpreted as a costly signal of both its military capacities and also its resolve to fight (Slantchev 2011). This helps reduce information asymmetries about the probability p of being able to win a conflict with military means in the long term. This will not only create the scope for a bargained solution to ending the conflict but will also shift the bargaining result in one's favour (cf. Licklider 1993). Hence, both parties are incentivized to increase their military capacities to escalate the conflict. This is particularly true for rebel groups with no hope of defeating the government militarily. They aim to force the government into a settlement by severely increasing conflict intensity. This could be observed, for instance, during the civil wars in South Sudan (2011–2016), where different insurgent groups were able to procure arms from abroad before escalating hostilities in the hope of forcing the government into a settlement (Mehrl 2017). This reasoning suggests that increased armament levels will lead to more intense conflicts with higher human losses.

While this argument seems straightforward at first glance, it becomes more complicated when defining intensity in terms of casualties not only in the short run but over the whole duration of a conflict. While the preceding argument again suggests more causalities, it also implies that conflicts could be shorter. A short, intensive civil war may have fewer losses of human life overall than a long, protracted contest with less intense fighting. From this perspective, increasing armament levels may lead to more intense but shorter and therefore ultimately less bloody conflicts. Therefore, in the addendum to chapter 3, I sketch an empirical approach to examine this potential trade-off of short-term and long-term "intensity" of conflicts.

Regarding the second mechanism, even armament dynamics before the onset of violence may affect conflict intensity. As Kalyvas and Balcells (2010) and Balcells and Kalyvas (2014) argue, conflict intensity corresponds to specific configurations of military capabilities. These are decided in the armament round of our general model, where both parties decide how much to invest in their military capacities. This will therefore not only affect conflict onset but also the military technologies and tactics that are employed by the combatants, which in turn strongly influence conflict inten-

[12] There is also important literature that focuses on civilian casualties and the motivations of both governments and rebels to explicitly target them (for an overview, see Balcells and Stanton 2021). It is reasonable to assume that armament levels of both government and rebels have a profound effect on civilian fatalities. While most analyses in this area focus on the strategic incentives of combatants to target civilians (e.g. Azam and Hoeffler 2002), some empirical work has also been devoted to studying the effects of military capabilities. For instance, Wood (2010) concludes that stronger rebel groups are less likely to target civilians than weaker ones.

sity. A monadic view of arms procurement as simply increasing one party's ability to fight and thus leading to more violence may thus be misleading. A dyadic conception of conflict implies that military capability is a relational concept. How additional military technology affects conflict should depend on how the parties' military interactions are structured. It is therefore important to look at the way changing armament levels relate to combat strategies and their effectiveness.

In irregular conflicts, rebel groups attempt to compensate for their inferior military equipment with guerrilla and terrorist tactics, thus avoiding open battles where the government military would have a decisive advantage. Conventionally fought conflicts, in contrast, require the rebel side to have sufficient military technology, i.e. heavy weapons, available—otherwise, rebels choose different battle tactics, as they would not be able to withstand the open military confrontation with the government (Balcells and Kalyvas 2014; Butler and Gates 2009; Kalyvas and Balcells 2010; Mehrl and Thurner 2020).

These different tactics employed by the rebel group lead the government to respond with the military strategy that is best suited for this conflict environment (Mehrl and Thurner 2020). When dealing with irregular tactics on the rebel side, the government has to identify and target insurgents hiding in civilian populations (cf. Berman and Matanock 2015), requiring reliable information and the capacity for small-scale, precise operations (Berman et al. 2018; Galula 2006; Lyall 2010). This contrasts with the government tactics corresponding to conventional fighting from the rebel side: here, not tracking down insurgents is the main challenge, but successfully opposing them in open battles (Kalyvas and Balcells 2010).

The impact of additional governmental armament thus depends on how a conflict is fought. Mehrl and Thurner (2020) argue that importing small arms and major weapons enables the government to intensify its attacks on the opponents' positions in a conventional conflict. Then, arms acquisitions lead to higher intensity in a conflict. In an irregular conflict setting, however, this additional weaponry can arguably not be deployed accordingly, meaning that arms imports would have no substantial effect on intensity in such a conflict environment. Thus, whether rising armament levels really change p_i not only depends on raw armament volumes but also the types of arms and the conflict environment.

In sum, arming processes are intricately linked with the intensity of civil conflict violence. They allow the escalation of conflicts but also determine the military tactics employed. Again, simplistic hypotheses that posit *more arms always equals higher intensity* should be treated with caution as they ignore how the military capabilities of governments and rebels interact with one another. The lesson from this theoretical discussion is therefore that empirical research needs to take the armament of both governments and rebels into account to avoid spurious results.

6 Statistical and measurement challenges: Identifying causal relationships

6.1 Dealing with endogeneity

Analyzing the impact of arms transfers on intrastate conflicts, we are confronted with a problem of co-determination: military build-ups may not only affect conflict but could themselves be caused by its outbreak, the anticipation of such an event in the immediate future or its dynamics. This is all the more apparent if we conceptualize conflict onset as a latent, continuous variable. Despite what a binary conflict indicator might suggest, onset is the consequence of a development in which tensions rise until a certain threshold is reached. In our case, this threshold is marked by a certain number of conflict-related deaths. As a result, a positive correlation between higher armament levels—through weapon imports, for instance—and onset could either indicate a causal effect of weapon deliveries on conflict onset or be a consequence of the looming outbreak raising the demand for weapons (see e.g. Thurner et al. 2019 on empirical evidence on the relationship of arms imports and civil conflicts in general). So far, most of the few empirical studies on arms imports and intrastate conflict have not paid sufficient attention to this problem. There are several ways to address this endogeneity issue. I present them and discuss their benefits and shortcomings in the following.

Lagging endogenous variables Older studies often simply include time lags for armament variables such as arms imports (e.g. Craft and Smaldone 2003; Suzuki 2007). This does not really solve the issue, however. The problem that conflict anticipation drives the demand for arms cannot be circumvented by a simple one- or multi-year lag, as these anticipations might have already existed in these previous years as well. Furthermore, it is unclear what the correct lag structure should be, and each conflict case would need specific time lags.

Employing instrumental variables An alternative approach is to find suitable instrumental variables (IV). The estimation is done in two steps: First, changes in armament are predicted with an instrumental variable that is related to the military build-up decision (first-stage relevance) but not to the outbreak of civil war (exclusion restriction). Second, the exogenous variation induced by the instrument is used to analyze the endogenous variation between armaments and civil war (second stage). Alternatively, as implemented in chapter 3, the residuals resulting from the first stage estimation can be included in the second stage, controlling for the unobserved expectations driving armament demand.

Instrumental variables: different types of imports In Pamp et al. (2018), we proposed an instrument for civil-war-related arms imports. We use variation in types of weapons not typically used in civil wars as an instrument for weapons relevant to civil war. With few exceptions (e.g. naval warfare in the Sri Lankan civil war), these weapons are intended for external defense and interstate warfare. These types of weapons are purchased due to regional arms races, security concerns, external threats, corruption, and prestige-seeking (e.g. Ball 1993). At the same time, governments simultaneously seek to purchase other essential conventional weapons that can be used in interstate conflicts as well, but additionally in a civil war, e.g. tanks or aircraft. In Pamp et al. (2018), we find that the correlation between the two types is indeed strong.

Mehrl and Thurner (2020), in their analysis of conflict intensity, also instrument civil-war-related MCW weapons with irrelevant weapons and apply this logic also to small arms. They propose the use of civilian small arms such as hunting rifles and sporting weapons as an instrument for military small arms. Notably, this instrument was unsuitable for use in the analysis of conflict onset in Mehltretter (2022/chapter 2), since including the instrument as a separate control variable showed significant results by itself. Thus, hunting rifles and sporting weapons might affect conflict risk not only through their association with military small arms, but also by a separate mechanism that is not controlled for by standard sets of control variables, violating the exclusion restriction when analyzing conflict onset.

In Mehltretter (2022/chapter 2), I therefore employ a slightly different set of instrumental variables derived from arms trade regularities to analyze the impact of both MCW and SALW on conflict onset. For instrumenting these imports, I use three IVs: first, surface-to-air missiles, which are usually inept for government fighting in civil conflict, are used instead of the broader range of MCW not of use in intrastate conflicts, since they are found to exhibit higher relevance in the first stage. Second, in particular for instrumenting small arms imports, *exports* of small arms correlate highly and may arise because of intra-industry trade (Thies and Peterson 2015), but do otherwise not affect civil conflict. Third, I propose hostile countries' MCW imports as an instrument. When a country hostile to the focal country acquires additional arms, the focal country might resort to additional arms imports to restore the previous power distribution. These international arms race dynamics thus affect arms imports, but should affect civil conflict risk only due to these additional armaments. Spillover effects from nearby countries could pose a potential violation of this exogeneity assumption—therefore, the number of close conflicts is controlled for in all analyses. Overall, Pamp et al. (2018), Mehrl and Thurner (2020) and Mehltretter (2022/chapter 2) show that arms trade patterns provide valuable exogenous variance to identify effects of arms importation on civil conflict.

Instrumental variables: economic mechanisms While the instruments that were discussed above exploit coherences of demand for different kinds of weapons, a second strand in the literature aims to use the economic relationship of supply and demand to identify the causal effects of arms imports. For instance, illustrating such mechanisms of supply and demand, Barrett (2017) analyses the effect of small arms prices. He proposes the geographic distance from government stockpiles that have been looted as an instrument for the price of assault rifles. For instrumenting arms demand, Gallea (2023) and Magesan and Swee (2018) propose to exploit different economic relationships for identification.

Instrumental variables: variance from suppliers' conflict involvement Gallea (2023) proposes a sophisticated instrument based on the production constraints of arms exporters, restricting available supplies to a specific country when the delivering country gets involved in a conflict by itself, increasing the home demand for arms. Thus, a so-called "shift-share" instrument can be devised, where such an external shift in supply affects the importing country according to the share of imports usually obtained from this exporter.

Demand D of country i in year t can economically be modeled as a function of price p, conflict involvement C and other factors $\boldsymbol{X}$, with ϵ representing the idiosyncratic error term:

$$D_{it} = \alpha_1 p_{it} + \alpha_2 C_{it} + \boldsymbol{\alpha_3'} \boldsymbol{X_{it}} + \epsilon_{it}^D \tag{44}$$

Supply S then additionally depends on C^s, conflict involvement of a country's suppliers:

$$S_{it} = \beta_1 p_{it} + \beta_2 C_{it} + \beta_3 C_{it}^s + \boldsymbol{\beta_4'} \boldsymbol{X_{it}} + \epsilon_{it}^S \tag{45}$$

Setting demand equal to supply allows solving for the equilibrium quantity:

$$D_{it}^* = S_{it}^* = \gamma_1 C_{it} + \gamma_2 C_{it}^s + \boldsymbol{\gamma_3} \boldsymbol{X_{it}} + \epsilon_{it} \tag{46}$$

Thus, the exogenous shift in supply due to the suppliers' conflict involvement C^s can be used as an instrument for D_{it}^*. C^s can be constructed as a combined measure of $C_{jt,\neg R_i}$, a dummy for conflict involvement of the specific supplier countries j, weighted by w_{ij}, their average share of imports the country i received from j:

$$C_{it}^s = \sum_{j=1}^{n} w_{ij} C_{jt,\neg R_i} \tag{47}$$

To avoid endogenous spill-over effects in the same region R, defined as the same continent, $C_{jt,\neg R}$ excludes conflict involvements in the importing country's region R_i.

This supply and demand relationship adequately creates a shift-share instrument: the exogenous shift is provided by the conflict involvement of supplier countries, weighted by their supplying importance measured as their average share in the importing country's arms trade portfolio. While Gallea (ibid.) discusses a variety of possible exclusion restriction violations, the shift part of the instrument surprisingly relies only on interstate, internationalized intrastate and extrasystemic conflicts the supplier state is involved in, as defined by UCDP. Intrastate conflicts, making up the majority of all conflicts in UCDP data, are however excluded, presumably because the "shock must be large enough to influence arms supply" (ibid.: 6). Nevertheless, taking this theoretical reasoning seriously, the solution would rather be a minimum threshold of fatalities in these conflicts than excluding a whole category of conflicts.

In comparison to the instruments developed and used in Pamp et al. (2018), Mehrl and Thurner (2020) and Mehltretter (2022/chapter 2), Gallea's (2023) instrument has the advantage that it is not prone to internal portfolio balancing—albeit only a theoretical, not empirically founded objection to our instrument developed in Pamp et al. (2018), this could occur when internal threats spur up investment in armament dedicated to fighting internal conflicts, and these investments in turn drive other investments in e.g. ships and air-defense capabilities to restore the "balance" of capabilities between different branches of the military. However, Gallea's shift-share instrument has its own disadvantages, most prominently the underestimation of complexity in conflict spillovers and third-party intervention decisions. Even though conflict involvement on the same continent is excluded when building the instrument, conflicts at large distances can nevertheless affect conflicts nearby, leading to spill-over effects that violate the exogeneity assumption. For instance, conflict involvement in one distant country might produce international tensions that indirectly affect a rival state's conflict involvement in a country close enough for potential spill-over effects.

Instrumental variables: variance from US inflation An alternative approach, also based on the economic arms trade nexus, is laid out in Magesan and Swee (2018). Also designed as a shift-share instrument, the shift is triggered by US inflation: when US inflation is high, US arms exports rise, as their relative price due to the depreciated US-dollar exchange rate decreases. Arms imports from the US can then be instrumented by combining this shift with the importance of US imports for the importing country, measured by the share of US-imported arms in the country's total arms imports. While this avoids the pitfalls of conflict interactions inherent in Gallea's (2023) instrument, the logic only applies to *US-specific* arms trade. It does not allow for generalization to total armaments, which are of concern in this thesis: the changed demand for US arms might only reflect the composition of a country's imports. However, it might not affect the total amount of arms a country imports, as more expensive arms from other suppliers might be substituted by US arms. If the change in US demand does affect total demand of a country, that is presumably because finance-constrained countries use the convenient opportunity to arm for a looming conflict—then, again, the US-inflation instrument does not control for potential reverse causality. Additionally, this instrument requires carefully controlling for national economic factors not only at the general level (e.g. national GDP), but also with respect to trade and its impact on certain sectors, as US inflation might just be an indicator of the state of the global economy. This, in turn, would affect the economy in the focal country and thus potentially conflict risk.

Instrumental variables: conflict terminations In an unpublished manuscript, Benson and Ramsay (2016) propose using conflict terminations in a country's environment as an instrument for arms imports in conflict intensity analysis. Following their argument, termination of a conflict sets free arms previously employed in this conflict for transferring to another conflict. While discussing and testing for potential confounders like aid or economic trade, Benson and Ramsay (ibid.) are not able to pin down the effect mechanism such that conflict terminations affect only *governmental* arms imports: terminated conflicts can provide rebel groups with additional sources of armament as well, which clearly can have consequences on conflict intensity (and duration). Additionally, they cannot discern effects due to increased armament following nearby conflict terminations from other spill-over effects, presumably violating the exclusion restriction. Thus, the instrument is potentially ill-suited to provide causal identification of governmental arms imports' effects on conflict battle deaths.

Instrumental variables in survival models With conflict duration as the dependent variable, in Mehltretter and Thurner (2023/chapter 3), we have to overcome a different challenge, as there is no standard IV model used in Political Science or Economics for thorough survival analysis, not counting simple logit/probit models like they are used in Magesan and Swee (2018). Therefore, we adapt a two-stage residual-inclusion IV approach based on the Cox Proportional Hazard survival model (Martínez-Camblor et al. 2019) and additionally corroborate our results with an IV implementation using an additive hazard survival model (Aalen 1989), which had not been employed in conflict studies but is widely used in biology and health statistics. Due to its functional form, a standard two-step instrumental variable approach is feasible (Martinussen and Vansteelandt 2013).

Additional approaches The use of instrumental variables is a common strategy to address endogeneity problems in analyses of conflict onset and intensity (cf. e.g. Miguel et al. 2016). However, there are other approaches. In Pamp et al. (2018), we additionally employ a simultaneous equation model (SEM) as developed by Amemiya

(1978), Heckman (1978), and Maddala (1983). These SEMs are able to account for the endogeneity of arms import decisions explicitly, as they model not only an equation for determining conflict risk, but also a demand equation for determining armament. Nonetheless, identification in such demand equations again necessitates valid instruments.

The use of IVs and SEMs certainly is an important step forward. It is clearly preferable to simply ignoring the endogeneity problem or just using lagged variables. However, they are no panacea. One has to bear in mind that these are statistical fixes to a problem that arises because, so far, governments' and rebels' anticipation of future conflict dynamics cannot be sufficiently measured. Moreover, when taking the external conflict interventions into account, the endogeneity problem becomes even more complicated. Now, there are not only expectations about conflict onset and intensity to contend with but also conflict parties' expectations about the nature and extent of external military support (cf. Jackson et al. 2020). Identifying a causal chain from increasing armament levels to dynamics of intrastate violence remains difficult as a result.

Overall, this review of approaches to tackling endogeneity issues shows that there is not one perfect solution for all analyses. For the purposes of this thesis, instruments based on armament's relevancy for civil conflict and exploiting arms trade regularities, e.g. on the relationship of small arms imports and exports, are generally deemed best suited. However, for all types of instruments used in this thesis and presented above, potential issues for the exogeneity assumption might arise with regard to international interdependence—especially concerning international rivalries affecting the arms trade and demand for arms in a state, but potentially also to rebel groups as soon as a conflict escalates. Additional measures, e.g. controlling for such rivalries, might help overcome these shortcomings in future research.

6.2 Measuring armament levels and arms flows

A massive challenge for empirical research is to measure the size and composition of armaments of both rebels and governments as well as their changes over time. In their seminal study, Fearon and Laitin (2003) used GDP as a proxy for the military capacity of governments and mountainous terrain to capture the rebel side's abilities for insurgency. Others employed measures of relative tax extraction (Kugler and Tammen 2012) to gauge the level of a government's control and penetration of society. In our view, it is doubtful that these variables can really isolate the specific effect of military capacity on intrastate conflict. It is likely that it also strongly captures the impact of different economic and social factors.

Alternative measures for military armament that have been used are defense expenditures, armed forces personnel, procurement capabilities, stocks of equipment and arms transfers. However, they all suffer, to a different degree, from inconsistent definitions, insufficient coverage and data availability or questionable validity. For instance, military expenditures are an input measure, whereas military capabilities are an output. Furthermore, the national military budget's size does not convey the military equipment's technological sophistication and level of degradation. Therefore, such expenditure data provide primarily information on the economic burden of building and maintaining military capacities. However, it is a less comprehensive measure of the actual expansion of governmental military capabilities (cf. Pamp and Thurner 2017). What makes matters worse is that the numbers for many developing countries, especially during the Cold War, are either outright missing or very rough estimates. The best cross-country sources available come from SIPRI and the World Military Expenditures and Arms Transfers database (WMEAT) of the U.S. state department.

A different approach to measuring capacity is to look at the size of a government's or rebel group's armed forces as well as the stock and production of military hardware. Measuring existing stocks of military equipment and domestic arms production is very complicated. There are only limited sources that provide data for a number of countries over a longer time period.[13] A very rough first approximation is the defense spending per armed service member, which could be interpreted as an indicator of the capital intensity of the military. This measure, of course, suffers from all the problems associated with the expenditure data. NATO provides data on the military equipment spending of its member states, but this information is not universally available for many other countries. It is also much easier to account for new procurements than to estimate the entire existing stock in a given year.

Even if arms production data was complete, many countries do not have a domestic arms industry; and those with a sizeable arms sector rely on imports for certain weapon types, pre-products and components. Therefore, arms transfers play an integral part in the build-up of military capacities. It is helpful to distinguish between MCW and SALW on both a conceptual and an empirical level. Both differ in terms of their technological sophistication, ease of procurement, prices, the scenarios in which they can be used and the military strategies they enable. Furthermore, small arms are also used by the police to maintain law and order.

The best data sources for MCW transfers are SIPRI and WMEAT, which provide information on over 170 countries, starting as early as 1950 in SIPRI's case. For SALW, the situation is much bleaker. Data availability and reliability are far worse compared to MCW. The only available data source is the Norwegian Initiative on Small Arms

[13]See for instance the "Military Balance+" data provided by the International Institute for Strategic Studies (cf. Gannon 2023). Souva (2023) created a "material military power" index using Military Balance+ data as well as e.g. data on air power armament from Saunders and Souva (2019). In addition to counting equipment, quality should in the optimal case be accounted for (cf. Olsson 2022). Unfortunately, both Gannon's and Souva's datasets were available too late for exhaustive use in this thesis.

Transfers (NISAT). However, it stopped its data collection efforts in 2017, meaning 2015 is the latest year with available data. The earliest figures on SALW flows provided by NISAT are from 1962, but reliable and complete data is only available starting in 1992 for small arms and 2006 for light weapons. NISAT itself partly builds upon data from UN Comtrade, which is, in theory, continuing to provide raw data on arms transfers. However, without further corrections that NISAT conducted for their data, non-reporting and diverging importer and exporter reports limit UN Comtrade data's reliability and utility (Holtom and Pavesi 2017).

In sum, data on MCW transfers represent, in our view, currently the best approach to measuring armament levels for countries that do not possess any domestic arms production capabilities. This is the case for many states that have been involved in intrastate conflicts in the past. Furthermore, research on the causes and drivers of the international arms trade network has made great strides in recent years, providing better and better insights into its scope and dynamics (cf. Akerman and Seim 2014; Thurner et al. 2019; Lebacher et al. 2021a,b; Pamp et al. 2021). Both chapters 2 and 3 thus use SIPRI's MCW transfer data, and the former additionally relies on NISAT's data to distinguish the specific impact of small arms.

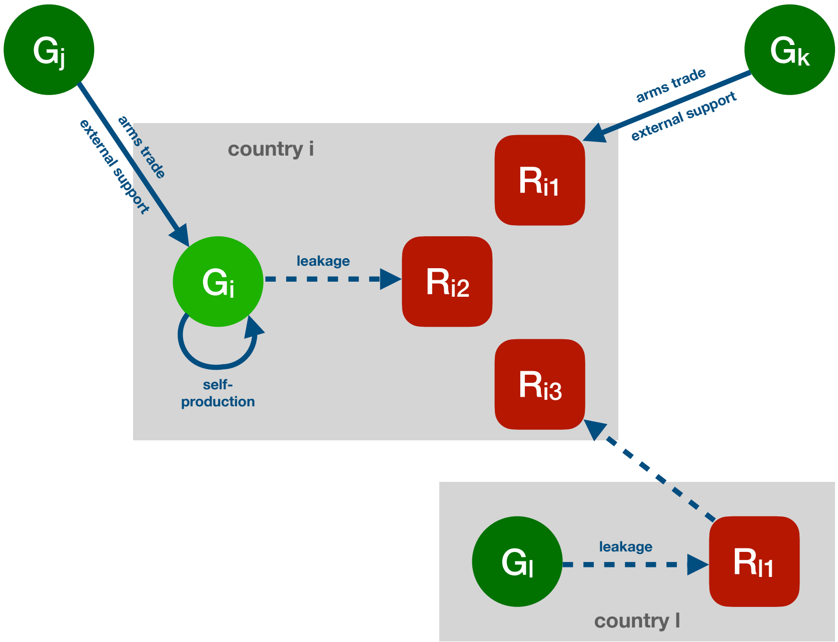
Data on rebel groups are far harder to come by. As they usually cannot produce arms themselves, rebels have to rely on leakages from government arsenals and, more importantly, arms transfers from abroad. Unfortunately, the SIPRI arms transfer databases only have extremely limited information on weapon flows to rebel groups. The Non-State Actors data set (Cunningham et al. 2013) provides some rough, ordinal information on rebel groups' military strength relative to the opposing government and their procurement capacities. However, there is little detail and no information on the specific arms endowments or military capabilities. While there is valuable data on external military support by UCDP (Högbladh et al. 2011; Meier et al. 2023) and an extension of it by San-Akca (2016), these data sets do not indicate what weapon types are transferred nor provide any quantitative measures about the size of the support. To overcome these blind spots, chapter 4 presents new data on rebels' armament.

7 Perspectives for future research and policy implications

7.1 Taking procurement channels and external actors into account

While a standard bargaining framework, as outlined above, provides a useful starting point, it is, in our view, still not satisfactory for two reasons. First, it assumes that the conflict parties can choose their armament levels to a certain extent. While a reasonable assumption for interstate conflicts, it is more problematic for civil conflicts. Rebel groups, if they are not break-away factions from the military, are often severely limited in their ability to procure arms. They often rely on leakage from state stockpiles

Figure 3: Internal and external provision of arms to governments and rebel groups



or battlefield capture (Jackson 2010). Their best hope, of course, is to get external support. Similarly, many governments are not able to produce their arms domestically. They also rely on arms transfers if existing weapon stocks are insufficient. Figure 3 illustrates how complex the resulting web of arms procurement is.

Changes to militarization levels are endogenous to external interventions and vice versa. This relationship introduces several essential factors that will likely affect how armaments relate to civil violence. Is only one side supported, or are all conflict parties supported, and to what extent? What are the motives of the external actors? How do they influence the armament decisions of governments and rebels? And how are conflict dynamics affected (cf. Jones 2017; Kydd 2022; Lawson 2022)? This is where research on armament dynamics needs to tap into the extensive literature on conflict delegation that encompasses both external support and proxy wars (for overviews, see Berman et al. 2019; Karlén et al. 2021; Rauta 2021a). The level of external arms provision depends on external actors' motives and their relationships. Humanitarian interventions like those in Kosovo 1999 and their moral hazard implications (cf. e.g. Cetinyan 2002; Kuperman 2008; Rauchhaus 2009) differ from proxy war scenarios, such as in the Syrian civil war or the Ukrainian conflict before the Russian invasion, where the conflict sides fight on behalf and in the interests of external powers that exert a larger degree of control (cf. e.g. Salehyan 2010; Salehyan et al. 2011; Heinkelmann-Wild and Mehrl 2022; Stein and Cantin 2021). This may even lead to a "proxy arms race" if rivaling outside powers decide to support different conflict parties simultaneously. Such so-called "competitive interventions" (Anderson 2019) could markedly increase the escalatory effects of armaments. As a result, future research should take international roots and ramifications more strongly into account.

In terms of theoretical development, there are two strategies moving forward. One is to embed the strategic armament choices of conflict parties into a principal-agent framework. This could be especially fruitful if a conflict can be characterized as a proxy war with powerful external control. Alternatively, one could stay in the well-familiar bargaining framework and use comparative statics to analyze how the provision of arms by third parties changes the model parameters, a route taken by Meirowitz et al. (2022). External interventions would then affect the nexus between armament levels and conflict in several, sometimes countervailing, ways. On the one hand, the provision of arms directly increases armament levels M_i and thus affects the probability of winning p_i. It could also reduce the costs of arming, thus allowing the conflict sides to achieve higher armament levels with any given resources. Whether this is conflict-enhancing depends, among other things, on whether foreign weapon inflows improve information asymmetries. Arms inflows by an external power to a rebel group could, for instance, help a government better screen a rebel group's true capabilities and therefore make peace more likely because the government will be compelled to make less aggressive demands in the bargaining round.[14] External arms provision to initially weak rebels that the government cannot observe will in turn make violence more likely or more severe because the government will be more aggressive in its demands, rendering a peaceful resolution harder to achieve. Arms flows thus affect both relative capabilities and bargaining behavior. Higher armament levels therefore do not necessarily make escalation more likely but can also lead to less contentious bargaining. The analysis becomes more complicated if we take the effects of anticipation and moral hazard into account. Even weak rebels with complete information may reject a "rational" offer inside the bargaining range hoping to attract external arms support. This may be compounded if rebels can improve their arms acquisition only after fighting has started.

In sum, using such a comparative static analysis is a valuable first step. However, it still does not fully capture the incentives of external actors in their own right and does not account for the endogeneity of the choice to intervene. Both, relative armament levels of the conflict parties and the international security environment, meaning conflicts elsewhere and the strategic relationships between foreign powers, strongly affect the

[14]Meirowitz et al. (2022) find in their model formulation that arming already strong challengers increases the probability of peace while arming weak rebels is less likely to lead to peace.

decision of one or more external powers to provide weapons. Formal modeling could be done by developing a three-actor (or four-actor) game where the external player has to decide if and how to provide support (for an example, see Powell 2017).

7.2 Additional research perspectives

Whereas the methodological approaches have been greatly improved, empirical research would benefit from more rigorous theoretical work to allow more precise derivations of often highly conditional hypotheses. Moreover, the severe lack of data on armament levels and their composition, particularly for rebel groups before and during civil conflict, seriously impedes empirical research. This is highly problematic because the endowment with military technology is vital for understanding different stages of crisis bargaining and the dynamics of military fighting, insurgency and counterinsurgency.

Future theoretical work should also aim to model all stages of conflict and escalation, starting with dissent, protests, repression, violent backlashes up to military challenges to governments (cf. e.g. Zhukov 2014; Rød and Weidmann 2023). We still do not know how different types of small arms and major conventional weapons, as well as new weapon technologies, contribute to each conflict stage in a country's march towards civil violence (cf. Horowitz 2020). For example, small arms may be used for repression and large-scale human rights violations, thus leading to more escalation. At the same time, they contribute to a state's administrative, policing and extractive capacities. Thus, they may prevent violent escalation in the mid- to longrun (Mehltretter 2022/chapter 2). To comprehend these mechanisms fully, a better understanding of the relationship between armament processes and different types of state capacities is required. Research on the long-term effect of institutions on civil conflicts (e.g. Acemoglu and Robinson 2020) should take this into account.

At the methodological level, we believe that endogeneity is still a big problem, and statistical fixes in the shape of IVs and SEMs are not optimal solutions. A superior approach would be to measure the expectations of the conflict parties directly. Such an indicator would enable us to estimate the extent to which these expectations drive the demand for arms. Obviously, deriving such a measure is easier said than done, as there is no way to observe this latent information in practice. One indirect approach would be to use conflict probability as a proxy, for instance, based on existing forecasting models (for an overview, see Hegre et al. 2021). Note that this does not necessarily require advanced statistical approaches. Conflict probabilities can be easily estimated based on standard empirical models of civil war onset.[15]

[15] For an example of the estimation of conflict probability and its effect on military expenditures see Nordhaus et al. (2012).

Furthermore, more disaggregation is needed. A network-oriented perspective of the disaggregated conflict actors could be a natural way to measure combined military capabilities. The use of weapons is a localized event. Conflict parties in different regions may possess different types of arms. While we may observe the use of MCW in some parts of a country, other areas may only see the use of SALW. Looking just at overall changes in armament levels, due to weapon imports, for instance, may therefore not be enough to precisely capture their effects on military fighting. Using a more local focus like the PRIO-grid level, as in Fritz et al. (2021), is an important step forward.

Finally, as argued in this chapter, we still lack comprehensive data to get a clear picture of the military hardware available of both governments and rebels. Thanks to the excellent work of SIPRI, the best we have so far is information on the international arms transfers of MCW from and to governments. The situation is much bleaker concerning SALW, where information is much more incomplete and far less reliable. The situation is even worse for rebel groups, where there is much less information to work with, even if taking the first efforts presented in chapter 4 into account. Better data is therefore clearly needed for advancing empirical research.

In sum, there can be no intrastate violence and war without weapons. The processes of domestic escalation are conditioned by the interaction between contesting social groups and governments and the strongly moderating external environment of intrusive, manipulative or mediating third parties that possibly provide arms to conflict parties. The availability and usage of weapons is not just an epiphenomenon of structures, but weapons are consciously chosen instruments of conflict resolution. They determine communication, expectation formation and the military fighting strategies. Thus, future research needs to become more theory-driven, more careful in its causal identification, and more disaggregated.

7.3 Policy lessons

As our survey of the literature has shown, some significant progress has been made in the last three decades in researching the actual role that weapons play in intrastate conflicts. While significant gaps remain both theoretically and empirically, some important lessons have been learned. Concerning the onset of violence, the available evidence does not suggest that large inflows of major conventional weapons are associated with successful deterrence. While the results are not uniform, most of the newer studies suggest that transfers of MCW to governments make outbreaks more likely if conflict conditions are present in a country. Therefore, propping up governments with MCW does not seem to be a reliable way to prevent the outbreak of violence. It could even be counterproductive as it may make governments more aggressive in their bargaining behavior, thus lowering the chances of a settlement. Unfortunately, due to data limitations, no empirical study has yet investigated whether arming rebels with MCW makes a peaceful settlement more likely. Regarding the role of small arms and light weapons on the government side, the available evidence is less clear-cut, yet it suggests that they do not make conflict onset more likely but may actually have a risk-reducing effect (Mehltretter 2022/chapter 2). This could be because they improve state capacities necessary for policing and conflict prevention.

With regard to ongoing conflicts, the results are very mixed. However, the empirical results suggest that arming a conflict party may actually reduce conflict duration (cf. chapter 3, Caverley and Sechser 2017). Strengthening the rebels' military capabilities leads to more symmetric but shorter warfare. Weaker rebels tend to resort to asymmetric strategies that are associated with protracted fighting. On the other hand, there is also evidence that external rebel support increases duration and makes a negotiated outcome more difficult. The military strength of rebel groups is also crucial when analyzing how the inflow of arms affects the intensity of military fighting. The available evidence supports the view that sending arms to a government that faces a militarily strong challenger leads to more intense warfare with more casualties. Enacting effective arms embargoes that cut combatants off arms deliveries has been shown to lead to lower conflict intensity. Nonetheless, the addendum to chapter 3 indicates that total casualties might actually be lower under some circumstances.

In sum, controlling and restricting the flow of arms to conflict regions is an important policy goal that has been shown to affect conflict intensity. Monitoring arms flows could therefore help predict conflicts and their dynamics. It should also be emphasized again, however, that inflows of weapons are usually not the primary reason for civil conflict but just a symptom of underlying issues. Nevertheless, they crucially affect the dynamic of a dispute emerging into a full-blown intrastate conflict. Without addressing the underlying issues, however, violence will always be likely.

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Chapter 2

Arming for conflict, arming for peace? How small arms imports affect intrastate conflict risk

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Abstract

Although a prevalent technology of conflict, the impact of small arms imports on the risk of intrastate conflict outbreak has not been examined so far. This article argues that small arms not only enhance general military capabilities, but also contribute to state capacities necessary for conflict prevention. These two mechanisms are incorporated in a formal model of power shifts. The derived hypotheses are tested on 146 countries for the period 1993–2014. Using split-population and penalized fixed effects logit models as innovative estimation methods for rare-events data, small arms imports are found to have no or even a risk-reducing impact.

1 Introduction

Most deaths in civil conflicts result from small arms (Wille and Krause 2005, cf. Kreutz and Marsh 2011). Exports of small arms are under public scrutiny because they are deemed to lead to more severe conflicts (Bourne 2007; Mehrl and Thurner 2020), destabilize whole countries and regions (e.g. Greene and Penetrante 2012) and worsen repression and human rights violations (Blanton 1999; de Soysa et al. 2010). As Pamp et al. (2018) have shown, *major conventional weapons* transfers indeed raise the risk of intrastate conflicts. However, conjectures on escalating effects of governmental *small arms* acquisitions cannot be backed up by proper theoretical and empirical evidence in the literature—their specific impact on the risk of intrastate conflicts has not been investigated so far.

Assessments of small arms' effects on conflict outbreaks usually focus on illicit trade and privately owned weapons (e.g. Banerjee and Muggah 2002; Ayuba and Okafor 2015; Stohl and Myerscough 2007; Chivers 2011). They mostly conclude that the availability of small arms facilitates violent uprisings of non-state actors and is thus conducive to insurgency and conflict risk. Therefore, the small arms trade is generally associated with detrimental regional and internal security developments (Wezeman 2003). However, in countries with appropriately structured security forces like Ghana and post-conflict Liberia, governmental small arms imports might contribute to domestic security and peacekeeping (cf. Blair 2020). Furthermore, international security aid programs, e.g. for countries like Mali, Kenya, Somalia or Afghanistan, specifically address military and police state capacities by providing training and equipment to security forces, as they deem these state capacities necessary to build up peace, stability and development (cf. Brown and Grävingholt 2016; Omelicheva et al. 2017).

Do the provision and importation of small arms to governments help to advance these goals, or do they rather have adverse effects? Should responsible supplier countries limit small arms exports as strictly as possible? Or can they play a sensible role in more comprehensive strategies integrating good governance, lawful security forces and economic development? These are essential questions for all strategies on conflict prevention, security and development. Therefore, evaluating the effects of arms imports is of paramount relevance. These questions have, however, only been investigated in the literature for the case of major conventional weapon (MCW) imports (Craft 1999; Suzuki 2007; Pamp et al. 2018) or without discerning between major weapons and small arms (Craft and Smaldone 2002; Magesan and Swee 2018).

Investigating small arms transfers separately from major conventional weapons seems warranted, from both an empirical and a theoretical standpoint. The observed trade patterns diverge substantially, as shown by a low correlation between the two types of imports.[1] At the same time, we theoretically expect differences in their demand and impact on conflict between these arms types. Small arms can play a distinct role in enhancing state capacities in peaceful times and during the emergence of conflict: not only the military but also police forces have to be equipped with small arms, which are a necessary tool for providing security and public order (cf. Karp 2018; Bourne 2007). While major weapons imports cause a considerable burden on the state budget, small arms have much lower unit costs. They might even lead to increased fiscal capacity when tax offices have sufficient authority to collect taxes, ensured by a sufficiently equipped police.

To capture these different mechanisms in a coherent framework, this article for the first time—to my knowledge—implements armament in a formal model of intrastate conflict, adapted from Powell (2012). In this model, power shifts in favor of the government can cause conflict when they are too large to allow compensation of the opposing rebel group. While arms imports generally lead to such shifts in power, small arms at the same time enhance conflict-preventing state capacities. Since they are also tools for securing the state's monopoly of violence, they might impede the emergence of conflict. When I integrate this mechanism into the formal model, small arms can have a neutral or even mitigating effect on conflict risk.

To evaluate the theoretical predictions, I employ innovative empirical methods on time-series cross-sectional data on 146 countries over the time span 1993–2014. Onsets of intrastate conflicts are rare events, posing particular challenges to empirical research. Usually, one would prefer to use models that can account for unobserved heterogeneity between countries. However, standard fixed effects models would lead to biased estimates, as countries without any onset, i.e. no variance in the dependent variable, have to be excluded. In fact, *many* countries do not exhibit any conflict over the observed period and have not done so for a long time or even since the beginning of their existence.

Cook et al. (2020) propose a penalized maximum likelihood-fixed effects estimator for such cases. This estimator uses the pool of all countries without onset as one joint category of fixed effects. Their infinity-tending parameter gets shrunk to zero by penalization. Thus, it enables one to correctly control for unobserved differences between all countries. As an alternative approach, Beger et al. (2014, 2017) develop a split-population duration estimator. It accounts for a distinction between consolidated countries that are "immune" to conflict and countries "at risk", as they term it. The split-population model incorporates two equations: the immunity equation determines the probability at which a country-year even is at risk of a conflict; and the duration equation estimates how the covariates affect the conflict risk for the sub-

[1]For the data and sample described and used in the remainder of the article, the correlation coefficient is .24.

population of country-years at risk.

As Pamp et al. (2018) point out, analyses of arms imports are also subject to potential reverse causality problems because the imminent risk of a conflict might prompt the state to increase its imports. Therefore, an instrumental variable (IV) approach guarantees that results are not substantially affected by endogeneity issues.

My empirical results corroborate previous findings regarding major weapons, which increase the risk of intrastate conflicts. However, small arms imports seem to have a distinct, different effect: while standard probit models do not show a statistically significant impact, instrumental variable models as well as fixed effects and split-population duration estimations exhibit a significant impact of small arms *reducing* conflict risk. In contrast to the destabilizing effect of major weapons, small arms are shown to *not* increase conflict risk and may actually have a rather stabilizing influence.

The remainder of the article is structured as follows: I begin by giving an overview of the literature on armament and intrastate conflict onset, presenting empirical results and presumed theoretical mechanisms. Next, the specific theoretical effects of small arms imports on state capacities are discussed informally. These are then formally implemented in a power shifts framework. The respective hypotheses are tested in the empirics section, which details methods, data and operationalization and delivers the results. Finally, I discuss the findings and avenues for further research.

2 The state of the literature: Empirical evidence and potential mechanisms for arms imports affecting conflict risk

The following review of the literature first summarizes the empirical evidence regarding the effects of arms imports on the risk of intrastate conflict onset. An overview of theoretical mechanisms on the emergence of conflict then provides context for the theoretical framework laid out in the subsequent section.

2.1 The empirical evidence so far

The few studies that analyze the effects of arms imports on civil conflict onset do not provide conclusive evidence on specific arms types like small arms, as they differ in scope, methods, data used and results. While Craft (1999) and Craft and Smaldone (2002) use both inter- and intrastate conflict onsets combined as a single dependent variable, Durch (2000), Suzuki (2007), Magesan and Swee (2018) and Pamp et al. (2018) focus explicitly on intrastate conflicts. Craft (1999), Craft and Smaldone (2002), Magesan and Swee (2018) and Pamp et al. (2018) find statistically significant positive effects of arms imports on conflict risk. However, the effect is not significant in Suzuki (2007) and negative but also not significant in Durch (2000). In contrast to the repression literature (Blanton 1999; de Soysa et al. 2010; Brender and Pfaff 2018), small arms have not played a role in research on civil conflict outbreaks so far: only Craft and Smaldone (2002) and Magesan and Swee (2018) even include small arms in their analyses, but lumped together with major weapons, using data from the World Military Expenditures and Arms Transfers (WMEAT) database and on US Direct Commercial Sales (DCS), respectively.

Actually, only Pamp et al. (2018) and Magesan and Swee (2018) use a wide-ranging sample of both developing and developed countries and apply advanced econometric techniques to not only establish correlations but provide causal findings. Pamp et al. (2018) cover 137 countries over the time 1953–2013. Employing simultaneous equations and instrumental variable models to account for reverse causality, they present strong evidence of a positive causal effect of major weapons imports on the probability of civil conflict outbreaks. However, they do not consider the role of small arms transfers. Magesan and Swee (2018) focus on US DCS to 191 countries over the period 1970–2008. Exploiting price effects owing to US inflation, their instrumental variable models indicate that higher US sales lead to a higher risk of conflict in the buyer country. Again, their data do not distinguish between small arms and major weapons. In sum, there exists no empirical assessment of the distinct impact of small arms so far.

2.2 Theoretical mechanisms of conflict onset

Like in empirical research, governmental imports of small arms have not been the subject of systematic theoretical considerations on intrastate conflict onset so far. To scrutinize the potential effects of small arms in the next section, it is nevertheless helpful to summarize the usually proposed theoretical mechanisms associated with military endowments and arms imports in general.

Arms imports are a significant cornerstone of state capacity, which in turn is integral to analyzing conflict risk (cf. e.g. Fearon and Laitin 2003; Fjelde and de Soysa 2009). Hendrix (2010) discerns different types of state capacity providing valuable guidance for examining the effects of arms imports. Obviously, arms imports are expected to enhance the *military* capacity of the state. In a simple contest success function framework (cf. Tullock 1980; Hirshleifer 2000), higher military capacity can deter potential adversaries, leading to lower conflict risk (for more complex analyses of interstate conflicts see e.g. Huth 1988; Huth and Russett 1988; Jervis 1989; Jackson and Morelli 2009). Chassang and Miquel (2010) develop a formal model for interstate conflicts to show that increases in weapons stocks might not always be deterring but can also lead to preemptive attacks, like Craft and Smaldone (2002) argue as well. Deterrence might also prove to be too costly for avoiding conflict (Coe 2011).

At the same time, arms imports also affect a state's *fiscal* capacity: in particular, major weapons require substantial expenses, reducing the fiscal capacity for different spending purposes like fiscal redistribution (cf. Azam 2001), as described by the gun-versus-butter trade-off in Powell (1993). The state's capabilities to prevent conflicts non-violently are thus reduced. In contrast, procurements of small arms might even result in a higher *extractive* capacity, meaning the ability to raise taxes and accrue rents, e.g. from natural resources.

Apart from direct capacity effects, arms imports affect the conflict risk owing to signaling and exacerbated commitment problems. In settings of asymmetric information, as is usually assumed for civil conflicts (Fearon 1995; Walter 2009), arms imports can function as a signal of resolve or of incompletely observable military state capacity. Again, this might lead to effective deterrence (cf. Huth and Russett 1988; Jervis 1989), but also to preemptive attacks by challengers to avoid further deterioration of the military balance in favor of the government. Powell (2006) argues that such power shifts can induce conflict if it is not possible to commit to future compensation of the challenger for the losses expected owing to the power shift. The following section adapts a formal model originally proposed by Powell (2012, 2013), incorporating these mechanisms.

3 Building a theory of small arms imports and conflict risk

So far, acquisitions of small arms have not been a subject of theories on the onset of intrastate conflict. In addition to the general theoretical mechanisms concerning armament and conflict outbreak laid out in the section above, I argue that small arms need to be analyzed separately because they potentially exhibit different theoretical mechanisms leading to different overall effects on conflict risk.

Under a perspective of specific state capacities, imports of major conventional weapons could be hypothesized to deter potential challengers owing to increased military state capacity. However, causal analyses actually find that major weapons imports increase conflict risk (Pamp et al. 2018). Powell (2012) provides a formal model that can help in understanding the underlying mechanisms: power shifts owing to arms importation diminish an opposition group's expectations of their future income share. Thus, the opposition has to be compensated for such power shifts. When the government cannot credibly commit to paying off the opponent in the future, this compensation cannot exceed the current income. With a power shift that is too large, the opposition cannot be compensated sufficiently, and it has to fight the government. The government's build-up of military capacity thus triggers conflict.

In general, small arms imports can be interpreted as power shifts too. However, as is argued in the next section, small arms also increase expected future wealth owing to enhanced state capacities. With this prospect of larger welfare, the impact of the power shift on the opposition group is counteracted, potentially resulting in a higher conflict threshold, i.e. reduced risk, in Powell's model.

This section sets out how small arms affect state capacities and thus intrastate conflict risk and how their effect differs from major weapons, resulting in potentially differing overall effects. Then, an appropriate adaptation of Powell's (2012) bargaining model of conflict is presented to implement these mechanisms formally.

3.1 Small arms, state capacities and conflict

One of the most important factors to prevent conflicts is a state with sufficient capacities to solve dissent non-violently, providing security, stability and economic welfare (cf. Fearon 2003). In the following, the specifics of small arms imports compared with major weapons are described, and their differential effect on conflict-preventing, security and fiscal state capacities—and thus conflict risk—is laid out.

Small arms as distinct technology of conflict This article argues that small arms should be considered a distinct "technology of conflict", separate from major weapons (cf. Jia and Skaperdas 2012; Hirshleifer 2000; Kalyvas and Balcells 2010). Small arms require only minimal training and are readily available to be imported at short notice (Boutwell and Klare 1999), while major arms acquisitions often take years to be realized.[2] Accordingly, small arms trade flows substantially diverge from trade of MCW: Lebacher et al. (2021) find that country dyads exhibit a negative correlation between small arms and MCW transfer volume. In the data used in the following sections, the correlation between small arms and MCW imports in a country-year is only $.24$. This indicates different underlying reasons for governments to import and consequently—at least intended—different effects.

Enhanced conflict-preventing and security state capacities Importing major weapons delivers fighting capacity in a particularly narrow sense. They are only suited for deployment in large-scale combats in intra- or interstate conflicts (or the deterrence thereof) as well as in situations preceding conflict, e.g. when repressing mass demonstrations with armored vehicles and tanks. This might in turn ignite conflict (Hultquist 2017; Sullivan et al. 2018) but could also support demobilizing of the opposition, as shown recently by Chiang (2021), and thus reduce conflict risk. Small arms are suitable for use in the same contexts (de Soysa et al. 2010)[3], but provide additional util-

[2] For the period after the Cold War, as is relevant for this article, major conventional weapons were delivered on average 4.4 years after ordering, according to SIPRI data.

[3] In contrast to de Soysa et al. (2010), I do not find a significant effect of small arms imports on human rights violations with the data used in this article, excluding years with ongoing conflict, as is theoretically

ity to governments in peaceful times. For governments, small arms are necessary instruments to provide domestic security and uphold public authority (cf. Bourne 2007; Gobinet et al. 2011; Karp 2018).

In contrast to major weapons, small arms are not only tools of the military, but also the primary endowment of police forces (Boutwell and Klare 1999; Karp 2018). Importing small arms can thus lift "material constraints" in the equipment of police and security forces that otherwise lead to insecurity (Mehler 2012: 49; Oreta 2009; cf. Adekoya and Abdul Razak 2017). Since these imports fulfill essential functions for governments, "richer, better-governed countries" can be expected to have higher volumes of such imports, as is in fact observed by de Soysa et al. (2009: 86). Describing the pathways to state collapse and conflict, Rotberg (2002: 129) argues that states in this situation are not able to provide necessary services to the population and "security, the most important political good, vanishes." Insecurity provokes reactions that further undermine a state's authority and monopoly of violence because groups and organizations begin to provide armed "security" services autonomously (Atwood 2005). Such non-state actors usually rely on poorly protected or managed government stockpiles for arming (Jackson 2010). However, with well-equipped forces, leakage of arms to non-state actors can be prevented, inhibiting the emergence of conflict. Likewise, contagious effects of nearby conflicts can be contained (Braithwaite 2010). Small arms imports crucially contribute to these security and police state capacities, which are central to preventing intrastate conflict (cf. Fearon and Laitin 2003; Collier and Hoeffler 1998). Thus, in contrast to major weapons, they might exhibit a risk-reducing impact—possibly counteracting the risk-inducing power shift effect caused by small arms imports as well.

For this conjunction of state capacity and small arms imports, Ghana provides an illustrative case. In our sample period, it is among the largest importers of small arms in Sub-Saharan Africa. As a contributor to peacekeeping missions, Ghana enhanced its military equipment, while also improving security within its borders. This strengthened the general development path resulting in a period of absence of state-based conflict (Banini et al. 2020; Birikorang 2007).

Similar developments took place in post-conflict Liberia, where recorded small arms imports started only after the conflict was settled in 2003 and the strict arms embargo was lifted. Blair (2020) details how Liberia's police was restructured from an "instrument of repression and terror" to a stabilizing factor in the post-conflict period (cf. Joyce 2020). In the process, the Emergency Response Unit and the Police Support Unit were formed as special units which received better equipment, including small arms, and training (Friedman 2011). They were specifically established to deal with arrests of armed criminals, violent crimes in progress, hostage situations and armed terrorist

warranted here.

activities (Washington 2008). In Liberia, small arms thus provided the necessary tools for the success of the restructured police and contributed to improved security and military state capacities for lowering conflict risks.

In like manner, US security aid programs specifically aim at enhancing counterterrorism efforts and general security by training and equipping the police and military forces (Berman 2003; Omelicheva et al. 2017). Supported countries like Bahrain, Belize and Saudi Arabia imported significant amounts of small arms over the sample period of this article, without outbreaks of intrastate conflicts. However, avoidance of larger-scale conflict in these countries came at the cost of other types of violence: in Bahrain, for instance, protests in 2011 were violently dispersed by security forces, curbing an uprise that could have emerged in a conflict. Nevertheless, in the event of potentially escalating smaller-scale unrest, military and security state capacities, enhanced by small arms imports, can effectively prevent the escalation to a conflict outbreak (Chiang 2021). In contrast, Syria, which experienced the deadliest conflict in the sample period, had only minor imports of small arms recorded.

Fiscal and economic state capacities In a standard guns-vs.-butter framework (Powell 1993), higher investments in the military diminish the state's fiscal capacity. It might then be less able to provide public goods and redistribution for alleviating potentially conflict-inducing grievances (cf. Azam 2001). However, small arms imports differ in their financial consequences decisively from major weapons.

While the global small arms trade in 2014, the latest year with data available from the Norwegian Initiative on Small Arms Transfers (NISAT), had a volume of 5.9 billion 2012 US-dollars (including 1.1 billion 2012 US-dollars for military-grade small arms), WMEAT records a total of 188.5 billion 2012 US-dollars in all arms trade. This disparity illustrates the different dimensions between small arms and major weapons trades and financial expenses for governments. Whereas investments in major weapons thus impact national budgets substantially, small arms acquisitions pose a relatively small financial burden. Fiscal capacities that enable governments to curb economic discontent are thus not significantly affected by expenditures for small arms imports. A state's security forces, adequately equipped with small arms, may even enhance its taxing capacity by supporting tax authorities' enforcement measures, potentially increasing government revenue (cf. Risse 2012). This can in turn foster public goods provision and reduce conflict-inducing grievances (Taydas and Peksen 2012; Justino and Martorano 2018; Azam 2001; Fjelde and de Soysa 2009).

General security is also deemed an "important determinant of welfare" (Fafchamps and Minten 2009: 831), as insecurity impedes the state's fiscal capacity, investment, competitiveness of firms, income and economic growth and thus general development (Atwood 2005; World Bank 2006; Cárdenas-Santamaría 2007; Oriakhi and Osemwengie 2012; Goulas and Zervoyianni 2013, 2015; Estrada and Ndoma 2014; Torres-Preciado et al. 2017; Adekoya and Abdul Razak 2017; Besley and Mueller 2021). With small arms enabling security forces to reduce insecurity, they might contribute to raising economic welfare in the future. Again, Ghana and Liberia provide illustrative examples with stable development and gross domestic product (GDP) growth over the observed period or, in the case of Liberia, since the end of the conflict in 2003, as a result of general stabilization. Fiscal and economic state capacities are in turn a critical determinant of conflict risk since they raise opportunity costs of conflict and provide financial capabilities to reduce grievances (Collier and Hoeffler 1998, 2004; Jakobsen et al. 2013; Ray and Esteban 2017; Taydas et al. 2010; Justino and Martorano 2018).

In sum, the theoretically deduced effects of small arms on fiscal and economic state capacities are not clear-cut risk-inducing as they are for major weapons. Instead, they could have a risk-reducing impact under certain circumstances. Since the financial burden of paying for small arms importation is substantially lower than for major weapons, it has a less detrimental effect on public goods provision, allowing the reduction of conflict-provoking grievances. At the same time, the emergence of conflict can be counteracted by raised security state capacities, also supporting future economic development. In the formal model in the next section, the enhancement of state capacities by small arms imports leads to an increase in expected future utility, thereby reducing conflict risk. In contrast to the effects of major weapons, these state capacity effects partly offset or even overcompensate for the negative, conflict-inducing impact of power shifts in favor of the government.

3.2 A formal model of conflict, power shifts and state capacity

To provide a more formal assessment of the concurrence of these different theoretical mechanisms, I apply a bargaining model from Powell (2012), where power shifts induced by arms imports are at the center of the analysis. If these power shifts are too large, meaning that the government cannot compensate the opposition sufficiently, conflict occurs.

Both small arms and major weapons induce power shifts in favor of the government. These increase the risk of conflict when the opponent sees conflict as a profitable strategy to prevent the government from consolidating its new position of strength. In contrast to major weapons, small arms imports also enhance specific state capacities, contributing to improved general security and economic development. In terms of the model, these state capacities, advanced by small arms importation, raise the expected future utility after the consolidation of the government has taken place. The formal model thus provides a valuable framework to reconcile the counteracting risk-inducing and risk-reducing mechanisms of small arms imports.

In the following, the model setup (section 3.2.1) and decisions in equilibrium (section 3.2.2) are laid out. I then adapt the model to derive hypotheses on effects of arms imports on conflict risk (section 3.2.3).

3.2.1 Model setup

The model is a stochastic game where two players $i \in \{G, R\}$, the government G and the rebel group R, compete for the distribution of the flow of income. The monetary utility of this income is normalized to 1 without loss of generality. However, future income and utility also depend on future state capacity c, affecting it as a multiplicative factor. Both the government and the rebel group discount future utility by the discount factor β, leading to the total value of future utility $V(c) = \frac{c}{1-\beta}$ over the infinite horizon.

The state is either consolidated or unconsolidated. In the unconsolidated state, the government wants to expedite consolidation, as consolidation parallels with a shift of power in favor of the government. However, the opposing rebel group can challenge the consolidation by fighting, thereby preventing the power shift and suspending consolidation for this period. Once consolidation has taken place, it will be sustained permanently.

In the unconsolidated state, the government G offers R a share of income $x_t \in [0, 1]$. When R accepts, the state consolidates in a single period, and the opponent and the incumbent receive incomes x_t and $1 - x_t$, respectively. When R rejects, fighting occurs, which stalls the consolidation of the state. In fighting periods, R and G receive incomes f_R and f_G, respectively. However, fighting is inefficient and costly, $f_R + f_G < 1$, which is lower than the income without conflict, which was normalized to 1.

The conflict either ends in a decisive outcome with probability d or in an indecisive stalemate with probability $1 - d$. With a stalemate, consolidation of the state is prevented for this period, and the game simply turns to the next round without changes in the power distribution. With a decisive outcome, either the rebel group eliminates the government with probability p or the government eliminates the rebel group with probability $1 - p$; elimination means that the winning side will obtain the undivided total of future expected utility, while the other side gets nothing. Note that p is the conditional probability, with dp being the probability of the rebels winning when a conflict occurs.

The probabilities (d, p) determine the reservation values F_G and F_R in the unconsolidated state for the government and the rebels, respectively. These reservation values denote the discounted payoffs of fighting to the finish, i.e. fighting until a militarily decisive outcome is reached. F_G and F_R thus give the expected utilities when a conflict occurs. Analogously, in the consolidated state, $(\tilde{d}, \tilde{p})$ determine $\tilde{F}_G$ and $\tilde{F}_R$. The rebel group's reservation value can be defined recursively as

$$F_R = d[f_R + \beta p V(c)] + (1-d)[f_R + \beta F_R] \,. \tag{1}$$

The first term entails the expected payoff when a decisive outcome can be reached, where the rebels always receive their fighting income and—with probability p, if the incumbent government can be eliminated—the total discounted utility $V(c)$, starting in the next period. The second term gives the expected payoff without a decisive outcome, again comprising the fighting income and the discounted reservation value when fighting to the finish.

Consolidation in the model's terms happens with a shift of power in favor of the government, implying a lower reservation value for the rebel group, $\tilde{F}_R < F_R$, owing to the reduced probability of winning a potential conflict.

3.2.2 Equilibrium decisions

In the consolidated state We solve the game backwards, starting in the consolidated state. The rebels will accept the government's offer $\tilde{x}$ whenever $\tilde{x}V(c) \geq \tilde{F}_R$. As fighting is costly, the government wants the rebel group to accept the offer, so it will always offer just enough to make R accept it: $\tilde{x} = \tilde{F}_R/V(c) = (1-\beta)\tilde{F}_R/c$. In equilibrium, there is thus never conflict in the consolidated state.

In the unconsolidated state In the unconsolidated state, R will fight when its reservation value for fighting F_R is greater than the future payoff $\beta\tilde{F}_R$ from fighting to the finish in the consolidated state, because consolidation always occurs when the rebel group decides not to fight. Again, the government wants to avoid fighting and thus can offer $\tilde{x}$ to make the rebels accept consolidation:

$$\tilde{x} = F_R - \beta\tilde{F}_R \tag{2}$$

However, when the shift of power is too large, i.e. $F_R - \beta\tilde{F}_R > 1$, the government is not able to fully compensate the rebel group because it cannot offer more than the utility of the total contemporary income, which we normalized to 1. As the government cannot commit to compensating its opponents in future periods, such a large power shift will result in conflict.

3.2.3 Effects of small arms' and major weapons' imports

To assess the impact of arms imports, we need to analyze how they affect the probabilities of winning and the associated reservation values. Yet, in the consolidated state, any changes in $(\tilde{p}, \tilde{d})$ only influence the income share that the government has to offer the rebel group. However, there is no distribution of power possible that will lead to conflict. Therefore, arms imports have no impact on conflict risk in consolidated states.

Corollary 1. *Arms imports do not affect conflict risk in consolidated states.*

In the unconsolidated state, the expected payoff for the rebels in case of a power shift determines whether the rebel group can be compensated; when the reservation value after the power shift, i.e. in the consolidated state, is too small, conflict occurs. Arms imports, of both small arms and major weapons, induce an ongoing shift in the distribution of power, where we can interpret F_R as the payoff before and without the power shift and $\tilde{F}_R$ after the power shift to $(\tilde{p}, \tilde{d})$ occurred:

$$\tilde{F}_R = \frac{f_R + \beta \tilde{d}\tilde{p}\tilde{V}(c)}{1 - \beta(1 - \tilde{d})} \tag{3}$$

As can be expected, lower probabilities of winning owing to increased arms imports lead to a lower expected payoff for the rebels, $\partial \tilde{F}_R / \partial \tilde{p} > 0$.[4]

We first turn to imports of major weapons, as their effect is unambiguous in this model. In addition to the power shift effect described above, their financial expenses also reduce the available future utility $\tilde{V}(c)$ derived from financial means and thus also the expected payoff $\tilde{F}_R$. With a lower $\tilde{F}_R$, the risk increases that the government cannot compensate the rebel group for the expected losses that arise owing to the power shift. Therefore, higher imports of MCW increase the risk of conflict by shifting the distribution of power and at the same time reducing the rebels' expected utility when the state would consolidate unchallenged.

Hypothesis 1. *Higher imports of MCW lead to higher conflict risk.*

In comparison, small arms imports invoke different mechanisms as laid out in the preceding section: the financial burden owing to small arms acquisitions is relatively low, and they are essential tools for conflict-preventing state capacities, contributing to security and taxing capabilities, which are in turn necessary for economic welfare and development. Formally, small arms can thus be assumed to affect the expected utility $\tilde{V}(c)$ positively owing to higher state capacity c. A larger $\tilde{V}(c)$ in turn raises the reservation value $\tilde{F}_R$, rendering conflict a relatively less valuable alternative for the rebel group. Thus, a larger $\tilde{V}(c)$, raised by small arms imports, does not directly alleviate commitment problems but makes situations where the rebel group cannot be

[4]The total effect of Δp and the probably concomitant change in the chance of a decisive outcome, Δd, on $\tilde{F}_R$ will be positive for all reasonable pairs of $(\Delta p, \Delta d)$, as higher arms imports of the government always aim at decreasing not only p, but also dp.

compensated less likely.

Combining both antagonistic mechanisms of power shifts and enhanced state capacity, our formal theory predicts small arms imports to have less of a risk-inducing effect than major weapons. If their impact via increased state capacities is large enough, the overall effect of small arms imports could even be to reduce the risk of a conflict onset.

Hypothesis 2. *Higher imports of small arms increase conflict risk less than MCW imports and may even reduce it.*

4 Testing the effect of small arms imports empirically

To evaluate the hypotheses empirically, I use time-series cross-sectional (TSCS) data on 146 countries from 1993 to 2014, covering the total time span for which reliable and comprehensive data on small arms transfers are available. The following section describes the research design and methods for analyzing the dataset as well as the operationalization and sources of the main and control variables.

4.1 Research design

Fixed effects and split-population models TSCS data, as used in this article, usually allows unobserved unit heterogeneity to be controlled for, e.g. by applying fixed effects estimation. However, in case of the rare event of conflict onset, this advantage of TSCS cannot be exploited by standard models, as fixed effects lead to biased estimates: countries without any onset, i.e. no variation in the outcome variable, drop out of the sample, biasing the estimated coefficients (Cook et al. 2020). Aiming at using the additional information contained in the TSCS data structure, this article proposes making use of two methodological advancements relevant for the conflict onset literature: first, the penalized maximal likelihood-fixed effects (PML-FE) estimator suggested by Cook et al. (ibid.), and second, a split-population duration (SP-D) model (Beger et al. 2017).

The PML-FE estimator includes fixed effects α_i for every country that experiences at least one onset over the observed period on the one hand and a common intercept α for all countries that do not experience an onset on the other hand:

$$\alpha_i = \begin{cases} \alpha_i & \text{if } \sum_{t=1}^{T} y_{it} > 0 \\ \alpha & \text{if } \sum_{t=1}^{T} y_{it} = 0 \end{cases} \tag{4}$$

As α will tend to infinity in standard models, Cook et al. (2020) use penalization to shrink the parameter. Zorn (2005) proposed using Jeffreys' invariant prior for penalizing estimates in the case of perfect separation, i.e. when the predictors perfectly explain the outcome, as is the case for fixed effects of countries that do not experience at least one onset (cf. Firth 1993). Using Jeffreys' prior for penalization introduces the assumption that each country would have an onset recorded if only the time series were long enough (Cook et al. 2020). As the penalizing term reaches its maximum with coefficients equal to zero, maximum likelihood estimation will shrink the parameters toward zero, allowing estimating fixed effects for countries without conflicts. Thus, PML-FE avoids restricting the sample to cases with at least one onset and provides more accurate estimates than standard unconditional fixed effects models (ibid.).

In simulations, the PML-FE estimator also outperforms conditional fixed-effects models already for $T = 20$, so the incidental-parameter bias problem (Neyman and Scott 1948; Lancaster 2000) should be of less concern, according to Cook et al. (2020). Nevertheless, the appendix (section A.6.1) also provides the results of conditional fixed effects logit models for comparison, although these can only take countries with at least one onset during the observation period into account.

The second innovative design that I propose for dealing with the rare-events nature of conflict onsets is a split-population model. Ninety-eight countries in my sample from 1993 to 2014 do not experience any civil conflict. This corresponds to our theoretical expectations: in the formal model laid out above, already consolidated states do not face any risk of conflict. Thus, we expect a set of countries with no risk of conflict, while other countries remain at risk. Of course, countries at risk may nevertheless not experience conflict over the sample period.

Beger et al. (2017) therefore propose a split-population duration model with the likelihood function as follows:

$$\mathcal{L} = \prod_{i=1}^{N} \{\pi_i f(t_i)\}^{\delta_i} \times \{(1 - \pi_i) + \pi_i S(t_i)\}^{1-\delta_i} \tag{5}$$

The likelihood function consists of two parts: first, the failure rate $f(t_i)$ when the country has already experienced an onset (δ_i) and, second, the survival, i.e. duration, function $S(t_i)$ when the country's observation is right-censored ($1 - \delta_i$). However, in the SP-D model, we distinguish sub-populations of countries being at risk of a conflict (π_i) or "immune" ($1-\pi_i$), as Beger et al. call it, referring to the origin of the concept in medical statistics. The probability of "immunity" of a country at a specific point in time is determined empirically through the logistic link function

$$\pi_i = \frac{1}{1 + e^{-\boldsymbol{Z_i}\boldsymbol{\gamma}}}\ , \tag{6}$$

where $\boldsymbol{Z}_i$ is the vector of country-specific covariates, which determine how likely the conflict risk in a country-year is strictly positive, and γ is the respective coefficients vector. We thus estimate at first how likely it is that a country-year belongs to the "at risk" or "immune" sub-population; then, we assess which factors render conflict in countries at risk more likely by estimating the Weibull survival function

$$S(t_i) = e^{(-e^{\{\boldsymbol{X}_i\boldsymbol{\beta}\}}\,t)^a}, \tag{7}$$

where $\boldsymbol{X}_i$ is the vector of covariates affecting the conflict hazard of countries at risk, $\boldsymbol{\beta}$ is its respective vector of coefficients, and a is the shape parameter. Country-years estimated to have a low probability of being "at risk", i.e. having a low π_i, receive only low weights when estimating the maximum likelihood function. Thus, the estimated effects appropriately account for the fact that the baseline risk of conflict is minimal for many countries.

Possibility of endogeneity A potential challenge for the aim of this article—to establish causal mechanisms of small arms imports affecting conflict risk—is the possibly endogenous demand for arms: when a government perceives high odds of civil conflict in the near future, it is reasonable to expect measures to prepare for conflict, including the importation of fighting equipment. To rule out the reverse path of causality, I follow Pamp et al. (2018) to employ an instrumental variable approach. Since civil conflict onset is a dichotomous dependent variable, IV probit models are used. In Pamp et al. (ibid.), we propose splitting major weapons imports into two mutually exclusive subsets: imports of major weapons that *are* used in civil war, like tanks and helicopters, are discerned from major weapons that *are not* employed in a typical civil war, e.g. ships and air-defense weapons. Then, the latter, i.e. arms not used in civil conflict, can be exploited as an instrument for the former, i.e. arms used in civil conflict.

For valid estimates, the instrumental variables have to be both relevant and exogenous. As relevance can be tested empirically, I find that a narrower set of weapons, i.e. surface-to-air missiles, has a closer association with civil-war relevant MCW for this article's shorter timeframe and is therefore used here. A positive relationship is expected because the general size of the military affects the acquisitions of all types of equipment, as they are complementary goods. As Pamp et al. (ibid.) argue, imports of weapons that cannot be employed in a civil war however do not affect the risk of such conflicts. Importing decisions for these arms are unrelated to the potentially increased demand for arms necessary to prepare for an internal conflict.

Weapons that cannot be used for providing internal security and preventing the formation of armed groups, as is certainly the case for surface-to-air missiles, have no impact on the specific state capacities determining the risk of conflict. Thus, they do not affect conflict risk through channels other than raising complementary imports of MCW and small arms, especially not after controlling for general state capacity and governance in the form of GDP and democracy scores (Fearon and Laitin 2003). Therefore, imports of surface-to-air missiles provide an exogenous instrument. Appendix section A.5 presents further tests that indicate or would be able to falsify exogeneity.

Additionally, I use MCW imports of potentially hostile countries and exports of small arms as instruments. Hostile countries' imports can affect imports of the country of interest owing to arms race dynamics (Pamp and Mehltretter 2020) but should not affect civil conflict risk. Potential contagion effects are controlled for by including the number of close conflicts in the analyses.[5] The relevance of small arms exports is due to their high correlation with imports, which may arise because of intra-industry trade (Thies and Peterson 2015). At the same time, exports do not affect civil conflict risk in the exporting country—certainly not after controlling for potential indirect spillover effects. If anything, exports are expected to be substituted for by production for domestic supply in light of an internal conflict and therefore rising domestic demand. The first-stage results of the models presented in the empirical results section are available in the appendix to provide evidence of the instrumental variables' relevance. All models use robust, country-clustered standard errors. Note that PML-FE and SP-D models do not control for potential endogeneity of arms imports; results on arms imports coefficients might thus be positively biased.

4.2 Data and operationalization

Arms imports To measure the build-up of specific military capabilities, I use data on imports of both small arms and major conventional weapons.[6] Governmental arms imports operationalize power shifts as the theoretical mechanism in Powell (2012) directly. They are also better suited to capturing the financial flow for military purposes than measures of endowments. To capture longer-term dynamics and build-ups of military equipment, the analyses use not only yearly imports, but also the average of imports of the preceding five years, as proposed by Pamp et al. (2018).

Small arms imports data are provided by NISAT. I exclude sporting guns and shotgun cartridges as well as primarily privately used categories like revolvers to provide a better proxy of small arms that are relevant to upholding public security or preventing an intrastate conflict.[7] Small arms imports are measured in inflation-adjusted

[5]A recent article (Bak et al. 2020) suggests rivalries could directly affect domestic violence; this would bias coefficients positively.

[6]Inclusion of light weapons as a third category would be preferable, but available data on light weapons imports are not sufficiently reliable over the time span of my analyses.

[7]Appendix section A.6.2 gives corresponding results of all models with a broader definition of small arms.

2012 US-dollars and used as natural logarithm, `log(SA imports)`[8], as well as in per capita terms to provide comparability with the previous repression literature (cf. Blanton 1999; de Soysa et al. 2010). Data on imports of major conventional weapons are collected by the Stockholm International Peace Research Institute (SIPRI) and measured in so-called "trend-indicator values". These values are based on production costs and account for the depreciation of second-hand purchases. The variable is also used logged as `log(MCW imports)`[8], which only includes major weapons types that can be used in civil conflict, following the distinction from Pamp et al. (2018) detailed above.

Note that small arms and major weapons imports are measured differently. Small arms transfers are evaluated strictly by the monetary reward for the seller, which can be affected by strategically adopted prices to support the buyer or to compensate for adversarial military gain. In contrast, major weapons imports data rather proxy the transferred military capability. Unfortunately, directly comparable measures of both types of arms transfers are not available.

Dependent variable: onset `onset` is a binary variable defined as the outbreak of at least one intrastate conflict with at least 25 battle-related deaths in one calendar year, after at least two years of less than 25 fatalities of the respective conflict were recorded. Data on intrastate conflicts is provided by the Uppsala Conflict Data Program (UCDP) (Pettersson et al. 2019; Gleditsch et al. 2002). Following McGrath (2015), I code years with ongoing conflicts as missing but preserve years coded with `1` because of additional onsets of other conflicts.[9]

Control variables All models include control variables to avoid omitted variable bias in the coefficients of interest. The control variables are lagged because a conflict onset might affect their values already in the same year. Following Cederman et al. (2013), I include the share of excluded population, which might indicate ethnic grievances that can increase conflict risk and repression (cf. Wimmer 2013; Cederman et al. 2010, 2020; for the Ethnic Power Relations data additionally see Vogt et al. 2015). Optimally, one would include information on existing armed opposition groups, but such data unfortunately is not available for non-conflict years. However, as Jackson (2010) shows, most rebel groups start arming only with the beginning of the conflict. For onsets of recurring conflicts, the time since the last conflict spell (see below) might proxy for the existence of such groups. Missing data on existing armed groups should thus only lead to minor omitted variable biases.

[8] Import values are multiplied by 100 before taking the logarithm, and negative values are replaced by 0 to account for the large share of country-years without any imports.

[9] Section A.6.3 in the appendix provides results with ongoing conflicts coded as 0 for comparison.

As is standard in the literature (Fearon and Laitin 2003; Collier and Hoeffler 2004), logged versions of population and GDP per capita are included to operationalize the wealth and poverty level (Jakobsen et al. 2013) and proxy general state capacities (Fearon and Laitin 2003). Additionally, GDP growth controls for potential tensions in dynamic economies or recessions (cf. Collier and Hoeffler 2004). Data on population and GDP is provided by the Maddison Project Database (Bolt et al. 2018). Fearon and Laitin (2003) argue that political instability may provide opportunities for insurgents to exploit the weakness of the political system; the variable `instability` is coded as a change of 3 or more on the Polity IV scale over the preceding three years. Additionally, the Polity IV score and, to account for potentially more conflict-prone anocracies (cf. ibid.), its square are included (Marshall et al. 2019).[10]

To capture potential contagion effects from intrastate conflicts in other countries in the region (cf. Buhaug and Gleditsch 2008; Reid et al. 2021), I include the number of conflicts in countries in less than 3000 km distance. Interstate disputes might offer opportunities for the opposition to challenge the government. Therefore, I control for the intensity of interstate violence, using data from Major Episodes of Political Violence (Marshall 2019).

Following the recommendations from Beck et al. (1998) and Carter and Signorino (2010) on time polynomials, the number of years since the last conflict end t, t^2 and $t^3/1000$ control for temporal dependence. For the SP-D model, the immunity equation and the duration equation have to be specified by differing sets of variables to allow identification. Thus, I use only structural variables with low variance over time for the immunity equation and the complete set of variables, including the arms imports variables (and excluding excluded population, which has too low variance over time to allow convergence of the model), for the duration equation.

4.3 Empirical results

In the following section, the results of the empirical analyses of arms imports' effects on conflict onset are presented. Standard probit as well as instrumental variable models are augmented by fixed effects and split-population models.

Probit and IV probit results Table 1 presents probit and IV probit models with `onset` as the dependent variable. Odd columns are standard probit models and even ones the respective instrumental variable model. The model pairs differ in the specification of small arms imports: models (1) and (2) use the logged average of a country's small arms imports over the past five years; models (3) and (4) use the logged 5-year average

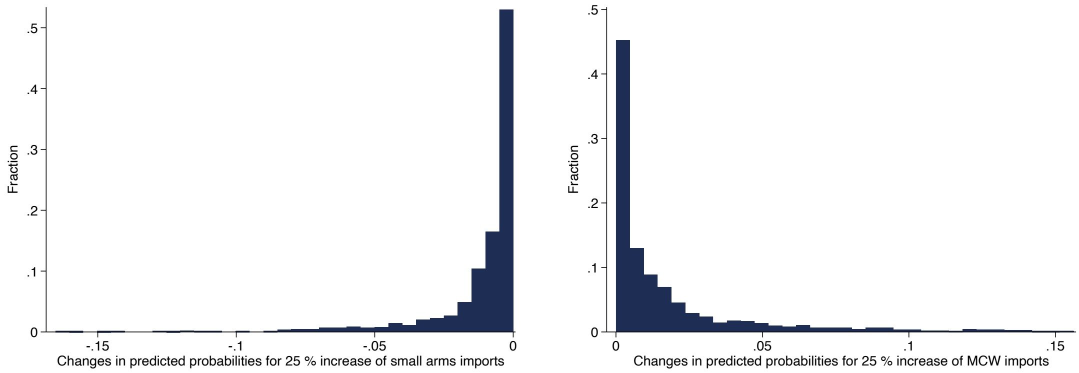
[10] Vreeland (2008) points out that using Polity IV in conflict analysis warrants caution. Section A.6.4 in the appendix therefore provides results of models using the Scalar Index of Polities (Gates et al. 2006) instead.

Table 1: Probit and IV probit results

	(1) Country SA	(2) Country SA, instr.	(3) SA p.c.	(4) SA p.c., instr.	(5) SA p.c.	(6) SA p.c., instr.
log(MCW imports), 5 yr. avg.	0.053*	0.129*	0.055*	0.151*		
	(0.026)	(0.061)	(0.026)	(0.071)		
L.log(MCW imports)					0.025	0.147**
					(0.018)	(0.054)
log(SA imports), 5 yr. avg.	–0.017+	–0.086*				
	(0.010)	(0.043)				
log(SA imp. per capita), 5 yr. avg.			–0.036+	–0.172+		
			(0.021)	(0.102)		
L.log(SA imp. per capita)					–0.014	–0.145*
					(0.016)	(0.073)
L.log(population)	0.160**	0.165*	0.140**	0.046	0.173**	0.015
	(0.057)	(0.078)	(0.054)	(0.099)	(0.050)	(0.094)
L.log(GDP p.c.)	–0.155+	0.011	–0.150+	0.009	–0.147*	–0.041
	(0.084)	(0.169)	(0.084)	(0.202)	(0.074)	(0.176)
LD.log(GDP p.c.)	0.962*	1.045**	0.950*	0.972**	0.945*	0.660+
	(0.378)	(0.376)	(0.376)	(0.377)	(0.388)	(0.396)
Instability	–0.149	–0.256	–0.163	–0.262	–0.178	–0.070
	(0.429)	(0.363)	(0.433)	(0.374)	(0.457)	(0.379)
L.Polity IV	0.011	0.029+	0.011	0.024	0.008	0.023+
	(0.014)	(0.015)	(0.015)	(0.016)	(0.015)	(0.014)
L.Polity IV × L.Polity IV	–0.004	–0.006*	–0.004	–0.005*	–0.003	–0.004+
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
L.Excluded population	0.245	0.154	0.245	0.158	0.309	0.094
	(0.195)	(0.220)	(0.195)	(0.228)	(0.189)	(0.210)
L.No. of nearby conflicts	0.052**	0.037+	0.053**	0.038+	0.055**	0.036
	(0.016)	(0.021)	(0.015)	(0.022)	(0.016)	(0.022)
L.Interstate violence	0.110	0.045	0.109	0.040	0.110	0.023
	(0.202)	(0.204)	(0.203)	(0.209)	(0.199)	(0.233)
Years since last conflict	–0.128**	–0.102**	–0.128**	–0.101**	–0.135**	–0.095*
	(0.033)	(0.032)	(0.033)	(0.036)	(0.034)	(0.037)
Years since last conflict, sq.	0.004**	0.003*	0.004**	0.003*	0.005**	0.003*
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Years since last conflict, cubic	–0.043*	–0.035*	–0.044*	–0.034+	–0.046*	–0.032+
	(0.018)	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)
N	2806	2806	2806	2806	2806	2785
Pseudo R^2	0.291		0.291		0.287	

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses. Dependent variable: intrastate conflict onset.
Odd models use probit, even models IV probit instrumenting both small arms and major weapons imports variables.

Figure 1: Changes in predicted probabilities for 25 % increases in small arms and MCW imports



Distributions of changes in predicted probabilities for country-years in sample when small arms imports (left panel) or MCW imports (right panel) were 25 % higher than observed. Years without imports of small arms or MCW, respectively, were excluded.

of small arms imports per capita; and models (5) and (6) use the logged 1-year lagged imports of MCW and per-capita imports of small arms.

Except for model (5), all specifications corroborate Pamp et al. (2018), finding a statistically significant effect of major weapons imports that increase conflict risk. All models exhibit negative coefficients for small arms imports. However, only in models (2) and (6) are small arms imports statistically significant. Notably, none of the specifications indicates that small arms imports would make conflict more likely—in contrast to imports of major weapons, which are consistently shown to increase the risk of intrastate conflicts. This highlights the necessity to differentiate between these specific types of weapons. Tables A1, A2 and A3 in the appendix show additional results for all models with different subsets of covariates. The negative effect of small arms imports does not hinge on specific variables that the model is controlling for and is already present in a model without any controls.

The coefficient of -0.086 (-0.017) in the IV (non-IV) model in table 1 implies a 2.5 (.3) percentage point decrease in conflict risk for a one standard deviation increase in logged imports. However, the risk decrease can be substantially larger in countries with higher baseline risk (see section A.2 in the appendix for more details on marginal effects under meaningful scenarios).

Figure 1 illustrates how conflict risk is affected by increases in small arms and major weapons imports. Under a hypothetical increase of small arms imports by 25 %, a substantial but not extreme rise, predicted probabilities naturally decrease, as the estimations exhibited a negative coefficient for small arms imports. The left panel of figure 1 shows how these changes in the predicted probabilities are distributed. Most country-years would only see single-digit or near-zero reductions in conflict risk when small arms imports were 25 % higher than those observed in reality. However, some countries like Turkey, Sudan, India and Pakistan were predicted to have—averaged over all years in the sample—a 9-12 percentage point lower conflict risk with such an increase in small arms imports.

When increasing major weapons by 25 % compared with the observed values, predicted probabilities rise owing to the positive coefficient of MCW imports in our results. The right panel of figure 1 shows the distribution of these changes in predicted conflict risk. Again, the increases are rather small in most cases, but some country-years see the conflict risk rise by more than 5 percentage points. With China, Turkey, India, Pakistan and Sudan, a similar set of countries would see the largest surge of risk with increases in MCW imports.[11]

Returning to our theoretical framework, the evidence substantiates predictions based on Powell's model of power shifts: major weapons imports do not seem to deter potential challengers, but instead might be better interpreted as ongoing power shifts that provoke preemptive opposition attacks, as expected by hypothesis 1. In line with hypothesis 2, small arms are shown to affect conflict risk differently, with imports not increasing or even decreasing the probability of onset.[12]

The control variables mostly show the expected effects, except GDP growth, where high growth seems to be associated with higher conflict risk after controlling for GDP level.

Fixed effects model Using the advantages of TSCS data, penalized maximum likelihood-fixed effects allow unobserved heterogeneity between countries to be controlled for. Table 2 gives the results of two specifications with country sum (model 1) and per capita (model 2) small arms imports. Overall, the results support the conclusions from the standard probit models. However, small arms show a more robust statistically significant effect, decreasing conflict risk as already suggested by the probit models. The coefficient of -0.070 means that the conflict risk is .9 percentage points lower with a one standard deviation rise in small arms imports, giving an estimated effect size between the also non-instrumented standard probit and the IV probit models.[13] Note that PML-FE does not instrument arms imports; possible endogeneity would bias results positively, i.e. the true effect of small arms imports might be more strongly

[11] Note that this does not imply that the total effects of MCW imports and small arms imports cancel out each other. Figure A2 in the appendix graphs the distribution of the sum of the linear predictors of both arms types, showing a large heterogeneity of both negative and positive total contributions of the arms variables to the linear prediction.

[12] Section A.2 in the appendix provides marginal effects for countries with generally low risk, lending support to corollary 1 predicting no effect of arms imports with consolidated institutions.

[13] Section A.3 in the appendix further compares the size of the effects in the PML-FE and probit models using marginal effects calculations for different low- and high-risk scenarios.

Table 2: PML-FE and SP-D results

	PML-FE	*PML-FE*	*SP-D*	
	Country SA	SA p.c.	Immunity eq.	Duration eq.
	(1)	(2)	(3)	
log(MCW imports), 5 yr. avg.	0.269**	0.279**		−0.105*
	(0.069)	(0.070)		(0.042)
log(SA imports), 5 yr. avg.	−0.070*			0.064**
	(0.029)			(0.021)
log(SA imp. per capita), 5 yr. avg.		−0.147*		
		(0.062)		
L.log(population)	1.676**	1.565**	0.645**	−0.084
	(0.359)	(0.363)	(0.186)	(0.086)
L.log(GDP p.c.)	−0.724*	−0.728*	−0.508+	0.192
	(0.308)	(0.310)	(0.271)	(0.160)
LD.log(GDP p.c.)	1.768+	1.759+		−1.859+
	(0.966)	(0.963)		(1.011)
Instability	−0.215	−0.236		0.545
	(0.992)	(0.995)		(0.980)
L.Polity IV	−0.032	−0.035	−0.011	−0.021
	(0.032)	(0.032)	(0.045)	(0.025)
L.Polity IV × L.Polity IV	−0.011	−0.010	−0.036*	−0.007
	(0.007)	(0.007)	(0.014)	(0.006)
L.Excluded population	−0.200	−0.249	0.553	
	(0.838)	(0.839)	(1.197)	
L.No. of nearby conflicts	0.072	0.074		−0.085**
	(0.049)	(0.049)		(0.024)
L.Interstate violence	0.428	0.441		−1.151**
	(0.471)	(0.469)		(0.239)
Years since last conflict	−0.122*	−0.131*		
	(0.062)	(0.063)		
Years since last conflict, sq.	0.006*	0.006*		
	(0.003)	(0.003)		
Years since last conflict, cubic	−0.077*	−0.082*		
	(0.036)	(0.037)		
Constant	−19.980**	−18.424**	−0.134	2.537
	(6.531)	(6.421)	(3.915)	(2.172)
log(α)			−0.155+	
			(0.084)	
Observations	2,907	2,907	3348	
Akaike Inf. Crit.	700.273	700.550	854.945	

Notes: * ($^{+}$, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses. Models (1) and (2) use penalized maximum likelihood-fixed effects estimation with intrastate conflict onset as dependent variable. Model 3 uses split-population duration estimation, with the immunity equation estimating whether a country-year is "at risk" and the duration equation estimating the risk for the observations at risk. Note that positive (negative) coefficients in the (Weibull) duration equation imply a risk-reducing (risk-increasing) effect of the variable.

negative than indicated by the PML-FE estimations.

Split-population model The split-population duration model employs two equations. The immunity equation determines the likelihood of a country-year being "immune" to conflict. For the sub-population of country-years at risk, the duration equation determines how long peace lasts until a civil conflict onset, i.e. how large the risk is.

Model (3) of table 2 presents results on an SP-D model using the country sum of small arms imports. The share of excluded population, level of GDP, population size and the Polity IV institutions indicator specify how likely a country in a given year is to be in the at-risk group. Arms imports only come into play when underlying tensions are present (cf. corollary 1) and are thus only included in the duration equation.

The results of the duration equation strongly support the results from standard probit and PML-FE models. When sufficient risk conditions apply, imports of major weapons are associated with shorter durations until the next onset, i.e. higher onset risk. Small arms imports, in contrast, prolong the duration, i.e. decrease onset risk.[14]

Note that here in the SP-D model, risk of conflict does not heavily depend on peace duration, as indicated by the statistically only weakly significant $\log(\alpha)$. Including time polynomials in standard probit models might thus primarily control for at-risk status, as countries not at risk experience exceptionally long peace spells.

5 Conclusion

This article presents a theory of how small arms can affect the risk of intrastate conflict and formalizes these effects in a bargaining model. The empirical results underpin the derived hypotheses: imports of major weapons increase conflict risk, while imports of small arms have no significant effect or even reduce it. The role of arms imports in the emergence of civil conflict thus seems to be less one of deterrence, but rather primarily characterized by Powell's (2012; 2013) reasoning of power shifts. Imports of major weapons change the distribution of power in favor of the incumbent government, potentially to such an extent that it is impossible to compensate the opposition for its likely loss of power. However, small arms seem to play a different role. This article argues that small arms are also necessary tools for enhancing conflict-preventing state capacities to improve security, taxation and general economic development. Thereby, they might rather support state stability than contribute to state fragility.

[14]Note that in the Weibull model, positive (negative) coefficients indicate a prolonging (shortening) effect on duration. A direct comparison with probit and PML-FE models in terms of marginal effects is not available for the SP-D model.

In no way do the results imply that importing more small arms is unconditionally good advice to achieve development and consolidation of a state. Further research is warranted on the exact usage of small arms that can counter the conflict-inducing effects shown by major weapons imports. Presumably, it is not only the positive authority of the rule of law that prevents the emergence of conflict but also repressive, deterring authority impeding any organization of opposition. Previous results in the literature arguing that small arms imports can deteriorate respect for human rights and encourage repression (de Soysa et al. 2010; Brender and Pfaff 2018) might indicate this.

Unfortunately, the state of data on small arms transfers is alarming: NISAT was deprived of funding in 2017, meaning that the most recent available data are from 2014 and will not be updated. Therefore, a new data collection effort is warranted to better understand the effects of the small arms trade on conflict. Findings in the literature, including this article, can only be the first indications that await confirmation in future research with more recent and reliable data.

6 Addendum: Effects of small arms and major weapons importation on repression

6.1 Arming and repression

Before a struggle over power crosses the threshold of onsetting an outright deadly conflict, governments may turn to use methods of repression, aiming to suppress the forming of organizational structures and potential upheaval. The availability of arms is a necessary condition for repression, as they constitute the tools usually applied when human rights are violated (de Soysa et al. 2010). Arms build-ups might also directly affect the government's reaction to dissent, potentially incentivizing a repression-focused strategy. In general, governments react to the perception of rising potential for rebellion with increased repression, aiming at pre-emptively hindering rebels to plan and carry out acts of insurgency (Danneman and Ritter 2014).

Section 3.4 in chapter 1 provides a formal theoretical framework based on a model from Besley and Persson (2011) that specifically includes repression as a subdued stage of conflict. In this model, repression is defined as strictly one-sided violence and used to increase the government's chances of staying in power; repressing challengers is enabled by investment in the military. Repression thus occurs when the government side invests in arming, while the opposition does not. Owing to this direct relationship, arms imports of both MCW and SA are predicted to induce increased repression. Similarly, Blanton (1999) argues that a government in fear of instability and loss of power resorts to repression when arms imports provide the necessary capabilities (cf. Klare and Arnson 1981).

The empirical literature on the effects of arms imports on repression and human rights violations is relatively conclusive and generally finds that arms imports lead to more repression. Blanton (1999) uses data from WMEAT, including both major weapons and small arms, finding the effect for the period 1982–1992. With data from NISAT, de Soysa et al. (2010) and Brender and Pfaff (2018) can test the association between repression and imports explicitly for small arms, corroborating the results from Blanton (1999). Sullivan et al. (2018) employ the instrumental variable design developed by Pamp et al. (2018) to provide a causal assessment for the effects of major weapons imports, finding again a negative effect on human rights. In contrast, Magesan and Swee (2018) find a positive impact of US DCS.

6.2 Research design and data

Why a government uses repression strongly depends on whether the country is already in a civil conflict or not. In general, human rights violations deteriorate in civil conflict years (e.g. Blanton 1999; de Soysa et al. 2010). However, following the theoretical exclusiveness of repression and conflict in the Besley and Persson (2011) model, the focus of this analysis lies on repression in peace years. Thus, only years without incidence of civil conflict are included in my analyses.[15]

As is the case for the analysis of conflict onset, including fixed effects can control for unobserved heterogeneity between countries. de Soysa et al. (2010), for instance, argue that repression might be associated with a country-specific but unobserved "gun culture"; fixed effects are thus warranted. At the same time, repression reveals strong state dependence, which would suggest to use a dynamic panel model including a lagged dependent variable (Wooldridge 2010). In addition to a pooled dynamic panel model, I estimate a fixed effects dynamic model. As this might give rise to concerns about Nickell bias (Nickell 1981), the analysis is also conducted using a difference GMM model (Arellano and Bond 1991; Roodman 2009). All models use robust, country-clustered standard errors.

Sullivan et al. (2018) suggest using an instrumental variable design for examining repression; however, their models do not find evidence for endogeneity. Also, theoretical considerations do not provide direct mechanisms for human rights violations causing arms transfers. Thus, an instrumental variable design is not deemed necessary.[16]

Concerning the measurement of the dependent variable, a variety of indicators attempts to measure repression, i.e. violations of human or "physical integrity" rights. However, many of these indicators, as mainly used in the existing studies on small arms and repression, suffer from reporting bias or systematic changes in information collection and evaluation (Fariss 2014). To generate a more reliable indicator, Fariss (ibid.) uses a dynamic latent variable model to combine information from different sources like the Political Terror Scale and the Cingranelli-Richards Human Rights Data to an arguably less biased measure of respect for human rights, which is therefore used in this article and in Brender and Pfaff (2018).

In addition to the variables used in analyzing conflict onset, the analyses additionally include the executive constraints indicator from Polity IV (cf. Davenport 2007).

6.3 Empirical analysis

Turning to repression as dependent variable, table 3 presents results of four different estimation models, each for both country sum and per capita small arms imports. The first model pair (1) and (2) employs a pooled OLS analysis with a lagged dependent variable (LDV), models (3) and (4) fixed effects (FE) OLS without and (5) and (6) with

[15]The indicator from Fariss (2014) that I use in my analyses also relies on different conflict measures like deaths from one-sided violence from UCDP, which convolutes the theoretical concept of repression in conflict periods.

[16]Instrumenting arms imports in this section's repression models leads to results similar to the non-instrumented models, see appendix section A.7.

LDV and (7) and (8) difference GMM estimation. Note that the dependent variable measures respect of human rights, i.e. higher values imply less repression.

Notably, the results from Sullivan et al. (2018), showing that higher imports of MCW lead to more repression, can only be corroborated in the static fixed effects models, which are similar to the model they employ in their analyses. Adding a LDV, MCW imports do not show a statistically significant effect; and in the pooled OLS and difference GMM, the coefficients even turn positive, implying larger MCW imports are associated with a better human rights situation, but not with statistical significance. As described in section 6.2, the existence of both unit heterogeneity (null hypothesis of no fixed effects is strongly rejected) and temporal dependence suggest models (5) and (6), or—mitigating potential Nickell bias—difference GMM models (7) and (8) obtain more credible results. The analyses thus cast doubt on the association of MCW imports with repression found by Sullivan et al. (ibid.). Instead, they are more in line with Brender and Pfaff (2018) who do not find a statistically significant effect of major weapons.

Regarding effects of small arms imports, results of the models are more consistent, indicating that small arms imports may deteriorate respect for human rights. However, the effect is very small in size, leading to the conclusion that small arms imports in my analyses seem not to affect repression substantially.

All in all, modeling choices prove essential to explain previous findings in the literature regarding arms imports' effects on repression. When using a difference GMM model, which allows to control for unit heterogeneity and the reliance of the dependent variable on past realizations, major weapons do not exhibit a significant effect; small arms do increase repression statistically significantly, but the effect is of no substantial size. Armament might thus play less of a role in the dissent-repression cycle that could lead to escalation to outright conflict (Lichbach 1987), but rather enact more direct mechanisms like preemptive attacking to prevent power shifts—which theoretical and empirical analyses in this thesis provide support for.

Table 3: Repression

	(1) *OLS*	(2) *OLS*	(3) *FE*	(4) *FE*	(5) *FE*	(6) *FE*	(7) *GMM*	(8) *GMM*
log(MCW imports), 5 yr. avg.	0.004**	0.004*	–0.023*	–0.024*	0.004	0.004	0.007+	0.007+
	(0.002)	(0.002)	(0.010)	(0.010)	(0.002)	(0.002)	(0.004)	(0.004)
log(SA imports), 5 yr. avg.	0.000		–0.004		–0.002		–0.005**	
	(0.001)		(0.005)		(0.001)		(0.002)	
log(SA imp. per capita), 5 yr. avg.		0.001		–0.004		–0.002		–0.007*
		(0.001)		(0.009)		(0.002)		(0.003)
L.Repression	0.973**	0.973**			0.881**	0.881**	0.928**	0.927**
	(0.006)	(0.006)			(0.010)	(0.010)	(0.077)	(0.077)
L.log(population)	–0.017**	–0.017**	–0.173	–0.171	–0.205**	–0.204**	–0.381**	–0.383**
	(0.004)	(0.004)	(0.229)	(0.229)	(0.045)	(0.046)	(0.131)	(0.132)
L.log(GDP p.c.)	0.006	0.005	0.231*	0.228*	–0.016	–0.017	–0.144*	–0.145*
	(0.007)	(0.007)	(0.112)	(0.112)	(0.021)	(0.021)	(0.056)	(0.056)
LD.log(GDP p.c.)	0.051	0.051	–0.335*	–0.336*	–0.015	–0.015	0.011	0.011
	(0.059)	(0.059)	(0.165)	(0.165)	(0.060)	(0.059)	(0.050)	(0.050)
Instability	–0.056	–0.055	–0.091	–0.091	–0.058	–0.058	–0.025	–0.026
	(0.057)	(0.057)	(0.153)	(0.153)	(0.046)	(0.046)	(0.069)	(0.069)
L.Polity IV	–0.001	–0.001	0.011	0.011	–0.005	–0.005	–0.016+	–0.016+
	(0.002)	(0.002)	(0.019)	(0.019)	(0.004)	(0.004)	(0.009)	(0.009)
L.Polity IV × L.Polity IV	–0.000	–0.000	0.001	0.001	0.000	0.000	–0.001	–0.001
	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)	(0.000)	(0.001)	(0.001)
L.Excluded population	–0.000	–0.000	–0.029	–0.029	–0.033	–0.033	–0.071	–0.071
	(0.021)	(0.022)	(0.207)	(0.207)	(0.074)	(0.074)	(0.108)	(0.107)
L.No. of nearby conflicts	–0.003**	–0.003**	–0.006	–0.005	–0.001	–0.001	0.002	0.002
	(0.001)	(0.001)	(0.010)	(0.010)	(0.003)	(0.003)	(0.003)	(0.003)
L.Interstate violence	0.015	0.015	0.039	0.039	0.028	0.028	0.049	0.049
	(0.037)	(0.037)	(0.071)	(0.071)	(0.022)	(0.022)	(0.034)	(0.035)
Years since last onset	–0.007**	–0.007**	0.029*	0.029*	–0.007*	–0.007*	–0.033**	–0.033**
	(0.002)	(0.002)	(0.013)	(0.013)	(0.003)	(0.003)	(0.012)	(0.012)
Years since last onset, sq.	0.000**	0.000**	–0.001+	–0.001+	0.000+	0.000+	0.001**	0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years since last onset, cubic	–0.001*	–0.001*	0.007	0.007	–0.001+	–0.001	–0.008**	–0.008**
	(0.001)	(0.001)	(0.005)	(0.005)	(0.001)	(0.001)	(0.003)	(0.003)
L.Executive constraints	0.013+	0.013+	0.062	0.060	0.024+	0.023+	0.072*	0.071*
	(0.007)	(0.007)	(0.054)	(0.054)	(0.013)	(0.013)	(0.030)	(0.030)
Constant	0.209+	0.217+	–0.457	–0.467	3.158**	3.158**		
	(0.114)	(0.112)	(3.843)	(3.844)	(0.718)	(0.724)		
N	2735	2735	2735	2735	2735	2735	2599	2599
R^2 (within for FE models)	0.987	0.987	0.215	0.214	0.869	0.868		
No. of instruments							43	43
Hansen stat. p-value							0.417	0.398
Sargan stat. p-value							0.020	0.015

Note: * (+, **) indicates $p < .05$ (.1, .01). Higher values of the dependent variable indicate lower levels of repression.

Appendix

A.1 Probit and IV probit results for different sets of covariates

To better evaluate the association of small arms imports and intrastate conflict onset, tables A1, A2 and A3 provide results of probit and IV probit models with different sets of covariates for the total sum of small arms imports, averaged over the preceding 5 years, for the per capita small arms imports, averaged over the preceding 5 years, and for the lagged per capita small arms imports, respectively. As seen in each table's first model column, the negative association between small arms imports and onset is already present without any covariates. For the different covariate subsets, the coefficient of small arms imports is always negative and in almost all cases statistically significant.

A.2 Marginal effects in different risk scenarios

Interpretation of probit models requires special care due to the inherent interdependence of effects (Hanmer and Kalkan 2013). Therefore, I additionally present predicted probabilities in different risk scenarios: in the high-risk scenario, all variables are set to their 90th-percentile (10th-percentile) value when increasing (reducing) conflict risk in our analysis and vice versa in the low-risk scenario (cf. Pamp et al. 2018). In figure A1 (p. 137), both panels depict the predicted probability of onset over the observed range of logged small arms imports, on the left for the 5-year average country sum, on the right for the lagged per capita version.

In the low-risk scenario, predicted probabilities are close to 0 and small arms imports have no particular effect. Our theoretical framework predicted this for consolidated institutions as in the low-risk case (see corollary 1 for arms imports having no effect in this case). In the high-risk scenario, both specifications show rather large decreases in predicted probabilities for increased small arms imports; however, confidence bands are also large, with statistically significant differences only between extreme levels of imports. Nevertheless, it is clear evidence that small arms do *not* render conflict *more* likely, underpinning hypothesis 2.

A.3 Comparing marginal effects between models

Marginal effects are also informative when comparing the results of the different probit estimation models used, i.e. standard probit, instrumental variable probit and PML-FE. Note that results from the SP-D models cannot be directly compared because it estimates the duration of peace spells instead of onset risk.

Table A1: Probit and IV probit results on different covariate sets: 5-year averaged SA imports

	No covariates		Only arms imports		Time dependence		Population/economics		Institutions		All, w/o external environm.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
log(MCW imports), 5 yr. avg.			0.114**	0.179**	0.091**	0.152**	0.070**	0.120*	0.097**	0.186**	0.066*	0.156**
			(0.031)	(0.048)	(0.021)	(0.039)	(0.026)	(0.050)	(0.023)	(0.044)	(0.026)	(0.049)
log(SA imports), 5 yr. avg.	–0.022**	–0.020	–0.053**	–0.100**	–0.031**	–0.069**	–0.022*	–0.083**	–0.026**	–0.079**	–0.021*	–0.105**
	(0.008)	(0.018)	(0.012)	(0.025)	(0.008)	(0.023)	(0.010)	(0.027)	(0.009)	(0.029)	(0.010)	(0.034)
Years since last onset					–0.160**	–0.136**	–0.136**	–0.112**	–0.151**	–0.119**	–0.133**	–0.096**
					(0.035)	(0.035)	(0.032)	(0.032)	(0.036)	(0.035)	(0.032)	(0.033)
Years since last onset, sq.					0.005**	0.005**	0.005**	0.004*	0.005**	0.004**	0.004**	0.003*
					(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Years since last onset, cubic					–0.056**	–0.047*	–0.047*	–0.038*	–0.052**	–0.042*	–0.045*	–0.033+
					(0.019)	(0.019)	(0.019)	(0.018)	(0.019)	(0.018)	(0.018)	(0.017)
L.log(population)							0.151*	0.172*			0.161*	0.161*
							(0.064)	(0.083)			(0.063)	(0.079)
L.log(GDP p.c.)							–0.196*	–0.029			–0.137	0.064
							(0.078)	(0.146)			(0.095)	(0.167)
LD.log(GDP p.c.)							0.692+	0.828*			0.673+	0.860*
							(0.392)	(0.387)			(0.383)	(0.371)
Instability									–0.210	–0.248	–0.040	–0.204
									(0.493)	(0.459)	(0.524)	(0.434)
L.Polity IV									0.002	0.019	0.001	0.026+
									(0.012)	(0.014)	(0.012)	(0.015)
L.Polity IV × L.Polity IV									–0.006**	–0.006**	–0.004+	–0.006*
									(0.002)	(0.002)	(0.003)	(0.003)
L.Excluded population									0.216	0.080	0.233	0.115
									(0.222)	(0.215)	(0.209)	(0.228)
N	2806	2806	2806	2806	2806	2806	2806	2806	2806	2806	2806	2806
Pseudo R^2	0.015		0.077		0.231		0.268		0.244		0.274	

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses. Dependent variable: intrastate conflict onset. Odd models use probit, even models IV probit instrumenting both small arms and major weapons imports variables.

Table A2: Probit and IV probit results on different covariate sets: 5-year averaged per capita SA imports

	No covariates		Only arms imports		Time dependence		Population/economics		Institutions		All, w/o external environm.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
log(MCW imports), 5 yr. avg.			0.116**	0.186**	0.094**	0.160**	0.072**	0.147**	0.102**	0.187**	0.068**	0.187**
			(0.031)	(0.044)	(0.021)	(0.038)	(0.026)	(0.051)	(0.023)	(0.040)	(0.026)	(0.051)
log(SA imp. per capita), 5 yr. avg.	–0.065**	–0.060+	–0.124**	–0.232**	–0.079**	–0.172**	–0.045*	–0.182**	–0.070**	–0.186**	–0.043*	–0.226**
	(0.016)	(0.033)	(0.025)	(0.043)	(0.018)	(0.046)	(0.022)	(0.063)	(0.020)	(0.055)	(0.022)	(0.079)
Years since last onset					–0.155**	–0.118**	–0.137**	–0.107**	–0.147**	–0.104**	–0.134**	–0.089*
					(0.034)	(0.036)	(0.033)	(0.034)	(0.035)	(0.035)	(0.032)	(0.037)
Years since last onset, sq.					0.005**	0.004**	0.005**	0.004*	0.005**	0.004*	0.005**	0.003+
					(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Years since last onset, cubic					–0.054**	–0.042*	–0.048*	–0.036+	–0.050**	–0.037*	–0.046*	–0.030
					(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.017)	(0.018)	(0.018)
L.log(population)							0.125*	0.051			0.136*	0.010
							(0.060)	(0.082)			(0.058)	(0.088)
L.log(GDP p.c.)							–0.193*	–0.006			–0.134	0.096
							(0.080)	(0.166)			(0.095)	(0.198)
LD.log(GDP p.c.)							0.669+	0.769*			0.649+	0.757*
							(0.390)	(0.387)			(0.381)	(0.370)
Instability									–0.219	–0.255	–0.057	–0.241
									(0.494)	(0.453)	(0.529)	(0.425)
L.Polity IV									0.003	0.016	–0.000	0.023
									(0.013)	(0.013)	(0.012)	(0.016)
L.Polity IV × L.Polity IV									–0.006**	–0.004+	–0.004+	–0.006*
									(0.002)	(0.003)	(0.003)	(0.003)
L.Excluded population									0.203	0.097	0.234	0.114
									(0.221)	(0.241)	(0.211)	(0.251)
N	2806	2806	2806	2806	2806	2806	2806	2806	2806	2806	2806	2806
Pseudo R^2	0.033		0.103		0.241		0.268		0.253		0.274	

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses. Dependent variable: intrastate conflict onset. Odd models use probit, even models IV probit instrumenting both small arms and major weapons imports variables.

Table A3: Probit and IV probit results on different covariate sets: lagged per capita SA imports

	No covariates		Only arms imports		Time dependence		Population/economics		Institutions		All, w/o external environm.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
L.log(MCW imports)			0.065**	0.161**	0.056**	0.148**	0.036+	0.141**	0.059**	0.169**	0.031+	0.170**
			(0.024)	(0.030)	(0.017)	(0.028)	(0.020)	(0.044)	(0.018)	(0.026)	(0.019)	(0.036)
L.log(SA imp. per capita)	–0.041**	–0.057+	–0.068**	–0.193**	–0.039**	–0.151**	–0.019	–0.152**	–0.029*	–0.162**	–0.014	–0.180**
	(0.011)	(0.031)	(0.016)	(0.026)	(0.013)	(0.031)	(0.016)	(0.049)	(0.014)	(0.035)	(0.016)	(0.054)
Years since last onset					–0.175**	–0.114**	–0.148**	–0.102**	–0.165**	–0.095**	–0.143**	–0.080*
					(0.036)	(0.034)	(0.034)	(0.033)	(0.038)	(0.033)	(0.033)	(0.036)
Years since last onset, sq.					0.006**	0.004**	0.005**	0.003*	0.006**	0.003*	0.005**	0.003+
					(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Years since last onset, cubic					–0.062**	–0.041*	–0.052**	–0.034+	–0.057**	–0.034*	–0.049**	–0.027
					(0.020)	(0.018)	(0.019)	(0.018)	(0.020)	(0.016)	(0.019)	(0.017)
L.log(population)							0.166**	0.023			0.176**	–0.019
							(0.055)	(0.077)			(0.054)	(0.076)
L.log(GDP p.c.)							–0.186**	–0.048			–0.131	0.028
							(0.070)	(0.156)			(0.085)	(0.171)
LD.log(GDP p.c.)							0.623	0.461			0.597	0.362
							(0.404)	(0.371)			(0.390)	(0.353)
Instability									–0.230	–0.032	–0.062	0.022
									(0.507)	(0.422)	(0.543)	(0.406)
L.Polity IV									–0.003	0.016	–0.006	0.020+
									(0.012)	(0.011)	(0.012)	(0.012)
L.Polity IV $\times$ L.Polity IV									–0.005*	–0.004	–0.004	–0.005*
									(0.002)	(0.002)	(0.002)	(0.002)
L.Excluded population									0.266	0.029	0.304	0.022
									(0.227)	(0.217)	(0.210)	(0.209)
N	2806	2785	2806	2785	2806	2785	2806	2785	2806	2785	2806	2785
Pseudo R^2	0.019		0.052		0.222		0.260		0.235		0.267	

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses. Dependent variable: intrastate conflict onset. Odd models use probit, even models IV probit instrumenting both small arms and major weapons imports variables.

Table A4: Comparing marginal effects between models

	At mean values	Low risk	High risk
Standard probit	$-.0005$	$-.0000$	$-.0063^{+}$
IV probit	$-.0033$	$-.0001$	$-.0273^{*}$
PML-FE	$-.0012^{*}$	$-.0000$	$-.0038^{+}$

Marginal effects of `log(SA imports, 5 yr. avg.)` for probit, IV probit and PML-FE models, based on the results presented in tables 1 and 2, respectively. Low-risk and high-risk scenarios are defined in appendix section A.2.

Table A4 presents marginal effects for both low and high-risk scenarios as defined above in appendix section A.2 as well as the mean scenario using the mean observed values of all variables. Consistent with demand endogeneity, the instrumental variable model exhibits smaller, i.e. more negative, marginal effects than the non-instrumented probit and PML-FE models. All models show their relatively greatest effect in the high-risk scenario. In the standard probit model, an increase of small arms imports from zero to the mean value reduces conflict risk of ca. 7 percentage points, c.p. The IV probit model even predicts a reduction of ca. 30 percentage points. The PML-FE model exhibits the smallest absolute effect in the high-risk scenario, but in comparison to the standard probit model a greater, significant effect at mean values.

A.4 Total risk contribution of small arms and major weapons imports

To evaluate whether the negative effect of small arms imports is just counteracting the positive effect of major weapons imports, leading to a net-zero overall effect, figure A2 (p. 137) presents the distribution of the overall effect. This overall effect is computed as the total contribution of both arms imports variables to each observation's linear predictor in the IV probit model, i.e. $\beta_{SA}\times$ `SA imports` $+\beta_{MCW}\times$ `MCW imports`. The figure shows a wide heterogeneity of total effects, with both positive and negative overall effects arising, but a greater number of observations having an overall negative effect predicted. Also, note the spike at 0 for country-years with neither small arms nor major weapons imports recorded.

A.5 IV validity and exogeneity plausibility tests

A.5.1 IV probit results, first stages

In table A5, the relevance of the instrumental variables used in the probit analyses is confirmed, as indicated by the statistically significant coefficients in the first stage and a sufficiently large F-statistic for testing the relevance of the instruments, although instruments for small arms imports are not particularly strong. Nevertheless, the IV

Table A5: IV probit results, first stages

	Country SA, instr.		SA p.c., instr.		SA p.c., instr.	
	(1)	(2)	(3)	(4)	(5)	(6)
	MCW	SA	MCW	SA	MCW	SA
log(Air force missiles), 5 yr. avg.	0.542**	0.381**	0.542**	0.260**		
	(0.053)	(0.106)	(0.053)	(0.060)		
L.log(Air force missiles)					0.402**	0.195**
					(0.043)	(0.040)
log(Hostile MCW imports), 5 yr. avg.	0.452+	–1.807**	0.452+	–0.547		
	(0.240)	(0.602)	(0.240)	(0.338)		
L.log(Hostile MCW imports)					0.135	–0.513
					(0.275)	(0.350)
log(SA exports), 5 yr. avg.	0.038*	0.253**	0.038*	0.117**		
	(0.019)	(0.061)	(0.019)	(0.034)		
L.log(SA exports per capita)					0.027	0.158**
					(0.027)	(0.049)
L.log(population)	0.579**	0.678*	0.579**	–0.217	0.793**	–0.033
	(0.105)	(0.292)	(0.105)	(0.164)	(0.097)	(0.148)
L.log(GDP p.c.)	0.971**	3.016**	0.971**	1.697**	1.228**	1.807**
	(0.133)	(0.425)	(0.133)	(0.237)	(0.133)	(0.217)
LD.log(GDP p.c.)	0.214	1.078	0.214	0.252	2.483**	1.562
	(0.655)	(1.478)	(0.655)	(0.854)	(0.735)	(1.047)
Instability	–1.272**	–2.625	–1.272**	–1.590+	–2.024**	–1.205
	(0.399)	(2.011)	(0.399)	(0.893)	(0.732)	(0.965)
L.Polity IV	0.008	0.178**	0.008	0.073*	0.012	0.094**
	(0.020)	(0.060)	(0.020)	(0.033)	(0.023)	(0.033)
L.Polity IV × L.Polity IV	–0.007+	–0.030*	–0.007+	–0.015*	–0.008	–0.016*
	(0.004)	(0.012)	(0.004)	(0.007)	(0.005)	(0.007)
L.Excluded population	2.084**	1.568	2.084**	1.001	1.889**	0.500
	(0.566)	(1.745)	(0.566)	(1.007)	(0.607)	(1.003)
L.No. of nearby conflicts	0.051*	0.012	0.051*	0.013	0.073*	0.030
	(0.025)	(0.085)	(0.025)	(0.048)	(0.029)	(0.050)
L.Interstate violence	–0.458*	–1.183	–0.458*	–0.698*	–0.042	–0.631
	(0.193)	(0.719)	(0.193)	(0.335)	(0.340)	(0.470)
Years since last onset	–0.014	0.154	–0.014	0.078	–0.011	0.088
	(0.040)	(0.101)	(0.040)	(0.056)	(0.043)	(0.058)
Years since last onset, sq.	0.000	–0.006+	0.000	–0.003	0.000	–0.004+
	(0.001)	(0.004)	(0.001)	(0.002)	(0.002)	(0.002)
Years since last onset, cubic	–0.003	0.058+	–0.003	0.031	–0.001	0.035+
	(0.013)	(0.034)	(0.013)	(0.019)	(0.015)	(0.020)
Constant	–20.967**	–31.404**	–20.967**	–13.181**	–25.049**	–16.702**
	(2.960)	(8.141)	(2.960)	(4.532)	(2.791)	(3.999)
N	2806	2806	2806	2806	2785	2785
IV F-stat.	40.831	10.082	40.831	9.018	29.904	10.940

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses. Columns (1) and (2) show first stage results corresponding to model (2) in table 1; columns (3) and (4) to model (4) in table 1; and columns (5) and (6) to model (6) in table 1.

analyses substantiate the main point that small arms do not lead to *higher* conflict risk. Note that the negative coefficient of hostile MCW imports can be explained by price effects: when higher global demand, partly captured by `Hostile MCW imports`, raises global small arms prices, the overall correlation with `Hostile MCW imports` might be negative with sufficiently low price elasticity of demand. Additionally, note that exports do not have a distinct effect on onset separately from imports, as de Soysa et al. (2010) found for repression.

A.5.2 Probit results with instruments as independent variables

In addition to validity, table A6 indicates that exogeneity of the instruments is also given, as the instruments prove to not affect onset risk significantly when used as independent variables along our variables of interest.

A.6 Additional robustness tests

A.6.1 Alternative estimation methods: Conditional FE, linear probability and rare events models

Although Cook et al. (2020) provide convincing arguments that the PML-FE specification in this article should not be affected by the incidental parameter problem (see Lancaster (2000) for an overview), table A7 provides additional results of a conditional fixed effects logit model. These results corroborate the general direction and significance of the effects found in the PML-FE models. The conditional fixed effects logit model includes a country-specific intercept α_i in the linear predictor. As this country intercept would explain onset perfectly for countries without any onset coded over the observed time period, the conditional fixed effects models can only take countries into account that have at least one onset coded, leading to a lower number of observations used in the analysis and potential bias in the estimated coefficients (Cook et al. 2020).

Following the recommendation by Angrist and Pischke (2009), table A7 also presents results from a standard and an instrumental variable linear probability model, i.e. a simple linear OLS regression treating the dichotomous dependent variable onset like a continuous variable. Results are similar in sign and size in comparison to the marginal effects of (IV) probit models (see section A.3 in the appendix).

Because intrastate conflict onsets are a rare event, table A8 additionally presents results from a rare-events logit model (Firth 1993) that is also widely used in conflict research. It uses penalized maximum likelihood estimation to correct for potential biases due to low numbers of the rare event conflict outbreak. Results are similar in sign and size to other non-instrumented models but are only for the 5-year averaged variables

Table A6: Probit results with instruments included

	(1) Country SA, instr.	(2) SA p.c., instr.	(3) SA p.c., instr.
log(MCW imports), 5 yr. avg.	0.036	0.039	
	(0.027)	(0.028)	
L.log(MCW imports)			0.011
			(0.018)
log(SA imports), 5 yr. avg.	–0.012		
	(0.010)		
log(SA imp. per capita), 5 yr. avg.		–0.029	
		(0.020)	
L.log(SA imp. per capita)			–0.013
			(0.015)
log(Air force missiles), 5 yr. avg.	0.026	0.026	
	(0.027)	(0.027)	
L.log(Air force missiles)			0.031
			(0.021)
log(SA exports), 5 yr. avg.	–0.017	–0.018	
	(0.011)	(0.011)	
L.log(SA exports per capita)			–0.012
			(0.017)
L.log(Hostile MCW imports)			0.252^{+}
			(0.133)
log(Hostile MCW imports), 5 yr. avg.	0.177	0.187	
	(0.137)	(0.136)	
L.log(population)	0.183^{**}	0.168^{**}	0.152^{**}
	(0.049)	(0.050)	(0.043)
L.log(GDP p.c.)	–0.127	–0.118	$–0.170^{*}$
	(0.079)	(0.080)	(0.076)
LD.log(GDP p.c.)	1.041^{+}	1.048^{+}	1.081^{+}
	(0.550)	(0.550)	(0.571)
Instability	–0.196	–0.207	–0.216
	(0.587)	(0.586)	(0.590)
L.Polity IV	0.018	0.018^{+}	0.014
	(0.011)	(0.011)	(0.011)
L.Polity IV $\times$ L.Polity IV	$–0.004^{+}$	$–0.004^{+}$	$–0.004^{+}$
	(0.002)	(0.002)	(0.002)
L.Excluded population	0.257	0.252	0.317
	(0.240)	(0.240)	(0.234)
L.No. of nearby conflicts	0.044^{**}	0.044^{**}	0.047^{**}
	(0.014)	(0.014)	(0.014)
L.Interstate violence	0.096	0.091	0.148
	(0.218)	(0.217)	(0.229)
Years since last onset	$–0.123^{**}$	$–0.123^{**}$	$–0.131^{**}$
	(0.025)	(0.025)	(0.025)
Years since last onset, sq.	0.004^{**}	0.004^{**}	0.004^{**}
	(0.001)	(0.001)	(0.001)
Years since last onset, cubic	$–0.042^{**}$	$–0.042^{**}$	$–0.044^{**}$
	(0.014)	(0.014)	(0.014)
Constant	$–3.298^{*}$	$–3.266^{*}$	$–2.812^{*}$
	(1.481)	(1.478)	(1.313)
N	2806	2806	2785
Pseudo R^2	0.297	0.297	0.297

Note: * ($^{+}$, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses.

Table A7: Alternative estimation methods

	Conditional effects logit		Linear probability		IV Linear probability	
	(1)	(2)	(3)	(4)	(5)	(6)
log(MCW imports), 5 yr. avg.	0.170*	0.175*	0.006*	0.006*	0.013+	0.016+
	(0.085)	(0.087)	(0.003)	(0.003)	(0.007)	(0.009)
log(SA imports), 5 yr. avg.	–0.081*		–0.002+		–0.008	
	(0.039)		(0.001)		(0.005)	
log(SA imp. per capita), 5 yr. avg.		–0.150+		–0.003+		–0.019
		(0.082)		(0.002)		(0.013)
L.log(population)	0.071	–0.278	0.012*	0.010+	0.014+	0.002
	(1.237)	(1.210)	(0.006)	(0.005)	(0.007)	(0.009)
L.log(GDP p.c.)	0.288	0.290	–0.013	–0.013	0.003	0.007
	(0.514)	(0.523)	(0.009)	(0.009)	(0.019)	(0.023)
LD.log(GDP p.c.)	1.108	1.039	0.105*	0.103*	0.114*	0.108*
	(1.351)	(1.347)	(0.048)	(0.048)	(0.047)	(0.046)
Instability	–1.280	–1.274	–0.016	–0.017	–0.028	–0.031
	(1.403)	(1.391)	(0.051)	(0.051)	(0.047)	(0.049)
L.Polity IV	–0.032	–0.030	0.000	0.000	0.002	0.002
	(0.048)	(0.048)	(0.001)	(0.001)	(0.002)	(0.002)
L.Polity IV sq.	–0.013	–0.012	0.000	0.000	–0.000	–0.000
	(0.011)	(0.011)	(0.000)	(0.000)	(0.000)	(0.000)
L.Excluded population	–5.327**	–5.247**	0.022	0.023	0.016	0.015
	(1.602)	(1.594)	(0.028)	(0.028)	(0.034)	(0.036)
L.No. of nearby conflicts	0.014	0.018	0.004*	0.004*	0.003	0.003
	(0.068)	(0.068)	(0.002)	(0.002)	(0.002)	(0.002)
L.Interstate violence	0.850	0.836	0.020	0.020	0.012	0.011
	(0.583)	(0.580)	(0.052)	(0.052)	(0.074)	(0.075)
Years since last conflict	0.058	0.050	–0.016**	–0.016**	–0.014**	–0.014**
	(0.096)	(0.096)	(0.004)	(0.004)	(0.004)	(0.004)
Years since last conflict, sq.	–0.003	–0.003	0.000**	0.000**	0.000**	0.000**
	(0.006)	(0.006)	(0.000)	(0.000)	(0.000)	(0.000)
Years since last conflict, cubic	0.132	0.130	–0.004**	–0.004**	–0.003**	–0.003**
	(0.101)	(0.102)	(0.001)	(0.001)	(0.001)	(0.001)
Constant			0.126	0.148	–0.069	0.034
			(0.154)	(0.153)	(0.273)	(0.283)
N	747	747	2806	2806	2806	2806

Note: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses.

Table A8: Rare events logit results

	(1) Country SA	(2) SA p.c.	(3) SA p.c.
log(MCW imports), 5 yr. avg.	0.102*	0.107*	
	(0.047)	(0.047)	
L.log(MCW imports)			0.047
			(0.033)
log(SA imports), 5 yr. avg.	–0.032+		
	(0.019)		
log(SA imp. per capita), 5 yr. avg.		–0.070+	
		(0.040)	
L.log(SA imp. per capita)			–0.023
			(0.030)
L.log(population)	0.298**	0.257**	0.332**
	(0.088)	(0.089)	(0.079)
L.log(GDP p.c.)	–0.322*	–0.313*	–0.300*
	(0.141)	(0.142)	(0.131)
L.D.log(GDP p.c.)	1.743	1.715	1.705
	(1.063)	(1.056)	(1.076)
Instability	0.221	0.179	0.114
	(0.917)	(0.916)	(0.937)
L.Polity IV	0.015	0.015	0.008
	(0.021)	(0.021)	(0.020)
L.Polity IV (sq.)	–0.005	–0.005	–0.005
	(0.005)	(0.005)	(0.005)
L.Excluded population	0.523	0.515	0.653
	(0.461)	(0.460)	(0.455)
L.No. of nearby conflicts	0.110**	0.111**	0.114**
	(0.028)	(0.028)	(0.027)
L.Interstate violence	0.252	0.249	0.271
	(0.346)	(0.346)	(0.342)
Years since last onset	–0.279**	–0.280**	–0.295**
	(0.054)	(0.054)	(0.054)
Years since last onset, sq.	0.010**	0.010**	0.010**
	(0.003)	(0.003)	(0.003)
Years since last onset, cubic	–0.099**	–0.100**	–0.106**
	(0.033)	(0.033)	(0.034)
Constant	–2.747	–2.313	–3.455+
	(2.316)	(2.270)	(2.020)
N	2806	2806	2806
Pseudo R^2			

Note: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses.

significant at the 10 % level.

A.6.2 Alternative specification of small arms imports

Tables A9 and A10 replicate the main analyses of the article but use a broader definition of small arms: here, only sporting guns and shotgun cartridges, which are also mostly traded as a commodity for civilian use, are excluded. With this specification, small arms imports exhibit statistically significant risk-reducing effects in the IV probit and SP-D models, while the effect is not significant in standard probit and PML-FE models. Note that `population` had to be removed from the SP-D model's immunity equation to allow for convergence.

A.6.3 Alternative specification of onset

To further corroborate the robustness of the onset analyses, tables A11–A14 show results using different specifications of conflict onsets. Alternative specification 1 only requires one year of peace in a conflict before a new onset of this specific conflict is coded (two years of peace is used in the main analyses in the article). Tables A11 and A13 provide results for alternative specification 1 using probit, PML-FE and SP-D models, respectively. Alternative specification 2 includes ongoing conflict years without new onsets as zeros (instead of coding them as missing as in the main analyses in the article). Tables A12 and A14 provide results for alternative specification 2 using probit and PML-FE models, respectively. Note that the SP-D model in table 2 is already based on alternative specification 2, as it does not converge with ongoing conflicts coded as missing.

In general, all models indicate risk-reducing effects of small arms imports. Statistical significance varies between models, being stronger in some (e.g. standard probit models (1) and (3) in table A12) and weaker in others (e.g. IV models (4) and (6) in table A12 and models in table A11). All in all, different onset specifications do not alter the results substantially, also finding either a risk-reducing or no statistically significant effect of small arms imports.

A.6.4 Alternative specifications of Polity indicator

As noted in Vreeland (2008), the Polity IV indicator, although widely used in the literature and therefore also preferred in this article, uses conflict-related measurements to build up the indicator, potentially distorting conflict analyses. Therefore, table A15 provides results of (IV) probit models using the Scalar Index of Polities (Gates et al. 2006) instead. The effect of small arms imports is similar in direction and size, but with lower significance, corroborating the other models' results of small arms having no or even a risk-reducing effect on conflict risk.

Table A9: Probit and IV probit results, only sporting guns excluded

	(1) Country SA	(2) Country SA, instr.	(3) SA p.c.	(4) SA p.c., instr.	(5) SA p.c.	(6) SA p.c., instr.
log(MCW imports), 5 yr. avg.	0.049^{+}	0.080	0.049^{+}	0.094^{+}		
	(0.026)	(0.051)	(0.027)	(0.055)		
L.log(MCW imports)					0.022	0.104^{*}
					(0.018)	(0.051)
log(SA imports), 5 yr. avg.	–0.020	-0.093^{*}				
	(0.015)	(0.046)				
log(SA imp. per capita), 5 yr. avg.			–0.030	-0.130^{+}		
			(0.025)	(0.079)		
L.log(SA imp. per capita)					–0.008	-0.144^{+}
					(0.018)	(0.079)
L.log(population)	0.172^{**}	0.231^{**}	0.142^{**}	0.090	0.173^{**}	0.052
	(0.055)	(0.084)	(0.054)	(0.081)	(0.050)	(0.085)
L.log(GDP p.c.)	-0.158^{*}	–0.017	-0.155^{+}	–0.035	-0.155^{*}	–0.032
	(0.079)	(0.151)	(0.081)	(0.170)	(0.071)	(0.180)
LD.log(GDP p.c.)	0.957^{*}	1.153^{**}	0.935^{*}	1.049^{**}	0.933^{*}	0.893^{*}
	(0.380)	(0.413)	(0.375)	(0.398)	(0.385)	(0.407)
Instability	–0.176	–0.240	–0.179	–0.239	–0.184	–0.114
	(0.436)	(0.375)	(0.439)	(0.397)	(0.461)	(0.398)
L.Polity IV	0.010	0.026^{+}	0.010	0.023	0.007	0.027^{+}
	(0.014)	(0.014)	(0.014)	(0.016)	(0.014)	(0.016)
L.Polity IV × L.Polity IV	–0.004	-0.005^{*}	–0.004	-0.004^{+}	–0.003	-0.004^{+}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
L.Excluded population	0.277	0.347^{+}	0.278	0.342^{+}	0.322^{+}	0.308
	(0.190)	(0.194)	(0.190)	(0.207)	(0.187)	(0.200)
L.No. of nearby conflicts	0.053^{**}	0.042^{*}	0.053^{**}	0.042^{*}	0.055^{**}	0.040^{*}
	(0.015)	(0.018)	(0.015)	(0.019)	(0.016)	(0.020)
L.Interstate violence	0.074	–0.103	0.090	–0.022	0.112	–0.055
	(0.200)	(0.230)	(0.200)	(0.224)	(0.201)	(0.268)
Years since last onset	-0.127^{**}	-0.100^{**}	-0.128^{**}	-0.106^{**}	-0.134^{**}	-0.101^{**}
	(0.031)	(0.032)	(0.032)	(0.032)	(0.034)	(0.033)
Years since last onset, sq.	0.004^{**}	0.003^{*}	0.004^{**}	0.004^{*}	0.005^{**}	0.003^{*}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years since last onset, cubic	-0.044^{*}	-0.036^{*}	-0.044^{*}	-0.037^{*}	-0.046^{*}	-0.035^{*}
	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)	(0.017)
N	2806	2806	2806	2806	2806	2785
Pseudo R^2	0.291		0.290		0.286	

Notes: * ($^{+}$, **) indicates $p < .05$ ($.1$, $.01$). Standard errors in parentheses. Models replicate table 1, but use a broader definition of small arms.

Table A10: PML-FE and SP-D results, only sporting guns excluded

	PML-FE	*PML-FE*	*SP-D*	
	Country SA	SA p.c.	Immunity eq.	Duration eq.
	(1)	(2)	(3)	
log(MCW imports), 5 yr. avg.	0.246**	0.249**		−0.100**
	(0.067)	(0.067)		(0.037)
log(SA imports), 5 yr. avg.	−0.054+			0.042*
	(0.028)			(0.021)
log(SA imp. per capita), 5 yr. avg.		−0.085		
		(0.055)		
L.Excluded population	−0.257	−0.222	0.911	
	(0.832)	(0.829)	(1.984)	
L.Interstate violence	0.406	0.437		−0.425**
	(0.483)	(0.478)		(0.126)
L.log(GDP p.c.)	−0.829**	−0.826**	1.680**	0.613**
	(0.298)	(0.308)	(0.639)	(0.139)
LD.log(GDP p.c.)	1.734+	1.731+		−1.311
	(0.966)	(0.967)		(1.023)
L.log(population)	1.631**	1.531**		−0.288**
	(0.351)	(0.349)		(0.075)
Instability	−0.331	−0.304		0.057
	(1.020)	(1.012)		(0.965)
L.Polity IV	−0.043	−0.043	0.402+	0.012
	(0.031)	(0.031)	(0.237)	(0.024)
L.Polity IV_squared	−0.011	−0.010	0.039	0.006
	(0.007)	(0.007)	(0.051)	(0.005)
L.No. of nearby conflicts	0.061	0.064		−0.087**
	(0.049)	(0.049)		(0.022)
Years since last onset	−0.124*	−0.129*		
	(0.062)	(0.062)		
Years since last onset_sq	0.006*	0.006*		
	(0.003)	(0.003)		
Years since last onset_cubic	−0.079*	−0.080*		
	(0.036)	(0.036)		
Constant	−17.578**	−16.412**	−18.374*	−0.161
	(6.236)	(6.135)	(7.228)	(2.138)
log(α)			−0.132+	
			(0.078)	
Observations	2,907	2,907	3348	
Akaike Inf. Crit.	703.009	704.175	851.973	

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses.
Models replicate table 2, but use a broader definition of small arms.

Table A11: Probit and IV probit results with alternative onset specification 1

	(1) Country SA	(2) Country SA, instr.	(3) SA p.c.	(4) SA p.c., instr.	(5) SA p.c.	(6) SA p.c., instr.
log(MCW imports), 5 yr. avg.	0.055*	0.106+	0.057*	0.124+		
	(0.023)	(0.057)	(0.022)	(0.069)		
L.log(MCW imports)					0.028+	0.120+
					(0.016)	(0.064)
log(SA imports), 5 yr. avg.	–0.011	–0.069+				
	(0.010)	(0.042)				
log(SA imp. per capita), 5 yr. avg.			–0.026	–0.141		
			(0.021)	(0.101)		
L.log(SA imp. per capita)					–0.009	–0.094
					(0.015)	(0.091)
L.log(population)	0.150**	0.171*	0.136**	0.075	0.168**	0.054
	(0.053)	(0.068)	(0.050)	(0.092)	(0.048)	(0.098)
L.log(GDP p.c.)	–0.150+	0.009	–0.142+	0.014	–0.129+	–0.090
	(0.077)	(0.153)	(0.076)	(0.184)	(0.067)	(0.180)
LD.log(GDP p.c.)	0.912*	0.955*	0.902*	0.891*	0.910*	0.719+
	(0.379)	(0.374)	(0.377)	(0.375)	(0.390)	(0.426)
Instability	–0.409	–0.510	–0.419	–0.514	–0.436	–0.350
	(0.409)	(0.351)	(0.410)	(0.356)	(0.435)	(0.423)
L.Polity IV	0.011	0.025	0.011	0.022	0.008	0.018
	(0.013)	(0.015)	(0.013)	(0.016)	(0.013)	(0.015)
L.Polity IV $\times$ L.Polity IV	–0.006*	–0.007**	–0.006*	–0.007**	–0.005*	–0.006**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
L.Excluded population	0.142	0.088	0.139	0.094	0.204	0.065
	(0.206)	(0.211)	(0.206)	(0.214)	(0.202)	(0.193)
L.No. of nearby conflicts	0.043**	0.032+	0.043**	0.033+	0.046**	0.033+
	(0.014)	(0.018)	(0.014)	(0.019)	(0.014)	(0.019)
L.Interstate violence	0.156	0.083	0.153	0.077	0.147	0.091
	(0.213)	(0.206)	(0.213)	(0.211)	(0.207)	(0.217)
Years since last onset	–0.173**	–0.153**	–0.173**	–0.152**	–0.180**	–0.155**
	(0.030)	(0.033)	(0.030)	(0.037)	(0.031)	(0.041)
Years since last onset, sq.	0.006**	0.005**	0.006**	0.005**	0.006**	0.005**
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Years since last onset, cubic	–0.059**	–0.053**	–0.059**	–0.052*	–0.062**	–0.053*
	(0.019)	(0.019)	(0.019)	(0.020)	(0.019)	(0.021)
N	2841	2841	2841	2841	2841	2820
Pseudo R^2	0.340		0.340		0.337	

Notes: * (+, **) indicates $p < .05$ $(.1, .01)$. Standard errors in parentheses. Models replicate table 1, but use a broader definition of intrastate conflict onset.

Table A12: Probit and IV probit results with alternative onset specification 2

	(1) Country SA	(2) Country SA, instr.	(3) SA p.c.	(4) SA p.c., instr.	(5) SA p.c.	(6) SA p.c., instr.
log(MCW imports), 5 yr. avg.	0.050*	0.123*	0.050*	0.140*		
	(0.021)	(0.050)	(0.021)	(0.058)		
L.log(MCW imports)					0.019	0.137**
					(0.015)	(0.046)
log(SA imports), 5 yr. avg.	–0.025**	–0.083*				
	(0.009)	(0.038)				
log(SA imp. per capita), 5 yr. avg.			–0.045**	–0.152+		
			(0.017)	(0.092)		
L.log(SA imp. per capita)					–0.019	–0.126+
					(0.013)	(0.070)
L.log(population)	0.135**	0.132+	0.104**	0.014	0.138**	–0.007
	(0.042)	(0.071)	(0.039)	(0.075)	(0.034)	(0.070)
L.log(GDP p.c.)	–0.150*	–0.037	–0.153*	–0.062	–0.156**	–0.108
	(0.066)	(0.151)	(0.065)	(0.186)	(0.058)	(0.167)
LD.log(GDP p.c.)	0.758*	0.809*	0.725*	0.711*	0.762*	0.519+
	(0.337)	(0.330)	(0.335)	(0.321)	(0.350)	(0.311)
Instability	0.005	–0.043	–0.020	–0.097	0.004	0.119
	(0.466)	(0.427)	(0.469)	(0.430)	(0.475)	(0.427)
L.Polity IV	0.007	0.023+	0.005	0.017	0.002	0.016
	(0.012)	(0.013)	(0.013)	(0.014)	(0.013)	(0.012)
L.Polity IV $\times$ L.Polity IV	–0.002	–0.004+	–0.002	–0.003	–0.002	–0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
L.Excluded population	0.037	–0.037	0.053	–0.002	0.103	–0.044
	(0.183)	(0.180)	(0.183)	(0.184)	(0.186)	(0.165)
L.No. of nearby conflicts	0.037*	0.025	0.037*	0.026	0.040**	0.026
	(0.015)	(0.017)	(0.015)	(0.018)	(0.015)	(0.017)
L.Interstate violence	–0.125	–0.161	–0.117	–0.142	–0.123	–0.204+
	(0.127)	(0.121)	(0.128)	(0.123)	(0.127)	(0.117)
Years since last onset	–0.066**	–0.049*	–0.066**	–0.048+	–0.072**	–0.047+
	(0.025)	(0.025)	(0.025)	(0.028)	(0.026)	(0.027)
Years since last onset, sq.	0.002*	0.002	0.002*	0.002	0.002*	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years since last onset, cubic	–0.023+	–0.018	–0.023+	–0.018	–0.025*	–0.017
	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.012)
N	3237	3237	3237	3237	3237	3214
Pseudo R^2	0.181		0.180		0.173	

Notes: * (+, **) indicates $p < .05$ ($.1$, $.01$). Standard errors in parentheses. Models replicate table 1, but use a broader definition of intrastate conflict onset, not coding ongoing conflicts as 0.

Table A13: PML-FE and SP-D results with alternative onset specification 1

	PML-FE	*PML-FE*	*SP-D*	
	Country SA	SA p.c.	Immunity eq.	Duration eq.
	(1)	(2)	(3)	
log(MCW imports), 5 yr. avg.	0.218**	0.222**		−0.123**
	(0.061)	(0.062)		(0.035)
log(SA imports), 5 yr. avg.	−0.052**			0.038**
	(0.026)			(0.015)
log(SA imp. per capita), 5 yr. avg.		−0.103+		
		(0.057)		
L.Excluded population	−0.133	−0.158	6.154	
	(0.810)	(0.809)	(5.866)	
L.Interstate violence	0.345	0.356		0.074
	(0.406)	(0.405)		(0.206)
L.log(GDP p.c.)	−0.651*	−0.660*	−1.051+	0.309**
	(0.279)	(0.282)	(0.554)	(0.112)
LD.log(GDP p.c.)	1.603+	1.586+		−1.374
	(0.854)	(0.852)		(0.845)
L.log(population)	1.641**	1.546**	1.429**	−0.129+
	(0.327)	(0.326)	(0.554)	(0.075)
Instability	−0.573	−0.593		0.266
	(0.974)	(0.977)		(0.978)
L.Polity IV	−0.024	−0.027	−0.084	−0.036*
	(0.030)	(0.030)	(0.077)	(0.018)
L.Polity IV_squared	−0.015*	−0.014*	−0.106**	−0.001
	(0.006)	(0.006)	(0.040)	(0.004)
L.No. of nearby conflicts	0.047	0.048		−0.082**
	(0.047)	(0.047)		(0.020)
Years since last onset	−0.190**	−0.198**		
	(0.059)	(0.059)		
Years since last onset_sq	0.009**	0.009**		
	(0.003)	(0.003)		
Years since last onset_cubic	−0.104**	−0.108**		
	(0.036)	(0.037)		
Constant	−19.320**	−17.900**	1.350	1.760
	(5.954)	(5.800)	(7.778)	(1.849)
log(α)			−0.031	
			(0.068)	
Observations	2,942	2,942	3348	
Akaike Inf. Crit.	798.399	798.979	1019.557	

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses.
Models replicate table 2, but use a broader definition of intrastate conflict onset.

Table A14: PML-FE results with alternative onset specification 2

	Dependent variable:	
	Onset	
	(1)	(2)
log(MCW imports), 5 yr. avg.	0.124^{+}	0.128^{+}
	(0.066)	(0.067)
log(SA imports), 5 yr. avg.	-0.089^{**}	
	(0.029)	
log(SA imp. per capita), 5 yr. avg.		-0.151^{*}
		(0.061)
L.Excluded population	-3.116^{**}	-3.087^{**}
	(1.080)	(1.074)
L.Interstate violence	−0.139	−0.140
	(0.245)	(0.246)
L.log(GDP p.c.)	0.235	0.213
	(0.360)	(0.358)
LD.log(GDP p.c.)	0.636	0.572
	(1.005)	(1.001)
L.log(population)	−0.357	−0.801
	(0.695)	(0.671)
Instability	−0.240	−0.277
	(0.889)	(0.884)
L.Polity IV	−0.028	−0.027
	(0.038)	(0.037)
L.Polity IV_squared	−0.007	−0.007
	(0.008)	(0.008)
L.No. of nearby conflicts	0.026	0.035
	(0.055)	(0.055)
Years since last onset	0.072	0.069
	(0.073)	(0.073)
Years since last onset_sq	−0.002	−0.002
	(0.004)	(0.004)
Years since last onset_cubic	0.087	0.084
	(0.066)	(0.066)
Constant	-25.143^{*}	−18.615
	(11.959)	(11.504)
Observations	3,325	3,325
Akaike Inf. Crit.	726.716	730.168
Note:	* ($^{+}$, **) indicates $p < .05$ (.1, .01)	

Table A15: Alternative specifications with SIP

	(1) Country SA	(2) Country SA, instr.	(3) SA p.c.	(4) SA p.c., instr.	(5) SA p.c.	(6) SA p.c., instr.
log(MCW imports), 5 yr. avg.	0.063*	0.114+	0.066*	0.134+		
	(0.027)	(0.063)	(0.026)	(0.076)		
L.log(MCW imports)					0.026	0.132*
					(0.019)	(0.063)
log(SA imports), 5 yr. avg.	–0.018+	–0.077+				
	(0.010)	(0.046)				
log(SA imp. per capita), 5 yr. avg.			–0.039+	–0.148		
			(0.021)	(0.112)		
L.log(SA imp. per capita)					–0.017	–0.129
					(0.016)	(0.087)
L.log(population)	0.140*	0.159+	0.118*	0.052	0.162**	0.025
	(0.059)	(0.082)	(0.055)	(0.103)	(0.051)	(0.104)
L.log(GDP p.c.)	–0.205*	–0.053	–0.196*	–0.061	–0.173*	–0.096
	(0.083)	(0.168)	(0.084)	(0.206)	(0.071)	(0.188)
LD.log(GDP p.c.)	0.915*	0.970*	0.901*	0.909*	0.922*	0.663
	(0.445)	(0.443)	(0.441)	(0.436)	(0.452)	(0.431)
Instability	0.285	0.230	0.265	0.216	0.167	0.003
	(0.575)	(0.445)	(0.580)	(0.475)	(0.601)	(0.458)
L.SIP	1.212	1.966+	1.223	1.899+	1.008	1.541
	(1.141)	(1.110)	(1.140)	(1.146)	(1.154)	(1.094)
L.SIP × L.SIP	–0.929	–1.409	–0.947	–1.419	–0.783	–1.073
	(1.012)	(1.002)	(1.004)	(1.010)	(1.016)	(0.979)
L.Excluded population	0.272	0.244	0.267	0.233	0.339+	0.163
	(0.202)	(0.245)	(0.204)	(0.250)	(0.199)	(0.244)
L.No. of nearby conflicts	0.054**	0.043+	0.055**	0.044+	0.059**	0.042+
	(0.017)	(0.023)	(0.017)	(0.025)	(0.017)	(0.025)
L.Interstate violence	0.071	0.010	0.069	0.011	0.072	0.032
	(0.185)	(0.190)	(0.185)	(0.194)	(0.184)	(0.235)
Years since last onset	–0.131**	–0.113**	–0.131**	–0.114**	–0.139**	–0.109**
	(0.035)	(0.034)	(0.035)	(0.036)	(0.036)	(0.039)
Years since last onset, sq.	0.004**	0.004*	0.004**	0.004*	0.005**	0.004*
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Years since last onset, cubic	–0.044*	–0.038*	–0.045*	–0.038*	–0.047*	–0.036+
	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)
N	2754	2754	2754	2754	2754	2736
Pseudo R^2	0.296		0.296		0.290	

Notes: * (+, **) indicates $p < .05$ (.1, .01). Standard errors in parentheses. Models replicate table 1, but use SIP instead of the Polity score.

A.7 Repression: instrumental variable models

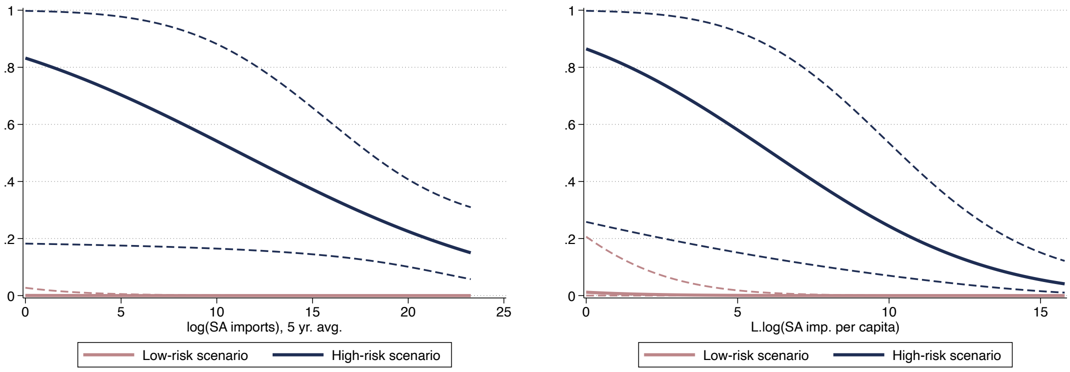
As argued in section 6.2, endogeneity of arms imports should be of no concern for repression analyses. To corroborate this argument, table A16 applies the instrumental-variable approach used for onset analyses to the repression models of table 3. Results for small arms imports remain statistically insignificant and of small size. However, major arms imports also do not exhibit statistically significant effects. The conclusion stands that results on arms imports' effects on repression depend heavily on modeling and specification choices.

Table A16: Repression: instrumental variable models

	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE
log(MCW imports), 5 yr. avg.	0.003	0.003	–0.036	–0.042	–0.003	–0.003
	(0.004)	(0.004)	(0.029)	(0.029)	(0.010)	(0.011)
log(SA imports), 5 yr. avg.	–0.001		–0.010		–0.003	
	(0.002)		(0.013)		(0.004)	
log(SA imp. per capita), 5 yr. avg.		–0.001		–0.012		–0.005
		(0.004)		(0.022)		(0.008)
L.Repression	0.972**	0.972**			0.874**	0.875**
	(0.006)	(0.006)			(0.011)	(0.011)
L.log(population)	–0.015**	–0.016**	–0.190	–0.177	–0.200**	–0.202**
	(0.005)	(0.005)	(0.247)	(0.250)	(0.054)	(0.057)
L.log(GDP p.c.)	0.010	0.010	0.233+	0.230+	–0.025	–0.026
	(0.009)	(0.009)	(0.123)	(0.122)	(0.025)	(0.025)
L.D.log(GDP p.c.)	0.066	0.066	–0.238	–0.242	0.011	0.010
	(0.061)	(0.061)	(0.156)	(0.156)	(0.060)	(0.060)
Instability	–0.060	–0.060	–0.095	–0.099	–0.062	–0.061
	(0.060)	(0.059)	(0.161)	(0.161)	(0.052)	(0.052)
L.Polity IV	–0.001	–0.001	0.018	0.019	–0.003	–0.003
	(0.002)	(0.002)	(0.019)	(0.019)	(0.004)	(0.004)
L.Polity IV (sq.)	–0.000	–0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)	(0.000)
L.Excluded population	0.004	0.004	0.014	0.017	–0.022	–0.022
	(0.022)	(0.022)	(0.214)	(0.216)	(0.076)	(0.076)
L.No. of nearby conflicts	–0.003**	–0.003**	–0.009	–0.009	–0.001	–0.001
	(0.001)	(0.001)	(0.010)	(0.010)	(0.003)	(0.003)
L.Interstate violence	0.017	0.017	0.032	0.030	0.004	0.004
	(0.039)	(0.040)	(0.085)	(0.083)	(0.010)	(0.010)
Years since last onset	–0.007**	–0.007**	0.028*	0.029*	–0.007*	–0.006*
	(0.002)	(0.002)	(0.013)	(0.013)	(0.003)	(0.003)
Years since last onset, sq.	0.000*	0.000*	–0.001+	–0.001+	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years since last onset, cubic	–0.001+	–0.001+	0.007	0.007	–0.001	–0.001
	(0.001)	(0.001)	(0.005)	(0.005)	(0.001)	(0.001)
L.Executive constraints	0.014*	0.014*	0.029	0.023	0.012	0.012
	(0.007)	(0.007)	(0.053)	(0.053)	(0.014)	(0.014)
Constant	0.130	0.142				
	(0.164)	(0.158)				
N	2671	2671	2668	2668	2668	2668
R^2 (within for FE models)	0.986	0.986	0.139	0.138	0.854	0.854

Note: * (+, **) indicates $p < .05$ (.1, .01). Higher values of the dependent variable indicate lower levels of repression.

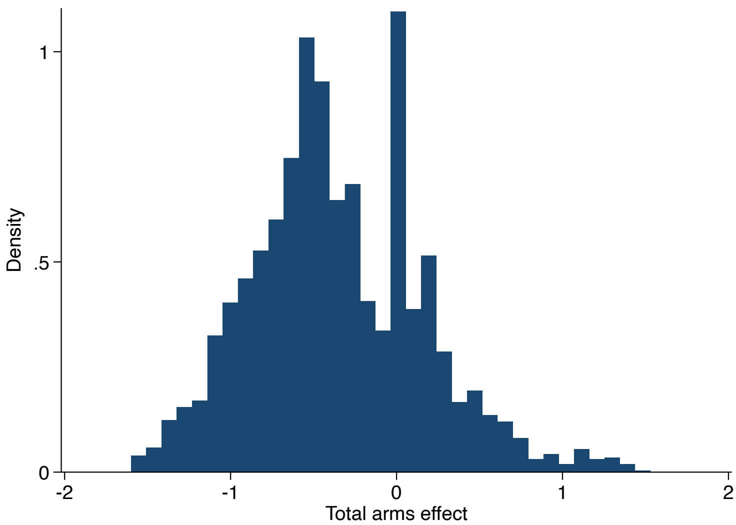
Figure A1: Different risk scenarios



Predicted probability of intrastate conflict onset for range of observed values of small arms imports (`log(SA imports, 5 yr. avg.)`). Confidence bands shown as dashed lines.

Predicted probability of intrastate conflict onset for range of observed values of small arms imports (`L.log(SA imports)`). Confidence bands shown as dashed lines.

Figure A2: Distribution of sum of SA and MCW effects



Histogram of total arms ($\beta_{SA} \times$ `SA imports` $+\beta_{MCW} \times$ `MCW imports`) effects for country-years in sample (using `log(SA imports, 5 yr. avg.)` in the IV probit model)

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Chapter 3

Duration of intrastate wars of attrition: The causal impact of military build-ups

Article, co-authored with Paul W. Thurner, not yet published, with an addendum on a "casualties-as-time" analysis approach to the duration-intensity nexus. Paul W. Thurner contributed core ideas and advice, especially on the theoretical framework and methodological issues, and revised the manuscript.

Replication material: www.andreas-mehltretter.de/thesis-replication

Abstract

Scientific knowledge on the effectiveness of governmental military build-ups in terminating intrastate conflicts is sparse and inconclusive. Adopting a war-of-attrition game-theoretic framework, we derive consistent hypotheses for the relevant factors explaining conflict duration variability. Focusing on governments' armaments, as reflected in inflows of major conventional weapons, the model predicts that arming enables the government to inflict higher costs onto its adversaries. This forces them to withdraw earlier from the conflict when the government is the stronger party. Major weapons are required in particular to project military power over larger distances and fight relatively weaker rebels in remote areas. Therefore, armament's effects are expected to co-depend on geographical distance. An instrumental variable Cox survival approach ensures causal identification. Using SIPRI arms transfer data for the first time in a dyadic design, we empirically corroborate the formal model's predictions.

1 Introduction

Although military capabilities are part of most theoretical rationales for explaining the duration of civil conflicts, only a few studies explicitly focus on their role in conflict persistence either in theory building or empirical applications, apart from abstract notions of relative military "strength" (see e.g. Cunningham et al. 2009 for a notable exception). Kalyvas and Balcells (2010) present convincing arguments on how different configurations of relative military capabilities shape conflicts, and Balcells and Kalyvas (2014) examine their effects on intensity and duration. They find that conflicts with asymmetric distributions of capabilities, i.e. "irregular" conflicts, last longer than symmetric non-conventional and conventionally fought ones.

However, inferential evidence on the consequences of governments' specific armaments for the duration of intrastate conflicts is very limited. Moore (2012) indicates that imports of major conventional weapons (MCW) are associated with prolonged conflicts. In contrast, Caverley and Sechser (2017) find the opposite effect: focusing on so-called mechanization as the ratio of military equipment to troops, they reveal that combined air and ground mechanization reduces the length of conflict spells. Regarding US weapons sales, Magesan and Swee (2018) do not find any effects. Thus, we have inconclusive, partly contradictory findings raising a puzzle on a fundamental and practically relevant question: are governmental military build-ups effective in terminating militarized intrastate conflicts?

Our contribution comprises three particular innovations: first, we adopt Powell's (2017) war-of-attrition model to derive consistent hypotheses on governmental military build-ups' effects on the duration of government-rebel conflict dyads. Second, we provide the first dyadic empirical analysis of these effects thoroughly based on a formal framework, using arms imports as a valid proxy for such armament. The dyadic empirical design overcomes the highly aggregated country-year or conflict-year perspectives, allowing us to follow the theoretical micro-perspective as closely as possible. Third, we propose a solution to potential problems of demand endogeneity. Governments presumably raise their imports of military equipment when they expect a conflict to last longer to be prepared for protracted fighting. Such demand, induced by an anticipated longer duration, potentially leads to biased estimations of the armament's impact on conflict duration. Using a two-stage residual-inclusion instrumental variable (IV) strategy for survival models, it is possible to account for this endogeneity, ensuring the causality of the results.

The empirical literature on the effects of armament and military strength on the duration of internal conflict has not related to coherent formal frameworks so far. To overcome the peril of inconsistent arguments, we make use of Powell's (2017) formal war-of-attrition game: the government and each rebel group decide how long to fight in a conflict, and whenever one party quits, the conflict ends, and the other party wins. The decision on how long to fight depends on the valuation of winning and on the costs that arise when fighting, e.g. due to casualties. Using this framework, we arrive at the theoretically remarkable prediction that not symmetric, but particularly asymmetric configurations of military capabilities lead to shorter civil conflicts, reviving older results by Bennett and Stam (1996) on interstate war duration.

Based on this formal framework, we argue that build-ups of military capability due to arms transfers enable the government to inflict higher costs on the rebels. When the rebels are the weaker party, these additional costs cause the rebels to end the conflict earlier. In practice, major weapons are effective arms not only in large-scale battles, but also in asymmetric settings, e.g. when aircraft allow to target rebels in difficult-to-access areas (cf. Corum and Johnson 2003; Caverley and Sechser 2017). Specifically when the conflict is located at a larger distance to the government stronghold, major weapons like transport helicopters or armored vehicles contribute significantly to the government's ability to project power over geographic distance (cf. Read 2010; Johnson 2017). Our empirical analyses demonstrate these mechanisms to be effective: we find that higher imports of major weapons lead to shorter conflict durations, but only when the conflicts are located in more remote areas and the power distribution favors the government.

2 Insights on the relationship between armament and duration of intrastate conflicts

The opponents' military capabilities are essential determinants for how conflicts are fought and whether they can be terminated sooner or sustained longer (cf. Balcells and Kalyvas 2014). While research on the government's military endowment affecting conflict outbreak and intensity gained traction again recently (cf. e.g. Pamp et al. 2018; Magesan and Swee 2018; Mehrl and Thurner 2020; Boutton 2021; Mehltretter 2022; Gallea 2023) only a few studies have empirically investigated its impact on conflict duration. Sislin and Pearson (2001) find that total imports are usually higher in prolonged conflicts. However, they acknowledge that the endogenous demand for arms might drive this correlation. Focusing exclusively on imports of major conventional weapons, Moore (2012) finds that increased volumes of major weapons imports by the government are associated with longer durations. Note that Moore only uses one observation per conflict, not allowing for variations over time in the covariates and not discerning between different non-state adversaries involved in the same conflict.[1] Magesan and Swee (2018) use a logit model to capture US weapons sales' effects on

[1] Additionally, the study's reliance on SIPRI's data for measuring rebels' military power is highly questionable, as noted below.

the probability of conflict “offset”, finding no statistically significant effect.

While not directly investigating the temporal extent of conflicts, Lyall and Wilson (2009) argue that increased ground mechanization, i.e. the ratio of military equipment to troop size, leads to less direct contact with civilians and combatants, thereby losing access to valuable intelligence. Consequently, mechanization might have a prolonging effect because governments would be less capable of forcing an end to the conflict. Caverley and Sechser (2017) reach strikingly different conclusions with their focus on the mechanization of government forces, distinguishing between “ground” and “air” mechanization. They find that especially combined (i.e. interacted) mechanization leads to shorter conflicts, making ground and air mechanization strong complements.

Not only do the military capabilities of governments shape conflict dynamics, but so do the capabilities of their adversaries. Kalyvas and Balcells (2010) and Lockyer (2010) argue that rebels’ specific warfare strategy is the direct consequence of the configuration of military power in a conflict: strong rebels and strong states fight conventional civil wars, weak rebels facing strong states employ guerrilla tactics, and when states and rebels both have limited capabilities, “symmetric non-conventional” warfare emerges (Kalyvas and Balcells 2010: 418). In turn, the type of warfare affects the duration of conflicts, as conventionally fought conflicts are assumed to bring forth faster resolution, while guerrilla and symmetric non-conventional tactics allow rebels to evade decisive battles and to survive longer (Balcells and Kalyvas 2014).

In line with these arguments, Cunningham et al. (2009) suggest that greater rebel strength leads to shorter conflicts due to a higher capacity to target the government. Using the Non-State Actors dataset (Cunningham et al. 2013), they find empirical evidence that “rebels at parity” with and “rebels stronger” than the government fight shorter conflicts. These results are corroborated in other studies using these data, e.g. Buhaug et al. (2009) and Wucherpfennig et al. (2012). Employing a different measure for the relative capabilities of rebels based on the troop ratio of government and rebels, Hultquist (2013) shows that more symmetric capabilities distributions increase the probability of ceasefires or settlements. However, he finds no statistically significant relationship for government and rebel victories. Caverley and Sechser (2017) employ the Non-State Actors dataset in their analyses and do not encounter significant effects of their rebel strength variables.

Only Moore (2012) measures the rebels’ military capabilities more directly by using major conventional weapons transfers to rebels, for which SIPRI provides some data. However, this approach is highly problematic because SIPRI only records a minimal number of those transfers to a very limited number of groups. Probably as a consequence, Moore’s analysis exhibits no statistically significant effect of rebels’ major weapons imports.

Rebels' military capabilities might also depend on the provision of external support. In general, interventions and military support are found to prolong conflicts due to complicating bargaining processes and enabling rebels to pursue more enduring battles (Regan 2002; Balch-Lindsay and Enterline 2000; Brandt et al. 2008; Cunningham 2010; Cederman et al. 2013). However, Collier et al. (2004) find the opposite, namely a conflict-shortening effect of external support, whereas in Moore (2012) and Caverley and Sechser (2017), military assistance does not affect conflict duration. Using more granular data discerning between different types of support, Sawyer et al. (2017) show that more fungible support for rebels, like funding and weapons, prolongs conflicts, while direct intervention by sending troops leads to shorter conflicts.

In sum, existing research surprisingly provides highly contradictory results on the effect of governmental military capabilities: some studies find them to be associated with longer durations (Sislin and Pearson 2001; Moore 2012) and some with shorter durations (Caverley and Sechser 2017). None of these analyses deals with the potential issue of endogenous demand for arms in more protracted conflicts, thus disregarding the possibility of reverse causation.

3 A consistent framework for analyzing civil conflict duration

3.1 The war-of-attrition game

The empirical research has not made stringent use of formal models of conflict duration, with few notable exceptions like Fearon (2004) and Buhaug et al. (2009). Buhaug et al. (ibid.) base their theory partly on the contest success function model (Tullock 1980; Hirshleifer 1988), which they repurpose for theoretical examinations on the duration of fighting. Hirshleifer (2000) integrates the contest success function into a model where the government and the rebel group decide on their economic production and fighting investments. Due to decreasing marginal returns, weak rebel groups have a distinctly high incentive to invest in fighting. In an extension of this logic, Buhaug et al. (2009) expect weaker rebels to fight more prolonged conflicts, but the formal model does not explicitly incorporate other factors like geography.

To provide a stringent formal framework, we adopt a war-of-attrition model introduced by Powell (2017) and apply its logic to armament and civil conflict duration. As the model directly relates input factors to the length of the conflict, it is tailor-made for the given context and allows for explicit predictions of how military capabilities and other conflict characteristics translate into the continuation or termination of fighting. In the model, the government fights against rebels until one side decides to quit. Each side chooses the right time to quit depending on costs that occur while fighting, among other things casualties and material destructions, and the valuation they attribute to winning the conflict.

The "war of attrition" modeled here does not relate to specific conflicts typified by attritional warfare, where wearing down the adversary's military equipment and personnel is the primary strategy to win the conflict, in contrast to seeking a quick, decisive battle. It instead refers to the game-theoretic concept (cf. e.g. Maynard Smith 1974; Bishop and Cannings 1978; Fudenberg and Tirole 1986) formalizing the war of attrition's characteristic constant comparison of costs that both conflict parties have to bear, thus abstracting from the specific form of warfare. Accordingly, the model provides a general framework for all conflicts where two players are rationally determining how long their expected utility from potentially winning outweighs the costs of continuing fighting—which is a reasonable assumption for all types of intrastate conflict. Langlois and Langlois (2009) employ an attrition model for analyzing interstate conflict duration, while Crisman-Cox (2022), for instance, has shown the value of a war-of-attrition model derived from Abreu and Gul (2000) for civil conflict analysis, albeit in a different context of reputation and resolve. Zeng's (2020) application of a war-of-attrition model is more similar to our framework, but is implemented in the context of interstate conflicts.

In Powell's (2017) model that we base our framework on, the two parties $i \in \{g, r\}$, government and rebels, are engaged in a conflict lasting for some time t. By definition, the conflict is lost by the side that quits first. Both have distinct valuations for winning the conflict w_i. While the game and other parameters are common knowledge, the valuation of winning is private information to each side. The respective opponent assumes it to be distributed according to $G_i(w_i)$. Fighting is costly due to the expenses of fighting itself, destruction and human losses. Both parties thus have to take their respective costs c_i, amounting to total costs $c_i t$ throughout the conflict, into account for their decision when to quit.

Note that Powell (2017) develops this model as a Bayesian game. In the following, we deviate and simplify the model by assuming that both sides have to choose the quitting time at the start of the game according to their valuations of winning and costs. We are thus collapsing the game to only one decision point, with both sides always fighting deterministically until reaching their ex-ante-determined optimal quitting time. The valuations of winning are thereby not entering the model as random variables, but as exogenously given parameters. This simplification allows for a more straightforward way of deriving hypotheses and additional, intuitive reasoning about the effects of different valuations of winning. Section A.3 in the appendix shows that the hypotheses regarding the effects of changes in the cost ratio or total costs can as well be derived from the Bayesian original model by Powell (ibid.). The theoretical predictions do thus not hinge on our specific version of the framework. Notably, the model does not incorporate the possibility of negotiated settlements, as Powell (ibid.: 224) notes as a limitation. In section 4.4 of chapter 1, I discuss a framework (Powell 2012) that additionally allows for negotiated settlements.

To determine the duration of the conflict, we derive the optimal quitting times for both parties, $\sigma_i^*(w_i)$, which depend on their valuations of winning. Intuitively, if winning, i.e. not quitting, the conflict is more valuable, higher costs will be endured for a longer time, leading to a protracted conflict. Each player quits the conflict when their marginal costs exceed the expected marginal gains. The marginal gain will be the valuation of winning, but only when the adversary quits in the next instance. The expected marginal gain is thus w_i times the probability the adversary will quit in the next instance, given they have not quit earlier. Over time, the marginal gain decreases, as the expected value for the adversary's valuation rises: when time passes, the other side will drop out of the conflict if it has a lower valuation. Thus, only when the government and the rebel group have a higher valuation will they keep fighting. Essentially, the conflict occurs because the valuations are private information, and only fighting reveals the adversary's valuation.

Maximizing the expected utility gives the equilibrium solution for each party's strategy, taking the opponent's strategic actions into account.[2] $\sigma_i^*(w_i)$ then denotes the optimal time to quit fighting:

$$\sigma_g^*(w_g) = \frac{\overline{w}_g \overline{w}_r}{c_g + c_r} \left(\frac{w_g}{\overline{w}_g} \right)^{1+c_g/c_r} \tag{1}$$

$$\sigma_r^*(w_r) = \frac{\overline{w}_r \overline{w}_g}{c_g + c_r} \left(\frac{w_r}{\overline{w}_r} \right)^{1+c_r/c_g} \tag{2}$$

Note that in our simpler version of the model, only the time of the side that quits *first* is relevant for determining the total duration of the conflict. Thus, there are two cases: either $\sigma_g^*(w_g) \geq \sigma_r^*(w_r)$, then the duration is given by the rebels' stopping time $\sigma_r^*(w_r)$, or $\sigma_g^*(w_g) < \sigma_r^*(w_r)$, then the duration is given by the government's, $\sigma_g^*(w_g)$. Using equations 1 and 2, it can be shown that

$$\sigma_g^* \gtreqless \sigma_r^* \iff \left(\frac{w_g}{\overline{w}_g} \right)^{c_g/c_r} \gtrless \frac{w_r}{\overline{w}_r} \, , \tag{3}$$

giving the conditions under which either the government's or the rebels' quitting time determines the conflict's end (Powell 2017: 222). Intuitively, the rebels' quitting time is shorter when their valuation of winning w_r, relative to its rational maximum $\overline{w}_r$, is smaller than the respective valuation of the government $w_g/\overline{w}_g$ weighed by the relative

[2]For details on the formal derivation of the expected utility and technical assumptions as well as for the specific description of rational actors' upper bound of valuation $\overline{w}_i$, see section A.1 in the appendix.

costs c_g/c_r. Conflict duration D is thus defined by

$$D = \begin{cases} \frac{\overline{w}_g\overline{w}_r}{c_g+c_r}\left(\frac{w_g}{\overline{w}_g}\right)^{1+c_g/c_r} & \text{if } \left(\frac{w_g}{\overline{w}_g}\right)^{c_g/c_r} \leq \frac{w_r}{\overline{w}_r} \\ \frac{\overline{w}_r\overline{w}_g}{c_g+c_r}\left(\frac{w_r}{\overline{w}_r}\right)^{1+c_r/c_g} & \text{if } \left(\frac{w_g}{\overline{w}_g}\right)^{c_g/c_r} > \frac{w_r}{\overline{w}_r}. \end{cases} \tag{4}$$

When both sides value winning similarly, the cost ratio c_g/c_r is central in determining who quits first.[3] When the government is the stronger party, meaning the cost ratio favors the government, the rebels' quitting time determines the conflict duration, and vice versa. In our case of intrastate conflict, the government is usually the stronger side, as demonstrated by Lockyer (2010: 97) and Buhaug et al. (2009: 550) and also indicated by the troop ratio data used in this article (see section 4.2.4). Then, $\sigma_r^* < \sigma_g^*$, meaning the rebels' quitting time determines the duration of the conflict. In other cases, where the rebels are relatively stronger, the government's decision to quit fighting yields the duration. The following section will illustrate these different settings.

3.2 Comparative statics and general predictions

To derive general hypotheses on how conflict duration changes with changes in costs, we analyze the comparative statics of the model. As described above, it is essential to distinguish which of the two parties is the relatively stronger one. As it is the substantially more frequent case, we focus at first on settings where the rebels are weaker than the government. Then, $\sigma_r^* < \sigma_g^*$, meaning the rebels are expected to quit first and end the conflict. To analyze how the cost ratio $\rho = c_g/c_r$ and total costs $k = c_g + c_r$ affect the conflict duration in this case, we conduct comparative statics of $\sigma_r^*(w_r)$ with respect to ρ and k. For changes in c_r and accordingly the cost ratio ρ, higher (relative) costs for the rebels reduce conflict duration:

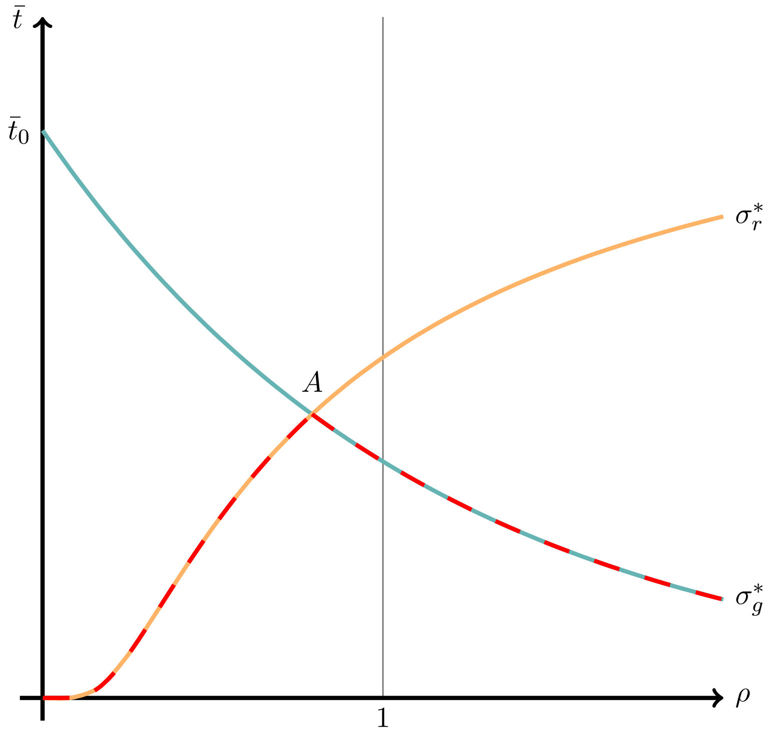
$$\frac{\partial\sigma_r^*}{\partial\rho} = -\frac{\overline{w}_g\overline{w}_r}{k\rho^2}\ln\left(\frac{w_r}{\overline{w}_r}\right)\left(\frac{w_r}{\overline{w}_r}\right)^{1+1/\rho} > 0 \tag{5}$$

$$\frac{\partial\sigma_r^*}{\partial c_r} = -\frac{\overline{w}_g\overline{w}_r}{(c_g+c_r)^2}\left(\frac{w_r}{\overline{w}_r}\right)^{1+c_r/c_g} + \frac{\overline{w}_g\overline{w}_r}{c_g(c_g+c_r)}\ln\left(\frac{w_r}{\overline{w}_r}\right)\left(\frac{w_r}{\overline{w}_r}\right)^{1+c_r/c_g} < 0 \tag{6}$$

whenever $\frac{w_r}{\overline{w}_r} < 1$, i.e. for all strategically planning rebels.

[3] Cases where the rebel side puts a higher valuation on winning than the government does are also plausible. However, the difference in the cost ratio, favoring the government, is presumably usually larger than the difference in the valuations: the more equal the cost ratio, the more existential a threat of the rebel side is for the government, leading to equalization of valuations due to a higher valuation of winning on the government's side. Thus, in the following, we mean one party is *sufficiently* stronger to counteract potentially differing valuations when speaking of one side being stronger. Note that the model in Powell (2017) does not require this assumption on the valuations of winning. However, only specific sets of $(w_r, w_g = \overline{w}_g(w_r/\overline{w}_r)^{c_g/c_r})$ do not stop fighting at the earliest possible time (ibid.: 222f.), placing a different inherent restriction on the configuration of valuations of winning.

Figure 1: Quitting times $\bar{t}$ depending on cost ratio ρ



Intuitively, the rebels' expectation for the valuation of the government $\mathbb{E}_t(w_g)$ increases throughout the conflict, as governments with low w_g would have dropped out and ended the conflict. Marginal costs c_r occur at a constant rate, so the rebel groups quit the conflict as soon as marginal expected gains no longer exceed the marginal costs. In a situation with higher costs c_r' for the rebels, and thus a less favorable, i.e. lower, cost ratio ρ', costs overtake expected gains earlier, leading the rebels to end the conflict earlier.

We depict this relationship between conflict duration $\bar{t}$ and the cost ratio in figure 1. It plots both quitting times σ_g^* and σ_r^* of the government and rebels depending on the cost ratio ρ, given their valuations of winning ($w_r > w_g$ here for illustration purposes). As derived formally above, the rebels quit the conflict later with increasing ρ, i.e. relatively lower costs for the rebel side, and the government quits the conflict later with decreasing ρ, since this means decreasing relative costs for the government side.

The conflict ends when at least one party decides to withdraw from the conflict. Thus, conflict duration is given by the quitting time of the party that quits first. In figure 1, the dashed red line represents the duration of the conflict dependent on ρ. For ρ left to the point A, $\sigma_r^* < \sigma_g^*$ means that the conflict duration is determined by the rebels' quitting time, which is increasing in ρ. For ρ right to A, the government decides to quit first, and σ_g^* thus determines conflict duration. At point A, both quit the conflict simultaneously, which is the case for $\rho = \ln(\frac{w_r}{\overline{w}_r})/\ln(\frac{w_g}{\overline{w}_g})$, leading to the longest possible conflict duration. In a symmetric setting where $w_g = w_r$, the most extended duration is reached for a symmetric cost ratio $\rho = 1$. Thus, we arrive at a remarkably different prediction than brought forward in many other non-formal analyses of civil conflict duration, where asymmetric settings of these conflicts are deemed to lead to longer durations (cf. e.g. Buhaug et al. 2009; Balcells and Kalyvas 2014).

As argued above, in intrastate conflicts, the government usually is the stronger party, i.e. it has the more favorable cost ratio. Assuming the valuations of both sides are relatively similar, the intrastate conflict situation is best described by the part of figure 1 left to point A. Under such circumstances, an increase in ρ, meaning the cost ratio is becoming less favorable to the government, leads to a longer conflict duration, as we derived formally in equation 5. We can thus state the following corollary[4]:

Corollary 1. *When the rebels are the weaker party, higher costs for the government, i.e. a cost ratio less favorable to the government, lead to longer conflicts, whereas higher costs for the rebels lead to shorter conflicts.*

Turning to total costs, a higher k unconditionally leads to shorter conflicts:

$$\frac{\partial \sigma_r^*}{\partial k} = -\frac{\overline{w}_g \overline{w}_r}{k^2} \left(\frac{w_r}{\overline{w}_g}\right)^{1+1/\rho} < 0 \tag{7}$$

Corollary 2. *Given a cost ratio, higher total costs lead to shorter conflicts.*

Although rebels are usually the weaker party in a civil conflict, the complimentary setting of a stronger rebel side might also be relevant. In this case, the quitting time of the government yields the conflict's duration. To determine the effect of governmental armament, we thus need to derive the comparative statics of $\sigma_g^*(w_g)$ with respect to c_r:

$$\frac{\partial \sigma_g^*(w_g)}{\partial c_r} \gtreqless 0 \iff -\frac{c_r^2}{c_g(c_g + c_r)} \gtreqless \ln(\frac{w_g}{\overline{w}_g}) \tag{8}$$

In contrast to the unambiguous effect when the rebels are weaker, we have two counteracting forces at work here: usually, when the government is the weaker side and thus determines the conflict ending, increasing rebel costs lead to a more symmetric power distribution and thereby prolong the conflict, meaning $\partial\sigma_g^*/\partial c_r > 0$. However, when the government's valuation w_g is sufficiently large, $\partial\sigma_g^*/\partial c_r < 0$, thus leading to the same impact of increased rebel costs as when the rebel side quits first. Additionally, a more significant shift of ρ in favor of the government might also lead to a

[4]Note that this corresponds to the inferences from the original Bayesian model in Powell (2017: 223).

reversal of the power asymmetry, where again the *rebels'* quitting time is determining the conflict end—and where the armament's effect is again unambiguously reducing duration.

Corollary 3. *When the rebels are the stronger party, higher costs for the rebels can have ambiguous effects on conflict duration.*

Appendix section A.2 additionally provides comparative statics and the respective corollary on effects of the governments' and rebels' valuation of winning.

3.3 Costs and valuations in civil conflict

Based on these general but precise and consistent predictions, we can now provide the first encompassing application of the model to an empirical question. In the war-of-attrition model, conflict duration is essentially determined by the costs c_g and c_r, which the government and the rebels respectively suffer during the conflict, and their ratio ρ. To capture the wide variety of factors that affect these costs, we understand costs not only as directly measurable fatalities and destruction. In a broader sense, the costs reflect the capabilities that allow one side to inflict these costs on the other side.

The cost ratio ρ thus depends on the military capabilities that cause fatalities and destruction for the adversary. Specifically, armament should raise the fighting costs of the rebels c_r, as this is the main reason the government procures military equipment in the first place, even though they might also raise financial costs for the government. Distance between the government stronghold, i.e. the capital, and the conflict might reduce the effective military capabilities deployed at the fighting site and thus decrease the relative costs of rebels. We lay out the mechanisms in more detail in the next section.

In contrast to distance, a troop ratio more favorable to the government side positively affects the government's capabilities and thus increases the relative costs for rebels. Taking these factors as inputs to the costs occurring to both sides, they respectively affect the cost ratio. As a higher ρ in the setting where the government is the stronger party increases conflict duration, we can derive the following hypothesis:

Hypothesis 1. *When the government is the stronger party, factors raising the government's capabilities to inflict costs on the rebel side, i.e. arms procurement and a favorable troop ratio, lead to shorter conflicts, while factors weakening the government's capabilities, i.e. distance between the capital city and the conflict site, prolong conflicts.*

Fighting costs of the government c_g are correspondingly affected by the capabilities of the rebel side. External support directly strengthens these capabilities and should result in higher costs for the government (Schulhofer-Wohl 2020). Mountainous terrain provides opportunities for rebels to hide and elude government forces, rendering effective fighting more costly for the government (Fearon and Laitin 2003; Brandt et al. 2008; Buhaug et al. 2009). Larger populations offer possibilities to retreat and more potential for rebel recruitment (Fearon and Laitin 2003), lowering the relative costs of the rebels. Groups that conduct terror attacks can inflict costs on the government, but are challenging to target, leading to a cost ratio less favorable to the government and thus prolonging conflicts (Butler and Gates 2009).

Hypothesis 2. *When the government is the stronger party, factors raising the rebels' capabilities to inflict costs on the government side, i.e. external support, mountainous terrain, larger populations and terrorist tactics, lead to more protracted conflicts.*

Given a specific cost ratio, the costs of government and rebels also affect the conflict duration due to total costs k, with higher k leading to shorter conflicts. Casualties in a conflict can be interpreted as such costs (cf. Brandt et al. 2008; Balch-Lindsay et al. 2008). Then, more intense conflicts should end earlier. Similarly, when conflict negatively impacts the economy, increased economic costs should lead to shorter conflicts (see Rohner and Thoenig 2021 for an overview of the potential socioeconomic costs of conflicts).

Hypothesis 3. *Higher total costs, as measured by casualties and reductions in economic welfare, lead to shorter conflicts.*

As governments in democracies face audience costs, meaning they are held responsible for the costs of fighting, we expect democracies to have higher valuations of these costs and thus fight shorter conflicts (Stam 1996; Filson and Werner 2004), even though anocracies might have advantages in reaching negotiated settlements (cf. Zarpli 2020). Additionally, the model predicts that parties with higher valuations of winning fight longer conflicts, given costs. If a group is politically excluded, we might expect that conflict success is valued relatively higher, as political paths to reach their goals are blocked (cf. Cederman et al. 2011, 2013).

Hypothesis 4. *When the government is the stronger party, higher valuations of costs on the government side, as is the case for democracies, lead to shorter conflicts, while higher valuations of winning on the rebel side, as are presumed for politically excluded groups, lead to longer conflicts.*

3.4 Armament, distance and the projection of power

Armament in the form of arms imports can be interpreted as additions to a government's multidimensional military capabilities, lifting constraints of the existing military equipment in specific dimensions, like ground transport, intelligence, or airpower. Since only a few countries can produce major weapons and other military technology on their own (cf. Thurner et al. 2019), governments will decide to import a portfolio of arms that is tailored to the conflict environment their military capabilities are used in. This allows them to benefit from the usefulness of specific types of weapons in different conflict scenarios. From the perspective of the war-of-attrition model presented above, arms imports thus enhance the government's capabilities to inflict costs on the rebel group.

Research on conflict severity by Mehrl and Thurner (2020) has shown that armament with MCW can lead to higher intensity, meaning they provide and are used as effective fighting tools—which is why a government chooses to obtain them in the first place. Smith and Toronto (2010) demonstrate that well-equipped ground troops in particular are very effective in counterinsurgency settings. Research for the US military concludes that combined with matching tactics, tanks and other ground armor can be a decisive factor for effective warfare, including in irregular wars and terrain difficult to maneuver (Johnson and Gordon IV 2010; Rogers 2012). Deployment of airpower might require an appropriate strategy, but can be very useful in conventional conflicts as well as in combating rebels in "small wars" and insurgencies (Corum and Johnson 2003; Dunlap Jr. 2008). Caverley and Sechser (2017) argue that combined mechanization in particular, i.e. the combination of both well-equipped ground and air forces, leads to shorter conflict. However, their estimations actually show that both types independently already reduce duration.

How effectively the government can fight in battles depends crucially on the conflict site location and its capabilities to project its military power to the conflict site. Boulding (1962) introduced the concept of a loss-of-strength gradient in international conflicts, which Buhaug et al. (2009) adopt for the intrastate conflict case: the distance between the government stronghold, operationalized by the country's capital city (cf. Buhaug and Gates 2002), and the conflict location reduces the relative military capabilities of the government. Distance can function as a proxy for diminishing government authority in remote areas (Herbst 2000), less local knowledge, and lack of support from the local population (Buhaug et al. 2009). Primarily, distance is an obstacle that must be overcome before the government can fight in a conflict, as all fighting equipment, troops and supplies have to be transported to the conflict site, and weaponry with a larger range has to be deployed. Thus, distance in general prolongs conflicts owing to reduced relative capacities of the government (cf. Buhaug et al. 2009; Buhaug 2010).

Importantly, we argue that armament with MCW increases the government's capabilities to project its military power over distance, as major weapons can be effective tools to overcome the challenges of fighting at remote conflict sites. First, imports of these MCW can provide the necessary mobility to transfer troops and other equipment to the battlefield (cf. Caverley and Sechser 2017). Transport aircraft and helicopters (Read 2010) as well as armored vehicles (Johnson 2017) enable fast and extensive transportation, reducing the negative impact of distance. Second, the outreach of the government's military can be sufficiently extended by importing attack helicopters and aircraft and long-range missiles that allow fighting a conflict more distant from military bases (Johnson 2017; Caverley and Sechser 2017; Read 2010). Third, in conflicts at larger distances, reliable intelligence is increasingly important to fight effectively. Aircraft, helicopters, drones, and satellites that can provide such information are thus even more valuable for deploying military capabilities in distant conflicts (cf. Tucker 2014). Indonesia, for instance, fought particularly protracted conflicts against GAM and CNRT over Aceh and East Timor, respectively, both at large distances and remotely located islands, with only medium levels of imports. These circumstances made it difficult for its military to bring the contested territories under control again. In Aceh, the Indonesian army had to resort to indiscriminate, crucial violence instead of targeted counterstrikes, leading to both militarily less advantageous results and counterproductive strengthening of GAM's resolve (cf. Ross 2005).

Arms procurement might of course also pose a significant financial burden, as framed in the well-known guns-versus-butter trade-off from Powell (1993). While not all imports have to be paid for one-to-one in financial terms, the arms trade was commercialized particularly after the Cold War (Brzoska 2004). Therefore, arms imports not only enhance a government's military capabilities, but also imply non-negligible financial costs (cf. SIPRI 2021; Holtom et al. 2012). Thus, costs of importation have to be outweighed by their benefits in fighting to improve the cost ratio in favor of the government ultimately. This depends crucially on the fighting effectiveness of MCW. As argued above, we expect MCW to be especially effective in projecting power over longer distances, meaning the conditional conflict-shortening effect on the cost ratio increases with the distance between the government stronghold and the conflict site. We can thus state the following hypothesis, specifying hypothesis 1 for the role of arms imports:

Hypothesis 1a. *Imports of MCW are effective tools for projecting the government's power over distances and therefore reduce the duration of conflicts at larger distances when the government is the stronger party.*

4 Research design and methods

4.1 The dyadic design

To match the dyadic perspective of the war-of-attrition framework as closely as possible, we employ a dyadic dataset design, following the contributions of Cunningham et al. (2009) and Cederman et al. (2009). On one side of each conflict dyad is a state government, and on the other side stands a non-state group, meaning we exclude conflicts without government involvement. Conflicts with multiple rebel groups involved are disaggregated into multiple dyads. Making conflict dyads and not conflicts our unit of analysis enables us to include group-specific covariates in our analysis, e.g. troop size or external support. Additionally, the composition of the government's adversaries in a conflict might change when it achieves an agreement with or victory over one involved group, but not with all groups to end the conflict as a whole.

To allow for time-varying covariates, a single observation in our dataset usually is a dyad-year, as all included covariates only feature yearly updates. As conflicts often come to a halt, but re-occur afterward, the specific duration used in our analyses is the duration of a continuous, uninterrupted spell of a dyad's conflict. Thus, a conflict dyad can appear multiple times in the data with different durations for different episodes of the same conflict.[5]

4.2 Data

4.2.1 The main independent variable: arms imports

To operationalize the build-up of military capacities on the government side, we use arms trade data provided by the Stockholm International Peace Research Institute's (SIPRI) Arms Transfer Database[6], which records trades of MCW.[7] SIPRI measures trade volumes by so-called trend-indicator values (TIV) that allow comparing transferred military capabilities based on production costs, but adjusted for inflation, wear and age of the arms at the time of transfer.

SIPRI covers a wide range of MCW types. Following Pamp et al. (2018), we differentiate between weapons that usually cannot be employed in intrastate wars—namely air-defense systems, anti-submarine weapons, and ships—and those employed in intrastate wars— namely aircraft, armored vehicles, artillery, and missiles. The former are then used as an instrument for the latter (see section 6.3).

[5] For additional details, see section A.4 in the appendix.

[6] Data of the SIPRI Arms Transfer Database can be retrieved at www.sipri.org/databases/armstransfers.

[7] The Norwegian Initiative on Small Arms Transfers (see http://nisat.prio.org/Trade-Database/) provides data on small arms and light weapons imports, but these are only available in reliable quality after the Cold War, with light weapons transfer data starting only in the 2000s (cf. Lebacher et al. 2021).

Trades of MCW often exhibit years-long spans between order and delivery. While orders might already constitute signals to rebel groups, the effect on military capacity is only realized when the weapons are delivered. To smooth out these discrepancies, we use the 3- and 5-year moving average of $t-1$ to $t-3/t-5$ additionally to lagged imports. Note that the averaged values can also be interpreted as a measure of arms build-ups, as they are just a rescaled version of the sum of imports over the preceding three or five years. In comparison, the lagged variable specification rather operationalizes the short-term lifting of specific, binding constraints in a government's arms portfolio.

4.2.2 Relative rebel strength

To account for the power distribution in a conflict, we use the ratio of government troops to rebel troops. This is the most direct possible operationalization of the cost ratio. Such a troop ratio was proposed in a similar form by Wood (2010) and Hultquist (2013). The data on rebel troops is taken from the UCDP Conflict Encyclopedia (Uppsala Conflict Data Program 2018), while data on government troops is taken from the Correlates of War's (COW) National Material Capabilities dataset[8] (Singer et al. 1972) because of missing and inconsistent reporting of government troop size from UCDP.[9] For both rebel (UCDP) and government (COW) troop data, we overcome problems with data availability by interpolating missing data points between records.

To operationalize the different regimes of predictions, that depend on the specific distribution of power, we use a dummy variable that indicates whether the rebels or the government are the relatively stronger side. Technically, the dummy states whether the troop ratio lies below or above the median of the observed distribution. Note that using the median is a conservative measure to ensure that only relevant cases of rebels weaker than the government are included and to avoid false positives. Clearly, this also results in cases where rebels are stronger than in the median case, but still relatively weaker than the government in the theoretical sense of the cost ratio ρ. In models that only contain the weaker-than-median-rebels observations, as is the case in additional estimations in the appendix, we control for the power distribution using a logarithmic rescaling of the troop ratio, to account for a potentially non-linear effect of large asymmetries (the largest being 40000:1 in favor of the government).

4.2.3 Distances

Following Buhaug and Gates (2002) and Buhaug et al. (2009), we operationalize the government stronghold as the country's capital city to derive a distance from the con-

[8]We use version 5.0 of the National Material Capabilities dataset, which can be retrieved from www.correlatesofwar.org/data-sets/national-material-capabilities.

[9]Note that potentially differing definitions of "troops" by UCDP and COW do not affect our results, as they would affect the troop ratio for all observations equally, just rescaling the variable.

flict site. To calculate this distance between the capital city and the conflict, we use two different datasets on conflict locations: the PRIO Conflict Site dataset (Buhaug and Gates 2002), covering conflicts from 1946 to 2005, and the UCDP Georeferenced Events dataset (Pettersson and Öberg 2020; Sundberg and Melander 2013), starting in 1989. For conflicts only covered by the PRIO dataset, we calculate the geodesic, i.e. beeline, distance, between the capital and the "conflict center" as provided by the dataset, which is the same for all dyads in a conflict in a given year. When data from UCDP, which provides geolocations for every conflict event, is available, we compute the geodesic distance for each event separately and use the mean of all events in a dyad-year.

4.2.4 Additional operationalizations of costs and valuations

External support for rebel groups enables rebels to enhance their capabilities to increase the government's costs and the cost ratio, thus prolonging conflicts. In our analysis, we include two dummy variables to account for weapons and logistics support as well as direct troop support. This data is provided by the Dangerous Companions dataset (San-Akca 2015, 2016).

A direct measurement of costs that both sides have to cope with is casualties. However, data distinguishing which side fatalities can be attributed to is only available for a minor part of our observation period. We thus use the total number of deaths per conflict/dyad per day to operationalize the total cost k. It is essential to calculate the number of casualties *per day*, as the dependent variable, conflict duration, is also measured per day—although the data are reported per conflict-year, it is necessary to distinguish between two conflicts with the same reported casualties, where one only lasts a few days in this year and the other one is active the whole year. To obtain a larger temporal scope, we use data from the UCDP Battle-related Deaths dataset (Pettersson and Öberg 2020) and combine it with data from the PRIO Battle Deaths dataset (Lacina and Gleditsch 2005).

Terrain that is difficult to operate militarily in might increase the relative costs of fighting for the government. We operationalize this as mountainous terrain, using geographic elevation and slope data from the United Nations Environment Programme World Conservation Monitoring Centre UNEP-WCMC (2002). With this data, we are able to compute a mountains index for each conflict site for every year, as is recorded by the PRIO Conflict Site dataset and UCDP. For the size of the population, GDP per capita, and GDP growth, we use data from the Maddison Project (Bolt et al. 2018). As is standard in the literature, we use population and GDP per capita in logarithmic form. To capture whether a group might use terrorist tactics, we include a dummy variable that indicates whether the Global Terrorism Database also records a rebel group as a terrorist group.

As democracies might value the costs of fighting higher (Stam 1996; Filson and Werner 2004), we include a dummy variable for countries with a Polity IV score of 6 and higher (Marshall et al. 2016). Data from the Ethnic Power Relations dataset (Vogt et al. 2015) is used to indicate whether a group is politically excluded. Additionally, we control whether the conflict is fought over government or territory, as coded by UCDP, for the Cold War period (cf. Kalyvas and Balcells 2010), and, in alternative specifications, for military expenditure as a potential proxy for the government's conflict costs, using data from SIPRI's Military Expenditure Database[10] imputed with COW's National Material Capabilities data.

4.3 Standard duration estimation with the Cox model

For our main correlational results, we employ a standard Cox Proportional Hazards model (Cox 1972). The Cox model specifies the functional form of the hazard $h_i(t)$ as

$$h_i(t) = h_0(t) \exp[\boldsymbol{\beta'} \boldsymbol{X_i}(t) + \boldsymbol{\gamma'} \boldsymbol{W_i}(t)], \tag{9}$$

where $h_0(t)$ is the baseline hazard, which depends only on analysis time t, $\boldsymbol{X_i}(t)$ comprises the main explanatory variables of conflict dyad i and $\boldsymbol{W_i}(t)$ the control variables (for simplicity referred to as $\boldsymbol{X_i}$ and $\boldsymbol{W_i}$ in the following, respectively). $\boldsymbol{\beta}$ and $\boldsymbol{\gamma}$ denote their respective vectors of coefficients. To account for the three-way dependence of the main hypotheses, $\boldsymbol{X_i}$ contains MCW imports and their interactions with both distance and the rebel-strength dummy variable, as well as their three-way interaction.

The Cox model is estimated by "partial" likelihood, using only the ordered failure times instead of time intervals (Therneau and Grambsch 2000). As proposed e.g. by Cunningham et al. (2009) and Cederman et al. (2013), we use robust standard errors to correct for clustering at the conflict level in all models.

5 Empirical results

Table 1 presents the results of models with different specifications of arms imports as lagged (models 1 and 2), 3-year averaged (models 3 and 4), and 5-year averaged variables (models 5 and 6). While odd-numbered models use the main variables without their interactions, even-numbered models include both two-way and three-way interactions of arms imports, distance and the rebel-strength dummy. Note that coefficients in these survival models represent the variables' effect on the hazard of the termination event, meaning that a negative coefficient implies a lower termination risk and thus a conflict-*prolonging* effect of a variable.

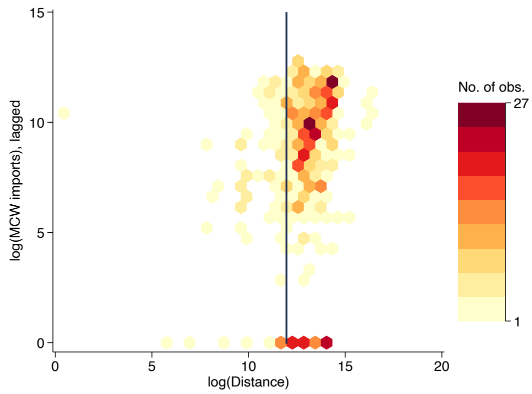
[10] Data of the SIPRI Military Expenditure Database can be retrieved at www.sipri.org/databases/milex.

Table 1: Conflict duration – Cox results w/o instrumentation

	Lagged		3-year avg.		5-year avg.	
	(1)	(2)	(3)	(4)	(5)	(6)
log(Distance)	–0.126**	–0.153**	–0.127**	–0.165**	–0.127**	–0.144**
	(0.031)	(0.036)	(0.032)	(0.033)	(0.032)	(0.042)
Rebels are weaker dummy	0.212	1.646*	0.211	3.272*	0.219	3.821*
	(0.133)	(0.778)	(0.137)	(1.397)	(0.138)	(1.711)
Rebels are weaker dummy x log(MCW imports)		–0.361*		–0.542**		–0.639**
		(0.150)		(0.198)		(0.236)
Rebels are weaker dummy x log(Distance)		–0.138*		–0.255*		–0.300*
		(0.065)		(0.113)		(0.136)
Rebels are weaker dummy x log(MCW imports) x log(Distance)		0.033**		0.045**		0.053**
		(0.012)		(0.015)		(0.018)
log(MCW imports)	–0.009	0.026	–0.005	0.015	–0.013	0.066
	(0.017)	(0.091)	(0.020)	(0.090)	(0.024)	(0.112)
log(MCW imports) x log(Distance)		–0.005		–0.003		–0.008
		(0.008)		(0.007)		(0.009)
Group receives weapons support	–0.390**	–0.395**	–0.390**	–0.388**	–0.389**	–0.388**
	(0.124)	(0.121)	(0.123)	(0.122)	(0.124)	(0.122)
Group receives troops support	0.486**	0.518**	0.481**	0.515**	0.488**	0.515**
	(0.152)	(0.160)	(0.156)	(0.163)	(0.155)	(0.161)
Battle-related deaths per day in thousands	0.199**	0.209**	0.197**	0.206**	0.200**	0.209**
	(0.023)	(0.023)	(0.021)	(0.021)	(0.022)	(0.023)
Conflict over government (vs. territory)	–0.250*	–0.260*	–0.254*	–0.276*	–0.250*	–0.279*
	(0.118)	(0.124)	(0.118)	(0.127)	(0.117)	(0.127)
Group is excluded	–0.254*	–0.232+	–0.255*	–0.246*	–0.255*	–0.242*
	(0.120)	(0.124)	(0.121)	(0.124)	(0.120)	(0.122)
Democracy dummy	–0.196	–0.215	–0.196	–0.227	–0.194	–0.232
	(0.167)	(0.173)	(0.168)	(0.173)	(0.168)	(0.174)
GDP p.c. growth	0.280	0.608	0.261	0.477	0.254	0.467
	(0.559)	(0.553)	(0.569)	(0.553)	(0.575)	(0.561)
log(GDP p.c.)	–0.037	–0.026	–0.043	–0.019	–0.034	–0.009
	(0.065)	(0.065)	(0.068)	(0.068)	(0.068)	(0.068)
log(Population)	–0.041	–0.077	–0.045	–0.074	–0.037	–0.064
	(0.056)	(0.054)	(0.055)	(0.054)	(0.056)	(0.056)
Group is terrorist group	–0.356**	–0.370**	–0.358**	–0.378**	–0.357**	–0.380**
	(0.122)	(0.123)	(0.123)	(0.125)	(0.122)	(0.125)
Mountainous terrain index	–0.241*	–0.225*	–0.246*	–0.229*	–0.242*	–0.225*
	(0.110)	(0.103)	(0.110)	(0.103)	(0.109)	(0.103)
Cold War dummy	–0.458**	–0.443**	–0.468**	–0.473**	–0.451**	–0.447**
	(0.129)	(0.132)	(0.129)	(0.137)	(0.132)	(0.140)
No. of observations	1683	1683	1683	1683	1683	1683
No. of conflict dyads	418	418	418	418	418	418
No. of failures	378	378	378	378	378	378

Note: * (+, **) indicates $p < .05$ (.1, .01)

Figure 2: Imports-distance combinations and estimated direction of duration effect



The plot shows the frequency of arms imports–distance combinations for all observations where the rebel side is weaker. The vertical line indicates the distance above which the total effect of arms imports is positive, i.e. duration-reducing.

Within all models, the expectations of our theoretical war-of-attrition model are corroborated for the majority of hypotheses and variables: when the government is the stronger side and able to inflict higher (lower) costs on the rebel side, the conflict takes shorter (longer). Regarding arms imports, as stated in the hypotheses 1 and 1a, our theory predicts an increasingly duration-reducing effect of governmental military buildups at larger distances—when the government is the stronger party. In all models, the results corroborate this relationship: arms imports exhibit, *ceteris paribus* at zero distance, a negative effect on the termination hazard when the rebels are the weaker party. The coefficient of the three-way interaction is positive in all models, indicating an increasingly conflict-shortening effect of arms imports at longer distances in the setting of the government being the stronger party.

Interpreting the total effect of arms imports needs caution, as it can only be retrieved by combining the arms imports variable's and the interaction terms' effects, with the latter depending on distance and rebel strength. As these effects have different signs, there exists a specific distance at which they cancel out for cases where the government is the stronger side. Below this threshold distance, arms imports prolong conflicts, while they reduce the duration above it. Regarding model 2, for instance, arms imports have a shortening effect for all distances above 156 km. Figure 2 illustrates that for 85 % of rebels-weaker observations, arms imports lead to shorter duration, as they lie to the right of the vertical line depicting the threshold distance. Note that we have only a limited number of observations with small distances, leading to weaker informative support regarding specific effects of armament at low distances.

Our theory can explain why the duration-reducing effect only exists at more considerable distances: procurement of MCW raises the government's ability to inflict costs on the rebel group, particularly for conflicts in such remote areas. At the same time, the government has to bear the costs of acquiring and maintaining these major weapons independent of the conflict circumstances. Thus, arms imports might render the cost ratio more favorable to the rebels in conflicts at small distances and more favorable to the government in conflicts at larger distances. Therefore, armament exhibits a conflict-prolonging effect at small distances and a conflict-shortening effect at larger distances, when the rebels are the weaker side.

Most of the literature has found either a conflict-prolonging or no significant effect of government strength relative to rebel strength. However, in our war-of-attrition model, government strength is seen as another factor raising the costs of fighting for rebels, analogously to armament (hypothesis 1). In our analysis, the positive coefficient of the rebel-strength dummy, which is based on the ratio of government troops to rebel troops, indicates that conflicts are indeed shorter the more favorable this ratio turns out for the government (see section A.7 in the appendix for results of the continuous troop ratio variable instead of the primary dummy variable). We thus further corroborate our theoretical framework, but partially contradict the results from Cunningham et al. (2009) and Buhaug et al. (2009) obtained with data from the Non-State Actors dataset.

Mirroring the expectations regarding increased government capabilities, the war-of-attrition model predicts increased rebel capabilities to prolong conflicts, when the rebels' quitting time determines the conflict end (hypothesis 2). We find that external support by weapons transfers prolongs conflicts as predicted, but direct troop support affects conflict duration negatively, corroborating Sawyer et al. (2017) with different data on external support. From the perspective of the theoretical framework, this finding might indicate that third-party troops can shift the distribution of military capabilities substantially, so that the government would not be the stronger side anymore. Then, increasing the capabilities of the stronger, i.e. rebel in this case, side would be expected to lead to shorter conflict durations (cf. Balch-Lindsay et al. 2008). Powell (2017) describes this logic as "taking sides", where a third party generally prefers to join the conflict side such that a shorter duration is feasible, since shorter durations mean lower costs to bear. As another factor favoring the rebel side apart from external support, mountainous terrain exhibits conflict-prolonging effects, in line with the predictions from hypothesis 2. This result supports the theoretical considerations—highly contested in the literature—from Fearon and Laitin (2003).

Higher total costs are expected to lead to shorter conflicts. Corroborating the corresponding hypothesis 3, the number of battle-related deaths per day exhibits a clear duration-reducing impact, meaning more intense conflicts end earlier. Notably, we can thereby substantiate similar theoretical considerations from Brandt et al. (2008), although they themselves were not able to corroborate their hypothesis empirically. In contrast to casualties, costs in terms of economic growth are not found to play a role for conflict duration. The same is the case for democracy, contradicting the first part of hypothesis 4 that democracies value costs higher. However, the expectations of higher valuations of winning for excluded groups are consistent with findings that excluded groups fight more extended conflicts. Additionally, conflicts fought over government last longer than conflicts over territory.

6 Corroborating causality

6.1 Endogenous demand for arms imports

The above-presented empirical results greatly support our theoretical ramifications. However, these standard survival models would not be able to rule out that results are affected by the endogenous demand for major weapons imports. Theoretically, one expects a forward-looking government to import major weapons when it anticipates a protracted involvement in a conflict. Since transfers of major weapons can take extended time from an order agreement to delivery, governments have to plan ahead, making their import decisions dependent on expected conflict dynamics and duration.

Another type of endogeneity might result directly from our theoretical war-of-attrition framework: Powell (2017) shows that a third party that “takes sides” in general prefers to support the stronger conflict party to reach a shorter conflict duration because the conflict is costly as well for the third party. Of course, exporting arms can be interpreted as a form of support for such a third party (cf. Sawyer et al. 2017). Then, arms transfers to a government in conflict are of higher volume when the conflict is expected to be shorter, rendering armament dependent on conflict duration. This potential endogeneity must be controlled for to allow for a causal interpretation of arms imports’ effect on conflict duration.

6.2 Implementing a two-stage-residual-inclusion Cox IV model

Whenever the effect of an independent variable is presumably endogenous to the dependent variable, an established method to achieve estimates of causal effects are instrumental variables models (Angrist and Pischke 2009). Regarding the wide use of the Cox Proportional Hazards model as a quasi-standard in the Political Science literature, it would be the obvious path to implement an instrumental variable Cox model. However, the non-collapsibility of the Cox model leads the standard two-stage estimation to give inconsistent estimates (Martinussen and Vansteelandt 2013).

To overcome these problems, Martínez-Camblor et al. (2019) propose using a two-stage residual inclusion approach with a frailty in the second, i.e. the Cox model, stage. In this two-stage instrumental variable survival model, the first stage uses OLS to regress the endogenous variables on the exogenous regressors and the instruments. The estimations from the first stage are used to calculate the residual of each observation, which is then included as a control variable in the second-stage frailty Cox model. When arms imports are affected by the unobserved expectation of a protracted conflict, this is contained in the first stage's residuals. Controlling for these residuals in the second stage thus allows to capture the true effect of the armament in these estimations.[11]

In our analyses, the vector of instruments $\boldsymbol{Z_i}$ comprises the arms imports unrelated to civil conflict (see below) and their interactions with distance and the rebel-strength dummy as well as their three-way interaction. Correspondingly, the vector of endogenous covariates $\boldsymbol{X_i}$ consists of the arms imports variable, their interaction terms with both distance and the rebel-strength dummy and the three-way interaction.

The first stage equation is then

$$\boldsymbol{X_i} = \boldsymbol{\delta}'\boldsymbol{Z_i} + \boldsymbol{\lambda}'\boldsymbol{W_i}, \tag{10}$$

where $\boldsymbol{\delta}$ and $\boldsymbol{\lambda}$ are regression coefficient vectors of the instruments $\boldsymbol{Z_i}$ and the exogenous covariates $\boldsymbol{W_i}$, respectively. This equation is estimated by standard OLS to obtain the estimated coefficients $\hat{\boldsymbol{\delta}}$ and $\hat{\boldsymbol{\lambda}}$, which are then used to calculate the residuals

$$\hat{\boldsymbol{R}}_{\boldsymbol{i}} = \boldsymbol{X_i} - \hat{\boldsymbol{X}}_{\boldsymbol{i}}. \tag{11}$$

Then, we can include these residuals in the second stage Cox model. However, to avoid biases due to the non-collapsibility of the Cox model introduced by such a two-stage approach (Martinussen and Vansteelandt 2013), Martínez-Camblor et al. (2019) use a Cox model with individual frailty α_i for each unique conflict dyad:

$$h_i(t) = h_0(t)\alpha_i \exp[\boldsymbol{\theta}'\boldsymbol{X_i}(t) + \boldsymbol{\beta}'\boldsymbol{W_i}(t) + \boldsymbol{\xi}'\hat{\boldsymbol{R}}_{\boldsymbol{i}}(t)] \tag{12}$$

where $\boldsymbol{\theta}$ is the endogenous variables', $\boldsymbol{\beta}$ the exogenous covariates' and $\boldsymbol{\xi}$ the residuals' vector of coefficients, respectively.[12] Again, we use robust standard errors cor-

[11] An alternative approach for implementing an instrumental variable survival model is to use an Aalen additive hazards model instead of Cox, as the Aalen model does not suffer from the non-collapsibility issue. We also implement a simpler version of the Aalen model in Appendix section A.9, leading to similar results as the Cox IV model.

[12] As with the standard Two-Stage-Least-Squares estimators, standard errors of the second stage's coefficients might need correction, as the uncertainty in estimating the first stage has to be accounted for. Simulations for similar models show that default standard errors only slightly underestimate the true uncertainty of the IV estimates, enabling us to draw valid conclusions about statistical significance (Lergenmuller 2017). Additionally, we calculated standard errors by bootstrapping, but the results do not

recting for clustering at the conflict level (cf. Cunningham et al. 2009; Cederman et al. 2013).

6.3 Instrument

To apply this instrumental variable approach, we need a valid instrument, i.e. an instrument that is a) related to the instrumented variable and b) exogenous to the duration of intrastate conflict. For instrumenting governments' arms imports, we propose to use arms imports that are usually not employed in civil conflicts as an instrument. This instrument was proposed by Pamp et al. (2018) to instrument arms imports in conflict onset analysis. Also, Sullivan et al. (2018) used it for researching the impact of arms imports on human rights conditions and Mehrl and Thurner (2020) in their assessment of armaments' effects on civil conflict intensity.

This instrument exploits the fact that governments fighting intrastate conflicts can only employ a subset of their weapons systems, as rebel groups usually only fight on the ground and do not use aircraft or ships.[13] Therefore, imports of anti-aircraft systems, for instance, are not caused by a threat or the fighting of intrastate conflicts because they are of no use in such a scenario and are consequently exogenous to the risk and duration of a civil conflict. Hence, we can use arms imports of weapons not related to intrastate fighting—naval weapons, air defense systems, missiles and ships—to instrument for arms imports that governments can bring to the battlefield in such conflicts, i.e. tanks, armored vehicles, aircraft, and artillery.[14]

On instrument relevance, as Pamp et al. (2018) argue, arms imports relevant and not relevant for fighting intrastate conflicts are closely related because they are both affected by decisions over the general armament level, e.g. due to external threats. While such portfolio balancing might violate exogeneity, Mehrl and Thurner (2020) ascertain that governments fighting in an intrastate conflict have to focus their arms acquisitions on that conflict, temporarily breaking the portfolio-balancing link and thereby maintaining exogeneity. While the instrumental variable's exogeneity cannot be proven empirically, we can provide evidence for its relevance (see table A1 in the appendix).

6.4 Empirical results of Cox IV model

Table 2 presents the results of models with different specifications of arms imports as lagged (model 1), 3-year averaged (model 2), and 5-year averaged variables (model 3), using the Cox two-stage-residual-inclusion instrumental variable model mirroring the standard Cox model presented in table 1.

differ substantially.

[13]There are exceptions, like the LTTE employing some ships, as noted by Pamp et al. (2018). Using a narrower set of arms, i.e. only air-defense missiles, as instrument does not alter the results.

[14]See Pamp et al. (2018) for a more detailed explanation of the instruments' exogeneity.

Table 2: Conflict duration – Cox IV results

	Lagged (1)	3-year avg. (2)	5-year avg. (3)
MCW imports	-0.415	0.030	0.206
	(0.537)	(0.300)	(0.254)
Rebels weaker dummy	7.313	8.203*	8.292**
	(4.885)	(3.405)	(3.037)
log(Distance)	-0.209	-0.094	-0.045
	(0.162)	(0.117)	(0.113)
MCW imports × log(Distance)	0.025	-0.010	-0.023
	(0.043)	(0.025)	(0.022)
MCW imports × weaker-rebels dummy	-0.793	-1.104*	-1.214**
	(0.800)	(0.502)	(0.436)
MCW × log(Distance) × weaker-rebels dummy	0.070	0.094*	0.102**
	(0.062)	(0.040)	(0.035)
log(Distance) × weaker-rebels dummy	-0.604	-0.671*	-0.676**
	(0.382)	(0.271)	(0.243)
First-stage residuals (MCW imports eq.)	0.462	0.005	-0.228
	(0.559)	(0.411)	(0.426)
First-stage residuals (distance-interaction eq.)	-0.031	0.008	0.025
	(0.045)	(0.034)	(0.036)
First-stage residuals (weaker-rebels-interaction eq.)	0.452	0.853	1.216
	(0.863)	(0.726)	(0.749)
First-stage residuals (threeway interaction)	-0.040	-0.074	-0.102+
	(0.067)	(0.058)	(0.060)
Group receives weapons support	-0.408**	-0.385**	-0.387**
	(0.123)	(0.121)	(0.121)
Group receives troops support	0.544**	0.542**	0.521**
	(0.199)	(0.196)	(0.194)
Battle-related deaths per day in thousands	0.264**	0.261**	0.261**
	(0.072)	(0.072)	(0.072)
Conflict over government (vs. territory)	-0.308*	-0.315*	-0.320*
	(0.152)	(0.150)	(0.149)
Group is excluded	-0.252+	-0.242+	-0.237+
	(0.136)	(0.133)	(0.133)
Democracy dummy	-0.275+	-0.288+	-0.274+
	(0.159)	(0.158)	(0.157)
GDP p.c. growth	1.062+	0.636	0.592
	(0.582)	(0.519)	(0.519)
log(GDP p.c.)	0.041	0.040	0.027
	(0.085)	(0.077)	(0.075)
log(Population)	-0.098	-0.065	-0.062
	(0.071)	(0.068)	(0.066)
Group is terrorist group	-0.419**	-0.411**	-0.404**
	(0.125)	(0.124)	(0.124)
Mountainous terrain index	-0.168+	-0.206*	-0.214*
	(0.096)	(0.090)	(0.090)
Cold War dummy	-0.392**	-0.408**	-0.402**
	(0.146)	(0.140)	(0.138)
No. of observations	1683	1683	1683
No. of failures	378	378	378

Note: * (+, **) indicates $p < .05$ (.1, .01). Cluster-robust standard errors in parentheses.

The results of the IV approach solidify those of the non-instrumented models: arms imports have a causal effect with weaker rebels, at least in the 3- and 5-year cumulative imports specifications. Based on the point estimates for model (3), the duration-reducing effects materialize for distances above 348 km.[15]

With these IV estimations, we find no evidence for endogenous demand, as is indicated by the widely insignificant included residuals. When comparing the results between the non-instrumented and IV models, confidence intervals of the relevant coefficients of the arms imports variable and its interactions overlap, meaning the results do not differ statistically significantly. In fact, this corroborates the results by Mehrl and Thurner (2020), where the effects of arms imports were also found to not depend substantially on the conflict's anticipated intensity.

7 Conclusion

In this paper, we find robust results on the impact of governmental armament on the duration of intrastate conflict when the power distribution in general favors the government: in conflicts located at considerable distances from the capital, arms imports can lead to shorter fighting. In contrast to the existing literature, we propose a causal design using an instrumental variable survival approach based on the Cox Proportional Hazards model. With this model, we are able to rule out potential demand endogeneity due to anticipated protracted fighting. Thus, we substantiate that arms imports indeed affect conflict duration causally. In our opinion, this IV survival model is a worthwhile addition to the empirical toolbox of Political Science and should be considered whenever endogeneity is of concern in event history analysis.

It is a widely shared view in the literature that weakness of rebels relative to the government prolongs conflicts (cf. e.g Cunningham et al. 2009). However, our formal war-of-attrition framework rather delivers the prediction that symmetric power distributions *prolong* conflicts. In fact, we robustly find that higher rebel strength, leading to more symmetric settings, increases conflict duration when using rebel troop size from UCDP as a measure of rebel strength. Further investigation seems warranted to clarify whether the different results hinge on differences in data quality of the UCDP and Non-State-Actors data (Cunningham et al. 2013) or if they operationalize rebel strength in diverging but meaningful ways. Either way, these empirical results underline the value of consistent hypothesis-building using our encompassing war-of-attrition framework. Regarding the role of conflict distance, a more differentiated perspective of the conflicts' geographic situation concerning terrain and spatial dispersion (cf. e.g. Schutte and Weidmann 2011) might bring clearer evidence on the exact conditions and circumstances when specific types of armament can help the government to

[15]Figure A1 in the appendix section A.6 further illustrates the marginal effects of arms imports.

project military power.

Overall, we do not assess whether governments fight conflicts for a just cause. Rather, we extract general patterns of causality in the relationship between governmental armament, the geographical context of the conflict and its duration. Notably, when governments fight civil wars with rebel groups, they are faced with the decision to shift scarce resources to invest in armament (cf. Powell 1993). This trade-off usually concerns low- or middle-income countries with high opportunity costs, making military investments a burdensome decision. Furthermore, increased armament might cause conflicts to intensify (Mehrl and Thurner 2020), rendering conflict severity and duration a complex trade-off. Future research should investigate how this trade-off depends on the specific circumstances regarding e.g. distance and rebel strength. Although we find that arming the government might help in some cases to end conflicts earlier, it arguably is not a generally advisable policy.

8 Addendum: A "casualties-as-time" analysis approach to conflict severity

8.1 The question of conflict intensity and total deaths

Research on conflict duration can provide valuable insights into what causes prolonged conflicts, and research on conflict intensity provides valuable insights into what causes low- or high-intensity fighting. From an ethical and political standpoint, an important perspective is not covered by those two separate approaches: the total number of casualties. Intensity and duration are undoubtedly linked to the number of casualties, but in uncertain ways: a low-intensity, long-duration conflict could result in a higher or lower number of casualties than a short-duration, high-intensity conflict.

The literature so far has not always clearly distinguished between intensity or severity as a measure of deaths per time on the one hand and the total number of fatalities in a conflict on the other hand. For instance, Heger and Salehyan (2007) find that stronger rebels fight in more deadly conflicts, using the total count of fatalities as the dependent variable, despite referencing the "severity of civil conflict" in the article's title. With the same ambiguous concept of "severity", Lacina (2006) finds no relationship between military quality and total conflict deaths.

Other studies focusing on governmental armament do not analyze the total death toll of conflicts, but actual intensity. Balcells and Kalyvas (2014) find that conventionally fought conflicts, i.e. where both sides are sufficiently armed with major weapons, are more intense. Results from Moore (2012) indicate that imports of MCW increase intensity in the following year, but this effect loses statistical significance after correcting for a coding error (Mehrl and Thurner 2020). Mehrl and Thurner (ibid.) show that increased government arming can lead to more intense fighting under specific conditions, and Gallea (2023) corroborates this relationship even unconditionally. The preceding sections of this chapter provide evidence that governmental armament can shorten conflict duration, again under specific conditions. Thus, the overall impact in terms of "lives endangered or saved" by additional arms imports is unclear.

8.2 A new approach: the "casualties-as-time" model

With the brief remarks in this section, I propose an approach to deal with this type of question. Instead of using conflict duration as the dependent variable of survival analysis models, I suggest treating the casualties count as the dependent variable in such models. The analysis can thus determine causes impacting how many casualties have to occur until a conflict is ended, i.e. the last casualty was recorded.

In contrast to OLS models usually applied to estimate total deaths models (cf. e.g.

Lacina 2006; Heger and Salehyan 2007), this approach has the substantial advantage of allowing for time-varying independent variables. Thus, it is possible to take heterogeneity over time into account. Especially for changes in armament, as measured by arms imports, this heterogeneity over time can be significant, e.g. when governments ramp up their weapons acquisition after the start of a conflict. Then, averaging arms imports over the whole conflict period obfuscates the true effect on the casualty total.

In the "casualties-as-time" model, the observational unit is the dyad-year, like in the duration analysis in the preceding sections. However, for the analysis, the "duration" of one observation used for the survival analysis is determined by the number of casualties in this year. Generally, different time scales would be possible, as casualty data is available with daily precision, at least since 1989. However, most other covariates like arms imports are only available in yearly form.

While using a survival model enables covariates to change over time, the dependent variable, i.e. the progressing number of casualties, is independent of a time dimension. This differentiates the approach from standard counting-process approaches, such as using negative binomial regression on casualties per year or casualties over the whole conflict.

8.3 Empirical results

Table 3 presents the results of a Cox model, with the same specification as used for duration analyses above, but with the number of casualties as the dependent "survival time" variable. Notably, most effects show the same sign and are similar in (relative) size to effects in analyses with conflict duration as the dependent variable (see table 1).

Regarding the main variables of interest, distance exhibits a very similar "prolonging" effect, meaning that *ceteris paribus*, more casualties are observed in conflict dyads fought at larger distances from the government stronghold. Also, dyads where the rebels are the relatively weaker side might end after fewer casualties than conflicts with relatively stronger rebels; the effect is statistically significant when including arms imports averaged over the preceding five years.

Especially when using this 5-year average of arms imports as a measurement for governmental armament, a similar impact of armament as in the duration analyses is found. When interacted with the dummy on rebels' relative weakness and distance, the arms imports variable is statistically significant—with a positive coefficient, meaning that higher levels of armament lead to conflicts ending with lower numbers of casualties when the conflict is fought against weaker rebels and at more considerable distances. However, the interaction effect of arms and the rebels-weaker dummy is

Table 3: Casualties-as-time – Cox results

	Lagged		3-year avg.		5-year avg.	
	(1)	(2)	(3)	(4)	(5)	(6)
log(Distance)	–0.085*	–0.134**	–0.087*	–0.163**	–0.087*	–0.134*
	(0.036)	(0.045)	(0.035)	(0.050)	(0.035)	(0.068)
Rebels are weaker dummy	0.596**	1.337	0.578**	2.613+	0.582**	4.128*
	(0.127)	(0.880)	(0.128)	(1.500)	(0.128)	(1.930)
Rebels are weaker dummy x log(MCW imports)		–0.258		–0.422+		–0.654*
		(0.189)		(0.251)		(0.316)
Rebels are weaker dummy x log(Distance)		–0.086		–0.174		–0.293+
		(0.076)		(0.121)		(0.154)
Rebels are weaker dummy x log(MCW imports) x log(Distance)		0.025+		0.036+		0.054*
		(0.015)		(0.020)		(0.025)
log(MCW imports)	–0.003	–0.087	0.016	–0.080	0.011	0.007
	(0.019)	(0.104)	(0.026)	(0.117)	(0.030)	(0.167)
log(MCW imports) x log(Distance)		0.004		0.006		–0.002
		(0.009)		(0.009)		(0.013)
Group receives weapons support	–0.358*	–0.358*	–0.371**	–0.357*	–0.368**	–0.351*
	(0.140)	(0.140)	(0.142)	(0.142)	(0.142)	(0.143)
Group receives troops support	0.130	0.165	0.115	0.168	0.123	0.174
	(0.169)	(0.177)	(0.172)	(0.179)	(0.171)	(0.177)
Battle-related deaths per day in thousands	–0.337	–0.337	–0.339	–0.337	–0.339	–0.335
	(0.261)	(0.277)	(0.258)	(0.271)	(0.260)	(0.274)
Conflict over government (vs. territory)	–0.439**	–0.443**	–0.452**	–0.479**	–0.447**	–0.488**
	(0.143)	(0.148)	(0.145)	(0.149)	(0.145)	(0.150)
Group is excluded	–0.148	–0.112	–0.152	–0.152	–0.149	–0.147
	(0.133)	(0.131)	(0.134)	(0.132)	(0.133)	(0.130)
Democracy dummy	0.251	0.232	0.240	0.194	0.243	0.180
	(0.183)	(0.191)	(0.185)	(0.191)	(0.186)	(0.194)
GDP p.c. growth	1.031	1.384*	1.046+	1.254*	1.052+	1.242+
	(0.627)	(0.645)	(0.623)	(0.629)	(0.631)	(0.641)
log(GDP p.c.)	–0.144	–0.126	–0.162+	–0.133	–0.156+	–0.124
	(0.088)	(0.083)	(0.091)	(0.090)	(0.094)	(0.092)
log(Population)	–0.135*	–0.175*	–0.150*	–0.175*	–0.146*	–0.168*
	(0.069)	(0.070)	(0.068)	(0.069)	(0.071)	(0.073)
Group is terrorist group	–0.154	–0.162	–0.174	–0.178	–0.166	–0.178
	(0.145)	(0.142)	(0.143)	(0.142)	(0.143)	(0.141)
Mountainous terrain index	–0.319*	–0.317*	–0.333*	–0.321*	–0.328*	–0.317*
	(0.155)	(0.150)	(0.154)	(0.145)	(0.153)	(0.143)
Cold War dummy	–0.907**	–0.889**	–0.958**	–0.955**	–0.943**	–0.940**
	(0.128)	(0.128)	(0.121)	(0.127)	(0.125)	(0.128)
No. of observations	1683	1683	1683	1683	1683	1683
No. of conflict dyads	418	418	418	418	418	418
No. of failures	378	378	378	378	378	378

Note: * (+, **) indicates $p < .05$ (.1, .01)

also statistically significant, with a negative coefficient. Thus, the total effect depends on the distance of the conflict to the government stronghold: at distances less than 182 km, arms imports lead to a higher number of deaths; at distances above this threshold, the effect reverses—and both only occur when the rebels are the relatively weaker side.

The latter effect of armament decreasing the death toll in conflicts at larger distances can be explained by the theoretical ramifications provided when analyzing duration: the impact on conflict duration overall outweighs the potentially intensity-increasing impact (cf. Mehrl and Thurner 2020). Explaining the opposing effect at shorter distances requires a deeper foundation of this empirical approach in theoretical frameworks.

This short analysis shows that the answers to the questions of "does X cause conflicts to go longer or to intensify" and "does X cause more deaths until the conflict ends" might very well be two (or three) different answers. Thus, I believe such a casualties-as-time approach could prove valuable also for different issues in conflict research.

Appendix

A.1 Details and technical assumptions of the war-of-attrition model

Each player quits the conflict when their marginal costs exceed the expected marginal gains. The marginal gain will be the valuation of winning, but only when the adversary quits in the next instance. The expected marginal gain is thus w_i times the probability the adversary will quit in the next instance, given it has not quit earlier. Over time, the marginal gain decreases as the expected value for the adversary's valuation rises: when time passes, the other side would drop out of the conflict if it had a lower valuation.

The optimal quitting times are derived by maximizing the expected utility $U_i(t_g, t_r)$ the parties receive from the game, which depends on who quits first: the winner, i.e. the party lasting longer in the conflict, gets their w_i, while the quitting opponent gets nothing. When both stop simultaneously, both get half of their valuation of winning. According to Powell (2017: 221), the government's utility is thus given by

$$U_g(t_g, t_r) = \begin{cases} -c_g t_g & \text{if } \ t_g < t_r \\ w_g/2 - c_g t_g & \text{if } \ t_g = t_r \\ w_g - c_g t_g & \text{if } \ t_g > t_r \ . \end{cases} \tag{13}$$

The rebels' utility function $U_r(t_g, t_r)$ follows analogously.

To be able to derive a unique solution for the optimal quitting times, Powell introduces two technical assumptions about these valuations: first, w_i is expected by the other side to be distributed exponentially with $G(w_i) = 1 - e^{\underline{w}_i - w_i}$. For clarity, we additionally assume that the lowest possible valuation $\underline{w}_i = 0$. Second, there exists a very large $\overline{w}_i$, expected to occur with an "arbitrarily small probability", above which the players would behave nonstrategic, always fighting until the other player ends the conflict (cf. ibid.: 222, originally introduced as an assumption by Fudenberg and Tirole 1986). To ensure the equation system is solvable, we assume $\overline{w}_i$ is common knowledge for both players and when $w_g = \overline{w}_g$ and $w_r = \overline{w}_r$, they both would stop at the same time, which is also known to both sides. Using these assumptions, the optimal quitting times $\sigma_{g,r}$ can be derived as described by Powell (2017: 232f.).

A.2 Deriving comparative statics for valuations

Equations 1 and 2, defining the quitting times of the government and the rebel side, respectively, depend not only on the costs of the two fighting parties, but also on their respective valuation of winning. Since we take these valuations as exogenous parameters, we can determine the effects of changes in these valuations. The comparative statics are thus for the government's valuation w_g

$$\frac{\partial \sigma_g^*(w_g)}{\partial w_g} = \frac{\overline{w}_r \overline{w}_g}{c_g + c_r} \left(\frac{w_g}{\overline{w}_g}\right)^{c_g/c_r} \frac{1 + c_g/c_r}{\overline{w}_g} \tag{14}$$

$$= \frac{\overline{w}_r}{c_r} \left(\frac{w_g}{\overline{w}_g}\right)^{c_g/c_r} > 0 \tag{15}$$

and for the rebels' valuation w_r

$$\frac{\partial \sigma_r^*(w_r)}{\partial w_r} = \frac{\overline{w}_g}{c_g} \left(\frac{w_r}{\overline{w}_r}\right)^{c_r/c_g} > 0 \tag{16}$$

Both comparative statics showing positive effects leads to the following corollary:

Corollary 4. *A higher valuation of winning on the rebels or government side leads to longer conflicts when the respective party's quitting time determines the conflict's ending.*

A.3 Deriving hypotheses from the original model in Powell (2017)

In the Bayesian game in Powell (ibid.), duration is not deterministically stated by the two optimal quitting times like it is the case in our simpler model, but by forming the expected duration over the course of the game, taking Bayesian updating of the stopping decision into account. The valuations of winning in this original version of the framework are random variables. As the following is all provided in Powell (ibid.) and the accompanying online appendix, only the relevant results are laid out here.

Expected duration D is given by

$$D = \int_0^\infty t dF(t, \rho, k) \tag{17}$$

with

$$F(t, \rho, k) = [1 - e^{\underline{w}_g + \underline{w}_r - \tau_g(t) - \tau_r(t)}]/R(\underline{w}_g, \underline{w}_r) \tag{18}$$

being the probability that fighting ends at or before t, with $\tau_i(t)$ being the type of valuation that stops at t and

$$R(\underline{w}_r, \underline{w}_g) = e^{w_r + w_g - \underline{w}_r - \underline{w}_g} - e^{\underline{w}_r + \underline{w}_g - \overline{w}_r - \overline{w}_g} \tag{19}$$

being the probability that $w_r > \underline{w}_r, w_g > \underline{w}_g$ and at least one type is strategic.

Because Powell designs this as a Bayesian game expectations are built as the best response for each "type" of opponent. In this game, the different types are discerned by their valuation of winning w_i, and the best responses $\tau_i(t)$ are derived by maximizing the individual expected utility as described in section A.1 above.

Given the expected duration D, we can derive the relevant comparative statics with respect to ρ. Powell (ibid.) shows in the accompanying online appendix that $\partial D/\partial\rho > 0$. It suffices to prove that the cumulative density function of the expected duration $F(t, \rho, k)$, i.e. the probability that fighting ends at or before t, first-order stochastically dominates $F(t, \rho', k)$ for $\rho > \rho'$. Using $\underline{w}_r = 0, \underline{w}_g = 0$, differentiation gives

$$\operatorname{sgn}\Big\{\frac{\partial F(t,\rho,k)}{\partial\rho}\Big\} = \operatorname{sgn}\Big\{\overline{w}_g\left(\frac{kt}{\overline{w}_g\overline{w}_r}\right)^{1/(1+\rho)} - \overline{w}_r\left(\frac{kt}{\overline{w}_g\overline{w}_r}\right)^{\rho/(1+\rho)}\Big\}. \tag{20}$$

A negative sign arises when

$$\left(\frac{\overline{w}_g\overline{w}_r}{kt}\right)^{\frac{1-\rho}{1+\rho}} > \frac{\overline{w}_g}{\overline{w}_r}. \tag{21}$$

Powell assumes that $1 > \rho > \overline{w}_g/\overline{w}_r$, which prevents a change of the relative position over the course of the game (with the rebels starting as weaker but becoming the stronger party over time). Additionally, he shows that $t < \sigma_i(\overline{w}_i = \overline{w}_g\overline{w}_r/k)$, limiting the time t to the duration the types with the highest valuation stop at. Then, we can conclude that

$$\left(\frac{\overline{w}_g\overline{w}_r}{kt}\right)^{\frac{1-\rho}{1+\rho}} > 1 > \rho > \frac{\overline{w}_g}{\overline{w}_r}. \tag{22}$$

This proofs that

$$\frac{\partial F(t,\rho,k)}{\partial\rho} < 0, \tag{23}$$

meaning that the expected duration decreases with the relative strength of the government.

Note that in this Bayesian setting, no comparative statics with respect to w_i are possible, as only a specific set of types $(w_r, w_g = \overline{w}_g(w_r/\overline{w}_r)^{\rho})$ only ever fights a positive duration, i.e. even starts fighting.

A.4 Details on the dyadic dataset design

While each observation in the dataset is usually a dyad-year, the first and last observation of a conflict dyad can comprise less than one year of duration when the conflict dyad's start date is after January 1 and when the end date is before December 31. As usual in dyadic analyses, the unit of time in our analyses is days, not years, mainly to account for shorter conflicts.

Using the duration of uninterrupted conflict spells for our analyses follows the data provided by the Armed Conflict Dataset from the Uppsala Conflict Data Program/Peace Research Institute Oslo (UCDP/PRIO, cf. Harbom et al. 2008; Pettersson and Öberg 2020) on the duration of conflicts and the respective warring parties. Our analyses cover 134 unique conflicts between 1951 and 2010, with durations of conflict-dyad episodes ranging from 1 day to 14732 days.

UCDP/PRIO record a conflict when it reaches at least 25 battle-related deaths in a calendar year. To avoid bias regarding conflicts that never reach 25 deaths and are therefore not part of the dataset, we use the second start date provided by the Armed Conflict Dataset, which marks the date the 25 battle-deaths threshold is reached. Extra-territorial conflicts are excluded.

A.5 First stage results

While the instrumental variables' exogeneity cannot be proven empirically, we can provide evidence of their relevance. Table A1 shows the results of the OLS regressions at the first stage of the IV model estimation (see section 6.2 for details). The three variables of MCW imports are the respective dependent variables. As independent variables, the instrument, the instrument's respective interactions and all control variables of the second stage are included. For all models, we find that civil-conflict-unrelated MCW imports are significantly positively associated with a country's imports. To better illustrate instrument relevance, the even models exclude the interaction terms (but those are not used for second-stage estimation).

A.6 Marginal effects

Figure A1 illustrates the marginal effects of arms imports. Using model 3 of table 2 (Cox IV with 5-year averaged arms imports), the marginal effects are displayed for all observations where the rebels are the relatively weaker party. As can be seen, for log(Distance) values of 12.76 and above, i.e. a distance of 348 km and higher, the marginal effect of arms imports is positive, indicating a duration-reducing effect of arms imports.

A.7 Additional specifications

To reflect on additional, theoretically motivated issues, tables A2 and A3 present six models specified with different sets of covariates. While the first one delivers results on the sample restricted to relatively weaker rebels, the second one employs the full sample.

Table A1: First stage results

	MCW imports (lagged)		MCW imports (3 yr. avg.)		MCW imports (5 yr. avg.)	
	Full spec.	W/o interactions	Full spec.	W/o interactions	Full spec.	W/o interactions
	(1)	(2)	(3)	(4)	(5)	(6)
log(MCW imports, unrelated), lagged	0.989***	0.395***				
	(0.342)	(0.020)				
log(MCW imports, unrelated), 3 yr. avg.			1.909***	0.538***		
			(0.300)	(0.018)		
log(MCW imports, unrelated), 5 yr. avg.					2.228***	0.611***
					(0.257)	(0.017)
log(MCW imports, unrelated) × log(distance)	−0.048*		−0.107***		−0.126***	
	(0.027)		(0.024)		(0.020)	
log(MCW imports, unrelated) × weaker-rebels dummy	−1.331***		−1.552***		−1.484***	
	(0.433)		(0.390)		(0.344)	
Unrelated MCW × log(distance) × weaker-rebels dummy	0.107***		0.120***		0.115***	
	(0.034)		(0.030)		(0.027)	
Rebels weaker dummy	7.411***		6.144***		5.319***	
	(2.074)		(1.752)		(1.585)	
log(Distance)	0.489***	0.305***	0.508***	0.219***	0.560***	0.212***
	(0.095)	(0.068)	(0.077)	(0.051)	(0.068)	(0.044)
log(distance) × weaker-rebels dummy	−0.597***		−0.494***		−0.427***	
	(0.164)		(0.138)		(0.125)	
External support (weapons)	0.008	0.051	0.018	0.045	0.024	0.053
	(0.155)	(0.154)	(0.116)	(0.116)	(0.098)	(0.099)
External support (troops)	0.883***	0.929***	0.721***	0.823***	0.386**	0.496**
	(0.304)	(0.303)	(0.227)	(0.227)	(0.193)	(0.194)
Battle-related deaths per day_thds	0.217	0.226	0.215**	0.223**	0.211**	0.219**
	(0.142)	(0.142)	(0.106)	(0.107)	(0.090)	(0.091)
Conflict over government (vs. territory)	0.605***	0.696***	0.646***	0.761***	0.590***	0.694***
	(0.212)	(0.208)	(0.159)	(0.157)	(0.135)	(0.134)
Group is excluded	−0.030	0.001	0.089	0.114	0.093	0.115
	(0.193)	(0.193)	(0.145)	(0.145)	(0.123)	(0.124)
Democracy dummy	0.066	0.179	0.331**	0.357**	0.474***	0.465***
	(0.211)	(0.207)	(0.158)	(0.156)	(0.134)	(0.133)
GDP p.c. growth	2.615***	2.734***	0.867	1.101*	0.713	0.885
	(0.888)	(0.884)	(0.664)	(0.664)	(0.564)	(0.568)
log(GDP p.c.)	0.756***	0.719***	0.454***	0.422***	0.275***	0.257***
	(0.095)	(0.093)	(0.073)	(0.072)	(0.063)	(0.062)
log(Population)	0.357***	0.390***	0.260***	0.246***	0.175***	0.151***
	(0.082)	(0.078)	(0.063)	(0.061)	(0.054)	(0.053)
Group is terrorist group	0.096	0.190	0.106	0.190	0.028	0.105
	(0.169)	(0.165)	(0.126)	(0.124)	(0.106)	(0.106)
Mountainous terrain	0.467***	0.444***	0.297***	0.278***	0.247***	0.229***
	(0.118)	(0.118)	(0.088)	(0.089)	(0.075)	(0.076)
Cold War dummy	1.028***	1.120***	0.819***	0.920***	0.826***	0.912***
	(0.174)	(0.169)	(0.131)	(0.128)	(0.111)	(0.109)
Constant	−17.422***	−15.503***	−13.032***	−9.179***	−10.455***	−5.821***
	(2.069)	(1.868)	(1.623)	(1.453)	(1.415)	(1.268)
Observations	1,683	1,683	1,683	1,683	1,683	1,683
R^2	0.430	0.425	0.593	0.586	0.681	0.670
Adjusted R^2	0.423	0.420	0.589	0.582	0.677	0.667
F Statistic	65.991***	87.937***	127.684***	168.348***	186.723***	241.792***

Note: *p<0.1; **p<0.05; ***p<0.01

Table A2: Robustness results – rebels-weaker sample

	Sq. troop ratio	Arms types split		Rebel MCW	Mil. exp.	Conflict-level
	(1)	(2)	(3)	(4)	(5)	(6)
log(Distance)	–0.284**	–0.264**	–0.241**	–0.290**	–0.260**	–0.184**
	(0.067)	(0.078)	(0.074)	(0.068)	(0.076)	(0.052)
log(MCW imports), lagged	–0.287**			–0.294*	–0.327*	–0.163*
	(0.111)			(0.115)	(0.128)	(0.067)
log(MCW imports), lagged × log(Distance)	0.023**			0.024**	0.027**	0.013*
	(0.009)			(0.009)	(0.010)	(0.006)
log(Ground imports), lagged		–1.173*				
		(0.459)				
log(Ground imports), lagged × log(Distance)		0.087*				
		(0.035)				
log(Air imports), lagged			–0.456*			
			(0.229)			
log(Air imports), lagged × log(Distance)			0.037*			
			(0.018)			
log(Troop ratio)	–0.297	0.155*	0.165*	0.168*	0.154*	0.197*
	(0.653)	(0.074)	(0.071)	(0.072)	(0.071)	(0.084)
log(Troop ratio), squared	0.022					
	(0.031)					
log(MCW imports of rebel group), 5 yr. avg.				–0.257		
				(0.655)		
log(Military expenditures)					–0.140**	
					(0.054)	
No. of dyads in conflict-year						–0.600
						(0.450)
Group receives weapons support	–0.179	–0.241	–0.183	–0.181	–0.193	–0.381+
	(0.167)	(0.169)	(0.169)	(0.176)	(0.180)	(0.217)
Group receives troops support	0.252	0.243	0.242	0.242	0.402	0.011
	(0.219)	(0.224)	(0.215)	(0.224)	(0.274)	(0.918)
Battle-related deaths per day in thousands	0.289**	0.306**	0.289**	0.287**	1.234*	2.173**
	(0.095)	(0.102)	(0.095)	(0.095)	(0.511)	(0.544)
Conflict over government (vs. territory)	–0.429*	–0.359+	–0.421*	–0.429*	–0.453+	–0.217
	(0.197)	(0.204)	(0.201)	(0.208)	(0.234)	(0.213)
Group is excluded	–0.441*	–0.478*	–0.424*	–0.444*	–0.410+	–0.428*
	(0.192)	(0.193)	(0.192)	(0.201)	(0.215)	(0.212)
Democracy dummy	–0.459*	–0.571*	–0.469*	–0.439+	–0.441+	–0.770**
	(0.227)	(0.226)	(0.225)	(0.227)	(0.234)	(0.262)
GDP p.c. growth	1.188	0.814	1.096	1.157	0.306	–0.498
	(1.324)	(1.304)	(1.311)	(1.323)	(1.359)	(1.055)
log(GDP p.c.)	–0.015	0.035	–0.008	–0.020	0.155	0.024
	(0.089)	(0.091)	(0.089)	(0.090)	(0.115)	(0.101)
log(Population)	–0.087	–0.060	–0.075	–0.089	–0.001	–0.066
	(0.068)	(0.077)	(0.068)	(0.070)	(0.069)	(0.085)
Group is terrorist group	–0.227	–0.265	–0.233	–0.231	–0.250	–0.377+
	(0.177)	(0.185)	(0.176)	(0.177)	(0.181)	(0.221)
Mountainous terrain index	0.011	0.037	0.021	0.019	–0.023	–0.007
	(0.139)	(0.133)	(0.139)	(0.138)	(0.161)	(0.160)
Cold War dummy	–0.370*	–0.338+	–0.370*	–0.352*	–0.450*	–0.701**
	(0.175)	(0.178)	(0.176)	(0.175)	(0.191)	(0.255)
No. of observations	812	798	812	812	743	669
No. of conflict dyads/conflicts (model 6)	235	233	235	235	213	165
No. of failures	190	188	190	190	173	121

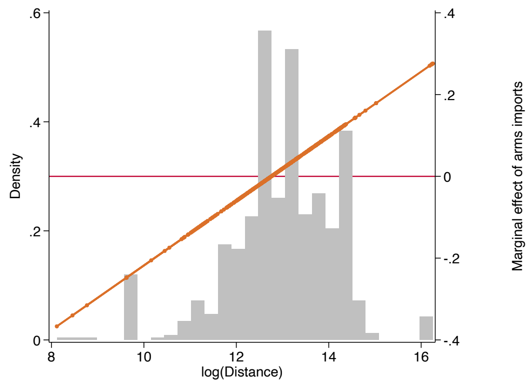
Note: * (+, **) indicates $p < .05$ (.1, .01)

Table A3: Robustness results – complete sample

	Sq. troop ratio	Arms types split		Rebel MCW	Mil. exp.	Conflict-level
	(1)	(2)	(3)	(4)	(5)	(6)
log(Distance)	–0.193**	–0.195**	–0.195**	–0.197**	–0.177**	–0.207**
	(0.032)	(0.032)	(0.031)	(0.032)	(0.035)	(0.037)
log(MCW imports), lagged	–0.172*			–0.179**	–0.192*	–0.184**
	(0.068)			(0.069)	(0.075)	(0.067)
log(MCW imports), lagged × log(Distance)	0.013*			0.014*	0.016*	0.014*
	(0.006)			(0.006)	(0.006)	(0.006)
log(Ground imports), lagged		–0.792**				
		(0.285)				
log(Ground imports), lagged × log(Distance)		0.059**				
		(0.022)				
log(Air imports), lagged			–0.426**			
			(0.159)			
log(Air imports), lagged × log(Distance)			0.032*			
			(0.013)			
log(Troop ratio)	0.015	0.115**	0.121**	0.119**	0.113**	0.160**
	(0.132)	(0.037)	(0.037)	(0.036)	(0.038)	(0.047)
log(Troop ratio), squared	0.006					
	(0.008)					
log(MCW imports of rebel group), 5 yr. avg.				0.147		
				(0.174)		
log(Military expenditures)					–0.079	
					(0.048)	
No. of dyads in conflict-year						–0.756**
						(0.215)
Group receives weapons support	–0.368**	–0.396**	–0.362**	–0.377**	–0.352*	–0.458**
	(0.123)	(0.124)	(0.124)	(0.125)	(0.141)	(0.157)
Group receives troops support	0.525**	0.553**	0.562**	0.538**	0.580**	0.434+
	(0.152)	(0.157)	(0.156)	(0.153)	(0.162)	(0.261)
Battle-related deaths per day in thousands	0.199**	0.204**	0.202**	0.198**	0.982**	0.443*
	(0.024)	(0.023)	(0.024)	(0.023)	(0.215)	(0.175)
Conflict over government (vs. territory)	–0.222+	–0.191	–0.223+	–0.220+	–0.288*	–0.189
	(0.122)	(0.126)	(0.122)	(0.121)	(0.138)	(0.155)
Group is excluded	–0.277*	–0.303*	–0.276*	–0.270*	–0.374**	–0.314*
	(0.120)	(0.124)	(0.119)	(0.123)	(0.142)	(0.148)
Democracy dummy	–0.241	–0.284	–0.218	–0.226	–0.201	–0.351+
	(0.171)	(0.177)	(0.168)	(0.170)	(0.189)	(0.206)
GDP p.c. growth	0.331	0.331	0.260	0.308	1.338+	–0.235
	(0.567)	(0.573)	(0.557)	(0.556)	(0.812)	(0.629)
log(GDP p.c.)	–0.037	–0.028	–0.036	–0.042	0.023	–0.024
	(0.064)	(0.066)	(0.066)	(0.065)	(0.086)	(0.070)
log(Population)	–0.114*	–0.109+	–0.109+	–0.109+	–0.094+	–0.175**
	(0.056)	(0.060)	(0.057)	(0.057)	(0.054)	(0.064)
Group is terrorist group	–0.368**	–0.388**	–0.373**	–0.373**	–0.399**	–0.483**
	(0.123)	(0.124)	(0.122)	(0.123)	(0.125)	(0.151)
Mountainous terrain index	–0.192+	–0.176+	–0.190+	–0.194+	–0.128	–0.082
	(0.110)	(0.105)	(0.107)	(0.110)	(0.103)	(0.120)
Cold War dummy	–0.472**	–0.482**	–0.476**	–0.484**	–0.519**	–0.588**
	(0.135)	(0.123)	(0.129)	(0.135)	(0.139)	(0.166)
No. of observations	1683	1669	1683	1683	1380	1339
No. of conflict dyads/conflicts (model 6)	418	416	418	418	342	257
No. of failures	378	376	378	378	306	220

Note: * (+, **) indicates $p < .05$ (.1, .01)

Figure A1: Marginal effects of arms imports



The orange graph plots the marginal effect of arms imports in model (3) of table 2 depending on distance, when the rebels are coded weaker than in the median. The grey histogram displays the density of distances in the data.

Model 1 examines whether the theoretically derived effects of asymmetry in military capabilities can be found in the measure of troop ratio. In contrast to the other variables, we have data available to capture the military capabilities of the government and rebel side in relative terms: the troop ratio is an accurate measure of relative capabilities. It should thus exhibit the asymmetric effects found in the war-of-attrition duration framework. However, when including the squared troop ratio, the coefficients of both the regular and squared variables lose statistical significance. Nevertheless, this is most likely a consequence of the very limited number of cases where the overall cost ratio favors the rebel side. Arms imports' effects are nonetheless robust to implementing a non-linear impact of the troop ratio.

As argued in section 3.4, arms of different types might provide specific advantages in different conflict environments at different distances. Thus, models 2 and 3 review hypothesis 1a for subcategories of arms imports, i.e. for imports of ground equipment like tanks, armored vehicles and artillery and imports of air weapon systems like aircraft, helicopters and missiles, respectively. Results differ only slightly, corroborating the prediction of hypothesis 1a—that imported major weapons are effective tools to end conflicts at larger distances—for both types of ground and air imports separately as well.

Models 4 and 5 include additional measures of rebel and government capabilities, respectively. Following Moore (2012), model 4 uses SIPRI data on major weapons imports of rebel groups. Their effect is not statistically significant—most likely, this is not a substantial finding on the (non-)impact of rebels' armaments, but is a result of only very limited information on the arms trade to rebels, as is comprised in the SIPRI data. Model 5 includes military expenditures as a direct financial measure of potential conflict costs. They do exhibit a statistically significant prolonging impact on conflict duration—but only in the restricted sample, not for the full-sample model.

Finally, model 6 shifts the level of analysis—instead of analyzing conflict dyads, its unit of analysis is the conflict, consisting of one or multiple dyads. Notably, the effects of arms imports are robust to changing the duration specification to the duration of either a conflict dyad or a potentially multi-dyadic conflict. To test this, model 6 also includes a dummy of whether the conflict comprises multiple dyads in the year of observation, showing that multi-dyadic conflicts have longer durations than single-dyadic conflicts, in line with results from Cunningham et al. (2009)—but only for the full sample of all conflicts.

A.8 Estimation results for full and split sample with two-way interaction

To corroborate the robustness of our main results, we provide results with differing estimation methods and specifications. Tables A4 and A5 show that the results of the main models do not hinge on a sample restriction or the three-way interaction of armament, distance and the rebel strength dummy. Table A4 delivers results for a Cox estimation with just the two-way interaction of armament and distance for the sample where the rebels are relatively weaker. This is in line with the main Aalen results (see A.9.2), which are also derived from this restricted sample.

At the same time, the found effects do not require restricting the sample or implementing the rebel strength dummy: table A5 provides the estimations of the corresponding models of the main tables, but with only the two-way interaction of arms imports and distance.

Over all models, the main models' results are confirmed, with coefficients for the arms imports and distance variables being negative and statistically significant, when their interaction is included. This interaction variable, in turn, consistently exhibits a significant positive sign. Thus, the results presented above do not hinge on the choice of the estimator, assumptions on the proportionality or additive nature of the hazards or conditioning on the relative weakness of the rebel side.

A.9 Aalen's Additive Hazards model

When implementing an instrumental variable approach for survival analysis, the widely used Cox Proportional Hazards model would be a natural starting point. However, the non-collapsibility of the Cox model leads to inconsistent results when employing the standard two-stage estimation (Martinussen and Vansteelandt 2013).

Building on work from Li et al. (2015), Tchetgen Tchetgen et al. (2015) and Lergenmuller (2017) propose to use the Aalen Additive Hazards model instead, allowing for

Table A4: Cox estimation results - rebels-weaker sample, two-way interaction

	Lagged		3-year avg.		5-year avg.	
	(1)	(2)	(3)	(4)	(5)	(6)
log(Distance)	–0.105*	–0.289**	–0.105*	–0.400**	–0.105*	–0.407**
	(0.047)	(0.068)	(0.047)	(0.122)	(0.047)	(0.142)
log(Troop ratio)	0.172*	0.167*	0.174*	0.166*	0.174*	0.161*
	(0.073)	(0.072)	(0.073)	(0.073)	(0.072)	(0.073)
log(MCW imports)	0.006	–0.291*	0.000	–0.460*	0.001	–0.475*
	(0.023)	(0.114)	(0.023)	(0.185)	(0.027)	(0.224)
log(MCW imports) x log(Distance)		0.024**		0.036*		0.037*
		(0.009)		(0.014)		(0.017)
Group receives weapons support	–0.193	–0.189	–0.191	–0.194	–0.191	–0.196
	(0.167)	(0.169)	(0.168)	(0.168)	(0.168)	(0.168)
Group receives troops support	0.238	0.253	0.243	0.274	0.242	0.257
	(0.209)	(0.216)	(0.208)	(0.214)	(0.210)	(0.214)
Battle-related deaths per day in thousands	0.284**	0.286**	0.289**	0.297**	0.288**	0.295**
	(0.097)	(0.095)	(0.097)	(0.096)	(0.098)	(0.097)
Conflict over government (vs. territory)	–0.370+	–0.416*	–0.371+	–0.448*	–0.371+	–0.447*
	(0.192)	(0.198)	(0.191)	(0.203)	(0.191)	(0.203)
Group is excluded	–0.425*	–0.434*	–0.426*	–0.449*	–0.426*	–0.440*
	(0.193)	(0.193)	(0.191)	(0.190)	(0.191)	(0.191)
Democracy dummy	–0.460*	–0.446*	–0.459*	–0.491*	–0.460*	–0.490*
	(0.231)	(0.227)	(0.230)	(0.226)	(0.232)	(0.227)
GDP p.c. growth	1.031	1.159	0.991	0.919	0.994	0.889
	(1.318)	(1.323)	(1.309)	(1.302)	(1.324)	(1.317)
log(GDP p.c.)	–0.022	–0.021	–0.016	0.014	–0.016	0.014
	(0.089)	(0.089)	(0.086)	(0.089)	(0.087)	(0.089)
log(Population)	–0.049	–0.084	–0.044	–0.065	–0.044	–0.063
	(0.071)	(0.067)	(0.067)	(0.066)	(0.070)	(0.071)
Group is terrorist group	–0.192	–0.233	–0.188	–0.251	–0.188	–0.252
	(0.169)	(0.177)	(0.169)	(0.181)	(0.168)	(0.182)
Mountainous terrain index	–0.016	0.018	–0.013	0.014	–0.013	0.010
	(0.141)	(0.138)	(0.140)	(0.134)	(0.140)	(0.136)
Cold War dummy	–0.298+	–0.352*	–0.291+	–0.370*	–0.291+	–0.365*
	(0.171)	(0.175)	(0.164)	(0.173)	(0.164)	(0.172)
No. of observations	812	812	812	812	812	812
No. of conflict dyads	235	235	235	235	235	235
No. of failures	190	190	190	190	190	190

Note: * (+, **) indicates $p < .05$ (.1, .01)

Table A5: Cox estimation results – complete sample, two-way interaction

	Lagged		3-year avg.		5-year avg.	
	(1)	(2)	(3)	(4)	(5)	(6)
log(Distance)	–0.131**	–0.197**	–0.133**	–0.205**	–0.132**	–0.212**
	(0.029)	(0.032)	(0.030)	(0.035)	(0.030)	(0.038)
log(MCW imports), lagged	–0.013	–0.182**				
	(0.017)	(0.069)				
log(MCW imports), lagged × log(Distance)		0.014*				
		(0.006)				
log(MCW imports), 3 yr. avg.			–0.010	–0.175**		
			(0.020)	(0.068)		
log(MCW imports), 3 yr. avg. × log(Distance)				0.014*		
				(0.006)		
log(MCW imports), 5 yr. avg.					–0.018	–0.190*
					(0.023)	(0.079)
log(MCW imports), 5 yr. avg. × log(Distance)						0.014*
						(0.006)
log(Troop ratio)	0.116**	0.118**	0.116**	0.116**	0.118**	0.118**
	(0.037)	(0.036)	(0.038)	(0.037)	(0.038)	(0.037)
Group receives weapons support	–0.365**	–0.371**	–0.365**	–0.368**	–0.364**	–0.367**
	(0.122)	(0.124)	(0.122)	(0.123)	(0.123)	(0.123)
Group receives troops support	0.533**	0.537**	0.530**	0.546**	0.535**	0.550**
	(0.150)	(0.154)	(0.153)	(0.155)	(0.152)	(0.155)
Battle-related deaths per day in thousands	0.192**	0.197**	0.192**	0.196**	0.194**	0.199**
	(0.023)	(0.024)	(0.021)	(0.021)	(0.022)	(0.022)
Conflict over government (vs. territory)	–0.213+	–0.221+	–0.216+	–0.218+	–0.213+	–0.215+
	(0.117)	(0.121)	(0.117)	(0.121)	(0.116)	(0.121)
Group is excluded	–0.270*	–0.276*	–0.270*	–0.276*	–0.271*	–0.275*
	(0.118)	(0.121)	(0.119)	(0.123)	(0.118)	(0.122)
Democracy dummy	–0.238	–0.221	–0.238	–0.230	–0.235	–0.234
	(0.172)	(0.170)	(0.173)	(0.172)	(0.173)	(0.171)
GDP p.c. growth	0.131	0.287	0.111	0.217	0.097	0.189
	(0.558)	(0.562)	(0.569)	(0.571)	(0.576)	(0.578)
log(GDP p.c.)	–0.052	–0.044	–0.057	–0.050	–0.048	–0.041
	(0.067)	(0.065)	(0.069)	(0.068)	(0.070)	(0.068)
log(Population)	–0.088	–0.113*	–0.089	–0.109+	–0.082	–0.096+
	(0.059)	(0.056)	(0.058)	(0.057)	(0.059)	(0.058)
Group is terrorist group	–0.366**	–0.375**	–0.366**	–0.368**	–0.366**	–0.365**
	(0.123)	(0.122)	(0.123)	(0.123)	(0.122)	(0.122)
Mountainous terrain index	–0.218*	–0.195+	–0.223*	–0.210+	–0.219*	–0.210+
	(0.110)	(0.111)	(0.110)	(0.109)	(0.110)	(0.108)
Cold War dummy	–0.464**	–0.479**	–0.471**	–0.489**	–0.453**	–0.464**
	(0.132)	(0.134)	(0.131)	(0.132)	(0.135)	(0.136)
No. of observations	1683	1683	1683	1683	1683	1683
No. of conflict dyads	418	418	418	418	418	418
No. of failures	378	378	378	378	378	378

Note: * (+, **) indicates $p < .05$ (.1, .01)

a consistent two-stage instrumental variable estimator. In contrast to the Cox model, the Aalen Additive Hazards model has no history of usage in Political Science. However, it is a commonly used model in Biology and Health statistics. The covariates' influence is not represented in proportion to the baseline hazard, as is the case in the Cox model, but as an additive contribution to the hazard. The functional form of the hazard $h_i(t)$ is specified as

$$h_i(t) = h_0(t) + \boldsymbol{\beta' X_i}(t) + \boldsymbol{\gamma' W_i}(t), \tag{24}$$

where $h_0(t)$ is the baseline hazard, $\boldsymbol{X_i}(t)$ and $\boldsymbol{W_i}(t)$ are the vector of conflict dyad i's main and control covariates, respectively, and $\boldsymbol{\beta}$ and γ are their respective coefficient vectors. Due to this functional form, the baseline hazard is a parameter that has to be estimated. Since tests for time-invariant effects of our variables of interest do not reject the hypothesis of the coefficients not varying over time, all coefficients are estimated as constant.

A.9.1 Implementing a two-stage instrumental variable survival model with Aalen's Additive Hazards model

In a two-step instrumental variable survival model, the first stage uses OLS to regress the endogenous variables on the exogenous regressors and the instruments. The predicted values of the first-stage regressions are then in the second stage used in the Aalen survival model, instead of the endogenous covariates (Tchetgen Tchetgen et al. 2015; Lergenmuller 2017). In our analyses, the vector of instruments $\boldsymbol{Z_i}$ comprises the arms imports unrelated to civil conflict and their interaction with distance. The vector of endogenous covariates $\boldsymbol{X_i}$ consists of the arms imports variable as well as their interaction term with distance. Note that we avoid the three-way interaction used in the Cox model, as convergence cannot be achieved; instead, for the Aalen estimations, we use only the subsample where the rebel-strength dummy indicates rebels weaker than the government.

The first stage equation is then

$$\boldsymbol{X_i} = \boldsymbol{\delta' Z_i} + \boldsymbol{\lambda' W_i}, \tag{25}$$

where $\boldsymbol{\delta}$ and $\boldsymbol{\lambda}$ are vectors of the instruments' and the exogenous covariates' $\boldsymbol{W_i}$ regression coefficients, respectively. This equation is estimated by standard OLS to obtain the estimated coefficients $\hat{\boldsymbol{\delta}}$ and $\hat{\boldsymbol{\lambda}}$, which are then used to predict the values of the endogenous covariates:

$$\hat{\boldsymbol{X}}_{\boldsymbol{i}} = \hat{\boldsymbol{\delta}} \boldsymbol{Z_i} + \hat{\boldsymbol{\lambda}} \boldsymbol{W_i} \tag{26}$$

In the second stage, the predicted values $\hat{\boldsymbol{X}}_{\boldsymbol{i}}$—instead of the observed values $\boldsymbol{X}_{\boldsymbol{i}}$—enter the Aalen model equation as covariates:

$$h_i(t) = h_0(t) + \boldsymbol{\theta}\hat{\boldsymbol{X}}_{\boldsymbol{i}}(t) + \boldsymbol{\beta'}\boldsymbol{W}_{\boldsymbol{i}}(t), \tag{27}$$

where $\boldsymbol{\theta}$ is the endogenous variables' and $\boldsymbol{\beta}$ the exogenous covariates' vector of coefficients, respectively.[16]

A.9.2 Empirical results from the Aalen IV model

Table A6 presents the results of models with different specifications of arms imports as lagged (models 1 and 2), 3-year averaged (models 3 and 4), and 5-year averaged variables (models 5 and 6). While odd-numbered models use the standard Aalen Additive Hazards estimator, even-numbered models employ the two-stage instrumental variable Aalen estimator. Note that we only use observations where the rebels are deemed the weaker side, as our war-of-attrition model only for these expects the duration-reducing effect of governmental armament. To account for changes in relative rebel strength, we include the logged troop ratio variable.

The results of the IV approach solidify those of the non-instrumented Aalen and Cox models: arms imports have a causal effect, at least in the 3- and 5-year cumulative imports specifications. Taking the point estimates for model 4, the duration-reducing effects only materialize for larger distances above 686 km.

As with our Cox IV models, we find no evidence for endogenous demand with the Aalen IV models. When comparing the results between the non-instrumented and the IV model, confidence intervals of the relevant coefficients of the arms imports variable and its interaction with distance overlap, meaning the results do not differ statistically significantly.

[16] Again, as for the Cox IV model, standard errors of the second stage's coefficients might need correction, as the uncertainty in estimating the first stage has to be accounted for. Simulations show that default standard errors only slightly underestimate the true uncertainty of the IV estimates, enabling us to draw valid conclusions about statistical significance in the two-stage Aalen IV model (Lergenmuller 2017). Additionally, we calculated standard errors by bootstrapping, but the results do not differ substantially.

Table A6: Aalen IV results – rebels-weaker sample

	Lagged		3-year avg.		5-year avg.	
	Std.	IV	Std.	IV	Std.	IV
	(1)	(2)	(3)	(4)	(5)	(6)
MCW imports	-0.814^{+}	-1.506*	-0.853*	-1.357*	-0.906*	-1.455*
	(0.494)	(0.768)	(0.431)	(0.621)	(0.428)	(0.664)
MCW imports × log(Distance)	0.062^{+}	0.113^{+}	0.064^{+}	0.101*	0.067*	0.108*
	(0.038)	(0.058)	(0.033)	(0.048)	(0.032)	(0.051)
log(Distance)	-0.644^{+}	-0.952*	-0.657*	-0.929*	-0.677*	-1.001*
	(0.374)	(0.438)	(0.327)	(0.395)	(0.306)	(0.424)
log(Troop ratio)	0.183*	0.182*	0.189*	0.186*	0.186*	0.176*
	(0.080)	(0.083)	(0.081)	(0.082)	(0.081)	(0.081)
Group receives weapons support	-0.201	-0.178	-0.207	-0.213	-0.214	-0.238^{+}
	(0.139)	(0.126)	(0.136)	(0.138)	(0.134)	(0.137)
Group receives troops support	0.011	0.214	0.060	0.212	0.063	0.180
	(0.293)	(0.324)	(0.291)	(0.310)	(0.291)	(0.301)
Battle-related deaths per day in thousands	138.336*	135.432*	139.698*	141.732*	140.132*	141.269*
	(64.769)	(64.954)	(64.562)	(64.766)	(61.569)	(65.318)
Conflict over government (vs. territory)	-0.488*	-0.556**	-0.500*	-0.575**	-0.522**	-0.579**
	(0.208)	(0.206)	(0.201)	(0.202)	(0.200)	(0.202)
Group is excluded	-0.356^{+}	-0.402*	-0.370^{+}	-0.427*	-0.392*	-0.413*
	(0.202)	(0.198)	(0.197)	(0.203)	(0.196)	(0.204)
Democracy dummy	-0.324	-0.289	-0.322	-0.335	-0.314	-0.348
	(0.248)	(0.241)	(0.242)	(0.242)	(0.237)	(0.239)
GDP p.c. growth	1.886	2.083	1.823	1.996	1.710	1.841
	(2.030)	(2.014)	(2.015)	(2.023)	(2.057)	(2.028)
log(GDP p.c.)	0.005	0.045	0.024	0.064	0.028	0.073
	(0.108)	(0.107)	(0.104)	(0.106)	(0.107)	(0.107)
log(Population)	-0.089	-0.144	-0.071	-0.083	-0.067	-0.072
	(0.099)	(0.112)	(0.101)	(0.105)	(0.106)	(0.106)
Group is terrorist group	-0.080	-0.094	-0.067	-0.092	-0.072	-0.119
	(0.161)	(0.157)	(0.151)	(0.159)	(0.150)	(0.160)
Mountainous terrain index	-0.050	-0.005	-0.047	-0.042	-0.039	-0.043
	(0.141)	(0.146)	(0.140)	(0.144)	(0.141)	(0.143)
Cold War dummy	-0.601**	-0.602**	-0.579**	-0.599**	-0.575**	-0.591**
	(0.182)	(0.174)	(0.178)	(0.174)	(0.173)	(0.175)
No. of observations	812	812	812	812	812	812
No. of conflict dyads	235	235	235	235	235	235
No. of failures	190	190	190	190	190	190

Note: * ($^{+}$, **) indicates $p < .05$ ($.1$, $.01$). All coefficients and standard errors are multiplied by 10^3.

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Chapter 4

What we know about rebel groups' armament. Collecting evidence for a more comprehensive assessment

Single-authored version of the article, not yet published.

Replication material: www.andreas-mehltretter.de/thesis-replication

Abstract

Little data exists on the military equipment of rebel groups in intrastate conflicts. However, rebels' armament is crucial for understanding onset, intensity, duration and outcome of conflicts. To overcome this deficiency, publicly available information was collected from the NISAT document library, web searches and news agency reports to gain insights into the military technologies available to non-state actors. In the Rebels' Armament Dataset (RAD), evidence on arms usage and possession was assembled for 270 of 345 groups fighting in conflicts between 1991 and 2018, differentiating between 14 discrete categories of small arms, light weapons, explosives and major weapons. In this article, I review the state of knowledge on rebel groups' armament, describe the data collection process and provide an overview of the collected evidence. While only an early assessment that can be extended with the ever-increasing density of public information, pictures and videos, RAD provides for the first time evidence on rebels' armament, showing for instance a wider availability of light weapons and also major weapons than previously assumed.

1 Introduction

Rebels' distinct military capabilities and equipment play little role in most civil conflict research—potentially as a result of missing data. Nevertheless, research has undoubtedly shown that the configuration of military capabilities between the government and a rebel group crucially determines how the conflict is fought out (Kalyvas and Balcells 2010; Balcells and Kalyvas 2014). In essence, Kalyvas and Balcells (2010) argue that conventional warfare with decisive battles in the open field might emerge when both opponents have access to major weapons. When the rebels only have small arms available, though, they might rather choose guerrilla tactics, potentially leading to protracted indecisive fighting.

This postulated relationship between armament and conflict dynamics exemplifies how understanding the extent of the lethality, the duration and outcomes of conflicts requires an encompassing knowledge of both sides' fighting equipment. Unfortunately, such data is only available for the government side so far. Analyses on conflict duration and intensity, for instance, can use precise measures of the government's military capabilities, e.g. in the form of arms imports data provided by the Stockholm International Peace Research Institute (SIPRI) (cf. e.g. Mehrl and Thurner 2020; Mehltretter and Thurner 2023/chapter 3), but have to rely on a mostly dichotomous assessment of rebels' "arms procurement capacity" from the Non-State Actors dataset (Cunningham et al. 2013), simple troop counts or a coarse ordinal measure of "rebel strength," without any information on the types of arms these groups are using.

Without proper data, only a few studies have emerged that seek to take rebel groups' armaments explicitly into account. Only Moore (2012) attempts to broadly examine the impact of rebel groups' arms importation on conflict duration and severity, finding that additional imports increase fatalities in a conflict. However, the results are based on SIPRI's data on transfers of major conventional weapons, which only include a minimal number of transfers to rebel groups, potentially missing out on most of the occurring trades.[1] Based on a case study of the Tamil Tigers in Sri Lanka, Sislin and Pearson (2006) argue that the patterns of arming shape the following escalation, with arms acquisitions usually preceding conflict expansions. However, due to missing data, such assessments could not be corroborated more generally, as the necessary data was unavailable. Although hindered by the lack of appropriate data, based on their theoretical reflections alone, these studies already hint at the importance of accounting for rebels' armament.

Only very limited data on non-state actors' military capabilities is available. Merely

[1] Additionally, Mehrl and Thurner (2020) raise serious issues with the coding used by Moore (2012), casting doubt on the validity of the presented results.

the Non-State Actors dataset by Cunningham et al. (2013) provides an assessment of the fighting capacity and capabilities to procure arms of non-state actors in intrastate conflicts, albeit only in relation to the respective government's capacities. Additionally, those measures are rather crude, with almost all groups having either "low" or "moderate" capabilities. Differentiation between types of arms is missing entirely. For that, Sislin et al. (1998) provide the only data available so far, with research on 49 ethnic conflicts in the 1990s, but only with a distinction between small arms and light weapons on the one hand and major weapons on the other, and without any information on the extent these arms were available. SIPRI includes some transfers to non-state actors in its arms transfers database, but only for some groups and with an overall very limited scope. Other datasets merely provide binary information on whether arms were provided externally to a group (Högbladh et al. 2011; San-Akca 2015). Thus, a wide variety of questions cannot be examined thoroughly because the necessary data on non-state actors' arms endowments are lacking. Even basic facts are in doubt. For instance, Bourne (2007: 25) states that more sophisticated weapons like MANPADS flow to non-state actors "comparatively rarely," while Schroeder (2007) lists a large number of non-state actors using MANPADS to take down aircraft.

To overcome these informational blindspots, evidence from publicly available sources was collected to provide a first assessment of how rebel groups are endowed in different arms categories, assembling the Rebels' armament dataset (RAD). For a total of 14 arms categories, ranging from rifles and machine guns over missile launchers to tanks and aircraft, the RAD provides data on a group's endowment on a 5-point ordinal scale. The data collection efforts were restricted to groups that were at least at some point in time after the Cold War involved in an intrastate conflict, as coded by UCDP; for other groups, as listed in the Ethnic Power Relations dataset or the Minorities at risk dataset, no substantial information on military equipment could be obtained. Of the 345 groups fighting in active conflicts between 1991 and 2018, my research assistants and I were able to gather information on at least some arms possessions for 270 groups.

Several publicly accessible sources were used to collect this information. After taking available information from the UCDP Conflict Encyclopedia and other conflict-related sources, we evaluated the total stock of relevant documents in the Norwegian Initiative on Small Arms Transfers (NISAT) document library. Then, searches via Google for the Web, Google Scholar for scientific literature and Nexis for press reports were conducted using various arms-specific key terms. This research delivered 10,665 raw entries of observations or reports of specific arms types or categories in use or possession. Additional information was coded for each observation, like the type of evidence—whether the arms were seized by the military, observed or captured in the battlefield, or reported as a transfer—and, for the smaller share of entries where it was reported, where the arms originated from.

To obtain a condensed assessment of the rebel groups' overall arming capacities, the raw data entries are aggregated per group and arms category, using all information on precisely or imprecisely given amounts, where available. The resulting dataset shows for each of the 270 groups the total amount of arms that evidence was found of and the composition of these arms over different categories of small arms, light and major weapons. Additionally, a time-series version of the data was compiled, indicating when a specific group might have possessed or used arms of a particular category.

As was expected, most groups my research assistants and I were able to find evidence for were relatively well equipped with small arms like rifles and machine guns, with 116 of the 270 groups being classified with the highest or second-highest categorization on the 5-level ordinal scale. In contrast, major weapons like tanks are rather rare in rebels' armaments, but not as rare as often assumed. We found evidence for at least one unit of major weapons for 125 groups, but only 36 of these groups rank in the two highest armament levels. Light weapons were notably more widespread, particularly the above-discussed MANPADS. 223 of the 270 groups were found to possess at least some light weapons, making them—compared to major weapons—a relatively ubiquitous type of rebels' armament.

Importantly, one has to be aware of some severe limitations in this data collection design resulting from information availability or resource constraints, e.g. that it reflects reporting and publication biases and underreports large groups' arms holdings (see section 5 for a detailed discussion). Nevertheless, it is the first comprehensive account of rebel groups' endowments of specific arms types and adds an essential piece of knowledge about the non-state side in intrastate conflicts.

The following section provides an overview of existing data on rebels' military capabilities and the literature on their arms acquisition methods showing that not only transfers, but leakages from state stockpiles are at least as important to assess the arming capacities of non-state actors. Section 3 then describes the data collection process in detail. Section 4 presents some results for both the raw data collection entries as well as the aggregated datasets. Then, differences and similarities to other datasets' assessments and the limitations to acknowledge before using the data are discussed.

2 The state of knowledge about rebels' armament

2.1 Data on rebels' armament, only sparsely available so far

The state of knowledge on the armament of non-state actors in intrastate conflicts is very limited and fragmented at best. Some older studies provide at least rudimentary evidence on a small number of groups, while more systematic datasets are only able

Table 1: Sislin et al. (1998) data

Subject	Small arms/light weapons and major conventional weapons
Scope	Ethnic conflicts in the 1990s
	No. of cases
Total	49 ethnic groups, 41 with information available
Possession of arms	
Small arms/light weapons	37
Major weapons	11
Arms acquisition	
Domestic procurement	19
Importation	31
Indigenous production	7

to give rough estimates of rebel groups' military capabilities. The following section presents the existing evidence on rebel groups' armament.

As a first, Sislin et al. (1998) undertook a significant task of collecting data on the possession of arms for 49 ethnic conflicts occurring in the 1990s. They used openly accessible sources from media, scholars and non-governmental organizations. For each group, they researched information on whether they were endowed with arms in two categories: on the one side, small arms and light weapons, i.e. weapons systems that can be employed by one person or a small team, like rifles and missile launchers, and major conventional weapons, like tanks and aircraft, on the other side. Additionally, they assessed if weapons were "indigenously" produced, domestically procured or imported.

For 41 of the 49 cases, they were able to gather at least some information. In 38 cases, the non-state side was equipped with either small arms/light weapons or major weapons or both (see table 1 for an overview). In most, i.e. 37, conflicts, the ethnic groups involved were found to possess small arms and light weapons. In 11 conflicts, evidence of major weapons endowments was found, which Sislin et al. attribute to diversions of stocks after total or near state collapses like in Afghanistan, Iraq or Yugoslavia. Presumably, only in the Georgian Abkhazia conflict major weapons but no small arms were identified. Notably, the results from Sislin et al. (ibid.) cannot be taken directly as evidence for the non-state groups involved in the conflicts, as in some cases, it is the government side that is also comprised of an ethnic group: in Rwanda, for instance, the authors note that only the Hutu, i.e. the government side, possessed major weapons.

For the state-to-state arms trade, the Stockholm International Peace Research Institute (SIPRI) provides the most comprehensive data, covering all publicly known transfers of major conventional weapons since 1950. Unfortunately, transfers to non-state (and

Table 2: SIPRI Arms Transfer data

Subject	Major conventional weapons
Scope	International trade with data for non-state actors (1954–2019)

	Total time span (1954–2019)		Post-Cold War (1991–2019)	
	No. of transfers	No. of groups	No. of transfers	No. of groups
Total	291	51	111	23
Air defence systems	2	2	1	1
Aircraft	33	10	7	4
Armored vehicles	51	18	17	6
Artillery	54	11	15	5
Missiles	151	39	71	20

non-supranational) organizations only account for a small share of 291 cases out of the 56199 total cases in the 2021 version of their data. The 291 relevant transfers are distributed over 51 groups, with only some groups like Hezbollah (66 entries) and the Mujahedin in Afghanistan (28 entries) featuring a larger number of transfers. Comparing SIPRI-recorded transfers in the 1990s with the cases from Sislin et al. (ibid.), one would expect to have at least some overlap with groups Sislin et al. identified as having imported arms and possessing major weapons. However, none of these groups from Sislin et al. (ibid.) have transfers recorded by SIPRI in the relevant time period, and some cases that do appear in the SIPRI dataset, like LTTE in Sri Lanka, are not coded in Sislin et al. as having access to major weapons. As SIPRI mostly relies on official reports, it does not claim to provide a reliable picture of transfers to non-state groups. SIPRI data on these groups is thus not well suited for empirical analysis of civil conflict (and has therefore not been used by scholars, with, to my knowledge, Moore (2012) being the only exception).

Concerning arms types, only five of eleven SIPRI major weapons categories have at least one transfer to rebel groups coded, notably leaving only categories of ground and air equipment, but no naval armament. Table 2 gives an overview of entries in each category. More than half of all recorded transfers are missiles, particularly portable surface-to-air and anti-tank missiles, followed by artillery, mainly multiple rocket launchers, and armored vehicles, primarily tanks.

The Non-State Actors (NSA) dataset delivers a more encompassing picture of arms procurement capacities, detailing 462 rebel groups in civil wars from 1945 to 2011 (Cunningham et al. 2013). Regarding military capabilities, the dataset provides indicators for "arms procurement" and "fighting" capacities as well as the groups' "strength" relative to the government side. All measurements are provided as ordinal variables, ranking capacities as "low," "moderate" or "high" (and "no"), and the groups' relative strength as "much weaker," "weaker," "parity," "stronger" or "much stronger" compared to the government. For 289 (286) groups, the ability of the group to procure arms (numbers on relative fighting capacity in brackets) is low, while it is moderate for 144 (149) and high for only 15 (21) groups.[2] The data also indicates that the power distribution usually favors the government side, with 405 of 477 conflict dyads featuring weaker or much weaker rebels. Thus, the NSA dataset provides a valuable assessment for a larger number of groups, but only in low detail, without more specific information on their military capabilities and armament.

Other – related, but only partly informative on rebel groups' armament – data collection efforts include the Illicit Arms Trade Database by Kinsella (2006), which focuses on illicit small arms trade which is however mainly directed to states, not non-state groups; the Illicit Small Arms Seizures Dataset by Pinson (n.d.), which is focused on seizures of small arms, but is currently not publicly available; the iTrace platform of Conflict Armament Research, which tracks diverted weapons and ammunition, but does not provide information on whom the found arms can be attributed to; and the Military Balance by the International Institute for Strategic Studies, which has minimal information on around a dozen groups in 2005-2007 and three groups for 2014 and later.

In addition to Sislin et al. (1998), SIPRI and the NSA dataset, there are various sources on specific regions or groups. Unfortunately, only in some, primarily recent, cases do efforts exist to provide a comprehensive picture of the military endowments for specific groups. For instance, the non-governmental organization Conflict Armament Research (2017) examined the procurement and origins of small arms and light weapons of the Islamic State, finding that the AK-47 was the most common type of arms, with over 70 % of all documented weapons being 7.62mm rifles. Additionally, larger quantities of machine guns and rocket launchers were found, as well as smaller quantities of other small and light weapons like shotguns and mortars. For Mali, Conflict Armament Research and Small Arms Survey (2013) documented the use and seizures of a variety of arms, primarily small arms and light weapons like rifles, machine guns, heavy machine guns and recoilless guns. However, they also found evidence of major weapons, including (multiple) rocket launchers and towed howitzers. Unfortunately, they could not attribute the evidence to specific groups like MNLA or MUJAO. Additional valuable reports from Conflict Armament Research include closer investigations of mines and IEDs used by the Houthis in Yemen and weapons supply into the South Sudan conflict, for instance. For the Southeast Asia region, Nsia-Pepra and Pearson (2009) assembled information on arms transfers in the 1990s, finding evidence for various transfers to non-state groups in eight countries. Notably, they include details on the specific arms types these groups received. The transferred weapons mainly were small arms, but also rockets and surface-to-air missiles. Unfortunately, the data only covers a limited number of countries and seems not to be publicly available.

[2] Note that capacities of some groups change over time, which are thus counted twice.

Armament of the Liberation Tigers of Tamil Eelam (LTTE) is also notably well-known, particularly concerning their procurement of attack boats, submersibles and aircraft, resulting in the "Sea Tigers" and "Air Tigers" special divisions of the LTTE (Richards 2014). At the beginning of the conflict in 1983, LTTE was mainly concerned with access to small arms and explosives, but gathered more sophisticated weapons in the 1990s (Sislin and Pearson 2006). In 1995, for instance, LTTE began using surface-to-air missiles, and at latest in 1998, they had light aircraft; in 2000, tanks, howitzers, cannons, mortars and small arms were reportedly captured (ibid., cf. also Moorcraft 2012). LTTE also had one of the most developed indigenous production not only of small arms (Smith 2003), but presumably also of major weapons like armored personnel carriers, rocket launchers and cannons as well as semi-submersibles (cf. Cook and Lounsbery 2017; n.n. 2012).

On small arms, the non-governmental organization Small Arms Survey conducted a large number of analyses on the diffusion and trade of small arms, focusing either on specific regions (e.g. Tessières (2018) for Niger; Best and Von Kemedi (2005) for Nigeria; Florquin and Berman (2005) for groups in Niger, Nigeria, Côte d'Ivoire, Liberia and Uganda; Florquin and Pézard (2005) and Anders (2015) for Northern Mali; LeBron and Leff (2014) for Sudan; Carlson (2016) for Somalia; Nowak (2016) for Honduras; Debelle and Florquin (2015) for the FDLR; Bevan (2006) for the LRA; Berman et al. (2005) for the Central African Republic) or arms types like anti-tank guided missiles (e.g. Rigual 2014) or artillery (e.g. Schroeder 2014). Unfortunately, in most cases documented in this body of work, attributing the found evidence to specific groups is not possible.

All in all, only the Non-State Actors dataset's scope covers a larger number of groups, but it has minimal information on the groups' armaments. More detailed information on the composition of these endowments – which groups possess what kinds of arms – is only very fragmented and incomplete. Is the LTTE the only group with major weapons like aircraft and ships? Are MANPADS really quite rare with rebel groups, as argued in Bourne (2007)? These questions cannot be answered so far with the available data. Thus, the effort to collect information on rebel groups' arms endowments is warranted, and it has to use primary sources since existing data collections do not provide a suitable starting point.

2.2 Acquisition channels

Compared to the specific military capabilities, the channels by which groups attain these weapons are at least qualitatively described in more detail in the literature. For designing an effort to collect armament data, knowing about the pathways weapons take when getting into the possession of rebel groups is indispensable. Therefore, this section provides an overview of the state of the literature on how rebels acquire arms.

Sislin et al. (1998) classify acquisition patterns by ethnic groups into three categories: indigenous production, importation and domestic procurement, which includes all cases of "use of personal weapons, raids on police or army facilities, or battlefield capture" (p. 398). For the same ethnic conflicts on which they collected data on arms possessions, they also attempted to identify the respective acquisition mechanisms. Based on their theoretical ramifications, they expected domestic procurement to be the dominant form of acquiring arms, as it is the most attractive way financially and manageable to organize. However, they found evidence suggesting domestic procurement only for 19 out of the 38 ethnic conflicts for which they could determine the sources of weapons. In contrast, evidence of arms importation was discovered in 31 cases. Indigenous production only took place in 7 conflicts. Thus, importation is deemed to be the most important acquisition channel. The authors note that media biases might contribute to this unexpected result, as international arms transfers gain more attention, while domestic sourcing is considered underreported. Nevertheless, the result indicates that the arms trade plays a vital role for these rebel groups.

Concerning external support by delivering arms, the UCDP external support (Meier et al. 2023) and the "Dangerous Companions" dataset (San-Akca 2015) provide information on whether rebel groups received support from third parties in the form of weapons. Unfortunately, both external support datasets do not provide any information on what types and amounts of arms are provided to the groups. Nevertheless, they give indications of how widespread external support with weapons is. Of the 518 groups for which the UCDP dataset contains evidence, 227 received external arms support in at least one conflict-year. Considering all conflict-years in the period 1975–2017, 37 % recorded evidence for such support, corroborating that arming through weapon inflows is a significant channel of acquisition—but with 291 groups having never operated this channel, other acquisition patterns are also essential for an encompassing picture of rebels' armament.

Other studies therefore point to domestic procurement as the main arms acquisition method for most groups. Khakee and Florquin (2005) and Florquin and Berman (2005) find for groups in Liberia, Mali and other states of the Economic Community of West African States that only small shares of their military capabilities were obtained via international transfers, and domestic stockpiles were in fact the primary source of arms. Bourne (2007) argues that only a few groups even have the ability to get access to the international arms market themselves or establish external support.

According to Jackson (2010), it is thus not globalization or the international flow of arms that primarily drive arms acquisitions of non-state groups, but leakage from readily available stocks of arms. Jackson (ibid.: 141) lists various possibilities for such leakages: "Weapons leak when state forces mutiny or if they abandon weapons during fighting. They leak through raids on military stockpiles (looting), and ambushes on armed forces and the police. The main incentives and opportunities for leakage are corruption (at various levels), personal gain, sympathetic elements within the armed forces (for ethnic, ideological and religious reasons), and state weakness." Especially in weak states that do not have sufficient capacity or miss organizational focus to protect the state's stockpiles, looting is a preferable acquisition method for groups, as arms are available fast and in large numbers. Therefore, Marsh (2007: 61) calls the state's ability to protect its stockpiles "the most important factor in determining the availability of weapons to insurgents" (cf. Karp 2006).

In contrast to international transfers, raiding arms from stockpiles and capturing weapons on the battlefield do not require financial resources, which are usually scarce for groups in conflict. Therefore, specifically smaller groups or groups in their formation phase at the beginning of conflicts rely on domestic acquisition, as they lack other acquisition channels (Marsh 2007; Duquet 2009; Jackson 2010; Marsh 2020). Bourne (2007) describes this method as "bottom-up" arming, as it does not require top-level organization—like trading arms does—but can be conducted by smaller, independent units. Even if each event of leakage only consists of small amounts of weapons, the distributed execution allows for more significant amounts in total (Markowski et al. 2008, 2009).

On the incidence of groups producing their own weapons, Bourne (2007) corroborates the findings of Sislin et al. (1998) and Sislin and Pearson (2001) that only a few groups engage in production (cf. Duquet 2009). Usually, they produce mostly simple small arms—except LTTE, which also developed light and major weapons systems (Kreutz and Marsh 2011). Of the three channels defined by Sislin et al. (1998)—domestic procurement, importation and production—the latter is clearly the least common.

To sum up, no per-group data on the importance of specific arms acquisition channels is available, except for dichotomous indications of whether groups received external arming support. However, on a general level, existing research elucidates that stockpile leakages, battlefield captures and importation are all common ways for non-state groups to acquire weapons. When collecting data to gather a comprehensive picture of what weapons these groups are endowed with, all of these acquisition channels must be considered. Focusing on external support alone, as the available data does, is not sufficient.

3 The data collection effort

3.1 General approach

To gain insights into the military capabilities of rebel groups, one has to combine information from many different sources, a generally advised method for collecting evidence on conflicts (Högbladh et al. 2011). While official records on states' capabilities, e.g. for arms transfers, exist, evidence of rebels' arming only surfaces under specific circumstances. Acquisition usually takes illicit paths, from importation, "ant trade," stockpile leakage or battlefield captures, as described in the preceding section. Only when groups lose equipment, for instance due to arms seizures by government security forces, is information about this equipment officially recorded.

Initial research efforts focusing on specific data corpora revealed that no single source contains sufficient information to gather a cohesive judgment on armaments of different categories. Non-governmental organizations like Conflict Armament Research and Small Arms Survey provide valuable collections of material on specific regions, groups or arms types (see section 2.1). Accordingly, their work is highly useful in contributing pieces of information, but must be substantially supplemented with additional sources.

A relatively high density of useful information was found in the NISAT document library, which provides news and reports aggregated from different sources, containing specific information on arms transfers, seizures and the black market. Additionally, web searches were used to retrieve information both on homepages and in the scientific literature. While those web searches often provided useful material, coverage of military capabilities, even for specific conflicts, proved rare in the literature.

Using a few selected groups as test groups, an unexpected variance in the extent of information that was retrievable emerged. On the one hand, substantial evidence was found for groups involved in conflicts over a longer time and in conflicts with higher casualty numbers. On the other hand, in a majority of cases, information was hard to come by, even when researching multiple sources. As a result, the decision was made to additionally conduct searches of comprehensive news databases (cf. Öberg and Sollenberg 2011). These partly delivered helpful information, but with a low signal-to-noise ratio.

3.2 Collecting and assembling evidence on rebels' armament

To overcome the wide dispersion of valuable information on rebel groups' armament, a multi-stage process was developed for retrieving information from different sources in a comprehensible, transparent procedure, which was conducted in the same way for each rebel group. The following section describes the data collection process, the coding of retrieved information and how the dataset was aggregated from the collection of singular information.

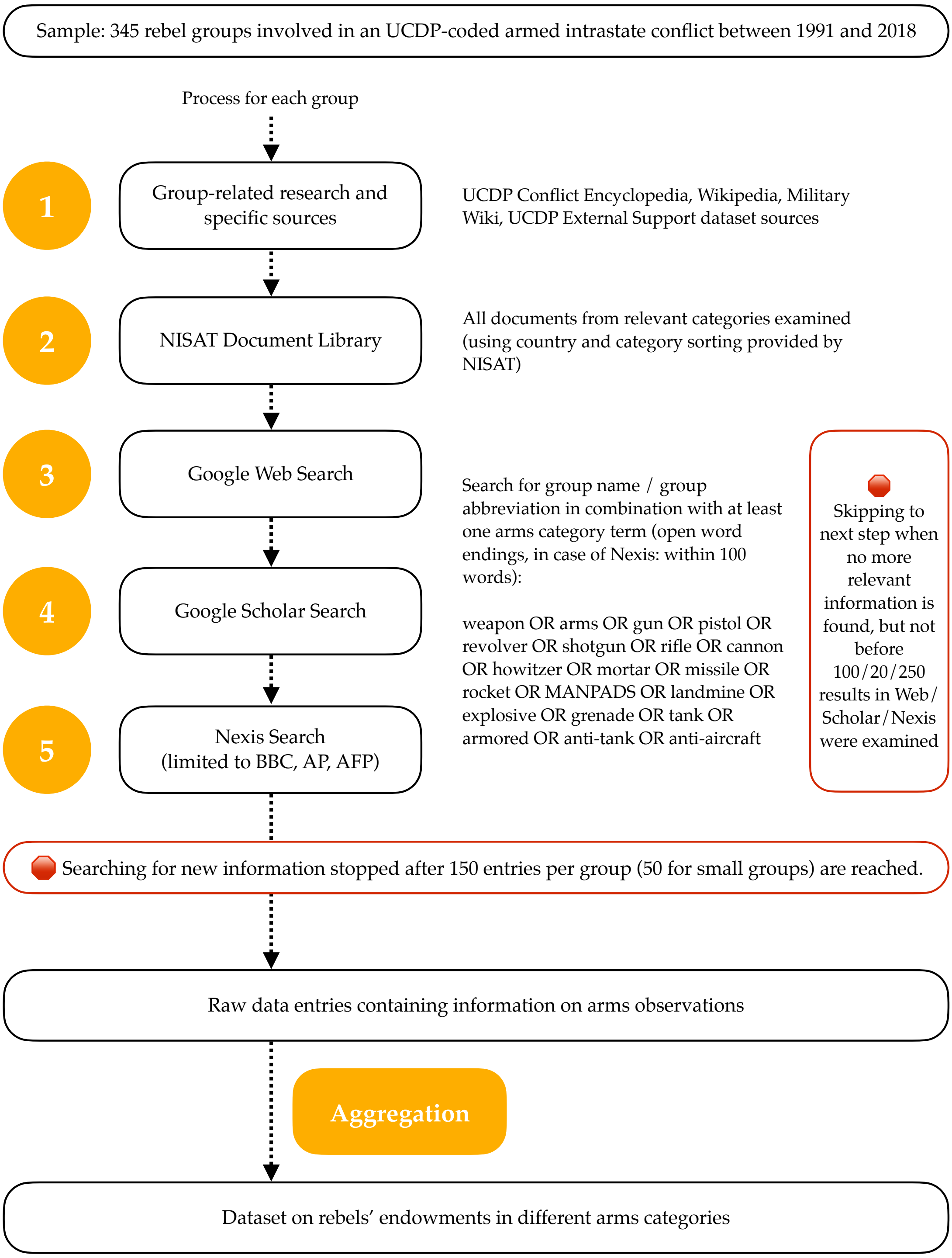
3.2.1 The data collection process

Each piece of information was collected as a separate observation in a "raw data" table. Thus, if the retrieved evidence provided sufficient detail, an observation contains information on the arms type, the recorded amount of this arms type, the year of observation, and which type of evidence was given in the source. For the coding of the different types of evidence, categorizations from Conflict Armament Research were used in addition to categories from Markowski et al. (2008) and Jackson (2010), like battlefield capture, leakage from state stockpiles, transfers, seizure of authorities, usage in battle or, if no specific background was given, "general statement." In the case of transfers, the origin of the transfer, where provided, was additionally recorded. To maintain transparency and allow for backtracking the evidence, exact references to the source of every observation were included.

For each group, the process was conducted in the same manner, following the specific research steps to ensure that the same sources of information were conclusively examined for every group, as recommended by Sundberg and Harbom (2011). This process, executed for each group separately, is depicted in the flowchart in figure 1 and contains the following steps:

1. In the first step, standard sources of conflict-related information like the UCDP Conflict Encyclopedia were consulted, where material on the group in general sometimes also includes knowledge about their military capabilities.
2. The second step involved going through all the documents in the NISAT document library classified into relevant categories like "Armed Groups and Small Arms," "Black Market" or "Stockpiles." As documents in the NISAT library are sorted by country, this step was only conducted once per country for all respective groups.
3. Third, my research assistants and I conducted web searches using Google, the search engine widely acknowledged to deliver the most accurate results. Again, we used standardized search queries comprising the group's name and/or its acronym, as recommended by Öberg and Sollenberg (2011), if necessary combined with the name of the country to identify the relevant group; and, connected with an "AND" condition to the group name query, twenty different denominations of arms types and categories like "arms", "gun", "cannon", "MANPADS" or "tank", with an "OR" condition connecting the various arms types strings. Note that in preliminary examinations, adding more specific categories did not improve results, but delivered more irrelevant results.

Figure 1: Flowchart of the data collection process



4. Next, the same search query was used in a Google Scholar search to more specifically find scientific articles and books that might already have examined the group in question in more detail.
5. In the fifth and last step, the Nexis research database was searched using similar queries. As the results from less relevant sources led to a meager hit rate of useful information, the Nexis search was restricted to only include sources from the news agencies BBC, Associated Press and Agence France-Press (following the selection from Daxecker et al. 2019).

To be able to conduct the data collection process for all 345 groups, given the available resources and staff, it proved necessary to limit the number of search results to examine in each step as well as the number of pieces of information to be included in the raw data table. As searches on Google or Nexis easily reach tens of thousands of hits, we stopped examining the results when their relevance decreased substantially, but always closely reviewed at least the first 100 or 250 results for Google and Nexis, respectively. For larger groups, after 150 entries only substantially new information, such as large reported amounts or arms types not yet recorded for a group, was included. Of course, this approach means the data can provide only a lower bound estimate of the military capabilities of larger groups, but at the benefit of delivering a broader picture for all groups in the sample, for which there would not have been sufficient resources otherwise. Note however that we always included all information found in the NISAT documents, making this the base corpus that is extended by evidence from all these additional sources.

3.2.2 Aggregating the collected raw data to monadic group and group-year datasets

While the dataset of assembled information already presents a large amount of valuable evidence on rebels' military endowments, a standardized process was developed to gain an assessment of rebel groups' overall possession of specific arms categories, based on the collected raw pieces of information. In short, all entries for a given group and arms category were combined to derive an ordinal measure of how well a group is armed in absolute terms.

To achieve this, in the first step, the almost 2400 different types of arms or denotations were classified into 14 different categories like rifles, mortars, tanks or aircraft, which were adopted from NISAT and SIPRI classifications of arms. Thereby, the aggregated data allows to examine which types of arms feature especially prominent in rebel groups' military arsenals. To provide a more concise picture, these 14 categories were combined into the broader classification of small arms, light weapons, major weapons and explosives.

Aggregation of information was unfortunately not as straightforward as adding up all recorded entries, because only for 58 % of all entries the original sources contained information on arms volumes. Additionally, where available, volumes were often not given as precise numbers, but in verbal form as e.g. "a few" or "hundreds." To be able to combine these different types of information and, at the same time, not overstate the precision of the data, a 5-level ordinal scale was derived to denote the armament in each arms category. The levels of this scale align with the numerical order of magnitude of the weapons count. Thus, a zero denotes no evidence of these arms at all, a 1 the first order of magnitude, i.e. volumes ranging from 1 to 9, a 2 the second order of magnitude (10-99), a 3 the third (100-999) and a 4 arms counts of 1000 and higher.

For the summation of entries with imprecisely given amounts, this information was first coded into this 5-level ordinal measure capturing the order of magnitude the term implies. For instance, "a few" was coded as 1, "dozens" as 2, "hundreds" as 3 and "thousands" as 4. Additionally, entries just giving information on ammunition of specific arms were also included, ranking down these entries by one or two (for MCW and SALW, respectively) orders of magnitude compared to entries stating information on weapons.

For example, assume a group has three entries for rifles, one coded with the exact number of 90, one recorded as "several" and one as "dozens." "Several" was coded as 1 and "dozens" as 2 on the ordinal scale, then the ordinal value 1 was converted into numerical form, i.e. the ratio scale of weapons counts, as 2 and the ordinal value 2 as 30. 2 and 30 are determined by the respective mean of entries with precisely given amounts in the respective ordinal category, i.e. 30 is the mean value of all rifle entries with numerically given amounts between 10 and 99. Then, these auxiliary numerical values were added up, $90 + 2 + 30 = 122$, and re-converted into an ordinal value using the orders-of-magnitude scale described above, meaning the three rifle entries result in an ordinal rifle armament value of 3.

To additionally allow for the inclusion of entries without given amounts, the assumption was put in place that the amounts correspond to the mean of other entries in the same group and category or, when the group does not have other entries coded in the same category, the same category in all groups, respectively. Be aware that this is a rather strong assumption, which will be incorrect for each entry from an isolated perspective, but will allow for a more complete picture of rebels' armament overall. These considerably large uncertainties are why the overall measures are only coded on an ordinal 5-level scale, not as metric, ostensibly precise volumes. The dataset also provides "lower-bound" metric and ordinal variables. For the former, only entries with metrically given amounts are included, and for the latter, there are additionally the entries with imprecisely given amounts included, but entries without any amount given are excluded.

In addition to the group-version of the dataset, a group-year time-series version is also assembled. This version indicates when the respective evidence on a group's armament was dated to, allowing for a tentative assessment of potential developments over time. However, this posed two additional crucial challenges: first, it was often not straightforward to pinpoint the evidence to a specific year, and, second, substantial assumptions about the period of validity had to be made.

Regarding the first challenge, the temporal reference point was inferred based on the source's content, whenever possible. When reports stated a specific year of an observation, this information was included as the relevant year. When no date was given, the date of the source was recorded. In cases where the information was a general statement on a group, the evidence was assigned to all in-conflict years of the group.

Regarding the second challenge, the dataset aims to achieve the most encompassing assessment of the groups' armament over time that is possible, even though information on most groups is sparse. Usually, the sources do not include information on how long before or after, respectively, this event the weapons were in possession of the group—it could be a few days, the full year or decades. Adding up only the entries where the information stems from precisely that given year to derive the respective value for this year would thus provide a very fragmentary picture of a group's armament, as most weapons will be in possession longer than would be indicated by the dataset: many groups would only having specific measurements of armament at some points in time, while in reality, they of course were in possession of these arms before and/or after the specific evidence were publicly reported.

Therefore, the period of validity for each entry was widened to a 5-year shifting time window according to the type of evidence. For reports from seizures by authorities or disarmament processes, it is clear that only before the reported event took place the group could have possessed these arms. Thus, the evidence is factored in for the reported year as well as the four years prior. For transfers, leakage from stockpiles or state collapses and battlefield captures, the group could only be accredited for possessing these weapons afterward, so the evidence was attributed to the recorded year and the four successive years. Lastly, for observations of possessing or using the arms on the battlefield, possession before and after the event is plausible. Therefore, the time window is shifted to two years before and two years after the evidence was reported. Note that it was also assumed that the end of a conflict would not automatically mean that the rebels were disarmed unless the evidence indicated otherwise. Thereby, this version of the dataset deviates from providing the "minimum reported armament", as in the group version, to providing a generally more valuable but less precise, smoothed picture of groups' armament evidence evolving over time. These limitations are strongly advised to be acknowledged when operating this dataset.

All in all, aggregating the collected information with this process delivered information about rebel groups' military endowments of small arms, light weapons, major conventional weapons and explosives both in summary as well as over time. As building the monadic datasets is completely mechanic and transparent, both strengths and limitations are transparent as well.

3.3 Overview of the Rebels' Armament Dataset

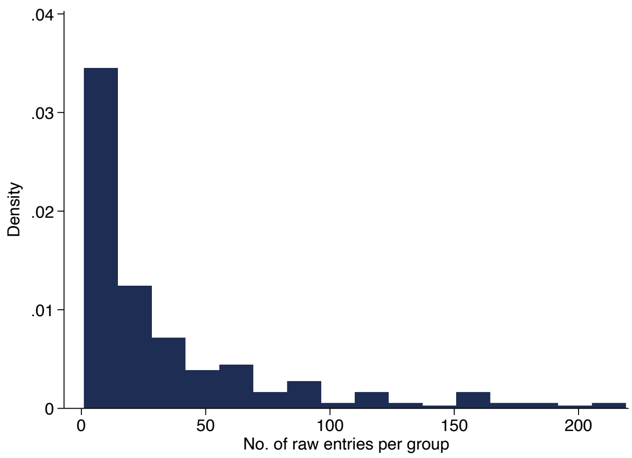
Using the process described in the section above, the available evidence on armaments was collected for all 345 non-state groups that experienced conflict in the period 1991–2018 according to the UCDP definition. In total, this effort resulted in 10,665 raw entries concerning these groups' weapons.

3.3.1 Information availability

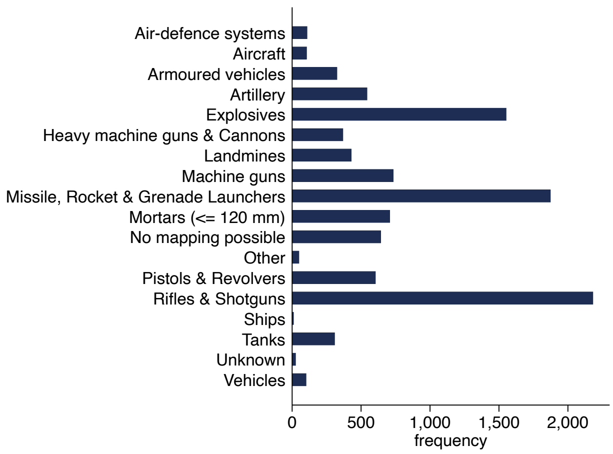
Cases without any observations Although a large variety of sources for each group was investigated, it turned out that for a significant share of the groups, i.e. 75 out of 345, no information on their specific military capabilities, meaning at least some form of evidence of endowment or usage of a specific type of arms, could be found. Exploring this set of groups with data from other sources, these groups were shown to be involved in comparatively small conflicts with low numbers of casualties. Furthermore, they were found to have rarely received external support, according to the UCDP External Support dataset. Finding no evidence at all could be interpreted as an indication that these groups actually possessed only minimal armament. In fact, when aggregating the data as described in the previous section, all arms categories of such a group denote 0 armament. However, even sparsely armed groups have to possess at least some amount of arms to be even able to conduct a violent conflict. Thus, groups with no evidence recorded can be identified in the dataset to be excluded from analyses, but they remain in the dataset to transparently present the full sample of researched groups.

Generally scarce information Not only for these 75 cases, but for the majority of groups, evidence that was sufficiently reliable and specific to be included in the dataset proved to be very scarce. For the 270 groups for which at least some information was found, an average of 40 entries were recorded. However, as shown in figure 2a, the distribution is right-skewed, with many groups having considerably fewer entries recorded. Note that, as the search algorithms rank results according to their match with the query, this is not a result of research assistants stopping their search too early, but that no additional relevant information in the sources used for the data collection was available for these groups.

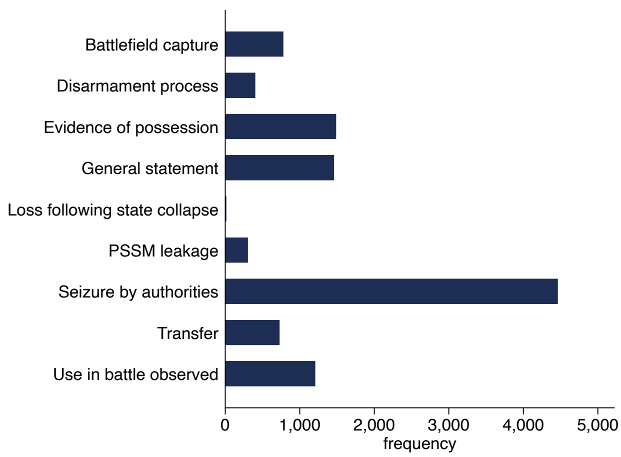
Figure 2: Entries per group and per type of evidence



(a) Distribution of the number of entries per group, excluding groups without any entries and outlier groups



(b) Distribution of the number of entries per arms category



(c) Distribution of the number of entries per type of evidence

3.3.2 Evaluating the collected evidence

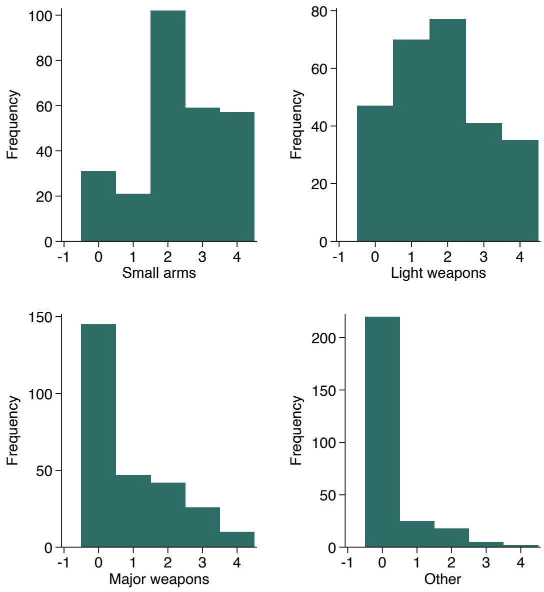
Findings of different arms categories To evaluate the prominence of different types of arms endowments, figure 2b gives an overview of the number of entries recorded for each of the 14 detailed arms categories. Small arms have long been claimed to cause most deaths in civil conflict (cf. Wille and Krause 2005), and the data fits that assertion: most entries were attributed to the category "rifles and shotguns", almost a quarter of all observations. However, with a share of about a fifth of all entries, the second-largest category is missile, rocket and grenade launchers, which are not classified as small arms according to NISAT, but as light weapons. Notably, evidence of explosives comprises the third-most entries. Although in considerably smaller numbers than the small arms, light weapons and explosives categories, substantial numbers of entries were also recorded for major weapons, particularly artillery, tanks and armored vehicles. However, evidence of boats and ships with military use cases was only found in 10 instances, making it the category with the fewest entries.

Types of evidence Figure 2c provides the distribution of entries over the different coded types of evidence. Over 40 % of all entries are based on seizures by authorities. This relates back to the basic fact that the data collection can only include evidence that is somewhere reported on, and security forces usually openly communicate the results of police or military raids of rebel arms stocks. Therefore, evidence from seizures proved to be the primary source of information, followed by information on the possession of specific types of arms, general statements that do not specify more concretely what evidence the statement is based upon and use in the battlefield, either when it is observed or when state forces report on captured equipment. Notably, only in 5.3 % of all cases (6.7 % when adding SIPRI transfer data) did the recorded information refer to a transfer of arms. This confirms the expectation derived in section 2.2 that collecting only data on arms transfer would have provided very limited insights into rebels' overall military capabilities. Nonetheless, where information on the source of an arms transfer was given, the dataset includes it in the list of potential origins of a group's armament.

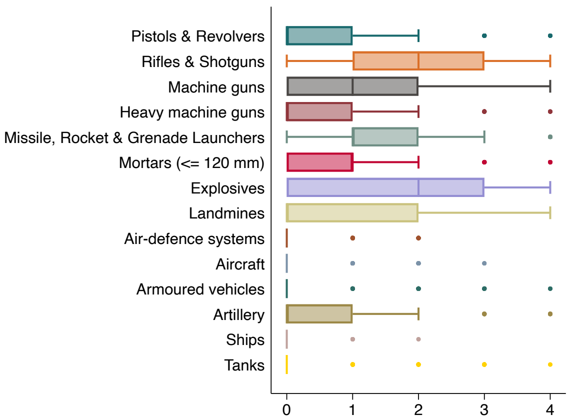
3.3.3 Evidence on groups' armament

Overall distribution of rebel groups' armament Combining the singular information found for each group to a comprehensive assessment of each group's military capabilities, the general finding from the right-skewed distribution of the number of entries found per group translates to rather bi-modal distributions of military endowments for broader categories of arms. Figure 3a shows the distribution of arms endowments on a 5-level ordinal scale from 0, indicating no endowments at all, to 4 as the highest level of endowments, for the four major categories as well as endowments

Figure 3: Distribution of ordinal armament measures



(a) Distribution of ordinal measures for the main arms categories (only groups with at least one entry)



(b) Distribution of ordinal measures for the 14 specifc arms categories (only groups with at least one entry) that either did not fit the main categories, like military-grade radio or sensor equipment, or were insufficiently explicitly defined in the source evidence. For small arms and light weapons, the most frequent value is a medium armament level of 2, while it is no armament recorded for explosives, major weapons and other. Only for the major weapons category (and other[3]), however, the absolute majority are groups with no such armament recorded.

Small arms As for the number of entries, the group-level data shows that rebel groups widely possess small arms. Only a few groups with at least some information recorded did not turn up evidence of small arms endowments. 116 out of the 345 groups scored with the second-highest or highest categorization, corroborating the general assertion that rebel groups are usually well-equipped with small arms. Figure 3b breaks the armament measures down into 14 more specific categories. As is commonly assumed, many groups have access to rifles and shotguns, first and foremost the infamous AK-47. In fact, the rifles-and-shotguns category is the only specific category for which the median armament over all groups lies at level 2. Nevertheless, machine guns are also widely used.

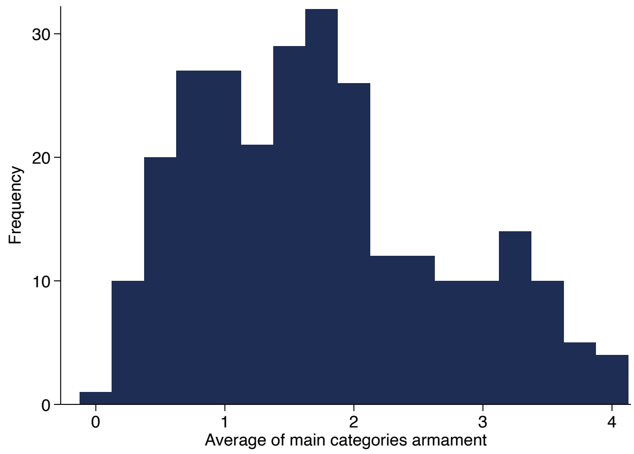
Light weapons Compared to small arms, the average volume of light weapons is considerably smaller, with only a few groups possessing particularly high levels. It is notable, however, that the large volume of evidence found for missile, rocket and grenade launchers, especially MANPADS, does not only apply to a few selected groups. Instead, it is distributed over a significant number of groups, meaning that over 100 groups have at least a mid-level endowment of light weapons. Evidence for heavy machine guns and mortars, on the other hand, was more concentrated on fewer groups, with only a small number of groups reaching the two higher levels coded (see figure 3b).

Explosives For slightly more than half of the researched groups, evidence for using or possessing explosives was found, a lower share than for small arms and light weapons. However, those groups with explosives available usually have considerable amounts, with 100 groups reaching at least the second-highest level of endowments. As figure 3b shows, not only explosives like bombs and hand grenades but also landmines are widely spread.

Major weapons In stark difference to the other categories, usage or possession of major conventional weapons like tanks or aircraft are only found for a limited number of groups. 145 out of the 270 groups where at least some information was found had

[3] Other arms also include chemical weapons, which were only very rarely found in the data collection process, but may nevertheless play a relevant role in some cases (cf. Asal et al. 2023).

Figure 4: Distribution of average values for the main arms categories (only groups with at least one entry)



no major weapons recorded at all. Even when major weapons have been associated with a group, the volumes recorded are usually relatively small. However, as the numerical foundation of the categorizations is one unit of a weapon, one has to consider that an army of a given size needs significantly fewer units of tanks or aircraft than e.g. rifles to be adequately equipped and fully operable. The main major conventional weapons category that rebel groups seem to be able to get hold of (and put in use) is artillery, consisting mainly of howitzers and anti-tank guns, but also missile systems, for instance. Concerning tanks, at least one record is found for 63 groups. However, almost half of these groups reach only the lowest level 1 on the armament score, and only 15 reach the two highest levels, with UNITA being the only group reaching the level 4 score. Even fewer groups have been registered to be armed with aircraft—only 28, of which 17 only reach the lowest level. However, drones might already be changing the importance—and proliferation—of the aircraft category in civil conflict (cf. e.g. Boyle 2020; Beccaro 2023).

All in all, this newly collected data provides notable evidence that rebel groups do not only rely on small arms, but also light weapons, in particular missile, rocket and grenade launchers, and explosives. In addition, the data shows that a significant number of groups can resort to at least some major conventional weapons.

A closer look at well-endowed groups We can derive a ranking of groups' armament by taking the average armament levels in the four main categories—small arms, light weapons, explosives, and major weapons. Figure 4 shows the distribution over all 345 groups. Taking only groups with at least one piece of evidence recorded, the median lies at 1.625 and the 75th percentile at 2.25. However, a considerable number of 43 groups rank at an average of 3 or above, of which 38 reach at least level 2 armament in each of the four categories and 16 at least level 3. As can be seen relatedly in figure 3a, also in the top groups, the major weapons category usually is where groups often lack endowments compared to their armament in the other categories.

Using these metrics, the best-armed groups include the United Armed Forces of Novorossiya, Hezbollah, the Khmer Rouge, IS, Hamas, Syrian insurgents in the Syrian civil war, the Taleban and LTTE. Broadening the view from the four main categories to the 14 more detailed categories, we generally find a similar ranking of groups, meaning that the specific classification of the different category schemes does not change the overall assessment. Notably, UNITA, mentioned above with regard to its high tank endowments, joins the United Armed Forces of Novorossiya as the best-armed group when averaging over the 14 more specific categories.

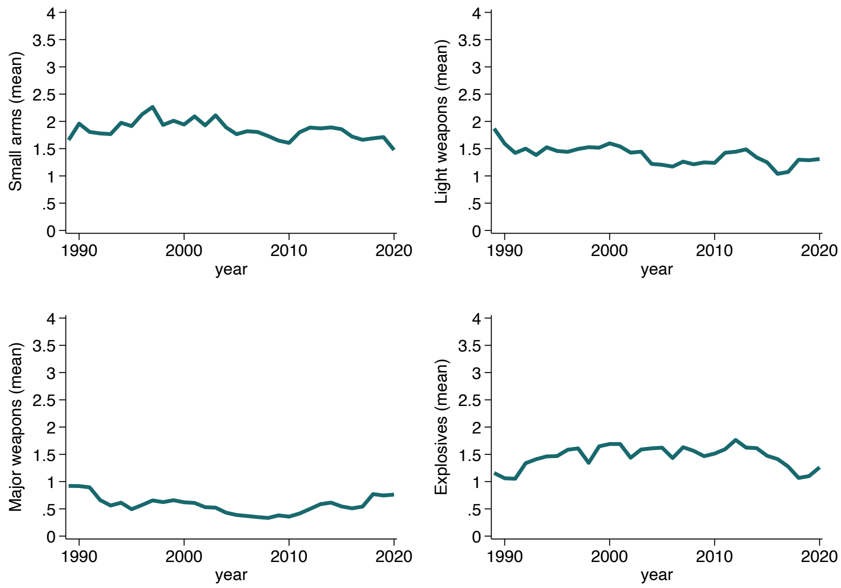
Distribution of groups' armament evidence over time For the time-series version of the dataset, the collected evidence had to be attributed to a specific period as accurately as possible (see section 3.2.2 for details and section 5 on notable limitations). Figure 5a exhibits the averages of all groups' armaments in the four main categories over the time horizon of 1989 to 2020. Overall, these average armaments were rather stable over time.[4] Notably, major weapons armament decreased at first over the observation period, with an uptake in the 2010s, while evidence for explosives increased in the 1990s with a slight downward trend after 2016.

It is important to acknowledge that the stability of average armament levels masks the variance of evidence over time for many groups individually. For illustration, figure 5b displays the time series of four well-armed groups' average of the four main categories' values. For some groups, large amounts of evidence were obtained, such that these groups exhibit stable high measures of armament, like Hezbollah. For most cases, however, evidence varies substantially over time, as can be seen for FARC, LTTE or UNITA, for instance. Since this variance over time potentially is less pronounced in reality, it illustrates the imprecision in the accounting of armament in this time-series dataset owing to the sparseness of available information, as described in section 3.2.2.

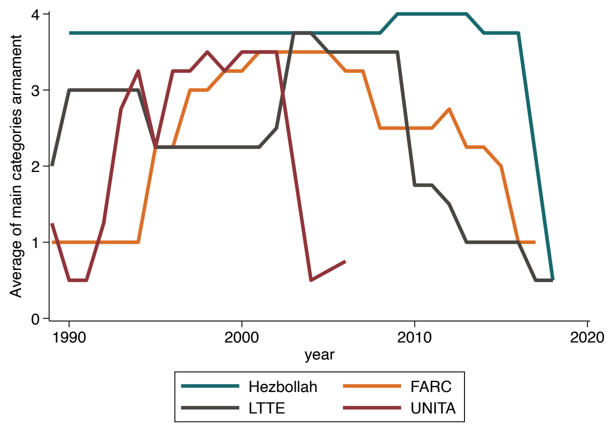
Comparing the evolvement of evidence on LTTE to the assessment in Sislin and Pearson (2006), RAD does not show the uptake of armament in the mid- to late-1990s that Sislin and Pearson described, but an increase in the early 2000s. This difference highlights an important issue with the dynamics of armament evidence: evidence does not have to surface when acquisition occurs. In the LTTE case, acquisitions might very well have taken place earlier than recorded in the RAD data collection, as the acquired

[4]The lack of an overall time trend in armament levels increases confidence in the data collection procedure, since potentially biasing impacts of e.g. increased availability of data due to social media or other modern communication technologies in more recent years could have resulted in period effects concerning the amount of collected evidence.

Figure 5: Rebel groups' armament over time



(a) Time series of armament averages (only groups with at least one entry)



(b) Examples of groups' armament averages time series

Table 3: Spearman correlations of Non-State Actors dataset (Cunningham et al. 2013) and RAD variables

	Rel. arms procurement cap.	Rel. fighting cap.	Rel. rebel strength
Mean armament	0.0198	−0.0600	−0.0229
Small arms armament	−0.0928	−0.1139	-0.0152
Light weapons armament	0.1565*	0.0572	0.0595
Explosives armament	−0.1563*	−0.1981**	−0.2297**
MCW armament	0.2546**	0.1773*	0.1865**

Note: * indicates $p < 0.05$, ** indicates $p < 0.01$. Groups without any evidence recorded in the RAD are excluded from the comparison.

weapons are only later observed when used in fighting or when the group is losing equipment owing to general military decline. Awareness of these and other limitations (see section 5) is vital for fully apprehending the presented data.

4 Correlates of RAD with other data

4.1 Comparing armament data to the Non-State Actors dataset

As noted in section 2.1, only the Non-State Actors dataset (Cunningham et al. 2013) provides a comparable scope of data on rebel groups' arming capacity. This dataset, last updated in 2013, provides information on 462 non-state groups in civil conflicts between 1945 and 2011. The RAD, in contrast, starts in 1989 and ends in 2020, leaving an overlap of 248 groups contained in both datasets. Of these 248, 196 groups was at least some armament information obtained for.

In the Non-State Actors dataset, information on armaments is tangentially comprised of the arms procurement and fighting capabilities indicators as well as the composite "rebel strength" indicator. Table 3 gives an overview of the correlations between these measures and the found armament information in our dataset, using Spearman rank correlation coefficients accounting for the ordinal measurements of all variables. Notably, there is no significant relation between the coding of any of the three relevant variables from the NSA dataset and the overall armament variable, which averages the armament values of the four main categories small arms, light weapons, explosives and major weapons.

However, when turning to these four categories separately, some weak to moderate correlations emerge, in particular for major weapons. All three NSA variables are significantly positively correlated with MCW armaments. The strongest correlation is present for the arms procurement measurement, with a significant positive relationship also with light weapons armament. This was to be expected, as the arms procurement capacity variable focuses especially on whether groups "have access to high-tech weaponry" or "are hampered in their ability to secure weapons" (ibid.: 522). It is thus the measurement conceptually most similar to RAD. However, it disregards "low-tech" weaponry like small arms, potentially leading to these insignificant correlations with our respective data.

At the same time, explosives are significantly *negatively* correlated to all three NSA measures. While this relationship may seem counterintuitive, it could hint at the endogeneity of armament capabilities and fighting strategies. Groups that are less able to obtain weapons for conventional warfare resort to guerrilla and potentially terrorist tactics (cf. Kalyvas and Balcells 2010), as e.g. explosives are easier to come by than light or major weapons (see section 3.3.3). Thus, their high levels of explosive armaments, according to RAD, are actually partly a consequence of what the NSA dataset codes as low "rebel strength."

Overall, the correlations are rather small. This is not particularly surprising since it points to a decisive difference between the two datasets: while the armament of each group is measured in absolute terms, all measures from the NSA dataset are relative measures, evaluating the rebel groups' capabilities in relation to the respective government's capabilities. Additionally, RAD allows a more detailed picture of the underlying arms portfolio used and available to the groups. For instance, the Revolutionary United Front (RUF) has a "moderate" fighting capacity in relation to Sierra Leone's government, meaning a relatively high capacity compared to other groups. RAD shows that RUF was indeed highly armed with small arms, but also with light weapons, in particular missile launchers, and less so with MCW. For other groups, the overall assessment might disagree substantially. For instance, LTTE is found to be equipped with a wide variety of not only small arms, but also light weapons like missile launchers and mortars and even MCW with artillery and some tanks, aircraft and ships—but LTTE's arms procurement capability relative to the Sri Lankan government is coded as "low" in the NSA dataset.

4.2 Relating to external support and other data

Evidence of transfers and external support Due to the clandestine nature of most rebel groups' arms acquisitions (see section 2.2), only for a relatively small portion of the recorded armament evidence could information on the arms' origin be obtained. Nevertheless, it is clear that external support plays a major role in arming rebel groups. While the UCDP External Support provides information on the specific suppliers of support in general and weapons in particular, it unfortunately only indicates the existence of support, but not its extent. To compare the presence of external support with the RAD armament data at the group level, the share of in-conflict years in which such external support was recorded in the UCDP data is used. For weapons support, the correlation with the average armament and MCW armament lies at .24 and .27, respectively, significant at the 1 % level. For all types of support, the correlation is also highly significant and even slightly higher, at .30 and .36, respectively. Thus, receiving external support is clearly associated with higher armament levels.[5]

"Mechanization" of troops As RAD provides absolute estimates of armament, it is to be expected that groups with many troops on average are also equipped with more weapons—after all, maintaining personnel is costly, meaning it is only sensible to an extent where basic weapons supply to the troops is possible. This is also found in the data, albeit with a relatively weak relationship: UCDP troop measures are significantly correlated with the average armament levels, at a correlation coefficient of .16. Notably, correlation with MCW armament is substantially higher, with a coefficient of .34, significant at the 1 % level. These rather low correlations indicate that "mechanization", i.e. the availability of military technology per soldier (cf. Lyall and Wilson 2009), varies widely between groups.

Availability of "heavy weapons" according to Sislin et al. (1998) In Sislin et al. (ibid.), 11 country cases were provided where groups were found to have heavy weapons like tanks or artillery available. For 10 of these 11 cases, Burundi being the exception, RAD corroborates the presence of MCW, in most cases with rather high armament levels recorded.

Overall, these comparisons show that measures of RAD are indeed related to existing data. However, the mostly relatively low correlations imply that the newly collected data on rebels' armaments measure distinctly different concepts of capabilities compared to all other available datasets. Thus, RAD provides novel insights and value for the intrastate conflict literature.

5 Limitations, biases and the way forward

The Rebels' Armament Dataset provides the first encompassing assessment of rebels' arms possessions and technologies, expediting our knowledge about the extent to which different military technologies are available to rebel groups. Nevertheless, users of these data need to be aware of their limitations and potential biases.

First and foremost, my research assistants and I could only collect evidence when it was publicly available and came onto the radar using the different layers of the research process. Concerning most acquisition channels depicted in section 2.2, however, information on rebel groups' armament is rather illicit: usage of specific types of wea-

[5] The same pattern emerges using the time-series version of the data. Here, the correlation of weapons support with the average armament and MCW armament lies at .09 and .19, significant at the 5 % and 1 % level, respectively. For all types of support, the correlation is also highly significant and again higher, at .24 and .23, respectively.

pons in conflicts usually is not well documented, transfers of externally supplied weapons are often carried out without official acknowledgment, and governments sometimes attempt to cover up losses from their stockpiles. This explains why over 40 % of all raw data entries are based on seizures by authorities: here, incentives exist on the side of the government to spread the word broadly. In line with these considerations, data in civil conflicts is generally known to suffer from incomplete reporting and biases (Öberg and Sollenberg 2011; Weidmann 2015, 2016). Accordingly, the fact that no evidence at all for 75 out of 345 researched groups could be found does not imply that these groups had no arms whatsoever. Rather, it highlights that RAD can only be understood as a lower bound of the potential endowment of a specific group's arms.

Understanding which groups evidence could be found for is also crucial for interpreting the data and their missingness. Groups with at least some information recorded have fought conflicts with an average of 3154 battle-related deaths. By comparison, groups without any information recorded fought conflicts with an average of only 485 fatalities. Thus, obtaining evidence was clearly harder for smaller groups in less severe conflicts. Unfortunately, the general sparseness of evidence also inhibits "triangulation" procedures, which are usually proposed to increase data accuracy (Davenport 2010; Cook and Weidmann 2019; Weidmann and Rød 2015). With such methods, the information from several reports on the same event or issue is combined to gain a more accurate picture. However, this is not possible when—as is the case for this data collection—in almost all cases only one source is available for each observation of arms (cf. Weidmann 2016).

Furthermore, language bias might affect whether or not evidence for some groups was obtained (cf. Phillips and Greene 2022). However, the NISAT document library, the first source of information used for RAD, also includes translated reports in other languages like Spanish and French. Nevertheless, web searches and other sourcing were only conducted in English, potentially missing evidence in other languages.

When aggregating the found pieces of evidence into condensed measures of a group's armament levels in different categories, additional sources of uncertainty play a role: evidence on specific sightings of weapons might relate to the same weapons—it could stem from different sources either on the same occasion or at different occasions. While it was thoroughly avoided to count identifiable double entries, they might not always be identifiable. Many pieces of evidence also do not provide actionable estimates of amounts, leaving us with informed assumptions, as described in section 3.2.2, for a large part of the evidence. Regarding the time-series version of the dataset, it is essential to note that specifying a temporal scope for evidence on a group's possession of arms is often tricky: how long was a weapon in possession of the group before and after it was seen being used in a battle? How long after it was raided from a government stockpile? It again is possible to derive well-informed assumptions for compiling armament time series for specific groups, but these come again with significant uncertainties, as described in section 3.2.2.

Finally, relying on historical information is more challenging than collecting new evidence on contemporary events. It would thus be valuable to establish an ongoing effort to combine all kinds of data to allow for a more complete picture of rebels' armament going forward in time. In addition to the manual examination of information in text, recently developed large language models might help to cope with the enormous amounts of data generated daily on the web or in print. Even larger potential for encountering relevant evidence on rebel groups' arms lies in investigating image and video material; again, modern technology can help, with fast-evolving machine learning techniques being able to identify patterns—like specific arms types—in evidence e.g. from social media, accompanying existing (and mostly manual) efforts for specific cases from investigation platforms like Bellingcat. Bundling automated information analysis and extending it to visual media would allow for vast improvements in rebels' armament data over what the data collection effort described in this article was able to collect. Nonetheless, the very nature of illegal arming in secrecy and situational complexity in battles will always pose a substantial challenge for researchers aiming to understand rebels' military capabilities better.

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