UNIVERSITÀ CATTOLICA del Sacro Cuore

DIPARTIMENTO DI POLITICA ECONOMICA

Arms Import and Civil Conflict Onset: Risk-Set Evidence from Sub-Saharan Africa, 1960–2022

Anna Balestra

Raul Caruso

Quaderno n. 55/ January 2026

VP VITA E PENSIERO

Università Cattolica del Sacro Cuore

DIPARTIMENTO DI POLITICA ECONOMICA

Arms Import and Civil Conflict Onset: Risk-Set Evidence from Sub-Saharan Africa, 1960–2022

Anna Balestra

Raul Caruso

Working Paper n. 55 - January 2026

VP VITA E PENSIERO

Anna Balestra, Department of Economic Policy & International Peace Science Center (IPSC), Università Cattolica del Sacro Cuore, Milano, Italy

✉ anna.balestra@unicatt.it

Raul Caruso, Department of Economic Policy & International Peace Science Center (IPSC), Università Cattolica del Sacro Cuore, Milano, Italy – European Center of Peace Science, Integration and Cooperation (CESPIC), Catholic University 'Our Lady of Good Counsel', Tirana, Albania

✉ raul.caruso@unicatt.it

Dipartimento di Politica Economica
Università Cattolica del Sacro Cuore – Largo A. Gemelli 1 – 20123 Milano
Tel. 02-7234.2921

✉ dip.politicaeconomica@unicatt.it

https://dipartimenti.unicatt.it/politica_economica

© 2026 Anna Balestra, Raul Caruso

ISBN digital edition (PDF): 978-88-343-6198-6

www.vitaepensiero.it

This E-book is protected by copyright and may not be copied, reproduced, transferred, distributed, rented, licensed or transmitted in public, or used in any other way except as it has been authorized by the Authors, the terms and conditions to which it was purchased, or as expressly required by applicable law. Any unauthorized use or distribution of this text as well as the alteration of electronic rights management information is a violation of the rights of the publisher and of the author and will be sanctioned according to the provisions of Law 633/1941 and subsequent amendments.

PROGETTO DI RICERCA DI RILEVANTE INTERESSE NAZIONALE 2022
Climate Change, Violent Conflicts and Welfare: A Multi-Scale Investigation of Causal Pathways in Different Institutional Contexts

PNRR per la Missione 4, Componente 2, investimento 1.1. Avviso 104/2022 Finanziato dall'Unione europea – Next Generation EU. CClimate-Conflicts – Prot. 2022RSZW83 – CUP J53D23005870008

PRIN 2022
Climate Change, Violent Conflicts and Welf
A Multi-Scale Investigation of Causal Pathw
in Different Institutional Contexts (CC2C)

Abstract

[1] Civil conflicts impose massive costs, yet their onset determinants remain contested. This paper examines whether deliveries of major conventional weapons (MCW) precipitate new intrastate violence episodes, using annual panel data for 46 Sub-Saharan African countries (1960–2022). We construct civil conflict onset on an explicit risk set - excluding ongoing-conflict years per McGrath (2015) - and model duration dependence via cubic peace-years polynomials (Carter and Signorino, 2010). The key regressor is inverse hyperbolic sine-transformed SIPRI TIV deliveries (contemporaneous plus lags), capturing realized coercive inflows. Within-country fixed effects models reveal a robust positive association: unusual delivery spikes elevate onset probability. Placebo leads yield no pre-trends; leave-one-country-out diagnostics confirm broad-based effects. Fiscal capacity enters negatively, supporting crowding-out channels. To connect with climate-related stress pathways, we control for lagged climatic anomalies using the Standardized Precipitation Evapotranspiration Index (SPEI) and its square: these aggregate country-year terms are imprecisely estimated and do not display robust direct effects on onset once fixed effects and institutional covariates are included, but their inclusion leaves the arms-onset relationship essentially unchanged. Results survive nonlinear triangulation (logit, PPML, cloglog), wild bootstrap, and permutation inference. Cumulative availability and binned exposures display monotone escalation. Findings advance Pamp et al. (2018) conditional insight - arms amplify hazard selectively in high-risk settings - via risk-set precision and Sub-Saharan identification, while suggesting that climate-conflict links may be difficult to detect in annual national panels where climatic exposure and its institutional mediation are highly heterogeneous. Policy cautions against unconditioned MCW to fragile recipients, favoring capacity-contingent restraint.

JEL classification: D74, F51, H56, O17, O55, Q54.

Keywords: Civil Conflict Onset, Arms Imports, State Capacity, Climate Stress (SPEI), Sub-Saharan Africa, Risk-set Estimation

[1]We thank participants at the “Peace, Conflicts and the Economy” workshop (UCSC, Milan), NEPS 2025 (Barcelona), ICES 2025 (Belfast), and SIE 2025 (Napoli) for helpful comments and suggestions. Special thanks to Valerio Leone Sciabolazza, Christos Kollias, Todd Sandler, and Luca Pieroni for insightful discussions and encouragement.
The authors acknowledge the financial support of the Italian Ministry of University and Research (MUR), PRIN-2022 project 2022RSZW83 "Climate change, violent conflicts and welfare: A multi-scale investigation of causal pathways in different institutional contexts (CLIMATE-CONFLICTS)" (principal investigator: Raul Caruso).

1 *Introduction*

Civil conflicts impose staggering human and economic costs, killing hundreds of thousands annually and displacing millions while stunting development for generations. Understanding their onset — particularly the role of arms availability — remains a cornerstone challenge for scholars and policymakers. This paper examines whether imports of major conventional weapons (MCW) triggered intrastate violence, in Sub-Saharan Africa (1960–2022), where lumpy SIPRI-recorded imports coincide with recurrent peace-to-war transitions The relationship between arms imports and domestic conflict is empirically contested. Early cross-national studies detect no causal link (Craft, 1999; Dixon, 2009; Suzuki, 2007), relegating shipments to negligible factors amid grievances or feasibility. Regional exceptions emerge in Africa (Craft and Smaldone, 2002), with arms enabling repression (Blanton, 1999) or rebel escalation (Sislin and Pearson, 2001). Pamp et al. (2018) resolve key ambiguities via instrumental variables, revealing that total imports elevate onset risk selectively in high-risk environments — weak institutions, polarization, low capacity — by amplifying coercive capabilities without deterring insurgents.

Theoretical rationales invoke signaling and capacity trade-offs. Rationalist models frame arms as outside-option shifts in asymmetric bargains (Powell, 1999), yet signals may backfire, provoking preemption (Lichbach, 1998; Powell, 1999) or empowering hardliners (Fordham, 2004). Fiscal crowding-out undermines stabilization (Besley and Persson, 2011; Fearon and Laitin, 2003), tipping fragile equilibria toward violence (Collier, 2004).

Despite advances, gaps persist: sparse within-country identification of delivery-onset dynamics; untested micro-foundations in Sub-Saharan panels; underexplored onset-robust estimation amid duration dependence (McGrath, 2015). No study integrates Pamp et al. (2018) style conditioning with explicit risk-set coding, distributed lags, and fiscal mediation.

We address these voids using annual country-year data for 46 Sub-Saharan states. The outcome is civil conflict onset—new episodes after three-year peace windows, excluding ongoing years per McGrath (2015) and Pamp et al. (2018) — estimated on the at-risk set with cubic peace-years polynomials (Carter and Signorino, 2010). The key regressor is inverse hyperbolic sine-transformed SIPRI TIV imports (contemporaneous plus lags), capturing realized coercive inflows. Fixed effects absorb country invariants and common shocks; controls span GDP, democracy, coups, fiscal capacity, resources, MIDs, and SPEI climate stress.

Baseline linear probability models reveal a robust positive association: unusual delivery spikes raise onset probability. Leads are insignificant, ruling out pre-trends; lags concentrate effects near-term. Fiscal capacity enters negatively, supporting crowding-out. Results survive nonlinear triangulation (logit, PPML, cloglog), leave-one-country-out diagnostics, wild bootstrap, and permutation inference. Cumulative availability and binned exposures confirm non-linear escalation.

These findings recast arms as accelerants in high-risk settings, aligning with Pamp et al. (2018) while advancing identification via risk-set precision and Sub-Saharan focus. Policy cautions against unconditioned MCW to fragile recipients, favoring capacity-tied restraint.

Section 2 reviews the literature. Section 3 details data and methods and section 4 presents results. Section 5 offers robustness. Section 6 concludes.

2 *Literature Review*

The relationship between arms imports and domestic conflict remains empirically contested, with quantitative studies offering divergent conclusions on whether total major conventional weapons (MCW) inflows precipitate intrastate violence. Early cross-national analyses, such as Craft (1999); Suzuki (2007) and Dixon (2009), detect no robust causal association between arms imports and civil war onset, relegating them to negligible factors amid dominant drivers like ethnic polarization (Montalvo and Reynal-Querol, 2005), horizontal inequalities (Østby, 2008), or economic feasibility (Collier, 2004). Craft and Smaldone (2002) qualify this null finding regionally, identifying imports as a significant predictor of civil war incidence in sub-Saharan Africa (1967–1997), while Blanton (1999) links them to heightened political repression by lowering the feasibility costs of violence. Sislin and Pearson (2001) extend this to ethnic conflicts, positing that arms embolden rebels and escalate violence levels during crises. These inconsistencies persist in broader samples: Durch (2000) affirms no effect on internal wars across 106 developing states (1970–1995), underscoring measurement challenges in SIPRI trend indicator values (TIVs).

Pamp et al. (2018) advance the literature decisively, deploying simultaneous equations and novel instrumental variables — non-intrastate weapons (e.g., anti-submarine systems, satellites) as proxies for civil war-relevant imports (aircraft, artillery) — to purge endogeneity from 137 countries (1949–2013). Their core insight: total arms imports exert no genuine causal effect on conflict onset in low-risk settings but substantially elevate probability (by up to 20 percentage points) in high-risk environments characterized by weak institutions, polarization, or low state capacity. This conditional escalation arises not from deterrence failure but from amplified coercive capacity sparking preemptive dynamics, aligning with McGrath (2015) onset estimators that mitigate biases in panel binary events.

Theoretical rationales, adapted from interstate models, illuminate these mechanisms amid intrastate asymmetry. Rationalist bargaining frameworks cast arms acquisition as capability enhancement, shifting governments' outside options in costly contests against insurgents (Bueno de Mesquita, 2014; Hirshleifer, 1991; Powell, 1999). Signaling interpretations frame imports as reputation-building signals of resolve, akin to entry deterrence under imperfect information, potentially deterring rebels by inflating their expected costs (Huth, 1988; Kreps and Wilson, 1982; Most and Starr, 2015). Yet countervailing risks loom large: rebels may decode signals as precommitment to force, triggering preemptive strikes before capabilities solidify (Powell, 2006), or exploit them to overcome collective action via provocation (Lichbach, 1998; Walter, 2009). Fordham (2004) further posits that inflows bolster policymakers' willingness to fight by elevating expected utilities or empowering hardliners.

Arms imports reshape state capacity ambivalently, compounding these tensions. Fearon and Laitin (2003) emphasize military weakness as insurgency's root enabler; MCW inflows could stabilize by hiking suppression costs (Buhaug, 2006; Hegre, 2001). Fiscal reallocations, however, undermine this: armament crowds out redistributive investments addressing grievances (Azam, 2001), while non-inclusive institutions prioritize arms over fiscal-legal capacity, rationalizing violence (Besley and Persson, 2011; Cederman et al., 2013). Collier (2004) integrates greed-grievance trade-offs, where arms feasibility tips fragile equilibria toward war; Thies (2010, 2015) corroborates via state capacity panels.

These empirical-theoretical strands reveal gaps: sparse integration of endogeneity-corrected total imports with grievance-feasibility mediators; neglect of rebel micro-foundations in asymmetric signaling; underexplored interactions with polarization or extractive capacity (Boswell and Dixon, 1990; Huber and Mayoral, 2019; Muller and Weede, 1990). No study fully models dynamic arms mediation under Pamp et al. (2018) high-risk conditioning using onset-robust estimators (McGrath, 2015; Verbeek, 2017). This paper fills these voids via heterogeneous effects of total SIPRI imports on conflict propensity.

3 Data & Methodology

This section describes (i) the data construction and measurement choices, (ii) the baseline empirical specification, and (iii) the identification logic and the diagnostic exercises that discipline interpretation. Throughout, the unit of observation is the annual country-year. Unless otherwise noted, all covariates are one-year lagged to reduce mechanical contemporaneous feedback.

3.1 Data: Sample Construction and Measurement

We exploit an annual panel for 46 Sub-Saharan African countries spanning 1960-2022. The country-year is the natural unit because the outcome of interest - civil conflict onset - and the principal explanatory variable - deliveries of major conventional weapons - are both dated at the annual frequency. We document the implied coverage in Appendix B.

The dependent variable captures the initiation of a new civil conflict episode. Conflict information is drawn from the UCDP/PRIO Onset Dataset, using the standard UCDP definition of "armed conflict" as incompatibility-related violence reaching at least 25 battle-related deaths within a calendar year. We focus on civil conflict and construct an onset indicator that requires a sustained pre-onset peace window. Specifically, define:

$$ConflictOnset_{it} = \begin{cases} 1 & \text{if a new civil conflict starts in country } i \text{ in year } t \\ 0 & \text{if no new conflict starts in } (i,t) \\ . & \text{if civil conflict is ongoing in } (i,t). \end{cases}$$

The three-year peace requirement operationalizes "episode initiation" rather than intensity or continuation: an onset occurs only if the conflict begins in year t following at least three consecutive years without active civil conflict.

A central measurement issue in discrete-time onset settings is how to treat years of ongoing conflict. Coding ongoing-conflict years as zero creates two qualitatively different meanings for a zero outcome: (i) the absence of conflict and (ii) the continuation of conflict. This conflation can bias estimates because the model is then forced to interpret "0" both as "peace persists" and "war persists" (McGrath, 2015). Following McGrath (2015) and the implementation in Pamp et al. (2018), we treat ongoing-conflict years as missing and estimate the model on the corresponding risk set. Intuitively, the estimand is the probability of transitioning from peace into a new episode; years in which conflict is already underway are, by definition, not "at risk" of episode initiation.

Conflict onset is not memoryless: the baseline hazard plausibly depends on the time elapsed since the last episode ended. We therefore construct a peace-spell identifier and a "peace-years" counter within spells. Let $AtRisk_{it}$ denote the indicator that a country-year is eligible for episode onset (i.e., not currently in ongoing conflict under the coding above). Within each country, we define peace spells as maximal contiguous runs of $AtRisk_{it} = 1$; within each spell, we define:

$$PeaceYears_{it} = 1, 2, 3, \dots$$

as the within-spell year index, resetting to 1 at the start of each new peace spell. To model duration dependence flexibly, we follow Carter and Signorino (2010) and include a cubic polynomial in $PeaceYears_{it}$ in all onset regressions. The purpose of this term is to absorb systematic hazard dynamics associated with peace duration. In a hazard-style design, explicitly modeling duration dependence is less a robustness flourish than a guard-rail: without it, persistent baseline hazard movements can be spuriously loaded onto other covariates whose trajectories are correlated with time since the last episode.

The main explanatory variable measures imports of major conventional weapons (MCW) by year. Imports are drawn from the SIPRI Arms Transfers Database and measured using SIPRI's Trend Indicator Value (TIV). TIV is designed to proxy the volume of realized transfers at delivery rather than contractual commitments or orders. This distinction is economically relevant: deliveries reflect production schedules, logistics, and exporter authorization decisions that typically unfold over multi-year horizons, so delivery timing is plausibly more predetermined with respect to short-run political shocks than measures based on procurement intent.

Let $\textit{Arms Import}_{it}$ denote total SIPRI TIV deliveries received by country i in year t. We code $\textit{Arms Import}_{it} = 0$ when no deliveries are recorded. The empirical distribution is highly skewed with a substantial mass at zero (Appendix B). For this reason, baseline analysis uses the inverse hyperbolic sine transformation,

$$\textit{IHSArms}_{it} \equiv \operatorname{asinh}(\textit{ArmsDeliveries}_{it}),$$

which behaves like a logarithm for large values but remains well-defined at zero. Baseline specifications include contemporaneous imports and two lags, allowing for delayed effects and reducing sensitivity to within-year ordering. This distributed-lag structure is also conceptually aligned with the notion that delivered equipment may affect domestic coercive capacity, organization, and bargaining conditions over a multi-year horizon rather than instantaneously.

The control set aims to capture standard time-varying correlates of episode initiation emphasized in the conflict literatures, while remaining parsimonious enough to preserve within-country identifying variation after absorbing high-dimensional fixed effects.

Economic covariates include V-Dem latent-variable point estimates of GDP per capita and population from Fariss et al. (2022). Political controls include the V-Dem Electoral Democracy Index, a democratic transition indicator coded -1 for democratic breakdown, 0 for no change, and 1 for democratic transition, and the annual number of successful coups. Resource endowments are proxied by per-capita production-value measures of total non-renewable resources, petroleum, and fuels from Haber and Menaldo (2011). State capacity is captured by an ordinal indicator for the government's primary fiscal revenue source, where higher categories reflect greater ability to raise revenue through formal, market-based and tax-based channels that require information, enforcement, and bureaucratic reach, as opposed to reliance on less capacity-intensive sources. External security conditions are measured using a dummy indicating whether the country is involved in a Militarized Interstate Dispute (MID) in that year, drawn from the Correlates of War MID dataset. We additionally include a climate control. To proxy for climate-related stress and opportunity costs, we include the Standardized Precipitation Evapotranspiration Index (SPEI) and its square, both lagged. In practice, these aggregate country-year climate controls are not expected to be precisely estimated in onset regressions: climatic anomalies plausibly operate through heterogeneous subnational exposure and through mediating institutions and markets, and country-year aggregation can wash out relevant variation. Eventually, we include a lagged indicator for arms sanctions to capture supply-side constraints and shifts in access to external military equipment that may correlate with both deliveries and conflict risk.

3.2 Empirical Strategy

We estimate linear probability models relating episode initiation to arms deliveries, conditioning on the onset risk set and controlling for duration dependence and unobserved heterogeneity. Let $\textit{Conflict Onset}_{it}$ denote the onset indicator defined above, observed only when the country-year is at risk. Our baseline specification is:

$$\textit{ConflictOnset}_{it} = \beta_0 \textit{IHSArms}_{it} + \beta_1 \textit{IHSArms}_{i,t-1} + \beta_2 \textit{IHSArms}_{i,t-2} + g(\textit{PeaceYears}_{it}) + \alpha_i + \lambda_t + \varepsilon_{it}, \quad (1)$$

where α_i are country fixed effects and λ_t are year fixed effects. The function $g(\cdot)$ is implemented as a cubic polynomial in peace-years following Carter and Signorino (2010). Some specifications add country-specific linear trends, implemented as interactions between a linear time index and country indicators, to absorb slow-moving unobservables that could otherwise generate low-frequency co-movement between arms deliveries and onset risk. All inference is based on heteroskedasticity-robust standard errors clustered at the country level. The identifying variation is within-country over time after partialling out common year shocks and time-invariant country factors.

We adopt the linear probability model (LPM) as the baseline estimator for three reasons. First, it transparently maps into a within-country design once fixed effects are included: β_0 is identified from deviations of a country's deliveries from its own long-run mean, net of contemporaneous year shocks common to the region. Second, the LPM accommodates rich fixed-effects structures and multiple lags with minimal friction, which is especially useful when the outcome is rare and the risk-set structure is explicit. Third, the LPM is a convenient platform for the paper's diagnostic architecture: cumulative lag effects, placebo-lead tests, leave-one-country-out influence checks, and permutation-based inference are straightforward and comparable across specifications.

This does not mean functional form is innocuous. Accordingly, we explicitly triangulate across nonlinear alternatives - conditional logit, PPML, and complementary log-log hazard models - in a dedicated robustness section (Table 8), and we include same-sample comparisons to separate estimator differences from sample-selection artifacts induced by separation and singleton dropping.

3.3 Identification

Equation (1) delivers a well-defined within-country association between delivered arms and the probability of episode initiation. Interpreting that association as causal is more demanding. This subsection makes the identification challenges explicit, then motivates the battery of falsification and robustness checks that structure the empirical narrative.

A standard concern is reverse causality. In fact, it may be expected that incipient conflict or realized onset could trigger arms acquisitions, mechanically producing a positive correlation between imports and conflict. Three features of the design attenuate the most direct version of that story.

First, the outcome is episode initiation after a three-year peace window and is defined on a risk set that excludes ongoing-conflict years. By construction, we are not estimating whether arms deliveries rise during conflict; we are estimating whether delivered imports predict the transition from peace into a new episode. Second, the regressor is measured at delivery rather than at order or contract. Deliveries reflect production pipelines and exporter authorization processes that tend to operate over multi-year horizons, so the timing of deliveries observed in year t is often the endpoint of decisions initiated before t. Third, exporters can delay or suspend transfers in response to instability (including in response to sanctions), which works against a mechanical positive contemporaneous feedback from onset to deliveries.

These considerations do not exclude endogeneity. They instead narrow the most immediate reverse-causality concern: the notion that the onset event itself contemporaneously causes the deliveries recorded in that same year.

Two further sources of endogeneity could be especially relevant: (i) anticipation dynamics, whereby procurement and deliveries respond to evolving pre-onset conditions, and (ii) time-varying omitted factors that simultaneously shift delivery flows and the latent propensity for conflict.

(i) Anticipation is a concern because arms deliveries may respond to rising latent conflict risk before that risk is recorded as a discrete onset. Conflict onset is coded at annual frequency and typically represents the culmination of a longer period of political deterioration, mobilization, or low-intensity violence that remains below the coding threshold. Governments may adjust procurement decisions and delivery pipelines in response to these developments. In that case, deliveries can increase prior to the onset year not because arms shipments trigger conflict, but because both shipments and eventual onset reflect a latent escalation process that is only imperfectly observed.

(ii) Time-varying omitted factors are a concern because both deliveries and onset risk may be jointly driven by evolving shocks—fiscal, political, regional — that are not fully captured by fixed effects and observables. Such determinants are not removed by country fixed effects and may not be fully captured by the available controls or parametric time structures. Annual aggregation further tightens the problem by aligning deliveries and onset at the same horizon, potentially compressing multi-year procurement and escalation dynamics into contemporaneous covariation.

The empirical strategy therefore treats baseline fixed-effects estimates as informative but not self-justifying. The paper's contribution is to discipline interpretation using timing tests, estimator triangulation, influence diagnostics, and randomization-based inference designed to be valid in the relevant finite-sample environment.

Instrumental variables would be a natural response to anticipation and omitted variable concerns. We nonetheless do not pursue an IV strategy because the types of share-shift instruments commonly used in related contexts turn to be weak or degenerate in the two-way fixed-effects environment that is required for credible identification in our setting. Appendix A documents four key facts: (i) pre-period supplier shares strongly predict between-country levels of arms imports but do not generate within-country variation once country fixed effects are included; (ii) supplier "shifts" (global exporter fluctuations) transmit only weakly into Sub-Saharan African deliveries; (iii) the standard Bartik index loses predictive power once both country and year effects are absorbed; and (iv) excluded-instrument diagnostics in the fully saturated first stage (Kleibergen-Paap and related statistics) indicate weak identification. In short, while IV would be conceptually attractive, the available candidate instruments do not deliver strong, policy-relevant variation in the only specification we would defend as credible.

Table 1: Variable definitions and sources

Variable	Definition	Source
Onset	Conflict onset indicator: equals 1 in an onset year following at least 3 year of inactivity; coded 0 in peace years at risk; set to missing during continuing-conflict years (excluded from risk set).	UCDP/PRIO onset data
Onset3 (with zeros)	As Onset3 for the 3-year inactivity definition.	UCDP/PRIO onset data
IHS Arms	Arms imports measured as SIPRI deliveries in Trend-Indicator Value (TIV).	SIPRI Arms Transfers Database
GDP per capita	V-Dem latent-variable point estimate of GDP per capita.	V-Dem; Fariss et al. (2021)
Population	V-Dem latent-variable point estimate of population.	V-Dem; Fariss et al. (2021)
Democracy Index	V-Dem Electoral Democracy Index.	V-Dem
demtrans (e_democracy_trans)	Democratic transition coding: -1 democratic breakdown, 0 no change, $+1$ democratic transition.	V-Dem
Coups	Annual number of successful coups.	V-Dem
Natural Resources	Per-capita production-value measure of total non-renewable resources.	Haber and Menaldo (2011)
Oil	Per-capita production-value measure of petroleum.	Haber and Menaldo (2011)
Fuel	Per-capita production-value measure of fuels.	Haber and Menaldo (2011)
SPEI	Standardized Precipitation-Evapotranspiration Index (SPEI), a standardized measure of climatic water balance (drought/wetness) for the country-year.	SPEI Global Drought Monitor (Vicente-Serrano et al.)
Fiscal Capacity	Ordinal proxy for state capacity: government's primary fiscal revenue source.	V-Dem
MID	Dummy equal 1 if the country is involved in a Militarized Interstate Dispute in a given year.	Correlates of War MID

Table 2: Descriptive statistics

Variable	Obs.	Mean	Std. dev.	Min	Max
Onset	2,429	0.032112	0.1763338	0	1
Onset (with zeros)	2,898	0.0269151	0.1618633	0	1
IHS Arms	2,898	1.457241	1.92788	0	7.789041
IHS GDP pc	2,760	2.236822	0.6982746	0	5.301806
IHS Population	2,760	6.885176	1.4998	0	10.64407
IHS Natural Resources	1,892	2.600708	2.784794	0	10.05874
IHS Oil	1,892	0.9671078	2.258132	0	10.00463
IHS Fuel	1,892	1.376355	2.423011	0	10.05822
SPEI	2,346	-0.1703012	0.594366	-1.720621	1.716011
Democracy Breakdown	2,578	0.0085337	0.0920011	0	1
Democracy Transition	2,578	0.0131885	0.1141036	0	1
Coups	2,000	0.0485	0.214874	0	1

4 Results

Table 3 presents baseline linear probability models for civil conflict onset estimated on the risk set defined above, with country and year fixed effects and a cubic polynomial in peace-years capturing duration dependence. Column (1) relates onset in year t to contemporaneous IHS-transformed arms deliveries. The estimated coefficient is positive and statistically significant. Because identification is within-country over time, this estimate indicates that in years when a country receives unusually large deliveries relative to its own mean—net of shocks common to the region—the probability of initiating a new civil conflict episode is higher.

Table 3: Baseline Estimates

	(1)	(2)	(3)	(4)
Dep. Var.	Onset	Onset	Onset	Onset
IHS Arms	0.012***	0.013***	0.011***	0.011***
	(0.003)	(0.004)	(0.004)	(0.004)
Lag 1 of IHS Arms		-0.004	-0.004	-0.003
		(0.003)	(0.003)	(0.003)
Lag 2 of IHS Arms		0.006*	0.006*	0.006*
		(0.003)	(0.003)	(0.003)
Lead 1 of IHS Arms			0.003	0.002
			(0.003)	(0.003)
Lead 2 of IHS Arms			-0.001	-0.002
Peace years	0.009**	0.009**	0.008**	0.012***
	(0.004)	(0.004)	(0.004)	(0.003)
Peace years sq.	-0.000**	-0.000**	-0.000*	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Peace-years cub.	0.000**	0.000**	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.035	-0.042*	-0.040	-8.069***
	(0.022)	(0.022)	(0.024)	(0.916)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country trends	No	No	No	Yes
N	2429	2339	2277	2277
Within R^2	0.014	0.018	0.016	0.066

Notes: Standard errors are clustered at the country level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Columns (2) and (3) interrogate timing. Column (2) adds two lags of deliveries, allowing effects to materialize with delay and reducing sensitivity to within-year ordering. The contemporaneous coefficient remains positive and significant; the first lag is statistically indistinguishable from zero; the second lag is positive but only marginally significant. Column (3) adds two placebo leads. The lead coefficients are statistically insignificant, and thus provide no evidence that future deliveries predict current onset. This pattern is consistent with a near-term association rather than systematic long-horizon pre-trends; at the same time, the annual and rare-event environment limits power to detect subtle anticipation effects, so leads are best interpreted as disciplined diagnostics rather than definitive proof of exogeneity. Sample size declines mechanically as lags and leads are introduced due to edge missingness; Appendix B documents that this attrition is modest and distributionally benign.

Column (4) adds country-specific linear trends, absorbing slow-moving within-country changes that could otherwise generate spurious correlations between procurement cycles and conflict risk (e.g., gradual institutional transitions, demographic trends, long-run changes in security sector organization). The contemporaneous coefficient remains positive and statistically significant, and leads remain insignificant. The within-R^2 rises markedly, as expected when trends soak up low-frequency within-country variation, yet the core association persists.

Two features deserve emphasis for interpretation. First, duration dependence is empirically important: the peace-years polynomial is statistically meaningful across specifications, confirming that onset hazards vary systematically with time since the last episode. Second, inference is based on country-clustered standard errors with 46 clusters. Later sections complement this asymptotic benchmark with wild cluster bootstrap inference and randomization-based p-values designed for finite-sample environments where conventional approximations can be optimistic.

Table 4 evaluates whether the baseline association survives conditioning on a richer set of lagged controls capturing economic conditions, institutions and instability, state capacity, external security, climatic stress, and policy constraints. Column (1) reproduces the distributed-lag baseline. Column (2) adds the full lagged control set (GDP per capita, population, democracy index, democratic transitions/breakdowns, coups, fiscal capacity, MID involvement, SPEI and SPEI^2, and arms sanctions). Because these covariates are not fully observed, the sample contracts sharply. Column (3) therefore re-estimates the baseline on the same sample used in column (2), isolating specification effects from sample composition. Columns (4) and (5) repeat this logic after further adding lagged resource measures (total non-renewable resources, petroleum, and fuels).

The key comparative result is stable: the contemporaneous IHS import term remains positive and statistically significant in the controlled specifications and in the corresponding same-sample baselines. The lag profile remains qualitatively similar, with limited precision on delayed effects once controls are introduced. Among the controls, fiscal capacity enters negatively and significantly in the controlled specifications, consistent with the state-capacity view that stronger fiscal collection is associated with lower onset risk conditional on fixed effects. The SPEI terms are statistically insignificant throughout, a pattern that is common in annual country panels once fixed effects and institutional covariates absorb persistent differences and common shocks; this should not be read as evidence against climate mechanisms per se, but as evidence that national aggregation and a discrete onset outcome provide a blunt reduced-form lens for those channels.

Table 5 focuses on inference. Column (1) estimates the baseline specification under two-way clustering by country and year. The variance-covariance matrix is non-positive semi-definite and we applies a Cameron-Gelbach-Miller adjustment, which is not unusual when one clustering dimension has limited effective variation in a rare-event setting. Column (2) returns to one-way country clustering and yields the same qualitative conclusion: the contemporaneous arms coefficient remains positive and significant. Column (3) reports a wild cluster bootstrap-t test for the contemporaneous coefficient, which constructs p-values and confidence intervals by resampling at the country-cluster level and is therefore more reliable than conventional cluster-robust inference in settings with a moderate number of clusters. The stability of significance across these procedures makes it difficult to dismiss the main result as an artifact of variance estimation, while leaving the identification question to the timing and falsification exercises that follow.

Table 6 re-parameterizes exposure to better align measurement with the economic object of interest: near-term availability of coercive capacity rather than the literal arrival of a delivery in a calendar year. Column (1) uses contemporaneous deliveries. Column (2) replaces the flow with a three-year cumulative availability proxy (deliveries in t, $t-1$, $t-2$), which mechanically reduces sample size by dropping panel edges. Column (3) uses a geometrically discounted index that downweights older deliveries to reflect depreciation, leakage, learning, and the notion that fresh equipment may matter more than older equipment. Across columns, the coefficient remains positive and highly significant. This consistency suggests that the baseline relationship is not an artifact of a particular functional-form choice tied to contemporaneous deliveries, but instead persists under alternative,

Table 4: Baseline Specification with Extended Controls and Matched Samples

Dep. Var.	(1) Onset	(2) Onset	(3) Onset	(4) Onset	(5) Onset
IHS Arms	0.013***	0.011**	0.011**	0.011**	0.011**
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Lag 1 of IHS Arms	-0.004	-0.003	-0.003	-0.004	-0.004
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Lag 2 of IHS imports	0.006*	0.005	0.005	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Peace years	0.009**	-0.003	-0.004	-0.002	-0.004
	(0.004)	(0.005)	(0.006)	(0.004)	(0.005)
Peace-years sq.	-0.000**	0.000	0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Peace-years cub.	0.000**	-0.000	-0.000*	-0.000*	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lag 1 of IHS GDP pc		-0.005		-0.014	
		(0.024)		(0.033)	
Lag 1 of IHS Pop.		0.083		0.086	
		(0.075)		(0.094)	
Lag 1 of Democracy Index		0.082		0.139**	
		(0.053)		(0.058)	
Lag 1 of Democracy breakdown		-0.017		-0.024	
		(0.027)		(0.031)	
Lag 1 of Democracy transition		-0.022*		-0.032**	
		(0.012)		(0.016)	
Lag 1 of Coups		-0.016		-0.010	
		(0.024)		(0.025)	
Lag 1 of Fiscal Capacity		-0.043**		-0.057**	
		(0.020)		(0.023)	
Lag 1 of MID		0.009		0.009	
		(0.018)		(0.018)	
Lag 1 of SPEI		0.006		0.005	
		(0.007)		(0.007)	
Lag 1 of SPEI^2		-0.003		-0.001	
		(0.005)		(0.005)	
Lag 1 of Arms Sanctions		0.039		0.029	
		(0.026)		(0.031)	
Lag 1 of IHS Natural Resources				-0.010	
				(0.008)	
Lag 1 of IHS Oil				-0.012	
				(0.016)	
Lag 1 of IHS Fuel				0.025	
				(0.018)	
Constant	-0.042*	-0.587	-0.008	-0.604	-0.007
	(0.022)	(0.514)	(0.027)	(0.665)	(0.027)
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	2339	1297	1297	1185	1185
Within R^2	0.018	0.032	0.019	0.036	0.017

Notes: All specifications include country and year fixed effects and a cubic polynomial in peace years (time since last conflict). Column (1) reports the baseline distributed-lag model. Column (2) adds a full set of lagged controls. Column (3) re-estimates the baseline model on the same sample as column (2) to isolate sample-composition effects. Column (4) augments column (2) with lagged resource endowments (non-renewable resources, oil, fuel). Column (5) re-estimates the baseline model on the same sample as column (4). Standard errors are clustered at the country level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

economically motivated measures that translate deliveries into short-run arms availability.

Additional robustness checks are reported in the Appendix. Appendix C.1 implements Fisher-style within-

Table 5: Inference Robustness for the Baseline

	(1)	(2)	(3)
Dep. var.	Onset	Onset	Onset
IHS Arms	0.013***	0.013***	0.013***
	(0.003)	(0.004)	(0.004)
Lag 1 of IHS Arms	-0.004	-0.004	-0.004
	(0.003)	(0.003)	(0.003)
Lag 2 of IHS Arms	0.006*	0.006*	0.006*
	(0.003)	(0.003)	(0.003)
Peace years	0.009**	0.009**	0.009**
	(0.003)	(0.004)	(0.004)
Peace years sq.	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)
Peace years cub.	0.000**	0.000**	0.000**
	(0.000)	(0.000)	(0.000)
Constant	-0.042*	-0.042*	-0.044***
	(0.023)	(0.022)	(0.015)
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	2,339	2,339	2,339
Within R^2	0.018	0.018	0.045
Wild bootstrap t			3.530
Wild bootstrap p-value			0.001

Notes: All specifications include country and year fixed effects. Column (1) reports two-way clustered standard errors (country and year); Cameron-Gelbach-Miller adjustment applied when the two-way variance matrix is not positive semi-definite. Columns (2)-(3) cluster at the country level. Column (3) additionally reports a wild cluster bootstrap-t test for the contemporaneous IHS Arms coefficient (bootstrap inference based on country clustering). Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

year permutation (randomization) inference and shows that the observed estimate lies beyond the upper support of the placebo distribution (RI $p = 0.0001$). Appendix C.2 replaces the continuous IHS import measure with delivery bins; the results display a clear monotone pattern in which higher-delivery bins are associated with larger onset probabilities, while small positive deliveries are generally indistinguishable from zero.

Additional robustness checks are reported in the Appendix. Appendix Table C1 implements Fisher-style within-year permutation (randomization) inference Appendix Table C2 together with Figure C2 replaces the continuous IHS import measure with delivery bins; the results display a clear monotone pattern in which higher-delivery bins are associated with larger onset probabilities, while small positive deliveries are generally indistinguishable from zero.

Table 6: Arms Import and Conflict Onset: Alternative Measures of Near-Term Availability

	(1) Onset	(2) Onset	(3) Onset
IHS Arms (flow, t)	0.012***		
	(0.003)		
IHS Arms Availability (3-year cumulative)		0.009***	
		(0.003)	
IHS Arms Availability (discounted)			0.011***
			(0.004)
Peace years	0.009**	0.009**	0.009**
	(0.004)	(0.004)	(0.004)
Peace years sq.	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)
Peace years cub.	0.000**	0.000**	0.000**
	(0.000)	(0.000)	(0.000)
Constant	-0.035	-0.043*	-0.044*
	(0.022)	(0.023)	(0.023)
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	2,429	2,339	2,339
Within R^2	0.014	0.011	0.012

Notes: Arms measures. "IHS Arms (flow, t)" is the inverse hyperbolic sine (IHS) of SIPRI TIV imports in year t. "IHS Arms Availability (3-year cumulative)" is the IHS of cumulative imports over $(t, t-1, t-2)$. "IHS Arms Availability (discounted)" is the IHS of $Arms_t + 0.5Arms_{t-1} + 0.25Arms_{t-2}$. Standard errors clustered at the country level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5 *Robustness and Falsification*

Table 7 asks whether results depend on the preferred risk-set coding. Column (1) reproduces the baseline risk-set model. Column (2) switches to an alternative pooled dependent variable that codes all non-onset observations as zero, including years of ongoing conflict. The contemporaneous import coefficient remains positive and significant, indicating that the headline sign-and-significance pattern is not mechanically created by excluding ongoing-conflict years. However, the dynamic profile changes, with the one-year lag turning negative and significant. We interpret this as evidence that pooling changes the estimand by mixing peace and war continuation into the same outcome category. Column (3) adds the full lagged control set under the pooled coding and the contemporaneous coefficient remains positive and significant.

Table 8 evaluates whether the core relationship between arms imports and civil-conflict onset is sensitive to the functional form and likelihood assumptions imposed by the estimator. The baseline design is held fixed throughout: we estimate on the onset risk set (ongoing-conflict country–years coded missing), include country fixed effects and (where feasible) year fixed effects, and model duration dependence in the onset hazard using a cubic polynomial in peace years. Column (1) reports the two-way fixed-effects linear probability model (LPM), which identifies the effect from within-country changes over time net of common shocks and time-invariant country characteristics. Column (2) switches to a conditional fixed-effects logit. In nonlinear fixed-effects models with rare outcomes, saturated year dummies often induce separation/perfect prediction; we therefore replace year fixed effects with a parsimonious quadratic time trend. Column (3) estimates a PPML model with high-dimensional fixed effects. While PPML is often attractive with many fixed effects and heteroskedasticity, in rare-outcome environments it may drop observations because of separation and singleton issues. Column (4) then re-estimates the LPM on the exact PPML estimation sample, isolating estimator-induced differences from

Table 7: Robustness to Alternative Conflict-Onset Coding

Dep. Var.	(1) Onset	(2) Onset (with zeros)	(3) Onset (with zeros)
IHS Arms	0.013***	0.009***	0.010**
	(0.004)	(0.003)	(0.004)
Lag 1 of IHS Arms	-0.004	-0.005**	-0.004
	(0.003)	(0.002)	(0.003)
Lag 2 of IHS Arms	0.006*	0.003	0.004
	(0.003)	(0.003)	(0.002)
Peace years	0.009**		
	(0.004)		
Peace years sq.	-0.000**		
	(0.000)		
Peace years cub.	0.000**		
	(0.000)		
Lag 1 of IHS GDP pc			0.012
			(0.015)
Lag 1 of IHS Pop.			0.079*
			(0.045)
Lag 1 of Democracy Index			0.098**
			(0.040)
Lag 1 of Democracy breakdown			-0.013
			(0.021)
Lag 1 of Democracy transition			-0.030***
			(0.009)
Lag 1 of Coups			-0.021
			(0.017)
Lag 1 of Fiscal Capacity			-0.030**
			(0.014)
Lag 1 of MID			0.005
			(0.014)
Lag 1 of SPEI			0.003
			(0.007)
Lag 1 of $SPEI^2$			-0.004
			(0.008)
Lag 1 of Arms Sanctions			0.017
			(0.015)
Constant	-0.042*	0.016***	-0.579*
	(0.022)	(0.004)	(0.312)
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	2,339	2,806	1,527
Within R^2	0.018	0.008	0.018

Notes: Standard errors clustered at country level are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

sample-composition differences. Column (5) reports a complementary log-log specification, which corresponds to a discrete-time hazard model and is frequently used for onset processes.

The contemporaneous coefficient on arms import is positive and statistically significant in every column. Since coefficient scales differ across estimators (probability points in the LPM versus index-function coefficients in logit/cloglog and multiplicative mean effects in PPML), the meaningful comparison is qualitative rather than literal magnitude. The stable sign across estimators indicates that the baseline association is not an artifact of linearity, nor of any single likelihood assumption about the conditional mean of onset. Across

Table 8: Estimator Robustness: Civil Conflict Onset and Arms Imports

	(1) LPM (TWFE)	(2) FE Logit	(3) PPML (HDFE)	(4) LPM on (3) sample	(5) FE Cloglog
IHS Arms	0.013***	0.353***	0.329***	0.020***	0.350***
	(0.004)	(0.091)	(0.082)	(0.005)	(0.098)
Lag 1 of IHS Arms	-0.004	-0.174*	-0.134	-0.007	-0.123
	(0.003)	(0.093)	(0.088)	(0.005)	(0.101)
Lag 2 of IHS Arms	0.006*	0.167**	0.215***	0.012**	0.235***
	(0.003)	(0.083)	(0.078)	(0.005)	(0.090)
Peace years	0.009**	0.089	0.193**	0.012**	0.217**
	(0.004)	(0.072)	(0.076)	(0.005)	(0.089)
Peace years sq.	-0.000**	-0.003	-0.007**	-0.000*	-0.008**
	(0.000)	(0.003)	(0.003)	(0.000)	(0.003)
Peace years cub.	0.000**	0.000	0.000***	0.000*	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Year		0.472			
		(2.449)			
Year sq.		-0.000			
		(0.001)			
Constant	-0.042*		-4.685***	-0.061*	-4.102***
	(0.022)		(0.627)	(0.034)	(1.027)
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	Yes	Yes
Quadratic time trend	No	Yes	No	No	No
Observations	2,339	1,790	1,295	1,295	1,295

Notes: The dependent variable is civil conflict onset, estimated on the McGrath-style risk set. Column (1) is the baseline two-way fixed-effects linear probability model (LPM). Column (2) reports a conditional fixed-effects logit; year dummies are replaced by a quadratic time trend to mitigate separation/perfect prediction in nonlinear fixed-effects settings. Column (3) estimates PPML with high-dimensional fixed effects; observations may be dropped due to separation/singletons. Column (4) re-estimates the LPM on the exact estimation sample used in column (3). Column (5) estimates a complementary log-log discrete-time hazard model with fixed effects. Standard errors are clustered at the country level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

specifications, the dynamic profile is broadly consistent: the second lag is typically positive and often statistically significant, while the first lag is weaker and less precisely estimated (and in some nonlinear specifications turns negative at conventional marginal levels). Given the persistence and lumpiness of arms import at annual frequency, adjacent lags can be highly collinear; the main takeaway is therefore not a sharp year-by-year causal timing claim, but that the association is concentrated in contemporaneous and short-horizon past deliveries rather than being washed out (or reversed) under alternative estimators. The PPML column is estimated on a reduced sample because the combination of rare onsets and many fixed effects can generate separation and singletons. Column (4) shows that, when the LPM is estimated on the exact same observations as PPML, the qualitative conclusion remains unchanged: arms deliveries retain a positive and significant association with onset. This indicates that the corroboration provided by PPML is not simply a by-product of estimating on a different subset of the data. Taken together, Table 8 provides estimator-level triangulation: the positive relationship between arms imports and conflict onset appears robust to moving from a linear probability model to nonlinear fixed-effects models and to a PPML specification with high-dimensional fixed effects, and it persists when differences in feasible estimation samples are explicitly controlled for.

Table 7 extends the placebo-lead logic to longer horizons, augmenting the baseline model with multiple leads (up to $t+4$ and $t+6$) while retaining contemporaneous deliveries, two lags, duration dependence, year effects, and country-specific trends. Extending lead horizons mechanically reduces sample size. The contemporaneous coefficient remains positive and significant, while leads are jointly insignificant (joint placebo p-values 0.439 and 0.294, respectively). This pattern weakens a long-horizon pre-trend interpretation and is consistent with a near-term relationship between delivered capacity and episode initiation.

Table 9: Long-Horizon Lead Placebos: Arms Imports and Civil Conflict Onset

Dep. Var.	(1) Onset	(2) Onset
IHS arms imports (deliveries)	0.011**	0.011*
	(0.004)	(0.004)
Lag 1 of IHS Arms	-0.003	-0.003
	(0.003)	(0.003)
Lag 2 of IHS Arms	0.006	0.007
	(0.003)	(0.003)
Lead 1 of IHS Arms	0.002	0.002
	(0.003)	(0.003)
Lead 2 of IHS Arms	-0.000	0.000
	(0.003)	(0.003)
Lead 3 of IHS Arms	-0.003	-0.004
	(0.003)	(0.003)
Lead 4 of IHS Arms	0.003	0.002
	(0.002)	(0.002)
Lead 5 of IHS Arms		0.000
		(0.004)
Lead 6 of IHS Arms		0.004
		(0.004)
Peace years	0.012**	0.011**
	(0.004)	(0.004)
Peace years^2	-0.000	-0.000
	(0.000)	(0.000)
Peace years^3	0.000	0.000
	(0.000)	(0.000)
Constant	-8.660***	-9.880***
	(0.900)	(0.998)
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Country-specific linear trends	Yes	Yes
Observations	2,215	2,146
Joint test: leads (all)	0.96	1.26
p-value (joint leads test)	0.439	0.294

Notes: Standard errors are clustered at the country level. The joint test reports an F-test of the null that all lead coefficients included in the column are equal to zero (placebo lead test). $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10: Leave-One-Country-Out Influence Diagnostics (LOCO)

Statistic (across LOCO re-estimates)	Mean	Median	Min	Max
Coefficient on IHS Arms ($\hat{\beta}$)	0.013	0.012	0.010	0.013
t-statistic for IHS arms imports	3.082	3.102	2.878	3.390

Notes: This table summarizes a leave-one-country-out (LOCO) influence analysis. We re-estimate the baseline two-way fixed-effects linear probability model 46 times, each time excluding one country from the risk-set sample and recording the contemporaneous coefficient on IHS Arms, its clustered standard error (clustered by country), and the implied t-statistic. The baseline specification includes country and year fixed effects and a cubic polynomial in peace years to flexibly absorb duration dependence in civil conflict onset. The reported moments summarize the distribution of LOCO coefficients and t-statistics across the 46 re-estimates. The standard deviations are 0.001 for $\hat{\beta}$ and 0.128 for the t-statistic.

Table 10 evaluates whether the baseline relationship between arms deliveries and civil conflict onset is disproportionately driven by a single country. In panels of this kind, this concern is nontrivial: (i) onsets are rare events, (ii) arms deliveries are lumpy and highly skewed, and (iii) clustered inference is computed over a moderate number of country clusters. A small number of influential countries - for example those experiencing large procurement episodes or unusually concentrated conflict dynamics - could therefore exert outsized leverage on the estimated contemporaneous arms coefficient. To probe this, we conduct a leave-one-country-out (LOCO) influence diagnostic. Concretely, we re-estimate the baseline model 46 times, each time dropping one country, while holding fixed the specification (country and year fixed effects, cubic peace-years polynomial, and country-clustered standard errors). For each re-estimate we store the contemporaneous coefficient on IHS-transformed arms deliveries and its corresponding t-statistic. The identifying variation in each run remains within-country over time, but the LOCO procedure reveals whether any single unit is mechanically anchoring the baseline estimate. The distribution of LOCO estimates is tight and uniformly positive. The contemporaneous coefficient has mean 0.013 and median 0.012, with a minimum of 0.010 and a maximum of 0.013 (standard deviation 0.001). Inference is similarly stable: the associated t-statistics have mean 3.082 and median 3.102, ranging from 2.878 to 3.390 (standard deviation 0.128). Even the most influential exclusions - Angola, Mozambique, the Democratic Republic of Congo, Togo, and Liberia for the smallest coefficients, and Senegal, Sudan, Zambia, Rwanda, and Uganda for the largest - shift the point estimate only modestly and do not threaten sign or conventional statistical significance.

Two implications follow. First, the baseline association is not a "single-country result": it does not hinge on the conflict history or delivery profile of one outlier unit. Second, the estimate appears to reflect a broad-based pattern across the panel rather than a small set of extreme-delivery years concentrated in one country. This strengthens the robustness case against leverage-driven findings and supports interpreting the baseline estimates as representative of within-country co-movement between delivered arms and the risk of episode initiation.

6 *Conclusion*

The analysis establishes a robust positive association between arms deliveries and the onset of civil conflict episodes in Sub-Saharan Africa. Countries receiving unusually large shipments of major conventional weapons—relative to their own historical norms, net of common regional shocks—experience a marked increase in the probability of transitioning from sustained peace into organized armed violence. This relationship holds across distributed lags, alternative measures of arms availability, and rich conditioning sets encompassing economic conditions, institutions, resources, fiscal capacity, external security threats, and climatic stress. Placebo leads provide no evidence of systematic pre-trends, while leave-one-country-out diagnostics confirm the finding is broad-based rather than outlier-driven. Estimator triangulation—from linear probability models to conditional logit, PPML, and complementary log-log hazard specifications—yields consistent qualitative evidence of near-term escalation risk tied to delivered coercive capacity.

These results advance the empirical literature in three ways. First, they replicate Pamp et al. (2018) conditional insight—that total SIPRI imports elevate onset hazard selectively in high-risk environments—using explicit risk-set coding and Sub-Saharan panels spanning 1960–2022. Unlike prior cross-national designs averaging over heterogeneous conflict-prone regions, the within-country identification here isolates delivery spikes from procurement intent, leveraging the multi-year horizon of production pipelines to attenuate mechanical reverse causality. Second, the stability across nonlinear onset estimators and duration-dependence controls (cubic peace-years polynomials) addresses McGrath (2015) methodological critique, confirming that arms effects are not artifacts of pooled coding or unmodeled hazard dynamics. Third, fiscal capacity enters negatively and significantly, lending direct support to state-capacity channels: armament crowds out revenue mobilization, amplifying vulnerability in fragile equilibria (Fearon and Laitin, 2003; Thies, 2010, 2015).

Theoretically, the patterns align with signaling ambiguities in asymmetric contests. Rationalist frameworks anticipate arms acquisition as outside-option enhancement (Powell, 1999), yet backfire risks—preemptive rebel mobilization (Lichbach, 1998; Powell, 2006) or hardliner empowerment (Fordham, 2004) — dominate in practice. Insignificant leads weaken deterrence interpretations (Huth, 1988; Kreps and Wilson, 1982), while contemporaneous escalation implicates feasibility shifts tipping grievances into violence (Collier, 2004). Null first lags suggest effects concentrate on fresh deliveries, consistent with organizational learning and depreciation rather than persistent stockpiles.

Policy implications follow directly. For donors, the findings caution against indiscriminate MCW transfers to at-risk recipients: even absent overt instability, large shipments signal vulnerability to latent escalation, exacerbating repression-rebel spirals (Blanton, 1999; Sislin and Pearson, 2001). Conditional aid tying deliveries to fiscal-legal reforms could mitigate crowding-out, bolstering extractive capacity without inflating hazard (Besley and Persson, 2011). Recipient governments face a stark trade-off: short-run coercive boosts invite preemption, while underinvestment perpetuates weakness (Cederman et al., 2013). Regional bodies might prioritize arms-transparency protocols, monitoring TIV spikes against peace-spell durations to preempt delivery-onset co-movements.

Limitations clarify the scope. Annual aggregation compresses subnational dynamics—elite bargains, militia formation, or climate-mediated grievances (SPEI controls notwithstanding) — potentially masking heterogeneous exposure. The Sub-Saharan focus, while sharpening identification amid lumpy deliveries, limits external validity to low-capacity contexts. Instruments falter in two-way fixed effects (Appendix A), underscoring that supplier shifts weakly transmit to episodic African imports — ruling out Bartik but highlighting political mediation over market mechanics.

In sum, arms imports do not ignite conflict ex nihilo but decisively elevate hazard when layered atop fragility. By conditioning on risk sets and purging endogeneity via timing, this paper resolves longstanding empirical ambiguities, recasting deliveries as accelerants in grievance-feasibility bargains. The policy pivot — from volume caps to capacity-contingent restraint — offers a tractable lever amid rising strategic arms flows.

References

Azam, J.-P. (2001). The redistributive state and conflicts in Africa. Journal of Peace Research, 38:429–444.

Besley, T. and Persson, T. (2011). The logic of political violence. The Quarterly Journal of Economics, 126:1411–1445.

Blanton, S. L. (1999). Instruments of security or tools of repression? arms imports and human rights conditions in developing countries. Journal of Peace Research, 36:233–244.

Boswell, T. and Dixon, W. J. (1990). Dependency and rebellion: A cross-national analysis. American Sociological Review, 55:540.

Bueno de Mesquita, B. (2014). Principles of International Politics: War, Peace, and World Order. CQ Press, 5 edition.

Buhaug, H. (2006). Relative capability and rebel objective in civil war. Journal of Peace Research, 43:691–708.

Carter, D. B. and Signorino, C. S. (2010). Back to the future: Modeling time dependence in binary data. Political Analysis, 18(3):271–292.

Cederman, L.-E., Gleditsch, K. S., and Buhaug, H. (2013). Inequality, Grievances, and Civil War. Cambridge University Press.

Collier, P. (2004). Greed and grievance in civil war. Oxford Economic Papers, 56:563–595.

Craft, C. and Smaldone, J. P. (2002). The arms trade and the incidence of political violence in Sub-Saharan Africa, 1967-97. Journal of Peace Research, 39:693–710.

Craft, C. B. (1999). Weapons for Peace, Weapons for War. Routledge.

Dixon, J. (2009). What causes civil wars? integrating quantitative research findings. International Studies Review, 11:707–735.

Durch, W. J. (2000). Constructing Regional Security: The Role of Arms Transfers, Arms Control, and Reassurance. Palgrave Macmillan, Houndmills, Basingstoke.

Fariss, C. J., Anders, T., Crabtree, C. D., Jones, Z. M., Markowitz, J. N., and Barnum, M. E. (2022). New estimates of over 500 years of historic GDP and population data. Journal of Conflict Resolution, 66(9):1579–1603.

Fearon, J. D. and Laitin, D. D. (2003). Ethnicity, insurgency, and civil war. American Political Science Review, 97:75–90.

Fordham, B. O. (2004). A very sharp sword: The domestic sources of military force quality and international conflict outcomes. Journal of Conflict Resolution, 48(5):724–748.

Haber, S. and Menaldo, V. (2011). Do natural resources fuel authoritarianism? a reappraisal of the Resource curse. American Political Science Review, 105(1):1–26.

Hegre, H. (2001). Toward a democratic civil peace? Democracy, political change, and civil war, 1816–1992. American Political Science Review, 95:33–48.

Hirshleifer, J. (1991). The paradox of power. Economics Politics, 3:177–200.

Huber, J. D. and Mayoral, L. (2019). Group inequality and the severity of civil conflict. Journal of Economic Growth, 24:1–41.

Huth, P. K. (1988). Extended deterrence and the outbreak of war. American Political Science Review, 82(1):189–217.

Kreps, D. M. and Wilson, R. (1982). Reputation and imperfect information. Journal of Economic Theory, 27:253–279.

Lichbach, M. I. (1998). The Rebel's Dilemma. Economics, Cognition, and Society. University of Michigan Press.

McGrath, L. F. (2015). Estimating onsets of binary events in panel data. Political Analysis, 23:534–549.

Montalvo, J. G. and Reynal-Querol, M. (2005). Ethnic polarization, potential conflict, and civil wars. American Economic Review, 95:796–816.

Most, B. A. and Starr, H. (2015). Inquiry, logic and international politics. University of South Carolina Press.

Muller, E. N. and Weede, E. (1990). Cross-national variation in political violence. Journal of Conflict Resolution, 34:624–651.

Østby, G. (2008). Polarization, horizontal inequalities and violent civil conflict. Journal of Peace Research, 45:143–162.

Pamp, O., Rudolph, L., Thurner, P. W., Mehltretter, A., and Primus, S. (2018). The build-up of coercive capacities: Arms imports and the outbreak of violent intrastate conflicts. Journal of Peace Research, 55:430–444.

Powell, R. (1999). In the Shadow of Power: States and Strategies in International Politics. Princeton University Press.

Powell, R. (2006). War as a commitment problem. International Organization, 60.

Sislin, J. and Pearson, F. F. (2001). Arms and Ethnic Conflict. Rowman Littlefield Publishers.

Suzuki, S. (2007). Major arms imports and the onset of civil and ethnic wars in the postcolonial world, 1956–1998: A preliminary reassessment. The Social Science Journal, 44:99–111.

Thies, C. G. (2010). Of rulers, rebels, and revenue: State capacity, civil conflict and the onset of political order. Journal of Peace Research, 47(6):713–725.

Thies, C. G. (2015). The temporal dynamics of state capacity and civil conflict. Research Politics, 2(3):1–9.

Verbeek, M. (2017). A Guide to Modern Econometrics. Wiley.

Walter, B. F. (2009). Bargaining failures and civil war. Annual Review of Political Science, 12:243–261.

A Appendix: Instrumental-Variables Strategy

This appendix clarifies why we do not pursue a Bartik-style shift–share IV strategy for arms import. The obstacle is not construction — one can always mechanically define pre-period exposure shares and interact them with supplier-level "shifts" — but usable identifying variation in the saturated two-way fixed-effects setting required for a credible design.

As a first diagnostic, we document the cross-sectional relevance of alternative pre-period supplier shares. Using two exposure windows — (i) 1960–1975 and (ii) 1960–1979 — we relate each country's post-1979 average arms-import intensity (measured as the inverse hyperbolic sine of SIPRI-TIV deliveries, averaged over 1980–2022) to the corresponding vector of pre-period supplier shares (China, Russia, United States, and Europe [2]). Because the dependent variable is collapsed to a country mean, the regression exploits between-country variation only. Standard errors are heteroskedasticity-robust, and we report a joint test of the four shares.

Table A1 shows that historical supplier composition is informative about later average import intensity: the shares are jointly significant at conventional levels, with especially strong predictive content for the Russia/USSR and Europe exposure measures. This pattern is consistent with persistent procurement relationships and long-lived political–military ties. Importantly, however, this is precisely not the kind of variation that identifies our baseline models. In a two-way fixed-effects environment, time-invariant shares are absorbed by country fixed effects; what remains for a Bartik instrument to work is differential within-country transmission of common supplier shocks over time. Table A1 therefore establishes a key point: pre-period shares are relevant mainly through cross-sectional levels, which does not translate into a strong first stage once identification is restricted to within-country variation net of common shocks.

Column (3) turns to the "shift" component—exporter-wide fluctuations intended to proxy common supply-side movements. We collapse the data to a year-level time series (one observation per year, 1980–2022) and regress the SSA mean of IHS arms imports on exporter shifts (measured in levels and scaled by 1,000). Unlike the shares, these shifts show limited explanatory power: coefficients are small and imprecisely estimated, the regressors are jointly insignificant, and overall fit is low. Substantively, this indicates weak and irregular transmission of exporter-level global movements into the SSA import series at the annual frequency. Methodologically, it foreshadows the central first-stage problem for Bartik-style designs in this setting: even before interacting shifts with exposure shares, the shifters themselves do not generate strong time-series signal for SSA deliveries, making it difficult to obtain a strong, policy-relevant source of within-country variation once the two-way fixed-effects structure of the main specifications is imposed.

Table A2 makes a simple but consequential point: once we impose the fixed-effects structure required for a defensible within-country design, Bartik-style share–shift variation has little remaining power to explain arms imports in Sub-Saharan Africa. That empirical fact is consistent with a broader economic interpretation—SSA behaves less like a market that mechanically absorbs global exporter "supply shocks," and more like a politically mediated, episodic destination whose deliveries are weakly tied to aggregate external fluctuations.

Columns (1)–(3) use baseline supplier shares computed over 1960–1975. With year fixed effects only (column 1), the Bartik index significantly predicts IHS imports. With country fixed effects only (column 2), it also predicts IHS imports. But in the two-way fixed-effects specification (column 3) — the environment that parallels the main empirical design — the coefficient shrinks sharply and becomes only marginally significant. This is exactly what one would expect if the "relevance" in the looser specifications is coming from components that two-way FE deliberately remove: persistent cross-country exposure (shares) and common global movements (shifts). What must remain for a credible first stage is differential within-country pass-through of

[2] Throughout, 'Europe' refers to the four major exporters—France, Germany, Italy, and the United Kingdom. For the Soviet period, we treat the USSR as Russia prior to the 1991 collapse, so that pre-1991 observations are assigned to Russia for consistency over time.

Table A1: Predictive Content of Pre-Period Supplier Shares and Exporter Shifts for Post-1980 Arms Imports

	(1)	(2)	(3)
Dep Var.	IHS Arms	IHS Arms	IHS Arms
China share (1960–1975)	0.156		
	(0.479)		
Russia share (1960–1975)	2.000**		
	(0.802)		
U.S. share (1960–1975)	1.385		
	(1.366)		
Europe share (1960–1975)	0.655**		
	(0.291)		
China share (1960–1979)		0.807	
		(0.811)	
Russia share (1960–1979)		1.385**	
		(0.583)	
U.S. share (1960–1979)		1.178	
		(0.911)	
Europe share (1960–1979)		0.970**	
		(0.403)	
China shift			0.040
			(0.086)
Russia shift			0.547
			(0.547)
U.S. shift			0.128
			(0.131)
Europe shift			-0.390
			(0.268)
Constant	0.831***	0.630**	1.720
	(0.192)	(0.259)	(1.141)
Joint test (F/Chi^2)	2.858	2.354	0.921
p-value	0.035	0.070	0.462
N	46	46	43
R^2	0.219	0.112	0.081

Notes: Columns (1)-(2) collapse the panel to one observation per country, using the post-1980 (1980-2022) mean of inverse-hyperbolic-sine (IHS) arms imports as the dependent variable. Regressors are pre-period supplier shares: the fraction of total arms imports sourced from each exporter over 1960-1975 in Column (1) and 1960-1979 in Column (2). Column (3) collapses the panel to one observation per year (1980–2022) and regresses the annual SSA mean of IHS arms imports on exporter shifts, measured in levels and scaled by 1,000 for readability. Shifts capture exporter-wide fluctuations net of the recipient-country flow (i.e., global deliveries excluding country i), so they are conceptually year-level supply movements. All regressions report heteroskedasticity-robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

exporter shocks over time, and that residual variation is limited.

Columns (4)–(6) repeat the exercise with baseline shares computed over 1960–1979. The pattern is even starker. The Bartik index is strongly predictive under one-way fixed effects (columns 4–5), but becomes small and statistically indistinguishable from zero under country and year fixed effects simultaneously (column 6). In other words, the instrument "works" when it is allowed to load on between-country exposure differences or common time movements, but it largely stops working when the design demands the kind of within-country, net-of-common-shocks variation that IV needs in a two-way FE panel.

The economic reading is not merely statistical. If SSA arms deliveries were systematically driven by global exporter fluctuations — say, by broad production cycles, exporter-side capacity expansions — then exporter "shifts" would transmit into SSA imports in a way that survives year effects once interacted with predetermined exposure. Instead, the weak two-way-FE relevance suggests limited and unstable transmission: SSA imports appear less dependent on aggregate external shocks and more shaped by idiosyncratic, country-specific factors. Under this interpretation, the Bartik design fails not because the instrument cannot be constructed, but because the underlying market does not provide the stable pass-through that shift–share identification requires.

Table A2: Bartik Relevance Under Alternative Fixed-Effects Structures

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	IHS Arms	IHS Arms	IHS Arms	IHS Arms	IHS Arms	IHS Arms
Bartik (1960-1975 shares × shifts)	0.485***	0.313***	0.181*			
	(0.073)	(0.091)	(0.101)			
Bartik (1960-1979 shares × shifts)				0.478***	0.298***	0.165
				(0.077)	(0.094)	(0.102)
Constant	1.208***	1.305***	1.380***	1.213***	1.314***	1.389***
	(0.129)	(0.051)	(0.057)	(0.132)	(0.053)	(0.057)
Country FE	No	Yes	Yes	No	Yes	Yes
Year FE	Yes	No	Yes	Yes	No	Yes
Observations	1,978	1,978	1,978	1,978	1,978	1,978
R^2	0.165	0.330	0.368	0.160	0.330	0.368

Notes: The table reports reduced-form regressions of IHS arms imports on a Bartik-style index constructed as baseline supplier shares interacted with contemporaneous supplier "shifts." Columns (1)–(3) use shares computed over 1960–1975; columns (4)–(6) use shares computed over 1960–1979. The Bartik indices are scaled by 10^{-6} (i.e., expressed in millions of TIV units) for readability. All regressions are estimated on the post-1979 sample (1980–2022). Standard errors are robust. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3 takes the Bartik instrument into the same specification space as the baseline design: the onset risk set, two-way fixed effects, and country-specific trends, plus the full baseline controls (two lags and cubic duration dependence). This is exactly where a shift–share instrument must deliver identifying variation if it is to support an IV interpretation.

The first-stage results show that it does not. In columns (1) and (4), the excluded instrument coefficients are small and statistically insignificant, and the excluded-instrument F-statistics are 1.88 and 2.24 (p-values 0.178 and 0.141). These values are far below conventional weak-instrument benchmarks and align with the broader diagnostic pattern in the appendix: once we partial out year shocks, country levels, and smooth country-specific trends, very little independent movement remains in the Bartik index that can meaningfully predict within-country changes in import intensity.

Columns (2)–(3) and (5)–(6) then illustrate the mechanical implication. With a weak first stage, IV estimates become uninformative: the coefficient on imports in the onset equation is imprecise and not statistically distinguishable from zero under either 2SLS or LIML. Substantively, this suggests that SSA import dynamics in the saturated specification are not strongly driven by exporter-level aggregate shocks as transmitted through predetermined supplier exposure. Put differently, after controlling for common time shocks and country-specific trajectories, SSA arms-import intensity appears to be shaped mainly by country-specific factors rather than pass-through from global supplier fluctuations — precisely why a Bartik-style design does not generate a credible first stage in this setting.

Finally, we consider an alternative Bartik-style instrument of the form

$$Z_{it} = \underbrace{\text{Share}_i^{SSA}}_{\text{baseline exposure}} \times \underbrace{\left(\text{World}_t - \text{SSA}_t\right)}_{\text{common shift}}, \tag{2}$$

where Share_i^{SSA} is country i's baseline share of SSA arms imports in global arms transfers in the period 1960-1975 (in percent), and the shift is the rest-of-world (ROW) level of global arms imports, $\text{World}_t - \text{SSA}_t$. Conceptually, the instrument assigns each country a fixed "exposure" to SSA's position in the global market and

Table A3: Bartik IV in the Two-Way Fixed-Effects Design

	Baseline shares 1960–1975			Baseline shares 1960–1979		
	(1) First stage	(2) 2SLS	(3) LIML	(4) First stage	(5) 2SLS	(6) LIML
Bartik index	0.069			0.069		
	(0.050)			(0.046)		
IHS Arms		-0.067	-0.067		-0.065	-0.065
		(0.190)	(0.190)		(0.177)	(0.177)
Lag 1 of IHS Arms	0.199***	0.012	0.012	0.199***	0.011	0.011
	(0.054)	(0.037)	(0.037)	(0.054)	(0.035)	(0.035)
Lag 2 of IHS Arms	0.086**	0.014	0.014	0.086**	0.013	0.013
	(0.040)	(0.016)	(0.016)	(0.040)	(0.015)	(0.015)
Peace years	0.035	0.014*	0.014*	0.035	0.014*	0.014*
	(0.033)	(0.007)	(0.007)	(0.033)	(0.007)	(0.007)
Peace years sq.	-0.002	-0.001	-0.001	-0.002	-0.001	-0.001
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Peace years cub.	0.000*	0.000	0.000	0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-106.098***			-106.147***		
	(23.157)			(23.166)		
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	1.88	1.88	1.88	2.24	2.24	2.24
p-value	0.178	0.178	0.178	0.141	0.141	0.141
Observations	1,433	1,433	1,433	1,433	1,433	1,433

Notes: Columns (1) and (4) report the saturated first-stage regression of asinh(imports) on the Bartik index (Z), controlling for two lags of imports and cubic duration dependence in peace years, with country FE, year FE, and country-specific linear trends (year $\times$ country). Columns (2)–(3) and (5)–(6) report 2SLS and LIML estimates of the onset equation instrumenting contemporaneous imports with Z. Country-specific trend coefficients are included. Standard errors are clustered at the country level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

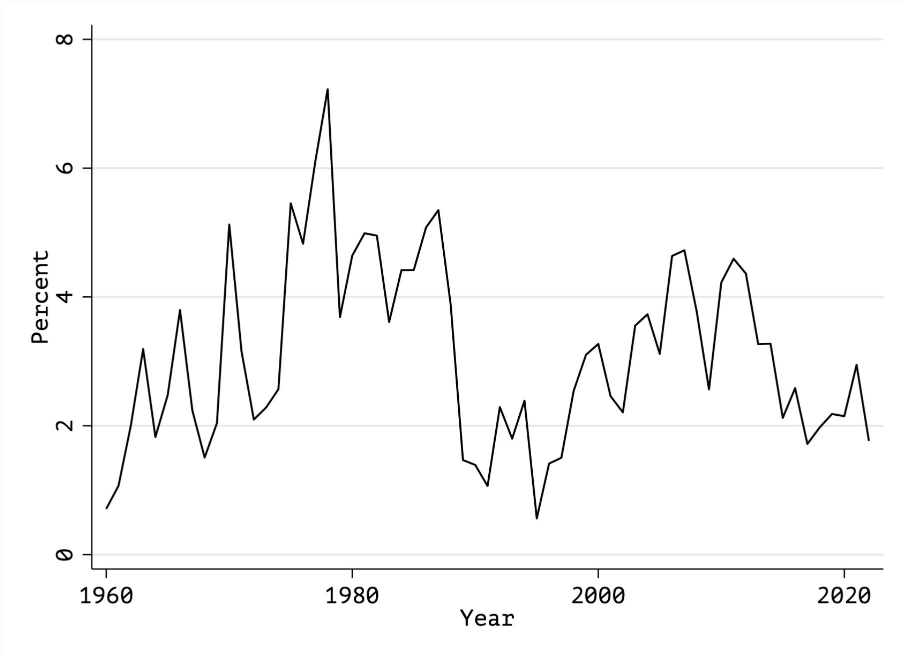

Figure A1: SSA share of global arms transfers over time.

interacts it with aggregate non-SSA fluctuations that are plausibly exogenous to any single SSA country.

Under the two-way fixed-effects structure required for identification, however, this design is mechanically degenerate. With country fixed effects, Share_i^{SSA} is absorbed because it is time-invariant; with year fixed effects, $(\text{World}_t - \text{SSA}_t)$ is absorbed because it is common across countries. As a result, Z_{it} contains no remaining within-country time variation once we partial out both sets of fixed effects. The residualization diagnostic confirms this directly: regressing Z on country and year fixed effects yields essentially zero within-R^2 and produces residuals centered at zero with negligible dispersion, indicating collinearity with the fixed effects.

This is not merely an algebraic artifact. Figure A1 shows that SSA represents only a small fraction of global arms transfers (on average about three percent), with fluctuations modest relative to world totals. In such a setting, global movements are overwhelmingly driven by ROW dynamics, while SSA imports may reflect episodic political, logistical, and contracting constraints rather than smooth pass-through of "world shocks." This aligns with our earlier evidence using historical supplier shares and exporter "shifts": baseline shares can look predictive in cross-sectional comparisons (because they proxy for persistent differences in exposure), but the time-series shock component does not generate strong, clean within-country variation for imports once the panel is treated appropriately.

Table A4: SSA Share in Global Arms Deliveries

Variable	Sample	N	Mean	Std. dev.	Min–Max
SSA Imports	1960–1975	16	732.688	460.395	126–2003
World Imports	1960–1975	16	26929.880	6807.036	16544–36733
SSA share of world (%)	Full sample	63	3.103	1.431	0.561–7.225

Notes: SSA and *World* are total arms deliveries in levels (SIPRI TIV). The SSA share is defined as $100 \times \text{SSA}_t/\text{World}_t$ and is reported in percentage points. The first two rows summarize annual values over 1960–1975 (16 years). The share series is summarized over the full available span in the data (63 annual observations).

The regression evidence in Table A4 makes the same point transparent. When we estimate specifications with year fixed effects only, Z can appear statistically relevant - because the identifying variation is effectively between-country, loading on persistent differences correlated with Share_i^{SSA}. But in the two-way fixed-effects environment that matters for credible identification, the instrument collapses by construction. In short, the apparent "power" of this SSA-based Bartik comes from the wrong place (time-invariant cross-country exposure), while the variation we need for IV - within-country changes driven by plausibly exogenous shifts - either vanishes mechanically or is too weak to support a defensible first stage.

Table A5: Diagnosing the identifying content of SSA-Exposure Bartik under alternative fixed-effects structures (post-1980)

Dep. var.	(1) SSA-Exposure Bartik	(2) IHS Arms
SSA-Exposure Bartik		206.861***
		(64.097)
Constant	0.001***	1.091***
	(0.000)	(0.204)
Country FE	Yes	No
Year FE	Yes	Yes
Country-specific linear trends	No	Yes
Additional controls	No	Yes
F-test on $z3$ (excluded)		10.42
p-value		0.003
Observations	1,512	1,125
R^2	0.859	0.391

Notes: Sample restricted to 1980–2022. Column (1) is a pure fixed-effects decomposition of SSA-Exposure Bartik on country and year effects, i.e. $Z_{it} = \alpha_i + \gamma_t + \varepsilon_{it}$, estimated with standard errors clustered by country. The high R^2 indicates that most of the variation in SSA-Exposure Bartik is absorbed by additive country and year components. The residualized series from this decomposition has mean -1.70×10^{-20}, s.d. 0.001, min -0.009, max 0.008.
Column (2) estimates IHS Arms on SSA-Exposure Bartik absorbing year fixed effects only (standard errors clustered by country) and includes $L1/L2$ lags of IHS Arms, a cubic in peace years, and country-specific linear trends as controls. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6 reinforces the interpretation of SSA as a residual market for global arms transfers. In column (1), aggregate SSA import growth is essentially uncorrelated with growth in imports to the rest of the world: the estimated elasticity is small and imprecise (0.197 with a standard error of 0.451), and the regression explains virtually none of the time-series variation ($R^2 \approx 0.002$). This is consistent with SSA absorbing idiosyncratic reallocations rather than tracking common global demand or supply conditions.

Columns (2)-(3) push the same idea to the exporter level (China, Russia, the U.S., and Europe). If SSA were simply a scaled-down mirror of global reallocations, exporter-by-exporter changes in deliveries to ROW should pass through into changes to SSA. Instead, the estimated pass-through in differences is negative and statistically indistinguishable from zero, even before imposing year fixed effects. In levels, once common global shocks are absorbed by year fixed effects, within-exporter co-movement remains weak.

Taken together, the appendix evidence supports a single conclusion: share–shift instruments either (i) load mainly on between-country exposure differences absorbed by country fixed effects, (ii) are mechanically degenerate under two-way fixed effects, or (iii) are weak in the fully saturated specification due to limited and irregular transmission of global shocks into SSA deliveries. For these reasons, the paper prioritizes a transparent fixed-effects design paired with falsification, influence, and randomization-based diagnostics.

Table A6: SSA as a residual market: weak co-movement with the rest of the world

Dep. Var.	(1) $\Delta \ln(\text{SSA}_t)$	(2) $\Delta\text{IHS}(\text{SSA}_{jt})$	(3) $\text{IHS}(\text{SSA}_{jt})$
$\Delta \ln(\text{ROW}_t)$	0.197		
	(0.451)		
$\Delta\text{IHS}(\text{ROW}_{jt})$		-0.170	
		(0.178)	
$\text{IHS}(\text{ROW}_{jt})$			0.596
			(0.444)
Exporter FE		Yes	Yes
Year FE		No	Yes
N	62	248	252
R^2 / Within-R^2	0.002	0.002	0.047

Notes. Column (1) uses annual aggregates and regresses SSA import growth on growth in the rest of the world (ROW = World − SSA), with heteroskedasticity-robust standard errors. Columns (2)–(3) use exporter-year data (exporters: China, Russia, the U.S., and Europe) and relate SSA deliveries to the same exporter's deliveries to ROW. Column (2) estimates pass-through in changes with exporter fixed effects (clustered by exporter). Column (3) estimates the relationship in levels with exporter and year fixed effects (clustered by exporter). Robust standard errors in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Appendix: Data diagnostics and variable distributions

Appendix Tables B1 - B4 summarize key features of the estimation environment and motivate the specification choices.

Table B1: Incidence of conflict onset in the risk set (three-year onset)

Onset_{it}	Freq.	Percent	Cumulative
0	2,351	96.79	96.79
1	78	3.21	100.00
Total	2,429	100.00	

Notes. The table reports the distribution of the three-year conflict-onset indicator in the estimation risk set.

Conflict onset is rare in the estimation risk set. Only 78 out of 2,429 country-years (3.21%) are coded as onset, while 96.79% are non-onset (Table B1). This low base rate motivates modeling duration dependence explicitly and interpreting inference with care in high-dimensional fixed-effects settings.

Table B2: Distribution of peace-years in the risk set

Variable	Mean	Median	p25	p75
Peace years	22.985	19	8	35

Notes. Peace-years measure time since the last conflict episode and are used to model duration dependence (via polynomial terms) in the onset regressions.

Peace-years exhibit long spells and substantial dispersion (Table B2). Within the risk set, the mean is 23.0 years and the median is 19, with an interquartile range from 8 to 35 years; values extend up to 63.

Table B3: Arms Imports: mass at zero and IHS transformation

Variable	Mean	Median	Std. Dev.	Obs.	% Zero
Arms Imports (levels)	20.718	0	74.590	2,898	54.83
Arms Imports (IHS)	1.457	0	1.928	2,898	

Notes. IHS denotes the inverse hyperbolic sine transformation, $\text{IHS}(x) = \ln\left(x + \sqrt{x^2+1}\right)$, which is defined at zero and approximately logarithmic for large x.

Arms deliveries are highly skewed with a large mass at zero (Table B3). In levels, more than half of country-years record zero deliveries. The inverse hyperbolic sine (IHS) transformation retains zeros while compressing the upper tail, reducing leverage from extreme episodes and yielding an exposure measure better suited to fixed-effects panel estimation.

Table B4: Coverage of lead and lag IHS imports

Variable	Missing	Non-missing	Unique values	Min	Max
$L1$(IHS Arms)	46	2,383	138	0	7.512
$L2$(IHS Arms)	90	2,339	137	0	7.512
$F1$(IHS Arms)	30	2,399	147	0	7.512
$F2$(IHS Arms)	62	2,367	146	0	7.512

Notes. Missingness arises mechanically at the start/end of country panels when constructing leads and lags. The range is reported for the non-missing observations.

Dynamic coverage is high, with missingness in leads/lags driven mechanically by panel endpoints (Table B4). Most observations have non-missing lags and leads, and the support of the transformed delivery variable remains similar across leads and lags, indicating that dynamic specifications alter sample size modestly without changing the underlying exposure distribution.

C Appendix: Additional Robustness Checks

C.1 Permutation-based randomization inference

To assess whether the estimated association could arise spuriously from year-specific common shocks or mechanical time-series structure, we implement Fisher-style randomization inference (RI). We permute arms import across countries within each year and re-estimate the baseline specification on each permuted dataset. This procedure preserves the cross-sectional distribution of deliveries in every year - and thus any global arms-market - while breaking the link between a country's import path and its own conflict risk.

The observed estimate is positive and precisely estimated: the coefficient on IHS arms deliveries equals 0.0128 with clustered standard error 0.0041 (t = 3.15; within-$R^2 = 0.107$; $N = 2{,}225$). The permutation distribution of placebo coefficients is tightly centered around zero (mean essentially 0; standard deviation ≈ 0.0023). Across $R = 9{,}999$ within-year permutations, no draw is as extreme as the observed coefficient ($k = 0$). Using the standard finite-sample adjustment, the two-sided RI p-value is

$$p_{RI} = \frac{k+1}{R+1} = 0.0001,$$

and the right-tail p-value is also 0.0001[3]. The histogram (Table C1) shows a symmetric placebo distribution concentrated near zero, with the observed estimate lying beyond its upper support.

[3] The randomization-inference comparison is conducted on the coefficient scale: we compare permuted coefficients to the observed coefficient (not permuted t-statistics to the observed t-statistic). Since the observed coefficient exceeds the maximum placebo draw, $k = 0$.

Table C1: Randomization inference: within-year permutation placebo test

Dep. Var.	(1) Onset
IHS Arms	0.0128***
	(0.0041)
	[RI p = 0.0001]
Country FE	Yes
Year FE	Yes
Country-specific trends	Yes
Observations	2,225
Within R^2	0.107
Permutations (R)	9,999

Notes. Fisher-style randomization inference permutes arms imports across countries within each year and re-estimates the baseline specification for each permuted dataset. The reported RI p-value is computed on the coefficient scale using $p_{RI} = (k+1)/(R+1)$, where k is the number of placebo coefficients at least as extreme as the observed estimate (two-sided). Standard errors are clustered at the country level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

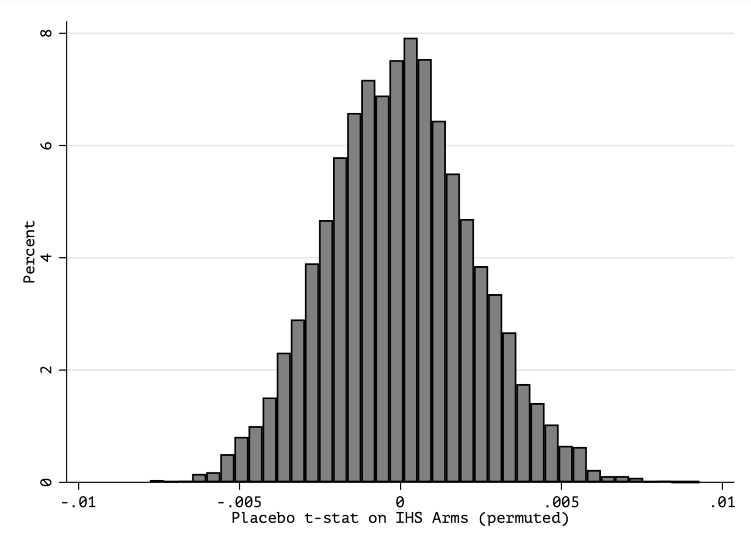

Figure C1: Placebo t-stat on IHS Arms (permuted)

C.2 Binned exposure specifications

To probe functional form and potential leverage of extreme observations, we replace the continuous IHS Arms measure with coarse exposure bins while keeping the remainder of the baseline design unchanged (same risk set, country and year fixed effects, cubic duration dependence in peace years, and standard errors clustered at the country level). In all specifications, the omitted category is zero imports, so the coefficients can be read as differences in onset probability (in percentage points) relative to zero-import years. Results are reported in Table C2; the corresponding coefficient plot for the quartile specification is shown in Figure C2.

Column (1) partitions positive deliveries into quartiles (plus the zero category). Relative to zero, the lowest positive quartile is indistinguishable from zero, whereas higher quartiles show progressively larger positive coefficients: +2.9 pp (Q2), +4.1 pp (Q3), and +5.1 pp (Q4), with Q3 and Q4 statistically significant. This pattern suggests that the association is not driven by the extensive margin alone, but increases meaningfully with delivery intensity.

Table C2: Binned arms deliveries and conflict onset (robustness to functional form).

Dep. var.	(1) Onset	(2) Onset	(3) Onset
Q1 (pos)	0.000		-0.008
	(0.009)		(0.008)
Q2	0.029*		0.011
	(0.016)		(0.017)
Q3	0.041**		0.042**
	(0.017)		(0.021)
Q4	0.051***		0.058***
	(0.018)		(0.017)
Q5 (top)			0.050**
			(0.020)
Positive (<p90)		0.023***	
		(0.009)	
High (≥p90)		0.065*	
		(0.034)	
Constant	-0.033	-0.030	-0.034
	(0.022)	(0.021)	(0.023)
Peace years (cubic)	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	2,429	2,429	2,429
Within R^2	0.013	0.010	0.016

Notes: Standard errors clustered at the country level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
The omitted category is always zero imports.

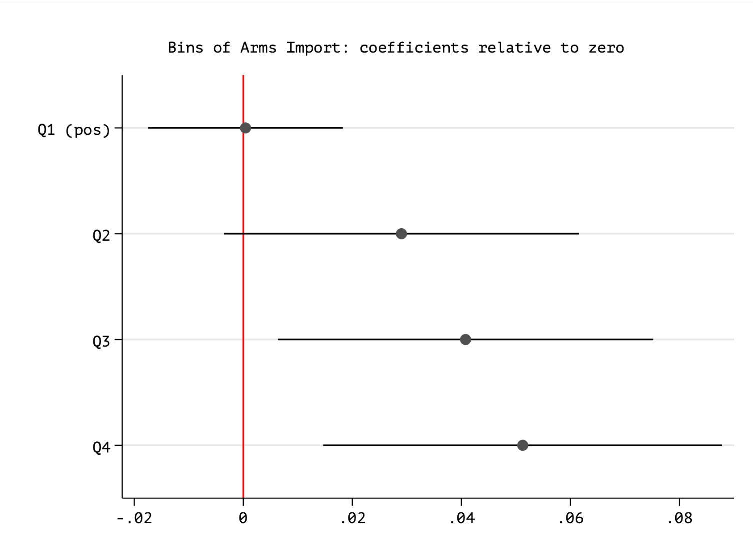

Figure C2: Bins of Arms Import

Column (2) implements a tail split at the 90th percentile among positive deliveries (Zero / Positive below p90 / High at or above p90). Any positive deliveries below p90 are associated with a statistically significant increase of +2.3 pp. The high-tail coefficient is larger (+6.5 pp) but imprecisely estimated, consistent with limited support in the extreme tail. The difference between high-tail and non-high positives is positive but not statistically distinguishable from zero (p = 0.214).

Column (3) refines the binning into quintiles among positives (plus zero). The lower part of the positive distribution remains small and statistically indistinguishable from zero, while coefficients become economically and statistically meaningful in the upper range: +4.2 pp (Q3), +5.8 pp (Q4), and +5.0 pp (top quintile). Overall, these binned specifications point to a threshold-like pattern: large delivery episodes — rather than routine small deliveries — are associated with conflict onset, while the estimated duration dependence remains stable and statistically meaningful across designs.

Working Papers
Dipartimento di Politica Economica

1. *Innovation, jobs, skills and tasks: a multifaceted relationship.* M. Piva, M. Vivarelli. Vita e Pensiero, maggio 2018 (ISBN 978-88-343-3654-0)
2. *A bridge over troubled water: Interdisciplinarity, Novelty, and Impact.* M. Fontana, M. Iori, F. Montobbio, R. Sinatra. Vita e Pensiero, settembre 2018 (ISBN 978-88-343-3793-6)
3. *Concordance and complementarity in IP instruments*. M. Grazzi, C. Piccardo, C. Vergari. Vita e Pensiero, gennaio 2019 (ISBN 978-88-343-3879-7)
4. *Sustainable finance, the good, the bad and the ugly: a critical assessment of the EU institutional framework for the green transition*. L. Esposito, E.G. Gatti, G. Mastromatteo. Vita e Pensiero, febbraio 2019 (ISBN 978-88-343-3892-6)
5. *Technology and employment in a vertically connected economy: a model and an empirical test.* G. Dosi, M. Piva, M.E. Virgillito, M. Vivarelli. Vita e Pensiero, giugno 2019 (ISBN digital edition [PDF]: 978-88-343-4008-0)
6. *Testing the employment impact of automation, robots and AI: A survey and some methodological issues.* L. Barbieri, C. Mussida, M. Piva, M. Vivarelli. Vita e Pensiero, settembre 2019 (ISBN digital edition [PDF]: 978-88-343-4052-3)
7. *A new proposal for the construction of a multi-period/multilateral price index*. C.R. Nava, A. Pesce, M.G. Zoia. Vita e Pensiero, ottobre 2019 (ISBN digital edition [PDF]: 978-88-343-4114-8)
8. *Lo Stato Sociale: da "lusso" a necessità*. L. Campiglio. Vita e Pensiero, febbraio 2020 (ISBN digital edition [PDF]: 978-88-343-4184-1)
9. *Robots and the origin of their labour-saving impact*. F. Montobbio, J. Staccioli, M.E. Virgillito, M. Vivarelli. Vita e Pensiero, marzo 2020 (ISBN digital edition [PDF]: 978-88-343-4196-4)
10. *Business visits, technology transfer and productivity growth*. M. Piva, M. Tani, M. Vivarelli. Vita e Pensiero, marzo 2020 (ISBN digital edition [PDF]: 978-88-343-4210-7)
11. *Technology, industrial dynamics and productivity: a critical survey*. M. Ugur, M. Vivarelli. Vita e Pensiero, settembre 2020 (ISBN digital edition [PDF]: 978-88-343-4406-4)
12. *Back to the past: the historical roots of labour-saving automation*. J. Staccioli, M.E. Virgillito. Vita e Pensiero, novembre 2020 (ISBN digital edition [PDF]: 978-88-343-4473-6)
13. *The present, past, and future of labor-saving technologies.* J. Staccioli, M.E. Virgillito. Vita e Pensiero, dicembre 2020 (ISBN digital edition [PDF]: 978-88-343-4479-8)
14. *Why Do Populists Neglect Climate Change? A Behavioural Approach*. L.A. Lorenzetti. Vita e Pensiero, dicembre 2020 (ISBN digital edition [PDF]: 978-88-343-4483-5)
15. *Relative wages, payroll structure and performance in soccer. Evidence from Italian Serie A (2007-2019)*. C. Bellavite Pellegrini, R. Caruso, M. Di Domizio. Vita e Pensiero, gennaio 2021 (ISBN digital edition [PDF]: 978-88-343-4490-3)
16. *Robots, AI, and Related Technologies: A Mapping of the New Knowledge Base*. E. Santarelli, J. Staccioli, M. Vivarelli. Vita e Pensiero, gennaio 2021 (ISBN digital edition [PDF]: 978-88-343-4499-6)
17. *Detecting the labour-friendly nature of AI product innovation*. G. Damioli, V. Van Roy, D. Vertesy, M. Vivarelli. Vita e Pensiero, aprile 2021 (ISBN digital edition [PDF]: 978-88-343-4600-6)
18. *Circular Economy Approach: The benefits of a new business model for European Firms*. C. Bellavite Pellegrini, L. Pellegrini, C. Cannas. Vita e Pensiero, luglio 2021 (ISBN digital edition [PDF]: 978-88-343-4817-8)
19. *The impact of cognitive skills on investment decisions. An empirical assessment and policy suggestions.* L. Esposito, L. Marrese. Vita e Pensiero, luglio 2021 (ISBN digital edition [PDF]: 978-88-343-4822-2)
20. *"Thinking of the end of the world and of the end of the month": the Impact of Regenerative Agriculture on Economic and Environmental Profitability*. L.A. Lorenzetti, A. Fiorini. Vita e Pensiero, ottobre 2021 (ISBN digital edition [PDF]: 978-88-343-4898-7)

21. *Labour-saving automation and occupational exposure: a text-similarity measure*. F. Montobbio, J. Staccioli, M.E. Virgillito, M. Vivarelli. Vita e Pensiero, novembre 2021 (ISBN digital edition [PDF]: 978-88-343-5089-8)
22. *Climate reputation risk and abnormal returns in the stock markets: a focus on large emitters*. G. Guastella, M. Mazzarano, S. Pareglio, A. Xepapadeas. Vita e Pensiero, novembre 2021 (ISBN digital edition [PDF]: 978-88-343-5092-8)
23. *Carbon Boards and Transition Risk: Explicit and Implicit exposure implications for Total Stock Returns and Dividend Payouts*. M. Mazzarano, G. Guastella, S. Pareglio, A. Xepapadeas. Vita e Pensiero, novembre 2021 (ISBN digital edition [PDF]: 978-88-343-5093-5)
24. *Innovation and employment: a short update*. M. Vivarelli. Vita e Pensiero, gennaio 2022 (ISBN digital edition [PDF]: 978-88-343-5113-0)
25. *AI technologies and employment. Micro evidence from the supply side*. G. Damioli, V. Van Roy, D. Vertesy, M. Vivarelli. Vita e Pensiero, gennaio 2022 (ISBN digital edition [PDF]: 978-88-343-5119-2)
26. *The Effect of External Innovation on Firm Employment*. G. Arenas Díaz, A. Barge-Gil, J. Heijs, A. Marzucchi. Vita e Pensiero, febbraio 2022 (ISBN digital edition [PDF]: 978-88-343-5146-8)
27. *The North-South divide: sources of divergence, policies for convergence*. L. Fanti, M.C. Pereira, M.E. Virgillito. Vita e Pensiero, maggio 2022 (ISBN digital edition [PDF]: 978-88-343-3524-4)
28. *The empirics of technology, employment and occupations: lessons learned and challenges ahead*. F. Montobbio, J. Staccioli, M.E. Virgillito, M. Vivarelli. Vita e Pensiero, novembre 2022 (ISBN digital edition [PDF]: 978-88-343-5383-7)
29. *Cognitive biases and historical turns. An empirical assessment of the intersections between minds and events in the investors' decisions*. L. Esposito, L. Malara. Vita e Pensiero, gennaio 2023 (ISBN digital edition [PDF]: 978-88-343-5420-9)
30. *Interaction between Ownership Structure and Systemic Risk in the European financial sector*. C. Bellavite Pellegrini, R. Camacci, L. Pellegrini, A. Roncella. Vita e Pensiero, febbraio 2023 (ISBN digital edition [PDF]: 978-88-343-5446-9)
31. *Was Robert Gibrat right? A test based on the graphical model methodology*. M. Guerzoni, L. Riso, M. Vivarelli. Vita e Pensiero, marzo 2023 (ISBN digital edition [PDF]: 978-88-343-5457-5)
32. *A North-South Agent Based Model of Segmented Labour Markets. The Role of Education and Trade Asymmetries*. L. Fanti, M.C. Pereira, M.E. Virgillito. Vita e Pensiero, maggio 2023 (ISBN digital edition [PDF]: 978-88-343-5529-9)
33. *Innovation and the Labor Market: Theory, Evidence and Challenges*. N. Corrocher, D. Moschella, J. Staccioli, M. Vivarelli. Vita e Pensiero, giugno 2023 (ISBN digital edition [PDF]: 978-88-343-5580-0)
34. *The Effect of Economic Sanctions on World Trade of Mineral Commodities. A Gravity Model Approach from 2009 to 2020*. R. Caruso, M. Cipollina. Vita e Pensiero, dicembre 2023 (ISBN digital edition [PDF]: 978-88-343-5686-9)
35. *Education and Military Expenditures: Countervailing Forces in Designing Economic Policy. A Contribution to the Empirics of Peace*. A. Balestra, R. Caruso. Vita e Pensiero, gennaio 2024 (ISBN digital edition [PDF]: 978-88-343-5757-6)
36. *Vulnerability to Climate Change and Communal Conflicts: Evidence from Sub-Saharan Africa and South/South-East Asia*. S. Balestri, R. Caruso. Vita e Pensiero, maggio 2024 (ISBN digital edition [PDF]: 978-88-343-5829-0)
37. *Assessing changes in EU innovation policy programs: from SME instrument to EIC accelerator for start-up funding*. M. del Sorbo, C. Faber, M. Grazzi, F. Matteucci, M. Ruß. Vita e Pensiero, luglio 2024 (ISBN digital edition [PDF]: 978-88-343-5860-3)
38. *AI as a new emerging technological paradigm: evidence from global patenting.* G. Damioli, V. Van Roy, D. Vertesy, M. Vivarelli. Vita e Pensiero, settembre 2024 (ISBN digital edition [PDF]: 978-88-343-5873-3)
39. *The KSTE+I approach and the AI technologies*. F. D'Alessandro, E. Santarelli, M. Vivarelli. Vita e Pensiero, settembre 2024 (ISBN digital edition [PDF]: 978-88-343-5880-1)
40. *Quo Vadis Terra? The future of globalization between trade and war*. L. Esposito, E.G. Gatti, G. Mastromatteo. Vita e Pensiero, settembre 2024 (ISBN digital edition [PDF]: 978-88-343-5895-5)
41. *The Agents of Industrial Policy and the North-South Convergence: State-Owned Enterprises in an International-Trade Macroeconomic ABM.* L. Fanti, M.C. Pereira, M.E. Virgillito. Vita e Pensiero, ottobre 2024 (ISBN digital edition [PDF]: 978-88-343-5909-9)

42. *The impact of US elections on US defense industry: Firm-level evidence from 1996 to 2022*. A. Balestra, R. Caruso. Vita e Pensiero, January 2025 (ISBN digital edition [PDF]: 978-88-343-5937-2)

43. *Forecasting the Impact of Extreme Weather Events on Electricity Prices in Italy: A GARCH-MIDAS Approach with Enhanced Variable Selection*. M. Guerzoni, L. Riso, M.G. Zoia. Vita e Pensiero, January 2025 (ISBN digital edition [PDF]: 978-88-343-5938-9)

44. *The Theoretical Properties of Novel Risk-Based Asset Allocation Strategies using Portfolio Volatility and Kurtosis*. M.D. Braga, L. Riso, M.G. Zoia. Vita e Pensiero, January 2025 (ISBN digital edition [PDF]: 978-88-343-5939-6)

45. *Sustainable Finance in the New Geo-Political Era: A Difficult Balancing Act*. L. Esposito, M. Cocco. Vita e Pensiero, February 2025 (ISBN digital edition [PDF]: 978-88-343-5940-2)

46. *New technologies and employment: the state of the art*. M. Vivarelli, G. Arenas Díaz. Vita e Pensiero, March 2025 (ISBN digital edition [PDF]: 978-88-343-5941-9)

47. *Leveraging Knowledge Networks: Rethinking Technological Value Distribution in mRNA Vaccine Innovations*. R. Mastrandrea, F. Montobbio, G. Pellegrino, M. Riccaboni, V. Sterzi. Vita e Pensiero, March 2025 (ISBN digital edition [PDF]: 978-88-343-5991-4)

48. *A Twin Transition or a policy flagship? Emergent constellations and dominant blocks in green and digital technologies*. L. Nelli, M.E. Virgillito, M. Vivarelli. Vita e Pensiero, April 2025 (ISBN digital edition [PDF]: 978-88-343-5992-1)

49. *The role of business visits in fostering R&D investment*. M. Vivarelli, M. Piva, M. Tani. Vita e Pensiero, June 2025 (ISBN digital edition [PDF]: 978-88-343-5993-8)

50. *A Deep Learning procedure for the identification of Artificial Intelligence technologies in patent data*. F. D'Alessandro. Vita e Pensiero, June 2025 (ISBN digital edition [PDF]: 978-88-343-5994-5)

51. *ESGs Scoring and Its Divergencies: An empirical Investigation in the Food and Beverage Industry*. C. Bellavite Pellegrini, R. Camacci, P. Cincinelli. Vita e Pensiero, September 2025 (ISBN digital edition [PDF]: 978-88-343-5995-2)

52. *Artificial intelligence as a method of invention*. G. Arenas Díaz, M. Piva, M. Vivarelli. Vita e Pensiero, November 2025 (ISBN digital edition [PDF]: 978-88-343-5996-9)

53. *Does corruption trigger political violence? Evidence from Sub-Saharan Africa (1970-2020)*. R. Caruso, E. Galli, G. Tringali. Vita e Pensiero, December 2025 (ISBN digital edition [PDF]: 978-88-343-6190-0)

54. *The determinants of defense burden sharing in the European Union from 1980 to 2024*. A. Balestra, R. Caruso, S. Mombelli. Vita e Pensiero, December 2025 (ISBN digital edition [PDF]: 978-88-343-6193-1)

55. *Arms Import and Civil Conflict Onset: Risk-Set Evidence from Sub-Saharan Africa, 1960–2022*. A. Balestra, R. Caruso. Vita e Pensiero, January 2026 (ISBN digital edition [PDF]: 978-88-343-6198-6)