Interrupted Access in International Firearm Markets[1]

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Abstract

How do export controls change the continuity of trade? I study the tightening and subsequent rollback of U.S. firearm export licensing using public monthly trade and quarterly licensing classifications. A saturated source-relative design estimates a 52.2-percentage-point decline in recorded U.S. trade activity and increases in three- and six-month reporting gaps. License-stream comparisons corroborate the contraction. After rollback, trade reappears along many previously active country–product links, while activity remains below its earlier frequency. Recovery contrasts are less precise than the interruption estimates. The findings distinguish restrictions on a supplier's participation from the restoration of regular trade.

Keywords: firearm markets; export licensing; trade continuity; supply networks; recovery.

JEL classifications: F14, F13, L14, K42.

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1 Introduction

Export controls can change who supplies a market even when trade with that market continues. Research on arms embargoes and trade restrictions establishes that policy can reduce cross-border flows, but annual trade totals reveal less about whether a supplier remains regularly available. A destination that receives one shipment after a restriction is relaxed has resumed trade; it need not have regained the frequency of access it previously enjoyed. This paper studies that distinction in international firearm markets, asking how targeted export licensing changes the continuity of publicly recorded trade and how that trade develops after the additional restrictions are withdrawn.

The U.S. firearm licensing cycle provides variation across suppliers, destinations, products, and time. A licensing pause beginning in October 2023 was followed by tighter rules in 2024 and a rollback effective September 30, 2025. I combine monthly UN Comtrade records with public Census data identifying commodities, export-control classifications, and licensing categories. The principal panel contains 107 destinations and a fixed set of five supplying countries over 2021–2025. The empirical design compares the United States with other suppliers within the same destination–product–month, while also absorbing supplier–product–month, supplier–destination–month, and supplier–destination–product effects. It isolates the differential U.S. response for firearm products in destinations designated high risk, under the assumption that no other contemporaneous shock produces that same interaction.

The first finding is a large interruption in recorded U.S. participation. During the tightening phase, the source-relative probability of positive monthly trade falls by 52.2 percentage points, with a 95 percent confidence interval of [−76.3, −28.1]. The event study shows no systematic differential movement before the pause and a sustained decline after the tightening. This is a multiple-difference estimate, not a 52.2 percent reduction in world firearm supply. The underlying descriptive change is also substantial: U.S. firearm activity in sampled high-risk destinations falls from 46.7 percent of monthly cells before the pause to 6.7 percent during tightening.

The second finding is that the interruption extends beyond the timing of individual monthly records. Three- and six-consecutive-month gaps increase by 56.3 and 55.2 percentage points in the same source-relative design. Results remain negative for trade activity when the sample is restricted to links that were active before the policy, and earlier quarterly aggregation checks also show contraction. Separately, Census licensing classifications locate a decline within the Commerce-licensed stream and show a contraction when licensing categories are aggregated. Together, these results connect the broad trade response to the regulated channel and make simple within-quarter batching or relabeling incomplete explanations.

The third finding concerns the distinction between reappearance and regularity during recovery. Among 39 high-risk firearm country–product links selected using only pre-policy U.S. activity, 89.7 percent record trade at least once in the nine-month window beginning October 2022, compared with 38.5 percent in the corresponding October 2024 window and 71.8 percent in the October 2025 window. The fraction of months with positive trade is 53.6, 8.3, and 27.9 percent, respectively. These fixed-cohort summaries reveal a recovery that a binary annual participation measure would describe differently from a monthly activity measure. The regression evidence is compatible with partial restoration, but does not precisely establish that the two margins recover at different rates or that a remaining gap is permanent.

These findings add a continuity margin to the evaluation of arms-market regulation. Baronchelli, Caruso, and Ricciuti (2022) study small-arms trade and the effectiveness of embargoes; Dube, Dube, and García-Ponce (2013) demonstrate that U.S. gun regulation can have consequences across the Mexican border. I study the upstream commercial channel through which a licensing change alters a supplier's repeated presence in foreign markets. Measuring the length of reporting gaps distinguishes a prolonged interruption from a small change in the frequency of otherwise continuing trade. The analysis does not use violence as a proxy for firearm deliveries or infer a welfare effect from an export contraction.

The recovery evidence also speaks to the persistence of trade disruptions without requiring a claim of permanent relationship destruction. Chakrabarti, Mahajan, and Tomar (2025) document persistent trade effects and reshoring after temporary Indian state-border closures. Kwon et al. (2026) study sanctions across their imposition and lifting and emphasize heterogeneous recovery. The contribution here is the combination of a targeted licensing cycle, monthly continuity outcomes, and a fixed pre-policy risk set. The same set of links can be followed through interruption and reopening, so the appearance of returning trade is not generated by selecting only links that survive or reenter.

Finally, the source dimension separates the disappearance of one supplier from a market's observed alternatives. The distinction is related to the importance of trading connections emphasized by Bernard and Moxnes (2018), but the links observed here connect countries and products, not individual firms. Foreign-only trade becomes more common in the earlier source-accounting exercise, while tests of additional foreign activity and entry are much less precise. This combination matters for interpretation: the continued presence of existing alternatives can change observed sourcing composition without a rapid expansion of replacement supply. The paper therefore evaluates supplier participation and continuity separately from total market availability.

Section 2 describes the licensing cycle. Section 3 introduces the data and measures. Section 4 presents the empirical design. Sections 5 and 6 study interruption and recovery. Section 7 examines licensing classifications and the scope of the source evidence. Section 8 concludes.

2 The licensing cycle

The institutional setting changes the conditions for exporting particular firearms rather than imposing a general ban on trade with the affected destinations. The Bureau of Industry and Security regulates Commerce-controlled firearms and related items through the Export Administration Regulations. Its October 27, 2023 announcement paused the issuance of new export licenses for specified firearm, ammunition, and related categories, subject to stated destination and end-user exceptions (BIS 2023). The distinction between new authorizations and all shipments is important: a pause in licensing need not stop recorded exports immediately.

The 2024 rule introduced a more restrictive review regime with an explicit high-risk destination dimension. Published on April 30 and effective May 30, the rule changed licensing requirements and review policies and introduced new Export Control Classification Numbers, or ECCNs (BIS 2024a). Its implementation rules make an instantaneous treatment indicator a poor description of the transition. The phase analysis therefore separates the initial pause from the subsequent tightening and excludes months straddling implementation. The calendar-quarter event studies retain transition quarters whenever the underlying panel supports them.

The September 2025 rollback supplies an opportunity to observe the aftermath of the additional restrictions. The final rule took effect on September 30, reversing the main 2024 restrictions while retaining the new ECCNs and specified reporting provisions (BIS 2025). I call the period beginning October 2025 the rollback phase. Ordinary export controls continued to apply, and the public files do not identify the restoration dates of individual authorizations. Rollback is therefore a change in the regulatory regime, not an observation that every exporter immediately regained permission to execute every previous transaction.

The policy's classification structure determines what the empirical comparisons can establish. Destination risk status comes from the published list, not from subsequent trade performance. The broad product comparison uses revolvers and pistols, HS930200, and other sporting, hunting, or target-shooting rifles, HS930330, against HS930400, which includes other arms such as air or gas guns. These statistical categories are not identical to legal treatment categories. I consequently use the broad comparison for a reduced-form trade response and examine ECCN and licensing categories separately in Section 7.

3 Data and measures of continuity

The analysis combines public trade records with public licensing classifications to observe both broad sourcing patterns and the channel most closely connected to the policy. The principal monthly data come from UN Comtrade exporter reports. The Census ECCN files provide a separate quarterly breakdown by destination, ten-digit commodity, control classification, and licensing category (United Nations Statistics Division n.d.; U.S. Census Bureau n.d.a). All estimates use archived public extracts. No confidential applications, firm identities, or pending information requests enter the analysis.

The main source panel holds suppliers fixed rather than allowing reporting coverage to change with the policy. It includes the United States, Germany, Czechia, Italy, and Türkiye, the three product groups, and 107 destinations, of which 22 appear on the high-risk list. The 2021–2025 grid contains 95,580 source–destination–product–month observations after excluding domestic source–destination cells. Removing four transition months leaves 89,208 observations for the monthly activity phase regression. Foreign reporter totals are reconciled with the sum of their bilateral partner records before completing the grid. An absent bilateral row in a supported reporting block is recorded as no publicly reported trade; an unsupported reporter month is not assigned a zero.

Positive trade and prolonged gaps capture different aspects of supply continuity. Monthly activity equals one when the source reports a positive export value in the destination–product cell. A three-month gap requires no positive record in the current month or the preceding two months; a six-month gap applies the same definition over six consecutive months. Every month in a gap window must be observed. These are indicators of public reporting continuity, not direct observations of inventories, orders, or physical stockouts. Descriptive baseline rates and the sample dimensions appear in Table 1.

[Table 1 is about here]

Predetermined cohorts allow recovery to be measured without selecting on the disruption itself. The main regression uses the full supported product grid. A supporting cohort retains the 281 destination–product links with at least one positive U.S. month during 2021–2022; a more regular cohort requires at least six positive months during those 24 months. Every available supplying country is retained within the selected markets. In the U.S.-only recovery exercise, equal-length windows beginning in October are compared across years on this fixed risk set. Among its high-risk firearm links, the cohort contains 39 country–product combinations.

Longer recovery coverage comes from narrower but still fixed source sets. Germany, Czechia, and Italy provide verified common support through May 2026; Germany and Czechia provide support through June 2026. I report both extensions, never switching the available sources from one month to the next within a regression. The Italian extension has only April and May in 2026Q2. These samples extend the horizon of the same policy episode rather than supplying independent policy shocks. Table A1 records the resulting sample and interpretation differences.

4 Empirical design

The primary estimand is the differential change in publicly recorded U.S. firearm activity in high-risk destinations relative to the other sources and products. The source panel permits unusually demanding controls for destination demand: importer–product–month fixed effects absorb changes common to suppliers of exactly the same product in exactly the same market and month. Supplier–product–month effects absorb source-specific product shocks, and supplier–destination–month effects absorb contemporaneous bilateral changes common across the included products. Persistent differences among source–destination–product cells receive their own fixed effects.

$$Y_{eipt} = \alpha_{eip} + \lambda_{ipt} + \mu_{ept} + \nu_{eit} + \sum_{s \in S} \beta_s U_e R_i C_p D_{st} + \varepsilon_{eipt} \quad (1)$$

In equation (1), e indexes supplying countries, i destinations, p products, and t months. Y is activity or a reporting-gap indicator; U equals one for the United States, R identifies high-risk destinations, and C identifies the two firearm groups. D identifies each policy phase s in the set S comprising pause, tightening, and rollback. The four fixed-effect sets correspond to the dimensions just described. The coefficient β is a saturated multiple-difference contrast in fraction units; tables multiply it by 100 for percentage points. It is not an unconditional average treatment effect on every shipment or a change in global supply.

Policy phases separate implementation from the periods over which a sustained response can be measured. The pre-period ends in September 2023; the pause covers November 2023–April 2024; tightening covers July 2024–August 2025; rollback begins in October 2025. October 2023, May–June 2024, and September 2025 are excluded from phase regressions. For quarterly event studies, phase interactions are replaced by quarter interactions and 2023Q3 is the omitted reference. All supported calendar quarters remain in the main event graphs. A coefficient relative to that one quarter need not equal a phase coefficient estimated relative to the whole pre-period.

Identification requires the absence of another shock specific to U.S. exports of these firearm products to the high-risk destinations at the same time. The fixed effects absorb broad import

demand, supplier product shocks, and bilateral shocks, but cannot absorb an omitted shock with the full treatment interaction. The destination list is policy-selected, so unconditional similarity between risk groups is not assumed. Pre-policy dynamics, placebo timing, alternative source support, and legal-stream comparisons assess different implications of the design. Policy-induced changes among comparison suppliers would make the coefficient a relative equilibrium response rather than the isolated direct effect on U.S. exports.

Inference allows dependence within destinations and retains the uncertainty created by the saturated design. The principal models use destination-clustered CRV1 standard errors with the full absorbed-fixed-effect correction and t reference distributions; the main sample has 107 clusters. A separate sensitivity clusters by destination and month. The three tightening outcomes form one Holm-adjusted family, while the network outcomes form another. The event graphs display pointwise 95 percent intervals; Figure A5 adds simultaneous bands for the main dynamic paths. Destination clustering does not turn this common policy cycle into 107 independent regulatory experiments.

Recovery requires comparisons distinct from those used to establish interruption. I report both the rollback coefficient relative to the pre-period and the rollback-minus-tightening contrast using the full coefficient covariance matrix. For reporting gaps, a pure-phase specification requires every month in the lookback window to belong to the same phase. The first fully post-rollback six-month window ends in March 2026. Fixed-window reappearance and activity-frequency comparisons then ask whether the same pre-policy cohort becomes active again, without conditioning on a realized post-policy exit. Appendix A reports their joint margin-difference tests and the sensitivity of late-period gaps to deviations from parallel trends.

5 Licensing and trade interruption

The tightening is associated with a large and prolonged loss of U.S. participation in the affected trade cells. Monthly presence falls by 52.2 percentage points in the source-relative specification, while three- and six-month gaps rise by 56.3 and 55.2 points (Table 2). All three tightening outcomes survive their joint Holm correction. For economic scale, the raw U.S. firearm presence rate in high-risk destinations falls by 40.0 points, from 46.7 to 6.7 percent. The difference between this raw change and the regression coefficient reflects the additional product, source, and destination comparisons; the regression coefficient should not be divided by the raw baseline to produce a shipment-loss percentage.

[Table 2 is about here]

The timing of the activity decline distinguishes the pause from the subsequent tightening. The pause coefficient is −18.0 points, with an interval that includes zero, whereas the later tightening estimate is more negative and more precise. Figure 1 shows the full quarterly path for activity and both gap measures. Pre-period coefficients fluctuate around the reference, with joint pre-period p-values of 0.688, 0.919, and 0.859. These diagnostics do not prove the identifying assumption, but they provide no evidence of a differential decline of comparable persistence already under way before the policy.

[Figure 1 is about here]

The activity decline is not confined to links that rarely traded before the policy. Restricting the source panel to markets with pre-policy U.S. activity gives a tightening coefficient of −57.7 percentage points; requiring at least six active pre-policy months gives −70.4 points. Three-month-gap increases also remain positive in both cohorts. The six-month-gap estimate for the more regular cohort is less precise, with an interval crossing zero, and is retained in Table A2. A 2022 placebo on the full grid gives no comparable activity decline. Cohort-building years are not treated as independent validation of a cohort selected from those same years.

Aggregating within quarters also leaves a contraction in recorded participation. The expanded source-pool quarterly exercise estimates a −59.5-point change in the probability of any quarterly trade, compared with −52.5 points for its monthly specification. Figure A1 reports this frequency check and its complete-quarter sensitivity. Its supplier support and phase convention differ from the main panel, as documented in Table A1. The comparison addresses a concrete explanation for monthly gaps: moving a record between months within a quarter cannot by itself make quarterly presence disappear.

6 Trade after the rollback

The extended data show returning trade alongside a remaining activity gap, with less precise evidence about the amount of recovery. In the three-foreign-source extension, the tightening coefficient is −52.6 percentage points and the rollback coefficient is −34.7 points. The two-foreign-source extension gives −52.3 and −31.5 points. Their rollback-minus-tightening contrasts are positive, at 17.9 and 20.8 points, but both confidence intervals include zero (Table 3). Figure 2 shows the entire event path for these extensions, including the earlier leads and the 2023Q3 reference.

[Table 3 is about here]

[Figure 2 is about here]

The remaining gap depends on the comparison period, which is why the phase and event results are presented together. In the two-foreign-source extension, the phase rollback coefficient is negative with an interval excluding zero. The equal-weight mean of the 2025Q4–2026Q2 event coefficients, measured against 2023Q3, instead has an interval that includes zero even before relaxing parallel trends. Table A7 makes the latter target and its HonestDiD sensitivity explicit. The evidence is stronger for interruption during tightening than for a lasting post-rollback departure from the pre-policy counterfactual.

Pure lookback windows reveal improvement in prolonged gaps that early rollback observations cannot measure. A six-month gap ending in late 2025 still contains months from the restrictive regime. When the full window is required to lie in one policy phase, the rollback-minus-tightening contrast is −49.2 percentage points in the three-source extension and −53.6 points in the two-source extension. The corresponding rollback levels are positive but imprecise. These comparisons use different calendar subsets from the ordinary rolling-gap regression. Reporting them prevents mechanically overlapping windows from being interpreted as evidence that reopening has no effect.

The fixed-cohort descriptions make the economic distinction between reappearance and frequency concrete. Figure 3 follows the same 39 pre-policy-active high-risk firearm links in October-starting nine-month windows. Between the restrictive and rollback windows, the fraction recording any trade rises from 38.5 to 71.8 percent, while the fraction of active months rises from 8.3 to 27.9 percent. Both measures improve; neither reaches its October 2022 benchmark. The comparison is descriptive and does not condition on whether a particular link disappeared during tightening.

[Figure 3 is about here]

Formal recovery tests support a narrower conclusion than the descriptive contrast alone might suggest. On the full fixed U.S.-active cohort, the nine-month reappearance contrast is 49.8 percentage points, with an unadjusted interval excluding zero, but its Holm-adjusted p-value is 0.230 across the six recovery tests. The corresponding activity-frequency contrast is 16.1 points and is less precise. Directly testing the difference between those recovery contrasts gives 33.7 points with a 95 percent interval of [−6.7, 74.0]. Table A3 reports all three window lengths and both margins. Thus, the data make the distinction worth measuring without establishing a statistically precise ranking of recovery speeds.

7 Licensing classifications and alternative supply

Public licensing classifications corroborate the trade interruption and help separate it from compositional changes in the reporting categories. The Census files identify ten-digit commodities,

ECCNs, and license types, allowing Commerce-licensed flows in the mapped firearm categories to be compared with other authorization streams. The initial exercise holds a pre-period destination–commodity union fixed and compares the Commerce C30 stream with State Department S05, S61, and S73 streams. This is a separate quarterly design, with 524 destination–commodity links and 116 destinations, rather than an exact-treatment relabeling of the main monthly sample.

$$Y_{ipgq} = a_{ipg} + b_{ipq} + c_{pgq} + \sum_{s \in S} \theta_s B_g R_i D_{sq} + \eta_{ipgq} \quad (2)$$

In equation (2), g indexes licensing streams and q quarters; p now indexes ten-digit commodities, and i continues to index destinations. Y indicates a positive reported filing frequency, B identifies the mapped Commerce stream, and R identifies destination risk. The fixed effects absorb destination–commodity–stream, destination–commodity–quarter, and commodity–stream–quarter variation. The phase coefficient θ measures the differential stream response in high-risk destinations. The public data identify licensing categories rather than individual licenses or end users, so this specification locates contraction in a regulated reporting channel without estimating an approval probability.

Recorded participation in the mapped Commerce stream falls by 42.8 percentage points during tightening. Figure 4 traces that response through 2025Q4, retaining 2024Q2 as a transition quarter; Table 4 reports its phase coefficients together with the principal alternative definitions. The State-licensed comparison stream is sparse, and its strict common-support sample is too small to carry the argument by itself. Replacing it with other Commerce C30 trade gives a −39.1-point presence estimate. In the transition quarter, the corresponding event coefficients are −12.2 and −8.3 points, with 95% intervals of [−45.3, 20.9] and [−37.3, 20.8]; the clearer contraction follows in the full tightening quarters. Aggregating all licensing categories within the target commodities gives −43.7 points in a less saturated model with destination–commodity and commodity–quarter fixed effects. This last diagnostic shows that the contraction is not confined to one licensing label, but does not separately absorb destination–quarter demand shocks.

[Figure 4 is about here]

[Table 4 is about here]

Alternative suppliers alter the interpretation of a U.S. interruption without automatically demonstrating replacement supply. Figure A2 decomposes trade into four states: U.S.-only trade, coexistence of U.S. and foreign trade, foreign-only trade, and neither source. The four coefficients

add to zero on the same sample. Foreign-only activity increases, while the loss of all recorded activity within the pool is less precisely estimated. A separate network exercise on the four-foreign-source sample associates one additional pre-policy foreign source with a 20.7-point larger foreign-only response, but the direct foreign-activity and entry interactions are imprecise (Table A4). The data therefore distinguish the continued presence of alternatives from an identified expansion in their supply.

The ammunition and parts extension provides a scope check rather than an independent policy replication. Harmonizing destinations, sources, and fixed effects shows that the ammunition estimate depends on the comparison product: firearm parts are themselves partly regulated and cannot serve as a clean untreated benchmark. With complete public files and predetermined ECCN exposure, a ten-percentage-point exposure difference corresponds to a 2.68-point relative U.S. presence decline in the combined ammunition-and-parts sample. Binary comparisons of ammunition and parts against the same other-arms group give similarly negative coefficients, with a direct between-product contrast of −1.4 points and an interval spanning zero. Table A5 and Figure A3 show the contraction under these alternative definitions. The extension supports a broader participation response without establishing equally strong duration effects across products.

The fixed foreign pool is not a measure of all-world availability. The importer-side ammunition audit finds that the pooled suppliers account for only 25.0 percent of pre-policy value and 37.2 percent during tightening in its high-risk overlap. Some months with no pooled-source imports have positive imports from other countries; the all-partner presence regression is imprecise (Table A6). These facts rule out interpreting the source-panel interruption as an estimated global shortage. They also explain why the paper's contribution concerns the continuity of a regulated supplier's market participation, rather than a demonstrated effect on violence, inventories, or the total quantity of firearms available to users.

8 Conclusion

Targeted firearm export licensing is associated with a substantial interruption in the United States' recorded participation in affected foreign markets. The contraction appears in a source-relative design with detailed demand controls, in prolonged reporting gaps, and in public licensing classifications. The combination shows that the response involves more than a shift in the date or reported licensing category of otherwise unchanged trade.

The rollback evidence makes recovery a separate empirical question. Many previously active country–product links record trade again, and pure six-month windows show improvement, but the amount and regularity of restoration remain less precisely measured than the initial interruption.

The contribution is to follow those margins on a fixed pre-policy risk set, not to infer permanent destruction from a short post-policy horizon.

For the economics of firearm markets, the results locate a consequential response upstream of domestic possession and violence. Regulation can remove a supplier from repeated market participation while other sources remain observable. Whether that changes total availability or downstream harm depends on stocks, alternative channels, and users that these public trade records do not identify. Evaluating the trade channel on its own terms is a necessary step toward answering those broader questions.

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Table 1 Sample construction and baseline trade activity

Panel A. Main source panel

Dimension	Coverage
Sources	United States, Germany, Czechia, Italy, Türkiye
Destinations	107, including 22 high-risk destinations
Products	HS930200, HS930330 and comparison HS930400
Calendar support	January 2021–December 2025, 60 months
Full grid after domestic-pair exclusions	95,580 observations
Phase activity regression	89,208 observations, 56 months
Pre-U.S.-active destination–product links	281; regular cohort: 176

Panel B. Pre-policy descriptive rates

Group	Active percent	Gap 3 percent	Gap 6 percent	Activity N
U.S., risk, firearms	46.7	35.2	25.9	1,452
U.S., other, firearms	43.2	40.3	31.5	5,610
U.S., risk, other arms	29.8	48.8	34.4	726
Foreign, risk, firearms	9.8	78.4	69.5	5,808

Notes: Pre-period: January 2021–September 2023. Gap denominators exclude unavailable initial lookbacks; rates are unadjusted, not regression counterfactuals. The underlying 96,300-cell Cartesian grid loses 720 domestic-pair cells. Four transition months remove 6,372 cells from the full grid. Sources: UN Comtrade and frozen support audit. No unsupported reporter-month is filled with zero.

Table 2 Interruption of U.S. firearm trade

	(1) Monthly activity	(2) Three-month gap	(3) Six-month gap
Pause	−18.0 (10.0)	22.9 (13.9)	29.2 (17.2)
Tightening	−52.2 (12.1)	56.3 (13.4)	55.2 (14.7)
Rollback through December 2025	−47.0 (15.4)	46.5 (18.2)	60.6 (19.4)
Tightening 95% interval	[−76.3, −28.1]	[29.8, 82.8]	[26.1, 84.3]
Holm-adjusted tightening p	<0.001	<0.001	<0.001
Joint event pre-period p	0.688	0.919	0.859
Four source FE sets	Yes	Yes	Yes
Observations	89,208	86,022	81,243
Destination clusters	107	107	107

Notes: Equation (1), OLS; percentage points, standard errors in parentheses. Fixed effects: source–destination–product, destination–product–month, source–product–month and source–destination–month. Destination-clustered CRV1 with full FE correction and t(106). Phase definitions follow Section 4. Gap windows may span phases; pure-window results are in Table 3. The initial rollback has only three months and cannot identify a fully post-rollback six-month window. Pretests come from full-calendar event regressions, not the smaller phase samples. Source: UN Comtrade.

Table 3 Recovery levels and changes on extended source support

	(1) Activity	(2) Activity	(3) Pure gap 6	(4) Pure gap 6
Tightening relative to pre	−52.6	−52.3	70.8	71.7
	(12.0)	(13.8)	(17.4)	(21.5)
Rollback relative to pre	−34.7	−31.5	21.7	18.1
	(13.4)	(13.6)	(17.7)	(19.7)
Rollback minus tightening	17.9	20.8	−49.2	−53.6
	(13.6)	(14.8)	(22.9)	(27.5)
Contrast 95% interval	[−9.1, 44.9]	[−8.6, 50.2]	[−94.7, −3.7]	[−108.0, 0.9]
Contrast p-value	0.191	0.163	0.034	0.054
Foreign source count	3	2	3	2
Last observed month	May 2026	June 2026	May 2026	June 2026
Observations	77,775	59,334	52,275	40,194
Destination clusters	107	107	107	107

Notes: Equation (1), percentage points, destination-clustered standard errors in parentheses and t(106) intervals. Three foreign sources: Germany, Czechia, Italy; two: Germany, Czechia; United States always included. All models absorb the same four FE sets. Pure gap 6 retains only endpoints whose complete six-month lookback lies in one phase, changing the calendar sample. Contrasts use the complete joint covariance. These are coverage-selected extensions of the same policy, not independent replications. Source: UN Comtrade.

Table 4 Licensing classifications and aggregation checks

Panel A. Tightening effects across licensing comparisons and aggregation levels

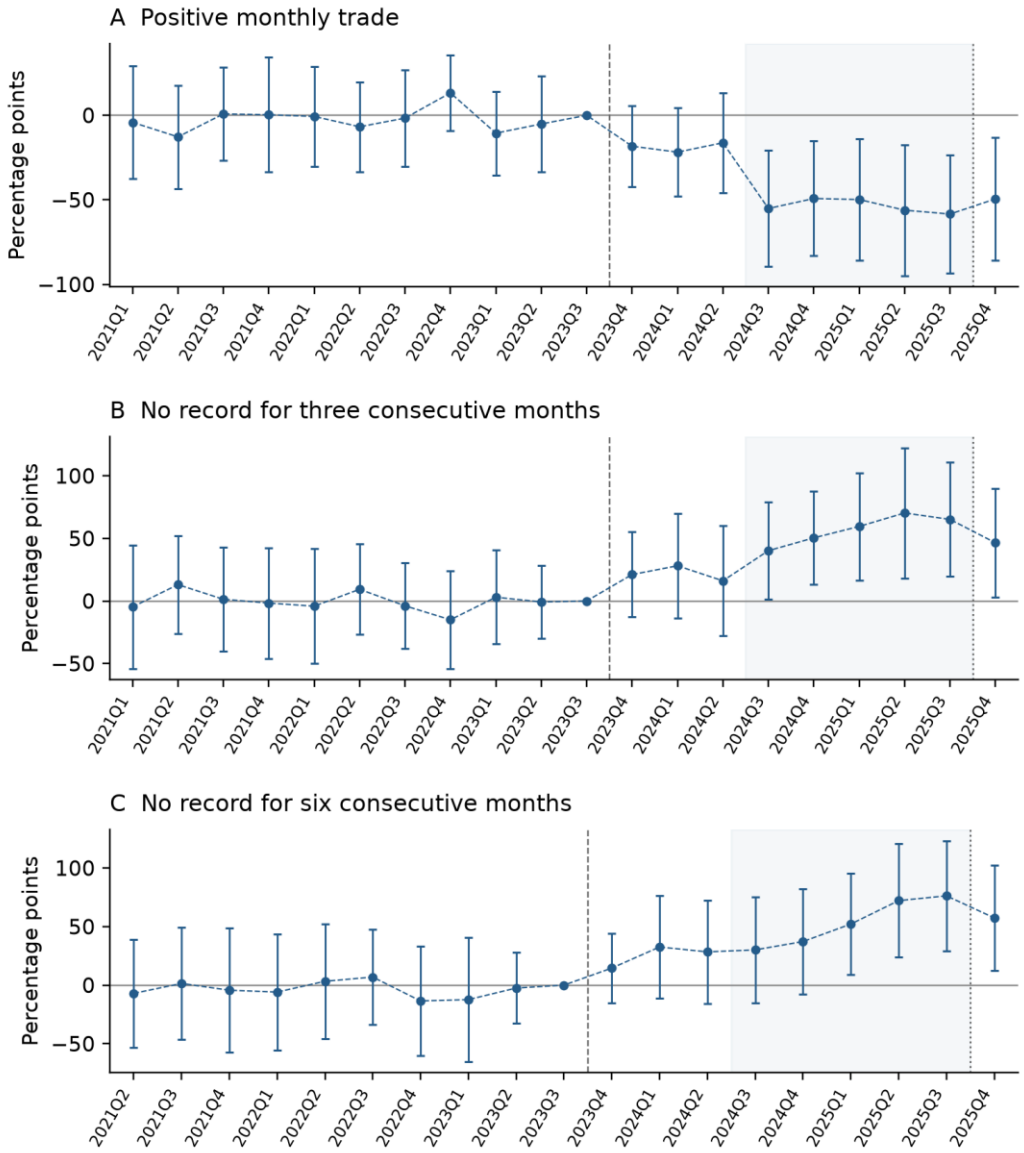
Definition	Tightening (SE)	95% interval	N	Clusters
Mapped Commerce vs State	−42.8 (6.2)	[−55.2, −30.5]	13,624	116
Mapped Commerce vs other C30	−39.1 (6.0)	[−51.0, −27.1]	15,080	122
Mapped ECCNs, all license types	−43.6 (3.9)	[−51.3, −35.9]	6,955	125
Target commodities, all categories	−43.7 (3.9)	[−51.5, −35.9]	6,955	125

Panel B. Main licensing-stream phase response

Phase	Presence, percentage points (SE)	95% interval
Pause	−3.7 (8.5)	[−20.5, 13.1]
Tight	−42.8 (6.2)	[−55.2, −30.5]
Reversal	−32.9 (8.2)	[−49.2, −16.6]

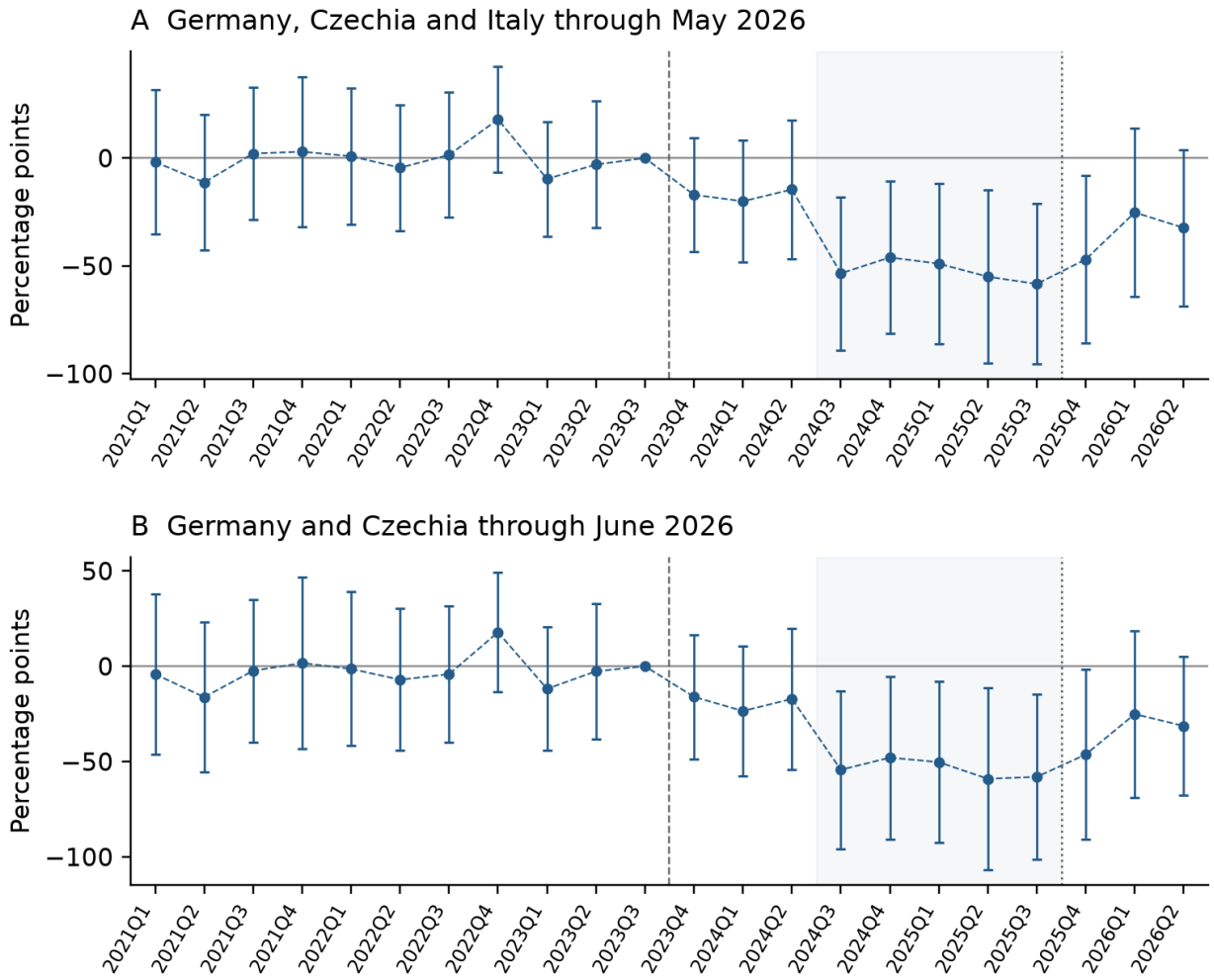
Notes: Census ECCN/licensing data, 13 quarters from 2022Q3–2025Q4, excluding transition quarter 2024Q2 from phase averages. The licensing-stream event study separately estimates that quarter using all 14 quarters. Stream contrasts use equation (2): destination–commodity–stream, destination–commodity–quarter, commodity–stream–quarter FE. Totals collapse licensing streams and use destination–commodity and commodity–quarter FE with risk × phase; they do NOT absorb destination–quarter demand shocks. Clustered by destination, full FE correction; t degrees of freedom equal clusters minus one. Main union: 524 links, 24 high-risk destinations; other-C30 union: 580 links, 25 high-risk destinations; totals: 535 links, 29 high-risk destinations. Strict common stream support is sparse and is not the estimation sample. License categories do not identify end users or approval probabilities. The totals are relabeling diagnostics, not equivalent causal replications.

Figure 1 Interruption in activity and prolonged reporting gaps



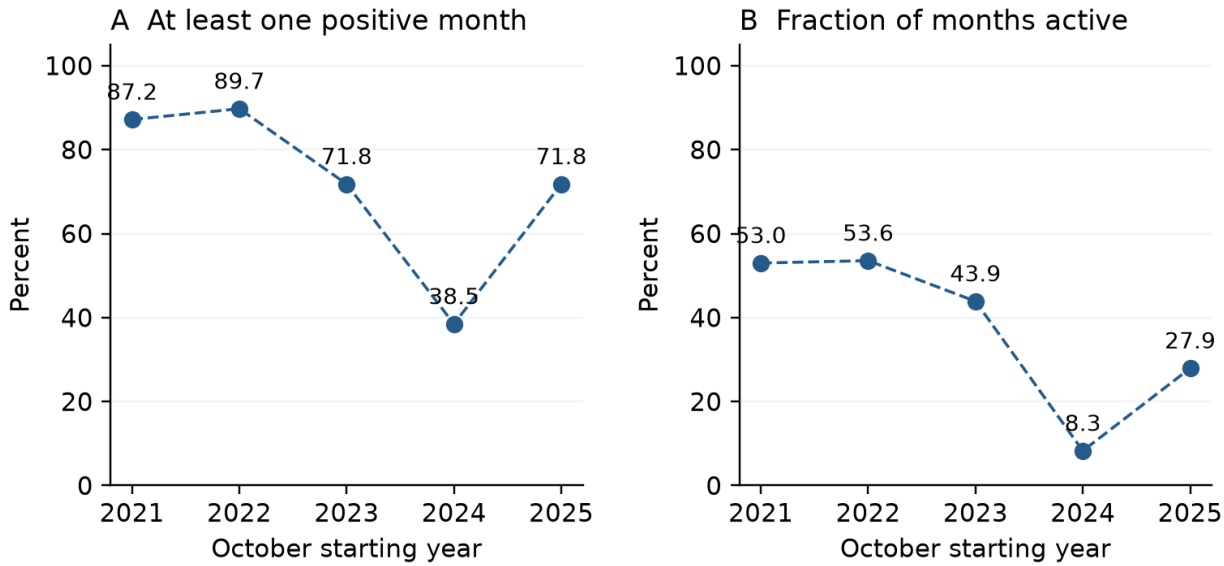
Notes: Quarterly event version of equation (1), all supported 2021–2025 quarters, fixed U.S. plus four foreign sources and 107 destinations. 2023Q3 omitted and shown at zero. Points and destination-clustered 95% t(106) intervals in percentage points. Dashed line precedes 2023Q4, dotted line precedes 2025Q4, shading spans 2024Q3–2025Q3. Transition quarters are retained. Event sample sizes: activity 95,580; gap 3 92,394; gap 6 87,615. Rolling gaps can span phases. Source: UN Comtrade.

Figure 2 Activity following the rollback



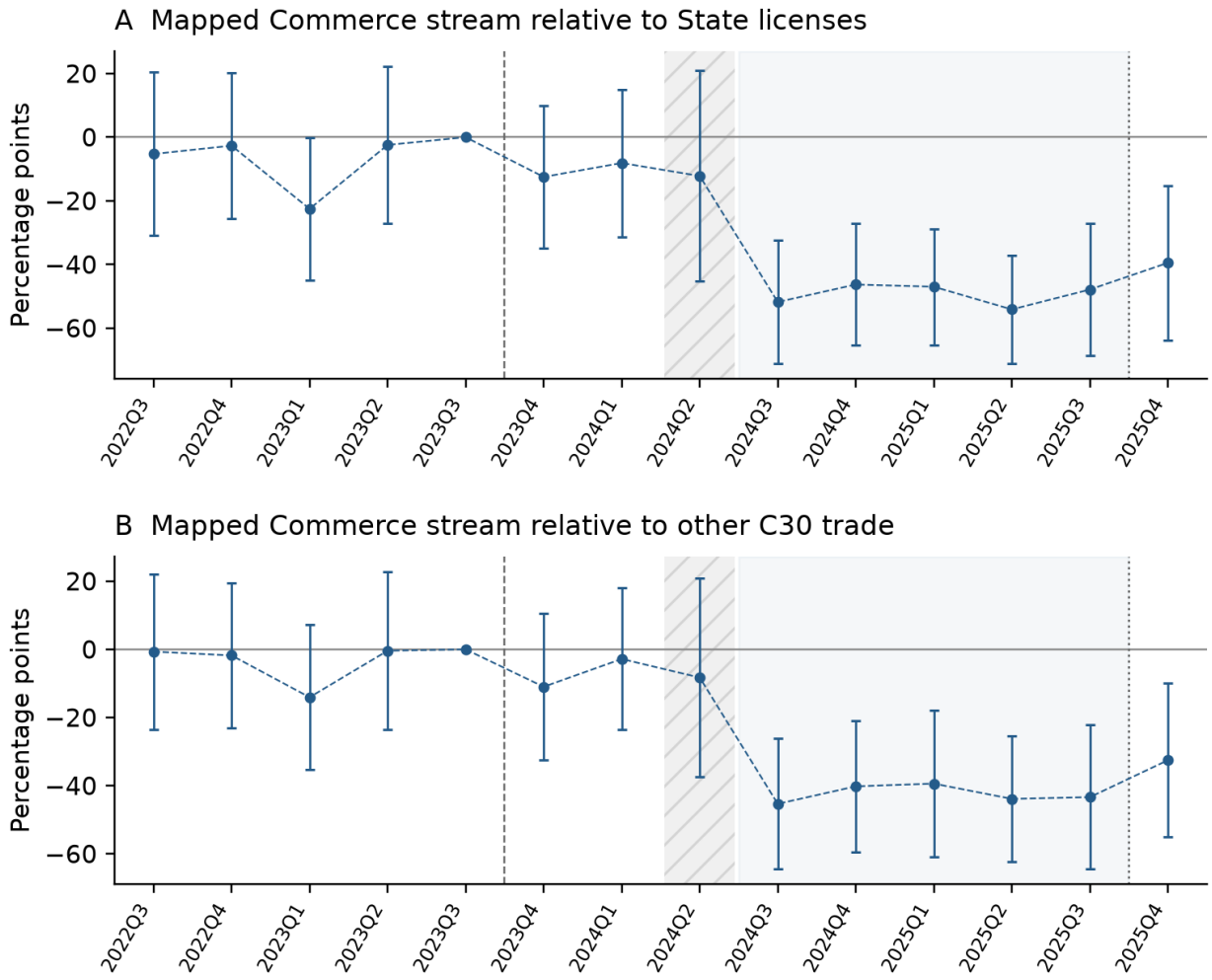
Notes: Quarterly event version of equation (1), 107 destinations and fixed source sets. The United States is compared with the foreign sources named in each panel. 2023Q3 is the reference. Points and destination-clustered 95% t(106) intervals; policy markers as in Figure 1. Panel A ends in May 2026, so 2026Q2 has two months; Panel B ends in June. Both panels retain every supported quarter, including transitions. These are extensions of one policy cycle; negative late coefficients do not establish permanence. Source: UN Comtrade.

Figure 3 Reappearance and regularity on a fixed pre-policy cohort



Notes: Descriptive percentages for the same 39 high-risk destination–firearm-product links with positive U.S. trade in 2021–2022. Each point uses nine months starting October of the indicated year; the 2025 window ends June 2026. Panel A is any positive month; Panel B is the mean fraction of active months. No regression or confidence interval is implied by these lines. Both panels include every qualifying link, regardless of post-policy survival. Formal contrasts and their direct difference appear in Table A3. Source: UN Comtrade.

Figure 4 The interruption within public licensing categories



Notes: Census quarterly ECCN/licensing-category event studies with destination–commodity–stream, destination–commodity–quarter, and commodity–stream–quarter FE. 2023Q3 is the reference; all 14 quarters are included. Hatching marks the 2024Q2 transition, whose coefficient is estimated separately. Panel A: 14,672 observations, 116 destinations; Panel B: 16,240 observations, 122 destinations. Points and destination-clustered 95% t intervals with full FE correction. Four-lead Wald p-values are 0.184 in Panel A and 0.369 in Panel B; one Panel A pre-period coefficient is individually negative and significant at 5%. The comparisons do not identify individual license decisions. Source: Census ECCN releases.

Appendix A. Data and additional evidence

A1 Samples and timing

Table A1 distinguishes the main firearm source panel, its extended-support variants, the earlier quarterly and state-accounting samples, the licensing-stream panel, and the ammunition-and-parts extension. Each exercise retains its original frozen sample and estimand. Supplier availability is checked before filling absent bilateral cells, and domestic pairs are removed. The four-source main panel has 95,580 full-calendar cells. Its 1,593 source–destination–product cells over 56 phase months give 89,208 activity observations. Three- and six-month windows lose the initially unavailable lookbacks, leaving 86,022 and 81,243 phase observations. None of these differences represents deletion based on an estimated policy effect.

The older quarterly exercise retains the original source pool including Spain and therefore does not have the same complete-calendar support as the new main panel. Figure A1 reports both its available-quarter and three-observed-month-quarter estimates. Its phase-calendar construction is preserved, including omitted transition quarters, rather than retrospectively filling them. Figure A2 uses the earlier pre-U.S.-active market cohort and three sets of market fixed effects; it is an accounting decomposition of a fixed pool, not the four-fixed-effect source estimand. Figure A4 restores the original U.S.-versus-foreign display as supporting historical evidence. The two plotted series in that graph are not a formal test of their difference; equation (1) supplies the source-relative contrast used in the main text.

A2 Cohorts and network measures

Pre-policy cohort restrictions use U.S. positive months in 2021–2022. Table A2 reports both any pre-policy activity and the six-positive-month threshold, together with two-way clustering and the 2022 placebo. The principal event studies use the full grid and do not condition on post-policy survival. The two quarters in early 2023 provide a short holdout for specifications whose cohorts or network depth are constructed from 2021–2022. Table A4 reports those holdout tests for every network outcome rather than regarding the construction period itself as independent evidence of parallel trends.

Network depth is the count of foreign sources with positive pre-policy activity in a destination–product market. The heterogeneity models contain risk × covered-product × depth × phase and all phase-related lower-order terms not absorbed by destination–product, destination–month, and product–month fixed effects. Outcomes include any foreign activity, the fraction of available foreign sources active, the active fraction of incumbent foreign sources, and any source entering

outside the incumbent set. Foreign-only activity additionally requires U.S. absence. This last definition means its increase alone cannot identify active foreign substitution.

A3 Recovery and inference

Fixed-window recovery is measured in three-, six-, and nine-month October-starting windows on the same pre-policy-active U.S. cohort. Table A3 uses the full covariance matrix to compare the 2025 window with the 2024 window for reappearance and the fraction of active months. Their direct difference is estimated on the same observations and regressors using the difference between the outcomes; this diagnostic was added after inspection of the recovery summaries and is exploratory. A larger point estimate for reappearance than for frequency is not itself a rejection of equal recovery contrasts.

Table A7 reports the late-period HonestDiD exercise following Rambachan and Roth (2023). Its target is the average of the three 2025Q4–2026Q2 activity event coefficients in the two-foreign-source extension relative to 2023Q3, not the phase-average rollback coefficient or the effect of repeal. The sensitivity parameter bounds post-policy deviations relative to pre-policy deviations. Wide bounds on this multiple-difference estimand are not truncated to the range of an individual binary outcome. The table also retains the separate ammunition-and-parts tightening sensitivity, whose intervals include zero at a relative-magnitude parameter of 0.25. Neither analysis certifies the comparison categories as legally untreated.

A4 Licensing and product scope

The ECCN exercise uses the archived ten-digit firearm commodities and aggregates the mapped 0A5xx Commerce categories, including successor codes introduced in 2024. The event study uses all 14 quarters from 2022Q3 to 2025Q4. Old and successor ECCNs are pooled under the same mapped Commerce definition; the pre-policy cohorts, comparison streams, and fixed effects are unchanged. Phase averages retain the 13-quarter specification, excluding 2024Q2 because implementation occurs within that quarter. Its coefficient is estimated separately in Figure 4. The initial comparison pools State S05, S61, and S73 license categories. The other-C30 comparison and target-product totals in Table 4 address comparison sparsity and relabeling, but are different specifications with different populations. Exact commodity and licensing labels do not reveal end users or individual approval decisions.

The ammunition extension combines Census monthly U.S. exports and the fixed foreign-source records, uses stable Schedule B mappings, and freezes its exposure numerator in 2021–2022 (U.S. Census Bureau n.d.b, 2026). Exposure is the mapped ECCN value divided by all ECCN value in

the destination–product cell. Continuous-exposure models include the U.S. × exposure × phase interaction in addition to the risk interaction; market models include the corresponding lower-order term. Figure A3 shows its source-presence and pooled-presence event studies. The pooled market outcome is not world supply, as Table A6 demonstrates. Asinh value specifications in the retained source archive use their stated currency scale and are not read as percentage changes, following Chen and Roth (2024).

A5 Reproducibility

The analysis can be traced to archived public records and their original reporting support. An accompanying exhibit register maps each figure or table to its archived data and generating script; input hashes preserve the vintages used in estimation. The package retains the complete recovery-window and network outcome families, including their imprecise estimates. Cohort and source restrictions are documented separately from estimation, and unsupported reporter months remain missing. These analyses developed sequentially and are not described as preregistered. All evidence concerns the same licensing cycle.

Appendix B Appendix Tables and Figures

Table A1 Sample and estimand crosswalk

Exercise	Destinations / risk	Support	Interpretation
Main firearm panel	107 / 22	U.S. + 4; 2021–2025	Source-relative activity/gaps
Recovery extensions	107 / 22	U.S. + 3 to May; + 2 to June 2026	Same source design, longer support
Fixed U.S. recovery cohort	99 fitted clusters	U.S.; October windows, 2021–2025 starts	Reappearance and frequency
Earlier frequency and states	107 / 22	Original pool includes Spain	Quarter activity; market-state accounting
ECCN vs State licensing	116 / 24	13 quarters, 2022Q3–2025Q4	Licensing-stream relative response
Harmonized product bridge	82 / 18	U.S. + 4; 2021–2025	Product comparator diagnostic
Updated ammunition/parts	81 / 18	U.S. + 4; 2021–2025	Predetermined exposure gradient
Importer scope audit	60 / 12	All partners, HS930630, 2021–2025	Scope diagnostic, not source replication

Notes: Risk counts refer to each sample, not the full policy list. Domestic source pairs and incomplete reporting support explain differences between Cartesian grids and fitted samples. The 99-cluster recovery design is the fitted fixed U.S.-active cohort, not the 39-link descriptive subgroup. Every source set is fixed within its own regression. Full sample files and hashes are in the exhibit register.

Table A2 Predetermined cohorts and falsification

Specification	Activity	Gap 3	Gap 6
Full source grid	−52.2 (12.1)	56.3 (13.4)	55.2 (14.7)
N	89,208	86,022	81,243
Any pre-policy U.S. activity	−57.7 (12.9)	57.8 (14.9)	52.6 (16.7)
N	75,768	73,062	69,003
At least six pre-policy active months	−70.4 (20.6)	56.7 (21.0)	40.0 (20.8)
N	43,568	42,012	39,678
Destination and month clustering	−52.2 (12.3)	56.3 (13.5)	55.2 (16.0)
N	89,208	86,022	81,243
2022 placebo	2.3 (7.9)	−5.0 (10.7)	−1.2 (13.1)
Placebo p-value	0.773	0.644	0.928
Placebo N	52,569	49,383	44,604

Notes: Tightening coefficients except the explicitly labeled placebo; percentage points with native corrected standard errors. All source specifications have four FE sets. Cohorts use only 2021–2022 U.S. positive months, not later exits. Gap outcomes require consecutive supported months. Destination clustering except the two-way row; native t degrees of freedom follow the limiting cluster count. The 2022 placebo uses only pre-policy observations. Source: UN Comtrade.

Table A3 Recovery contrasts and direct margin tests

Panel A Recovery contrasts by window length and participation margin

Months	Outcome	2025 minus 2024 (SE)	95% interval	Holm p
3	Any record	4.3 (22.2)	[−39.8, 48.4]	1.000
3	Active fraction	1.7 (15.9)	[−29.9, 33.3]	1.000
6	Any record	44.2 (25.0)	[−5.4, 93.8]	0.402
6	Active fraction	13.2 (13.9)	[−14.4, 40.7]	1.000
9	Any record	49.8 (23.7)	[2.7, 96.8]	0.230
9	Active fraction	16.1 (12.0)	[−7.8, 40.0]	0.738

Panel B. Reappearance recovery minus frequency recovery

Months	Difference (SE)	95% interval	Raw p
3	2.6 (12.1)	[−21.4, 26.6]	0.831
6	31.0 (17.1)	[−2.9, 64.9]	0.073
9	33.7 (20.3)	[−6.7, 74.0]	0.101

Notes: Percentage points. Fixed pre-U.S.-active cohort, identical October-start windows and destination–product, destination–window, product–window FE; risk × covered × window interactions, 2022 window omitted. Fitted N = 1,365, 99 destination clusters, t(98). Panel A uses complete covariance for 2025 minus 2024; Holm adjustment across six recovery tests. Panel B is an exploratory direct test on the same observations, using any-record minus active-fraction as the outcome, with unadjusted p-values clearly labeled. No null establishes equivalence. Source: UN Comtrade.

Table A4 Foreign-network heterogeneity including direct activity and entry

Outcome	Depth interaction (SE)	95% interval	Holm p	Holdout p	N
Foreign only	20.7 (5.5)	[9.7, 31.7]	0.002	0.623	15,288
Neither source in pool	−8.3 (7.6)	[−23.4, 6.8]	0.625	0.775	15,288
Any foreign source	7.5 (4.5)	[−1.5, 16.5]	0.515	0.805	15,288
Fraction of foreign sources active	3.0 (2.1)	[−1.2, 7.1]	0.625	0.854	15,288
Incumbent fraction active	6.3 (4.4)	[−2.4, 15.0]	0.625	0.661	13,776
Any nonincumbent source active	−0.1 (2.8)	[−5.5, 5.4]	0.978	0.558	15,288

Notes: Percentage points per additional pre-policy foreign supplier. Fixed pre-U.S.-active markets; 2021–2022 network depth. Destination–product, destination–month and product–month FE, with complete phase-related interaction hierarchy. Destination-clustered standard errors; 99 clusters for most outcomes and 89 for incumbent fraction, which requires positive incumbent depth. Holm covers all six tightening interactions. Holdout jointly tests 2023Q1 and Q2 against 2023Q3, not the 2021–2022 network-construction years. Foreign-only status combines foreign presence with U.S. absence; its significance is not a foreign-entry effect. Source: UN Comtrade.

Table A5 Product comparisons and the ammunition extension

Panel A. Same destinations and suppliers, alternative products

Contrast	Tightening (SE)	95% interval	N
Firearms vs other arms	−60.4 (13.2)	[−86.7, −34.0]	68,208
Ammunition vs other arms	−32.5 (11.4)	[−55.2, −9.8]	45,472
Ammunition vs firearm parts	−3.2 (12.7)	[−28.4, 22.1]	45,472
ECCN exposure, ammunition vs parts	−8.7 (10.5)	[−29.6, 12.2]	45,472

Panel B. Updated binary product comparison against other arms

	(1) Ammunition	(2) Pistol parts
Pause	−0.025 (0.123)	−0.020 (0.079)
Tightening	−0.318 (0.089)	−0.304 (0.106)
Reversal	−0.273 (0.171)	−0.257 (0.173)
Tightening p-value	<0.001	0.005
Observations	67,368	67,368
Joint regression R^2	0.797	0.797

Difference	Standard error	95% interval	p-value
-0.014	0.099	[-0.211, 0.183]	0.888

Notes: Panel A uses a harmonized 2021–2025 panel of 82 destinations, including 18 high-risk destinations, and five suppliers: the United States, Germany, Czechia, Italy, and Türkiye. Estimates include four sets of source-related fixed effects, with destination-clustered standard errors and a t(81) reference distribution. The first three rows report binary product contrasts in percentage points; the final row reports the coefficient per unit of predetermined exposure, including U.S. × exposure × phase interactions. Panel B uses the complete public-data panel of 81 destinations, including 18 high-risk destinations, and three product groups. Both products are compared with HS930400 under the saturated four-fixed-effect specification. Estimates are in fraction units, with standard errors in parentheses; the direct product contrast uses the joint covariance matrix. The separate continuous-exposure estimate is −0.268 (SE 0.100), equivalent to −2.68 percentage points per 10-percentage-point increase in exposure. Firearm parts are partly regulated, and product classifications do not identify shipment-level end-user treatment. Sources: Harmonized and complete public-data panels described in the text.

Table A6 All-partner coverage of the fixed source pool

Panel A. Descriptive importer coverage

Destinations	Phase	Supported months	Pool value share	Outside-only / positive months
High risk	Pre	384	25.0%	47/208
High risk	Tightening	162	37.2%	19/93
Other	Pre	1572	49.2%	53/1141
Other	Tightening	656	51.8%	27/486

Panel B. Importer-side presence diagnostics	(1) All partners	(2) Fixed pool	(3) Outside only
Tightening	−0.047 (0.177)	−0.038 (0.241)	−0.009 (0.124)
p-value	0.791	0.874	0.942
Observations	3,301	3,301	3,301
Destination clusters	60	60	60
R^2	0.701	0.639	0.171

Notes: UN Comtrade importer-reported HS930630, 2021–2025; supported overlap of 60 destinations, 12 high risk and 3,535 importer-months. Panel A compares actual bilateral sums in the same reporting direction. Outside-only means positive all-partner imports with zero imports from the fixed pool. Panel B excludes transition months, N = 3,301, destination and month FE plus risk × phase and exposure × phase lower-order terms; destination-clustered full FE correction, t(59), raw fraction units. This single-product scope audit is not a replication of equation (1). Missing reporting months are not zero-filled.

Table A7 Sensitivity of recovery and the ammunition extension

Panel A. Late firearm activity relative to 2023Q3

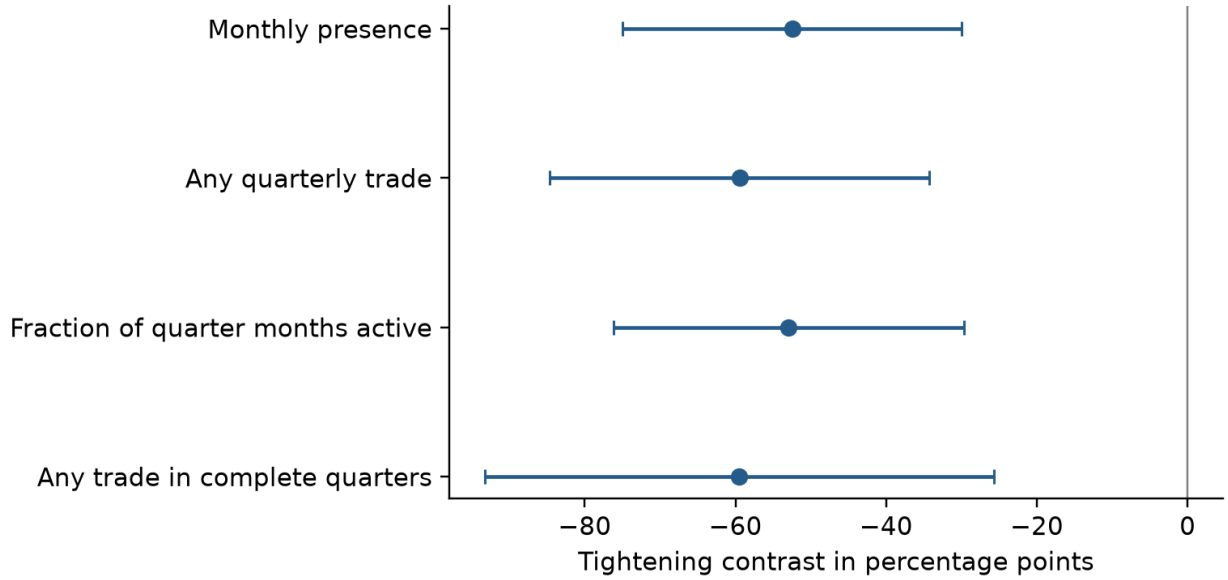
Relative-magnitude bound	95% interval
Original	[−70.6, 2.2]
M = 0	[−70.3, 1.8]
M = 0.5	[−307.4, 228.5]
M = 1	[−569.9, 490.8]

Panel B. Separate ammunition-and-parts tightening sensitivity

Outcome	Relative-magnitude M	95% interval
Source presence	Original	[−7.86, −1.40]
Source presence	0.0	[−7.84, −1.43]
Source presence	0.25	[−13.29, 5.39]
Source presence	0.5	[−20.44, 13.66]
Source presence	1.0	[−36.67, 29.81]
Pool any-source presence	Original	[−5.82, −1.60]
Pool any-source presence	0.0	[−5.80, −0.81]
Pool any-source presence	0.25	[−12.42, 5.25]
Pool any-source presence	0.5	[−20.74, 13.56]
Pool any-source presence	1.0	[−37.23, 30.12]
Pool asinh value	Original	[−0.39, −0.06]
Pool asinh value	0.0	[−0.39, −0.07]
Pool asinh value	0.25	[−0.64, 0.16]
Pool asinh value	0.5	[−0.99, 0.50]
Pool asinh value	1.0	[−1.69, 1.20]

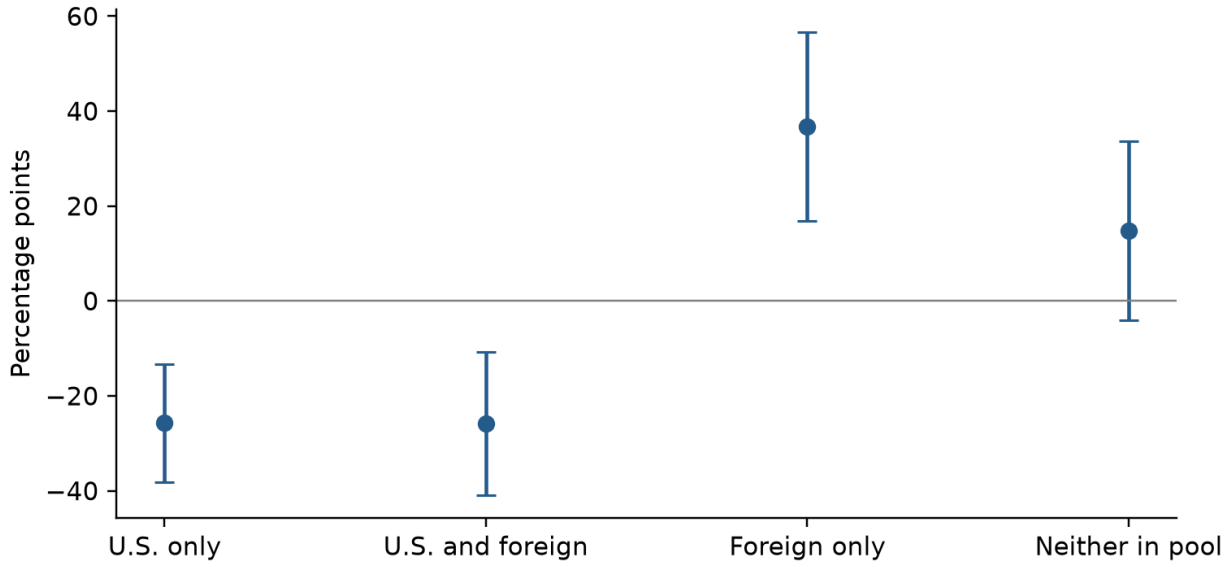
Notes: Both panels apply HonestDiD under the relative-magnitude restrictions of Rambachan and Roth (2023). Panel A targets the equal-weight average of the 2025Q4–2026Q2 source-event coefficients for the United States, Germany, and Czechia, relative to 2023Q3. This measures the remaining activity gap, not the phase effect relative to the entire pre-policy period or the causal effect of repeal. The original confidence interval uses normal critical values. Panel B uses the complete-data ammunition-and-parts sample with 81 destination clusters and targets the average of the 2024Q3–2025Q2 event coefficients relative to 2023Q3. Presence estimates are in percentage points per 10-percentage-point exposure difference; pooled-value estimates are in asinh units for the same exposure difference, with nominal trade values measured in thousands of U.S. dollars before transformation. At M = 0.25, all three Panel B intervals include zero. Bounds are reported without clipping to the binary outcome range. The panels concern different samples and estimands; their sensitivity bounds are not pooled.

Figure A1 Monthly and quarterly activity checks



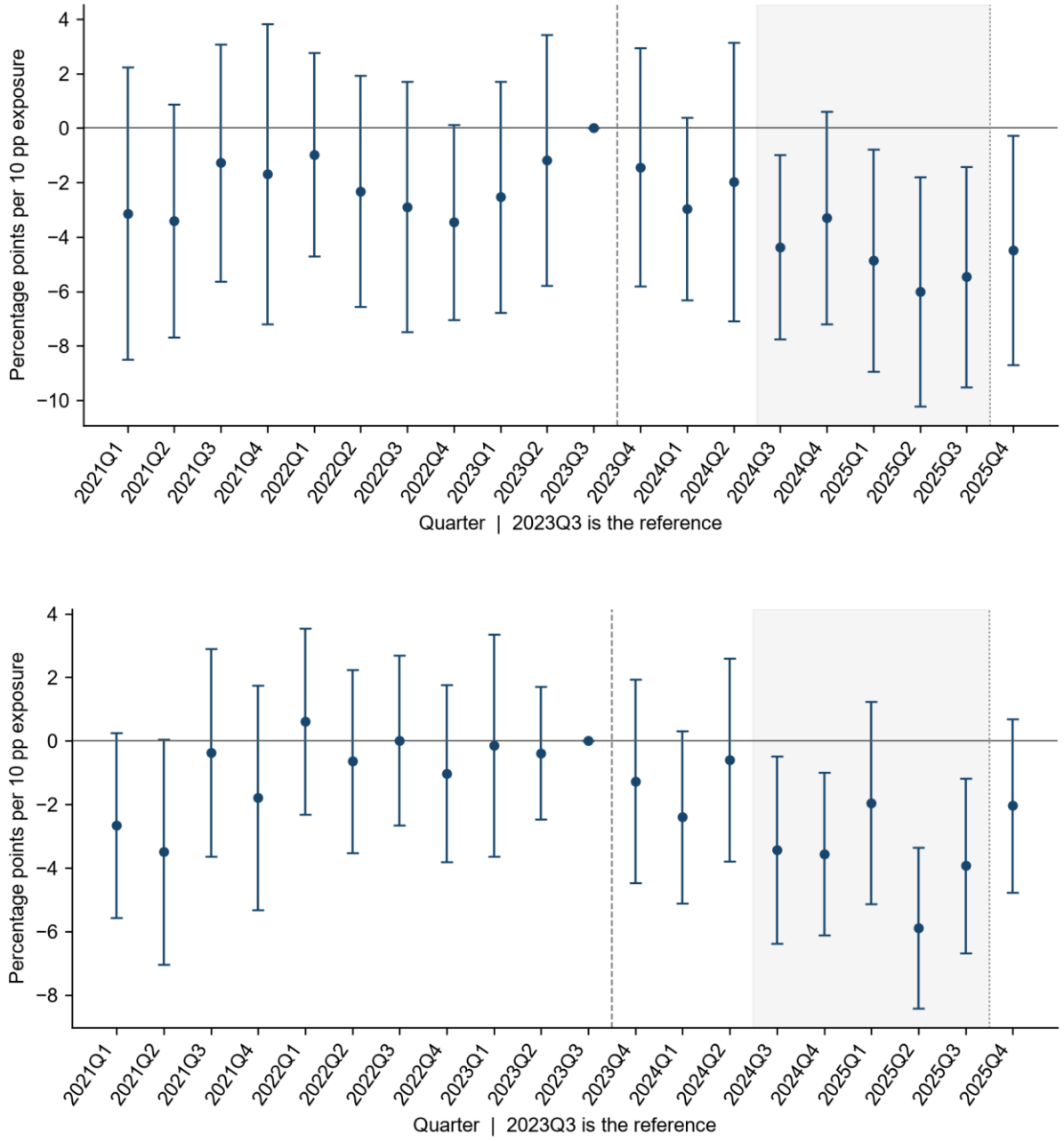
Notes: Frozen earlier source-pool analysis, 107 destinations, including Spain in the foreign pool. Saturated four-FE source contrasts; destination-clustered 95% native t intervals. Quarterly presence records any positive month among supported months; complete-quarter sensitivity requires all three months observed. Its original phase-calendar exclusions are preserved. N: monthly 103,194; available-quarter regressions 36,309; complete-quarter regression 28,665. The monthly N is the original fitted phase sample and is not the current 89,208-cell headline sample. Comparison across outcome scales is descriptive, not a formal equality test.

Figure A2 Accounting for the source composition of trade



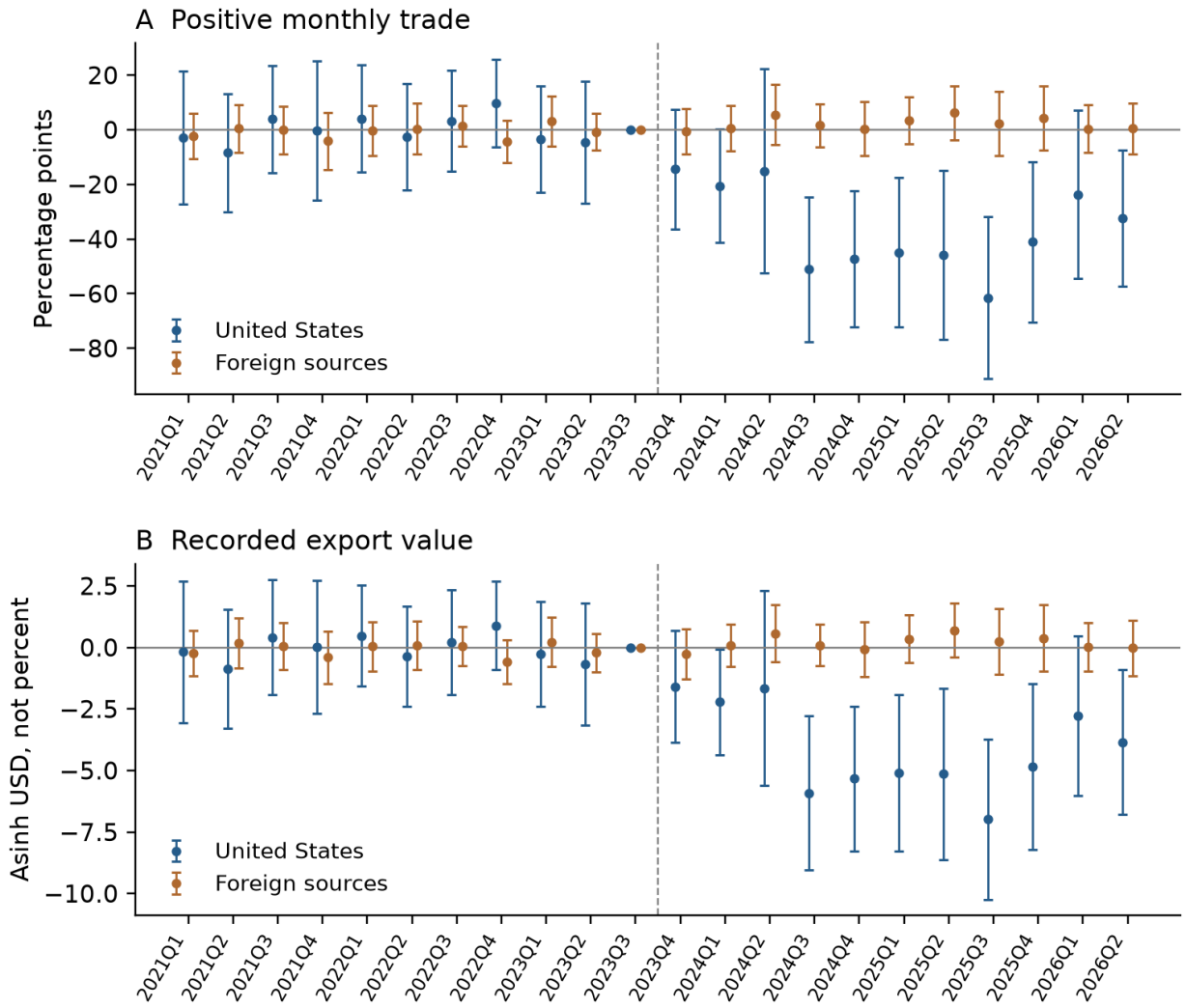
Notes: Original pre-U.S.-active cohort, 298 destination–product links, 107 destinations; phase regression N = 16,092. U.S. plus five foreign sources including Spain. Destination–product, destination–month and product–month FE; tightening interaction risk × covered product. Percentage points and destination-clustered t(106) intervals. States are mutually exclusive and exhaustive within the fixed pool, and their coefficients sum to zero. Neither has an interval including zero. This decomposition does not identify physical replacement, new suppliers, or all-world shortages.

Figure A3 Ammunition and parts under predetermined ECCN exposure



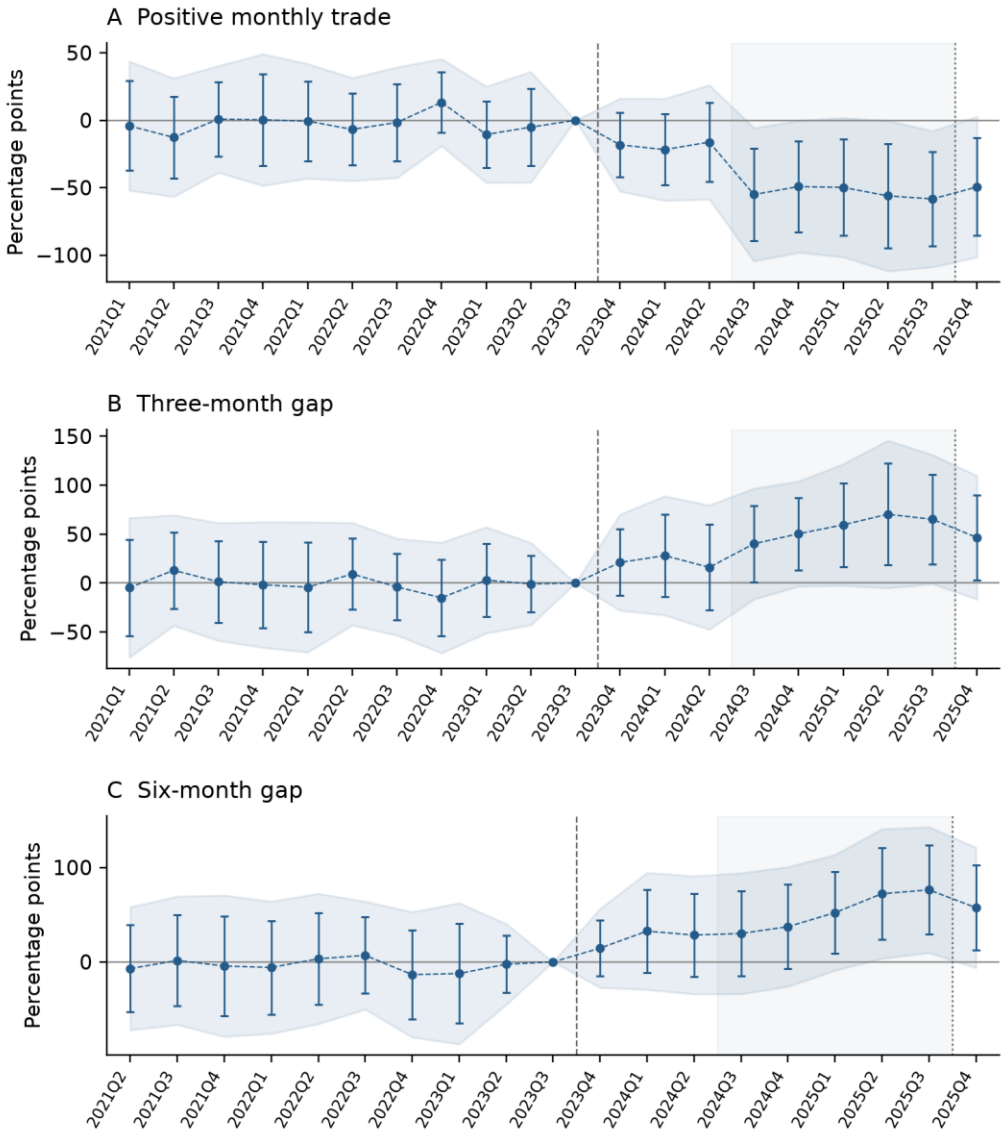
Notes: Top: relative U.S. source presence; bottom: any source in the fixed pool. 81 destinations, U.S. plus four foreign sources, 2021Q1–2025Q4, 2023Q3 omitted. Percentage points per 10-percentage-point predetermined ECCN exposure, destination-clustered 95% t(80) intervals. Source model: four FE sets and U.S. × exposure × quarter; pooled model: three market FE sets and exposure × quarter. Source event N = 72,180; pooled event N = 14,580. The pool is not world supply. Sources: Census and UN Comtrade.

Figure A4 U.S. and pooled foreign-source event paths



Notes: UN Comtrade, fixed observable EU4-source support. U.S. series: destination–product, destination–month and product–month FE. Pooled foreign series: source–destination–product, source–destination–month and source–product–month FE. Quarter × risk × covered-product interactions; 2023Q3 omitted and displayed at zero; original transition-month exclusions retained. Panel A: percentage points; Panel B: asinh nominal USD, NOT percent. 95% destination-clustered intervals. These separate series are not a formal U.S.-minus-foreign test; equation (1) and Figure 1 provide that contrast. No global-shortage or permanence claim is implied.

Figure A5 Simultaneous uncertainty for the main event paths



Notes: Same fitted coefficients and samples as Figure 1. Pointwise 95% destination-clustered t intervals remain visible; shading adds Gaussian max-t simultaneous bands from the full event covariance, using 30,000 simulated draws and seed 20260908. Bands are asymptotic diagnostics, not finite-policy randomization inference. The 2023Q3 reference is shown at zero by construction. Source: UN Comtrade and archived event covariance outputs.