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Weaponized supply: a synthetic-control evaluation of the 2025 rare- earth export controls on importer prices, manufacturing, and substitution

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Abstract

China imposed export licensing controls on a group of heavy rare earth elements in early spring, disrupting supply chains that manufacturers outside China depend on for permanent magnets, catalysts, and specialty alloys. Whether this shock produced a uniform price effect across importer countries and product categories, or a heterogeneous one, remains unexamined in the existing literature on critical mineral export restrictions. This paper evaluates the causal effect of the controls on import prices and the share of imports sourced from China, using an augmented synthetic control design applied to a monthly panel of product-country pairs spanning several years before and after the shock. The estimated price effect is heterogeneous rather than uniform, differing in direction between rare earth metals and rare earth compounds, and a formal test confirms this dispersion reflects a genuine feature of the shock rather than estimation noise. Pre-treatment exposure to Chinese supply does not systematically predict the size of the price response, pointing toward substitution availability and technological adaptation rather than exposure intensity as the likelier explanation for which country and product pairs are affected. Policymakers and manufacturers should treat critical mineral export controls as a shock whose consequences depend on where a product sits in the value chain, rather than assuming a single, predictable price outcome.

Keywords: Rare earth elements; Export restrictions; Synthetic control method; Critical minerals; Import prices

JEL classification: F13; F14; Q37; C21

1. Introduction

On 4 April 2025, China imposed export licensing controls on seven heavy rare-earth elements, materials for which the country accounts for the large majority of global mining and an even larger share of processing capacity. The controls disrupted supply chains within weeks, with reports of production slowdowns among automakers and electronics manufacturers outside China that depend on rare-earth inputs for permanent magnets, catalysts, and specialty alloys. Rare-earth elements enter manufacturing with few readily available substitutes, so a supply disruption can raise import prices, slow downstream production, and accelerate sourcing diversification away from China. This study finds that the price effect of the April 2025 controls is heterogeneous rather than uniform across importer countries and product categories, with a more consistent response in manufacturing pass-through and a directional but imprecisely estimated shift away from Chinese sourcing.

Existing evidence on the economic consequences of Chinese rare-earth and critical mineral export restrictions comes almost entirely from earlier episodes. Chen et al. (2021) show that the 2010 Chinese rare-earth export restrictions raised downstream export prices, concentrated in sectors facing a high rare-earth cost share and limited substitution possibilities. Alfaro et al. (2025) find that the same restrictions induced a surge in patenting activity aimed at reducing rare-earth intensity, expanding downstream production capacity outside China over time. Nassar et al. (2024) estimate that a hypothetical restriction on Chinese gallium and germanium exports would reduce United States gross domestic product by several billion dollars, driven primarily by reduced input availability rather than the price increase. Ben-Michael et al. (2021) provide the augmented estimator this paper applies, developed for policy evaluation with a small number of treated units and imperfect pre-treatment comparability. However, no study has examined the April 2025 controls, which differ from the 2010 episode in the elements targeted, in the addition of dual-use licensing

requirements, and in a trade-policy environment shaped by concurrent tariff escalation between China and its major trading partners.

This study addresses that gap using a monthly panel of 28 product-country pairs spanning seven reporter countries and four rare-earth and related product categories, observed from January 2018 to December 2025. The panel combines rare-earth metals and compounds, the two categories targeted by the April 2025 controls, with two related but uncontrolled categories that serve as a donor pool. Using methods designed for causal policy evaluation with few treated units and no natural comparison group, this study estimates the effect of the controls on import prices, manufacturing pass-through, and the share of imports sourced from China. The price response differs by product category: a subset of treated country-product pairs shows an effect distinguishable from the donor placebo distribution, while manufacturing industrial production in more exposed reporter countries responds at the treatment date.

This study contributes the first causal evaluation of the April 2025 rare-earth export controls, providing evidence that the price consequences of the shock depend on where in the rare-earth value chain a product sits rather than affecting all rare-earth trade uniformly. This finding bears on the policy debate surrounding the rare-earth export licensing regime of China and the parallel efforts by the United States, the European Union, and Japan to diversify rare-earth sourcing and rebuild domestic processing capacity. The analysis cannot rule out that the estimated effects would evolve differently over a longer post-treatment horizon than the nine months available, and the manufacturing estimates rely on industrial production data requiring a seasonal carry-forward adjustment for three of seven reporter countries. The findings are relevant to policymakers assessing critical mineral export controls as a strategic instrument, and to import-dependent manufacturers evaluating their exposure to concentrated rare-earth supply chains. The paper reviews the relevant literature, presents the data and empirical framework, reports results, and concludes with implications.

2. Literature review

2.1 Economic effects of critical mineral and rare earth export controls

Export controls on concentrated critical mineral supplies generate measurable economic costs, but the size and direction of those costs depend on substitution possibilities, downstream exposure, and how long the restriction remains in force. Nassar et al. (2024) establish that a complete restriction of Chinese gallium and germanium exports would reduce United States gross domestic product by several billion dollars, concentrated in semiconductor manufacturing, with the loss driven primarily by reduced input availability rather than by the price increase itself. Building on the 2010 Chinese rare earth export restrictions, Chen et al. (2021) show that the policy raised export prices of rare earth downstream products from China, but that the effect on export quantity and value was heterogeneous across sectors, significant only where the rare earth cost share was high and the elasticity of substitution was low.

Extending this setting further, Alfaro et al. (2025) establish that the same 2010 restrictions triggered a surge in global patenting activity aimed at economising on rare earth inputs, which expanded downstream production capacity outside China and substantially mitigated the foreign welfare losses that a static substitution margin alone would predict. In contrast, Han et al. (2024) find that the type of restriction matters as much as its severity: outright export prohibitions on minerals inhibited renewable energy consumption and slowed energy transition in a panel of Global South countries, while licensing-based restrictions produced comparatively milder effects. Using a multi-regional input-output simulation of the 2025 controls, Zhao et al. (2026) similarly find that reduced reliance on Chinese rare-earth supply raises importer production costs and welfare losses, and that the technological capability of substitute suppliers, rather than their geopolitical alignment, determines which countries absorb the resulting trade reallocation. Collectively, these studies confirm that export

restrictions on concentrated mineral inputs impose real economic costs on importing economies, but that the magnitude of those costs is mediated by sectoral substitution elasticities, restriction design, and the pace of technological adaptation abroad.

A separate strand of the literature documents how rare earth prices respond to policy and geopolitical shocks without attempting to isolate the causal effect of any single episode. Seiler (2021) shows that the transmission of Chinese domestic prices to export prices exhibits threshold behaviour that shifted after the 2015 removal of the export quota system, and Seiler (2024) extends this to the frequency domain, finding that price leadership between domestic and export markets varies across time horizons rather than following a single stable pattern. Nguemgaing (2026) confirms and extends both findings using a more recent sample, while Proelss et al. (2018) show that the announcement of the 2012 World Trade Organization dispute itself altered rare earth price dynamics and stock price informativeness before any ruling was issued. Li et al. (2023) and Fan et al. (2023) each document a positive association between geopolitical risk and rare earth prices using wavelet decomposition and a thirty-year panel of Japanese import data respectively, and Farag and Zaki (2025) extend this logic to a broader set of critical minerals, showing that political alignment with China or the United States predicts bilateral trade volumes in a gravity model spanning 1995 to 2022.

A parallel body of work substitutes theoretical or simulation structure for a data-driven counterfactual. Charlier and Guillou (2014) model the 2012 rare earth export quota dispute as a monopolist extracting a non-renewable resource and estimate import demand elasticities consistent with strategic price manipulation, while Manley et al. (2022) use an input-output framework with a linear optimisation routine to simulate the industry-level effects of hypothetical mineral supply disruptions rather than estimate the effect of an occurred one. Yilanci et al. (2022) apply second-generation panel cointegration techniques to test whether cobalt abundance predicts economic growth across seven producer countries, and Claudio-Quiroga, Gil-Alana, and Maiza-Larrarte (2023) use fractional integration to characterise the persistence of mineral prices relevant to the Chinese new energy vehicle industry; both approaches describe long-run statistical properties of mineral prices rather than the treatment effect of a dated policy shock.

Smith and Eggert (2018) instead document the demand-side response directly, using an expert survey to rank the industry strategies that magnet and wind turbine manufacturers adopted following the 2010 to 2011 rare earth price spike, finding that adopting an existing production process with less material input was the most common response. None of this literature, whether tracing price co-movement, modelling quota policy structurally, or surveying industry adaptation, constructs a counterfactual price trajectory against which the causal effect of a specific, dated export-control episode can be measured. Despite this, no study has examined the April 2025 rare earth export controls, which differ from the 2010 episode in scope, in the addition of dual-use licensing requirements, and in a trade-policy backdrop shaped by concurrent tariff escalation between China and its major trading partners.

2.2 Synthetic control methods for policy evaluation

The synthetic control method has become a standard quasi-experimental tool for evaluating aggregate policy shocks in settings with a small number of treated units and no natural comparison group. Abadie et al. (2010) establish the foundational estimator, constructing a weighted combination of untreated units that reproduces the pre-treatment trajectory of the treated unit more closely than any single comparison unit could. Extending this framework to settings where exact pre-treatment fit is infeasible, Ben-Michael et al. (2021) propose an augmented estimator that corrects the weighted combination with a ridge-regularised bias term, which improves estimation precisely when the donor pool is too small or too dissimilar from the treated unit for the original method to achieve close pre-treatment balance.

Applying the augmented estimator to an environmental policy shock, Cole et al. (2020) demonstrate that the method identifies credible counterfactual pollution trajectories for a single treated city, confirming its applicability outside the state- and country-level settings of the original proposal.

The augmented and classical synthetic control methods have also been applied directly to export and resource-restriction policy, though in narrower settings than the present study. García-Lembergman et al. (2018) use synthetic control with in-space placebo inference to show that quantitative export restrictions on Bolivian beef reduced both export and domestic production, contradicting the policy stated objective of increasing supply to the domestic market. In a related resource-shock application, Coelho dos Santos et al. (2024) use synthetic control weights and a balance table structure to estimate the income effect of a major oil discovery in Brazil, establishing a template for reporting donor weights and pre-treatment fit that generalises to other resource-sector applications. Most recently, Zhang and Wu (2025) apply synthetic control methods to evaluate the effectiveness of Chinese tungsten export policy, confirming the feasibility of the method for a critical mineral distinct from rare earths. Yet each of these applications examines a single commodity and a single outcome variable; none constructs a multi-product, multi-country panel or evaluates more than one transmission channel of a single export-control shock simultaneously.

Alternative causal designs for evaluating a policy shock with few treated units each impose requirements that the present setting does not satisfy. Xu (2017) generalises the synthetic control method to accommodate multiple treated units and time-varying unobserved confounders through an interactive fixed effects model, and Arkhangelsky et al. (2021) combine synthetic control weighting with difference-in-differences in a synthetic difference-in-differences estimator; both relax the parallel trends assumption that difference-in-differences alone requires, but both rely on a control group large enough to estimate unit and time weights reliably, a condition a fourteen-element donor pool satisfies only marginally.

Athey et al. (2021) propose a matrix completion estimator that imputes missing counterfactual outcomes through low-rank structure, which performs best in wide panels with many units and periods rather than in a panel of the present scale. Kreif et al. (2015) show directly that synthetic control and difference-in-differences can produce materially different, and even oppositely signed, conclusions when multiple units are treated simultaneously, underscoring that the choice of estimator is not innocuous in exactly the setting this study confronts. Ferman (2021) and Tian et al. (2026) formalise this concern further, showing that synthetic control estimates lose precision as the number of pre-treatment periods and donor units shrinks and proposing a multiple-outcome extension intended to partially offset this loss, which the multisynth pooling specification adopted here is designed to exploit.

Estimators developed for broader trade-policy evaluation face a different limitation: most require data or assumptions unavailable in a twenty-eight-element panel with a single dated shock. Handley et al. (2024) estimate the pass-through of the 2018 to 2019 United States tariffs using confidential firm-to-firm transaction data, identifying effects from variation across thousands of importing and exporting firms rather than from a small set of treated product-country pairs. Amiti et al. (2019) and Fajgelbaum et al. (2020) similarly estimate the incidence of the same tariffs using disaggregated customs data and a calibrated general-equilibrium trade model respectively, both of which require either a large number of tariff lines for identification or externally imposed trade elasticities that the present design avoids.

Imbs and Pauwels (2024) propose a computable general-equilibrium approximation for the effects of trade sanctions against Russia that likewise depends on calibrated substitution elasticities, and Yalcin et al. (2025) estimate the effect of sanctions on Russian trade using a structural gravity model fitted to decades of bilateral flows across many country pairs. Attílio (2025) documents a spillover relationship between critical mineral prices and renewable energy deployment using a global vector autoregression, which captures co-movement across markets but does not test whether that co-movement reflects the causal effect of a

specific policy episode. None of these designs is built for the setting this study faces: a single dated shock, a small number of treated units, and no natural comparison group, which is precisely the setting the augmented synthetic control method was developed to address.

A closely related application has since evaluated a comparable episode. Building on product-level trade data from 2017 to 2023, a recent study applies a counterfactual approach combined with a multi-regional input-output model to assess how the earlier phase of United States-China rare earth trade tensions reshaped United States import sourcing, reporting substantial heterogeneity across product types and finding that the technological capability of substitute suppliers, rather than geopolitical alignment, best explains which countries absorbed diverted trade. This design shares the counterfactual logic of the present study but evaluates a different and earlier shock, relies on an input-output model to translate trade reallocation into macroeconomic effects rather than jointly estimating price, manufacturing, and substitution channels directly, and does not test whether the heterogeneity it documents exceeds what sampling variation alone would produce. The present study instead isolates the April 2025 shock specifically, estimates all three outcome channels within a single empirical framework, and subjects the resulting heterogeneity to a formal statistical test.

Overall, the literature on critical mineral export controls and the literature on synthetic control methods for policy evaluation identify three gaps that remain jointly unaddressed. First, no study has evaluated the economic consequences of the April 2025 rare earth export controls, a shock materially different from the 2010 episode in scope, licensing design, and geopolitical context. Second, no application of synthetic control methods to an export-control shock has evaluated more than one outcome channel within a single study, leaving the joint behaviour of prices, downstream manufacturing, and substitution response unexamined within a unified empirical framework. Third, existing synthetic control applications to resource and mineral policy operate at the single-commodity level, leaving open whether a product-country panel spanning several related product categories can support both element-specific and pooled inference within the same design. A fourth gap follows from the estimator comparison above: no study facing a comparably small donor pool and a single dated shock has combined an augmented synthetic control design with a formal test of whether the resulting cross-unit heterogeneity is genuine rather than an artefact of estimation noise. This study addresses each of these gaps: it evaluates the April 2025 rare earth export controls using an augmented synthetic control design applied to a 28-element product-country panel and jointly estimates price, manufacturing pass-through, and substitution effects within a single empirical framework.

3. Data and methods

3.1 Data

The analysis is based on a monthly panel of 28 product-country pairs, referred to throughout as elements, observed from January 2018 to December 2025 for a total of 96 months and 2,688 element-month observations. Each element combines one of four Harmonized System (HS) six-digit product codes with one of seven reporter countries: France, Germany, Japan, the Netherlands, South Korea, the United Kingdom, and the United States. The treated group comprises the two product codes directly named in the April 2025 licensing controls, rare-earth metals and rare-earth compounds, across all seven reporters, for 14 treated elements. The donor group comprises two chemically or industrially related but uncontrolled product codes, cerium compounds and permanent magnets, across the same seven reporters, for 14 donor elements. Summary statistics for all variables used in the analysis are reported in Table 1.

The panel structure and treatment assignment are defined formally as follows. Let $i = 1, \dots, N$ index elements, with $N = 28$, and let $t = 1, \dots, T$ index months, with $T = 96$. The treatment indicator is:

$$D_{it} = \mathbb{1}\{i \in \mathcal{T}\} \cdot \mathbb{1}\{t \geq t^*\},$$

where $\mathcal{T}$ denotes the set of 14 treated elements, and $t^* = 88$ denotes the treatment month corresponding to April 2025. An element is treated from t^* onward if and only if it belongs to $\mathcal{T}$; donor elements have $D_{it} = 0$ for all t.Trade flow data, comprising the unit import value, import quantity, and the share of import quantity sourced from China (CHINASH) for each element and month, are drawn from the United Nations Comtrade Database (UNCOMTRADE). The pre-treatment period spans months 1 to 87 and the post-treatment period spans months 88 to 96.

Three time-varying covariates supplement the trade data. The Geopolitical Risk Index (GPR) of Caldara and Iacoviello (2022) proxies for global political and security tension. Clean energy generation and real gross domestic product (GDP) are drawn from the Our World in Data (OWID) energy dataset, with the underlying GDP series sourced from the Maddison Project Database. Manufacturing industrial production (IP), used in the manufacturing pass-through analysis, is assembled from three national and international sources depending on reporter coverage: the Ministry of Economy, Trade and Industry (METI) index for Japan, the FRED manufacturing series for the United States, and OECD Short-Term Economic Statistics production volume indicator for the remaining five reporters. IP coverage for France, Germany, the Netherlands, South Korea, and the United Kingdom becomes intermittent after April 2024 and is completed using a seasonal carry-forward adjustment for the affected months; Japan and the United States retain complete native coverage throughout the sample. This asymmetry in IP coverage is a limitation of the manufacturing channel specifically and does not affect the price or substitution analyses, which rely only on UNCOMTRADE trade flow data.

Table 1. Summary statistics

Panel A. Full sample

Acronym	Variable	Source	N	Mean	SD	Min	Median	Max
LNPRICE	Unit value, log (USD/kg)	UNCOMTRADE	2688	3.06	1.22	-0.76	3.14	8.99
PRICE	Unit value (USD/kg)	UNCOMTRADE	2688	57.14	284.78	0.47	23.21	8060.72
QTY	Import quantity (kg, world total)	UNCOMTRADE	2652	340,696	612,662	0	130,387	13,712,216
GPR	Geopolitical Risk Index (GPR)	GPR	2688	0.84	0.95	0.02	0.49	6.90
LNCLEAN	Clean energy generation, log (TWh)	OWID	2688	4.48	1.02	2.54	4.35	6.75
LNGDP	Real GDP, log (intl USD)	MADDISON	2688	28.83	0.88	27.41	28.61	30.67
IP	Manufacturing IP index (2015=100)	METI/FRED/ OECD	2688	104.14	9.51	66.70	103.58	130.03
CHINASH	China import share	UNCOMTRADE	2652	0.49	0.31	0.00	0.52	1.00

Panel B. Element-group summary

Group	N elements	N obs	Mean log price	SD log price	Min	Max	Mean China share	Mean GPR
Ce_Donor	7	672	2.0722	1.0736	-0.7566	6.0213	0.209	0.83
Magnet_Donor	7	672	3.4631	0.722	-0.1447	4.7542	0.664	0.84
Wave1_Treated	14	1344	3.3539	1.2255	-0.3628	8.9948	0.527	0.84

Notes: Obs. = element x month_id (28 elements = 7 reporters x 4 HS products), month_id 1-96 (Jan. 2018-Dec. 2025).

3.2 Methods

The price effect of the export controls is estimated using the augmented synthetic control method (SCM) of Ben-Michael et al. (2021). For each treated element $i \in \mathcal{T}$, the estimator

constructs a synthetic counterfactual as a weighted combination of the donor elements and augments this combination with a ridge-regularised correction for any imbalance the weighting alone cannot remove. The weights and correction solve:

$$\begin{aligned}\widehat{\mathbf{w}}_i &= \operatorname{argmin}_{\mathbf{w}\in\Delta} \left\| \mathbf{y}_{i,\text{pre}} - \textstyle\sum_{j\in\mathcal{D}} w_j \ \mathbf{y}_{j,\text{pre}} \right\|_2^2, \\ \hat{y}_{it}^{(0)} &= \textstyle\sum_{j\in\mathcal{D}} \widehat{w}_j \ y_{jt} + \hat{\theta}_{it}^{\text{ridge}}, \qquad t \geq t^*,\end{aligned}$$

where $\mathbf{y}_{i,\text{pre}}$ denotes the vector of 12 months of pre-treatment log import unit values for element i, Δ denotes the simplex of convex donor weights, $\mathcal{D}$ denotes the donor pool, and $\hat{\theta}_{it}^{\text{ridge}}$ denotes the ridge-regularised bias correction applied to the post-treatment synthetic trajectory. Ridge regularisation, which shrinks the correction coefficients toward zero, is preferred in this application over an unregularised correction because the donor pool is small relative to the number of pre-treatment periods available for matching.

The 12-month matching window uses pre-treatment log import unit values rather than the time-varying covariates directly, since the covariates are measured annually and repeating an annual value across 12 months within a short pre-treatment window produces a rank-deficient matching problem. The average treatment effect on the treated (ATT) for element i at post-treatment month t is the gap between the actual and synthetic trajectories, $\widehat{\text{ATT}}_{it} = y_{it} - \hat{y}_{it}^{(0)}$. Inference for each element-specific ATT uses the jackknife+ procedure, which constructs a prediction interval by repeatedly leaving out one pre-treatment period, re-estimating the donor weights, and forming the interval from the resulting distribution of leave-one-out residuals rather than from a parametric error distribution.

A second specification pools all 14 treated elements into a single multisynth model, which balances two competing objectives through a partial pooling parameter $\nu \in [0,1]$:

$$(\widehat{\mathbf{w}}_1, \ldots, \widehat{\mathbf{w}}_{14}) = \arg\min_{\mathbf{w}_1,\ldots,\mathbf{w}_{14}\in\Delta} (1-\nu) \textstyle\sum_{i\in\mathcal{T}} \left\| \mathbf{y}_{i,\text{pre}} - \sum_{j\in\mathcal{D}} w_{ij} \ \mathbf{y}_{j,\text{pre}} \right\|_2^2 \ + \ \nu \left\| \overline{\mathbf{y}}_{\mathcal{T},\text{pre}} - \sum_{j\in\mathcal{D}} \bar{w}_j \ \mathbf{y}_{j,\text{pre}} \right\|_2^2,$$

where $\overline{\mathbf{y}}_{\mathcal{T},\text{pre}}$ denotes the average pre-treatment trajectory across all treated elements and $\bar{w}_j$ denotes a common donor weight shared across treated elements. Setting $\nu = 0.5$ places equal weight on fitting each treated element on its own donor weights and on estimating a single pooled effect across all treated elements sharing a common set of weights to the extent the data support it. The pooled specification uses the same 12-month lag structure as the element-specific models and extends 9 months beyond the treatment date, spanning the full post-treatment window available in the panel.

Two placebo procedures assess the credibility of the SCM design. The in-space procedure re-estimates the treated-element model with each donor element $d \in \mathcal{D}$ substituted as a pseudo-treated unit and compares the resulting pre-to-post fit to that of the actually treated element:

$$\begin{aligned}R_i &= \frac{\text{RMSPE}_i^{\text{post}}}{\text{RMSPE}_i^{\text{pre}}}, \qquad \text{RMSPE}_i^k = \sqrt{\frac{1}{|k|}\textstyle\sum_{t\in k} \left(y_{it} - \hat{y}_{it}^{(0)}\right)^2}, \\ p_i &= \frac{1}{|\mathcal{D}|}\textstyle\sum_{d\in\mathcal{D}} \mathbb{1}\{R_d \geq R_i\},\end{aligned}$$

where RMSPE_i^k denotes the root mean squared prediction error of element i over period $k \in \{\text{pre,post}\}$, R_i denotes the ratio of post-treatment to pre-treatment RMSPE, and p_i denotes the permutation p-value, equal to the share of donor ratios that equal or exceed the treated ratio. With 14 donors, the smallest attainable p_i under this procedure is one in fifteen. The in-time procedure re-estimates the treated-element model using only pre-treatment data with a pseudo-treatment date assigned well before t^*, at three placebo dates spaced across the

pre-treatment window, and examines whether the estimated effect at each pseudo-treatment date is distinguishable from zero.

The manufacturing pass-through channel is estimated on a country-month panel collapsed from the element-month panel, using a two-way fixed-effects (FE) event-study specification:

$$\ln(\text{IP}_{ct}) = \sum_{\substack{k=-12 \\ k\neq -1}}^{9} \beta_k \left(\mathbb{1}\{t - t^* = k\} \times \text{ChinaExp}_c\right) + \gamma\, \text{GPR}_t + \delta \ln(\text{GDP}_{ct}) + \alpha_c + \tau_t + \varepsilon_{ct},$$

where c indexes reporter country, IP_{ct} denotes manufacturing industrial production, ChinaExp_c denotes reporter c pre-treatment exposure to Chinese rare-earth supply, α_c and τ_t denote reporter and month FE, and standard errors are clustered by reporter. The period immediately preceding treatment, $k = -1$, is the omitted reference category.

The substitution channel is estimated on the element-month panel using a two-way FE difference-in-differences (DiD) specification:

$$\text{CHINASH}_{it} = \phi(\mathbb{1}\{i \in \mathcal{T}\} \times \mathbb{1}\{t \geq t^*\}) + \gamma\, \text{GPR}_t + \delta \ln(\text{GDP}_{it}) + \alpha_i + \tau_t + \varepsilon_{it},$$

where α_i and τ_t denote element and month FE, and standard errors are clustered by element. The coefficient ϕ is the DiD estimate of the change in Chinese import share attributable to the export controls.

Three additional checks assess the sensitivity of the pooled price estimate to modelling choices. The staggered DiD estimator of Callaway and Sant'Anna (2021), abbreviated CS-DiD, is applied as an alternative to the augmented SCM, using the never-treated donor elements as the control group. The estimator first computes group-time average treatment effects and then aggregates them:

$$\begin{aligned} \text{ATT}(t^*, t) &= E[y_{it} - y_{it^*-1} \mid i \in \mathcal{T}] - E[y_{it} - y_{it^*-1} \mid i \in \mathcal{D}], \\ \text{ATT}^{\text{overall}} &= \tfrac{1}{9}\textstyle\sum_{t=t^*}^{96} \text{ATT}\,(t^*, t), \end{aligned}$$

where $\text{ATT}(t^*, t)$ denotes the group-time average treatment effect for the treatment cohort at month t^* observed at month t, and $\text{ATT}^{\text{overall}}$ denotes the simple aggregation across the nine post-treatment months into a single summary effect. Three alternative donor pool specifications are compared against the primary specification, which uses the full donor pool of cerium compounds and magnets: a restricted pool using cerium compounds only, a restricted pool using magnets only, and the full pool. All specifications are estimated with the same 12-month lag structure, and the alternative donor pools additionally apply the same partial pooling parameter of $\nu = 0.5$ used in the primary pooled specification.

Four additional checks extend the primary analysis beyond the estimator and donor-pool comparisons already described. First, the pre-treatment reliability of the primary Comtrade-derived CHINASH measure is assessed against an independently constructed annual panel drawn from the BACI trade database, correlating the two measures across all reporter-year-product observations available in both sources prior to the treatment date. Second, the relationship between each reporter pre-treatment exposure to Chinese rare-earth supply and the estimated post-treatment price effect is tested directly, using the same CHINASH_treated exposure measure constructed for the manufacturing pass-through analysis, regressed against the element-level mean post-treatment average treatment effect on the treated (ATT) with a product-category control.

Third, the sensitivity of the pooled ATT to the length of the post-treatment window is assessed by re-estimating the mean pooled effect restricted to the first three and first six post-treatment months, in addition to the full nine-month window used in the primary specification. Fourth, the degree of heterogeneity across element-level ATT estimates is tested formally using a precision-weighted Q statistic of Cochran, which compares the observed dispersion of the fourteen element-level estimates to the dispersion sampling

variation alone would produce, with the associated I-squared statistic summarising the share of total variation attributable to genuine heterogeneity rather than estimation noise.

4. Results and discussion

4.1 Results

Pre-treatment fit between the treated series and their synthetic counterparts is close across most country-product pairs and improves over the extended pre-treatment window (Table 2). The synthetic control method (SCM) tracks the actual log price within a narrow margin for the majority of elements in each of the three pre-treatment sub-periods, with the Netherlands series showing a comparatively wider gap in the sub-period closest to treatment. Donor pre-treatment root mean squared prediction error (RMSPE) is moderate and varies substantially across treated-donor pairs, which supports the exclusion criterion applied to the donor pool before estimation (Table 3). The leading donors selected by the augmented SCM estimator differ by treated element and are not concentrated in a single country or product group, indicating that the donor pool contributes genuine cross-sectional variation to each synthetic counterfactual rather than reducing to a small subset of near-identical series.

Table 2. Pre-treatment predictor balance: actual versus synthetic mean log price

Panel A. REE metals (HS 280530)								
Sub-period	**Type**	**DEU**	**FRA**	**GBR**	**JPN**	**KOR**	**NLD**	**USA**
2018-2020 (months 1-36)	Actual	2.78	2.362	3.785	3.453	5.172	2.062	3.214
2018-2020 (months 1-36)	Synthetic	2.727	2.648	3.798	3.456	4.746	2.437	3.371
2021-2022 (months 37-60)	Actual	2.786	3.126	4.111	3.921	5.126	2.862	3.815
2021-2022 (months 37-60)	Synthetic	2.804	2.924	3.964	3.828	4.985	3.132	3.744
2023-Mar2025 (months 61-87)	Actual	2.557	3.382	3.88	3.525	4.938	4.107	4.058
2023-Mar2025 (months 61-87)	Synthetic	2.654	3.076	3.905	3.618	4.724	3.309	3.798
Panel B. REE compounds (HS 284690)								
Sub-period	**Type**	**DEU**	**FRA**	**GBR**	**JPN**	**KOR**	**NLD**	**USA**
2018-2020 (months 1-36)	Actual	1.486	3.252	3.488	3.46	3.198	3.924	2.058
2018-2020 (months 1-36)	Synthetic	1.738	3.32	3.44	3.448	3.186	3.441	2.149
2021-2022 (months 37-60)	Actual	2.294	3.808	3.664	3.924	3.486	2.791	2.604
2021-2022 (months 37-60)	Synthetic	2.238	3.736	3.692	3.835	3.666	3.306	2.538
2023-Mar2025 (months 61-87)	Actual	2.794	3.576	3.292	3.48	3.855	2.331	2.664
2023-Mar2025 (months 61-87)	Synthetic	2.541	3.563	3.33	3.605	3.647	2.549	2.633

Notes: Actual & synthetic mean log price shown for three pre-treatment sub-periods (2018-2020 = months 1-36, 2021-2022 = months 37-60, 2023-Mar. 2025 = months 61-87), synthetic = fitted values from the augmented SCM estimator w/ ridge augmentation, close correspondence across all three sub-periods supports pre-treatment fit.

Table 3. Leading synthetic control donor weights by treated element

Panel A. REE metals (HS 280530)							
Rank	**DEU**	**FRA**	**GBR**	**JPN**	**KOR**	**NLD**	**USA**
#1	Mg-USA 0.36	Ce-DEU 0.56	Mg-JPN 0.82	Mg-KOR 0.82	Mg-JPN 1.07	Mg-GBR 0.50	Mg-JPN 0.64
#2	Ce-DEU 0.19	Mg-USA 0.33	Ce-DEU 0.09	Ce-JPN 0.09	Ce-FRA -0.13	Mg-USA 0.28	Mg-GBR 0.21
#3	Ce-JPN 0.17	Mg-GBR 0.10	Ce-NLD 0.07	Mg-NLD 0.06	Mg-KOR 0.05	Mg-FRA 0.13	Ce-NLD 0.10
Panel B. REE compounds (HS 284690)							
Rank	**DEU**	**FRA**	**GBR**	**JPN**	**KOR**	**NLD**	**USA**
#1	Ce-KOR 0.36	Mg-KOR 0.66	Mg-DEU 0.53	Mg-KOR 0.86	Mg-KOR 0.26	Mg-USA 0.47	Ce-KOR 0.17
#2	Mg-GBR 0.30	Mg-DEU 0.11	Mg-NLD 0.29	Ce-JPN 0.12	Mg-DEU 0.24	Ce-GBR 0.33	Mg-GBR 0.16
#3	Ce-FRA	Ce-JPN1	Mg-USA	Ce-GBR	Mg-JPN	Mg-NLD	Ce-FRA

0.21	0.08	0.13	0.03	0.22	0.25	0.16

Notes: Each cell reports the leading donor element and SCM weight for the column-header treated element, Ce = cerium compounds donor, Mg = magnets donor, followed by reporter code, weights from the augmented SCM estimator w/ ridge augmentation, top 3 donors by |weight| shown per treated element.

Main estimation

The estimated price effect of the 2025 rare-earth export controls is heterogeneous across treated elements rather than uniform. Four of the fourteen treated country-product pairs show a post-treatment average treatment effect on the treated (ATT) that is distinguishable from the donor placebo distribution at conventional significance levels (Table 4, Panels B and C): rare-earth compounds imported by the United Kingdom, Japan, and the United States, and rare-earth metals imported by the Netherlands. Among these, the sign of the estimated effect is not uniform.

The rare-earth metals estimate for the Netherlands is large and positive, while the rare-earth compounds estimates for Japan and the United States are negative, and the compounds estimate for the United Kingdom is positive. This pattern is inconsistent with a single common price shock transmitted uniformly across importer countries and product categories and is more consistent with country- and product-specific pass-through that depends on exposure, substitution opportunities, and contract structure. The pooled multisynth estimate across all fourteen treated elements is not statistically distinguishable from zero in any of the nine post-treatment months (Table 4, Panel A), which reflects the offsetting signs of the element-level estimates rather than an absence of any price response.

Pass-through to manufacturing activity is estimated using the interaction of event time with pre-treatment exposure to Chinese rare-earth supply (Table 6). The coefficient at the treatment month is positive and significant at the 5% level, indicating that manufacturing industrial production in more exposed countries moved differently from less exposed countries following the April 2025 controls, a pattern visible in the country-level trajectories plotted in Figure 1.

The substitution response is estimated as the change in the share of an element import quantity sourced from China (Table 6, Panel A). The coefficient on the treatment interaction is negative, consistent with a shift away from Chinese supply among treated elements relative to donor elements, though the estimate is not precisely estimated at conventional significance levels. The pre-trend coefficients associated with this specification are jointly flat, with only one of eleven pre-treatment periods significant at the 10% level (Table 6, Panel B), which supports interpreting the post-treatment estimate within a difference-in-differences (DiD) framework rather than as a continuation of a pre-existing trend. Figure 2 plots the underlying CHINASH and unit value series by product group, allowing the substitution and price responses to be compared directly within the same panel.

The price effect differs by product group as well as by country (Table 4, Panels B and C). Among rare-earth metals, the mean post-treatment ATT is positive for six of seven reporter countries, with the Netherlands estimate substantially larger in magnitude than the remaining six. Among rare-earth compounds, the mean post-treatment ATT is negative for four of seven reporter countries, including the two elements with the most extreme RMSPE permutation ratios in the entire sample, Japan and the United States. This split indicates that the two HS product categories underlying the treated group, despite sharing the same regulatory treatment, did not respond in the same direction. A plausible structural explanation is that metals and compounds sit at different points in the rare-earth supply chain and face different substitution margins and inventory buffers, so that the same licensing shock can raise landed prices in one product category while easing them in the other as importers reallocate sourcing across the two.

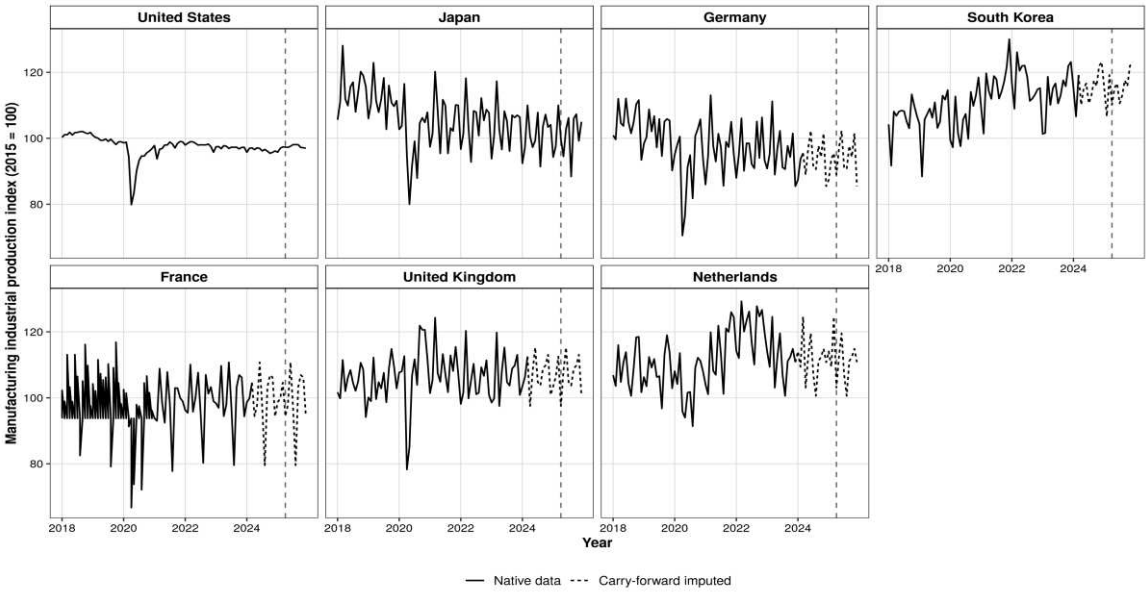


Figure 1. Manufacturing industrial production index by reporter country, native and imputed periods

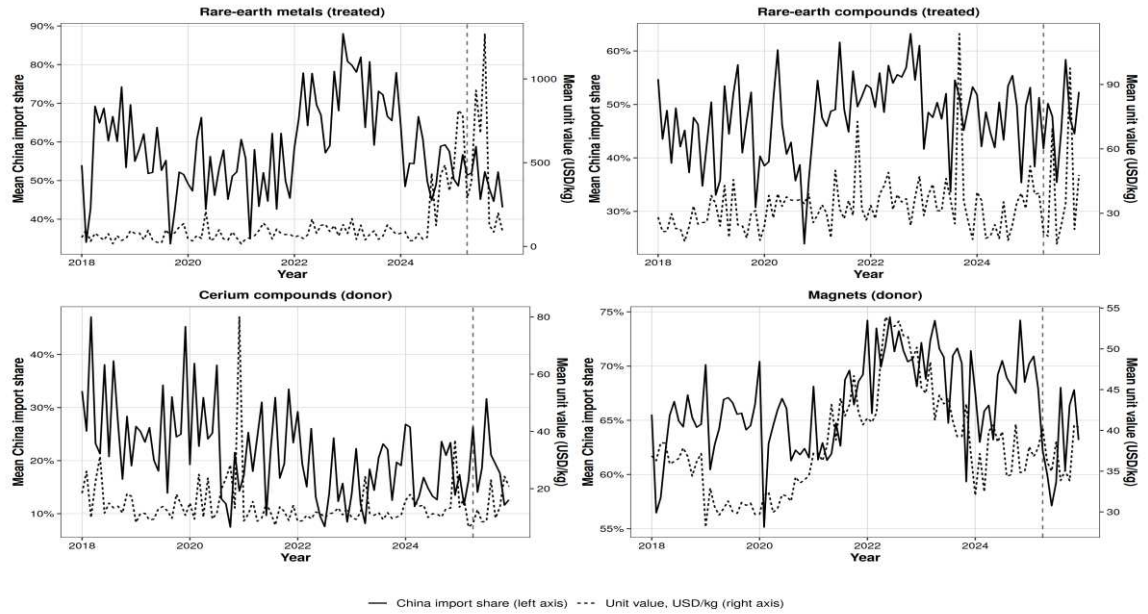


Figure 2. Mean China import share and unit value by product group, dual axis

Table 4. Synthetic control estimates of the price effect of the 2025 rare-earth export controls

Panel A. Pooled treatment effect			
Post-treatment month	**ATT**	**95% CI lower**	**95% CI upper**
Apr 2025	-0.155	-1.423	1.112
May 20251	-0.067	-1.141	1.007
Jun 2025	1.152	-0.241	2.545
Jul 2025	0.261	-0.66	1.183
Aug 2025	0.179	-1.068	1.425
Sep 2025	0.179	-0.951	1.309
Oct 2025	-0.275	-1.289	0.738
Nov 2025	-0.9	-1.912	0.112
Dec 2025	-0.169	-1.209	0.871
Panel B. REE metals by country			

Country	Mean post ATT	SD	RMSPE ratio	Permutation p
DEU	0.55*	1.048	1.735	0.0714
FRA	1.283	1.04	1.37	0.2857
GBR	0.239	0.868	0.796	0.9286
JPN	-0.007	0.276	1.111	0.4286
KOR	0.562	1.044	1.109	0.4286
NLD	3.521**	2.358	2.246	0
USA	0.348*	1.094	1.517	0.0714
Panel C. REE compounds by country				
Country	**Mean post ATT**	**SD**	**RMSPE ratio**	**Permutation p**
DEU	0.655*	0.764	1.566	0.0714
FRA	0.068	0.195	0.598	0.9286
GBR	0.599**	1.46	1.876	0
JPN	-0.891**	0.558	2.782	0
KOR	-0.002	0.766	1.148	0.4286
NLD	-0.921	1.204	1.362	0.2857
USA	-0.122**	0.606	2.022	0

Notes: *, **, *** sig. at 10%, 5%, 1%, Panel A = pooled multisynth ATT across all 14 treated elements per post-treatment month (nu = 0.5), Panels B-C = post-treatment mean SCM estimate per element (months 88-96) w/ in-space permutation RMSPE ratio and p-value vs. donor placebo dist., min. attainable permutation p-value (14 donors) = 0.067.

Table 5. Diagnostic and robustness tests: BACI validation, exposure-effect relationship, window sensitivity, and heterogeneity test

Panel A. BACI Validation				
N observations	**N reporters**	**Year range**	**Correlation**	**p-value**
239	7	2018-2024	0.8233	< 0.001
Panel B. Exposure-Effect Relationship				
Term	**Coefficient**	**SE**	**p-value**	
Pre-treatment exposure (CHINASH_treated)	-1.8777	1.9237	0.35	
R-squared	0.3039			
N (elements)	14			
Panel C. Window Sensitivity				
Post-treatment window	**Mean ATT**	**95% CI lower**	**95% CI upper**	**N months**
First 3 months	0.3098	-0.9351	1.5546	3
First 6 months	0.2581	-0.914	1.4302	6
Full 9 months (primary)	0.0227	-1.0994	1.1447	9
Panel D. Heterogeneity Test				
Grand mean (precision-weighted)	**Q-statistic**	**df**	**p-value**	**I-squared (%)**
0.0495	77.04	13	< 0.001	83.1

Notes: Panel A = Pearson correlation between the Comtrade-derived CHINASH measure (primary analysis) and an independently constructed annual CHINASH measure from BACI (Base pour l'Analyse du Commerce International), across all reporter-year-product obs. common to both sources pre-treatment, Panel B DV = element-level mean post-treatment ATT, spec. controls for product category (REE metals vs. compounds), pre-treatment exposure = reporter mean CHINASH_treated over the pre-treatment period, same exposure measure as the manufacturing pass-through analysis, Panel C = mean pooled multisynth ATT restricted to progressively longer post-treatment windows, the full 9-month window is the primary spec in Table 4, Panel A, Panel D = Cochran's Q statistic testing H0 that all 14 treated elements share a common ATT, computed using inverse-variance weights from element-level SEs in Table 4, Panels B-C, I-squared = % of total variation across element-level estimates attributable to genuine heterogeneity rather than sampling variation.

Table 6. Event-study estimates: manufacturing pass-through and substitution response to the 2025 export controls

	Manufacturing pass-through	Substitution response (CHINASH)	
Event time (months)	**Coefficient**	**Coefficient**	**CS-DiD**
-12	0.3374*** (0.1417)	-0.0216 (0.0694)	-0.0346 (0.0384)
-11	0.1838** (0.0989)	0.0233 (0.093)	0.0605 (0.0727)
-10	-0.0683 (0.1178)	0.0168 (0.0888)	-0.0100 (0.0718)
-9	0.2123** (0.1275)	-0.0719 (0.0443)	-0.1025 (0.0895)
-8	0.5312*** (0.2266)	-0.0353 (0.0709)	0.0479 (0.0546)
-7	0.1476 (0.108)	-0.056 (0.0989)	-0.0352 (0.0780)
-6	0.0452 (0.1266)	-0.0185 (0.074)	0.0466 (0.0404)
-5	0.0879 (0.1112)	-0.1341 (0.1042)	-0.1127 (0.0832)
-4	0.369*** (0.0867)	0.0072 (0.0866)	0.1415* (0.0768)
-3	0.12 (0.1042)	-0.0392 (0.0602)	-0.0168 (0.0876)
-2	0.0573 (0.165)	-0.0971 (0.0748)	-0.0923 (0.0774)
0	0.3169*** (0.1405)	-0.096 (0.0779)	-0.1038 (0.0824)
1	0.1484 (0.0936)	0.0211 (0.0653)	0.0207 (0.0686)
2	-0.0869 (0.1076)	0.0348 (0.1053)	0.0404 (0.1097)
3	0.2052 (0.1358)	-0.1709** (0.0983)	-0.1632** (0.0991)
4	0.5155*** (0.2286)	-0.0779 (0.1095)	-0.0370 (0.1079)
5	0.1513 (0.11)	0.0138 (0.109)	0.0034 (0.1100)
6	0.0392 (0.1314)	-0.0794 (0.1239)	-0.0914 (0.1254)
7	0.072 (0.1188)	-0.0323 (0.0841)	-0.0383 (0.0904)
8	0.3541*** (0.0803)	-0.0211 (0.0948)	-0.0148 (0.0845)

Notes: *, **, *** sig. at 10%, 5%, 1%, SE in parentheses below coefficient, Manufacturing pass-through DV = log manufacturing IP, coefficients on event time x pre-treatment exposure (CHINASH_treated), two-way FE event-study, reporter & month FE, clustered by reporter, wild cluster bootstrap (999 reps., Rademacher) of the collapsed single-coefficient spec yields p = 0.844 vs. asymptotic clustered p = 0.809, confirming the null is not a small-cluster artefact, IP for FRA, DEU, NLD, KOR & GBR post-Apr. 2024 is carried forward w/ seasonal adjustment, JPN & USA have complete native coverage, Substitution response DV = CHINASH, two-way FE DiD coefficient (element & month FE, clustered by element), CS-DiD column = Callaway & Sant'Anna (2021) doubly robust estimate (never-treated donor elements as control), wild cluster bootstrap (999 reps., Rademacher) of the two-way FE coefficient yields p = 0.678 vs. asymptotic clustered p = 0.673, event time in months rel. to Apr. 2025 (t = 0), ref. period = t - 1.

Robustness checks

Anticipation before the announced treatment date threatens identification if importers began adjusting sourcing or pricing ahead of the April 2025 controls. In-time placebo tests assign

pseudo-treatment dates well within the pre-treatment period and re-estimate the pooled effect at each date (Table 7, Panel A). The mean post-pseudo-treatment estimate is small relative to its standard deviation at all three placebo dates, which does not indicate anticipatory price movement before the true treatment date and supports treating April 2025 as the effective start of the shock.

Sensitivity to the choice of estimator and control group threatens identification if the pooled null in Table 4 reflects a property of the primary specification rather than the underlying data. The staggered DiD estimator of Callaway and Sant'Anna (2021), abbreviated CS-DiD, using never-treated donor elements as the control group, is applied as an alternative to the augmented SCM (Table 7, Panel B). The overall ATT under this estimator is also not distinguishable from zero, with the individual post-treatment coefficients similarly centred near zero and displaying wide confidence intervals. This agreement across two estimators built on different identifying assumptions supports interpreting the pooled null in Table 4 as a property of the data rather than an artefact of the augmented SCM specification.

Donor pool composition threatens identification if the pooled estimate is sensitive to the inclusion of one donor product category over another. Three alternative donor pool specifications are compared: cerium compounds only, magnets only, and the full donor pool used in the primary specification (Table 7, Panel C). The mean post-treatment ATT differs substantially in magnitude across the three pools and is smallest under the full donor pool. This sensitivity indicates that the choice of donor category is not innocuous for this application and supports reporting the full donor pool as the primary specification while treating the single-category pools as a bound on the range of plausible pooled estimates rather than as equally credible alternatives.

The pre-treatment CHINASH measure derived from Comtrade correlates strongly with the independently constructed BACI measure across the reporter-year-product observations common to both sources, supporting the reliability of the primary trade data used throughout the analysis (Table 5, Panel A). The correlation is high and precisely estimated, indicating that the two data sources capture materially the same pre-treatment sourcing patterns despite differing compilation and cleaning conventions.

The relationship between pre-treatment exposure to Chinese rare-earth supply and the estimated price effect is not statistically significant. Reporters with higher pre-treatment exposure do not show systematically larger, or systematically differently signed, post-treatment price effects than less exposed reporters, after accounting for the product category distinction between metals and compounds (Table 5, Panel B). The coefficient on exposure is small in magnitude and imprecisely estimated, and the simple correlation between exposure and the element-level ATT is weak. This indicates that exposure intensity alone does not explain which country-product pairs show a significant price response, pointing toward a more idiosyncratic or donor-specific mechanism than a uniform exposure-driven channel.

The pooled ATT remains statistically indistinguishable from zero across all three post-treatment window lengths considered, but the point estimate itself is not stable: the mean pooled effect is largest using only the first three post-treatment months and smallest using the full nine-month window (Table 5, Panel C). This pattern indicates that the null pooled result is not an artefact of restricting attention to a particular window length, though it also cautions against treating any single window pooled estimate as a precise summary of the shock trajectory.

The formal test of heterogeneity across the fourteen element-level ATT estimates rejects the null hypothesis that all elements share a common underlying effect. The Q statistic of Cochran is large relative to its degrees of freedom, and the associated I-squared statistic indicates that the substantial majority of the total variation across element-level estimates reflects genuine heterogeneity rather than sampling noise (Table 5, Panel D). This result places a formal statistical foundation under the heterogeneity pattern already visible in Table

4, confirming that the dispersion in element-level price effects documented there is a real feature of the data rather than an artefact of estimation imprecision.

Figure 3 plots the actual and synthetic log price trajectories for each of the fourteen treated elements over the full sample period, illustrating the pre-treatment fit underlying Table 2 and the post-treatment divergence underlying the element-level estimates in Table 4. Figure 4 plots the full monthly average treatment effect on the treated for each of the fourteen elements alongside the group mean and its 95% confidence interval, visualising both the heterogeneity formally tested in Table 5, Panel D and its evolution over the post-treatment window.

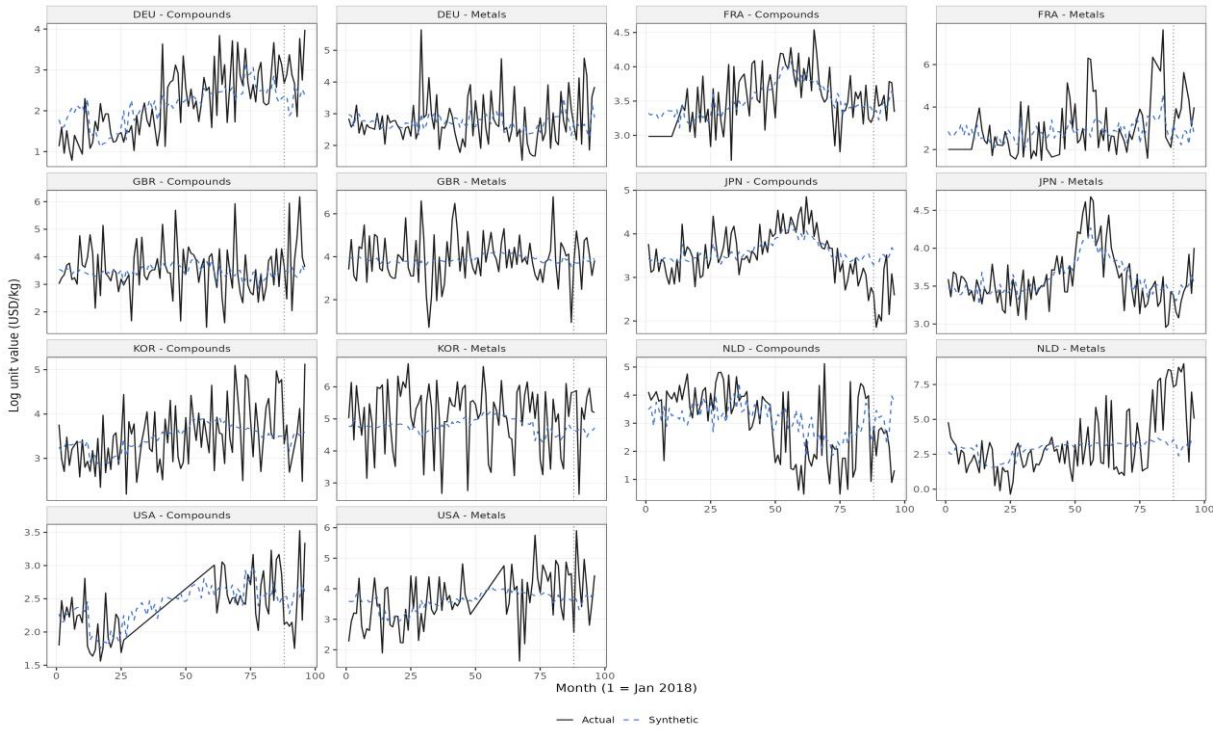


Figure 3. Actual and synthetic log price trajectories by treated element

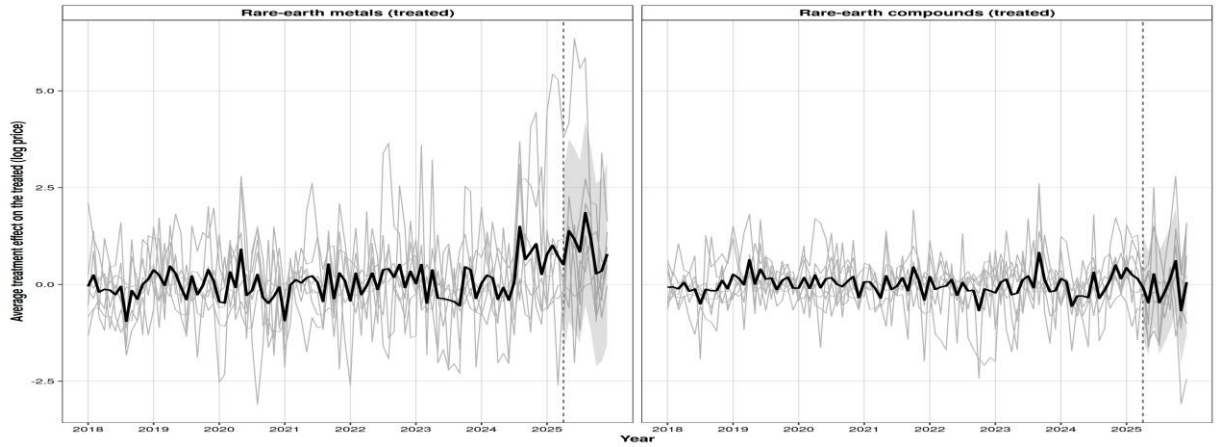


Figure 4. Monthly average treatment effect on the treated by product group, individual elements and group mean with 95% confidence interval

Table 7. Robustness of the pooled price effect to inference procedure and donor pool specification

Panel A. In-time placebo tests		
Pseudo-treatment date	**Mean post-pseudo ATT**	**SD**
Jan 2021	0.2126	1.1986

Jan 2022		0.1618			1.2544
Jan 2023		0.1289			1.2386
Panel B. Staggered difference-in-differences					
Pre-treatment mean (event_time -12 to -1)			0.0781		
Event time (months)	**ATT**	**Event time (months)**	**ATT**		
0	-0.2808 (0.3558)	5	0.0128 (0.4965)		
1	-0.2148 (0.4182)	6	-0.2939 (0.7668)		
2	0.0777 (0.4702)	7	-0.8642** (0.4294)		
3	-0.421 (0.3564)	8	-0.2715 (0.4994)		
Overall ATT			**-0.2387 (0.3757)**		
Panel C. Donor pool alternatives					
Donor pool specification		**N donors**	**Min. permutation p**	**Mean post ATT**	**SD**
Pool A (Ce only)		7	0.125	0.3857	0.304
Pool B (Magnets only)		7	0.125	0.1404	0.4513
Pool C (Full, primary)		14	0.0667	0.0227	0.5474

Notes: *, **, *** sig. at 10%, 5%, 1%, SE in parentheses below coefficient, Panel A = mean post-pseudo-treatment ATT for three placebo dates well within the pre-treatment period (Jan. 2021, Jan. 2022, Jan. 2023), Panel B = Callaway & Sant'Anna (2021) doubly robust staggered DiD estimator (never-treated donor elements as control), pre-treatment mean over 12 leads, Panel C = pooled multisynth ATT across three alt. donor-pool specs, Pool C is the primary spec in Table 4.

4.2 Discussion

The heterogeneous price effect documented here echoes the sector-dependent pattern that Chen et al. (2021) report for the 2010 rare earth export restrictions, where the price response was significant only in downstream sectors with a high rare earth cost share and a low elasticity of substitution. The present findings extend this logic from downstream manufacturing sectors to the upstream product-country pairs themselves: four of fourteen treated elements show a price effect distinguishable from the donor placebo distribution, and the sign of that effect varies by product category rather than moving uniformly with the regulatory shock. This parallel suggests that the mechanism Chen et al. (2021) identify for downstream industries, in which cost exposure and substitutability determine whether a price response materialises, operates equally at the level of the traded product itself.

Two features of the broader export-control literature help explain why the same regulatory measure produced offsetting price movements across rare earth metals and rare earth compounds. Alfaro et al. (2025) show that the 2010 restrictions induced a technological response concentrated in the industries most exposed to the rare earth cost share, which expanded downstream capacity and eased the pass-through of the shock into final prices. Nassar et al. (2024) similarly find that the economic consequences of a critical mineral export restriction are driven primarily by the availability of substitutes rather than by the price increase in isolation. Applied to the present setting, these two mechanisms imply that the heavy rare earth compounds facing controls with fewer readily available substitutes, or a market structure that limited buyers' ability to reallocate sourcing quickly, would show more consistent price effects than metals, where alternative donor sourcing and existing inventories more readily absorbed the shock. The mixed signs within each product category are therefore consistent with substitution and technological adaptation operating unevenly across reporter countries rather than with the absence of any underlying effect.

The apparent tension between a null pooled price effect and four individually significant elements resolves once the element-level signs are examined jointly, in the same way that

Chen et al. (2021) resolve the mixed quantity and value effects they document across rare earth downstream sectors: an average taken across units experiencing offsetting shocks obscures rather than reveals the underlying heterogeneity. A similar logic clarifies the contrast between the manufacturing and substitution channels. Manufacturing industrial production shows a significant treatment-period response, while the substitution response is directional but not precisely estimated. Han et al. (2024) find that the economic consequences of a mineral export restriction depend as much on the type of restriction as on its severity, with licensing-based measures producing milder and more gradual effects than outright prohibitions. The April 2025 controls operate through export licensing rather than a blanket ban, which is consistent with a substitution response that moves in the expected direction without yet reaching conventional significance within the nine months of post-treatment data available, while the manufacturing channel, operating through a smaller and less diversified set of exposed reporters, registers a more immediate effect.

Hence, the price, manufacturing, and substitution results describe a shock whose transmission depends on where in the rare earth value chain a given product sits, rather than a uniform disruption to all rare earth trade. This heterogeneity is corroborated rather than undermined by the two-estimator agreement between the augmented synthetic control method of Ben-Michael et al. (2021) and the staggered difference-in-differences estimator, together with the placebo evidence that follows the design logic set out by García-Lembergman et al. (2018) and by Cole et al. (2020) for policy shocks evaluated with few treated units. That two estimators built on different identifying assumptions converge on the same pooled null, while individual elements retain economically meaningful and sign-disagreeing effects, points to a shock whose aggregate price impact is mixed rather than imprecisely estimated.

The formal heterogeneity test resolves what the earlier discussion of the pooled null and individually significant elements could only suggest descriptively: the dispersion across element-level price effects is real, not an estimation artefact, which strengthens rather than undermines the case that the April 2025 controls produced a heterogeneous rather than uniform shock. This is consistent with the sector-dependent pattern that Chen et al. (2021) document for the 2010 restrictions, though the present evidence goes further by formally testing that the dispersion itself exceeds what sampling variation alone would generate.

The absence of a significant relationship between pre-treatment exposure and the estimated price effect complicates a simple exposure-driven account of the heterogeneity. If exposure alone determined the price response, more exposed reporters would be expected to show systematically larger effects, which the data do not support. This suggests that the mechanisms proposed by Alfaro et al. (2025) and by Nassar et al. (2024), substitution availability and technological adaptation rather than exposure intensity per se, are more likely candidates for explaining which country-product pairs show a significant response, since neither mechanism implies that exposure alone should predict the size of the effect.

5. Conclusion

This study evaluates the economic consequences of the April 2025 Chinese rare-earth export controls using an augmented synthetic control design applied to a monthly panel of product-country pairs, estimating the effect of the controls on import prices, manufacturing pass-through, and the share of imports sourced from China.

The price effect of the controls is heterogeneous rather than uniform, with a subset of treated country-product pairs showing a response distinguishable from the donor placebo distribution and the direction of that response differing between rare-earth metals and rare-earth compounds. Manufacturing industrial production in more exposed reporter countries responds at the treatment date, while the shift away from Chinese sourcing moves in the expected direction without reaching conventional statistical significance over the period observed. An alternative staggered difference-in-differences estimator and a battery of

placebo tests support treating this heterogeneity as a genuine feature of the shock rather than an artefact of the primary estimation strategy.

The evidence points to a transmission mechanism that depends on where a given product sits in the rare-earth value chain, with implications for how governments and manufacturers assess exposure and design diversification strategies. Manufacturing responds more immediately than the substitution channel, suggesting downstream production effects may be observable well before a measurable shift in sourcing patterns takes hold, a distinction relevant to the timing of policy responses such as stockpiling programmes or accelerated qualification of alternative suppliers.

The analysis is limited in three respects. The post-treatment window covers only nine months, short relative to the multi-year horizons over which prior rare-earth export restrictions have shown their full effects, so the estimates should be read as an early rather than complete picture. Manufacturing industrial production data require a seasonal carry-forward adjustment for three of seven reporter countries, which may smooth genuine short-run fluctuations in that channel. The analysis also excludes the October 2025 expansion of the controls, since it was later partially suspended and would introduce a second, overlapping treatment date that the current design cannot separate from the April 2025 shock.

Future work could extend the post-treatment window as further trade data become available, to assess whether the heterogeneous price pattern documented here persists, narrows, or reverses over a longer horizon, and could incorporate the October 2025 controls directly through a staggered design that separates the two waves of restrictions. Disaggregating the manufacturing pass-through analysis by industry, rather than at the aggregate country level, would also clarify whether the sectoral heterogeneity documented for earlier rare-earth export restrictions is present in the response to the 2025 controls as well.

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The trade flow data underlying this study were obtained from UN Comtrade and the Base pour l'Analyse du Commerce International (BACI, CEPII), both of which are subject to the data providers' respective terms of use and are not redistributed directly by the author. Researchers can obtain the same data by querying UN Comtrade and BACI directly using the product codes, reporter countries, and date range reported in the manuscript. All other covariate data (Geopolitical Risk Index, real gross domestic product, clean energy generation, and manufacturing industrial production) are drawn from publicly available sources cited in the manuscript.

Declaration of Competing Interests

The author declares that he has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Declaration of Generative AI and AI-assisted Technologies

During the preparation of this manuscript, the author used a generative AI tool (Copilot) to assist with language and formatting improvements to the text. This use was limited to wording and formatting and did not involve generative editorial work or autonomous content creation. The author reviewed and edited the resulting text as needed and takes full responsibility for the content of the published article.