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Can Cui

cc2121@cam.ac.uk

University of Cambridge

Dequn Teng

University of Cambridge, Institute for Manufacturing https://orcid.org/0000-0003-0667-8712

Steve Evans

Industrial Sustainability, Institute for Manufacturing, University of Cambridge https://orcid.org/0000-0003-1757-6842

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Unintended shift in supply bases away from well-governed jurisdictions after critical mineral export restrictions

Dequn Teng[1,2], Can Cui[1,*], and Steve Evans[1]

[1]Department of Engineering, University of Cambridge, Cambridge, UK
[2]Department of Economics, University of Sheffield, Sheffield, UK

*Corresponding author: Can Cui (cc2121@cam.ac.uk)

Abstract

Producing countries increasingly restrict exports of minerals the energy transition needs, and de-risking policies aim to move companies that rely on these minerals towards suppliers in secure, well-governed jurisdictions. How these companies' supply bases respond has received little study. Here we combine 142,150 annual reports, used to infer which US-listed companies use which minerals, with their disclosed supplier relationships and six export restrictions by China and Indonesia between 2010 and 2024. Relative to companies whose reports never name these minerals, the average rule of law in the headquarters countries of exposed companies' suppliers fell by 0.064 points on the World Bank index after a restriction (95% confidence interval 0.024 to 0.103), with no detectable difference in trends beforehand. The decline reflected a rising share of suppliers headquartered in China. It rested mainly on Indonesia's nickel-ore bans, on repeatedly exposed companies, on companies with few disclosed suppliers and partly on newly recorded suppliers, was only borderline significant with the restrictions as the units of inference, and was not clearly distinct from a smaller drift among companies using only unrestricted base metals. On average, exposed companies' supply bases moved away from, not towards, the well-governed sources that de-risking policies intended to favour.

Introduction

In August 2023, China began requiring export licences for gallium and germanium, two by-product metals used in power electronics and infrared optics; within three months its declared exports of both had fallen to almost nothing (Fig. 1c). In September 2024 similar controls took effect for antimony, and within a year China's antimony exports had fallen to 3% of their previous level. A decade earlier, Indonesia had banned the export of unprocessed nickel ore: China's imports of Indonesian ore fell from 41 million tonnes in 2013 to less than 0.2 million tonnes in 2015, recovered to 24 million tonnes in 2019 while the ban was relaxed, and have stayed below 1.1 million tonnes a year since 2021, after

it was reinstated in January 2020 (Fig. 1d). These are not isolated episodes. The International Energy Agency reports that more than half of a broader group of energy-related minerals are now subject to some form of export control, that the average market share of the top three refining countries for copper, lithium, nickel, cobalt, graphite and rare earths rose from about 82% in 2020 to 86% in 2024, and that China is the dominant refiner of 19 of the 20 strategic minerals it analyses[1].

Governments have asked the companies that use these minerals to de-risk. Until it was terminated for vehicles acquired after 30 September 2025, the US clean-vehicle tax credit was tied to the share of battery minerals extracted or processed in the United States or its free-trade partners[2-4]; the European Union's Critical Raw Materials Act sets the benchmark that no more than 65% of the Union's consumption of any strategic raw material, at any stage of processing, should come from a single third country[5]. Both aim to steer companies facing restricted supply towards diversified, secure sources, which these rules locate mainly in allied, well-governed jurisdictions; how companies respond to restrictions on their own is rarely observed. Recent analyses have quantified what is at stake, from the disruption exposure of battery chemistries[6] and the carbon footprint of sourcing choices[7] to the mineral constraints on energy-transition pathways[8-10]; in these analyses, the response of companies is usually assumed rather than observed. Research on supply-chain disruptions shows that shocks propagate through production networks[11-13] and that companies reshape their supplier composition when supply risk rises[14,15]. The most direct evidence on China's recent controls comes from import transactions: after the 2023 licensing of gallium, germanium and graphite, US importers of these goods shifted towards European suppliers[16]. How the supplier relationships of the companies that use restricted minerals respond has received little direct study.

What these companies do is not obvious, and two lines of reasoning point in opposite directions. On a de-risking view, companies move away from constrained sources. Resource-dependence theory predicts that organizations reduce their reliance on constraining exchange partners by diversifying, contracting or integrating[17,18]; the supply-chain resilience literature recommends redundancy and flexible sourcing[19,20]; and the criticality assessments that inform mineral policy count the governance of supplying countries as a component of supply risk[21,22]. Together, these arguments imply that companies move away from the restricting producer and, by the logic of criticality assessments, towards better-governed jurisdictions; induced-innovation arguments add input-saving technical change[23-25]. On a processing-hub view, the opposite can happen. Mining can move to new deposits, but refining, the midstream of mineral supply chains, is more concentrated than mining and is expected to diversify only slowly[1]. When a restriction cuts off a raw material, the most readily available alternative for a company that cannot refine it may therefore be a processed intermediate, or a component made from it, bought from companies that control the hub's midstream capacity; Indonesia's nickel-ore bans, for example, were followed by a build-up of refining in Indonesia, most of it controlled by Chinese companies or shareholders[26]. States with jurisdiction over the central nodes of global economic networks can turn such positions into leverage[27]; after the US tariffs of 2018 and 2019, for example, US imports moved away from China while supply chains remained intertwined with it[28]. Because companies are held responsible for their suppliers' conduct[29] and the hardest suppliers to monitor are those furthest from them[30], which of these responses prevails also matters for the sustainability of mineral supply chains[31,32]. Whether export restrictions move companies' supply bases towards or away from well-governed jurisdictions therefore remains an open empirical question.

Here we ask how export restrictions on critical minerals change the supply bases of the companies that depend on them, and we measure the two constructs this question involves with company-level data. Dependence on a restricted mineral is inferred from 142,150 annual reports filed by US-listed companies between 2005 and 2026: a company is exposed to a restriction when, in the years before it, its reports repeatedly name the mineral together with words describing an input, a supplier, a price or a shortage. We check this measure against supplier links to producers of the mineral, conflict-minerals filings and a sentence-level audit. The institutional quality of a company's supply base is measured as the average World Bank rule-of-law estimate of the countries where its disclosed suppliers in FactSet Revere are headquartered, an index of the contract enforcement, property rights and courts on which supplier due diligence relies. We date nine export restrictions imposed between 2010 and 2025 by China, Indonesia and the Democratic Republic of the Congo, six of which can be followed in the supplier data, and use customs and price data to show which of them visibly reduced supply. Because the timing and the mineral of each restriction were set by foreign governments, we compare exposed companies with comparison companies in the same years, before and after each event, in a stacked difference-in-differences design with company-by-event and year-by-event fixed effects, and we probe the comparability of the two groups with pre-restriction trends, placebo dates, reweighting and alternative comparison groups rather than assume it.

We find that after a restriction the suppliers of exposed companies were, on average, headquartered in jurisdictions with weaker rule of law, and that this reflected more suppliers in China, the country whose dominance de-risking policies aim to reduce. The decline holds across alternative comparison groups, outcome definitions and estimators, but it rests mainly on Indonesia's nickel-ore bans, on companies exposed more than once, on companies with few disclosed suppliers and partly on suppliers that entered the data after a restriction; it is not statistically distinct from a smaller drift among companies that use only unrestricted base metals, and it is only borderline significant when the six restrictions are treated as the units of inference. Conditions under which it is larger or smaller, and a rise in disclosed searches for alternative suppliers, are reported as exploratory. Supply security itself is not measured; the evidence indicates that restriction-related changes in companies' supply bases did not, on average, run towards the well-governed sources that policy expects.

Results

Nine export restrictions and the companies exposed to them

We study every export ban, quota or licensing requirement on a critical mineral imposed between 2010 and 2025 by the country that was then the world's largest producer of that mineral and whose date of effect can be established from official texts (Fig. 1a, Supplementary Table S8 and Supplementary Note 4): China's 2010 cut in rare-earth export quotas; Indonesia's 2014 ban on exports of unprocessed nickel ore and its reinstatement in 2020; China's 2023 licensing of gallium, germanium and graphite; its 2024 licensing of antimony; its 2025 controls on tungsten, tellurium, bismuth, molybdenum and indium and on seven medium and heavy rare earths; and the Democratic Republic of the Congo's 2025 suspension of cobalt exports. Five of the six measures whose aftermath we can follow visibly reduced what the producer shipped or widened its export price premium. After the 2010 quota cut, the gap between the export price of rare earths and the price paid inside China widened by an average

of 0.35 log points between 2011 and 2013 relative to its 2005 to 2009 level, while the same gap for gallium, germanium, indium and antimony, which were not newly restricted at the time, widened by 0.06; the rare-earth gap disappeared once China lifted the quotas in 2015 to comply with a World Trade Organization ruling (Fig. 1b). China's declared exports of gallium and germanium fell to 3% of their previous level within three months of the 2023 licensing rule before partly recovering, and antimony exports fell to 3% within a year of the 2024 rule; graphite exports, by contrast, did not fall visibly (Fig. 1c). China's imports of Indonesian nickel ore followed Indonesia's policy on, off and on again (Fig. 1d).

We classify a company as exposed to an event if, in at least two of its annual reports from the five years before the event, a sentence names the restricted mineral together with a word describing an input, a supplier, a price or a shortage. Across the nine events, 375 US-listed companies (541 company-events) meet this definition (Fig. 1e); the comparison group in each event consists of the 3,555 to 4,313 companies whose reports in the same years never name any of the restricted minerals, a group that may include some users that never write about them. Three checks, detailed in Methods, indicate that the measure records input dependence: in 11 of 15 comparisons, exposed companies were 6 to 20 times more likely than other companies to have a disclosed supplier that produces the mineral, they filed conflict-minerals reports more often, and in an audit 75% of exposed company-events had at least one flagged sentence describing input use (Supplementary Tables S9 and S10).

Exposed companies' suppliers moved towards weaker rule of law

We measure the governance of a company's supply base as the average rule-of-law estimate[33] of the countries where the suppliers it is linked to at the end of each year are headquartered. Supplier relationships can be followed after six of the nine restrictions, covering 237 exposed company-events of 173 companies among 5,199 companies. Before the restrictions, the suppliers of exposed and comparison companies were headquartered in similarly well-governed jurisdictions (average rule of law of 1.24 and 1.32; for reference, the 2010 to 2023 averages are 1.51 for the United States and -0.29 for China), and we detected no difference in their pre-restriction paths (Fig. 2a,b; joint test of the three pre-restriction coefficients, $\chi^2(3) = 3.75$, $P = 0.29$), although these pre-restriction estimates are imprecise (Supplementary Fig. S2).

After a restriction, the suppliers of exposed companies were headquartered in jurisdictions with weaker rule of law. With company-by-event and year-by-event fixed effects and lagged controls, the supply-base rule of law of exposed companies fell by 0.064 points relative to comparison companies (95% confidence interval (CI) -0.103 to -0.024; $t(5{,}198) = -3.15$; $P = 0.002$; Table 1). This is 0.16 standard deviations of its pre-restriction distribution and 5.2% of its pre-restriction level, about what replacing one supplier in 28 headquartered in the United States with one headquartered in China would produce. In the three events observed in every year from four years before to two years after the restriction, the estimates one and two years after it were -0.103 and -0.105 points (open markers in Fig. 2b); those for years 3 and 4 rest on two events only.

The estimate is of similar size with industry-by-year-by-event fixed effects, without the year before each restriction (in which the 2020 nickel ban was announced), with a looser exposure threshold, with errors clustered by industry and with control of corruption in place of rule of law (Fig. 3 and

Supplementary Tables S5, S11 and S13). Reweighting comparison companies to match exposed companies' pre-event characteristics gives -0.079 ($P < 0.001$), against -0.064 without weights in the same sample and -0.054 when only the controls are balanced. In the exploration and confirmation halves of the sample, defined before any outcome was examined, the estimate is -0.047 ($P = 0.072$) and -0.083 ($P = 0.008$), and permuting exposure among companies in the same industry and event gives a randomization-inference P value of 0.010 (Supplementary Fig. S1). Of the six events, five point in the same direction, and two are individually significant: China's 2010 rare-earth quotas (-0.139; $P = 0.012$) and Indonesia's 2020 nickel-ore ban (-0.095; $P = 0.022$; Fig. 2c).

The decline reflects more suppliers headquartered in China

The change in rule of law appears to reflect where suppliers were based rather than changes in country scores or in the number of suppliers. Holding each country's score at its 2005 to 2023 mean, so that the measure moves only when suppliers change, gives a larger decline (-0.090; 95% CI -0.133 to -0.047; $P < 0.001$). Exposed companies were not detectably less likely to have any disclosed supplier after a restriction (-0.002; $P = 0.92$), and their number of disclosed suppliers did not change detectably (log points 0.057; 95% CI -0.034 to 0.148; $P = 0.22$), so we find no evidence that the result arises from companies entering or leaving the supplier data. The share of exposed companies' suppliers headquartered in China, including Hong Kong, rose by 2.9 percentage points (95% CI 1.3 to 4.5), from 5.8% before the restrictions; with industry-by-year-by-event fixed effects the increase is 1.5 points ($P = 0.086$). Excluding suppliers in China and Hong Kong, supply-base rule of law did not change detectably (-0.019; $P = 0.31$; Supplementary Table S11), so the decline operates through the China share.

The suppliers that companies added and dropped show the same shift (Fig. 2d). As a share of each year's additions, suppliers in China rose by 3.5 percentage points from 8.3% ($P < 0.001$; Supplementary Table S14). In counts, exposed companies added 42% more suppliers headquartered in China per year after a restriction (15 to 76%; $P < 0.001$), against 7% ($P = 0.61$) for other jurisdictions with below-average rule of law and 26% ($P = 0.086$) for jurisdictions above it, and they also dropped more Chinese suppliers (9%; $P = 0.003$; Supplementary Tables S12 and S21), so the rise in the China share came with turnover in both directions. Additions of Chinese suppliers rose more steeply outside the materials value chain (58%; $P = 0.003$) than inside it (22%; $P = 0.070$); by stage of the value chain, they rose detectably for makers of components (31%; $P = 0.023$) and of other goods and services (53%; $P = 0.001$) but not for refiners and processors, whose estimate is imprecise (12%; $P = 0.54$; Supplementary Note 7). In the sourcing categories of US industrial policy, the share of suppliers in "foreign entities of concern" rose by 2.8 percentage points ($P < 0.001$) and the share in the United States, its free-trade partners and Japan fell by 2.8 points ($P = 0.050$).

Who records a relationship matters for how this shift is observed. The decline appears in relationships disclosed by suppliers or third parties (-0.060; $P = 0.006$) and not in those disclosed by the exposed companies themselves (0.014; $P = 0.63$), and relationships with Chinese suppliers are recorded mostly from the supplier's side: only 20% of those held by US-listed companies are disclosed by the customer, against 40% for suppliers in the United States (Supplementary Table S15). Part of the decline comes from suppliers that entered the data after the restriction: with only suppliers FactSet already covered, the estimate is -0.038 ($P = 0.055$), and with only those already recorded as suppliers,

-0.024 ($P = 0.23$; Supplementary Note 8). The data cannot tell whether these entrants reflect new relationships, existing ones that became large enough to be reported, or additions to the data's coverage; the estimate is similar, however, when pre-event company size is interacted with year-by-event fixed effects (-0.057; $P = 0.005$), which absorbs coverage trends that differ by company size. The rule of law of the countries where exposed companies' own customers are headquartered, which a restriction on inputs should not move, did not change detectably (0.022; $P = 0.21$).

How specific and how robust the decline is

Figure 3 and Supplementary Tables S11, S12 and S13 report the checks on whether the decline is specific to the restrictions and how much of it survives alternative comparisons. They fall into four groups; the first two limit the result.

First, companies whose reports name only unrestricted base metals (copper, aluminium or zinc) as inputs also drifted towards weaker rule of law relative to companies naming no metal (-0.023; $P = 0.029$), by about a third as much as exposed companies (-0.066). The difference between the two groups is -0.043 (95% CI -0.087 to 0.001; $P = 0.054$), so the decline is larger than, but not clearly distinct from, the drift among metal users (Fig. 3a); with base-metal users as the only comparison group the estimate is -0.052 ($P = 0.022$). The drift is concentrated among aluminium users, for reasons we cannot identify (candidates include Indonesia's bans on bauxite exports in 2014 and 2023 and the US tariffs on aluminium imports from 2018): copper and zinc users show no detectable drift (0.004; $P = 0.85$), and against them, in a split made after seeing this pattern, the difference is -0.069 (-0.123 to -0.015; $P = 0.012$).

Second, the decline rests mainly on Indonesia's nickel-ore bans, on companies exposed more than once and on companies with few disclosed suppliers. Pooled over the two nickel bans it is -0.071 ($P = 0.005$) and over China's restrictions -0.049 ($P = 0.095$). Leaving out any single event keeps it between -0.044 (without Indonesia's 2020 ban; $P = 0.046$) and -0.074. Removing the 68 company-events of companies that had already been exposed in an earlier event, most of them nickel users exposed again by Indonesia's 2020 ban, halves the estimate (-0.032; 95% CI -0.082 to 0.018; $P = 0.21$). Within the 2020 ban the decline is concentrated among these repeatedly exposed companies (-0.133; $P = 0.002$; 52 company-events) and is not detected among first-time exposed ones (0.022; $P = 0.83$; 30). This is consistent either with a cumulative response to repeated restrictions or with characteristics of these companies that we do not observe. The estimate is also carried by companies with few disclosed suppliers, whose average rule of law moves most when a single supplier changes: among companies with at least five disclosed suppliers before the event it is -0.026 (95% CI -0.064 to 0.013; $P = 0.19$; 95 exposed company-events), and weighting each company-event by its pre-event number of suppliers gives -0.024 (-0.062 to 0.014; $P = 0.21$). Treating each of the six events as one observation, a random-effects meta-analysis gives an average of -0.055 (95% CI -0.115 to 0.006; $t(5) = -2.32$; $P = 0.068$), and methods designed for few clusters, with the events as clusters, give P values between 0.039 and 0.094 (wild cluster bootstrap, 95% CI -0.142 to 0.014; $P = 0.092$; Supplementary Note 5): the estimate is similar in size but only borderline significant.

Third, the decline does not appear to be explained by the rival shocks we can measure or by the estimator. It is of similar size without fiscal 2020 and 2021, without conflict-minerals filers in the 2010

stack, among manufacturing companies only, with pre-event China revenue share or China supplier share and supply-base size interacted with year-by-event fixed effects, and with the corrective weights for stacked designs of ref. 34 (Fig. 3c); moving each event three years earlier gives no detectable change (0.020; $P = 0.46$). When China lifted its rare-earth quotas in 2015, the supply bases of the companies exposed in 2010 moved back towards stronger rule of law (0.131; 95% CI 0.020 to 0.241; $P = 0.020$), as expected if the quotas had caused the shift; when Indonesia relaxed its nickel-ore ban in 2017, those of nickel users did not move back detectably (-0.003; $P = 0.91$), which is uninformative on its own because supplier relationships, once formed, need not be undone when a restriction eases. Allowing for a linear pre-existing trend common to exposed companies, extrapolated from the three pre-restriction coefficients, gives an average post-restriction effect of -0.094 (95% CI -0.194 to 0.006; $P = 0.066$; Supplementary Table S16): no smaller, but too imprecise to exclude zero, because the slope of those coefficients is itself imprecise (-0.0187 to 0.0283 points per year; Supplementary Fig. S2). Nor does the decline appear to reflect a shift of supply chains towards China that is common to all companies, or a linear trend specific to each exposed company. Such a shift is visible among comparison companies, whose suppliers' China share rose from 1.5% in 2005 to 7.6% in 2024, but the year-by-event fixed effects remove it to the extent that it is shared, and the decline persists with company-by-event linear trends (-0.055; $P = 0.040$) and in the events after FactSet began covering Asian companies in 2013 (-0.065; $P = 0.035$; Supplementary Note 8).

Fourth, misclassified exposure is unlikely to drive the result. Removing the exposed company-events whose flagged sentences did not describe input use in the audit gives a larger estimate, -0.078 ($P < 0.001$; 182 exposed company-events), as expected if misclassified exposure dilutes the effect. We find no credible evidence that the decline grows with the number of pre-event reports naming the mineral as an input (0.001 per additional report; 95% CI -0.030 to 0.032; $P = 0.94$).

Where the decline was larger, and other responses (exploratory)

We also examined, as exploratory analyses, whether the decline varied with 52 pre-event characteristics, estimating each interaction in the exploration and confirmation halves of the sample and in the full sample (Methods; every estimate is in Supplementary Table S19). Two interactions pass false-discovery-rate control, with untransformed gross margin and with the number of pre-event reports naming the mineral together with China, but neither withstands checks of its precision (Supplementary Table S20). Two characteristics with a theoretical rationale do not survive adjustment either (Fig. 4 and Table 2). Each standard deviation of how often a company formed new supplier relationships before the event went with an interaction of -0.067 points (95% CI -0.128 to -0.006; $P = 0.031$; $q = 0.32$), estimated with precision in only one half of the sample; each standard deviation of a company's gross-margin rank within its industry and year, a proxy for its ability to pass higher input costs on to customers, went with an interaction of 0.047 points (95% CI -0.003 to 0.097; $P = 0.063$; $q = 0.36$). The decline was largest in the middle tercile of supply-base size (-0.132; $P = 0.005$), with smaller and imprecise estimates for companies with the fewest and the most disclosed suppliers (Fig. 4c), although equality of the three effects is not rejected ($F(2, 5{,}047) = 2.54$; $P = 0.079$). These patterns indicate where the decline may be larger, not why, and need testing in independent data.

Among the 84 outcomes we examined, 12 pass false-discovery-rate control; most are the supply-base measures reported above (Supplementary Table S19). Two others concern de-risking directly but

do not hold up as responses to the restrictions (Supplementary Table S21): raw-material inventories of exposed companies rose (13%; $P < 0.001$) but were already rising beforehand (pre-restriction coefficients, $P < 0.001$), and patenting fell (15%; $P < 0.001$), but both changes lose significance with industry-by-year-by-event fixed effects ($P = 0.27$ and $P = 0.14$).

Disclosed searches for alternative suppliers rose, but not distinctly from their earlier trend

We measured disclosed supplier search as the rate of annual-report sentences in which supply-chain words appear with words for qualifying, adding or diversifying to alternative, second or new suppliers. After a restriction, exposed companies wrote more about supplier search (0.058 sentences per 1,000; 95% CI 0.022 to 0.094; $P = 0.002$; 21.5% of the pre-event mean among exposed companies; Table 2), and companies that wrote more about it had supply bases with weaker rule of law, conditional on exposure and the fixed effects (-0.042 per sentence per 1,000; $P = 0.001$). But the rise continued a trend that was already under way: in the companies with supplier data, the coefficients for the years before the restriction are jointly borderline ($\chi^2(3) = 7.61$, $P = 0.055$), and net of the linear trend through them the average post-restriction rise is 0.009 (95% CI -0.064 to 0.083; $P = 0.80$; Fig. 5a). The same holds for the other annual-report measures of supply strategy we constructed, from supply disruption and stockpiling to vertical integration and reshoring: several rose after a restriction, but net of their earlier trends none rose significantly (Fig. 5c). We therefore do not interpret disclosed search as a channel through which the restrictions changed supply bases (Table 2 reports the product of its two associations for completeness).

Discussion

For US-listed companies that depend on restricted minerals, six export restrictions imposed by China and Indonesia between 2010 and 2024 were followed by a shift of their disclosed suppliers towards jurisdictions with weaker rule of law. The shift operated through a rising share of suppliers headquartered in China and, less clearly, a smaller share in the United States, its free-trade partners and Japan, and it reversed when China lifted its rare-earth quotas, though not when Indonesia relaxed its nickel-ore ban. It is modest (0.16 standard deviations), rests mainly on Indonesia's nickel-ore bans, on companies exposed more than once, on companies with few disclosed suppliers and partly on suppliers new to the data, and is not clearly distinct from a smaller drift among companies that use base metals in general.

These findings bear on the expectation, implicit in de-risking policy and explicit in organizational theory and resilience research, that companies facing restricted supply will reduce their reliance on constrained sources and turn to secure, well-governed ones[2,3,5,17,19]. In the supplier relationships we observe, we do not see a move towards better-governed jurisdictions. The result is consistent with evidence that the reallocation of US imports away from China after the 2018 and 2019 tariffs left supply chains intertwined with it[28], and with the argument that central positions in global networks confer leverage[27], a position that control of processing may give a hub in critical minerals. It is also compatible with transaction-level evidence on China's 2023 licensing of gallium, germanium and graphite, after which

US importers turned towards European suppliers[16]: for those events we find no credible evidence of a shift towards weaker rule of law (Fig. 2c). It also complements industry-level evidence of input-saving innovation abroad after the 2010 rare-earth quotas[25]: dependent companies also changed whom they bought from.

Our data cannot establish why companies added Chinese suppliers after restrictions, including after China's own, but two routes through the processing hub fit them. One runs through refined materials: Indonesia's bans, designed to move nickel refining into Indonesia, were followed by a shift of China's imports from Indonesian ore to processed nickel (US$24 billion in 2025; Supplementary Note 7), and Chinese companies or shareholders ultimately control at least three-quarters of that refining capacity[26]; buying such nickel registers as a supplier in China or in a jurisdiction with weaker rule of law. The other runs through components: after a restriction, exposed companies added detectably more Chinese makers of components, but not of refined materials, which points to restricted minerals bought embodied in parts and to a broader re-sourcing of manufacturing. Neither route is tested here.

The sustainability relevance of the finding concerns the environment in which suppliers are monitored rather than observed harm. The rule-of-law index measures how far contracts, property rights and courts are enforced in a supplier's home country[33]; weaker enforcement plausibly makes suppliers' commitments harder to verify and enforce, while buying companies are held responsible for their suppliers' conduct[29]. The index says nothing about any supplier's conduct. Environmental and social incidents reported about the exposed companies themselves did not change detectably (all incidents 0.025, $P = 0.32$; environmental incidents 0.034, $P = 0.083$; Supplementary Table S22), so what we document is exposure to weaker enforcement rather than evidence of damage.

Several limitations bound these conclusions. First, the outcome records where disclosed suppliers are headquartered, not where minerals are mined or refined; attributing suppliers instead to the subsidiary through which a relationship runs or to a parent gives similar estimates, and attributing them to a corporate equity holder a smaller one (-0.045; Supplementary Note 6), but these records are incomplete and plant locations are not observed, so the results describe the jurisdiction of contracting parties rather than of production sites or ultimate owners. Second, relationships with Chinese suppliers are recorded mostly by the suppliers, and the shift appears in those records and is smaller among suppliers that FactSet already covered before a restriction (-0.038; $P = 0.055$); we cannot tell new relationships from existing ones that grew large enough to be reported, and changes in reporting or data coverage could also create new records. Third, the outcome is an unweighted average over each company's disclosed suppliers, and the estimate is carried by companies with few of them: weighted by the number of suppliers, or among companies with at least five, it is smaller and not statistically distinguishable from zero. Fourth, exposure is inferred from text: about one exposed company-event in four could not be confirmed as an input user, users that never write about a mineral sit in the comparison group, and the measure does not grade intensity, errors that would be expected to bias the estimate towards zero rather than reverse its sign. Fifth, the evidence rests on few restrictions: with the events as the units of inference, methods designed for few clusters give P values between 0.039 and 0.094, extrapolating the linear trend of the event-study leads widens the interval to include zero, the nickel bans and repeatedly exposed companies carry much of the estimate, and companies using only unrestricted base metals show a smaller drift in the same direction, so part of the decline may reflect a broader shift of metal users' supply chains. What remains is a decline that is not detected

before the restrictions or at placebo dates, persists under reweighting, company-specific trends, alternative fixed effects and rival-shock exclusions, appears in the composition of suppliers rather than in country scores, and reverses at the lifting of China's rare-earth quotas. Sixth, the sample consists of US-listed companies; the 2025 restrictions remain to be evaluated. Seventh, supply security itself is not measured, and the conditions under which the decline was larger are exploratory.

For policy, the results suggest that de-risking objectives cannot be assumed to follow from companies' own responses to export restrictions: in the period we study, those responses did not, on average, move dependent companies towards the jurisdictions that sourcing rules favour, and were associated with a move of some towards the jurisdiction those rules seek to avoid. Monitoring where companies' suppliers are based after a restriction, and not only where minerals originate, would show whether sourcing incentives and due-diligence requirements achieve what they intend. For companies, the results suggest that the governance of a supply base deserves attention precisely when supply is under pressure. Plant-level shipment and ultimate-ownership data could test the explanations we cannot. For now, whether de-risking happens inside companies remains an empirical question; in companies' disclosed supplier relationships, a move towards better-governed sources did not happen on its own over the period we study.

Methods

Data sources

Companies and accounts. The sample frame is the Compustat North America Fundamentals Annual file (S&P Global Market Intelligence, via WRDS) for fiscal years 2003 to 2025, restricted to the standard industrial, consolidated, domestic-format records reported in US dollars (220,390 company-years; 24,225 companies). Fiscal years 2024 and 2025, which the annual extract covers only partly, were rebuilt from the Compustat quarterly file (flows summed over four quarters, stocks taken at the fourth quarter); in overlapping years the rebuilt values correlate with the annual file at $r \geq 0.995$ for sales, cost of goods sold, inventories, assets, research and development and capital expenditure (Supplementary Table S23).

Annual reports. We parsed the full text of every 10-K and 20-F annual report filed between 2005 and 2026 by a company with a valid link to Compustat (142,150 reports; 8.2 billion words), using the CIK-GVKEY link table and, for 2025 and 2026 filings stored without a central index key, the ticker and fiscal year-end. Each report was split into sentences; for 35 minerals or mineral groups we counted sentences that name the mineral, sentences that name it together with a word describing an input or its supply (raw material, supplier, supply, sourcing, purchase, procure, import, price, cost, shortage, availability, depend, component, input) and sentences that name it together with China. We also counted sentences that combine supply-chain words with words for disruption, diversification and alternative sources, export controls, geopolitics and other supply strategies (dictionaries in Supplementary Note 1).

Supplier relationships. FactSet Revere records customer-supplier relationships disclosed by companies, by their counterparties and by other public sources, with start and end dates, from April 2003 to February 2025. We linked FactSet companies to Compustat through eight-character CUSIPs from the

FactSet CUSIP history and the Revere company file (20,528 Compustat companies matched; ambiguous links dropped), recorded each supplier's headquarters country, which party disclosed each link, each supplier's products in the Revere business classification (RBICS) and, where recorded, the subsidiary through which a link runs, the supplier's parent and its corporate equity holders. Because the relationships end in February 2025, all supplier-based measures are defined through calendar 2024 and set to missing from calendar 2025 on (Supplementary Note 4).

Country governance. The rule-of-law and control-of-corruption estimates of the Worldwide Governance Indicators[33] (World Bank, 1996 to 2023; years without an estimate take the nearest earlier year) measure, on a scale with a world mean of zero and a standard deviation of one, perceptions of the extent to which agents abide by the rules of society, including the quality of contract enforcement, property rights, the police and the courts.

Export restrictions and first stage. The dates, documents and scope of each restriction were coded from official announcements and verified against secondary legal and policy sources (Supplementary Table S8): Ministry of Commerce and General Administration of Customs of China announcements No. 23 and No. 39 (2023), No. 33 and No. 46 (2024), and No. 10 and No. 18 (2025); the 2010 rare-earth export quotas and their removal in 2015 following World Trade Organization dispute DS431; Indonesia's 2014 ban on unprocessed nickel ore, its partial relaxation from 2017 to 2019 and its reinstatement on 1 January 2020; and the Democratic Republic of the Congo's suspension of cobalt exports on 22 February 2025. China's exports and imports of the affected eight-digit tariff lines, and its imports of nickel ore and processed nickel from Indonesia (HS 2604, 720260 and 7501), come from the RESSET all-port customs database (January 2003 to July 2026). Domestic (CNY) and free-on-board export (USD) prices of rare earths, gallium, germanium, indium and antimony come from Datastream (Asian Metal assessments, 2003 to July 2016), converted at the monthly CNY/USD rate (Federal Reserve Economic Data series DEXCHUS). The OECD inventories of export restrictions on industrial and critical raw materials[35,36], which cover measures through 2024, were used to cross-check the coded measures up to 2024; the 2025 measures were checked against their official texts only.

Validation and control data. SEC Form SD filings (Dodd-Frank Act Section 1502 conflict-minerals reports) were taken from the EDGAR filing index; producers of each mineral among FactSet suppliers were identified from RBICS product paths; companies' revenue earned in China comes from FactSet Revere geographic revenue exposure; environmental and social incidents come from RepRisk; for the other outcomes examined, emissions come from Trucost and patents from the extended data of ref. 37.

Exposure

For each event e with effect year Y_e, a company is exposed if at least two of its annual reports for fiscal years $Y_e - 5$ to $Y_e - 1$ contain a sentence naming a restricted mineral of e together with an input word; comparison companies are those whose reports in the same window never name any of the minerals restricted in any of the nine events. Companies that name a mineral but fewer than twice as an input are excluded from that event's stack. Financial companies (SIC 6000 to 6999), utilities (SIC 49), mining and extraction (SIC 10, 12, 13 and 14) and primary nonferrous smelters (SIC 3330 to 3339) are excluded because for them the minerals are outputs rather than inputs. The approach follows

text-based measures of company-level exposure to political and climate risks[38,39]. A company's annual report records that management wrote about a mineral as something the company buys, uses or needs; we use it to represent input dependence. Three checks support this reading. First, in 11 of 15 mineral-by-period comparisons, exposed companies were 6 to 20 times more likely than other companies to have a FactSet supplier that produces the mineral; the exceptions are graphite and lithium in the 2005 to 2009 period, when 12 and 26 companies named them as inputs, and tungsten in 2005 to 2009 and antimony in 2016 to 2022, when no exposed company had a linked producer (Supplementary Table S9). Second, 75% of companies naming tungsten, tantalum or tin as inputs between 2010 and 2013 filed a conflict-minerals report between 2014 and 2016, against 21% of other companies. Third, we audited the exposed company-events of the six analysed events (283 of 284 company-events with a retrievable flagged sentence, 424 sentences; two independent classifications against a written rubric, $\kappa = 0.90$; procedure in Supplementary Note 3): 75% (95% CI 69% to 80%) had a sentence describing input use, from 90% for the 2020 nickel event to 49% for the 2023 gallium and germanium event. We report the main estimate with the unconfirmed company-events removed and a dose-response in the number of reports naming the mineral as an input.

Outcomes and analysis variables

Supplementary Table S1 gives, for each construct, its definition, proxy, source fields, formula and validity evidence, and Supplementary Table S2 lists all 69 variables used. *Supply-base rule of law* for company i in year t is the mean rule-of-law estimate of the headquarters countries of the suppliers linked to i on 31 December of t (for a company with two US suppliers and one Chinese supplier in 2020: $(1.34 + 1.34 - 0.10)/3 = 0.86$). Suppliers without a headquarters country or a governance estimate are omitted from the mean. It records where a company's disclosed suppliers are domiciled; we use it to represent the institutional enforcement environment of the company's supply base, the dimension that supplier due diligence relies on. The headquarters locates the contracting party: FactSet records relationships between companies, and a buyer's contractual and due-diligence claims against a supplier would typically have to be enforced in the supplier's home jurisdiction. A supplier's headquarters is not necessarily where it mines or processes, nor where its owners are, and disclosed relationships cover a company's larger or more visible suppliers. Where FactSet identifies the subsidiary through which a relationship runs, a parent company or a corporate equity holder, we also attribute suppliers to that entity's country (Supplementary Note 6). Because country scores change over time, we also compute the measure with each country's score fixed at its 2005 to 2023 mean, so that it moves only when suppliers change; and because either party can disclose a link, we compute it separately for links disclosed by the company itself and for links disclosed by suppliers or other sources. As a placebo, we compute the same mean over the company's customers. *New suppliers* are counted by the year their relationship starts, by headquarters jurisdiction (China, including Hong Kong; other jurisdictions with rule of law below the world mean; jurisdictions above it), by whether the supplier's products lie in the materials value chain (RBICS products under Materials, or naming semiconductors, electronic components, batteries or magnets), and by the sourcing categories of the US Inflation Reduction Act (the United States, its free-trade-agreement partners and Japan; "foreign entities of concern", that is China, Hong Kong, Macao, Russia, Iran and North Korea).

Supply-base agility is the number of supplier relationships that begin in a year divided by the number of

active suppliers, averaged over the five pre-event years and capped at its 99th percentile. *Pricing power* is a company's percentile rank in gross margin, (sales − cost of goods sold)/sales, among companies in the same two-digit SIC industry and fiscal year, averaged over the five pre-event years; the within-industry rank is used because untransformed gross margin is dominated by companies with negligible sales. Both are standardized across company-events. *Disclosed supplier search* is the number of sentences per 1,000 in a fiscal year's annual report that combine a supply-chain word (supply chain, supplier, sourcing, raw material, vendor, procurement) with a word for diversifying, qualifying or adding alternative, second, multiple or new sources or suppliers, winsorized at the 1st and 99th percentiles; it records what management wrote about looking for other suppliers. *Control variables*, all lagged one year, are log total assets, leverage (long-term debt plus debt in current liabilities, over assets), return on assets (operating income before depreciation over assets), cash and short-term investments over assets, tangibility (net property, plant and equipment over assets) and sales growth (sales over the previous year's sales, minus one). Because some lagged controls could themselves respond to a restriction from the second post-restriction year, we also report the estimate without controls (Table 1, column 1), which is the same.

Measurement validity

Each construct is linked to its measure by an explicit argument and checked against independent evidence (Supplementary Table S1). Input dependence, inferred from annual-report text, agrees with supplier links to producers of the mineral, with conflict-minerals filings and with the sentence-level audit (Exposure); because the text predates each restriction, its errors are unlikely to be related to later changes in suppliers. The institutional quality of the supply base is measured with the rule-of-law dimension of the Worldwide Governance Indicators, which targets the enforcement of contracts and property rights on which supplier monitoring depends. Three checks support this reading: the results are similar with control of corruption in place of rule of law; they are larger with country scores held fixed, so they reflect the location of suppliers rather than changes in scores; and they do not appear in the governance of the countries where companies' customers are headquartered, which a restriction on inputs should not affect. Empirically, the measure moves through the China share: excluding suppliers in China and Hong Kong, it does not change detectably. Hong Kong, which is pooled with mainland China in that share, has a high rule-of-law score, so a shift towards suppliers in Hong Kong would raise rather than lower the measure. Disclosed supplier search is taken from different annual-report sentences than exposure and from a different source than the outcome, so its associations with both are not mechanical.

Design and estimation

We estimate stacked difference-in-differences regressions[40] in which each event forms a stack containing its exposed and comparison companies over fiscal years $Y_e - 5$ to $\min(Y_e + 4, \text{end}_e)$, where the end year precedes any lifting of the measure (Supplementary Note 4):

$$y_{ist} = \beta \, \text{Exposed}_{is} \times \text{Post}_{st} + \gamma' X_{i,t-1} + \alpha_{is} + \delta_{st} + \varepsilon_{ist}, \tag{1}$$

where α_{is} are company-by-event and δ_{st} year-by-event fixed effects, so that each comparison is made within an event between exposed and unexposed companies in the same calendar years, and $X_{i,t-1}$ contains the lagged control variables. Stacking avoids comparisons between early- and late-treated units that bias two-way fixed-effects estimates under staggered adoption[41-46]; we also report the corrective stack weights of ref. 34. Standard errors are clustered by company, the level at which exposure is assigned. Event studies replace Post with indicators for years -4 to $+4$ relative to the event (reference -1; endpoints binned). The identifying assumption is that, absent the restriction, the supply bases of exposed and comparison companies would have evolved in parallel. Exposure is not random (exposed companies are larger and hold less cash than comparison companies; Supplementary Table S3 and Supplementary Fig. S3), and only the timing and the mineral of each restriction were set externally, by foreign governments. The design is therefore a difference-in-differences with a comparison group whose comparability we probe rather than assume, with leads, placebo dates, reversals at liftings, entropy balancing[47] of comparison companies to the pre-event means of the controls and the outcome (Supplementary Fig. S3), industry-by-year-by-event fixed effects, and the sensitivity of the event-study estimates to a linear pre-existing trend (a bounded linear-trend analysis in the spirit of ref. 48; Supplementary Fig. S2). Indonesia's 2014 ban was legislated in 2009 and its 2020 reinstatement was announced in 2019, so companies could anticipate them; we therefore also report estimates without the year before each restriction. Several of China's measures between 2023 and 2025 followed US and allied export controls on advanced technologies[16], controls that imposed direct costs on US suppliers of the targeted Chinese companies[49] and may have affected some exposed companies directly, and Indonesia's 2020 ban coincided with the COVID-19 pandemic; we report estimates without the fiscal years 2020 and 2021, without companies that filed conflict-minerals reports (for the 2010 stack, which coincides with the Dodd-Frank conflict-minerals rule), for China's restrictions and Indonesia's bans separately, without companies exposed in an earlier stack, and with pre-event China revenue share interacted with year-by-event fixed effects. Shifts of supply chains towards China that are common to exposed and comparison companies are absorbed by the year-by-event fixed effects; for shifts specific to exposed companies, we add company-by-event linear trends, estimated over the whole window or over the pre-event years and extrapolated, and interact pre-event China supplier share and supply-base size with year-by-event fixed effects (Supplementary Note 8).

To ask whether the decline is specific to restricted minerals, we add to the same regression an indicator for companies whose reports name only unrestricted base metals (copper, aluminium, zinc) as inputs, interacted with Post, and report the difference between the two coefficients with its company-clustered confidence interval. New suppliers are modelled as counts with Poisson pseudo-maximum likelihood and the same fixed effects. Models of how the effect varies add $\mathrm{Exposed} \times \mathrm{Post} \times Z$ and year-by-event-by-Z terms, so that the interaction is identified only from exposed companies' post-event change. For disclosed supplier search, we estimate its change after a restriction with the same design and its association with supply-base rule of law conditional on exposure and the fixed effects, and bootstrap the product of the two coefficients by resampling companies (1,000 draws, on variables from which the fixed effects and controls have been partialled out); because the change in search does not survive allowance for its pre-existing trend, we do not interpret this product as evidence of transmission.

Sample splitting and multiple testing

Before any outcome was examined, companies were divided into an exploration half and a confirmation half by a fixed hash of their identifier. The exploratory analyses (variation of the effect with pre-event characteristics, and annual-report measures of supply strategy) were estimated in each half and in the full sample. Because results from both halves were inspected together, the half-sample estimates serve as replication diagnostics rather than as out-of-sample tests (Supplementary Note 2). We control the false-discovery rate within each family of outcomes, characteristics and candidate channels examined[50] (Supplementary Table S18) and list every estimate in Supplementary Table S19. The study was not preregistered.

Statistics

All tests are two-sided with $\alpha = 0.05$; we report exact P values and 95% confidence intervals. Test statistics are reported as statistic (degrees of freedom) = value, where the degrees of freedom of cluster-robust t tests are the number of clusters minus one. Effect sizes are reported in three ways: in units of the outcome; as a percentage of the pre-event mean of the outcome among exposed company-events; and in standard deviations of the outcome. For continuous regressors, including the control variables, we report the change in the outcome per standard deviation of the regressor, as a percentage of the outcome's mean; Supplementary Tables S5 and S6 give every coefficient of the models in Tables 1 and 2, with standard errors clustered by two-digit industry and two-way by company and fiscal year beneath each focal coefficient, and Supplementary Table S7 gives the standardized effects. Randomization inference permutes exposure among companies within industry-by-event cells (1,000 permutations; $P = (1 + \#\{|\hat{\beta}^*| \geq |\hat{\beta}|\})/(1 + \text{draws})$). Inference across the six shocks uses a random-effects meta-analysis of the per-event estimates with the Hartung-Knapp-Sidik-Jonkman variance[51]. Because identification rests on six events, we also treat the events as clusters, with CR2 standard errors and the degrees of freedom of Imbens and Kolesár[52], the wild cluster restricted bootstrap-t with Webb weights enumerated over all 6^6 draws[53,54] and a leave-one-event-out jackknife, and test the per-event estimates with a t test and a sign-change randomization test[55,56] (Supplementary Note 5). Benjamini-Hochberg q-values control the false-discovery rate within each family of outcomes, characteristics or measures examined[50]. Analyses used Python 3.9 with pandas 2.3, DuckDB 1.4, statsmodels 0.14 and pyfixest 0.18; all code runs from a single entry point with a fixed seed.

Data availability

The derived company-event-year analysis panel (company identifiers replaced by project keys where licences require), the export-restriction catalogue with its official sources, the exposure dictionaries, the exposure-audit sentences with their codes and rubric, the country governance scores used, and the Source Data for every figure will be deposited in Zenodo with a DOI on publication; during peer review they are available to editors and reviewers on request. Data obtained from third parties cannot be redistributed in raw form: Compustat North America (S&P Global Market Intelligence, via Wharton Research Data Services), FactSet Revere supply-chain relationships, business classifications and geographic revenue exposure (FactSet Research Systems), RepRisk ESG incident data (RepRisk AG),

Trucost environmental data (S&P Global), patent data linked to listed companies (extended data of Kogan, Papanikolaou, Seru and Stoffman, available from the authors of that study), Datastream commodity prices (LSEG) and the RESSET all-port China customs database (RESSET). They can be obtained from these providers under licence; the derived variables needed to reproduce every estimate are included in the deposit to the extent the licences permit, and the remainder can be rebuilt with the code from licensed extracts. Public sources: annual reports (SEC EDGAR, `https://www.sec.gov/edgar`); Worldwide Governance Indicators (World Bank, `https://www.worldbank.org/en/publication/worldwide-governance-indicators`); OECD Inventory of Export Restrictions on Industrial Raw Materials (OECD Data Explorer, downloaded 5 October 2026); CNY/USD exchange rate (Federal Reserve Economic Data, series DEXCHUS, downloaded 5 October 2026); official texts of the export-restriction measures (sources listed in Supplementary Table S8). Source data are provided with this paper.

Code availability

All code that reproduces the data construction, every estimate, table and figure and every number quoted in the text from the source extracts (Python 3.9; numbered scripts with a single run-all entry point and a fixed random seed of 20261005) is provided to editors and reviewers with this submission and will be deposited in Zenodo with a DOI on publication.

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Author contributions

D.T. conceived the study, assembled the data, carried out the analysis and wrote the original draft. C.C. contributed to the research design and the interpretation of the results and revised the manuscript. S.E. supervised the project and revised the manuscript. All authors approved the submitted version.

Competing interests

The authors declare no competing interests.

Materials & Correspondence

Correspondence and requests for materials should be addressed to Can Cui.

Figure legends

Fig. 1 | Nine export restrictions on critical minerals and the companies exposed to them. a, Timeline of the restrictions studied, by producing country: filled circles, restrictions whose aftermath can be followed in the supplier data; open circles, restrictions of 2025, whose aftermath cannot yet be followed; squares, lifting of China's rare-earth quotas (2015) and relaxation of Indonesia's nickel-ore ban (2017); shading, rare-earth quota regime and Indonesian nickel-ore bans in force. Ga, gallium; Ge, germanium; Sb, antimony; W, tungsten; Te, tellurium; Bi, bismuth; Mo, molybdenum; In, indium; heavy rare earths, the seven medium and heavy rare earths restricted in 2025. **b**, Price of rare-earth oxides and metals for export (free on board, China) relative to the price inside China, in log points relative to each series' mean over 2005 to 2009: means across dysprosium oxide, terbium oxide, praseodymium oxide and neodymium metal (red) and across gallium, germanium, indium and antimony (blue), with their range (bands); quarterly means of monthly data; shading, rare-earth quota regime (the rare-earth premium had already begun to rise in 2007 to 2009). **c**, China's monthly export volume of gallium plus germanium, of antimony and of graphite around the announcement of each control (three-month moving average; mean of the 12 preceding months = 100). **d**, China's imports of nickel ores and concentrates from Indonesia and from the Philippines (million tonnes per year; 2016 missing in the source); shading, Indonesian bans in force. **e**, Exposed companies per event (US-listed companies with at least two of five pre-event annual reports mentioning the restricted mineral together with an input-related word) and, in red, those whose disclosed suppliers can be followed after the event (the company-events entering Figs 2 to 5). Data: Datastream (Asian Metal), RESSET customs, company annual reports, FactSet Revere. Source data are provided as a Source Data file.

Fig. 2 | Supply-base rule of law of exposed and comparison companies before and after export restrictions. **a**, Change in supply-base rule of law (mean Worldwide Governance Indicators (WGI) rule-of-law estimate of the headquarters countries of a company's disclosed suppliers; world mean 0, s.d. 1) relative to each company-event's own pre-restriction mean, for exposed (red; 237 company-events) and comparison (blue) companies by year relative to the restriction, six events pooled; descriptive means with 95% confidence intervals of the mean, without controls or fixed effects (the estimates are in **b**). **b**, Stacked event study: coefficients on exposed $\times$ year relative to the restriction (reference year -1) from a regression with company-by-event and year-by-event fixed effects and lagged controls; shading, years after the restriction; error bars, 95% confidence intervals from standard errors clustered by company (5,199 companies); joint Wald test of the leads; open markers, the same for the three events observed in every year from -4 to $+2$ (China 2010 rare earths, Indonesia 2014 and 2020 nickel ore). **c**, Difference-in-differences estimates for each event estimated separately (n, exposed companies; marker area proportional to n), the pooled estimate (filled diamond) and pooled estimates for China's restrictions and Indonesia's nickel-ore bans (open diamonds); rows ordered by estimate; error bars, 95% confidence intervals. **d**, Change in the number of new suppliers added per year by headquarters jurisdiction and, for China (including Hong Kong), by whether the supplier's products lie in the materials value chain (solid) or not (hatched), and for other jurisdictions with rule of law below (lilac) and above (light blue) the world mean: Poisson pseudo-maximum-likelihood estimates with the same fixed effects, expressed as % change in the number added per year relative to comparison companies; error bars, 95% confidence intervals; two-sided Wald tests with standard errors clustered by company. Source data are provided as a Source Data file.

Fig. 3 | How specific and how robust the decline is. Difference-in-differences estimates of the effect of exposure on supply-base rule of law (WGI points; error bars, 95% confidence intervals from standard errors clustered by company, except for the meta-analysis and the wild cluster bootstrap, which treat the six events as the units of inference) under alternative comparisons, measures, samples and estimators, with the two-sided P value of each estimate at the right; red circles, estimates for exposed companies or their difference from base-metal users; blue squares, comparison-type estimates for which no effect (customers' countries, earlier dates), a smaller effect (base-metal users) or an effect of opposite sign (liftings) would be expected if the restrictions caused the decline; dashed red line, main estimate. **a**, Specificity: exposed companies and companies naming only unrestricted base metals, each against companies naming no metal in one regression, the difference between them, the difference without aluminium users, and base-metal users as the only comparison group. **b**, Change of suppliers, measurement and size of the supply base: country scores fixed at their means over 2005 to 2023; links disclosed by the company itself and by suppliers; control of corruption; at least three and at least five suppliers before the event; company-events weighted by their pre-event number of suppliers; exposed company-events not confirmed by the audit removed; suppliers attributed to their parent or, failing that, a corporate equity holder; only suppliers that FactSet covered, or had recorded as suppliers, before the event; customers' countries. **c**, Rival shocks, event composition and estimators (including entropy balancing on the controls only, company size by year by event fixed effects, the year before the restriction dropped and company-by-event linear trends), inference with few events (random-effects meta-analysis; wild cluster bootstrap with Webb weights, interval by test inversion), placebo dates and reversals at the 2015 rare-earth and 2017 nickel liftings. Main estimate, 237 exposed company-events among 5,199 companies. Sample sizes: Table 1 (entropy-balanced), Supplementary Note 3 (audit)

and Supplementary Tables S11, S12, S13, S24, S26 and S29 (all other rows). Source data are provided as a Source Data file.

Fig. 4 | Where the decline is larger or smaller (exploratory). a, **b**, Effect of exposure on supply-base rule of law as a function of pre-event supply-base agility (**a**; new supplier relationships per active supplier) and pricing power (**b**; percentile rank of gross margin within two-digit industry and year), both standardized, from regressions with exposed $\times$ post $\times$ condition, condition-by-year-by-event, company-by-event and year-by-event fixed effects and lagged controls, drawn between the 5th and 95th percentiles of exposed company-events; line, estimate; band, 95% confidence interval (standard errors clustered by company); ticks, exposed company-events (214 in **a** and 236 in **b**, among 5,048 and 5,195 companies); annotations, full-sample interaction with its P value and the false-discovery-rate q-value across all pre-event characteristics examined. P in the exploration and confirmation halves of the sample (companies split by a fixed hash before any outcome was examined): agility $P = 0.36$ and $P = 0.032$; pricing power $P = 0.082$ and $P = 0.38$; with both conditions in one model, $P = 0.042$ and $P = 0.099$. **c**, Effect of exposure by tercile of the number of disclosed suppliers before the event, with terciles formed over all exposed and comparison company-events (range in parentheses; company-events tied at a boundary are split by order) and each estimate using the exposed and comparison company-events of its tercile; error bars, 95% confidence intervals. Source data are provided as a Source Data file.

Fig. 5 | Disclosed searches for alternative suppliers before and after export restrictions, and their earlier trend. a, Stacked event study of disclosed supplier search (sentences per 1,000 annual-report sentences that combine supply-chain words with words for diversifying to, qualifying or adding alternative, second or new suppliers) in the companies with supplier data (236 exposed company-events among 5,169 companies); model and error bars as in Fig. 2b; dashed line, linear trend through the pre-restriction coefficients extrapolated forward; annotations, joint test of the leads and the average post-restriction coefficient before and after subtracting the extrapolated trend. **b**, Supply-base rule of law against disclosed supplier search, both as deviations after partialling out exposure $\times$ post, lagged controls and company-by-event and year-by-event fixed effects (20 equal-count bins), with the slope and its 95% confidence interval (standard errors clustered by company). **c**, For each of the 13 annual-report measures of supply strategy in Supplementary Table S16, t statistic of its average rise after a restriction as estimated (filled) and net of its extrapolated pre-restriction trend (open); dotted lines, ± 1.96. Source data are provided as a Source Data file.

Table 1. Export restrictions and the rule of law of dependent companies' supply bases. Stacked difference-in-differences over six export-restriction events; the dependent variable is the mean Worldwide Governance Indicators rule-of-law estimate of the headquarters countries of a company's suppliers (world mean 0, s.d. 1). Standard errors clustered by company in parentheses; 95% confidence intervals in brackets. Economic magnitude = coefficient / pre-event mean of the outcome among exposed company-events; effect in s.d. = coefficient / pre-event standard deviation of the outcome. Column (5): exposure defined from at least one, rather than two, pre-event reports naming the mineral as an input. Columns (6) and (7): the exploration and confirmation halves of the sample, defined by a fixed hash of the company identifier before any outcome was examined; results from both halves were inspected together, so they are replication diagnostics rather than hold-out tests. Every coefficient, including the control variables, and standard errors under alternative clustering are in Supplementary Table S5.

	(1) FE only	(2) + controls	(3) + industry×year ×event FE	(4) Entropy-balanced	(5) Exposure $K = 1$	(6) Exploration half	(7) Confirmation half
Exposed × post	−0.064	−0.064	−0.062	−0.079	−0.059	−0.047	−0.083
	(0.020)	(0.020)	(0.020)	(0.021)	(0.019)	(0.026)	(0.031)
	[−0.104, −0.025]	[−0.103, −0.024]	[−0.101, −0.022]	[−0.119, −0.039]	[−0.096, −0.022]	[−0.098, 0.004]	[−0.145, −0.022]
P value	0.001	0.002	0.002	<0.001	0.002	0.072	0.008
Lagged controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Company×event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year×event FE	Yes	Yes	Nested	Yes	Yes	Yes	Yes
Industry×year×event FE	No	No	Yes	No	No	No	No
Standard errors clustered by	Company	Company	Company	Company	Company	Company	Company
Company-event-years	76,477	76,477	76,477	72,661	76,612	38,233	38,244
Companies (clusters)	5,199	5,199	5,199	4,973	5,213	2,594	2,605
Exposed company-events	237	237	237	211	268	119	118
Within R^2	0.0005	0.0008	0.0007	0.0010	0.0007	0.0012	0.0023
Mean of dependent variable	1.283	1.283	1.283	1.285	1.283	1.280	1.287
Pre-event mean, exposed	1.234	1.234	1.234	1.234	1.228	1.239	1.229
Economic magnitude (% of pre-event mean)	−5.2	−5.2	−5.0	−6.4	−4.8	−3.8	−6.8
Effect in s.d. of the outcome	−0.16	−0.16	−0.16	−0.20	−0.15	−0.12	−0.21

Table 2. Exploratory analyses: where the decline is larger or smaller, and disclosed supplier search. Columns 1 to 3: dependent variable supply-base rule of law; agility and pricing power are pre-event means standardized across company-events and enter with condition×year×event fixed effects, so that each interaction is identified only from exposed companies' change after the restriction. Columns 4 to 6: disclosed supplier search (annual-report sentences per 1,000) as the dependent variable (4), and supply-base rule of law on search and exposure (5) and on exposure alone in the same sample (6). Standard errors clustered by company in parentheses; 95% confidence intervals in brackets. q: Benjamini-Hochberg false-discovery-rate value across all 52 pre-event characteristics examined (columns 1 and 2; Supplementary Note 2). Economic magnitude: change in the dependent variable as % of its pre-event mean among exposed company-events, per standard deviation of the condition (columns 1 to 3), for exposure (4, 6) or per standard deviation of search (5); effect in s.d.: the same change in pre-event standard deviations of the dependent variable. Product of the search coefficients, (4)×(5): −0.0025 (company-clustered bootstrap 95% CI −0.0050 to −0.0007; 1,000 draws), reported for completeness only. Net of the pre-restriction trend: average post-restriction event-study coefficient of column 4 after extrapolating the linear trend of the leads. Every coefficient, including the control variables, and standard errors under alternative clustering are in Supplementary Table S6.

	(1) Agility	(2) Pricing power	(3) Both conditions	(4) Search	(5) Search and exposure	(6) Exposure only
Exposed × post	−0.074	−0.063	−0.072	0.058	−0.062	−0.065
	(0.020)	(0.020)	(0.020)	(0.018)	(0.021)	(0.021)
	[−0.113, −0.034]	[−0.103, −0.023]	[−0.112, −0.033]	[0.022, 0.094]	[−0.103, −0.022]	[−0.105, −0.025]
P	<0.001	0.002	<0.001	0.002	0.002	0.002
Exposed × post × agility	−0.067		−0.063			
	(0.031)		(0.031)			
	[−0.128, −0.006]		[−0.124, −0.002]			
P	0.031		0.042			
Exposed × post × pricing power		0.047	0.041			
		(0.025)	(0.025)			
		[−0.003, 0.097]	[−0.008, 0.090]			
P		0.063	0.099			
Disclosed supplier search					−0.042	
					(0.013)	
					[−0.068, −0.017]	
P					0.001	
P, exploration half	0.363	0.082				
P, confirmation half	0.032	0.375				
q (false discovery rate)	0.32	0.36				
Net of the pre-restriction trend				0.009 [P = 0.799]		
Lagged controls	Yes	Yes	Yes	Yes	Yes	Yes
Company×event and year×event FE	Yes	Yes	Yes	Yes	Yes	Yes
Condition×year×event FE	Yes	Yes	Yes	No	No	No
Standard errors clustered by	Company	Company	Company	Company	Company	Company
Dependent variable	Rule of law	Rule of law	Rule of law	Search	Rule of law	Rule of law
Company-event-years	73,715	76,371	73,631	75,492	75,492	75,492
Companies (clusters)	5,048	5,195	5,044	5,169	5,169	5,169
Exposed company-events	214	236	213	236	236	236
Within R^2	0.0046	0.0016	0.0054	0.0020	0.0017	0.0008
Mean of dependent variable	1.285	1.283	1.285	0.196	1.283	1.283
Pre-event mean, exposed	1.234	1.236	1.236	0.270	1.234	1.234
Economic magnitude (% of pre-event mean)	−5.4	3.8	−5.1	21.5	−1.1	−5.3
Effect in s.d. of the dependent variable	−0.17	0.12	−0.16	0.18	−0.04	−0.16

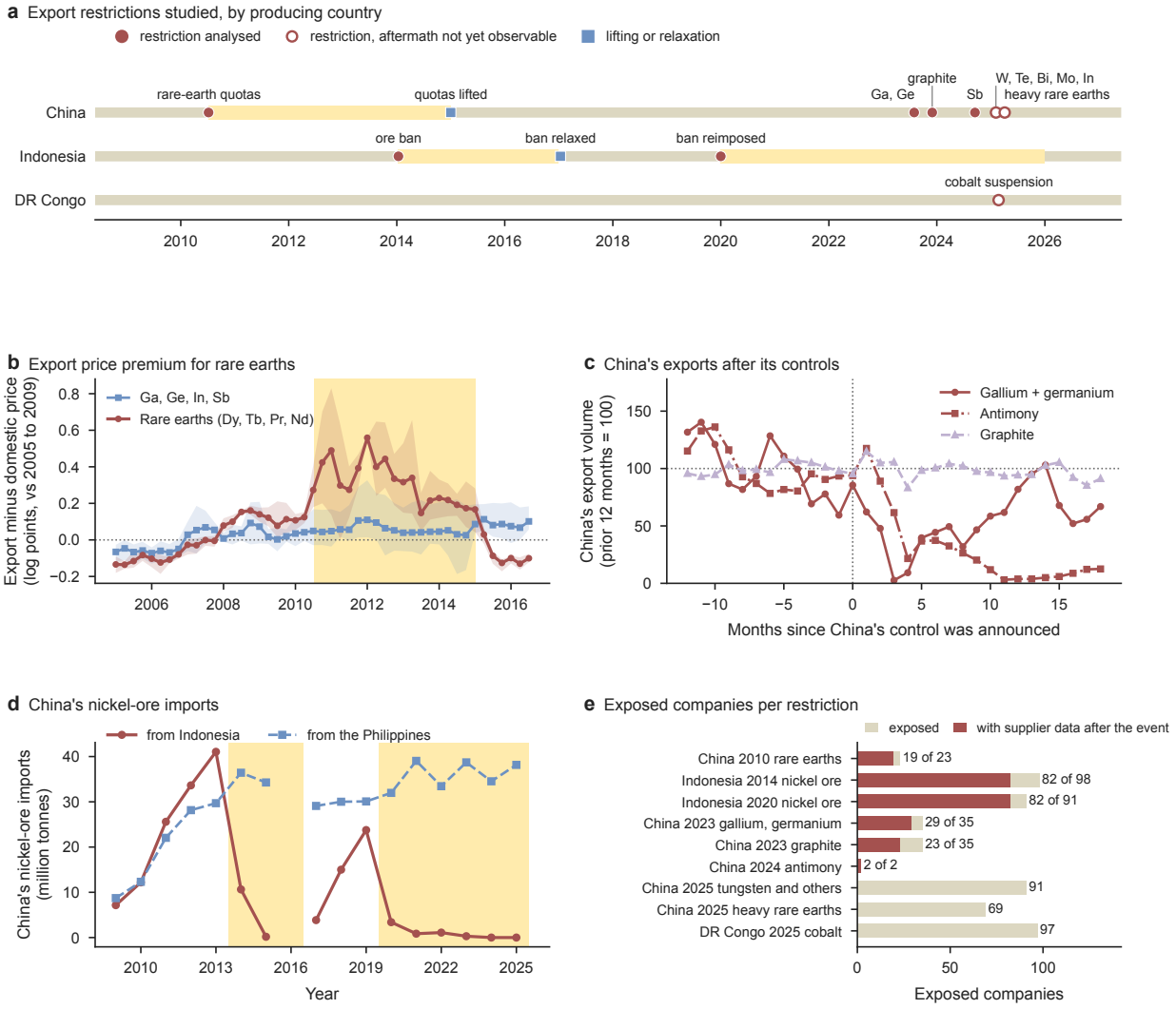


Figure 1. See figure legends.

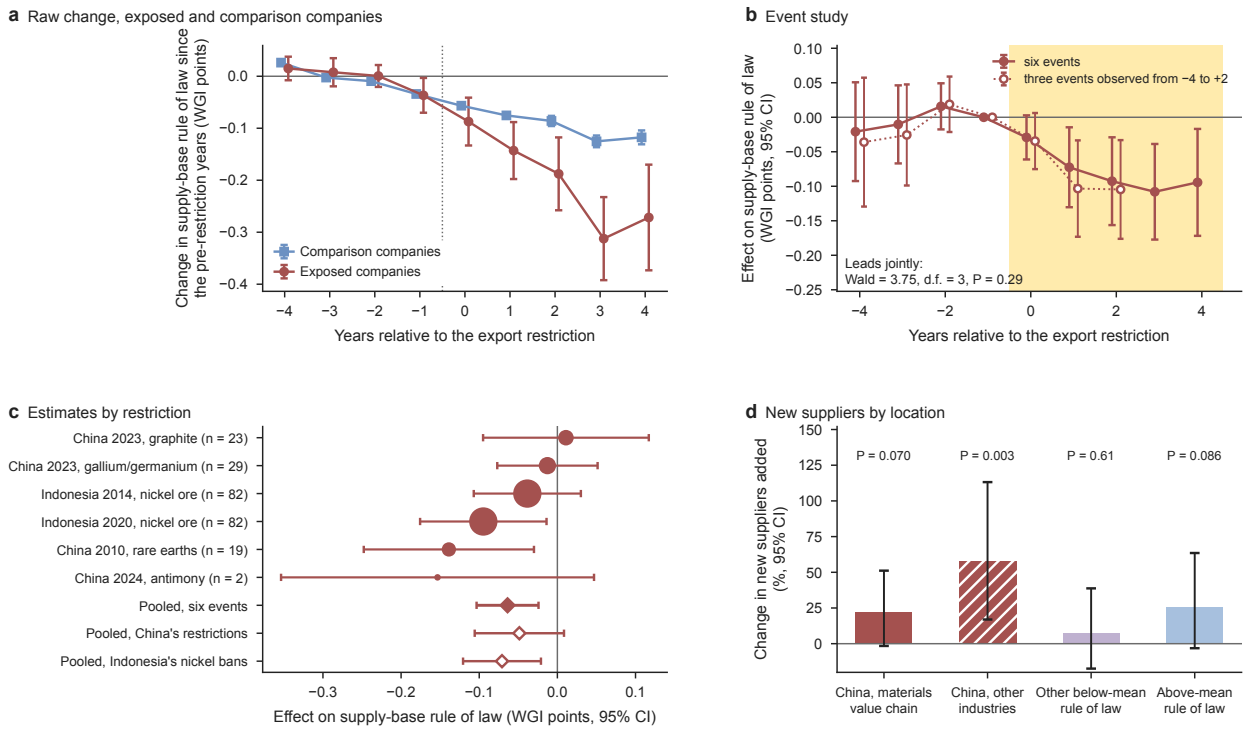
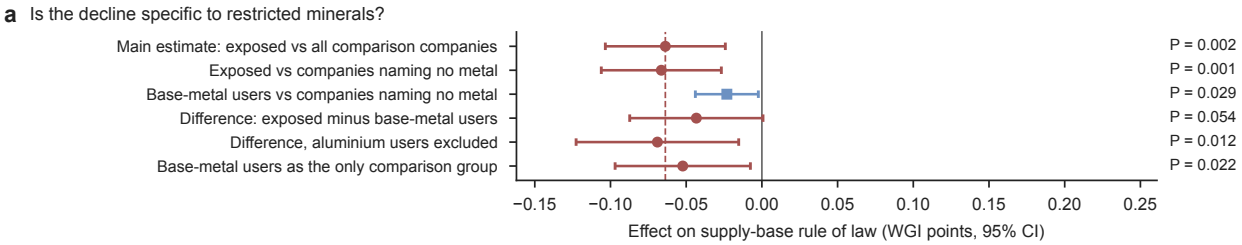
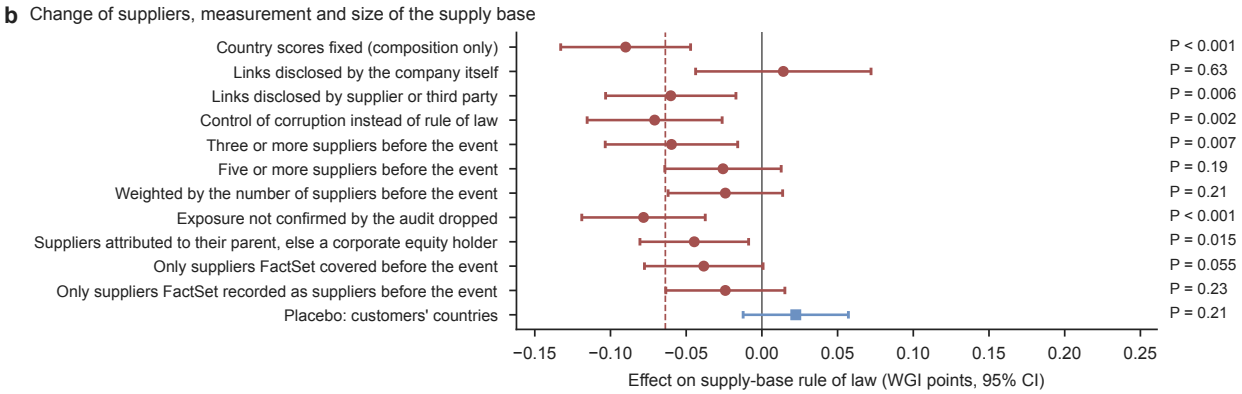


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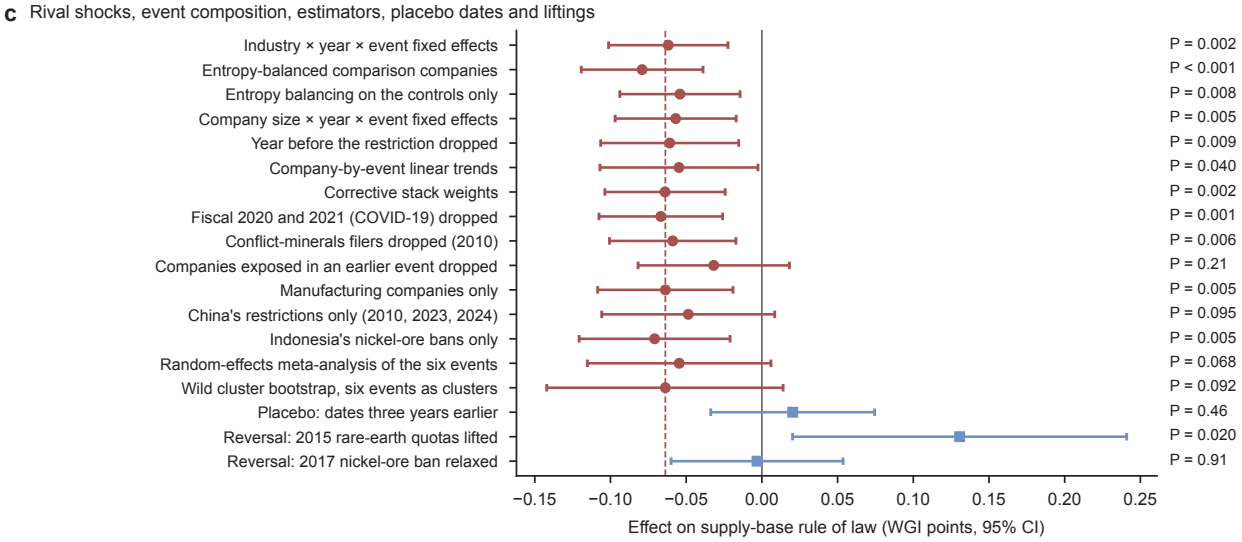


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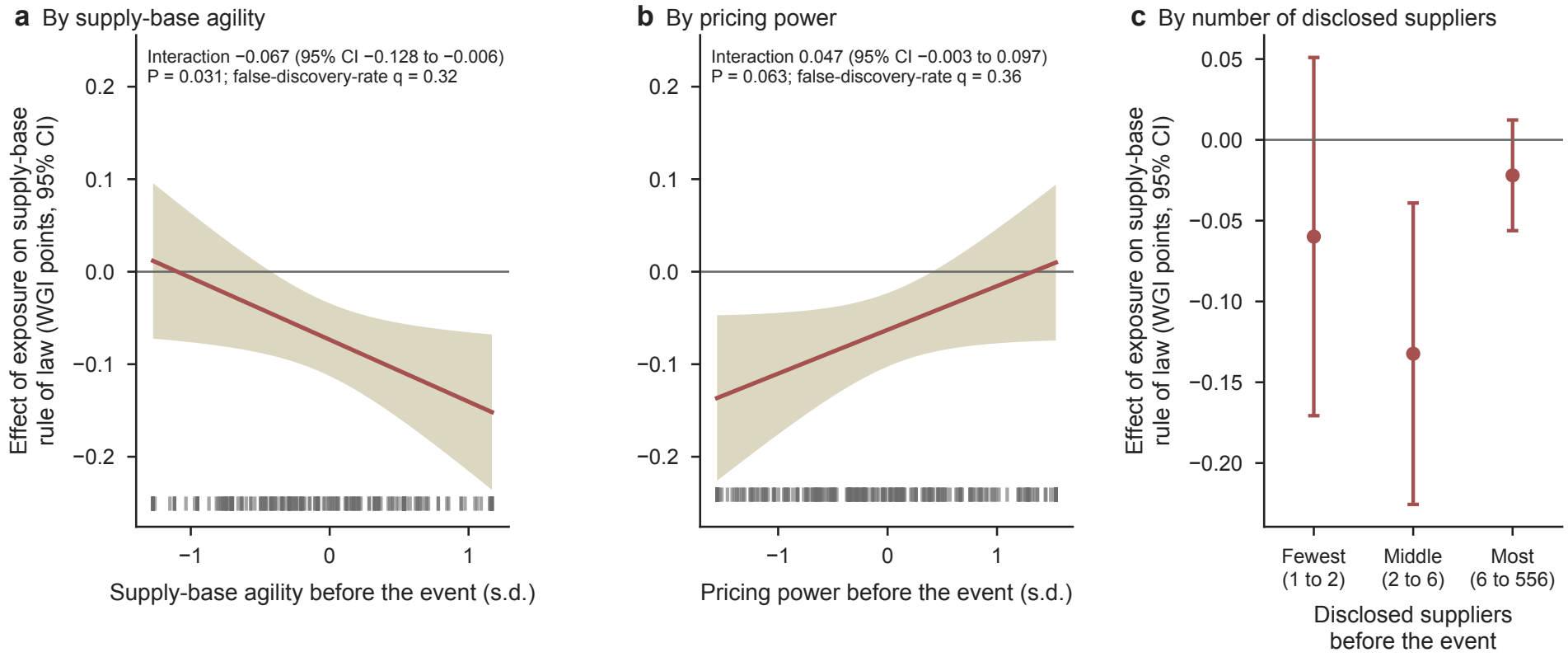


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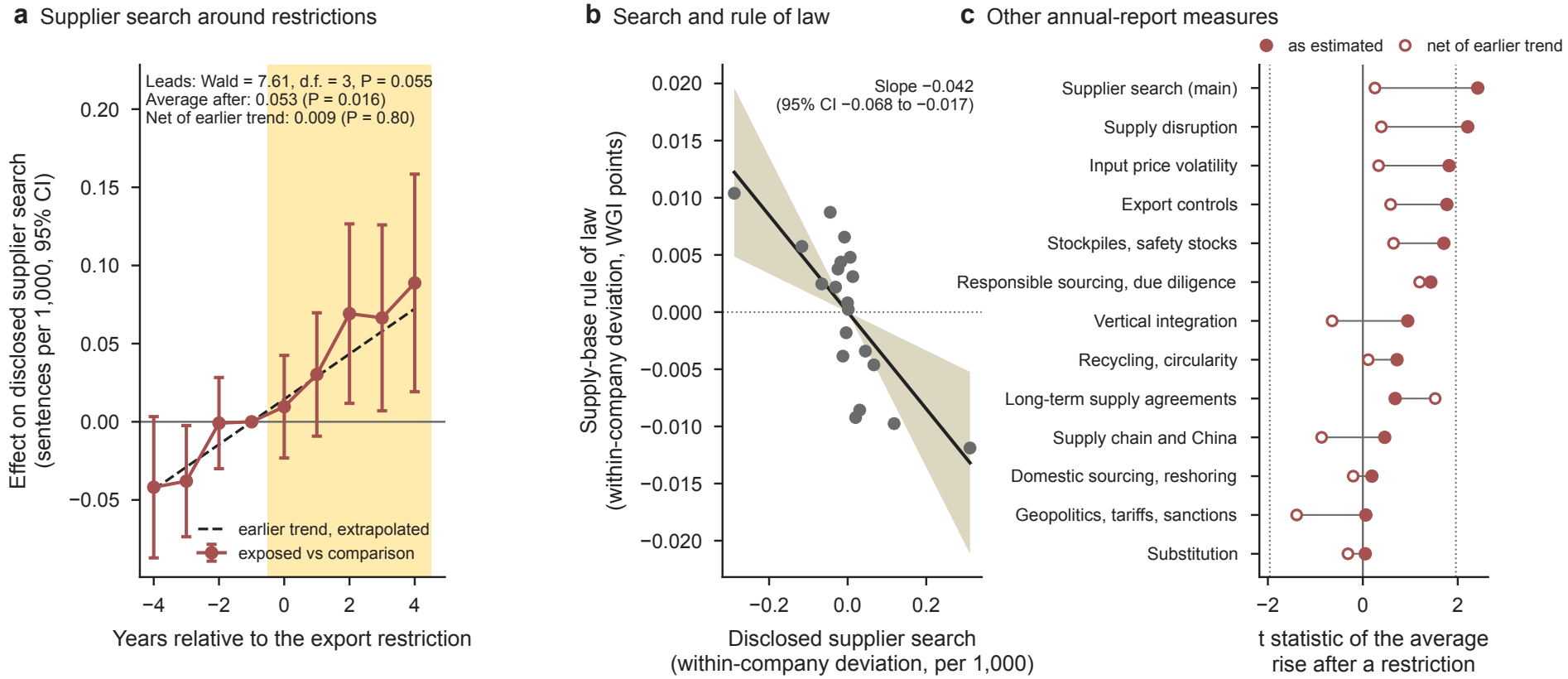


Figure 5. See figure legends.

Supplementary Files

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- 04SourceData.xlsx
- 03SupplementaryInformation.pdf