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JINJI, Naoto
RIETI

KAWAGUCHI, Shigenobu
Kyoto University

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The impact of global sanctions on foreign direct investment: A staggered difference-in-differences approach*

Naoto JINJI
(Kyoto University and RIETI)

Shigenobu KAWAGUCHI
(Kyoto University)

Abstract

Using a comprehensive database of global sanctions, we examine how sanctions affect cross-border mergers and acquisitions (M&A) over 2006-2023. Aggregating deal-level M&A data to the investor-host-year level, we estimate sanction effects using an extended two-way fixed effects estimator in a staggered difference-in-differences framework. Baseline results show that sanctions are associated with an approximately 50% reduction in bilateral cross-border M&A deals from sanctioning to target countries. Event-time estimates further suggest that this negative effect persists and deepens over time. Cohort-specific estimates indicate substantial heterogeneity across sanction episodes, likely driven by variations in participating countries, sanction types, and other contextual features.

Keywords: global economic sanctions; mergers & acquisition; extended two-way fixed effect estimator; staggered difference-in-differences
JEL classification: F21, F23, F51

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1 Introduction

Since Russia invaded Ukraine in February 2022, Western countries have introduced successive rounds of large-scale sanctions against Russia, and many multinational firms have announced the suspension, divestment, or withdrawal of their businesses in Russia. This episode suggests that economic sanctions can affect firms' cross-border investment decisions by changing transaction costs and uncertainty between sanctioning and sanctioned countries.

One might expect that sanctions discourage investment by raising bilateral costs and uncertainty; however, the net effect on bilateral foreign direct investment (FDI) is not theoretically well-defined. Sanctions can deter cross-border deals by increasing investment-related risk and by directly impeding transactions, for example, through financial restrictions that limit international payments and credit. Trade restrictions can also discourage investment by disrupting access to imported inputs and complicating production and operations in the host country. Yet sanctions may also induce investment in specific circumstances, for example, when trade restrictions strengthen market-access motives for local production (tariff-jumping) or when the scope of restrictions becomes clearer, and uncertainty diminishes. Accordingly, whether sanctions reduce or stimulate bilateral FDI is ultimately an empirical question.

Against this backdrop, we empirically examine how economic sanctions affect cross-border mergers and acquisitions (M&As) from sanctioning to sanctioned countries. Leveraging the staggered timing of the imposition of sanctions across sender–target pairs, we estimate a gravity model using the Poisson pseudomaximum likelihood (PPML) estimator in a staggered difference-in-differences (DiD) framework and measure the average effect of sanctions on bilateral M&A activity, measured by the number of deals and total deal value. We then examine how the effect varies across event time, sanction start-year cohorts, and sectors.

Our results find a sizable contraction in bilateral cross-border M&A activity following the imposition of sanctions. In the baseline specification, we estimate that sanctions are associated with a roughly 50% decline in the expected number of deals. We also estimate a large negative effect on total deal value—approximately an 83% decline—although this estimate should be interpreted with caution given both the higher incidence of missing deal values and the less reassuring pre-trend evidence. The event-time estimates further suggest that the negative effect on the number of M&A deals persists and grows over time. In the specification with border–year controls, the estimated decline grows from approximately 17% in the first post-sanction period to approximately 68% by the sixth period after sanctions were imposed.

Beyond these average and dynamic patterns, we find substantial heterogeneity across sanction start-year cohorts and sectors. The cohort-specific estimates reveal pronounced variation in impact across episodes, likely reflecting the heterogeneity of the countries involved, sanction types, and other relevant factors. This finding suggests that evidence from a single sanctions episode may not generalize to others. Moreover, sectoral estimates show a larger contraction in Manufacturing, Mining & Energy than in Services, with estimated declines of approximately 62% and 45%, respectively.

Our contribution is twofold. First, we explicitly account for the staggered timing of the imposition of sanctions across directional sender–target pairs. In settings with staggered adoption and potentially heterogeneous treatment effects, conventional two-way fixed effects estimates can be difficult to interpret (see, e.g., de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). We therefore estimate a gravity model using PPML with an extended two-way fixed effects (ETWFE) estimator (Wooldridge, 2023; Nagengast and Yotov, 2025), which delivers well-defined cohort-specific effects and a transparent aggregation into an overall sanction effect. To our knowledge, existing empirical work on economic sanctions and foreign direct investment, including Carril-Caccia (2025), has not studied bilateral M&A-type FDI in a gravity setting using a staggered DiD estimator.

Second, we construct a directional country-pair–year panel of cross-border M&A activity from transaction-level Orbis data for 2006–2023 and merge it with comprehensive sanctions data, enabling to examine the impact of sanctions on bilateral cross-border M&A within a unified dyadic panel setting. Previous research has largely focused on specific sanction episodes—such as the 2014 Western sanctions against Russia and subsequent retaliatory measures—and/or on individual countries (Besedeš, Goldbach, and Nitsch, 2017, 2024; Flach et al., 2024; Jäkel, Østervig, and Yalcin, 2024; Kohl, van den Berg, and Franssen, 2024; Miromanova, 2023). However, because the effects of sanctions may vary depending on when they are imposed and which sectors are involved, it is important to examine not only the aggregate average effect but also variations across sanction cohorts and sectors within a unified framework. This study contributes to the FDI literature by documenting such heterogeneity through cohort-specific and sector-level analyses.

This study proceeds as follows. Section 2 reviews the related literature. Section 3 presents the conceptual framework and discusses mechanisms. Section 4 describes the data and sample construction, and Section 5 presents the empirical framework. We report the main results in Section 6 and conclude in Section 7.

2 Literature review

The literature on the economic effects of sanctions has expanded rapidly. In particular, the impact of sanctions on international trade has been well documented (Besedeš, Goldbach, and Nitsch, 2024; Dai et al., 2021; Felbermayr et al., 2020; Jäkel, Østervig, and Yalcin, 2024; Larch et al., 2022; Miromanova, 2023). For example, studies such as Felbermayr et al. (2020) and Larch et al. (2022) have demonstrated that complete sanctions significantly reduce bilateral trade flows—by approximately 44% in sectors such as mining—though these effects are highly heterogeneous across sanctioning coalitions and trade directions. Dai et al. (2021) analyzed the evolution of the effects of sanctions on international trade and identified "anticipatory effects" before official imposition and "post-sanction effects" that can persist for up to eight years after measures are lifted. Their research indicates that the trade-suppressing impact of sanctions tends to strengthen the longer they remain in force, with unilateral US sanctions often having a greater effect than multilateral UN measures.

Moreover, Jäkel, Østervig, and Yalcin (2024) and Miromanova (2023) analyzed micro-level analysis to understand firm-level responses, revealing that sanctions impede both the extensive margin (probability of exporting/importing) and the intensive margin (value per transaction). Miromanova (2023) highlighted that firm attributes, such as size and government connections, play a crucial role in determining a firm's ability to navigate or circumvent trade barriers. Furthermore, detailed product-level exemptions often create loopholes that mitigate the overall effectiveness of embargoes.

Expanding the scope to financial measures, Besedeš, Goldbach, and Nitsch (2024) demonstrated that financial sanctions cause "collateral damage" to the sender's own trade. Focusing on Germany, they found that while financial sanctions primarily target asset flows, they also reduce trade in goods and services.

Compared with evidence on trade, evidence on FDI is more limited. Besedeš, Goldbach, and Nitsch (2017) demonstrated that financial sanctions have a significant and immediate negative impact on cross-border capital flows, reducing bilateral transactions between the sender (Germany) and target countries by approximately 50%. This suppression is greater for capital outflows than for inflows. Kohl, van den Berg, and Franssen (2024) examined Dutch firms' responses to 2014 EU–Russia sanctions, finding no evidence of "sanctions jumping" to bypass export prohibitions. However, while EU restrictions did not drive divestment, they found that Russian retaliatory import bans significantly increased the probability that firms would close local affiliates.

Carril-Caccia (2025) is most closely related to this study. Using a gravity model, he analyzed the effects of sanctions on bilateral cross-border M&A for 125 origin and 148 host countries during the 1995–2019 period, finding that sanctions reduce bilateral cross-border M&A projects by 13.5%, with the impact peaking between the third and sixth years. The decline is greatest for sanctions imposed by coalitions (33.4%) or those targeting low- to middle-income countries. Unlike our study, however, Carril-Caccia (2025) did not fully account for the staggered nature of sanction implementation. This study addresses this gap using an advanced DiD identification strategy to provide a more robust analysis of the impact of sanctions on cross-border M&A.

3 Conceptual framework

Economic sanctions may either deter or stimulate bilateral FDI; thus, the sign of the effect is theoretically ambiguous (Biglaiser and Lektzian, 2011; Carril-Caccia, 2025). In this section, building on the review in Carril-Caccia (2025), we outline how sanctions may affect bilateral FDI.

Sanctions can reduce FDI through several channels. Sanctions and the accompanying deterioration in bilateral political relations can raise investment-related risks and uncertainties, such as regulatory and expropriation risks, thereby discouraging investment (e.g., Naghavi and Pignataro, 2015; Meyer et al., 2023). Additionally, investing in a sanctioned country can entail reputational and compliance costs for firms, potentially increasing entry barriers and deterring investment (e.g., Vadlamannati, Janz, and Berntsen, 2018).

Sanctions can also suppress FDI by directly impeding the execution of cross-border transactions. Financial sanctions restrict international financial transactions, such as payments, remittances, credit provision, and insurance, potentially making it more difficult to finance acquisitions and to move funds after an acquisition (e.g., Cipriani, Goldberg, and La Spada, 2023). Travel sanctions limit movement and can hinder local information gathering and face-to-face communication, increasing the costs of negotiations and due diligence (e.g., Neumayer, 2011; Tanaka, 2019). Trade sanctions restrict the import and export of goods. In particular, when intermediate inputs are disrupted, they can hinder production and operations, lowering the expected profitability of production and investment in the host country (e.g., Kelishomi and Nisticò, 2022; Kim et al., 2023).

The literature also identifies mechanisms through which sanctions can increase bilateral FDI. For example, when sanctions impose trade restrictions that restrict market access through exports, horizontal FDI may be encouraged (e.g., Motta, 1992; Blonigen, 2002) to preserve market access through local production (tariff-jumping FDI). Additionally, if the imposition of sanctions clarifies the scope of restrictions and reduces uncertainty, investment may rebound toward its pre-sanction level (Biglaiser and Lektzian, 2011).

In addition to bilateral effects, sanctions may also affect inward FDI into the sanctioned country as a whole. For example, sanctions can reduce the profitability of investment by worsening macroeconomic conditions, thereby depressing inward FDI (e.g., Mirkina, 2018; Gutmann, Neuenkirch, and Neumeier, 2023). At the same time, if sanctions result in declines in firm valuations or asset prices, inward FDI may increase through discounted acquisition opportunities (fire sales) (e.g., Krugman, 2000; Aguiar and Gopinath, 2005). However, these factors are largely common to the host country as a whole. Accordingly, in our bilateral setting, they are primarily absorbed by host–year fixed effects and related controls (see Section 5).

To summarize, sanctions can increase bilateral investment costs and thereby discourage FDI, while also encouraging FDI through market-access motives under trade restrictions and related channels. Because these opposing forces can operate simultaneously, the sign and magnitude of the net effect of sanctions are not uniquely determined ex ante. Motivated by these considerations, we empirically examine how economic sanctions affect bilateral FDI from the sanctioning to the sanctioned country, with a focus on cross-border M&A.

4 Data

This section describes the key variables used in the analysis, including the bilateral FDI outcome variables and the economic sanctions treatment variable, and introduces the estimation dataset.

4.1 FDI

FDI variables are constructed from transaction-level data in Orbis M&A. Orbis M&A is an M&A deals database provided by Moody's that records acquisition and merger transactions between firms across countries at the deal level. Each deal contains information on the home countries of the acquiring firm (acquirer) and the acquired firm (target), the announcement and completion dates, and a status variable indicating the transaction's progress (e.g., completed, pending, withdrawn). Additionally, for the firms involved, the database provides firm characteristics, including industry classifications (e.g., NACE and SIC). For some deals, information on deal characteristics, such as transaction value, is also available.

Since the information is observed at the deal level, we aggregate it to the investor–host–year level. Specifically, we define the investor country i as the acquirer's home country and the host country j as the target's home country. Under this definition, $i = j$ corresponds to domestic deals, while $i \neq j$ corresponds to cross-border deals. Using the announced date to define the transaction year t, we construct FDI^N_{ijt}, the number of deals, and FDI^V_{ijt}, the total deal value by summing the deal values within each investor–host–year cell.

We restrict our observation period to 2006–2023. Under the sample constructions described in Section 4.3, treatment (economic sanctions) adoption cohorts are primarily concentrated after 2011. Therefore, we begin the sample in 2006 to ensure a sufficiently long pretreatment window even for the earliest treatment cohorts, allowing us to assess pre-trends and estimate dynamic effects in the event-study framework. We end the sample in 2023 because the sanctions database covers treatment information only through that year.

We impose several restrictions on the set of deals used for aggregation. First, to identify the investor country i and the host country j, we restrict the sample to deals in which the home countries of both the acquiring and target firms are nonmissing. Among all deals from 2006 onward (2,362,830 deals), both country identifiers are observed for 1,399,398 deals (59.23%). Missingness is driven primarily by cases in which the acquirer country is not recorded (911,417 deals; 38.57%), while it is much less frequent on the target side (49,709 deals; 2.10%); deals missing both countries are rare (2,306 deals; 0.10%).

Second, we exclude deals involving multiple countries on either the acquirer or target side because assigning such deals to country pairs and apportioning deal values would require additional aggregation rules that could introduce arbitrariness. We therefore retain only deals for which the investor and host countries can each be uniquely identified as single countries. Third, to focus on transactions that have been completed or are highly likely to proceed to completion, we restrict the sample to deals with the statuses of "Announced," "Completed," or "Completed Assumed," and exclude those labeled "Rumor," "Withdrawn," "Pending," and similar statuses. Applying these criteria yields 1,226,203 deals, which corresponds to 87.62% of deals with both country identifiers observed and approximately 51.90% of all deals from 2006 onward. Notably, Orbis M&A does not necessarily capture the full universe of M&A activity, and its coverage may vary across countries and over time.[1]

We then aggregate the restricted deal-level sample to a country-pair–year dataset. In doing so, we construct a balanced country-pair–year panel that includes all combinations of country pairs (i, j) and years t in the analysis period and assign zeros to the number of deals FDI^N_{ijt} and the total deal value FDI^V_{ijt} for country-pair–year cells with no recorded deals. The final estimation sample may be unbalanced across country pairs and years once additional sample-

[1]Notes on the coverage and representativeness of Orbis data are summarized in Bajgar et al. (2020).

selection criteria in Section 4.3 are imposed.

Finally, we briefly note another FDI measure available in Orbis (greenfield FDI) and describe an auxiliary trade outcome that complements the baseline M&A-based analysis. Orbis Crossborder Investment provides information on greenfield (GF) FDI. However, coverage in this dataset effectively begins in 2013, and observations before 2012 appear largely backfilled, raising concerns about time-series consistency. Thus, we do not use GF FDI as an outcome, focusing instead on M&A-type FDI based on the Orbis M&A dataset.

Although our main focus is M&A-type FDI, we also report bilateral trade flows as an auxiliary outcome. Since previous sanctions studies often found that sanctions depress trade, examining trade in our setting helps assess whether our estimates align with this established pattern. Bilateral exports are drawn from International Trade and Production Database for Estimation (ITPD-E) Release 3 (Borchert et al., 2021; Borchert et al., 2025). Because ITPD-E coverage ends in 2022, regressions with exports as the dependent variable are estimated over 2006–2022, which is one year shorter than the baseline sample used for FDI outcomes (2006–2023).

4.2 Sanction

Sanctions variables are constructed from the Global Sanctions Data Base (GSDB; Release 4). GSDB is a database compiled by Felbermayr et al. (2020), Syropoulos et al. (2024), and Yalcin et al. (2025), which systematically records annual information on sanctions episodes defined at the sender–target country-pair level, including the start and end years of each sanction. The database covers the period 1950–2023. It also provides sanction types (e.g., trade sanctions, financial sanctions)[2], as well as information on objectives, whether a sanction is imposed by multiple countries (i.e., multilateral sanctions), and indicators of sanction success or failure. In this study, we use GSDB's sender–target dyadic data to construct investor–host–year sanctions variables.

Using this database, we assign each investor–host pair to a cohort according to the first year in which it becomes sanctioned. We proceed in two steps. First, we define sanction status at the investor–host–year level. An investor–host–year cell (i, j, t) is considered sanctioned if sanctions imposed by country i on country j are in place in year t. In doing so, we do not distinguish among the types of sanctions recorded in the GSDB. Instead, we focus only on whether any sanctions are in place for a given country pair in a given year. Distinguishing the effects of specific sanction types or combinations would be policy relevant. In our sample period, however,

[2]The classification consists of six categories: trade, financial, arms, military assistance, travel, and other sanctions.

a more disaggregated treatment definition would leave too little variation to identify such effects separately.[3] Therefore, we abstract from the sanction type in the baseline analysis and interpret our estimates as capturing the average effect of sanctions under this broader definition.

Second, we align the treatment histories with the absorbing-treatment structure typically assumed in staggered DiD settings. Therefore, we restrict attention to investor–host pairs whose sanctions, once they begin, continue through the end of the sample period (2023). Pairs for which sanctions are lifted during the sample period are excluded. At the same time, we treat sanctions as continuous when one sanctions episode ends and another begins for the same country pair, without a gap in the annual data. This covers cases in which an additional sanction type is added to an existing episode and those in which one sanction ends while another remains in place. For the country pairs retained in the sample, the cohort is then defined as the first year in which the pair becomes sanctioned.

We then construct dummy variables $D_{ijt}^{g,s}$ indexed by cohort g (i.e., the treatment adoption year) and relative year s.[4] Specifically, $D_{ijt}^{g,s} = 1$ if country pair (i, j) first becomes sanctioned in year g and the observation year t is s, and $D_{ijt}^{g,s} = 0$ otherwise. For country pairs that are never treated, $D_{ijt}^{g,s} = 0$ for all g and s.

4.3 Sample construction

This section describes how we construct the estimation sample. We outline the procedures for merging sanction information into the country-pair–year panel of outcomes (M&A-type FDI and exports) and then describe the sample-selection criteria used to obtain the final dataset for the empirical analysis.

Because GSDB records sanctions as directed sender–target pairs, we set the pair direction so that i is the sanctioning (sender) country and j is the sanctioned (target) country, and then merge GSDB to the M&A country-pair–year panel (i, j, t) where i and j correspond to the investor and host countries. We next exclude deals in which either the investor or the host country is classified as a tax haven, according to the tax-haven list in Tørsløv, Wier, and Zucman (2022).[5]

[3] Sanctions in our sample period vary considerably in their composition: some episodes involve a single type, whereas others combine multiple types, and the combinations differ across episodes. As a result, defining treatment by sanction type or combination would generate many small treatment cohorts.

[4] Here, s is indexed in calendar years, but is interpreted relative to cohort g: for each cohort, we construct a separate dummy for each year from its treatment adoption year onward. For example, for the 2014 cohort, we define a full set of indicators with $s = 2014, 2015, \ldots, 2023$, corresponding respectively to the treatment year and each subsequent posttreatment year for that cohort.

[5] We implement this exclusion using code kindly shared by Haruka Takayama.

Moreover, we impose two additional restrictions to ensure stable estimation by excluding degenerate cohort–year cells with no deal activity and degenerate pair cells with no deal activity. First, we restrict the sample to treatment cohorts that exhibit at least some M&A activity in every calendar year of the estimation window. For treated country pairs (i, j), let g_{ij} denote the first year in which (i, j) becomes sanctioned. For each cohort g and relative year s, we compute the cohort–year total number of M&A deals as $N_{gs} \equiv \sum_{i,j} \mathbf{1}\{g_{ij} = g\}\,\mathrm{FDI}^{N}_{ijs}$, and we keep cohort g only if $N_{gs} > 0$ for all s in the estimation window. In our baseline sample, this criterion excludes the $g \in \{2007, 2008, 2009, 2010, 2012, 2013, 2015, 2016, 2017, 2018, 2020\}$ cohorts.[6] Second, regardless of treatment status, we drop country pairs (i, j) whose total number of M&A deals over the entire sample period is 0. These restrictions can limit the range of cases in which the outcome is concentrated at zero for particular cohorts or pairs, making PPML estimation and inference unstable.

After applying these restrictions, the final estimation sample comprises 50,454 country-pair–year observations. The set of treatment cohorts in the estimation sample is as follows. Of these observations, 45,942 (91.06%) are never treated. Among treated pairs, the 2014 cohort is the largest (2,077; 4.12%), followed by 2022 (843; 1.67%), 2021 (489; 0.97%), 2011 (485; 0.96%), 2019 (348; 0.69%), and 2023 (270; 0.54%).

Table 1 reports summary statistics for the dependent variables in the estimation sample. Two features are worth noting. First, both deal counts (FDI^{N}_{ijt}) and deal values (FDI^{V}_{ijt}) are highly right-skewed, with a large mass at zero (the median is zero in both cases), which is common in annual bilateral M&A data. Second, deal values are available for fewer observations than deal counts, indicating that transaction values are not reported for all deals in Orbis. These distributional features also motivate our use of PPML, as it naturally accommodates zero outcomes and is well-suited to highly skewed flows.

5 Empirical framework

In this study, following Nagengast and Yotov (2025), we estimate a gravity model using a staggered DiD framework to examine the effects of economic sanctions on bilateral economic activity.

[6] Since our sample starts in 2006, the cohort with first sanction year $g = 2006$ forms an always-treated group (it is treated in all sample years and thus has no pretreatment observations). In a staggered DiD design, such an always-treated group does not contribute to identification and is, therefore, excluded from the estimation sample.

Table 1: Summary statistics

	N	Mean	SD	P50	Min	Max
FDI_{ijt}^{N}	50454	21.014	342.514	0.000	0.000	19591.000
FDI_{ijt}^{V}	38553	1.512e+12	3.247e+13	0.000	0.000	1.932e+15
X_{ijt}	50658	9300.758	166651.294	448.886	0.000	14831440.700

Note: This table reports summary statistics for the country-pair–year estimation sample used in the baseline analysis. The unit of observation is the directed pair (i, j) in year t, where i is the investor (exporter) country and j is the host (importer) country. FDI_{ijt}^{N} is the number of cross-border M&A deals from i to j in year t (deal counts). FDI_{ijt}^{V} is the total value of those deals in USD. X_{ijt} denotes exports from i to j in year t.

Specifically, we implement the ETWFE approach and estimate the following model by PPML:

$$Y_{ijt} = \exp\left(\sum_{g=q}^{T}\sum_{s=g}^{T}\delta_{gs}D_{ijt}^{g,s} + \mu_{it} + \mu_{jt} + \mu_{ij} + \theta_{iit}\right)\varepsilon_{ijt}, \tag{1}$$

where Y_{ijt} is the outcome variable, such as FDI from country i to country j in year t (deal counts or values) or bilateral trade flows from i to j in t. The indicator $D_{ijt}^{g,s}$ equals one if country pair (i, j) belongs to treatment cohort g (i.e., the pair's first sanction year is g) and the observation year is s (i.e., $t = s$), and zero otherwise. q denotes the earliest treatment adoption year (i.e., the first treatment cohort) in the sample. The coefficient δ_{gs} captures the treatment effect for cohort g in relative year s, thereby tracing out the dynamic pattern of treatment effects.

We include three-way fixed effects: investor–year fixed effects μ_{it}, host–year fixed effects μ_{jt}, and directional country-pair fixed effects μ_{ij} (i.e., investor–host pair fixed effects). μ_{it} and μ_{jt} absorb country-specific shocks in each year. For example, if war or political instability in host country j increases uncertainty and leads to a decline in investment from all origins, this host–year shock common to all pairs involving j in year t is absorbed by μ_{jt}. Similarly, a recession or a deterioration in financing conditions in investor country i that reduces outward investment more generally is absorbed by μ_{it}. In turn, directional pair fixed effects μ_{ij} control for time-invariant bilateral determinants, such as distance, common language or colonial ties, institutional or cultural proximity, and historical links. Accordingly, identification in our setting comes from relative changes in directed bilateral flows around the adoption of sanctions, after netting out country–year shocks on each side and time-invariant directed pair factors.

Furthermore, we control for θ_{iit}, a full set of border-year indicators defined as year dummies interacted with an indicator for cross-border transactions. Our baseline specification includes both domestic and cross-border transactions, which is consistent with the structural gravity literature (e.g., Yotov et al., 2016). In this setting, the control group includes not only untreated cross-border transactions but also of domestic deals; thus, the estimated sanction effect is identified in part by comparing treated cross-border transactions with domestic deals. If domestic and cross-border transactions follow different time patterns, failing to account for them could confound the estimated sanction coefficients. The border-year indicators are included to capture these time-varying differences, i.e., the evolution of the border effect.[7]

To interpret δ_{gs} as causal effects, we primarily require the no-anticipation and parallel trends assumptions, as discussed in Nagengast and Yotov (2025). The former requires that outcomes do not change in anticipation of sanctions before they actually begin—i.e., that the pretreatment coefficients do not exhibit systematic movements. The latter requires that, absent sanctions, the treated group's potential outcomes can be inferred from the control group's evolution (i.e., the two groups would follow similar trends in the absence of treatment), which is essential for constructing the counterfactual for the treated group. In this study, we assess the plausibility of these assumptions by comparing pretreatment dynamics between treated and control groups and verifying whether pre-trends (lead coefficients or relative effects before treatment) are close to zero.

To facilitate interpretation, we summarize the estimated treatment effects in three ways: as a single overall posttreatment effect, as event-time-specific average effects, and as cohort-specific average effects. First, we report an aggregate treatment-effect estimate that compresses all posttreatment effects into one statistic. As in Nagengast and Yotov (2025), it is computed as a weighted average of the posttreatment δ_{gs}, with weights proportional to the number of treated observations in each cohort–year cell. Second, for each event time, we compute an average treatment effect by taking a weighted average of all δ_{gs} that belong to that event time. These estimates capture how the effects of sanctions evolve after treatment onset. Third, for each cohort g, we compute a cohort-specific average treatment effect by taking a weighted average of the posttreatment δ_{gs} within that cohort. These estimates allow us to compare sanction effects across treatment cohorts.

[7] See also Carril-Caccia (2025), who explains the role of domestic transactions and border-year controls in a similar setting.

6 Results

6.1 Assessing Pretreatment Trends

Following Nagengast and Yotov (2025), we examine pretreatment patterns using an event-study-style diagnostic based solely on untreated observations before turning to the main estimates. Specifically, we estimate cohort–relative-year placebo effects for presanction periods, using the last presanction year for each cohort as the reference category, and summarize them into event-time-specific average effects. These placebo coefficients summarize whether country pairs that will later be sanctioned already exhibit systematic differences in outcomes before treatment. Following Roth (2026), we present these pretreatment estimates separately from the posttreatment effects.

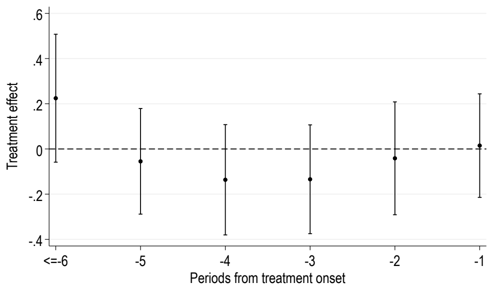
Figure 1 reports the estimated pretreatment coefficients for deal counts, deal values, and trade flows in event time relative to treatment. The coefficient for the period immediately before treatment is set to zero as the reference category; thus, the coefficient labeled -1 in the figure corresponds to two periods before treatment. Overall, border-year controls attenuate some of the apparent pretreatment deviations, but do not eliminate them.

For deal counts, the specification without border–year controls shows no individually significant lead coefficients, although the joint test rejects the null of no pretreatment differences. With border-year controls, the full-window joint test also rejects the null hypothesis, driven by a positive and statistically significant coefficient in the most distant lead-year bin. However, when the joint test is restricted to the coefficients labeled -1 through -5 in the figure, the null is no longer rejected ($p = 0.81$). Thus, for deal counts, the main concern is concentrated in the most distant pretreatment period rather than in those closer to the imposition of sanctions.

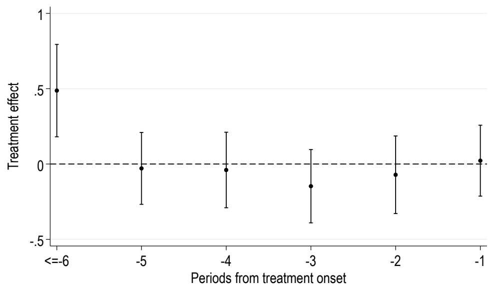
For deal values, the specification without border–year controls shows significant positive coefficients at -2 and -3, and the joint test rejects the null of no pretreatment differences. With border-year controls, only the coefficient at -2 is individually significant, although the joint test continues to reject. This suggests that border-year controls mitigate, but do not fully remove, pretreatment differences in deal values.

For trade flows, the specification without border-year controls shows significant negative coefficients from -2 through the most distant lead bin in the figure, and the joint test rejects the null. Once border-year controls are included, none of the individual lead coefficients is statistically significant. However, the joint null hypothesis is still rejected, indicating that systematic pretreatment differences persist even though the individual tests are not significant.

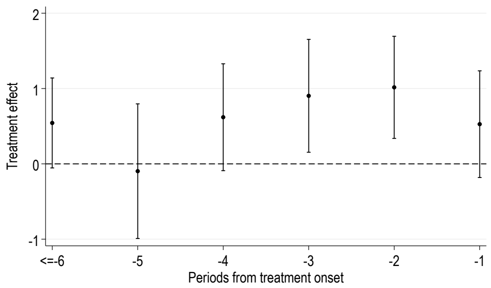
Taken together, these diagnostics suggest that border-year controls attenuate some of the



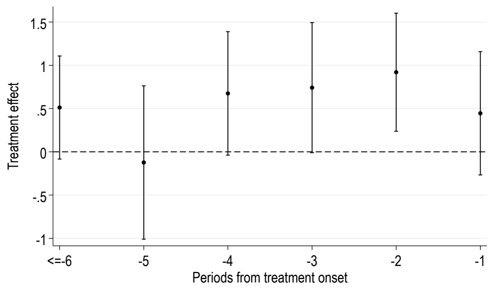
(a) FDI^N_{ijt} (without θ_{iit})



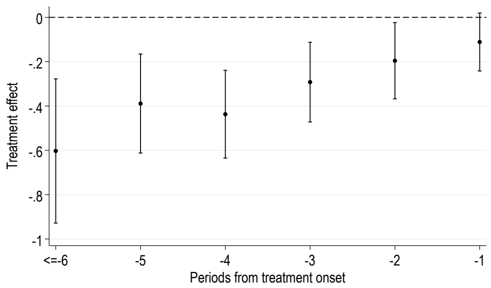
(b) FDI^N_{ijt} (with θ_{iit})



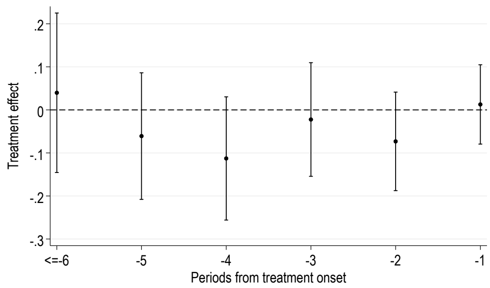
(c) FDI^V_{ijt} (without θ_{iit})



(d) FDI^V_{ijt} (with θ_{iit})



(e) X_{ijt} (without θ_{iit})



(f) X_{ijt} (with θ_{iit})

Figure 1: Pretreatment diagnostic

Note: This figure reports an event-study-style pretreatment diagnostic constructed from cohort–relative-year effects estimated under the staggered DiD gravity framework of Nagengast and Yotov (2025). We first estimate cohort–relative-year coefficients δ_{gs} using only untreated observations by including cohort-by-year placebo indicators for presanction years. We then aggregate these estimated placebo coefficients by relative event time $k = s - g$ (with $k < 0$) and plot the resulting pretreatment estimates. The omitted reference period is the year immediately preceding the imposition of sanctions ($k = -1$). Leads are binned so that $k \leq -6$ is pooled into a single category. Panels labeled "with θ_{iit}" include border-year controls (a border indicator interacted with year fixed effects), whereas panels labeled "without θ_{iit}" exclude these controls. All specifications include directed country-pair fixed effects and investor–year (exporter–year) and host–year (importer–year) fixed effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the directed country-pair level.

apparent pretreatment deviations, but do not eliminate them. For deal counts, the remaining concern is largely concentrated in the most distant lead bin. In contrast, for deal values and trade flows, the evidence of pretreatment differences remains more persistent. In what follows, we therefore report specifications with and without border-year controls for comparison, and interpret the posttreatment estimates with these pretreatment patterns in mind.

6.2 Baseline Results

We begin with a single aggregate estimate that summarizes the posttreatment effects in a single parameter. Table 2 reports the corresponding PPML gravity estimates with directed origin–destination pair fixed effects and origin–year and destination–year fixed effects. The coefficient on Sanction_{ijt} should be interpreted as a multiplicative effect on the outcome, with $(\exp(\beta)-1)\times 100$ giving the implied percentage change.

Table 2 shows that sanctions are associated with economically large declines in bilateral cross-border M&A activity. For deal counts, the estimated coefficient is -0.462 in Column (1), implying approximately a 37% decline ($e^{-0.462} - 1 \approx -0.37$) in the expected number of deals, and -0.687 in Column (2), implying an approximately 50% decline once border-year controls are included. For deal values, the estimated coefficients are -1.763 and -1.782 in Columns (3) and (4), respectively, corresponding to reductions of roughly 83% in expected aggregate deal value in both specifications.

For trade flows, the estimated effect is more sensitive to specification. Without border-year controls, the estimate is close to zero and statistically insignificant in Column (5). With border-year controls, however, the estimated coefficient is -0.528 in Column (6), implying an approximately 41% decline in expected bilateral exports. Given the pretreatment diagnostics reported above, we regard the specification with border-year controls as the more informative benchmark for trade.

Overall, the baseline estimates indicate that sanctions reduce bilateral cross-border M&A activity and, in the preferred specification, bilateral trade flows as well. The effects are negative and statistically significant for both M&A deal counts and deal values, with especially large magnitudes for the latter. These findings are broadly consistent with previous evidence that sanctions reduce cross-border economic exchange, including trade and foreign investment flows (e.g., Le and Bach, 2022, Nguyen and Ahmed, 2023, Carril-Caccia, 2025, Afesorgbor, 2019, Felbermayr et al., 2020).

Table 2: Effect of Sanctions (Single Treatment Effect)

Dependent var.:	FDI^N_{ijt}		FDI^V_{ijt}		X_{ijt}	
	(1)	(2)	(3)	(4)	(5)	(6)
$Sanction_{ijt}$	-0.462**	-0.687**	-1.763**	-1.782**	0.008	-0.528**
	(0.108)	(0.115)	(0.283)	(0.276)	(0.098)	(0.074)
Num. Obs.	50,454	50,454	38,553	38,553	50,658	50,658
Origins	130	130	115	115	128	128
Destinations	139	139	125	125	135	135
Years	18	18	18	18	17	17
Coefficients	34	34	34	34	28	28
Origin–Destination FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin–Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Destination–Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Border–Year FE		Yes		Yes		Yes

Note: This table reports a single aggregate posttreatment effect of sanctions constructed from cohort–relative-year treatment effects estimated in a staggered DiD gravity model following Nagengast and Yotov (2025). We first estimate cohort–relative-year effects δ_{gs}, where cohort g is defined by a pair's first sanction year and s denotes the relative year. We then summarize the posttreatment effects into a single statistic, a weighted average of $\hat{\delta}_{gs}$ for $s \geq g$, with weights proportional to the number of treated country-pair–year observations in each cohort–relative-year cell (normalized to sum to 1). The coefficient reported in the row labeled $Sanction_{ijt}$ corresponds to this aggregate estimate, and standard errors are obtained using the delta method. The dependent variables are: FDI^N_{ijt} (number of cross-border M&A deals from i to j in year t), FDI^V_{ijt} (aggregate deal value in USD), and X_{ijt} (exports from i to j). All specifications include directed origin–destination pair fixed effects and origin–year and destination–year fixed effects. Columns with "Border–Year FE = Yes" additionally control for border-year effects (a border indicator interacted with year fixed effects). Standard errors are reported in parentheses, clustered at the directed country-pair level. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

6.3 Robustness Checks

We next assess whether the aggregate treatment effects reported in Table 2 are robust to alternative sample constructions. First, we re-estimate the model after excluding domestic deals from the sample. Because domestic transactions are no longer present in this specification, border–year controls cannot be included. Second, we relax the baseline sample restriction that drops cohorts with zero aggregate M&A activity in some calendar years and instead estimate the model using a broader set of available cohorts.[8]

Table 3: Robustness Checks for Aggregate Sanction Effects

	Exclude domestic deals			All cohorts		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var.:	FDI^N_{ijt}	FDI^V_{ijt}	X_{ijt}	FDI^N_{ijt}	FDI^V_{ijt}	X_{ijt}
$\mathrm{Sanction}_{ijt}$	-0.609**	-1.642**	-0.291**	-0.852**	-1.765**	-0.538**
	(0.161)	(0.315)	(0.039)	(0.111)	(0.270)	(0.072)
Num. Obs.	45,570	34,120	48,835	50,859	38,781	51,117
Origins	122	105	122	130	115	128
Destinations	135	122	134	140	126	136
Years	18	18	17	18	18	17
Coefficients	34	34	28	83	83	71
Origin–Destination FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin–Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Destination–Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Border–Year FE				Yes	Yes	Yes

Note: This table reports robustness checks for the aggregate posttreatment sanction effect using the same staggered DiD gravity framework as in Table 2. Columns (1)–(3) exclude domestic transactions from the sample; because domestic observations are absent, border-year controls are not included in these specifications. Columns (4)–(6) use a broader set of available sanction cohorts than in the baseline specification while retaining border-year controls. In each case, the reported coefficient is a weighted average of estimated cohort–relative-year treatment effects for posttreatment periods, with weights proportional to the number of treated country-pair–year observations in each cohort–relative-year cell. Standard errors are obtained using the delta method and are clustered at the directed country-pair level. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

[8]In practice, this specification includes the 2011, 2013, 2014, and 2017–2023 cohorts. When earlier cohorts (2007–2010) are added, the variance matrix becomes singular or nonsymmetric; thus, standard errors cannot be computed.

Table 3 reports the results. In both alternative samples, the estimated sanction effects on M&A remain negative and statistically significant. The magnitudes are also broadly similar to those in the baseline specification, indicating that the main finding of a contraction in bilateral cross-border M&A activity is not driven by the treatment of domestic transactions or by the baseline cohort-selection rule. For trade, the estimated effect is also negative in both robustness checks.[9] Overall, these results suggest that the negative effects of sanctions on bilateral M&A and trade are not driven by the specific sample construction used in the baseline analysis.

6.4 Heterogeneity in Sanction Effects

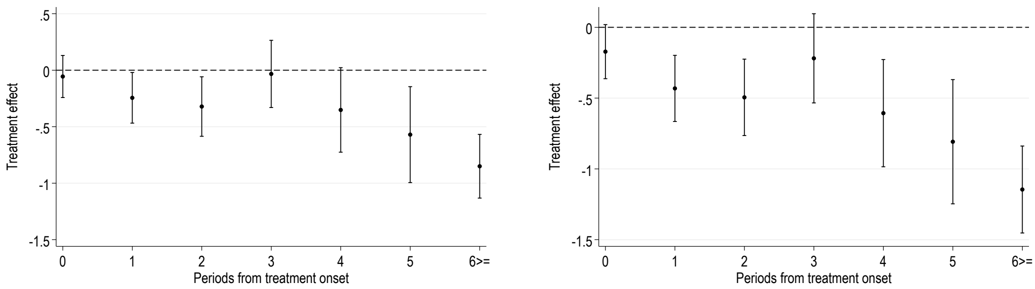
To move beyond the aggregate estimates reported above, we next examine heterogeneity in the estimated sanction effects along three dimensions: event time, treatment cohorts, and sectors. We focus on M&A deal counts, since the larger number of missing observations for transaction values makes heterogeneity analysis based on deal values less informative. We begin with event-time dynamics, then turn to cohort-specific effects, and finally consider sectoral heterogeneity.

Figure 2 plots the estimated treatment effects by event time for M&A deal counts in the specifications without (left panel) and with (right panel) border-year controls.[10] In both specifications, the posttreatment coefficients are negative in most periods, and the estimated effects become larger in magnitude over time. Without border-year controls, the estimated effect is close to zero in the treatment period and reaches approximately -0.85 by period 6, corresponding to a decline of roughly 57% in expected deal counts. With border-year controls, the decline is somewhat larger, ranging from approximately -0.17 in period 0 to approximately -1.15 by period 6, implying a reduction of approximately 68%. These estimates indicate that sanctions are associated with a persistent decline in cross-border M&A activity that intensifies over time.

Figure 3 reports cohort-specific ATTs. Across both specifications, most cohort-specific point estimates are negative, indicating that cross-border M&A activity tends to decline after sanc-

[9] In the specification that excludes domestic deals, border–year controls can no longer be included because domestic observations are removed from the sample. Although this estimate might be expected to be closer to the baseline result without border–year controls, it is closer to the baseline result with border–year controls and remains negative and statistically significant. A possible explanation is that, in the baseline sample, domestic transactions are part of the control group; thus, without border–year controls, the estimated sanction effect may partly reflect time-varying differences between domestic and cross-border transactions. Once domestic deals are excluded, the comparison is restricted to treated and untreated cross-border transactions, which may make the negative bilateral trade effect more visible.

[10] Caution is warranted for the longer-horizon event-time estimates. The average treatment effects on the treated (ATT) estimates for event time 5 and later are based only on the 2011 and 2014 cohorts. These coefficients should, therefore, be interpreted as reflecting a narrower set of sanction episodes than the earlier event-time estimates.

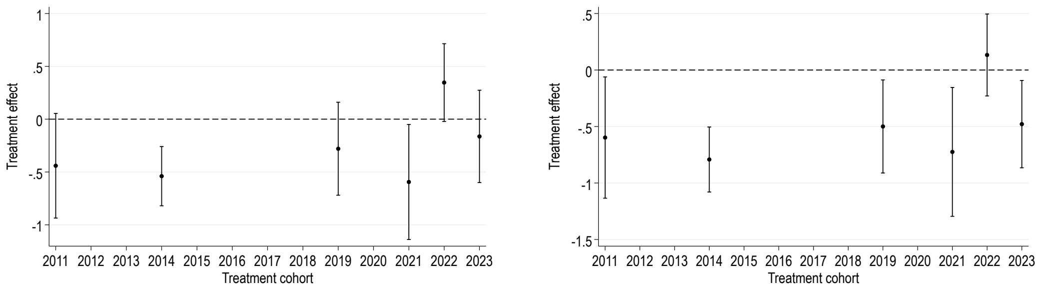


(a) Without θ_{iit}

(b) With θ_{iit}

Figure 2: Dynamic effects by event time

Note: This figure plots event-time-specific treatment effects for FDI^N_{ijt} (deal counts) estimated in the staggered DiD gravity framework of Nagengast and Yotov (2025) using PPML. Event time is defined relative to a pair's first sanction year, where 0 denotes the sanction year. We first estimate cohort–relative-year coefficients δ_{gs}, where cohort g is defined by the first sanction year and s denotes relative year, and then compute event-time-specific estimates by taking weighted averages across cohorts for each event time. The left panel excludes border-year controls, while the right panel includes θ_{iit}. All specifications include directed country–pair fixed effects and investor–year and host–year fixed effects. Dots denote point estimates and bars denote 95% confidence intervals. Standard errors are clustered at the directed investor–host country level.



(a) Without θ_{iit}

(b) With θ_{iit}

Figure 3: Sanction effects by cohort

Note: This figure plots cohort-specific ATTs by sanction start year g for FDI^N_{ijt} (deal counts) estimated in the staggered DiD gravity framework of Nagengast and Yotov (2025) using PPML. We first estimate cohort–relative-year coefficients δ_{gs}, where cohort g is defined by a pair's first sanction year and s denotes relative year. For each cohort g, we then construct the cohort-specific ATT as a weighted average of the posttreatment coefficients $\hat{\delta}_{gs}$ for $s \geq g$, with weights proportional to the number of treated country-pair–year observations in each cohort–relative-year cell. The left panel excludes border-year controls, while the right panel includes θ_{iit}. All specifications include directed country–pair fixed effects and investor–year and host–year fixed effects. Dots denote point estimates and bars denote 95% confidence intervals; inference is based on the delta method. Standard errors are clustered at the directed investor–host country level.

tions are adopted. At the same time, the estimates vary meaningfully across cohorts in both statistical significance and economic magnitude. In the specification without border-year controls, statistically significant declines are concentrated in the 2014 and 2021 cohorts, while the 2022 cohort shows a positive estimate. Once border-year controls are included, negative effects are statistically significant for several cohorts, including 2011, 2014, 2019, 2021, and 2023.

The magnitudes also differ substantially across adoption-year cohorts. Without border-year controls, the implied percentage change in expected deal counts ranges from approximately −45% to +41%. With border-year controls, the corresponding range is approximately −55% to +14%. Thus, the average sanction effect masks substantial cross-cohort heterogeneity.

The statistically insignificant estimate for the 2022 cohort warrants careful interpretation. The lack of a significant negative effect may seem counterintuitive given the escalation of sanctions and retaliatory measures between Western countries and Russia that year. However, this result is driven by our methodology. Since we group sender–target pairs by the year of the initial adoption of sanctions, many countries active in 2022 are already captured by the 2014 cohort—the year Western countries first imposed sanctions following the Russian annexation of Crimea. Consequently, in our analysis, the 2014 cohort absorbs the impact of these major Western sanctions and Russian counter-responses, rather than the 2022 cohort.

Finally, we turn to industry heterogeneity. Using the target firm's BvD primary industry code, we classify each deal into Agriculture, Manufacturing, Mining & Energy, or Services and aggregate transactions to the investor–host–year level separately for each sector.[11] Because Agriculture accounts for very few cross-border deals in our sample, we focus on Manufacturing, Mining & Energy and Services and re-estimate the baseline specification separately for each sector.

Table 4 reports sector-specific estimates for M&A deal counts. Sanctions reduce bilateral cross-border M&A activity in both Manufacturing, Mining & Energy and Services. For Manufacturing, Mining & Energy, the estimated coefficient is −0.757 in Column (1), indicating an average decline of approximately 53%, and −0.971 in Column (2), indicating a decline of approximately 62% once border–year controls are included. For Services, the corresponding estimates are −0.325 and −0.592, indicating declines of approximately 28% and 45%, respectively.

Collectively, these estimates indicate that the negative effect of sanctions is present in broad sectors yet is larger in Manufacturing, Mining & Energy than in Services.

[11] The industry classification table is presented in Appendix A.

Table 4: Effect of Sanctions by Sector

Dependent var.:	FDI^N_{ijt}			
Sector:	Manufacturing, Mining & Energy		Services	
	(1)	(2)	(3)	(4)
Sanction_{ijt}	-0.757**	-0.971**	-0.325**	-0.592**
	(0.195)	(0.191)	(0.105)	(0.116)
Num. Obs.	51,370	51,370	39,623	39,623
Origins	120	120	119	119
Destinations	133	133	128	128
Years	18	18	18	18
Coefficients	34	34	34	34
Origin–Destination FE	Yes	Yes	Yes	Yes
Origin–Year FE	Yes	Yes	Yes	Yes
Destination–Year FE	Yes	Yes	Yes	Yes
Border–Year FE		Yes		Yes

Note: This table reports sector-specific estimates of the effect of sanctions on the number of bilateral cross-border M&A deals, FDI^N_{ijt}, using the ETWFE gravity framework of Nagengast and Yotov (2025) estimated by PPML. Deals are classified into sectors using the target firm's BvD primary industry code and then aggregated by sector to the directed country-pair–year level. For each sector, we first estimate cohort–relative-year treatment effects δ_{gs} and then summarize posttreatment effects into a single statistic as a weighted average of $\hat{\delta}_{gs}$ for $s \geq g$, with weights proportional to the number of treated country-pair–year observations in each cohort–relative-year cell within the sector. Accordingly, the coefficient reported in the row labeled Sanction_{ijt} corresponds to this sector-specific aggregate posttreatment estimate, and standard errors are obtained using the delta method. All specifications include directional origin–destination pair fixed effects and origin–year and destination–year fixed effects. Columns (2) and (4) additionally control for border-year effects (a border indicator interacted with year fixed effects). Standard errors are reported in parentheses, clustered at the directed country-pair level. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

6.5 Discussion

This section shows that economic sanctions are associated with a contraction in bilateral cross-border M&A activity. Sanctions reduce both the number and the total value of bilateral M&A deals across specifications. At the same time, the pretreatment diagnostics indicate that the magnitudes should be interpreted with caution, especially for deal values. As auxiliary evidence, the trade outcome points to a decline in bilateral exports in the specification with border-year controls.

The analysis further shows that the average effect masks important heterogeneity. Event-time estimates indicate that the negative effect on M&A deal counts increases over time. Cohort-specific estimates vary meaningfully across sanction episodes in both statistical significance and economic magnitude. Sectoral estimates show that the contraction is larger in Manufacturing, Mining & Energy than in Services.

At the same time, the analysis does not identify the precise mechanisms underlying these effects. Moreover, the outcomes studied here capture the formation of new cross-border M&A deals rather than the stock of existing foreign investment. The results should, therefore, be interpreted as evidence that sanctions weaken the formation of new bilateral investment linkages, not as evidence on FDI stock, disinvestment, or firm exit. Nor do we decompose the effects by sanction characteristics, such as sanction object, sanction type, or whether the sanctioning side is multilateral.

Considering these caveats, the overall picture is clear: economic sanctions tend to weaken the formation of new bilateral cross-border M&A linkages, and the magnitude of this effect varies across time, cohorts, and sectors.

7 Conclusion

This paper examined how economic sanctions affect bilateral FDI, with a focus on cross-border mergers and acquisitions. As discussed in Section 3, the net effect of sanctions on bilateral FDI is theoretically ambiguous. Sanctions may deter investment by raising uncertainty, compliance costs, and transaction frictions, but they may also encourage investment through channels such as market-access motives under trade restrictions. Given these competing forces, the sign and magnitude of the overall effect are ultimately an empirical question.

To address this question, we used transaction-level data from Orbis M&A to construct an investor–host–year panel for 2006–2023 that includes domestic transactions, allowing us to account for changes in the relative importance of cross-border and domestic activity over time. We then estimated a gravity specification in a staggered DiD framework with high-dimensional fixed effects and PPML.

The empirical results suggest that sanctions are associated with a sizable contraction in bilateral cross-border M&A activity. This negative relationship is observed for both deal counts and deal values, although the latter should be interpreted more cautiously. Event-time estimates further indicate that the negative effect of sanctions on the number of cross-border M&A deals persists and intensifies over time. Furthermore, the effects vary substantially across both adoption-year cohorts and sectors.

The findings of this study point to several important policy implications. First, although sanctions may not explicitly prohibit cross-border investment, they often deter firms from investing in target countries, amplifying the sanctions' overall economic impact. Second, the negative effect on cross-border investment tends to compound over time as sanctions persist; policymakers should therefore account for this cumulative effect. Finally, caution is warranted in interpreting the results, as the impact on cross-border M&A is highly heterogeneous across sanction episodes. Further analysis is required to identify the specific factors that drive this variation.

Several limitations are worth noting. First, by construction, our analysis focuses on cross-border M&As, which is only one component of overall FDI, and even within M&As, the Orbis data cover only a subset of transactions. Second, our outcomes capture transaction flows and deal formation rather than stock outcomes, and therefore do not speak to the evolution of FDI stocks, affiliate networks, disinvestment, or exit. Third, the pretreatment diagnostics indicate that some concerns remain for several specifications, particularly for deal values, which should be considered when interpreting the estimates. Fourth, while the results are consistent with sanctions increasing the costs of cross-border investment, this paper does not attempt to identify the underlying mechanisms. Clarifying the channels through which sanctions affect firms' investment decisions remains an important direction for future research.

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Appendix

A Industry classification

Table A.1 reports the mapping between Bureau van Dijk's Orbis industry classification and the four broad sectors used in the analysis (Agriculture, Mining & Energy, Manufacturing, and Services). The original industry codes and descriptions are obtained from the Orbis M&A database, and the authors manually aggregate them into broad sectors.

Table A.1: Mapping of BvD industries to four broad sectors

BvD industry code	BvD industry	Sector
10	Agriculture, Horticulture & Livestock	Agriculture
11	Mining & Extraction	Mining & Energy
12	Utilities	Mining & Energy
13	Construction	Manufacturing
14	Food & Tobacco Manufacturing	Manufacturing
15	Textiles & Clothing Manufacturing	Manufacturing
16	Wood, Furniture & Paper Manufacturing	Manufacturing
17	Printing & Publishing	Manufacturing
18	Chemicals, Petroleum, Rubber & Plastic	Manufacturing
19	Leather, Stone, Clay & Glass products	Manufacturing
20	Metals & Metal Products	Manufacturing
21	Industrial, Electric & Electronic Machinery	Manufacturing
22	Computer Hardware	Manufacturing
23	Communications	Services
24	Transport Manufacturing	Manufacturing
25	Miscellaneous Manufacturing	Manufacturing
26	Wholesale	Services
27	Retail	Services
28	Transport, Freight & Storage	Services
29	Travel, Personal & Leisure	Services
30	Computer Software	Services
31	Media & Broadcasting	Services
32	Banking, Insurance & Financial Services	Services
33	Property Services	Services
34	Business Services	Services
35	Biotechnology and Life Sciences	Services
36	Information Services	Services
37	Public Administration, Education, Health Social Services	Services
38	Waste Management & Treatment	Services