Minority Stakes in Rivals and Innovation*

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Abstract

When a firm acquires a minority stake in a competitor, both parties subsequently innovate less. We document this using patent data from 34 countries over the period 2001–2019. In a staggered difference-in-differences design with matched controls, patent counts and citations fall by 9–15% within five years for both targets and acquirers. The decline is concentrated in technology classes where the two firms directly compete. Non-horizontal minority acquisitions—between firms that do not share technology space—have no effect. The within-firm pattern points to reduced competitive incentives rather than financial constraints or post-deal disruption. Most competition regimes do not scrutinize minority stake acquisitions, yet the innovation effects we document are economically large and persistent.

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1 Introduction

When one firm buys a minority stake in a competitor, antitrust authorities typically look the other way. Most merger control regimes only review transactions that confer control, leaving acquisitions of less than 50% largely unscrutinized—even when the buyer and target are direct rivals.[1] Yet these deals are common. Minority stake acquisitions account for roughly 25% of global M&A activity.[2]

We assemble patent data from 34 countries over the period 2001–2019 and estimate the effect of horizontal minority stake acquisitions on the innovation activity of both targets and acquirers. We find that these deals lead to a persistent decline in innovation. Patent counts and citations fall by 8–15% within five years, with effects that materialize in the first year and continue to grow. Point estimates are consistently larger for acquirers than for targets, consistent with the acquirer directly internalizing its rival's returns to innovation.

The decline is not uniform across a firm's technology portfolio. When we disaggregate to the level of individual technology classes, the drop is concentrated in areas where the two firms' innovative activities overlap. Financial constraints, managerial distraction, or general restructuring would depress innovation across the board. They would not produce this pattern. It points instead to reduced competitive incentives: once a firm holds a stake in a rival, both parties have less reason to invest in the technologies where they directly compete.

The incentive logic differs for the two sides of the deal. For the acquirer, the channel is direct: owning a share of a rival's profits reduces the marginal return to competing through innovation. The larger the stake, the weaker the incentive—and the dose-response estimates confirm this, with each additional percentage point of ownership associated with a 0.3–0.5% decline in patenting.

For the target, the mechanism is less immediate. When the acquirer softens its competitive posture, the target faces less pressure to innovate defensively, lowering its own return to R&D in the contested space. We cannot observe the acquirer's behavior directly, but the overlap pattern is consistent with this channel: target firms curtail patenting specifically in shared technology classes while continuing normally elsewhere. A sep-

[1] The European Commission's regulation, for instance, is limited to acquisitions that confer control and does not authorize the review of minority shareholdings. See Council Regulation (EC) No 139/2004, Article 3. In the U.S., Section 7 of the Clayton Act does cover partial stock acquisitions, but in practice very few minority deals are challenged (Rock and Rubinfeld, 2017). Some countries (e.g., Austria, Germany, the United Kingdom) give competition authorities scope to examine minority shareholdings under national rules, and a small number (e.g., Brazil, India, Mexico) may require filings for acquisitions as low as 5–10%.

[2] Authors' calculation from the Zephyr (Bureau van Dijk) database.

arate channel—the acquirer using board seats, information rights, or other governance leverage to discourage the target's R&D—may also contribute. Our data do not allow us to separate the two.[3]

These incentive effects have not gone unnoticed by policymakers. The 2023 FTC-DOJ Merger Guidelines dedicate an entire section to partial and minority interests, noting that such acquisitions may "change incentives so as to otherwise dampen competition."[4] The European Commission argued in a 2014 white paper that non-controlling minority stakes can harm competition and consumers (EC (2014), ¶26). The OECD has warned that minority shareholdings "can have negative effects on competition, either by reducing the minority shareholder's incentives to compete (unilateral effects), or by facilitating collusion (coordinated effects)" (OECD (2008), p. 9).

The concern is not new. When Alcoa began acquiring minority stakes in European aluminum producers entering the U.S. market after World War I, the result was "a simple gentlemen's agreement on prices" (Stocking and Watkins (1946), pp. 248–251). A century later, enforcement is catching up. The UK's Competition and Markets Authority forced Ryanair to reduce its 29.8% stake in Aer Lingus to 5%, having found that it was expected to substantially lessen competition in airline services.[5] In 2021, the DOJ settled with Geisinger over its planned 30% investment in a rival Pennsylvania hospital, limiting the stake to 7.5% as a passive investment.[6] The FTC has opened an investigation into Microsoft's investment in OpenAI.[7] In June 2025, the European Commission fined Delivery Hero and Glovo a total of €329 million for participating in a cartel in the online food delivery sector, after Delivery Hero had acquired a non-controlling stake in Glovo in 2018—the first time the Commission sanctioned the anti-competitive use of a minority share in a competing business.[8] Two months later, the Commission conditioned its approval of Naspers' €4.1 billion acquisition of Just Eat Takeaway.com on the divestiture of Prosus's 27.4% stake in competitor Delivery Hero, finding that the structural link could reduce Just Eat's incentive to compete.[9] The Delivery Hero case turned on coordinated

[3]The data do not record whether a given minority stake comes with board representation or governance rights. Disentangling the softened-incentives channel from the direct-influence channel requires more granular deal-level data than are currently available.

[4]Section 2.11 of the FTC-DOJ Merger Guidelines (2023), pp. 28–29, available at `https://www.justice.gov/atr/merger-guidelines`. See also Section 13 of the DOJ and FTC's Horizontal Merger Guidelines (2010), pp. 31–33.

[5]See `https://www.gov.uk/cma-cases/ryanair-aer-lingus-merger-inquiry`.

[6]See `https://www.fiercehealthcare.com/hospitals/doj-reaches-settlement-geisinger-health-evangelical-antitrust-case`.

[7]See `https://www.nytimes.com/2024/01/26/business/dealbook/ftc-ai-deals-microsoft-openai.html`.

[8]See `https://ec.europa.eu/commission/presscorner/detail/en/ip_25_1356`.

[9]Case COMP/M.11936, *Naspers/Just Eat Takeaway.com*, Commission decision of 11 August 2025. Prosus

effects—the minority stake facilitated market sharing and information exchange—while the Naspers case turned on unilateral effects: the structural link weakened the incentive to compete.

Acquiring a stake in a rival reduces the incentive to compete: the acquirer earns profits even when it loses business to the target, and may use its influence to discourage the target's competitive initiatives. This "quiet life" effect extends naturally from the product market into R&D (e.g., López and Vives, 2019; Shelegia and Spiegel, 2024). But minority stakes can also help internalize technology spillovers. If two firms' R&D efforts partly benefit each other, a financial link between them can reduce free-riding and potentially *increase* innovation (e.g., Stenbacka and Van Moer, 2023). Whether the competitive softening or the spillover internalization dominates cannot be settled without data.

A growing literature examines the relationship between common ownership by institutional investors and innovation (e.g., Antón et al., 2024; Li et al., 2023; Lindsey, 2008; Kostovetsky and Manconi, 2020; Borochin et al., 2020), but common ownership operates through different channels—the ownership is indirect, typically passive, and the mechanism runs through portfolio incentives of diversified investors rather than through direct strategic interaction between rivals. Direct corporate minority stakes in competitors have received almost no empirical attention.[10]

Determining which acquisitions are between competitors is not straightforward. Industry codes (NAICS, SIC) are too coarse—firms that share a four-digit code may not compete in any meaningful sense. We define horizontality based on firms' patent portfolios: a deal is horizontal if the acquirer and target hold patents in the same 4-digit IPC technology classes. With over 650 distinct classes, this captures competitive overlap far more precisely than standard industry classifications.

Firms that acquire minority stakes in competitors are not random. They tend to be larger, more innovative, and more liquid than other firms. We construct control groups through propensity score matching on pre-acquisition characteristics, restricting the pool of potential controls to firms active in the same IPC classes as the treated firm. We then estimate treatment effects using the de Chaisemartin and D'Haultfoeuille (2024) estimator for staggered difference-in-differences designs. Pre-treatment coefficients are close to zero across all specifications, and the results survive a formal sensitivity analysis (Ram-

was also required to waive its voting rights, board representation, and information rights even after the sell-down. This was the first Phase I clearance since 2021 in which the Commission required the acquirer to divest or radically dilute a minority stake in a rival (Paul, Weiss, Rifkind, Wharton & Garrison LLP, 2025).

[10]Existing work on minority stakes focuses on pricing (Nain and Wang, 2018) or collusion (Heim et al., 2022). Bostan and Spatareanu (2018) examine innovation but focus on financial constraints rather than horizontal competition.

bachan and Roth, 2023) that allows the pre-trend slope to bend substantially in the post-treatment period. Japan and South Korea account for roughly half the sample, reflecting the prevalence of keiretsu and chaebol cross-ownership structures, but the results are robust to excluding either or both countries.

The broader literature on minority stake acquisitions has documented anti-competitive motives such as stabilizing collusive agreements (Heim et al., 2022), partitioning geographic markets (Meadowcroft and Thompson, 1986), or escaping antitrust scrutiny through incremental "sneaky" takeovers (Jovanovic and Wey, 2014). Our contribution is to show that minority stakes in rivals also dampen innovation, and that the effect is concentrated where competitive pressure is most direct.

2 Related Literature

If a firm owns part of its competitor, aggressive pricing or innovation hurts the value of that stake. The firm has less reason to compete. Reynolds and Snapp (1986) and Bresnahan and Salop (1986) showed that even small stakes lead to lower output and higher prices in models of oligopoly with partial ownership. The result extends to passive stakes under a range of market structures (Farrell and Shapiro, 1990; Shelegia and Spiegel, 2012; Jovanovic and Wey, 2014) and to coordinated effects: structural links between competitors can stabilize tacit collusion by making deviation less attractive (Malueg, 1992; Gilo et al., 2006). Empirically, Nain and Wang (2018) estimate that horizontal minority acquisitions in U.S. manufacturing raised prices by roughly 2% and increased margins by 0.7%. Dietzenbacher et al. (2000) find elevated margins among Dutch financial firms with cross-shareholdings. Heim et al. (2022) document a spike in minority stake acquisitions following the introduction of leniency programs, suggesting that firms use these stakes to reinforce cartels rather than as passive investments.

Whether minority stakes increase or decrease innovation depends on the balance between two forces. Owning part of a rival softens the incentive to compete through R&D, but it can also help internalize technology spillovers that would otherwise be free-ridden on. López and Vives (2019) and Stenbacka and Van Moer (2023) show that the net effect hinges on the level of spillovers and on market structure: when spillovers are low, the competition-softening channel dominates; when they are high, internalization can outweigh the loss of competitive pressure. Shelegia and Spiegel (2024) show that the sign further depends on the symmetry of stakes and the cost of innovation. The parameter values that tip the balance differ by industry and deal structure, so the question is ulti-

mately empirical.

A large literature studies the effect of full mergers on innovation. In the context of M&A, the "innovation theory of harm" holds that mergers reduce innovation by internalizing the business-stealing externality (Federico et al., 2017), though mergers can also facilitate knowledge-sharing (Denicolò and Polo, 2019; Bourreau and Jullien, 2018). Marshall and Parra (2019) show that competition affects innovation through both the number of firms racing to innovate and the profit gap between leaders and followers, and that these channels can push in opposite directions. Empirically, Haucap et al. (2019) find that pharmaceutical mergers reduce innovation, with effects concentrated in R&D-intensive markets. Cunningham et al. (2021) identify "killer acquisitions" in pharmaceuticals—cases where incumbents acquire targets specifically to discontinue overlapping projects. Malek et al. (2025) add nuance, finding positive effects when incumbents acquire early-stage projects similar to their own. Bena and Li (2014) show that acquirers with prior technological linkage to their targets produce more patents after the deal. Full acquisitions, though, allow the acquirer to integrate the target's R&D operations and realize synergies. Minority stakes provide the financial link without the integration, leaving the competition-softening channel largely unopposed.

A growing literature examines how common ownership by institutional investors affects innovation (e.g., Antón et al., 2023, 2024; Li et al., 2023; Lindsey, 2008; Kostovetsky and Manconi, 2020; Borochin et al., 2020). The channels differ from direct corporate minority stakes. Common ownership is indirect and typically passive—the mechanism runs through portfolio incentives of diversified investors, not through strategic interaction between rival firms. Corporate minority stakes create sharper incentive distortions because the acquirer has a direct financial interest in its competitor's profits and, in many cases, access to its governance.

The closest empirical study is Bostan and Spatareanu (2018), who find positive innovation effects for U.S. targets of minority equity purchases. Their mechanism is financial: they view minority stakes as a funding bridge for constrained firms, without restricting attention to horizontal deals. The present paper isolates acquisitions between direct competitors, where the motive is more likely strategic than financial. None of the prior studies cited above examines what happens to innovation when a firm acquires a partial stake in a direct competitor.

3 Data

3.1 Data Sources

We measure innovation using PATSTAT, maintained by the European Patent Office, which covers worldwide patent applications from 1844 to 2023. We extract patent application IDs, application years, forward citations, and 8-digit IPC technology classes for all granted patents, dated by application year. This gives us two proxies for innovation output: patent quantity (number of grants) and patent quality (number of forward citations). Our M&A data come from Bureau van Dijk's Zephyr database, which records deal-level information—including completion dates, stake percentages, and acquirer and target identities—for transactions worldwide from 1997 to 2019. We merge both datasets with Bureau van Dijk's Orbis database, which tracks balance sheet and P&L information for approximately 45 million companies worldwide. The merged dataset provides, for each firm-year, patent counts, citation counts, the list of IPC classes in which the firm is active, and financial variables including sales, total assets, liquidity ratios, and employment.

3.2 Identifying Horizontal Minority Stake Acquisitions

From the universe of Zephyr transactions, we select completed deals where the reported final stake is below 50%. We exclude share buybacks and self-tenders, deals where either party is classified as an investor or investment office by primary SIC code,[11] and transactions where patent data are unavailable for both the target and the acquirer (or acquirer's parent). Approximately 17% of minority stake deals involve targets and acquirers that both hold at least one patent—well above the 5% population average of patenting firms in Orbis (Gugler et al., 2023)—suggesting that firms engaged in these transactions are more technology-intensive than the typical firm. This leaves 24,267 minority stake acquisitions.

The key classification step is identifying which of these deals are horizontal. Rather than relying on industry codes (SIC, NAICS), which are broad and often poorly suited to capturing competitive overlap between innovating firms, we define horizontality based on pre-acquisition patent portfolios. Specifically, we classify a deal as horizontal if the acquirer (or its parent) and the target share at least one 4-digit IPC class among patents granted before the year of the acquisition. With more than 650 distinct 4-digit IPC classes,

[11] We removed deals involving firms with SIC codes 6799 or 6722.

this approach provides a substantially finer-grained measure of competitive proximity than standard industry classifications.[12] Strictly speaking, IPC overlap captures proximity in technology space, which need not perfectly coincide with product market boundaries.[13] By this definition, 56% of the 24,267 minority stake acquisitions are horizontal, corresponding to 4,561 unique targets and 2,898 unique acquirers.

3.3 Estimation Sample

We analyze targets and acquirers separately, constructing two parallel datasets. The construction steps are detailed in Table A1 in the Appendix. Starting from the full set of horizontal deals, we drop observations without Orbis financial data, leaving 2,918 targets and 1,428 acquirers. To isolate the effect of minority stakes from confounding M&A activity, we exclude firms involved in any majority acquisition or merger within three years of the minority deal. We also remove non-corporate entities (financial firms, mutual funds, pension funds) and firms without at least three consecutive years of patent data. The final estimation sample consists of 1,082 targets and 579 acquirers. We discuss the representativeness of this sample in Appendix A.1.

For the control group, we start from the universe of Orbis firms with sufficient patent and financial information (at least five consecutive years of patent data), and remove any firm ever involved in a majority or horizontal minority stake acquisition, as well as non-corporate entities. This yields nearly 100,000 potential control firms—more than 60 times the number of treated firms—ensuring that a sufficiently close match can be found for each treated observation.

3.4 Descriptive Statistics

Roughly two thirds of deals in our sample are domestic.[14] Japan accounts for the largest share of horizontal minority stake acquisitions (35%), followed by South Korea (16%), reflecting the prominence of cross-ownership networks—keiretsu and chaebol—in these economies. The sample is concentrated in manufacturing (68%) and information industries (7%); detailed breakdowns by sector appear in the Appendix.

[12] Statistics based on World Intellectual Property Organization, available at `https://www.wipo.int/classifications/ipc/en/ITsupport/Version20240101/transformations/stats.html`.

[13] Marshall and Parra (2019) provide a formal distinction between technology and product market competition. For innovation outcomes, however, technology-space proximity is arguably the more relevant dimension: it identifies exactly the areas where the two firms' R&D efforts are in direct competition.

[14] 61% of targets were acquired by firms in the same country; 69% of acquirers acquired domestic targets. Detailed breakdowns by year, country, and industry appear in Figures A.1–A.3 in the Appendix.

The distribution of acquired stakes is right-skewed, with most observations below 25% (see Figure A.4 in the Appendix).[15] About 30% of sample firms are involved in more than one horizontal minority acquisition during the observation period, sometimes as acquirer, sometimes as target.[16]

Table A2 in the Appendix provides pre-acquisition summary statistics. Acquirers tend to be substantially larger than targets: the median acquirer has roughly eight times the sales and seven times the total assets of its target (Table A3). Innovation activity is highly concentrated: the top 15% of firms in our sample account for approximately 90% of the total patent stock, consistent with the well-documented concentration of global R&D activity.[17]

4 Research Design

Firms that acquire minority stakes in competitors differ systematically from other firms. They are larger, more innovative, and more liquid. Treatment timing also varies across firms. Both features shape the empirical strategy: we combine propensity score matching with a staggered difference-in-differences estimator.

4.1 Matching Procedure

We construct the control group using propensity score matching (Rosenbaum and Rubin, 1983, 1985). A probit model predicts the probability of being involved in a horizontal minority stake acquisition based on pre-treatment observables: cumulative patents, cumulative citations, sales, employment, total assets, intangible assets, liquidity ratio, and firm age, all measured at $t-1$. The model includes country, year, and industry (2-digit NAICS) fixed effects. Table 1 reports the estimates. The pseudo-R^2 is 0.40 for targets and 0.49 for acquirers, confirming that treated firms differ substantially from the general population along observable dimensions.

[15] The distribution differs slightly between the target and acquirer samples because some deals are retained in one sample but not the other due to data availability.

[16] Approximately 13% of targets were acquired multiple times by the same acquirer; about 8% of acquirers acquired the same target more than once. In addition, roughly 15% of sample firms were also involved in non-horizontal minority stake acquisitions.

[17] Based on the 2022 EU Industrial R&D Investment Scoreboard, the top 15% of the 2,500 largest R&D investors account for nearly 75% of total Scoreboard R&D expenditure. See `https://iri.jrc.ec.europa.eu/scoreboard/2022-eu-industrial-rd-investment-scoreboard`.

Table 1: Propensity Score Estimation (Probit Model)

	Targets	Acquirers
	(1)	(2)
Log Sales $(t-1)$	-0.138 (0.018)***	-0.104 (0.026)***
Log # Employees $(t-1)$	0.216 (0.017)***	0.116 (0.023)***
Log Total Assets $(t-1)$	0.214 (0.018)***	0.331 (0.026)***
Log Intangibles $(t-1)$	0.042 (0.004)***	0.052 (0.007)***
Log Liquidity Ratio $(t-1)$	0.129 (0.026)***	0.193 (0.035)***
Log Cum. Patents $(t-1)$	0.147 (0.020)***	0.056 (0.030)*
Log Cum. Patent Cites $(t-1)$	0.012 (0.015)	0.082 (0.024)***
Log Age $(t-1)$	-0.019 (0.018)	0.018 (0.027)
Observations N	776,272	769,627
Pseudo R^2	0.40	0.49

This table estimates the probability of being involved in a horizontal minority stake acquisition. The dependent variable equals one if the firm was involved in such a deal in the following period. The probit model uses all firm-year observations from the pool of potential controls, but only the $t-1$ observation for treated firms. For treated firms with more than one horizontal minority acquisition, matching is performed on the year before the first acquisition. Variables with zero values are transformed as $\log(\text{variable}+1)$. Country, year, and industry (2-digit NAICS) fixed effects are included. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Each treated firm is matched to the control firm with the closest propensity score, subject to three restrictions. First, treated and control firms must share the same observation year. Second, potential controls must hold at least one patent in the same IPC class as the treated firm in the year prior to the acquisition. This ensures that matched pairs operate in the same technology domain—a tighter counterfactual than matching on industry codes alone.[18]

Third, we enforce common support by dropping treated firms whose propensity score falls outside the range of the control distribution.[19] We match with replacement, meaning a control firm can serve as a match for more than one treated firm. Pairs whose propensity scores differ by more than 0.10 in absolute terms are dropped.

After matching, standardized biases fall by as much as 80% across covariates (Figure 1), and the pseudo-R^2 of the probit model drops from 0.40–0.49 to 0.07–0.09. Matched treated and control firms are also much more similar in their patent distributions than unmatched firms (Figures A.5 and A.6 in the Appendix).

[18] A potential concern is that control firms sharing 4-digit IPC classes with treated firms may themselves be indirectly affected if competitive pressure in those classes changes. We address this in Section 6 by rematching treated firms to controls that share only 3-digit IPC overlap but have no 4-digit overlap.

[19] This criterion discards 11 targets and 11 acquirers.

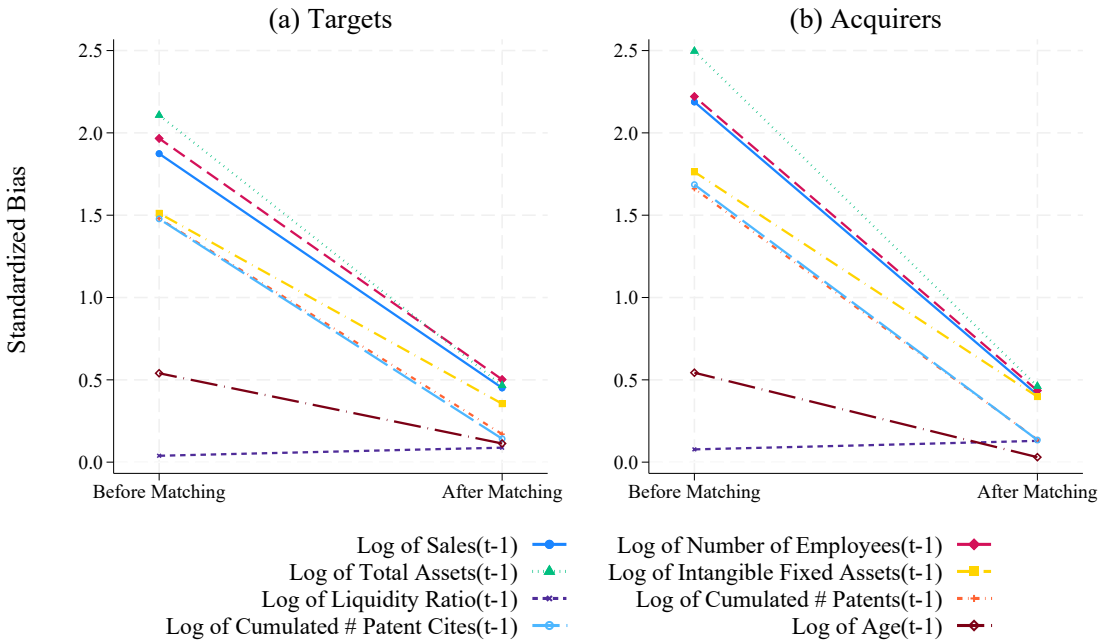

Figure 1: Standardized Biases, Before and After Matching

This figure plots the standardized biases for the covariates used in the propensity score estimation, before and after matching, separately for targets and acquirers. For each regressor X, the standardized bias is calculated as the difference of sample means in the treated and control samples as a percentage of the square root of the average of sample variances in both groups (Caliendo and Kopeinig, 2008). Before matching, biases are computed using all firm-year observations from the pool of potential controls alongside the $t-1$ observation for treated firms. After matching, biases are computed using the $t-1$ observation for matched treated and control firms only. The matched firms are then followed as a panel for the event-study estimation.

4.2 Staggered Difference-in-Differences

Acquisitions occur at different times across firms, so the standard two-way fixed effects (TWFE) estimator may produce biased estimates in the presence of heterogeneous or dynamic treatment effects.[20] We use the de Chaisemartin and D'Haultfoeuille (2024) estimator, which compares first-time switchers with not-yet-treated firms and accommodates heterogeneous treatment timing, dynamic effects, and varying treatment intensities—all features of our setting, where acquisitions of different sizes occur at different points in time and their effects on innovation may unfold gradually.

The dependent variable is the cumulative patent stock (or cumulative forward citations) of firm i in year t, expressed as $\ln(\text{Patents}_{i,t}+1)$ or $\ln(\text{Citations}_{i,t}+1)$. Cumulative stocks can only increase over time, so a negative treatment effect means the treated firm's innovation output grew more slowly than its matched control's—not that the stock de-

[20] See, e.g., de Chaisemartin and D'Haultfoeuille (2024), de Chaisemartin and D'Haultoeuille (2020), Borusyak et al. (2024), Callaway and Sant'Anna (2021), Sun and Abraham (2021), and Goodman-Bacon (2021).

clined in absolute terms. This distinction matters for interpreting the magnitudes we report in Section 5. Let $\tau \in \{-3, \ldots, 5\}$ denote event time relative to the firm's first horizontal minority stake acquisition, with $\tau = 0$ as the acquisition year and $\tau = -1$ as the reference period. The event-study specification is:

$$\ln(\text{Patent}_{i,t} + 1) \;=\; \gamma_i + \gamma_t \;+\; \sum_{\substack{\tau=-3,\\ \tau\neq -1}}^{5} \beta_\tau \, D_{i,t}^{\tau} \;+\; u_{i,t} \tag{1}$$

where $D_{i,t}^{\tau} = 1$ if firm i is observed at event time τ, and zero otherwise. The specification includes firm fixed effects γ_i and calendar-year fixed effects γ_t. Standard errors are clustered at the firm level. We consider a post-treatment window of five years; effects may persist beyond this horizon, but the sample thins at longer lags.

5 Results

5.1 Main Results

We begin with the event-study estimates from the staggered difference-in-differences design described in Section 4. Figure 2 plots the results for patent counts (left panel) and citations (right panel), separately for targets and acquirers.

Before the acquisition, the coefficients are generally small and close to zero, consistent with the parallel trends assumption. We examine the pre-trends more formally in Section 5.2. After the acquisition, all four series turn negative and deepen steadily. By year five, cumulative patent counts are 9.1% lower for targets and 12.3% lower for acquirers relative to matched controls. Cumulative citations are 7.7% and 14.8% lower, respectively. Both sides of the deal innovate less, and the firm that holds the stake is affected more than the firm whose stake is held.

The decline is gradual—consistent with the slow-moving nature of R&D, where a change in competitive incentives takes time to reach patent filings—but it shows no sign of leveling off. The sample thins at longer horizons, so we stop at five years.

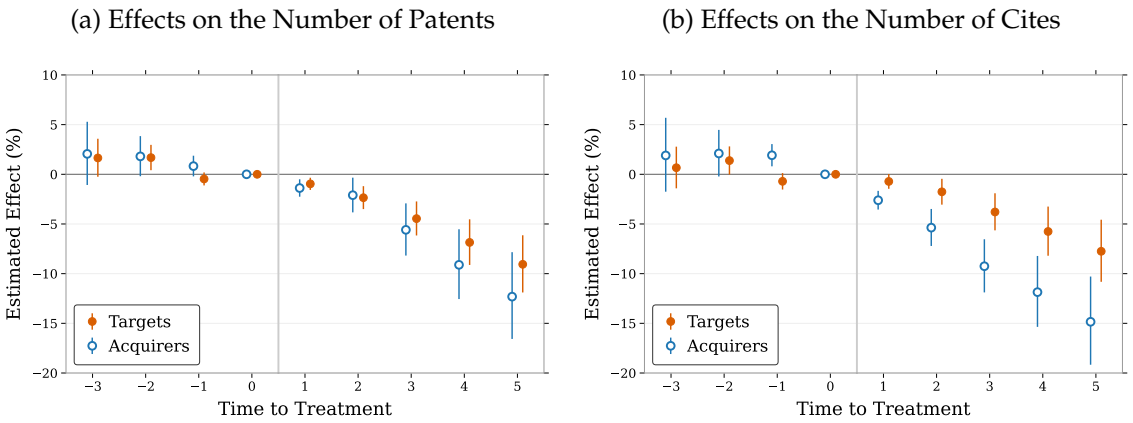

Figure 2: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

Table 2 complements the event-study figures by reporting the average treatment effect over the full five-year post-acquisition window. Because this averages across the early years when the effect is still building, the magnitudes are more conservative than the year-five endpoints.

Panel A uses a binary treatment indicator. Targets experience a 4.3% average decline in cumulative patent counts and a 3.6% decline in citations. For acquirers, the effects are larger: 5.5% and 8.2%.

Panel C tests whether the effect scales with the size of the stake. It treats the acquired ownership share as a continuous variable, so the coefficients represent the effect of each additional percentage point of ownership in a rival. The estimates indicate a 0.3–0.5% decline in patents and a 0.4–0.5% decline in citations per percentage point. Innovation does not just fall after a minority acquisition—it falls more when the stake is larger.[21]

Across all specifications, point estimates are larger for acquirers than for targets. The pattern is consistent with the incentive logic laid out in the introduction: the acquirer directly internalizes its rival's returns, weakening its incentive to compete through innovation.

[21] Panel B codes the treatment as a count of horizontal minority acquisitions per firm. The pattern is similar to Panel A, with slightly smaller point estimates.

Table 2: Estimated Effects of Horizontal Minority Stake Acquisitions on Patent Activity

	Targets		Acquirers	
	Cum. Patents	Cum. Cites	Cum. Patents	Cum. Cites
	(1)	(2)	(3)	(4)
Panel A: *Binary treatment*				
Treatment effect	-0.044^{***}	-0.036^{***}	-0.057^{***}	-0.085^{***}
	(0.011)	(0.014)	(0.017)	(0.017)
Panel B: *Discrete treatment*				
Treatment effect	-0.034^{***}	-0.028^{***}	-0.049^{***}	-0.073^{***}
	(0.009)	(0.011)	(0.014)	(0.015)
Panel C: *Treatment intensity*				
Treatment effect	-0.005^{***}	-0.004^{***}	-0.003^{***}	-0.005^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	17,430	17,430	8,571	8,571

Each column reports the estimated average effect over the five-year period post-acquisition from a staggered difference-in-differences regression using the de Chaisemartin and D'Haultfoeuille (2024) estimator. The results represent weighted averages of group-time effects. Panel A treats the acquisition as binary (0 before a horizontal minority stake deal, 1 thereafter). Panel B treats the treatment as discrete (0 before, 1 after the first deal, 2 after the second, etc.). Panel C uses the cumulative percentage stake acquired in a rival; coefficients represent the semi-elasticity with respect to a one-percentage-point increase in ownership. All specifications include firm and year fixed effects and use a 5-year post-acquisition window. Standard errors are clustered at the firm level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Addressing Potential Violations of Parallel Trends

The pre-treatment coefficients in Figure 2 are generally small and statistically indistinguishable from zero, but they are not uniformly so. Across the four event studies, two show no significant pre-treatment coefficients at all. The remaining two each have one marginally significant coefficient out of three; the most notable is for acquirers' citations, where the $t-3$ coefficient is somewhat elevated and there is a visible downward drift toward the acquisition date.[22] Mostly flat pre-trends do not guarantee that the parallel trends assumption holds exactly. Small pre-existing differences could in principle account for part of the post-treatment decline (Roth et al., 2023).

We address this concern in two ways. First, following Fenizia and Saggio (2024), we extrapolate the linear trend implied by the pre-treatment coefficients into the post-treatment period (panel (a) of Figure 3). The slopes are small, ranging from -0.004 to

[22] Extending the event study to five years before the acquisition yields similar results (see Figure A.7 in the Appendix).

-0.007, but they do trend in the same direction as the post-treatment estimates. Rotating the event-study coefficients around this extrapolated trend (panel (b)) leaves them negative, highly significant, and growing over time for all four outcomes.

Second, we implement the "honest" difference-in-differences approach of Rambachan and Roth (2023), which bounds the permissible change in the slope of the differential trend between treated and control firms:

$$\Delta\text{Slope Differential} = \{\theta : |(\theta_{k+1} - \theta_k) - (\theta_k - \theta_{k-1})| \leq M\}.$$

The parameter M controls how much the slope of the pre-trend is allowed to bend in each post-treatment period. We focus on the fifth post-treatment coefficient, where cumulative deviations from linearity are largest and the test is most demanding.

For targets' patents, the robust confidence intervals remain strictly below zero until M reaches 0.006 (panel (c) of Figure 3). Since the estimated pre-trend slope is -0.007 log-points per year, an M of 0.006 permits the slope to change by nearly its full magnitude in every post-treatment period—a substantial departure from linearity. Results are similar for acquirers' patents and for citations of both groups.

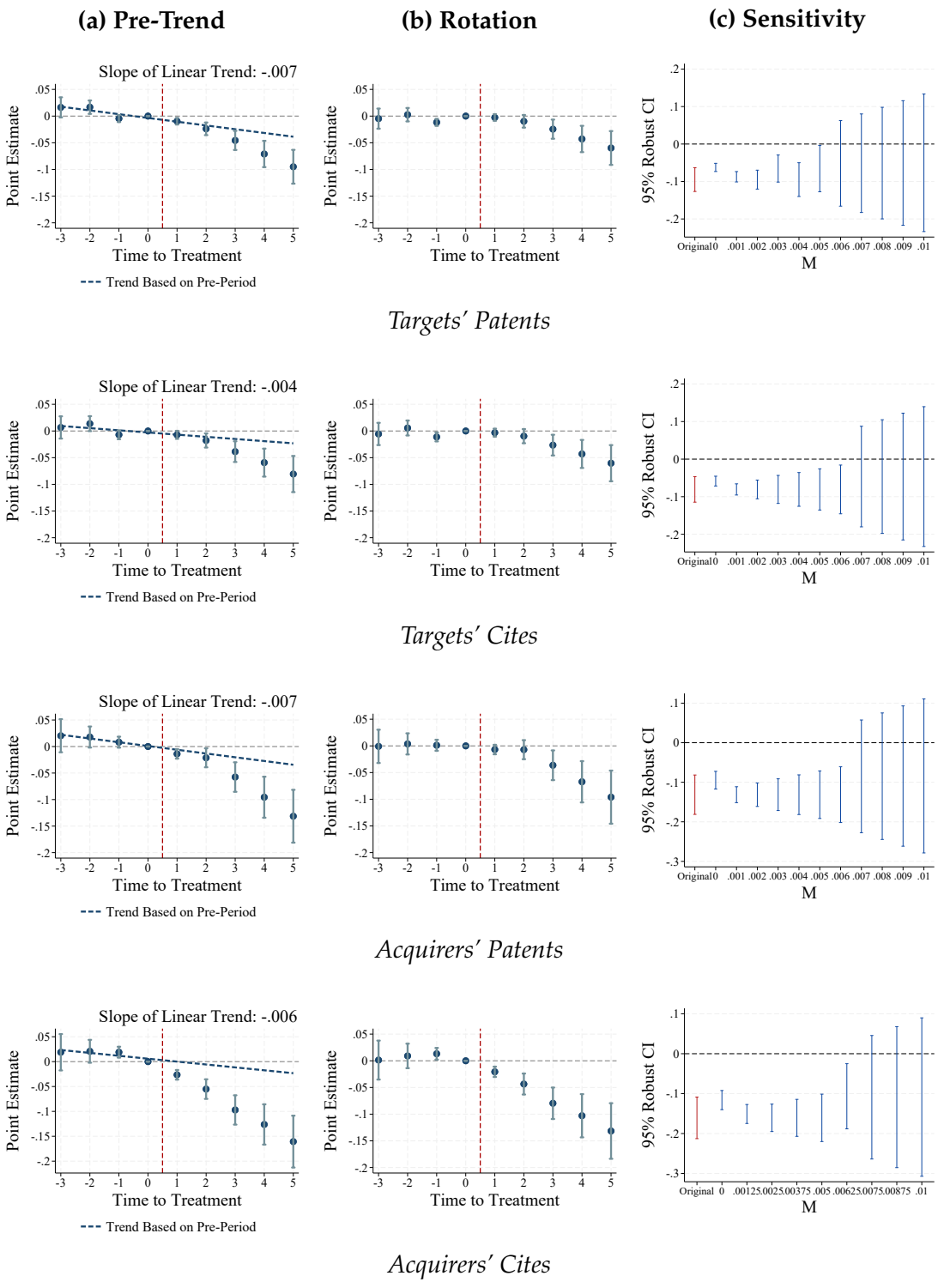

Figure 3: "Honest" Approach to Parallel Trends

This figure plots the results of the "honest" difference-in-differences approach proposed by Rambachan and Roth (2023). The first column extrapolates to the post-treatment period a linear trend estimated from the pre-treatment coefficients (three years before the minority acquisition). The second column rotates the event study around this linear trend and plots the deviations between the estimated coefficients and the linear trend. The third column reports 95% confidence intervals for the fifth-year post-acquisition effect under different values of M, the maximum permitted change in the slope of the differential trend between treated and control firms per post-treatment period. $M = 0$ implies the counterfactual trend is perfectly linear.

5.3 Where Does the Decline Occur?

Horizontal minority stake acquisitions reduce patenting at the firm level. This finding alone does not reveal the underlying mechanism. A general disruption to the firm's operations would depress patenting across its entire technology portfolio. A softening of competitive incentives, by contrast, would concentrate the decline in technology classes where the two firms' R&D programs directly compete—and leave the rest of the portfolio unaffected.

To distinguish between these explanations, we disaggregate to the firm-by-IPC-class-by-year level. For each deal, we classify a firm's technology areas into *overlap* classes—those 4-digit IPC classes where the counterpart also held at least one patent prior to the acquisition—and *non-overlap* classes, where it did not. We then estimate our event-study specification separately for each group. The classification is conservative: 4-digit IPC classes are narrow, so some technologically related classes end up in the non-overlap group, biasing against finding a difference.

Figure 4 shows the results. In both panels, overlap and non-overlap classes drift downward at similar rates prior to the acquisition. After the deal, the trajectories diverge: patenting in non-overlap classes stabilizes or continues its prior trend, while patenting in overlap classes declines at a markedly faster rate. This acceleration is specific to the technology areas where the two firms compete, consistent with a change in competitive incentives rather than a continuation of pre-existing dynamics.

Averaging the dynamic treatment effects over the post-treatment window ($t = 1$ to $t = 5$), the gap between overlap and non-overlap classes amounts to 10.8 percentage points for targets ($p < 0.01$) and 8.0 percentage points for acquirers ($p < 0.01$). For targets, non-overlap patenting remains flat throughout the post-treatment period, while overlap patenting drops by roughly 20% within five years. Acquirers show a similar pattern, though less sharply: overlap classes reach about -22% by year five, while non-overlap classes also decline, to approximately -12%. The effect is strongest where competition is most direct, but the non-overlap decline for acquirers suggests that reduced competitive pressure spills over into adjacent technology areas as well.[23] Taken together, the concentration of the decline in shared technology classes is difficult to reconcile with a firm-wide disruption story and points instead to a mechanism rooted in competitive incentives.

[23] An alternative, complementary explanation is that reduced competitive pressure induces a generalized "quiet life" effect across the acquirer's R&D operations, but one that is strongest where competition with the target is most direct.

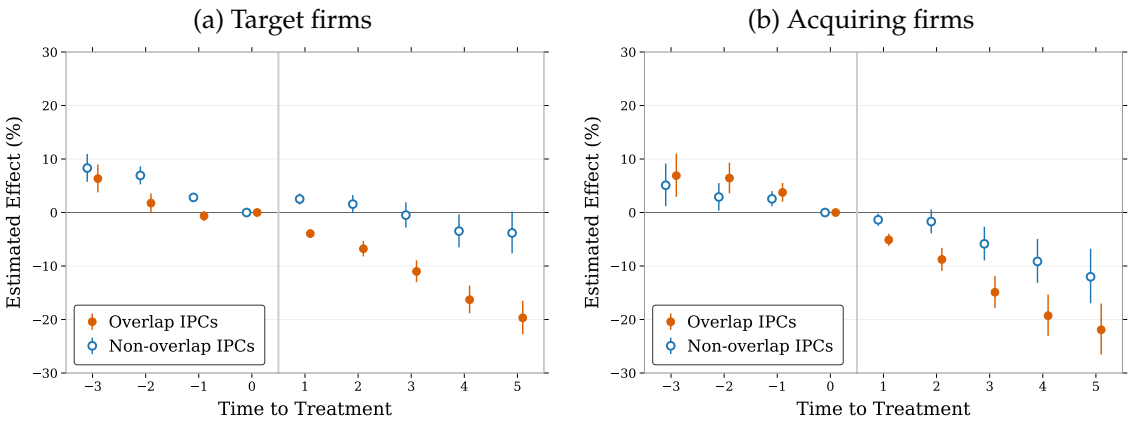

Figure 4: Effect on Patenting by Technological Overlap

The figure displays event-study estimates for patent counts at the firm-by-IPC-class-by-year level. For each deal, IPC classes are classified based on whether the target and acquirer had overlapping patent activity in that class prior to the acquisition (Overlap IPCs) or not (Non-overlap IPCs). The classification uses 4-digit IPC classes. The estimation uses the de Chaisemartin and D'Haultfoeuille (2024) estimator. Time fixed effects and firm fixed effects are included. The dashed red line indicates the time of treatment. Error bars represent 95% confidence intervals based on standard errors clustered at the firm level.

6 Robustness Checks

We subject the main findings to several robustness tests. The most important is a placebo exercise using non-horizontal deals, which directly tests whether the results reflect competitive incentives rather than deal-level confounders. We then consider alternative definitions of horizontality, alternative matching procedures, additional controls, and leave-one-out tests.

Placebo: Non-Horizontal Minority Acquisitions. If the innovation decline is driven by reduced competitive incentives, it should not appear in deals where the acquirer and target do not compete. If it reflected factors common to all minority acquisitions—managerial distraction, governance costs, or financial frictions associated with the deal itself—we would expect a similar drop for non-horizontal transactions.

We run our analysis on non-horizontal minority stake acquisitions, defined as deals where the acquirer and target share no 4-digit IPC class prior to the transaction. Figure 5 plots the event study. The coefficients are small and statistically indistinguishable from zero throughout the post-acquisition window. Table 3 confirms that the average treatment effects range from -2.2% to $+1.0\%$ and are never significant. Minority stakes reduce innovation only when the parties are direct competitors.

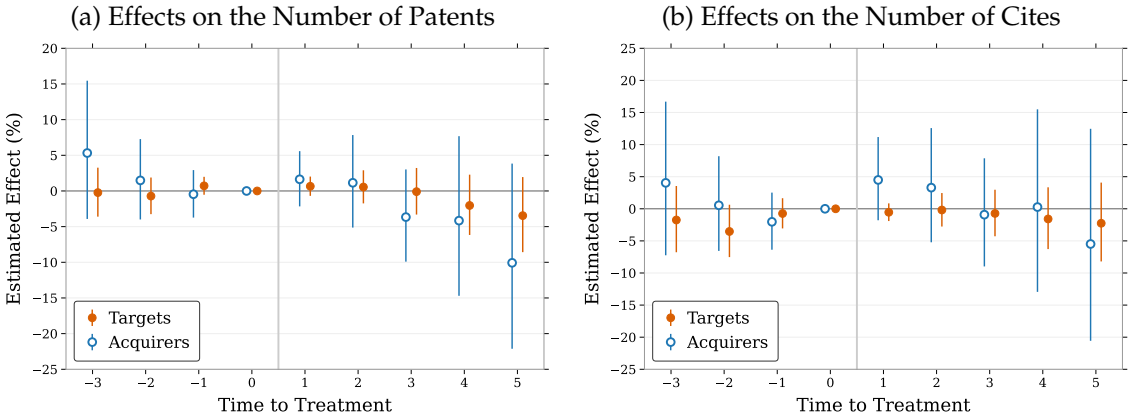

Figure 5: Event Study on the Effects of Non-Horizontal Minority Stake Acquisitions on Patent Activity

The figure presents coefficient estimates in percentage for each of the three years before and five years after the non-horizontal minority stake acquisition of treated firms. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

Table 3: Estimated Effects of Non-Horizontal Minority Stake Acquisitions on Patent Activity

	Targets		Acquirers	
	Cum. Patents	Cum. Cites	Cum. Patents	Cum. Cites
	(1)	(2)	(3)	(4)
Treatment effect	-0.006	-0.009	-0.022	0.010
	(0.014)	(0.016)	(0.036)	(0.047)
Observations	11,340	11,340	3,483	3,483

Each column reports the estimated average effect over the five-year period post-acquisition from a staggered difference-in-differences regression using the de Chaisemartin and D'Haultfoeuille (2024) estimator. The results represent weighted averages of group-time effects. The acquisition is treated as binary (0 before a non-horizontal minority stake deal, 1 thereafter). All specifications include firm and year fixed effects and use a 5-year post-acquisition window. Standard errors are clustered at the firm level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Horizontality Defined by Industry Codes. Our main specification defines horizontality through IPC overlap. As an alternative, we classify a deal as horizontal if the acquirer and target share at least one 4-digit industry code (NAICS, NACE, or SIC) and match on industry codes rather than IPC classes.

Figure 6 shows a negative effect on patents and citations, but the magnitudes are attenuated relative to the baseline. Industry codes cast a wider net than IPC classes, so this sample likely includes deals where the parties share a sector classification but do not meaningfully compete in the same technology market. The attenuation is consistent with the IPC-based measure capturing competitive overlap more precisely.

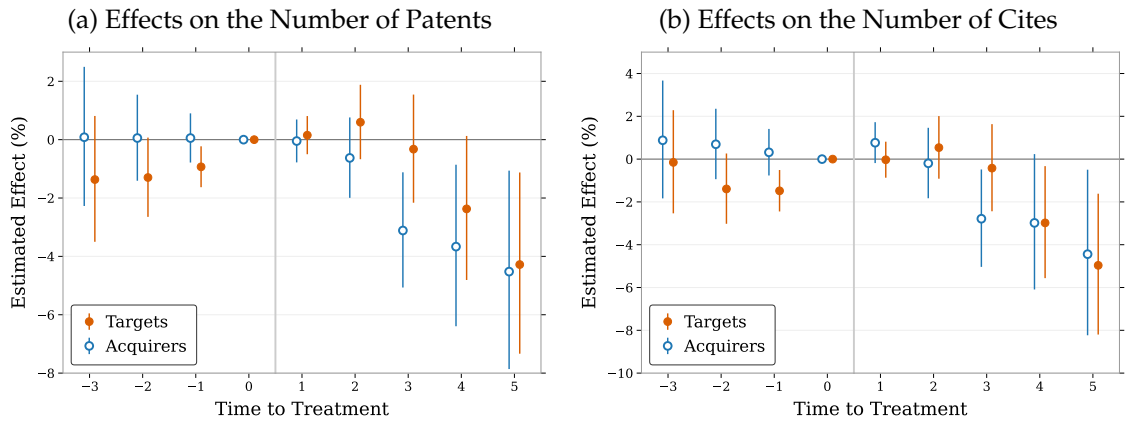

Figure 6: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity—Based on Industry Codes

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. Horizontal deals are selected based on whether there is an overlap in any of the 4-digit industry codes (NAICS, NACE, and SIC) of the target and its acquirer/parent. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

Alternative Matching Procedures. Matching each treated firm to its three nearest neighbors rather than one yields similar results (Figure A.8 in the Appendix). Imposing exact matching on country, so that treated and control firms operate in the same jurisdiction, does not materially affect the estimates (Figure A.9 in the Appendix). As noted in Section 4, control firms that share 4-digit IPC classes with treated firms may themselves be indirectly affected by the deal. Re-matching treated firms to controls that share only 3-digit IPC overlap but have no 4-digit overlap produces similar results (Figure A.10 in the Appendix).

Industry Controls and Within-Sector Matching. Technology cycles, regulatory changes, or shifts in global demand could bias our estimates if treated and control firms happen to be in sectors that evolve differently over time. Re-estimating the event study with industry-specific time trends—allowing each 2-digit NAICS sector to follow its own trajectory—leaves the coefficients similar to the baseline (Figure A.11 in the Appendix). Tightening the matching by imposing exact matching within the same 2-digit NAICS sector also leaves the treatment profile close to the baseline (Figure A.12 in the Appendix).

Firm-Level Controls. Our baseline excludes firm-level covariates, since propensity score matching already balances treated and control firms on these dimensions. Adding log sales, log employment, log total assets, log intangible assets, the liquidity ratio, and firm age as controls does not alter the estimates (Figure A.13 in the Appendix).

Leave-One-Out Tests. We drop all treated firms and their matched controls from each country one at a time. All estimates remain negative and statistically significant (Figure A.14 in the Appendix), confirming that the findings are not driven by any single jurisdiction—including Japan and South Korea, which together account for half the sample. Dropping all deals from each acquisition year one at a time produces similarly stable results (Figure A.15 in the Appendix).

7 Conclusion

Minority stake acquisitions between competitors reduce innovation. Using patent data from 34 countries over the period 2001–2019, we find that patent counts and citations fall by 8–15% relative to matched controls within five years of the deal. The effect scales with the size of the stake: each additional percentage point of ownership is associated with a further 0.3–0.5% decline in patenting. The decline is concentrated in technology classes where the two firms directly compete. In non-overlapping fields, patenting continues as before. Non-horizontal minority acquisitions have no effect.

Theory allows for the possibility that minority stakes boost innovation by helping firms internalize technology spillovers. The overlap pattern we find points in the opposite direction: partial ownership blunts the incentive to compete through innovation. The effect is larger for acquirers, who directly internalize their rival's returns, but targets also reduce innovation in shared technology classes. We interpret this as consistent with softened competition lowering the return to defensive R&D, though we cannot rule out

that acquirers use governance rights to directly influence the target's R&D decisions.

Most competition regimes only review deals that confer control, so the transactions we study receive little or no antitrust scrutiny. Expanding review to cover minority shareholdings would impose significant administrative costs on agencies and firms, and whether the documented harm justifies those costs requires careful consideration that falls outside the scope of the present work.

Our analysis has limitations. The definition of horizontality relies on patent overlap, restricting the sample to firms that patent. Minority stakes between competitors in low-patenting industries may have similar effects, but our data cannot speak to them. Patents capture only one dimension of innovative activity—effects on product development, process improvements, or R&D spending may differ. And while the placebo test and overlap decomposition substantially narrow the set of plausible alternative explanations, no observational design can fully rule out unobserved confounders.

Two extensions seem particularly promising. Data on governance rights attached to minority stakes would allow researchers to separate the softened-incentives channel from the direct-influence channel. And tracing what happens to innovation when minority stakes are later divested would provide a natural test of whether the effects we document are reversible.

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A Appendix

A.1 Sample Construction and Representativeness

Table A1 details how we constructed the datasets for targets and acquirers that we use in the paper and the number of firms that remained after each step. To ensure that our selected group of firms constitutes a representative sample, we compare our group of treated firms with the entire set of companies involved in minority stake acquisitions as reported in the Zephyr dataset. The distribution of our final sample across years is similar to other firms involved in minority stake acquisitions for the period from 2001 to 2019 (see Figure A.1). The later years (2018–2019) are under-represented in our sample because we impose that treated firms have at least three years of consecutive patent data. As shown in Figure A.2, the distribution of our final sample across countries is roughly similar to other firms involved in minority stake acquisitions. China is under-represented in our sample, while Japan and South Korea are over-represented. The distribution of our final sample across industries is also similar to other firms involved in minority stake acquisitions (see Figure A.3). Similar to other firms, our sample of acquirers and targets predominantly operate in the manufacturing sector. As expected, our sample is under-represented in terms of Finance, Insurance, and Management companies, since we excluded non-corporate firms and investment offices from the group of treated firms.

A.2 Figures

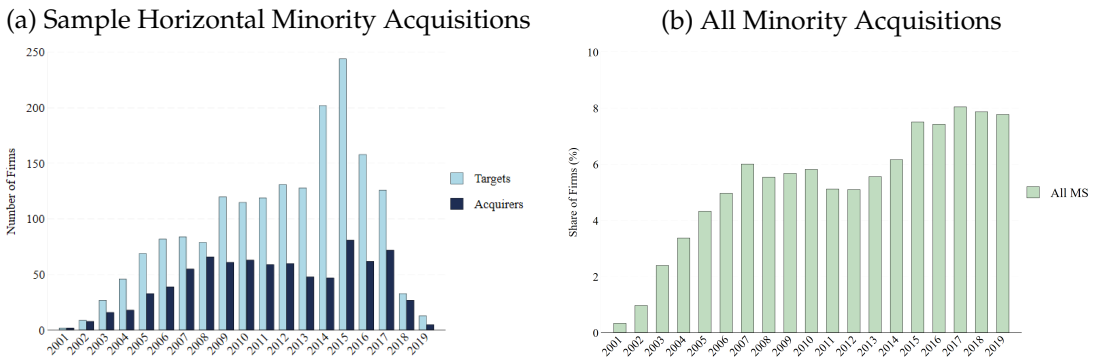

Figure A.1: Distribution of Minority Stake Acquisitions Over Time

Panel (a) shows the distribution of the number of targets and acquirers in our sample involved in horizontal minority stake acquisitions by year, for the period from 2001 to 2019. Panel (b) shows the distribution of all firms involved in any minority stake acquisition by year, as per the Zephyr database.

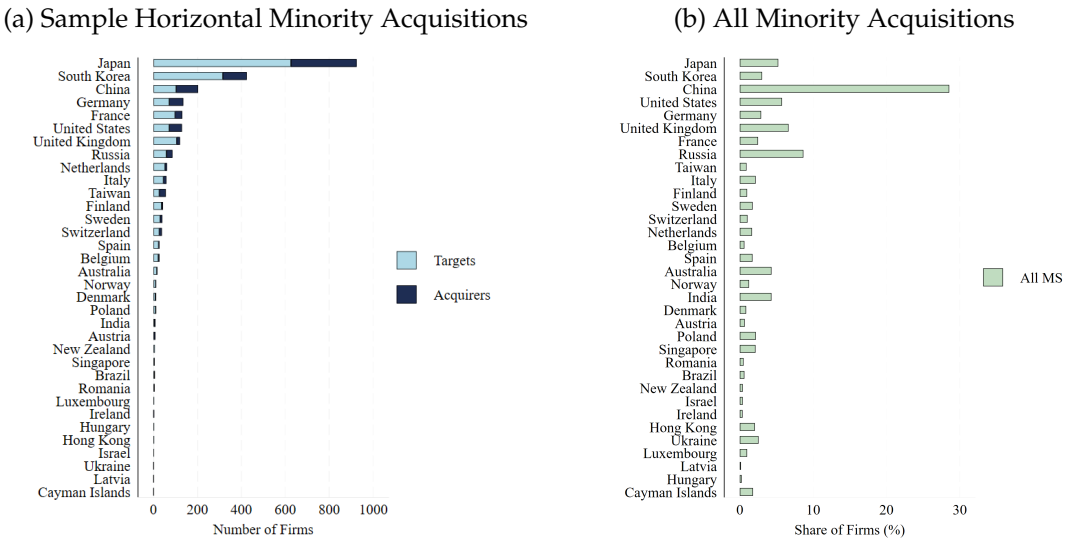

Figure A.2: Distribution of Minority Stake Acquisitions Across Countries

Panel (a) shows the distribution of the number of targets and acquirers in our sample involved in horizontal minority stake acquisitions by country. Panel (b) shows the distribution of all firms involved in any minority stake acquisition by country, as per the Zephyr database.

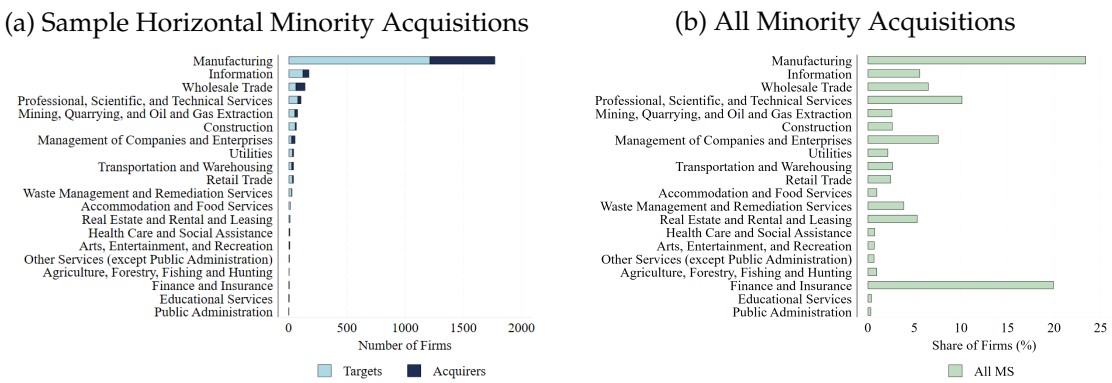

Figure A.3: Distribution of Minority Stake Acquisitions Across Industries

Panel (a) shows the distribution of the number of targets and acquirers in our sample involved in horizontal minority stake acquisitions by 2-digit NAICS code. Panel (b) shows the distribution of all firms involved in any minority stake acquisition by 2-digit NAICS code, as per the Zephyr database.

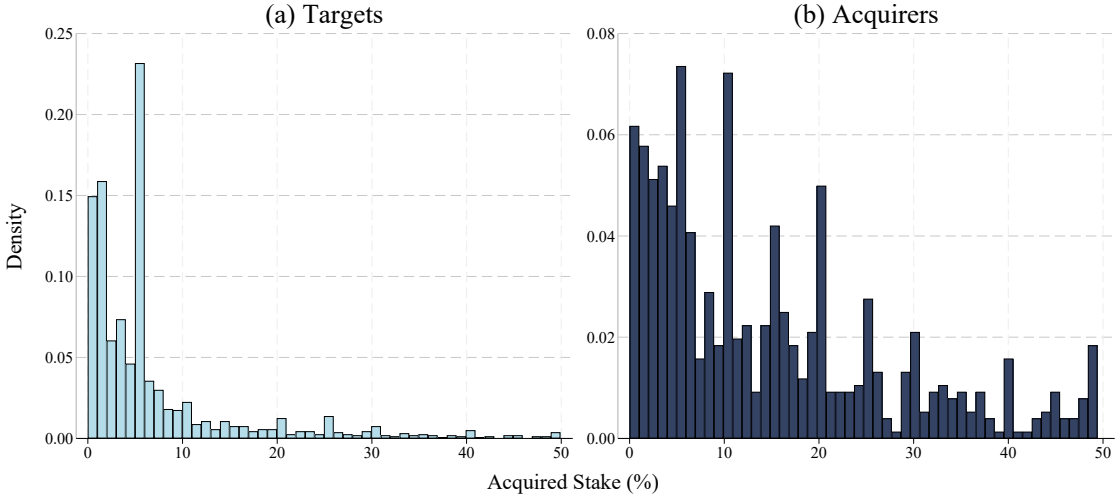

Figure A.4: Distribution of Horizontal Minority Stakes in Our Sample

This figure shows the distribution of horizontal minority stakes for targets and acquirers included in our sample. For approximately 5% of deals, the data displayed different stake levels (for instance, when a target was acquired by various acquirers on the same date). These observations are dropped from this figure.

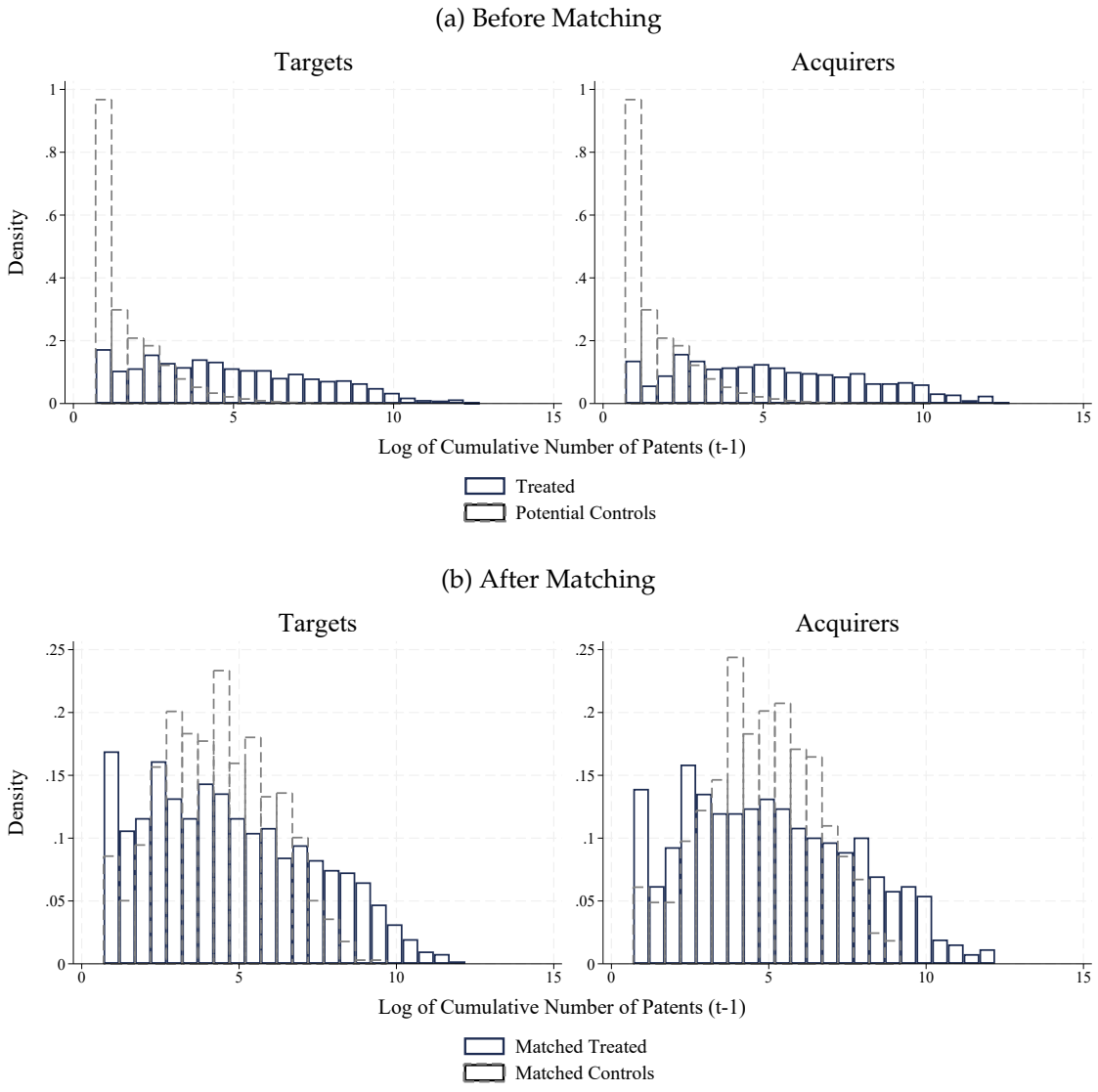

Figure A.5: Distribution of Patents for Treated and Control Firms, Before and After Matching

The top panels plot the distribution of the log of the cumulative number of patents for treated firms (the year before the acquisition) and all potential control firms. The bottom panels plot the distribution of the log of the cumulative number of patents for the matched treated and control firms (the year before the acquisition).

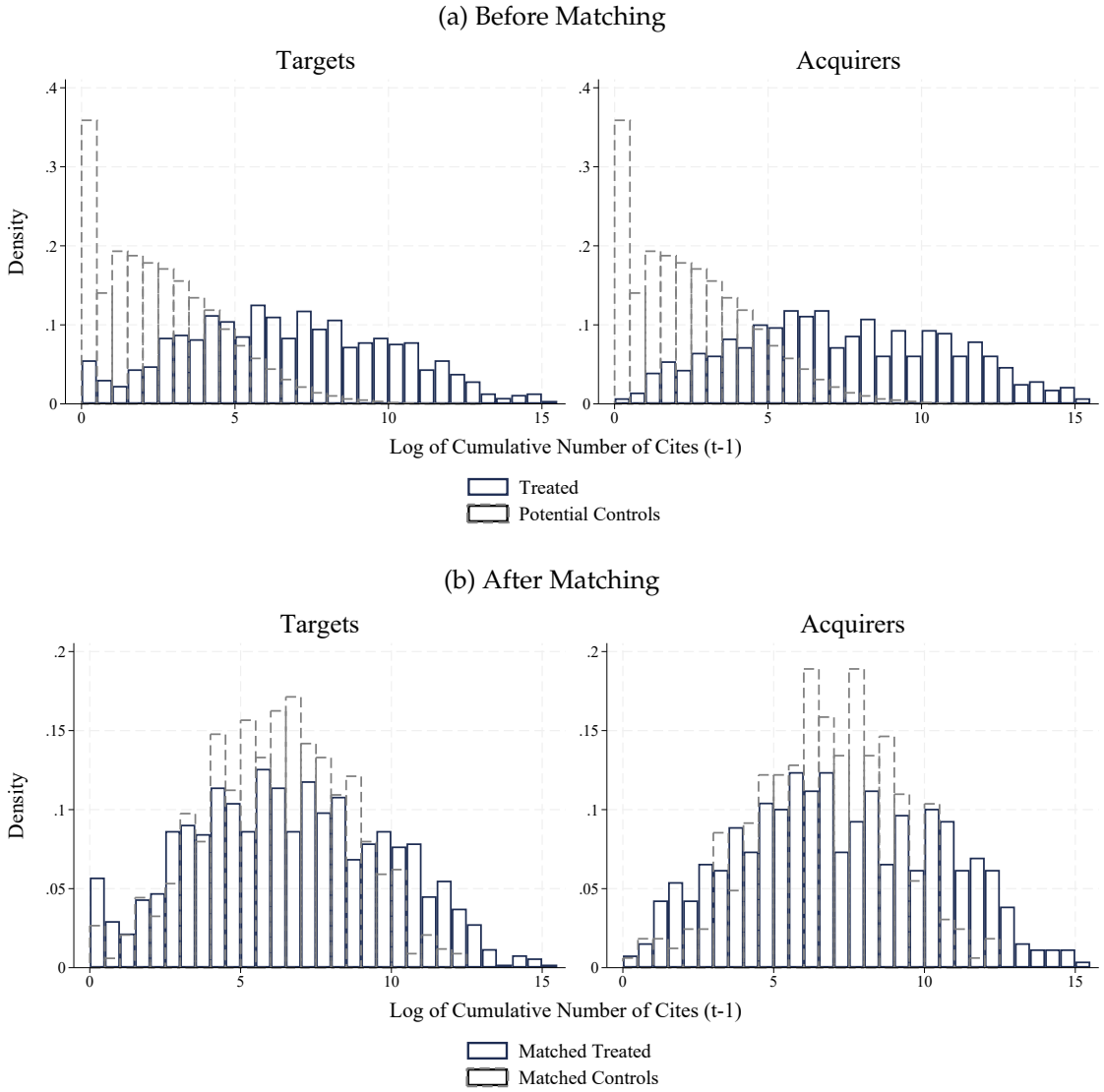

Figure A.6: Distribution of Patent Cites for Treated and Control Firms, Before and After Matching

This figure compares the distribution of patent cites for treated and control firms, before and after the matching. The top panels plot the distribution of the log of the cumulative number of patent cites for treated firms (the year before the acquisition) and all potential control firms. The bottom panels plot the distribution of the log of the cumulative number of patent cites for the matched treated and control firms (the year before the acquisition).

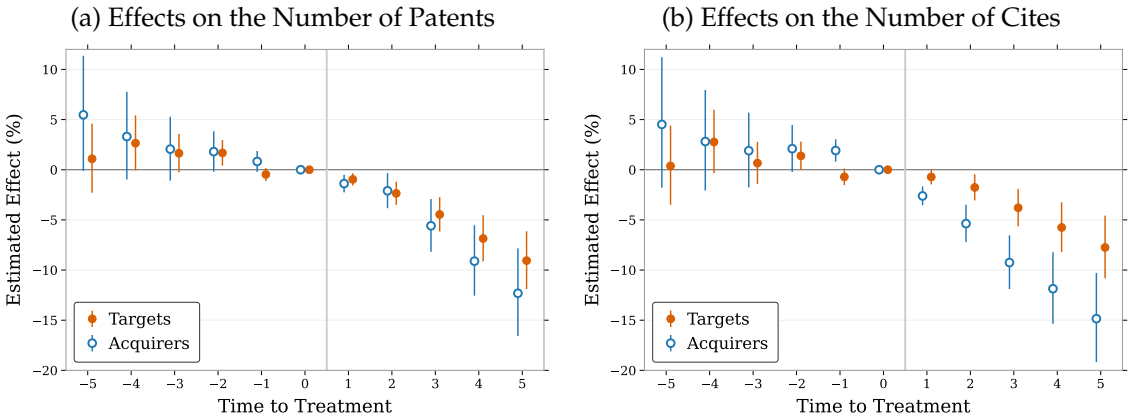

Figure A.7: Event Study on the Effects of Horizontal Minority Stake Acquisitions: Five-year Time Window

This figure extends Figure 2 to include five years before the horizontal minority stake acquisition. The figure presents coefficient estimates in percentage for each of the five years before and five years after the horizontal minority stake acquisition of treated firms. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

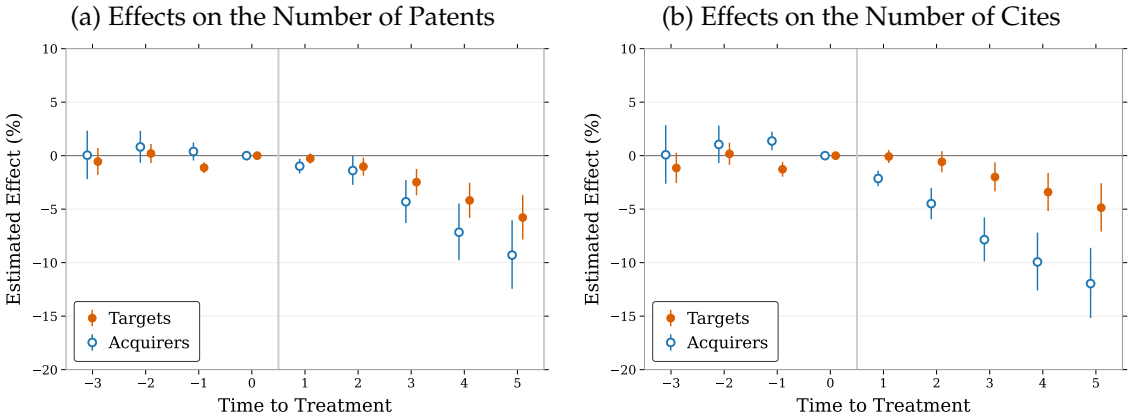

Figure A.8: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity – Matching on Three Nearest Neighbors

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. In contrast to the original event study in Figure 2, the event study uses a control group based on the three nearest neighbors. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

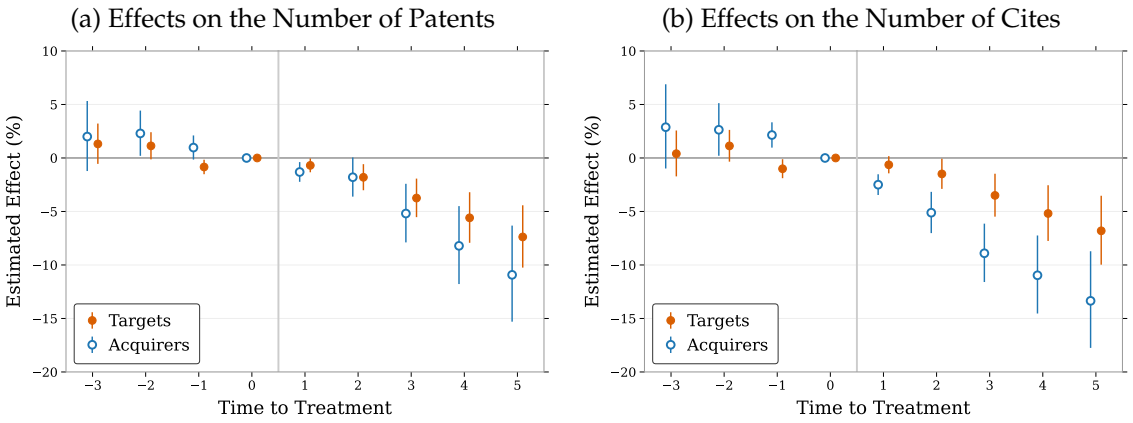

Figure A.9: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity – Estimation With Exact Matching on Country

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. In contrast to the original event study in Figure 2, the event study uses a matching procedure imposing an exact match between treated and control firms on the country code of the firm's headquarter. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

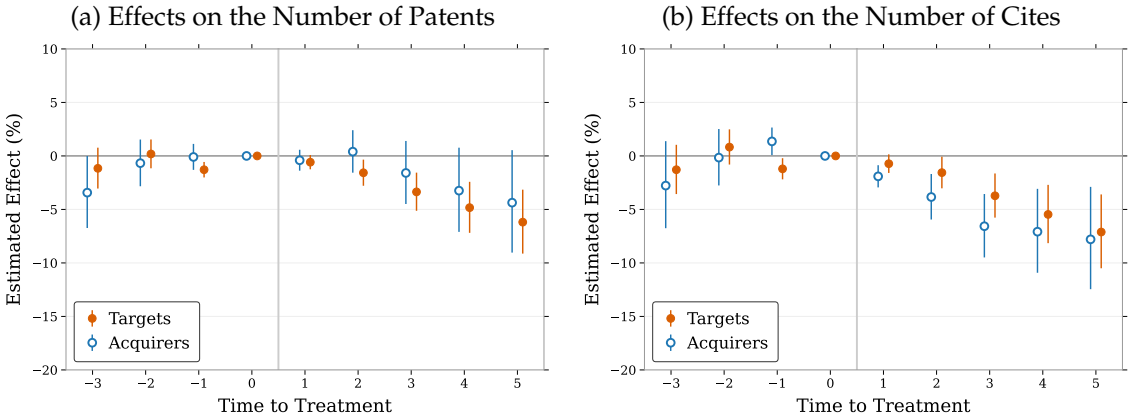

Figure A.10: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity – Matching on 3-digit IPC Overlap Excluding 4-digit Overlap

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. In contrast to the original event study in Figure 2, the event study uses a control group matched on 3-digit IPC overlap while excluding firms that share any 4-digit IPC class with the treated firm. This ensures that control firms operate in the same broad technology area but are not close competitors of the treated pair. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

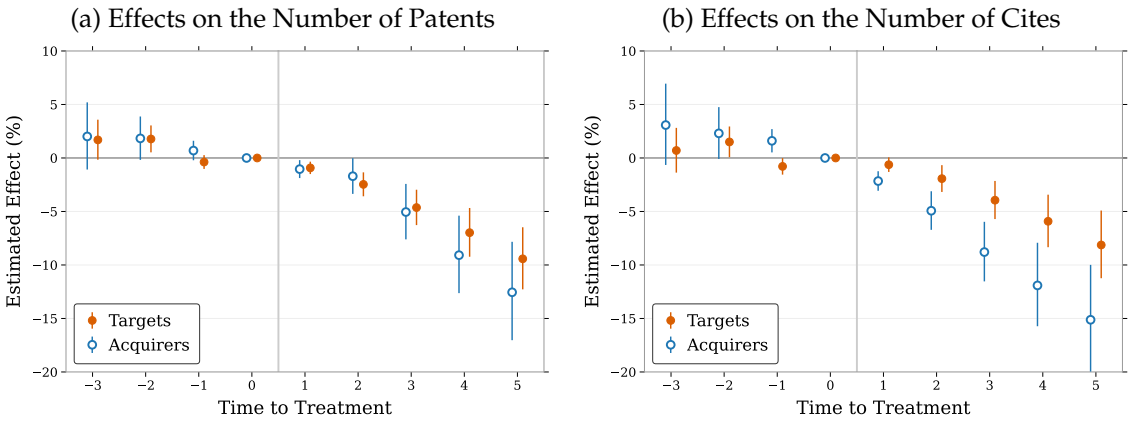

Figure A.11: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity – Estimations with Industry-Specific Trends

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. In contrast to the original event study in Figure 2, the event study also allows for differential industry-specific trends through the inclusion of linear trend variables per 2-digit NAICS code. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

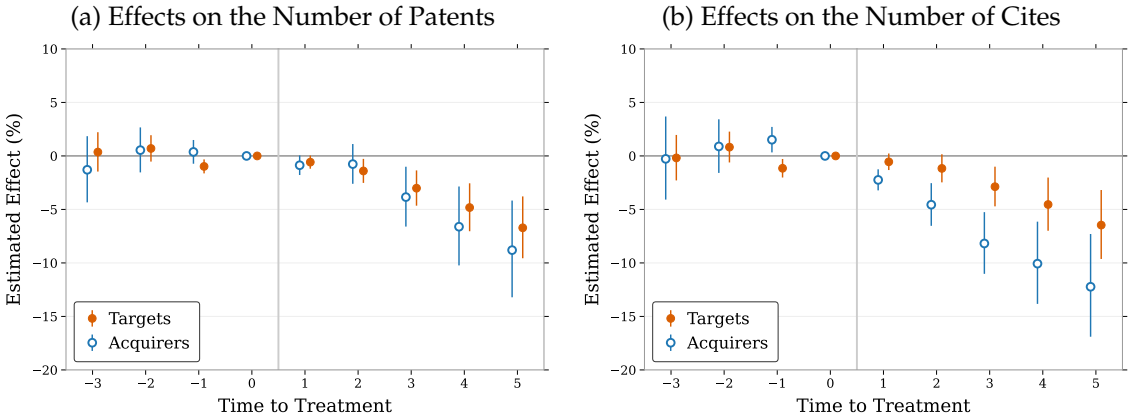

Figure A.12: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity – Estimation With Exact Matching on 2-digit NAICS

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. In contrast to the original event study in Figure 2, the event study uses a matching procedure imposing an exact match between treated and control firms on 2-digit NAICS codes. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

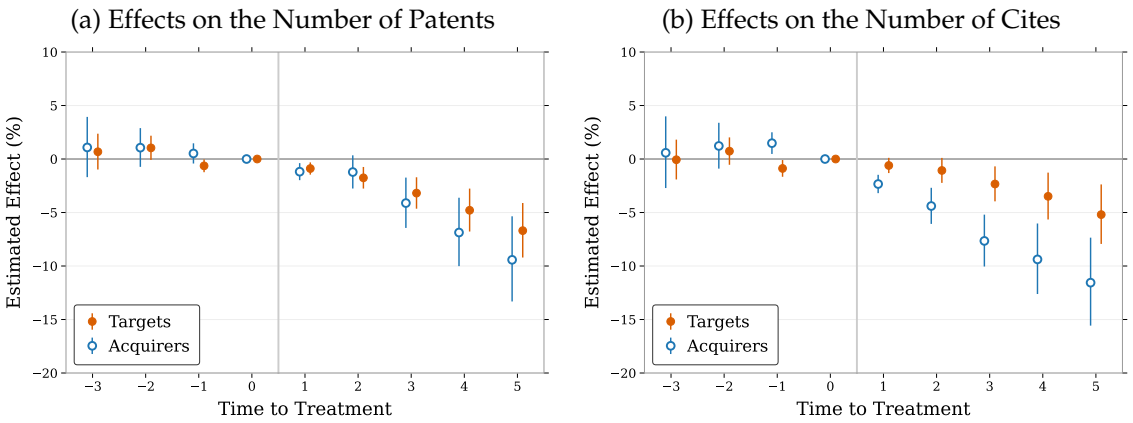

Figure A.13: Event Study on the Effects of Horizontal Minority Stake Acquisitions on Patent Activity – Including Controls

The figure presents coefficient estimates in percentage for each of the three years before and five years after the horizontal minority stake acquisition of treated firms. In contrast to the original event study in Figure 2, the event study also includes the control variables used in the probit estimation for the matching. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. This estimation uses the estimator presented in de Chaisemartin and D'Haultfoeuille (2024). Time fixed effects and firm fixed effects are included. Each panel shows the 95% confidence intervals based on standard errors clustered at the firm level.

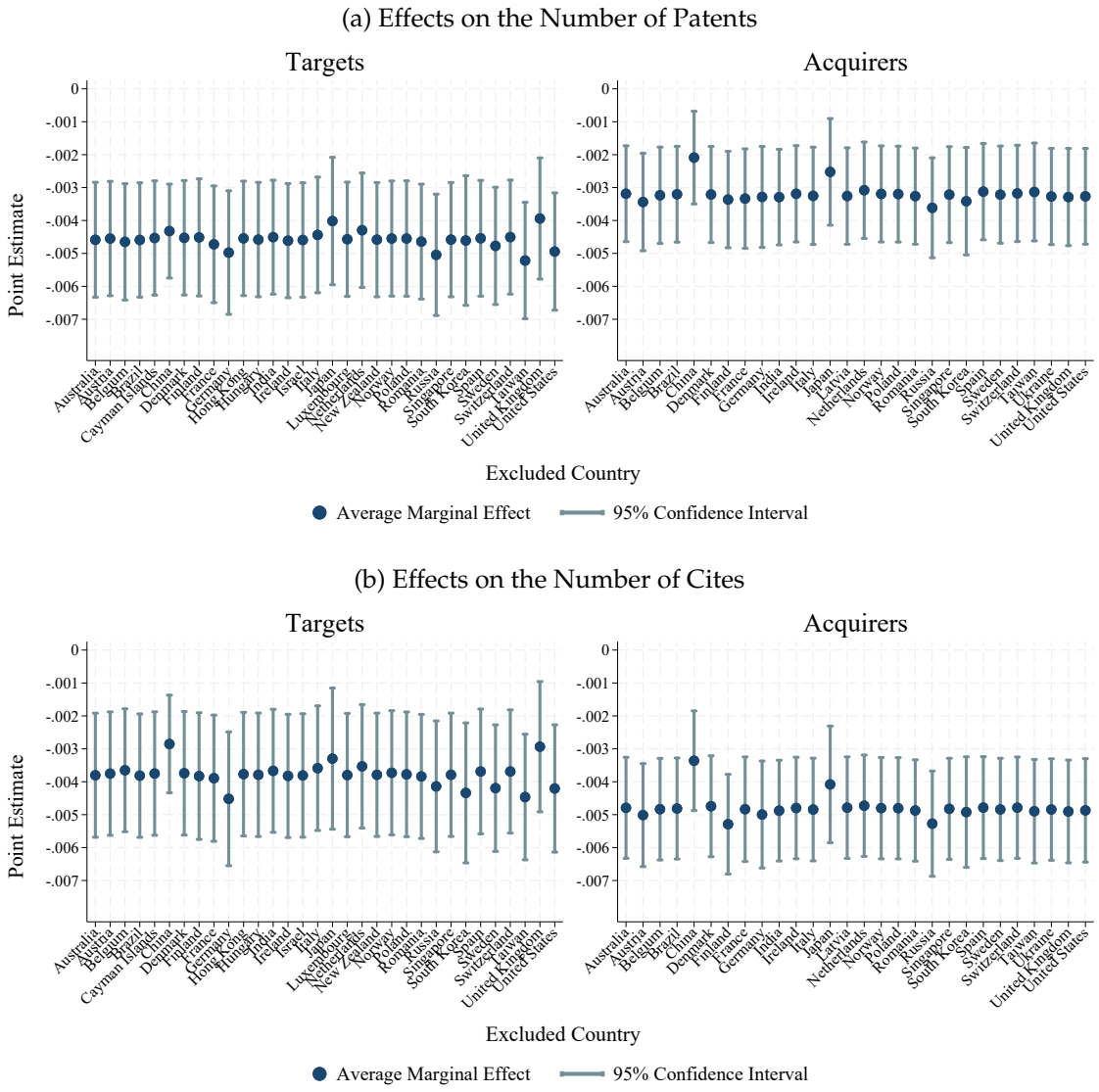

Figure A.14: Leave-One-Country-Out Test

This figure presents the difference-in-differences results for the "leave-one-country-out" test, i.e., dropping treated firms (and their controls) from each country from the sample, one at a time. The figure presents coefficient estimates for the average treatment effect of one additional percent point of minority stake over the five years after the deal. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. Time fixed effects and firm fixed effects are included.

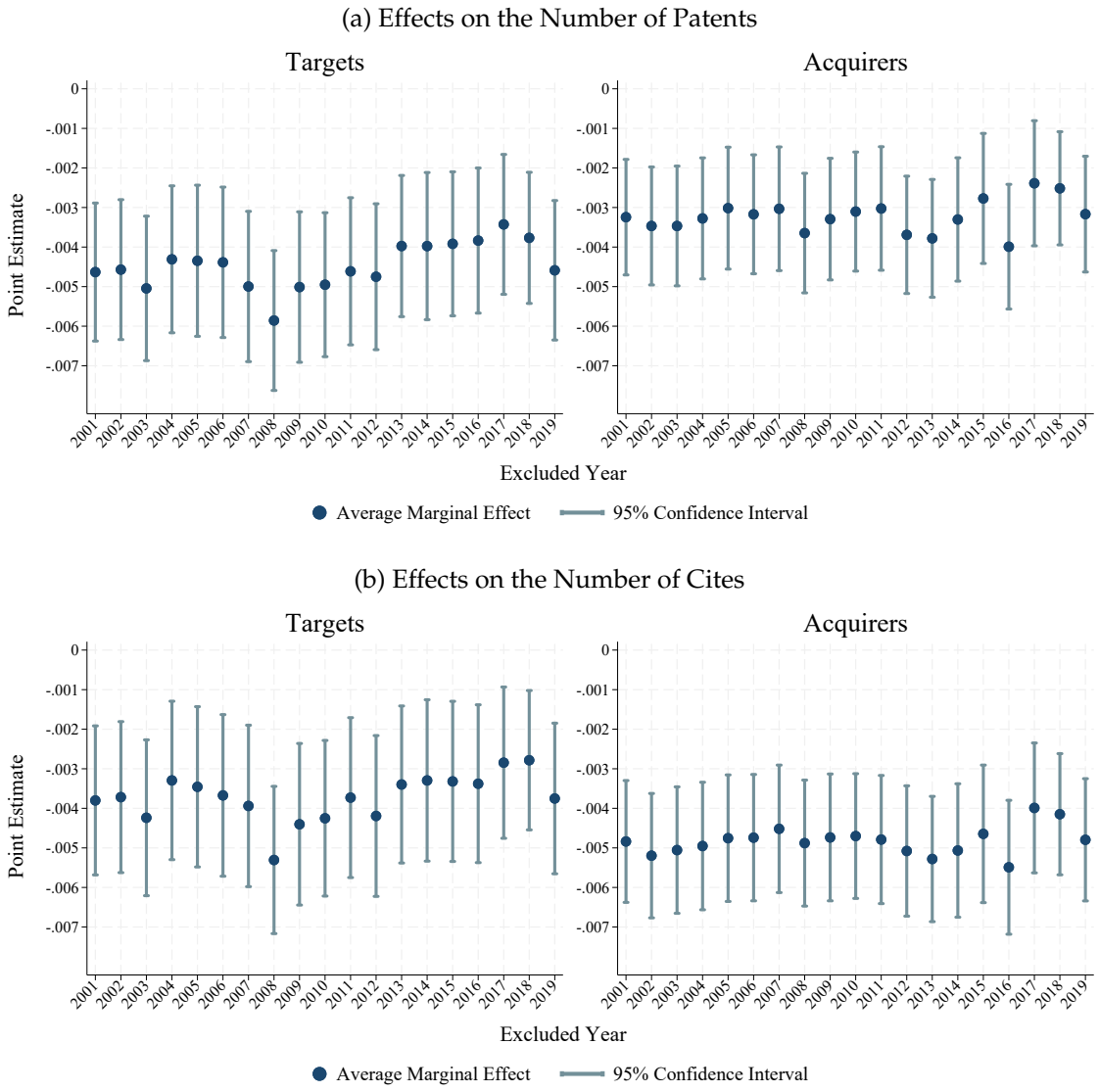

Figure A.15: Leave-One-Year-Out Test

This figure presents the difference-in-differences results for the "leave-one-year-out" test, i.e., dropping treated firms (and their controls) involved in an acquisition in a specific year from the sample, one year at a time. The figure presents coefficient estimates for the average treatment effect of one additional percent point of minority stake over the five years after the deal. The dependent variable is either the log of the cumulative number of granted patents (dated by application year) or the log of the cumulative number of forward citations, per firm and year. Time fixed effects and firm fixed effects are included.

A.3 Tables

Table A1: Dataset Construction

	Action	Remaining Companies	
		Targets	Acquirers
Step 1	Number of horizontal firms	4,561	2,898
Step 2	Eliminate firms without Orbis financial data	2,918	1,428
Step 3	Eliminate firms involved in majority acquisitions (within 3 years of the minority acquisition)	1,139	612
Step 4	Eliminating non-corporate firms (e.g., financial company, mutual and pension fund)	1,106	586
Step 5	Eliminate firms without 3 years of consecutive patent data	1,082	579

This table details how we constructed the datasets for targets and acquirers that we use in the paper and the number of firms that remained after each step.

Table A2: Pre-Acquisition Summary Statistics

(a) Targets

	Mean	10-Perc.	Median	90-Perc.	Obs.
Sales(t-1)	4.0	0.0	0.6	12	1,040
Number of Employees(t-1)	15,712	130	2,368	41,593	1,040
Age(t-1)	45	9.0	37	91	1,040
Fixed Assets(t-1)	3.2	0.0	0.3	8.1	1,040
Total Assets(t-1)	5.3	0.0	0.6	13	1,040
Intangible Fixed Assets(t-1)	0.9	0.0	0.0	1.1	1,037
Liquidity Ratio(t-1)	1.9	0.6	1.1	3.2	1,036
# Granted Patent Applications(t-1)	129	0.0	4.0	205	1,040
Cumulative # Granted Patent Applications(t-1)	3,286	3.0	84	5,463	1,040
# Patent Cites(t-1)	1,578	0.0	12	2,009	1,040
Cumulative # Patent Cites(t-1)	50,759	11	660	60,678	1,040

(b) Acquirers

	Mean	10-Perc.	Median	90-Perc.	Obs.
Sales(t-1)	7.6	0.0	1.3	24	552
Number of Employees(t-1)	25,040	180	4,135	70,336	552
Age(t-1)	44	10	30	93	552
Fixed Assets(t-1)	5.8	0.0	0.8	15	549
Total Assets(t-1)	9.6	0.1	1.6	25	552
Intangible Fixed Assets(t-1)	1.4	0.0	0.0	2.9	540
Liquidity Ratio(t-1)	2.0	0.6	1.2	3.4	542
# Granted Patent Applications(t-1)	248	0.0	8.0	392	551
Cumulative # Granted Patent Applications(t-1)	5,880	5.0	142	10,929	551
# Patent Cites(t-1)	4,263	0.0	46	5,436	551
Cumulative # Patent Cites(t-1)	101,483	15	1,127	168,165	551

This table provides summary statistics on the acquiring and target firms included in our sample. The table summarizes the values of firm-level variables for acquirers and targets in the pre-acquisition period. The Sales and the Assets are expressed in billion euro. The Liquidity Ratio is expressed in percentage. The unit of observation is the value one year prior the acquisition (t-1), per firm and year. Outliers are excluded from this table by dropping the top percentile of observations in terms of Sales(t-1).

Table A3: Relative Size of Parties Pre-Acquisition

(a) Targets

	10-Perc.	25-Perc.	50-Perc.	75-Perc.	90-Perc.	Obs.
Ratio of Sales	0.01	0.04	0.21	1.08	5.46	423
Ratio of # of Employees	0.02	0.07	0.31	1.84	10.98	423
Ratio of Age	0.20	0.50	0.93	1.64	3.17	421
Ratio of Fixed Assets	0.01	0.03	0.18	0.92	5.86	398
Ratio of Total Assets	0.01	0.04	0.20	1.02	4.60	423
Ratio of Intangible Fixed Assets	0.00	0.01	0.12	1.44	12.53	378
Ratio of Liquidity	0.30	0.58	1.05	1.89	3.69	395
Ratio of # Granted Patents	0.00	0.00	0.07	1.00	3.25	291
Ratio of Cumulated # Granted Patents	0.01	0.06	0.55	5.00	148.00	398
Ratio of # Patent Cites	0.00	0.00	0.04	1.00	3.45	293
Ratio of Cumulated # Patent Cites	0.00	0.02	0.38	3.01	27.80	371

(b) Acquirers

	10-Perc.	25-Perc.	50-Perc.	75-Perc.	90-Perc.	Obs.
Ratio of Sales	0.39	1.32	8.14	32.26	150.85	402
Ratio of # of Employees	0.35	1.27	5.36	22.25	77.46	401
Ratio of Age	0.43	0.86	1.27	2.40	6.00	401
Ratio of Fixed Assets	0.43	1.73	8.09	42.84	218.63	398
Ratio of Total Assets	0.49	1.53	7.34	30.31	107.35	402
Ratio of Intangible Fixed Assets	0.08	1.00	8.64	67.73	552.84	357
Ratio of Liquidity	0.27	0.55	0.88	1.49	2.54	395
Ratio of # Granted Patents	0.00	0.17	1.00	7.25	37.00	285
Ratio of Cumulated # Granted Patents	0.12	0.67	5.00	35.00	317.15	401
Ratio of # Patent Cites	0.00	0.20	1.00	9.13	80.54	283
Ratio of Cumulated # Patent Cites	0.10	0.59	5.41	53.28	613.86	390

Panel (a) compares the targets and their respective acquirers across several variables the year before the minority stake acquisition. Similarly, Panel (b) compares the acquirers and their respective targets across several variables the year before the minority stake acquisition. This table presents the average ratios of values between targets and their acquirers (and between acquirers and their targets) for different variables in the pre-acquisition period (t-1). Observations for which (i) there are more than two parties in the acquisition, (ii) there are missing data for the other party in the acquisition, are excluded from this table. Outliers are excluded from this table by dropping the top percentile of observations in terms of Ratio of Sales.