The Real Estate Collateral Channel Revisited: Evidence from a Quasi-Natural Experiment in China

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Abstract

The apparent absence of a real estate collateral channel for firm investment in China presents a major puzzle in corporate finance. This paper provides a new explanation by revealing profound heterogeneity linked to firm ownership. We find that for State-Owned Enterprises (SOEs), rising real estate values do lead to a significant increase in borrowing, demonstrating the financing part of the channel is active. Critically, however, this credit expansion does not translate into higher real investment. For non-SOEs, the channel appears inactive for both borrowing and investment. Our results show that the aggregate null finding on investment in prior work is not due to an inactive channel, but a dysfunctional one, where soft budget constraints for SOEs sever the link between financing and productive activity, leading to capital misallocation.

1 Introduction

The intricate relationship between real estate markets and corporate behavior is a cornerstone of modern financial economics. Housing prices, representing a significant component of national wealth, have profound implications that extend far beyond residential markets, influencing corporate financing capacity, investment decisions, and ultimately, aggregate economic activity. A prevalent channel through which these effects are transmitted is the collateral channel, where the value of real estate assets on a firm's balance sheet directly impacts its borrowing capacity (Chaney et al., 2012). An appreciation in real estate values can enhance a firm's collateralizable wealth, thereby relaxing credit constraints and facilitating new investment (Cvijanović, 2014). Conversely, a downturn in the property market can constrict access to credit, forcing firms to curtail investment projects.

The Chinese economy, with its unprecedented real estate boom over the past two decades, presents a fascinating and critical setting to test the external validity of this theory. However, the applicability of the collateral channel in China remains a subject of academic debate, creating a significant puzzle in the literature. In a study, Wu et al. (2015b) examined a large panel of Chinese firms and, contrary to the findings from other countries, found no evidence of a real estate collateral channel effect on firm investment. They argue that unique institutional features of China's financial system, where even financially constrained firms appear able to commit to debt repayment without relying on traditional collateral mechanisms, may render the channel inactive.

This paper builds directly on this foundational puzzle. We do not dispute the aggregate finding that the collateral channel fails to boost investment. Instead, we investigate the underlying mechanisms that produce this result. We argue that the aggregate null finding in studies like Wu et al. (2015a) is not evidence of a uniformly inactive channel, but rather masks two distinct and offsetting economic realities rooted in China' s dual-track economy. Specifically, the institutional landscape differs dramatically for State-Owned Enterprises (SOEs) versus non-SOEs. SOEs often operate under soft budget constraints and benefit from implicit government guarantees, granting them preferential access to credit irrespective of their collateral value (Kornai, 1986; Gao et al., 2023; Mo and Soudan, 2022). In contrast, non-SOEs, which are the primary engines of innovation and job growth, face more significant financial frictions and are more likely to rely on tangible assets to secure external financing.

To cleanly identify the causal effects of real estate price shocks, we construct a quasi-natural experiment from the staggered rollout of Home Purchase Restriction (HPR) policies across major Chinese cities. These policies, designed to cool overheating property markets, sharply limited housing purchases where they were enacted. This action, however, predictably diverted investment capital and speculative demand toward nearby, unrestricted cities (Li et al., 2020; Deng et al., 2022). This spillover created a plausibly exogenous and positive housing price shock in these adjacent areas, providing the variation needed for our study.

We leverage this policy-induced variation within a Difference-in-Differences (DiD) framework. Our empirical strategy compares the financing and investment decisions of firms headquartered in these adjacent, spillover-affected cities (the treatment group) to those of firms in otherwise similar but unaffected cities (the control group). By examining changes before and after a neighboring city implements an HPR policy, we can isolate the causal impact of the real estate price shock. This identification strategy is powerful because the timing and location of the shock are determined by regulatory decisions external to the firms we analyze, thus mitigating the critical endogeneity concerns that plague studies relying on broad market fluctuations.

Our findings reveal a dysfunctional, rather than inactive, collateral channel. We find that for SOEs, an exogenous increase in local real estate values leads to a statistically and economically significant increase in borrowing. The coefficient on our interaction term for SOEs' new borrowing is 0.190 and is statistically significant. To gauge the economic magnitude, we scale this effect by the sample variation; the increase in new short-term loans corresponds to

24.7% of a standard deviation, an economically meaningful expansion of credit. Critically, however, this credit expansion does not translate into higher real investment for SOEs. For non-SOEs, the channel appears entirely inert, with no significant effect on either borrowing or investment. The causal interpretation of these results is validated by extensive in-time and in-space robustness tests, including placebo analyses and alternative definitions of the treatment group.

Our study makes several key contributions. First, we resolve the 'missing channel' puzzle in the Chinese context by showing the collateral channel is not inactive, but rather dysfunctional. We reconcile our findings with the influential work of Wu et al. (2015b). Second, by using the spillover of HPR policies as a quasi-natural experiment, we provide clean causal evidence on the channel's effects, overcoming the endogeneity concerns that affect studies using broader market fluctuations. Finally, our 'borrowing without investment' result for SOEs offers direct evidence of severe capital misallocation driven by institutional frictions. This finding sheds new light on how implicit guarantees and soft budget constraints can fundamentally alter the transmission of macroeconomic shocks, with critical policy implications for stimulating productive growth in economies with large state sectors (Gao et al., 2023; Mo and Soudan, 2022; Chang et al., 2024; Jin et al., 2025).

The remainder of this paper proceeds as follows. Section 2 reviews the related literature. Section 3 briefly reviews the institutional background surrounding China's housing market and the motivations behind housing purchase restrictions. In Section 4, we describe the data and outline our empirical framework. We present our baseline DiD results and parallel assumption checks in Section 5. Section 6 reports additional robustness tests. Section 7 and 8 provides discussion and concluding remarks.

2 Literature Review

The collateral channel of corporate finance has long been recognized as a key mechanism linking asset values to real economic activity. Early work by Fisher (1933); Stiglitz and Weiss (1981); Bernanke and Gertler (1989) highlight how declines in asset values can tighten borrowing constraints, amplifying business cycle downturns. In such models, firms use tangible assets—particularly real estate—as pledgeable collateral to secure external financing. Empirical research in developed markets strongly supports this view. Using U.S. data, Chaney et al. (2012) document that firms increase investment by about 6 cents for each dollar increase in collateral value. Similarly, Gan (2007) exploits the 1990s Japanese land price collapse and finds large declines in corporate investment and debt capacity for firms suffering collateral losses. These studies establish that rising real estate prices relax financing constraints and spur corporate investment —a phenomenon often dubbed the real estate collateral channel.

Given these findings, the absence of a collateral effect in China's corporate sector has been puzzling. In a prominent study, Wu et al. (2015a) examine a broad panel of Chinese listed firms and report no significant link between firms' real estate holdings and their investment spending. This null result contrasts sharply with the U.S. and Japanese evidence, suggesting that China's institutional context may blunt the traditional collateral mechanism. Wu et al. (2015a) attribute the difference to unique features of China's financial system, where even firms that appear financially constrained can obtain credit without relying on hard collateral. China's state-dominated banking sector and relationship-based contracting may allow credit to flow based on implicit guarantees or government connections, rather than formal collateral values. Indeed, a substantial literature documents that Chinese state-owned enterprises (SOEs) enjoy preferential access to financing, whereas privately owned firms face much tighter credit constraints (Jin et al., 2023; Geng and Pan, 2024). In the credit allocation of China, private firms are often relegated behind SOEs, which benefit from implicit state backing.

The institutional features of China's financial system may explain why traditional collateral mechanisms operate differently than in developed markets. Implicit guarantees and political objectives can lead state firms to access credit for reasons beyond profit-maximizing investment, resulting in capital allocation that may not follow conventional collateral logic. Our study contributes to this literature by explicitly accounting for ownership structure when testing the collateral channel in China. We build on prior work emphasizing how institutions shape financial frictions, particularly the government-bank nexus in credit markets (La Porta et al., 2002; Allen et al., 2005)). By exploiting a policy-induced shock to real estate values, we provide new causal evidence that the collateral channel operates asymmetrically across ownership types: it increases debt capacity without corresponding investment among state-owned firms, while remaining largely inoperative for financially constrained private firms. These findings reconcile the apparent absence of China's collateral channel with international evidence and demonstrate that institutional context fundamentally shapes how asset values transmit to corporate financing and investment decisions.

3 Institutional Background

The housing market in China has witnessed a boom over the past two decades (Deng et al., 2022; Rogoff and Yang, 2024). The growth rate of real estates was even greater than the growth rate of per capita GRP in Tier 1 and Tier 2 cities Liu and Xiong (2018). They also find that the average housing price in Tier 1 cities (Beijing, Shanghai, Guangzhou and Shenzhen) had grown over 600% from 2003 to 2017. Tier 2 cities also had an enormous housing price appreciation of 400%, while Tier 3 cities had grown 200% on average in the same period.

The reason behind the substantial boom remained puzzling. Some researchers find that as China were in a transition stage, the expected capital return dropped, therefore the expected housing demand rose, driving up the current price (Chen and Wen, 2017). Some researchers argue that real estate is an popular investment vehicle because other alternative channels such as stocks, bonds has not offered attractive returns (Fang et al., 2016). Due to this reason, listed firms also participate actively in investing in real estate and land market. According to Liu and Xiong (2018), public non-financial and non-real estate firms holds around 400 Billion Yuan of lands in 2011, accounting for 30% of the firms' net investment.

3.1 The housing purchase restrictions policies

The central government implemented two waves of housing purchase restrictions (HPR) in attempt to cool down the soaring house price in some cities. In particular, the restrictions included raising the down payment requirement to higher levels, and increasing mortgage rates on, occasionally outright forbidding, investment purchases. The restrictions are tight and is the home purchase eligibility is estimated to be 22.5% of the original home (Zheng et al., 2023), demonstrating their effectiveness as demand-side controls. The first wave of HPR were enforced in 46 cities in 2010-2011 and in result reduced housing price in cities that implemented the restrictions (Wu and Li, 2018; Lu et al., 2021).

Despite the central government's continued advocacy for HPR as a tool to suppress speculative demand, 2013 marked a significant policy inflection point as several cities began easing restrictions. This shift reflected heterogeneous real estate market conditions throughout the country. While first-tier cities such as Beijing and Shanghai maintained robust housing demand and price appreciation, numerous smaller and lower-tier cities confronted challenges including inventory surpluses and transaction volume declines. In response to these localized market conditions, certain local governments implemented targeted relaxation measures—reducing down payment thresholds, easing restrictions for non-local residents, and streamlining homebuyer approval processes.

Following this period of policy relaxation post-2013, housing markets experienced another pronounced price surge, prompting the central government to institute a second wave of HPR in 2016 to restabilize the market.

3.2 Spillover and quasi-experiment

Housing prices in China are highly interactive with each other (Yang et al., 2018). Therefore we may suspect that the restriction policy may affect the price of nearby cities. In reality, although the HPR is indeed effective in controlling the housing price in implemented cities, there exists geographical spillover to cities near the implemented cities. To illustrate, buyers in restricted cities shifted their demand to cities nearby. For instance, when Beijing implemented HPR, housing demand shifted toward surrounding cities such as LangFang. This phenomenon is documented by various literature. Li et al. (2020) employed a regression discontinuity design and find that Langfang, a city adjacent to Beijing (HPR implemented city), witnessed a exogenous price increase in housing market. Deng et al. (2022) find that the average spillovered cities experienced an 8% price increase relative to control group which did not implemented HPR nor located near those cities.

The spillover offers researcher a quasi-experiment setting, since the increase in spillovered cities are perceived as exogenous, that is, unrelated to the fundamentals of the cities themselves. Deng et al. (2022) leveraged this setting and find that there are more automobile purchase in spillovered cities where home price increased, showing a positive wealth effect for homeowners.

In this study, we utilize the second wave of HPR and employ a similar approach by separating the cities into treatment and control group. Following Deng et al. (2022), the city is classified as a treatment city if it is located within 250 kilometers around the HPR implemented cities. The controlled cities are cities that didn't implement HPR nor locating within 250 kilometers around them. Since our primary aim is to find out the relationship between real estate holdings by firm and their behavior. The cities in our sample is further filtered where there exist listed companies' headquarter, since we assumed firms only hold real estate assets in their headquarter cities (Chaney et al., 2012; Cvijanović, 2014; Lu et al., 2019).

4 Data and Empirical Strategy

4.1 Data and Variables

This study utilizes a merged dataset of firm-level data for all A-share listed companies in China and city-level housing price data from 2013 to 2019. The sample period begins in 2013, coinciding with the relaxation of the initial round of Home Purchase Restrictions (HPR), and concludes in 2019 to exclude the confounding effects of the COVID-19 pandemic.

For firm-level variables, we collect Invest1, defined as the sum of the annual change in net fixed assets, depreciation and amortization, scaled by lagged total assets. We also collect an alternative measure Invest2, which is the annual change in net fixed assets, scaled by lagged total assets (Jiang et al., 2018). The control variables include the value of fixed assets, total assets, total operating revenue, total cash inflows from operating activities, the Book-to-Market ratio (Book/Market), the net profit margin (NetProfit, calculated as net profit divided by operating revenue), TobinQ. Additionally, the 2012 CSRC industry classification is included to control for industry-specific effects. These control variables account for factors that may influence the relationship between investment and firm-specific characteristics. The data for these variables are sourced from the CSMAR database and the WIND terminal, which provide the most comprehensive financial and economic data for Chinese firms.

The data preparation process follows the approach outlined by Chaney et al. (2012) and Lu et al. (2019) with several adjustments. First, consistent with standard practices in the literature, firms in the ‘finance’ and ‘insurance’ sectors are excluded, as their accounting data significantly differs from that of non-financial firms. Similarly, companies in the ‘real estate’ and ‘construction’ industries are removed to focus on the effects of a booming real estate market on firms whose primary activities are not directly tied to it. Additional exclusions include businesses in the ‘agriculture,’ ‘mining,’ ‘transportation and storage,’ and ‘production and supply of electricity, gas, and water’ sectors, as firms in these industries often hold real estate outside urban areas. Retaining these firms could bias the results since city-level housing prices are used to represent the real estate market. Next, observations missing critical accounting information, such as total assets or total sales, are removed. Firms registered inside of HPR implemented cities are excluded as well. All variables are winsorized at the 0.01 and 0.99 levels to mitigate the influence of extreme outliers.

Following Deng et al. (2022), we selected several time varying city-level variables to control. Namely the log of gross regional product, population, square meters of road per capita, the log for active residents.

Table 1 provides detailed definitions for all variables employed in our empirical analysis. Our primary dependent variable, New_Stloan, captures the value of new short-term loans, normalized by lagged fixed assets, to measure changes in firm borrowing. The key independent variable, Treatment, is an indicator variable used to distinguish firms within our designated treatment group. To control for confounding factors, we incorporate a comprehensive set of variables consistent with the corporate finance literature. These include standard firm-level characteristics measuring size (log(Assets), log(Revenue)), growth opportunities (Tobin's Q), valuation (BM), profitability (ROA), capital structure (Lev), and ownership concentration (TOP1). Furthermore, to account for the influence of local economic conditions, we control for the natural logarithm of Gross Regional Product per capita (log(GRP_per_capita)), regional population and active residents size (Population_size and log(Residents)), and infrastructure level (Road_area_per_capita).

4.2 Descriptive Statistics

Table 2 presents the annual distribution of firms in our sample, categorized by ownership structure for the period 2013—2019. The analysis that follows is conducted on two primary subsamples: State-Owned Enterprises (SOEs) and non-SOEs. As detailed in the table, the number of SOEs included in our sample ranges from 320 to 338 annually. The non-SOE subsample comprises between 385 and 469 firms each year. The total number of firms in the sample varies across the period, with the overall sample consisting of 5,337 firm-year observations. This table provides the descriptive context for the composition of the subsamples used in our main empirical tests.

Table 3 presents the summary statistics for the key firm-level and regional variables used in our analysis, partitioned by the ownership structure of the firms: state-owned enterprises (SOEs) and non-state-owned enterprises (Non-SOEs). For each variable, we report the number of firm-year observations, the mean, the standard deviation, and the median.

The statistics reveal some differences between the two groups. On average, SOEs are slightly larger and more levered. SOEs also exhibit greater ownership concentration, with the largest shareholder (TOP1) holding an average of 35.36% compared to 30.30% in Non-SOEs. Conversely, Non-SOEs appear to have higher market valuations, with a mean TobinQ of 2.20, which is notably higher than the 1.89 for SOEs. Furthermore, Non-SOEs report a higher mean for new short-term loans (New_Stloan) at 0.19, compared to 0.08 for SOEs, suggesting different financing behaviors between the two types of firms. The regional macroeconomic variables, such as log(GRP_per_capita) and Population_size, show less variation between the two subsamples.

5 Results

5.1 Effect of HPR Spillover on Real Estate Prices

Housing price regulation (HPR) policies represent an important lever for governments seeking to stabilize or influence housing markets. Understanding the effects of such policies on real estate prices is critical, as housing prices are not only a major component of household wealth but also deeply connected to broader economic activity. Changes in housing prices can affect household consumption (Deng et al., 2022), labor mobility, and firm behavior, making them a key transmission mechanism for policy impacts. This section examines the causal effect of the HPR policy on real estate prices, using a Difference-in-Differences (DiD) framework to estimate the policy's impact. The findings from this analysis also provide the basis for exploring the broader economic consequences of housing price changes in subsequent sections of this paper.

The following empirical specification estimates the effect of the treatment on house prices using a Difference-in-Differences (DiD) approach with city and year fixed effects and clustered standard errors at the city level:

$$
\begin{aligned}
\ln(\text{House\_Price}_{ct}) = {} & \beta_0 + \beta_1(\text{Treatment}_c \times \text{Post}_t) \\
& + \beta_2 \text{Treatment}_c + \beta_3 \text{Post}_t \\
& + \gamma_1 \ln(\text{GRP\_per\_capita}_{ct}) + \gamma_2 \text{Population\_Size}_{ct} \\
& + \gamma_3 \text{Road\_Area\_Per\_Capita}_{ct} + \gamma_4 \ln(\text{Residents}_{ct}) \\
& + \alpha_c + \delta_t + \epsilon_{ct},
\end{aligned} \tag{1}
$$

The dependent variable in this specification is the logarithm of house prices in city c at time t, $\ln(\text{House\_Price}_{ct})$. The key variable of interest is the interaction term $\text{Treatment}_c \times \text{Post}_t$, which captures the causal effect of the treatment on house prices. Here, Treatment_c is the treatment group indicator, equal to 1 for cities that received the treatment and 0 otherwise. Post_t is a time indicator that equals 1 for the post-treatment period and 0 for the pre-treatment period. The coefficient β_1 on the interaction term represents the DiD estimate of the treatment effect.

The models in Column (3) includes several control variables to account for local economic and infrastructure conditions. These controls are the logarithm of gross regional product per capita, population size, road area per capita in square meters, the logarithm of resident population, urbanization rate, and infrastructure level. Including these controls helps isolate the effect of the treatment by accounting for observable differences across cities. City fixed effects, α_c, and Year fixed effects, δ_t, are also included to control for city- and time-specific shocks or trends. To account for potential heteroskedasticity and autocorrelation within cities over time, standard errors are clustered at the city level.

Table 4 reveals the effect of HPR on treated cities. The coefficients for the interaction term Treatment X Post is 0.090 to 0.091, which is statistically significant at the 1% level across different specifications. This indicates that, on average, the treatment is associated with a 9.1% increase in house prices in treated cities during the post-treatment period, relative to non-treated cities and the pre-treatment period, after controlling for the included covariates and city and year fixed effects. The positive and significant coefficient suggests that the treatment had a meaningful impact on house prices.

This exogenous increase in housing prices has important implications beyond the housing market itself. Housing prices are deeply interconnected with broader economic activity, as they influence household wealth, consumption patterns, and local labor market conditions. Importantly, housing price changes can also directly affect firm behavior. For instance, rising housing prices may increase the collateral value of real estate assets held by firms, easing credit constraints and enabling firms to finance their investment activities.

In light of these dynamics, the next section leverages the exogenous variation in housing prices induced by the HPR policy to examine its impact on firm lending and investment behavior.

5.2 Effect on Firm Borrowing and Investment

To identify the causal effect of housing purchase restriction (HPR) spillovers on firm financing and investment, we employ a difference-in-differences (DiD) research design. The HPR allows us to compare changes in outcomes for firms in nearly connected ”treated” cities relative to firms in ”control” cities that are not adjacent to HPR restricted cities.

Our baseline econometric model is specified as follows:

For New Lending:

$$\begin{aligned}\text{New\_Stloan}_{ict} = \;& \beta_0 + \beta_1(\text{Treatment}_i \times \text{Post}_t) + \beta_2\,\text{Treatment}_i + \beta_3\,\text{Post}_t \\ & + \gamma_k\,\mathbf{X}_{ict} + \delta_m\,\mathbf{Z}_{ct} + \alpha_c + \alpha_j + \alpha_i + \alpha_t + \epsilon_{ict}\end{aligned}$$

For Investment:

$$
\begin{aligned}
\text{Investment}_{ict} = \; & \beta_0 + \beta_1(\text{Treatment}_i \times \text{Post}_t) + \beta_2\,\text{Treatment}_i + \beta_3\,\text{Post}_t \\
& + \gamma_k\,\mathbf{X}_{ict} + \delta_m\,\mathbf{Z}_{ct} + \alpha_c + \alpha_j + \alpha_i + \alpha_t + \epsilon_{ict}
\end{aligned}
$$

where Investment_{ict} is the dependent variable for firm i, representing a firm's capital expenditures divided by its lagged total assets (Invest1). We also use second measurement that we define investment expenditures as the annual change in net fixed assets divided by its lagged total assets (Invest2) (Jiang et al., 2018). New_Stloan_{ict} represents new short-term loan normalized by lagged fixed assets.

Firm-level control variables ($\mathbf{X}_{ict}$) are included to account for heterogeneity in firm characteristics that may influence investment. These controls include the logarithm of total assets which captures firm size, while Tobin's Q measures growth opportunities. Sales are included in logarithmic form to reflect firm revenue. Net profit, normalized by lagged fixed assets captures profitability. Additional firm-level controls include the book-to-market ratio.

City-level control variables ($\mathbf{Z}_{ct}$) are included to account for local economic and infrastructure conditions. These include the population size, road area per capita in square meters, and the logarithm of gross regional product per capita.

The model also incorporates fixed effects to control for unobserved heterogeneity. City fixed effects (α_c) capture time-invariant characteristics of cities, industry fixed effects (α_j) account for variations across industries, firm fixed effects (α_i) control for time-invariant characteristics of firms, and year fixed effects (α_t) capture time-specific shocks. The error term (ϵ_{ict}) is included, and standard errors are clustered at the city level.

Table 5 presents the core findings of our study, employing a difference-in-differences (DiD) estimation to analyze the causal impact of the 2016 HPR spillover on firm financing and investment decisions. The analysis critically distinguishes between State-Owned Enterprises (SOEs) and Non-State-Owned Enterprises (Non-SOEs) to uncover heterogeneous effects driven by ownership structure.

The regression results, presented in Table 5, indicate a significant and positive causal effect of the real estate price increase on the borrowing behavior of State-Owned Enterprises (SOEs). In Column (1), which details the specification for the SOE subsample, the coefficient on the interaction term Treat x Post is 0.190 and is statistically significant at the 5% level. This result suggests that following the positive housing price shock, SOEs in treated cities increased their new short-term borrowing to a greater extent than their counterparts in control cities.

To gauge the economic magnitude of this effect, we compare the coefficient to the standard deviation of the dependent variable for the SOE subsample (0.58). The estimated increase of 0.190 corresponds to approximately 25% of a standard deviation (0.190 / 0.765), indicating an economically meaningful expansion in credit access for these firms.

In stark contrast, Column (2) reveals that this credit expansion effect does not extend to non-SOEs. The coefficient on the interaction term for the non-SOE subsample is statistically indistinguishable from zero. Although the point estimate is positive, the large standard error precludes any reliable inference.

This heterogeneity in financing is central to our narrative. The preferential access to credit for SOEs is consistent with the well-established theories of soft budget constraints and politically-motivated lending (Geng and Pan, 2024; Jin et al., 2023). These firms, backed by implicit government guarantees, face lower credit risk from the perspective of lenders and are less subject to market discipline, allowing them to absorb additional debt readily.

On the other hand, a crucial question is whether this increased borrowing translates into productive economic activity. Columns (3) to (6) investigate this by examining the effect on new investment. Here, we find no statistically significant impact for either SOEs or Non-SOEs. The coefficients are close to zero and are not statistically significant. This "borrowing without investment" phenomenon for SOEs is a critical result. It suggests a significant potential for capital misallocation. Instead of financing new capital formation, the additional debt acquired by SOEs may have been used to cover operational inefficiencies, subsidize bloated payrolls, or simply be diverted for other non-productive purposes. This outcome aligns with theoretical arguments that the lack of market discipline under soft budget constraints leads SOEs to engage in value-destroying behavior rather than efficient investment (Kornai, 1986).

In summary, our DiD estimates suggest that the HPR spillover exacerbated existing distortions in the credit market. It triggered a significant expansion of credit directed exclusively at SOEs, but this infusion of capital failed to stimulate real investment. This highlights a critical policy challenge: while large-scale infrastructure projects may be intended to spur growth, their ultimate economic impact is mediated by institutional frictions, such as the preferential treatment of state-owned firms, which can lead to the inefficient allocation of capital and undermine the policy's objectives.

5.3 Dynamic Effects

To test the validity of the parallel trends assumption, we employ an event study framework, which allows for a more granular examination of the dynamic treatment effects over time. The event study specification is as follows:

$$\begin{aligned}\text{New\_StLoan}_{ict} = \beta_0 + \sum_{k \neq 0} \beta_k \big(\text{Treatment}_i \times D\{t = k\}\big) \\ + \gamma_k \mathbf{X}_{ict} + \delta_m \mathbf{Z}_{ct} \\ + \alpha_c + \alpha_j + \alpha_i + \alpha_t + \epsilon_{ict}\end{aligned}$$

Here, $D\{t = k\}$ is an indicator variable that equals 1 if year t corresponds to event time k, where $k = 0$ represents the treatment year. The coefficients β_k capture the differential trends in investment for treated firms relative to the control group across different event time periods.

This approach allows us to observe the evolution of treatment effects before and after the intervention, thereby providing a test for the parallel trends assumption. If the pre-treatment coefficients ($k < 0$) are statistically indistinguishable from zero, it suggests that treated and control firms followed similar investment trends prior to the treatment. Conversely, post-treatment coefficients ($k > 0$) that are significantly different from zero indicate the dynamic effects of the treatment.

The results of the event study are presented in Figure 1. The figure plots the estimated coefficients β_k and their 90% confidence intervals. The pre-treatment coefficients are close to zero and statistically insignificant, supporting the validity of the parallel trends assumption. Post-treatment, we observe a significant upward shift in investment for treated firms, consistent with our main findings.

This analysis strengthens the causal interpretation of our results by confirming that the observed increase in investment is not driven by pre-existing differences in trends between treated and control firms.

6 Robustness test

To further substantiate our primary findings and bolster their causal interpretation, we conduct a crucial placebo test. The purpose of this test is to mitigate concerns that our main results are driven by pre-existing differential trends between the treatment and control groups rather than the policy intervention itself. In this falsification exercise, we artificially shift the treatment period forward to a pre-treatment year, 2015. The analysis is restricted to the pre-treatment sample period, thereby excluding all observations from the actual post-treatment period (2016 onward).

Table 6 presents the results of this placebo test. The regression specification mirrors our main difference-in-differences model from Table 5, with new short-term loans as the dependent variable. The models includes a comprehensive set of city, stock, year, and industry fixed effects, in addition to firm- and city-level controls, to account for a wide range of unobserved heterogeneity. Standard errors are clustered at the city level to address potential intra-group correlation.

The key variable of interest is Treat X Post (Placebo), an interaction term where Post (Placebo) is an indicator variable equal to one for the year 2015 and zero otherwise. A statistically significant coefficient on this variable would cast doubt on our identification strategy, suggesting that our main findings could be spurious.

Within all models with varying specification, the coefficients are statistically indistinguishable from zero at all conventional significance levels. The lack of a significant effect in these placebo regression provides strong evidence against the presence of confounding pre-existing trends. This null result demonstrates that had the treatment occurred in a randomly selected year prior to the actual intervention, we would not have observed a similar effect on firms' access to short-term loans. Therefore, the findings from this placebo test significantly strengthen our confidence that the baseline results capture the true causal effect of the policy.

To further substantiate a causal interpretation of our findings and mitigate concerns that our primary DiD estimate is driven by unobserved confounding factors or is merely a statistical artifact, we conduct a placebo test. Specifically, we perform an "in-space" placebo analysis by creating a large number of simulated treatment groups.

The procedure involves iteratively re-assigning the treatment status to a randomly selected group of firms from the original control group—that is, firms that we know were not exposed to the treatment. We hold the size of this placebo treatment group equal to the size of the actual treatment group. We then re-estimate our baseline DiD specification using this falsified treatment assignment. This entire process is repeated 500 times, generating a distribution of 500 placebo DiD coefficients.

The underlying logic of this test is that if our research design is valid, these placebo regressions should, on average, yield a coefficient of zero. A significant coefficient from our actual regression that lies in the tails of this placebo distribution would strongly suggest a true treatment effect.

Figure 2 plots the results of this falsification exercise. The histogram displays the distribution of the 500 placebo coefficients, which is centered tightly around zero, as expected. The red dashed line indicates our actual estimated DiD coefficient from Table 5. The actual coefficient, at a value of approximately 0.2, lies far in the right tail of the empirical distribution of placebo effects. Crucially, none of the 500 placebo estimations produced a coefficient as large as our main finding.

This result demonstrates that the probability of obtaining our estimated treatment effect by random chance is exceedingly low. Therefore, this placebo test provides strong support for the robustness of our results and enhances our confidence that we have identified a genuine causal effect.

7 Discussions

Our empirical investigation provides a nuanced and revealing perspective on the real estate collateral channel within the unique institutional landscape of China. By exploiting the quasi-natural experiment induced by the spillover effects of the 2016 Home Purchase Restriction (HPR) policies, we uncover a stark heterogeneity in corporate financing and investment behavior, fundamentally tied to firm ownership. The core finding of this paper—that an exogenous positive shock to real estate values triggers a significant increase in short-term borrowing for State-Owned Enterprises (SOEs) but not for non-SOEs, and that this borrowing fails to translate into productive investment for either group—carries significant theoretical and policy implications.

7.1 Theoretical Contributions

This study makes several important contributions to the corporate finance literature. First and foremost, we help to reconcile a significant puzzle regarding the existence and potency of the real estate collateral channel in China. Prior influential work, such as Wu et al. (2015b), found no evidence of a collateral channel effect on firm investment when analyzing a broad panel of Chinese firms. Our findings suggest that this result may be changed. In our study, the preferential treatment afforded to SOEs effectively mutes the collateral channel for them. This aligns with a body of literature suggesting that SOEs operate under soft budget constraints and benefit from implicit government guarantees, which grant them privileged access to credit (Tang, 2023; Wu et al., 2025).

Second, our results for SOEs—”borrowing without investment”—shed new light on the consequences of soft budget constraints(Kornai, 1986). While the literature has extensively documented that SOEs receive preferential credit access (Jin et al., 2023; Geng and Pan, 2024), our findings go a step further by showing how they utilize this advantage in response to an exogenous shock. The infusion of debt without a corresponding increase in capital formation points towards significant capital misallocation (Jurzyk, 2021). This capital may be diverted to cover operational inefficiencies, achieving governmental goals, or even fuel speculative activities, rather than funding productive projects (O’Toole et al., 2016; Chang et al., 2024; Jin et al., 2025). This behavior deviates from the predictions of standard investment theories, which posit that firms borrow to fund value-enhancing projects. Our findings suggest that for firms not subject to hard market discipline, financing and investment decisions can become decoupled, a critical insight for understanding corporate behavior in economies with a large state sector.

Third, this paper contributes to the broader understanding of how macroeconomic policies are transmitted through the corporate sector in dual-track economies. Our results demonstrate that the impact of a policy shock—even one as seemingly targeted as housing market restrictions—is not uniform. The institutional fault lines between the state and private sectors fundamentally alter the transmission mechanism. This highlights the importance of moving beyond representative agent models and incorporating institutional heterogeneity to accurately predict the corporate response to economic shocks.

7.2 Policy Implications

The findings of this study offer several critical insights for policymakers in China and other emerging economies with significant state ownership.

First, our results cast doubt on the efficacy of using broad-based monetary or real estate policies to stimulate productive investment across the entire economy. The evidence suggests that policies leading to an appreciation in asset values may disproportionately channel credit towards less productive SOEs, exacerbating existing distortions in capital allocation. While SOEs can easily absorb more debt, this does not automatically translate into the kind of productive investment that drives sustainable long-term growth. This implies that stimulating the economy via the collateral channel may be inefficient and could even be counterproductive if it reinforces the dominance of less efficient state firms at the expense of more dynamic private enterprises.

Second, the "borrowing without investment" phenomenon observed among SOEs underscores a critical challenge of capital misallocation. This inefficient use of capital not only represents a drag on economic growth but also elevates financial stability risks. The accumulation of non-productive debt on SOE balance sheets increases the vulnerability of the banking sector and the broader financial system. Policymakers should, therefore, be vigilant about the end-use of credit extended to SOEs, particularly during periods of asset price booms. This calls for strengthened oversight and a renewed focus on SOE reform aimed at hardening budget constraints and improving corporate governance to instill greater market discipline.

Third, the study highlights the unintended consequences of policies like the Home Purchase Restrictions (Chen et al., 2024). While designed to cool housing markets, the spillover effects created an exogenous wealth shock that was disproportionately captured by the state sector for non-productive ends. This suggests that policymakers must consider the full general equilibrium effects of their interventions, including how they interact with pre-existing institutional frictions.

7.3 Limitations and Avenues for Future Research

While this study provides robust evidence on the heterogeneous effects of the collateral channel, it is not without limitations, which in turn open avenues for future research. First, while we document that the increased borrowing by SOEs does not flow into fixed asset investment, our study does not precisely trace the ultimate use of these funds. Future research could leverage more data to investigate whether this debt is used to acquire financial assets, engage in real estate speculation, or cover operating losses. Understanding these alternative uses of capital is crucial for a complete picture of capital misallocation.

Second, our analysis focuses on the effects of a single, albeit significant, policy event. It would be valuable to examine whether similar heterogeneous responses occur in response to other types of macroeconomic shocks, such as changes in monetary policy or exchange rate fluctuations. This would help to establish the generalizability of our findings regarding the role of ownership in mediating corporate responses to the economic environment.

Finally, our study centers on the distinction between SOEs and non-SOEs. However, both of these categories are themselves heterogeneous. Future work could explore the variation within these groups. For instance, are the effects more pronounced for centrally-controlled SOEs versus local ones? Among non-SOEs, do factors like political connections or access to shadow banking channels moderate their response to collateral shocks? Answering these questions would provide an even more refined understanding of the complex interplay between real estate markets, institutional structures, and corporate decision-making in China.

8 Conclusion

This paper revisits the debate on the real estate collateral channel in China and finds that its operation is critically contingent on firm ownership. Using the spillover effects of Home Purchase Restriction policies as a quasi-natural experiment, we show that positive shocks to real estate values led to a significant increase in short-term borrowing for State-Owned

Enterprises, but had no such effect on non-SOEs. Crucially, this credit expansion for SOEs did not translate into higher real investment, suggesting a significant misallocation of capital.

Our findings reconcile conflicting results in the prior literature by demonstrating that the aggregation of firms with fundamentally different budget constraints can obscure the true effects of the collateral channel (Wu et al., 2015a). The results highlight how implicit government guarantees and soft budget constraints can sever the link between financing and productive investment, leading to policy-induced distortions. From a policy perspective, our study cautions against the use of broad asset-based stimulus measures, which may inadvertently fuel inefficient credit expansion in the state sector while failing to support more productive private firms. The evidence underscores the persistent need for reforms that harden budget constraints for SOEs and level the playing field for all firms to ensure that capital flows to its most productive uses, fostering sustainable and high-quality economic growth.

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Tables

Table 1: Variable Definitions

This table provides definitions for the variables used in the study. Panel A describes the dependent variable. Panel B defines the key explanatory variable. Panel C lists the firm-level control variables, and Panel D lists the region-level control variables.

Variable	**Definition**
Panel A: Dependent Variable	
New_Stloan	New short-term loan normalized by lagged fixed assets.
Invest1	Sum of the annual change in net fixed assets, depreciation and amortization, scaled by lagged total assets.
Invest2	Annual change in net fixed assets, scaled by lagged total assets.
Panel B: Key Explanatory Variable	
Treatment	An indicator variable equal to one if the city is located within 250 kilometers of an HPR-implementing city, and zero for control cities that neither implemented HPR nor are located within 250 kilometers of HPR cities.
Panel C: Firm-Level Control Variables	
log(Assets)	The natural logarithm of a firm's total assets.
log(Revenue)	The natural logarithm of a firm's total revenue.
Tobin's Q	Market value of all shares (traded + non-traded) plus book value of liabilities, divided by book value of total assets. Non-traded shares are valued at net assets per share.
BM	The book-to-market ratio, calculated as the book value of equity divided by the market value of equity.
ROA	Calculated as net income divided by total assets.
Lev	Financial leverage, calculated as total liabilities divided by total assets.
TOP1	The ownership concentration, measured as the percentage of total shares held by the largest shareholder.
Panel D: Region-Level Control Variables	
log(GRP_per_capita)	The natural logarithm of the Gross Regional Product per capita.
Population_size	The natural logarithm of the total population in the region where a firm is located.
Road_area_per_capita	The square meters of road area per capita in the city.
log(Residents)	The logged number of people who regularly reside in a specific city for six months or more within a year

Table 2: Sample Composition by Ownership Type, 2013-2019

This table reports the annual distribution of firms by ownership type over the sample period. The sample includes Chinese listed non- financial firms, excluding finance, insurance, real estate, construction, agriculture, mining, transportation, and utilities sectors. Firms in cities with home purchase restrictions are excluded. SOE classification is obtained from WIND database.

Year	Number of SOEs	Number of non-SOEs
2013	338	385
2014	336	408
2015	331	437
2016	320	445
2017	320	469
2018	323	467
2019	321	437

Table 3: Summary Statistics by SOE Status

This table presents summary statistics partitioned by state-owned enterprise (SOE) status for the sample period 2013-2019. The sample includes Chinese listed non-financial firms, excluding finance, insurance, real estate, construction, agriculture, mining, transportation, and utilities sectors. Firms in cities with home purchase restrictions are excluded. New Stloan is new short-term loans scaled by lagged assets. Investment measures (Invest1, Invest2) are scaled by lagged assets. TobinQ is market-to-book ratio of assets. Other variables include standard firm characteristics (assets, revenue, leverage, ROA) and city-level controls (GRP per capita, population, road area per capita, residents). All variables are winsorized at the 1% and 99% levels.

	Non-SOE				SOE			
	N	Mean	SD	Median	N	Mean	SD	Median
New_Stloan	2278	0.19	0.92	0.04	1835	0.08	0.77	0.01
log(Assets)	2903	22.05	1.08	22.05	2264	22.64	1.24	22.56
log(Revenue)	2905	21.34	1.33	21.34	2264	22.04	1.39	21.94
Invest1	2849	0.06	0.06	0.04	2241	0.05	0.05	0.03
Invest2	2849	0.08	0.11	0.04	2241	0.05	0.06	0.03
Treatment	2532	0.71	0.45	1.00	2124	0.54	0.50	1.00
TobinQ	2764	2.20	1.46	1.74	2214	1.89	1.25	1.44
BM	2764	0.58	0.24	0.58	2214	0.67	0.27	0.69
ROA	2871	0.03	0.09	0.03	2261	0.02	0.06	0.02
Lev	2905	0.42	0.20	0.41	2264	0.50	0.21	0.50
TOP1	2905	30.30	12.98	29.08	2264	35.36	13.98	33.54
log(GRP_per_capita)	2241	11.15	0.51	11.17	1986	11.03	0.46	11.06
Population_size	2241	6.11	0.71	6.17	1986	6.18	0.70	6.27
Road_area_per_capita	2208	18.61	7.07	16.85	1953	17.88	6.38	16.55
log(Residents)	2215	15.46	0.69	15.50	1976	15.46	0.68	15.43

Table 4: The Spillover Effect of Home Purchase Restrictions on House Prices

This table presents difference-in-differences estimates of the spillover effects of home purchase restrictions (HPR) on house prices in neighboring cities. The dependent variable is the natural logarithm of average house price at the city level. The analysis exploits the second wave of HPR implementation as a quasi-experimental setting. Treat is an indicator variable equal to one if the city is located within 250 kilometers of an HPR-implementing city, and zero for control cities that neither implemented HPR nor are located within 250 kilometers of HPR cities. Post is an indicator variable equal to one for years 2016 and after (post-HPR implementation period), and zero otherwise.

	(1)	(2)	(3)
Treat X Post	0.091***	0.091***	0.090***
	(0.024)	(0.024)	(0.024)
Treat	0.101**		
	(0.038)		
Post	0.215***		
	(0.019)		
Constant	8.422***		
	(0.028)		
Observations	1617	1617	1580
R^2	0.174	0.858	0.861
Adjusted R^2	0.172	0.833	0.837
Std.Errors	City	City	City
City FE	No	Yes	Yes
Year FE	No	Yes	Yes
City Controls	No	No	Yes

* p <0.1, ** p <0.05, *** p <0.01

Table 5: The Impact of the HPR spillover on Firm Borrowing and Investment

This table presents the results from a Difference-in-Differences (DiD) regression analyzing the effect of the 2016 HPR spillover effect on firm financing and investment. The dependent variable in columns (1)-(2) is the new short-term loans scaled by lagged assets. The dependent variable in columns (3)-(6) is new investment. Treat X Post is the interaction term where Treat is an indicator for firms in cities spillovered by the HPR and Post is an indicator for the years 2016 and after. Columns (1), (3) and (5) show results for the subsample of State-Owned Enterprises (SOEs), while columns (2), (4) and (6) are for Non-SOEs.

	New_Stloan		Invest1		Invest2	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat X Post	0.190**	6.094	-0.006	-0.001	-0.006	0.017
	(0.074)	(6.514)	(0.005)	(0.007)	(0.005)	(0.011)
Subsample	SOE	Non-SOE	SOE	Non-SOE	SOE	Non-SOE
Observations	1557	1664	2053	1992	2053	1992
R^2	0.472	0.212	0.580	0.563	0.580	0.463
Adjusted R^2	0.240	-0.152	0.420	0.392	0.420	0.253
Std.Errors	City	City	City	City	City	City
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
City Controls	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Placebo Test with an Artificial Treatment Year (2015)

This table reports the results of a placebo test that artificially shifts the treatment year to 2015. The analysis is conducted on the pre-treatment SOE sample period, excluding all observations from 2016 onward. The dependent variable is new short term loans scaled by lagged assets.

	(1)	(2)	(3)	(4)
Treat X Post (Placebo)	−0.014	−0.002	0.015	0.016
	(0.030)	(0.033)	(0.084)	(0.114)
Constant	0.065***	−0.049		
	(0.015)	(0.496)		
Observations	816	705	705	705
R^2	0.000	0.014	0.279	0.280
Adjusted R^2	−0.001	0.002	−0.192	−0.892
Std.Errors	City	City	City	City
Year FE	No	No	Yes	Yes
Firm FE	No	No	Yes	Yes
City FE	No	No	No	Yes
Industry FE	No	No	No	Yes
Firm Controls	No	Yes	Yes	Yes
City Controls	No	Yes	Yes	Yes

* p <0.1, ** p <0.05, *** p <0.01

Figures

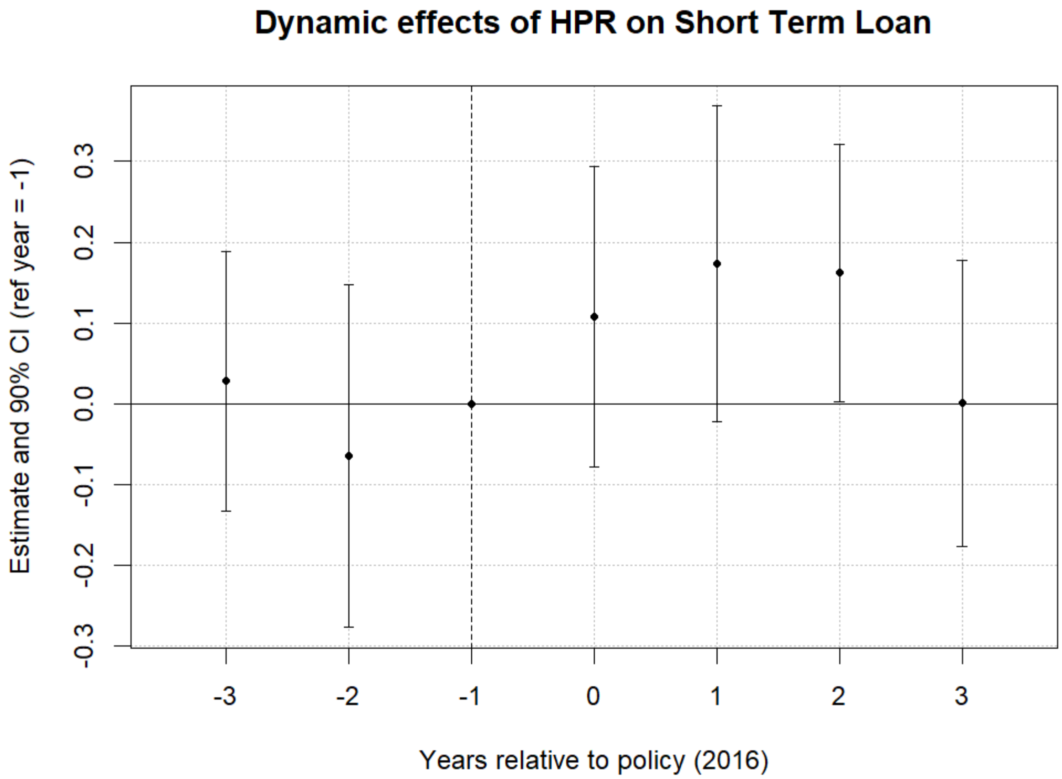

Figure 1: Dynamic Effects of Home Purchase Restrictions (HPR) on Short-Term Loans

Notes: This figure plots the dynamic effect of the 2016 Home Purchase Restriction (HPR) policy on corporate short-term loans. The graph displays the point estimates (dots) and 90% confidence intervals (vertical bars) for the coefficients from a difference-in-differences estimation. The dependent variable is the new short-term loans to lagged fixed assets. The model includes firm and year fixed effects, and the year prior to the policy (t-1) serves as the omitted reference category. Standard errors are clustered at the city level.

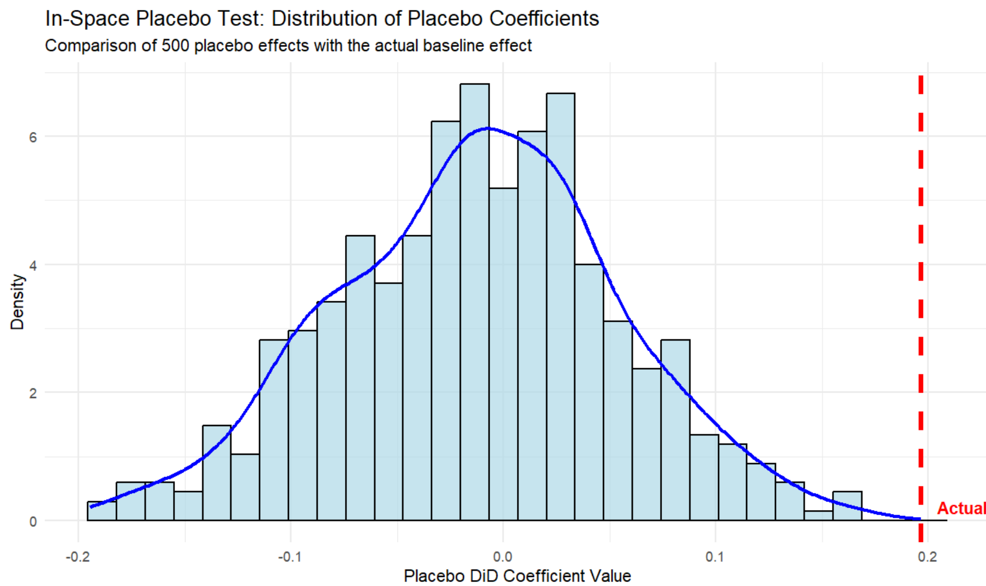

Figure 2: In-Space Placebo Test with Randomized Treatment Assignment

Notes: This figure displays the results of an in-space placebo test to assess the likelihood that our main finding occurred by chance. We construct a distribution of placebo difference-in-differences (DiD) coefficients by running our baseline regression 500 times. In each iteration, we randomly assign the treatment status to the same number of firms as in the actual treatment group and re-estimate the treatment effect. The histogram and the corresponding kernel density plot (blue line) show the distribution of these 500 placebo coefficients. The vertical red dashed line indicates the coefficient from our actual baseline regression.