Financing Constraints, Implicit Government Guarantee, and Real Estate Market

Sumit Agarwal, Arkodipta Sarkar, Yue Zhang and Zhe Zong*

May, 2026

Abstract

Real estate developers rely heavily on external financing, making them vulnerable to financial distress and prompting regulatory constraints, but how do such policies reshape market structure across ownership types? We examine this using China's Three-Red-Line policy based on a proprietary dataset of listed and unlisted developers. Difference-in-differences results show that the policy reduced land purchases and home sales among constrained firms, but only for private developers. Government-linked firms were largely unaffected due to implicit guarantees. As a result, market share shifted from private to government-backed developers, highlighting how deleveraging policies can backfire, concentrating market power and creating new systemic risks.

Keywords: financing constraint, real estate developer, land purchase, Three-red-line policy, implicit government guarantee, state-owned enterprise

JEL Classification: G32, G38, R3

*Sumit Agarwal is from National University of Singapore, email:bizagarw@nus.edu.sg; Arkodipta Sarkar is from National University of Singapore, email: asarkar@nus.edu.sg; Yue Zhang is from Sun Yat-sen University, email: zhangyue6@mail.sysu.edu.cn; Zhe Zong is from China Zheshang Bank, email: zongzhe.work@outlook.com. We appreciate the helpful comments from Jinfan Chang, Yuting Huang (discussant) and conference and seminar participants at 2026 CFRN conference and Peking University. Yue Zhang would like to acknowledge the research support from National Natural Science Foundation of China (No. 72402239). All errors are our own.

1 Introduction

Real estate development is an inherently capital-intensive industry where firms rely heavily on external financing for land acquisition and project development. The extended development cycles and substantial upfront capital requirements create acute sensitivity to credit market conditions, as restricted access to financing can force developers to forego profitable land investments or delay project completion (Brown, 2000; Favara and Imbs, 2015; Fieldhouse et al., 2018; Justiniano et al., 2019; Blickle, 2022). This vulnerability is amplified by the industry's heavy dependence on leverage, where developers typically finance 60-80% of their investments through debt, creating a direct transmission mechanism from credit markets to real investment decisions. Consequently, financing constraints are particularly relevant for understanding real estate investment patterns and market dynamics. However, while extensive literature demonstrates that financing constraints generally reduce investment intensity (e.g., Fazzari et al., 1988; Whited, 1992; Caggese, 2007; Czarnitzki and Hottenrott, 2011; Li, 2011; Campbell et al., 2012; Cohn and Wardlaw, 2016; Caggese et al., 2019), a critical gap remains: how do these effects vary when firms have differential access to credit markets? When some firms enjoy implicit government support while others face pure market discipline, do financing constraint policies achieve their intended deleveraging goals, or do they inadvertently concentrate market power in the hands of privileged firms?

We address this question using China's Three-Red-Line (TRL) policy as a natural experiment. Introduced in August 2020, the policy aimed to deleverage the real estate sector while mitigating systemic financial risks in the banking sector by constraining the debt financing of highly leveraged developers. Commercial banks, as the main providers of developer debt, play a central role in the sector's leverage accumulation, and the policy effectively targeted this channel. To achieve its objectives, the TRL policy imposed step-wise limits on allowable annual debt growth for real estate developers based on existing leverage ratios, generating exogenous variation in financing constraints while preserving differences in firms' access to credit markets through implicit government guarantees.

This combination of policy design and firm-level heterogeneity creates a compelling natural experiment, particularly suited to the Chinese real estate market, which is not only massive in scale but also systemically important to the national economy. The sector's size, leverage, and rapid expansion over past decades make it especially sensitive to financing constraints, meaning that policy-driven shocks can produce measurable structural effects. Indeed, the TRL policy's significance became immediately apparent when Evergrande, with outstanding debts of CNY 2 trillion (nearly 2% of China's GDP), collapsed shortly after implementation, raising concerns about widespread financial distress and contributing to a roughly 20% decline in residential land acquisitions during 2020–2022. Leveraging this shock, we investigate whether and how financing constraints produce structural effects in the real estate market, focusing on developers' land acquisition and home sales in the retail market, and the associated risk implications for commercial banks.

To answer the questions, we compile a large proprietary dataset of developers in China during the period between 2016 and 2022 after obtaining their quarterly financial performance information. The dataset covers 139 publicly listed developers and 722 private developers. After merging with land-parcel-level transaction data, our sample of developers cover 46% of the total transactions in the residential land market.[1]

Using difference-in-differences estimation, we document a notable decline in the probability of land purchases, and the amount of land purchases of developers that violated at least one red line during the pre-TRL period (i.e., treated group, subject to the cap on the annual debt growth of up to 10%) following the TRL policy than those with no violation of any red lines (i.e., control group, subject to the cap on the annual debt growth of 15%). In economic sense, the reduction of land purchases in a given quarter of the average developer from the treated group is approximately 22,964 square meters (or CNY 236 million).

Moreover, evidence suggests that the negative effect of the TRL policy on land purchases

[1]The limited existing studies on the effects of the TRL policy primarily focus on publicly listed firms (Chen et al., 2024; Jin et al., 2026). In contrast, our study incorporates both listed and private developers, thereby providing a more comprehensive view of market-wide responses to the policy.

intensifies with the severity of financing constraints—specifically, as the number of red lines violated in the pre-policy period increases—and is more pronounced among developers who had longer debt maturity structures prior to the policy.

Next, we find striking differences in the policy effects on land purchases among developers of different ownership types. First, the impediment of the TRL policy on land purchasers is more prominent for non-state-owned (non-SOE) developers. This is consistent with our working hypothesis that SOE developers carry implicit government guarantees and therefore benefit from better capability of improving financial performance to satisfy the policy requirements for higher cap on allowable debt growth. Second, we also find remarkable differences in the policy effects within the group of SOE developers. We document that local government financing vehicles (LGFVs) increase their land purchases following the policy more than non-LGFV SOE developers. The results echo the finding in Chang et al. (2025) that, LGFVs, as the off-balance-sheet borrowers of local governments, have the motivation to acquire lands for the purpose of upholding land prices to maintain governments' land sales revenues.

To deepen the understanding of the relationship between the TRL policy and developers' land purchases, we explore the channels underlying their association. First, we document that non-SOE developers, particularly those in the treated group, experience a significant decline in access to both bank loans and bond financing following the TRL policy, while SOE developers are not similarly affected. Second, we show that the TRL policy increases the cost of debt for developers, as measured by bond yields at issuance, with this effect being less pronounced for SOE developers. Together, these findings suggest that the TRL policy operates through tightening external financing conditions, with its impact concentrated on firms facing weaker implicit credit guarantees.

After analyzing the effect of the TRL policy on the land acquisition decisions of individual developers, we assess whether the aggregate effect is significant enough to restructure the land market. The statistics suggest an affirmative answer. We show that, the total transaction

value of residential lands slumps by over 35% from 2020 to 2022, and importantly, the market share of non-SOE developers shrunk by half (from 64% in 2020 to below 30% in 2022) during the post-TRL period. This contraction is particularly pronounced in cities whose fiscal revenues depend more heavily on land purchases by non-SOE developers, where LGFVs increasingly step in to acquire land, helping sustain local governments' land sale revenues.

Importantly, our estimates suggest that this reallocation comes at an efficiency cost: as SOEs, and especially LGFVs, acquire a larger share of land, their average revenue generated per unit of land declines, implying a measurable deterioration in land-use efficiency and indicating that the policy-induced shift entails tangible economic costs beyond changes in market structure.

Moreover, we find that the TRL not only affects developers' land purchases, but also their home sales on the retail market. Specifically, non-SOE developers experience a decline in home sales following the policy, potentially reflecting buyers' concerns about the negative impact of financing constraints on developers' ability to deliver projects on time or maintain construction quality. These findings imply that policy-driven financing restrictions can transmit beyond the upstream land market into downstream housing demand, highlighting the broader market consequences of credit tightening for developers without preferential access to debt.

Furthermore, we examine how debt financing constraints imposed on developers influence the risk profiles of commercial banks. While our evidence confirms that banks with greater loan exposure to the real estate sector (i.e., treated banks) substantially reduced their lending to the sector following the TRL policy, we find limited evidence of a systematic effect on overall bank risk. If anything, the results suggest that banks more exposed to the real estate sector may have experienced a slight increase in short-term financial vulnerability. This pattern points to a potential trade-off between the policy's credit-tightening objectives and short-term financial stability, highlighting the nuanced and transitional nature of banks' risk adjustments in response to sudden sector-specific constraints.

One concern is that the observed reductions in land purchases and home sales may reflect the impact of COVID-19 rather than the TRL policy, as the pandemic overlapped with the policy's rollout and could have tightened financing or dampened demand. To separate these effects, we leverage the TRL's sharp debt-burden thresholds in a Regression Discontinuity Design, comparing developers just above and below the regulatory cutoffs. The results confirm that stricter TRL constraints, rather than pandemic disruptions, drove lower land acquisitions and home sales for non-SOE developers, and these findings remain robust across multiple specifications. Overall, the evidence reinforces our main DiD estimates and supports a causal interpretation of the TRL policy's impact.

Collectively, the paper makes four important contributions. First, this study contributes to the literature examining the distributional and structural effects of government interventions in the real estate market. While prior research primarily examines policies such as government guarantees, tax credits, and credit shocks imposed on homebuyers and their impact on wealth concentration among households (e.g., Jeske et al., 2013; Floetotto et al., 2016; Favilukis et al., 2017; Gete and Zecchetto, 2018; Beraja et al., 2019; Di Maggio et al., 2020; Bosshardt et al., 2024; Greenwald and Guren, 2025), our analysis considers a setting in which credit constraints and implicit government guarantees apply to real estate developers. We show that tightening credit conditions leads to a reallocation of market shares toward state-owned developers benefiting from implicit government backing. By shifting the focus from household income heterogeneity to developer ownership heterogeneity, this paper extends the literature from household-level wealth inequality to firm-level market concentration, providing new insights into the unintended structural consequences of government interventions. In particular, it complements Agarwal et al. (2024), who document the democratization of housing ownership following regulatory reforms on developer transparency, by providing evidence of market concentration following leverage regulation targeting real estate developers. More broadly, the study contributes to research on how financial policies shape firm dynamics and market allocation (e.g., Acharya et al., 2025).

Second, the paper contributes to the literature on the economic implications of financial constraints. Prior studies have extensively examined how financing constraints influence firm behavior and outcomes (Fazzari et al., 1988; Whited, 1992; Moyen, 2004; Caggese, 2007; Li, 2011; Campbell et al., 2012; Lin and Paravisini, 2013; Cohn and Wardlaw, 2016; Carvalho, 2018; Caggese et al., 2019), as well as their specific effects on mortgages and housing markets (Blickle and Brown, 2019; Justiniano et al., 2019; Van Straelen, 2024). Our study extends this line of research by examining real estate developers with heterogeneous access to credit, providing novel evidence that financial constraint policies—while intended to enhance stability—may inadvertently amplify market concentration and create new systemic risks by favoring financially privileged firms.

Third, the paper adds to the literature on SOEs' preferential access to debt and its economic consequences in China. While prior studies document persistent credit misallocation between politically connected firms and private firms, and the resulting inefficiencies in resource allocation (Song et al., 2011; Chen et al., 2015; Cong et al., 2019; Bai et al., 2020; Huang et al., 2020; Li et al., 2022; Chen et al., 2023; Geng and Pan, 2024), as well as the economic implications of implicit government guarantees (Chen et al., 2020; Allen et al., 2023; Ang et al., 2023; Huang et al., 2023; Jin et al., 2023), our study advances this literature by showing that implicit guarantees can also distort market competition in the presence of tightening financial regulation. Specifically, the paper links implicit guarantees not only to credit allocation inefficiency but also to increasing market concentration and structural imbalances within the real estate industry.

Finally, the paper deepens the understanding of the evolving landscape of China's real estate market, offering timely perspectives on policy-induced market dynamics. On one hand, the sustained 20-year boom in the Chinese real estate market has captured global attention, prompting efforts to comprehend its origins and ramifications (Cai et al., 2013; Deng et al., 2012; Wu et al., 2012; Fang et al., 2016; Chen and Wen, 2017; Chen et al., 2017; Chen et al., 2019; Rogoff and Yang, 2021; He et al., 2023). On the other hand, since the

onset of the COVID -19 pandemic, changes have been observed in the real estate markets of both the US and China. For instance, Gupta et al. (2022) find that the COVID-19 pandemic flattens the bid-rent curve in most U.S. metropolitan areas, and D'Lima et al. (2022) document a decline in sales for housing markets under a shutdown during the COVID. In contrast, scant evidence has been provided to the real estate market in China during the post-COVID period when it started to slow down, with the exception of Chang et al. (2025). Chang et al. (2025) detect the anomaly of a divergence between the transaction volume and price in Chinese land market and attributes it to the active management of land and housing prices by local governments (through LGFVs). However, the plummet of land transactions remains unexplained in their research. Therefore, our paper complements to Chang et al. (2025) and deepens the understanding of China's land market by elaborating the shrink in land parcel demands since 2021.

2 Institutional background

This section presents the institutional background of China's land market, beginning with the land finance system—which is essential for understanding the broader context in which the land market operates—followed by an introduction to the TRL policy and the potential changes it may bring.

2.1 Land finance system

In China, local governments rely heavily on land sales to generate revenues (i.e., land finance system, also known as *tu di cai zheng*) particularly since the "Tax Sharing Reform" in 1994. This reform reduced the portion of tax revenue retained by local governments from 80% to about 50%, with the rest being allocated to the central government. However, the local governments experienced only a slight reduction in fiscal responsibilities and their incentives to boost local economic growth (e.g., through expanded investments) for career promotion

purposes remain intact, leading to budget deficits and prompting them to seek alternative sources of funding. Against this backdrop, urban land parcels have become increasingly valuable assets for local governments, not only for generating land-related profits but also for their crucial role in supporting debt financing activities.

According to the constitution, while all land in China is owned by the state, the "land usage rights" (LURs) are allowed for transactions since 1988. [2] LURs transactions are the basis of China's land finance system, which has been essential in driving the remarkable urban development and economic growth over the past decades. Specifically, the land finance system generates profits for local governments through the differences between the LURs acquisition costs and the revenues from selling LURs plus the developer-based and transaction-based tax revenues associated with real estate developments.[3] While it is challenging to gauge the precise statistics of local governments' land-sale profits due to the data inavailability of their LUR acquisition costs, Gyourko et al. (2022) provides rough estimates that, in 2018, local governments' land-related profits totaled CNY 4 trillion, accounting for over 41% of their total budgetary income.

Local governments were prohibited from directly engaging in debt financing before 2014. To circumvent the constraint, local governments began establishing local government financing vehicles (LGFVs) in the early 2000s to alleviate fiscal pressure following the 1994 tax reform. LGFVs are commonly perceived in the existing literature as "local government-owned and government-controlled legal entities that carry out government investment projects", which are allowed to conduct debt financing activities (Ambrose et al., 2015; Bai et al., 2016; Zhang and Xiong, 2020; Fan et al., 2022; Gyourko et al., 2022; Chang et al., 2025).[4]

[2]The lease period for the LURs varies with the type of land parcels. For industrial lands designated for industrial and manufacturing projects, the LURs can be leased for up to 30 years, and the lease period for the commercial lands designated for business establishments and for the residential lands designated for housing is up to 40 years and 70 years, respectively.

[3]According to Gyourko et al. (2022), there are two ways for local governments to acquire LURs: one is to convert rural-collective-owned land into new developable urban land after compensating rural collectives and farmers for the rural land ownership, and the other is to repurchase LURs of a land parcel by compensating the current LUR holder. In both cases, local governments may also bear with the costs associated with developing infrastructure on the new developable land parcel.

[4]It is common that a local government has multiple LGFVs. A LGFV is typically established by the government at province, city or county level. In our sample, the 636 LGFVs are registered in 141 cities. The number of LGFVs in a given city is, on average 9.53, ranging from 1 to 21.

It is noteworthy that LGFVs differ from other state-owned developers in their operational objectives. LGFVs primarily undertake the investment and financing functions of local governments, participating in infrastructure construction and public service projects. They typically focus on public welfare, with some engaging in quasi-public and commercial activities.[5] In contrast, the primary goal of non-LGFV SOE developers is to pursue profits through land development, property construction and sales. With such differences in operating objectives, LGFVs are subject to more oversights and control by corresponding local governments, while other SOE developers have more autonomy in their operations and decision-making processes. Moreover, since LGFVs tend to fund infrastructure projects that typically cannot generate sufficient returns to repay debts, they are more reliant on refinancing or government support to remain viable, and therefore, "local rating agencies, lenders, and investors tend to assess LGFVs based on the fiscal strength of the LGFV's parent LG[local government]". [6]

The usage of LGFVs for off-balance-sheet debt financing (including bank loans and bonds) have intensified since the global financial crisis and the subsequent economic stimulus plan. According to Fan et al. (2022), the debt accumulated by LGFVs accounted for over 70% of local governments' total debt, rising from CNY 1.7 trillion in 2007 to CNY 6.6 trillion by 2010, and then doubling again by 2014. In 2015, the Budget Law was amended to allow local governments to directly raise funds by issuing government bonds.

For local governments' debt financing, whether through the indirect use of LGFVs or the direct issuance of government bonds, LURs play a crucial role in providing support. First, a local government can transfer LURs to its LGFVs and the LURs could serve as collateral to support LGFVs' debt financing. Second, the local government can also provide implicit guarantees employing its future land-sale profits to support its LGFVs' debt financing or its direct bond issuance (Liu and Xiong, 2020; Gyourko et al., 2022). Therefore, the profitability

[5]See https://www.pimco.com/eu/en/insights/local-government-financing-vehicles-a-growing-risk-for-chinas-economy and https://www2.deloitte.com/cn/zh/pages/finance/articles/municipal-investment-development.html (in Chinese).
[6]See https://www.pimco.com/eu/en/insights/local-government-financing-vehicles-a-growing-risk-for-chinas-economy.

of LUR sales is likely to have a substantial impact on local governments' and their LGFVs' ability to borrow and the overall risk level of their debts. Indeed, Ang et al. (2023) provide evidence that rising real estate values lead to lower yields on bonds issued by LGFVs, highlighting the crucial role of a robust real estate market in offering implicit guarantees for local government debt.

The land finance system plays a crucial role in supporting economic growth but also creates a self-reinforcing cycle. Local governments reinvest revenues from land sales and debt financing to stimulate the local economy, which drives up land prices and further strengthens their financing capacity. However, this cycle also exposes the system's inherent vulnerability and unsustainability, particularly when adverse shocks occur in land supply or demand, or when local economic growth slows.

2.2 Pre-TRL landscape of real estate sector in China

China's real estate sector during the pre-TRL period is characterized by overinvestment and overleverage. Local bureaucrats, motivated by career concerns, have been prioritizing local economic growth, creating incentives to overinvest in infrastructure to solicit businesses (Li and Zhou, 2005; Song and Xiong, 2018; Xiong, 2023). These incentives amplify local governments' demand for funding which heavily relies on (expected) land prices and land sales revenues, as described in the previous section. To ensure the sustainability of the self-reinforcing cycle, local governments have motivation to provide explicit and implicit guarantees to the real estate sector. Anticipating these governmental guarantees, real estate developers are motivated to overbuild properties and engage in excessive leveraging (Xiong, 2023). The high leverage of developers could be further exaggerated by favorable credit policies where banks are pushed to lend to developers, which were present for a long period of time (Glaeser et al., 2017).

Our calculation shows that during the period of 2016Q1-2020Q2, the average annual debt growth for a sample of 861 Chinese developers reached 28%, and the average debt-to-asset

ratio reached 61%. In comparison, the average debt-to-asset ratio was just over 40% for non-developer firms for publicly listed Chinese firms during the same period, and the statistic was around 50% (45%) for real estate developers in the US (Europe) as of early 2020.[7]

According to Liu and Xiong (2020), in 2016, real estate-related debt comprises approximately 25% of banks' assets in China. This significant exposure of banks to the real estate sector renders it systemically important (Xiong, 2023), making the slowdown of China's urbanization process and the real estate market since 2016 particularly concerning for the central and local governments. Moreover, the outbreak of the COVID-19 pandemic exerted additional adverse pressure on the real estate market by diminishing housing demand (Chang et al., 2025).

2.3 Institutional details of the TRL policy

Alongside the high leverage and systemic importance of the real estate sector, the state introduced the Three-Red-Line policy in August 2020, aiming to curb debt accumulation among developers and mitigate the financial risks associated with the real estate market. Specifically, the policy states that real estate developers should adhere to the three rules to maintain a healthy financial condition: (i) the liability to asset ratio after excluding advance receipts (i.e., red line 1) not exceeding 70%, (ii) the net gearing ratio (i.e., red line 2) not exceeding 100%, and (iii) cash to short-term debt ratio (i.e., red line 3) greater than 100 %. With no red lines violated, a developer is allowed to have an annual growth in interest-bearing debts of 15%; the violation of any one red line reduces the allowable interest-bearing debt growth to 10%, and the upper limits go down to 5% and 0% if a developer violates two and three red lines, respectively. In sum, while all developers are subject to the limit on annual interest-bearing debt growth of 15%, those currently with higher financial burdens will be subject to more stringent debt financing constraints.

Given the high leverage of developers during the pre-policy period, it is reasonable to

[7]The data is available at https://pages.stern.nyu.edu/~adamodar/New_Home_Page/datafile/dbtfund.html.

conjecture that financing constraints would limit developers' ability to raise new debt and repay existing obligations, potentially leading to a contraction in their investments, including land acquisitions. By mapping this shock to developers onto the land finance system, we argue that the Three-Red-Line policy has the potential to trigger a chain reaction, beginning with a reduction in developers' demand for land parcels, followed by a decline in local governments' land sales revenues and financing capacity, lower economic prospects, and ultimately depressing land prices, creating a vicious cycle within the land finance system. Additionally, since banks are heavily exposed to developers' debts, if the policy's financing constraints cause financial distress for developers, financial risks in the real estate sector could spread to the broader financial market, increasing the likelihood of systemic risk. However, in light of arguments presented by Xiong (2023) and Chang et al. (2025), that governments typically intervene in the market to prevent potential real estate and financial crises [8], we conjecture that local governments would take actions (e.g., through more land purchases by LGFVs) to address the decline in land parcel demand in an effort to maintain the stability of the real estate and financial markets.

3 Data collection and empirical strategy

3.1 Data collection

We first obtain a complete list of transactions of residential land parcels from January 2016 to December 2022 from China Real Estate Information (CREI) website.[9] The dataset encompasses 170,584 transactions distributed across 293 prefecture-level cities and 4 direct-administered municipalities in China during the sample period.

For each land parcel transacted, we collect information of the transaction date, transaction

[8] Xiong (2023) provides the example of Evergrande, that the state privately instructed local governments to address Evergrande's financial situation and organize a partial bailout through state-owned banks and firms. Chang et al. (2025) documents that local governments managed land prices through land purchases by LGFVs during the post-COVID period.
[9] See http://www.crei.com.cn/. The website is maintained by the State Information Center (Administration Center of China E-government Network), which is a public institution directly affiliated to the National Development and Reform Commission (NDRC).

price, land area, and the name of purchaser. We identify the ownership type of each land purchaser by the ownership label retrieved from Qiyeyujingtong, a widely used financial database for firm registration and operation information.[10] Based on this information, for each city, we calculate the ratio of the land purchase value by non-SOE developers to the total land transaction value in each year (denoted as *NSOEdep*), to measure how much the residential land market of a city relies on non-SOE developers' transactions.

For each of the 297 cities in the sample, we collect the annual information of its total land sales revenues from WIND and of its general public budget revenue from CSMAR, and calculate a city's fiscal reliance on land sales revenues (*Landdep*) as the ratio of land sales revenues to the sum of general public budget revenue and land sales revenues. By multiplying *Landdep* with *NSOEdep*, we quantify the fiscal reliance of a city on non-SOE developers' residential land purchases (denoted as *NSOElanddep*).

The proprietary dataset of developers' financial performance is sourced from China Real Estate Index System (CREIS), a recognized commercial real estate data provider in China. CREIS identifies 2,801 developer firms, including 246 publicly listed firms and 2,555 private firms in China. To merge the dataset of developers with that of land purchases, we first compile a list of subsidiaries for each developer firm. This is motivated by the fact that developers typically set up local subsidiaries for land acquisition and development purpose, rather than conducting such activities at the parent firm level. We retrieve each developer's subsidiary list from Qiyeyujingtong, and identify 159,356 subsidiaries for the 2,801 developers in total. Second, we match names of developers (or corresponding subsidiaries) with names of land purchasers in the dataset of land parcels transacted.

After the merge, we identify that the residential land transaction value of the 2,801 developers accounts for 59% of the total transaction value on the residential land market, highlighting the large coverage of the developer sample.[11] After screening out developers

[10] The dataset of Qiyeyujingtong has been used by Chang et al. (2025).

[11] If only publicly listed listed firms are considered, their transaction value only accounts for 30% of all transactions on the market.

with no valid financial data, with no financial data in either the pre- or post-policy period and with no land purchases during the sample period, our final sample of 861 developers covers 46% of the total transaction value on the residential land market. Additionally, to identify whether a developer is a LGFV, or a non-LGFV SOE developer, we manually search developer names via Qiyeyujingtong which labels such information for each registered firm. The manual search gives us 122 non-SOEs, 103 non-LGFV SOEs and 636 LGFVs. While non-SOEs only represent 14% of the sample, their land purchases constitute over 50% of the total transaction value. Panel A of Table 1 presents the sample screening process.

3.2 Empirical strategy and summary statistics

In order to understand the role of financing constraints on developers' land purchases, we utilize the TRL policy as the exogenous shock to developers. While all developers are subject to the policy, developers with varying debt burdens will be exposed to different degrees of financing constraints. In light of this, we classify a developer into the treated or the control group according to the number of red lines it violated based on the statistics of the three red-line metrics over two years prior to the policy introduction (Q3 of 2020).[12] Specifically, we define the violation of the first red line if the debt-to-asset ratio after excluding advance receipt (*Redline*1) of a developer was greater than 70% in every quarter during the period of 2018Q3 - 2020Q2. The same approach to define red-line violation is also applied to the net gearing ratio (*Redline*2) and the cash to short-term debt ratio (*Redline*3).

Panel B of Table 1 reports the number of developers by sub-groups according to the number of red-line violations. It shows that out of the 861 developers in the sample, 440 developers violated no red lines during the pre-policy period, 289, 101, and 31 developers violated one, two and all the three red lines, respectively. We define the treated group of developers as those that violated at least one red line over two years prior to 2020Q3

[12] We limit the calculation to the two years immediately preceding the policy, as financial performance from a more distant past is less likely to accurately reflect the current financial condition of a given developer.

(indicated by the dummy *Treat*), and the control group as those that did not violate any red lines during the same period.

To quantify developers' land acquisition activities, we construct four variables. *Purchase* is an indicator for a developer purchasing any land parcels over a quarter, *Parcel* measures the number of land parcels purchased in a quarter, *Area* is the total area of land parcels purchased in a quarter, and *Value* is the total CNY amount of land parcels purchased in a quarter. We also construct control variables capturing developers' financial performance characteristics, including total assets (*Asset*), return on assets(*ROA*), net profit margin(*Netprofitmargin*), and sales growth rate (*Salegrowth*).

Panels A-C of Table 2 report the summary statistics of the above variables covering the sample period between 2016Q1 and 2022Q4, for the whole sample, the non-SOE developers and SOE developers, respectively. It suggests that non-SOE developers have a higher tendency to purchase lands (41.9% versus 23.6%), and purchase more lands in terms of number of land parcels (2.24 versus 0.67), area (1.20 versus 0.32 million square meter) and value (CNY 1,409 versus 278 million) of land parcels than SOE developers per quarter. Meanwhile, the statistics of the three red-line metrics suggest that non-SOE developers are, on average, more indebted as they have higher debt-to-asset ratio, and net gearing ratio than SOE developers.

The higher leverage of non-SOE developers is consistent with the fact that, despite exhibiting higher returns on assets, these firms have lower net profit margins, possibly because their higher financing costs and interest burdens offset the gains from more efficient asset utilization. Despite these differences in firm characteristics, there are a large fraction of developers in both groups that violated at least one red line prior to the policy introduction (65.6% for non-SOE developers and 46.1% for SOE developers).

To formally test the impact of the policy on developers' land purchase decisions, we estimate the following Difference-in-Differences (DiD) model,

$$LandPurchase_{i,t} = \alpha + \beta Treat_i \times Post_t + \gamma' \text{FirmControls}_{i,t-1} + \delta_i + \mu_t + \epsilon_{i,t} \quad (1)$$

where the dependent variable can be one of the four variables: *Purchase*, *Parcel*, *Area* and *Value*. Given the large dispersion of *Parcel*, *Area* and *Value*, Poisson regressions are adopted in their specifications whereas OLS regressions are adopted in the specifications for *Purchase*. $Treat_i$ is an indicator for developers in the treated group, and $Post_t$ is an indicator for the period after 2020Q3 when the TRL policy was introduced. $FirmControls_{i,t-1}$ indicates a set of developers' characteristics including natural logarithm of total assets (*LnAsset*), return on assets(*ROA*), net profit margin($Netprofitmargin$), and sales growth rate (*Salegrowth*) in the previous quarter. δ_i and μ_t indicates developer- and quarter-fixed effects. All the continuous variables in the regressions are winsorized at 1% and 99% level.

4 Financing constraints and land purchases

4.1 Baseline results

Panel A of Table 3 reports the results after estimating Eq. (1), with the first and last four columns showing the results without and with control variables, respectively. It shows that all the coefficients of $Treat \times Post$ are negative and statistically significant at 1 percent, suggesting that the reduction in the probability of land purchases, and in the number, area and value of land acquisitions by the treated group of developers after the TRL policy is greater than those for the control group of developers.

In terms of economic magnitude, specification (1) suggests that following the policy, treated developers were 6.5 percentage points less likely to purchase lands in a given quarter, corresponding to a 25% reduction relative to the overall probability of acquiring lands in a quarter (26.2%). Specifications (2) - (4) further suggest that the TRL policy reduces the number (area or value) of land purchases of the developers faced with more stringent financing constraint by 40.9% (41.5% or 42.9%) relative to those exposed to less stringent constraint. This translates to a reduction of approximately 0.44 land parcel (22,964 square

meters or CNY 236 million) for the average developer per quarter.[13] Specifications (5)-(8) show qualitatively similar results after controlling for developers' operating performance in the previous quarter.

To test whether the negative impact of TRL policy varies with ownership types of developers, we repeat the estimation of Eq.(1) on non-SOE and SOE developers separately, with the results being presented in Panels B and C of Table 3, respectively. The results suggest that all the coefficients of $Treat \times Post$ in the specifications for non-SOE developers are negative and highly statistically significant at 1 percent, whereas only three out of eight coefficients of $Treat \times Post$ are statistically significant at 10 percent in the specifications for SOE developers. Moreover, the magnitudes for the coefficients of $Treat \times Post$ in Panel B are all greater than those for the corresponding specifications in Panel C, suggesting that the negative impact of TRL policy on non-SOE developers is economically stronger than on SOE developers.

To visualize the dynamic effects of the TRL policy on developers' land purchases and to test the parallel trend assumption, we plot the coefficients after estimating Eq. (2). To conserve space, we report only the results based on *Value*, which serves as a comprehensive measure of developers' land acquisition activities.

$$\begin{aligned} Value_{i,t} = \alpha + \beta_1 D_{i,t}^{-6} + \beta_2 D_{i,t}^{-5} + \cdots + \beta_{13} D_{i,t}^{7} + \beta_{14} D_{i,t}^{8} + \gamma' \text{FirmControls}_{i,t-1} \\ + \delta_i + \mu_t + \epsilon_{i,t} \end{aligned} \tag{2}$$

where the dummies ('D's) equal to zero except that D^{-j} equals one for developers in the jth quarter prior to 2020Q3 (indicators for quarters prior to 2019Q1 are subsumed to D^{-6}), and D^{+j} equals one for developers in the jth quarter post 2020Q3 (indicators for quarters post 2022Q3 are subsumed to D^{8}). We exclude the quarter with the introduction of the TRL policy, thus estimating the dynamic effect of TRL policy on land purchases relative to quarter 2020Q3. Panels (a)-(c) of Figure 1 plot the coefficients results and the 99% confidence

[13] For the calculation of economic impact on number of land parcels purchase: the percentage change in number of land parcels is Exp(-0.526) - 1 = -40.9%. The mean of the number of land parcels purchased by the treated group per quarter during the pre-policy period is 1.07, therefore the change in number of parcels is 1.07 × (-40.9%) = -0.44.

intervals for the full sample, the non-SOE developers and SOE developers, respectively.

Figure 1 highlights two findings. First, for all the three panels, the land purchase value does not differ statistically significantly between the treated and the control groups of developers prior to the TRL policy. Second, the impact of the TRL policy on land purchases differs strikingly between the non-SOE and SOE developers: the non-SOE developers exposed to stricter financing constraints significantly reduce their land purchase amount, whereas the reduction is not observed for SOEs. The results suggest that the parallel trend condition in absence of the TRL policy is satisfied and that the effects of debt financing constraints on non-SOE developers' land purchases could persist for (at least) quarters after the policy introduction.

To test the robustness of the results, we perform the estimation on placebo treatment samples. To address the concern of omitted variables or random factors driving the results, we randomly select the treated group of developers and treated quarters, and repeat the draws 500 times to obtain the distributions of the estimates of $Treat \times Post$ after estimating Eq.(1) on the full sample. Panels (a)-(d) of Figure 2 plot the results corresponding to the specifications for *Purchase*, *Parcel*, *Area* and *Value* after controlling for developers' financial performance, respectively. For all specifications, the estimated coefficients of $Treat \times Post$ mostly concentrate around zero, with the "true" coefficient of $Treat \times Post$ (indicated by the vertical dash line) distant from the means of placebo distributions. Therefore, the placebo tests reinforce our inference that the negative effects of the TRL policy on developers' land purchases are neither random nor driven by omitted factors.

Overall, this section provides consistent evidence that financing constraints reduce developers' land purchases. Importantly, the effect is less pronounced for SOE developers. The evidence is in line with our hypothesis that SOE developers' land purchase activities are less adversely affected by the TRL policy, possibly arising from the implicit government guarantees prevalent in SOEs.

4.2 Cross-sectional analysis

4.2.1 Stringency of the financing constraints

To further validate the observed results, we examine the heterogeneity effects with the stringency of the financing constraints. Given the policy design, which imposes lower debt growth caps on more heavily indebted developers, it is natural to expect that developers who violated more red lines during the pre-policy period would experience greater financial distress and, as a result, make more significant reductions in land purchases following the policy implementation. To empirically test this conjecture, we estimate the following DiD model,

$$\begin{aligned} LandPurchase_{i,t} = \alpha + \beta_0 Treat1_i \times Post_t + \beta_1 Treat2_i \times Post_t + \\ \beta_2 Treat3_i \times Post_t + \gamma' \text{FirmControls}_{i,t-1} + \delta_i + \mu_t + \epsilon_{i,t} \end{aligned} \tag{3}$$

where $Treat1_i$ ($Treat2_i$, or $Treat3_i$) is an indicator for developers in the treated group that violated one (two, or all three) red line(s) during the pre-policy period.

Table 4 reports the results after estimating Eq.(3). The first column shows that the coefficients of $Treat1 \times Post$, $Treat2 \times Post$ and $Treat3 \times Post$ are all negative and statistically significant at 1-5% level. Importantly, the coefficient magnitude is greatest (smallest) for $Treat3 \times Post$ ($Treat1 \times Post$). Qualitatively similar patterns are observed when *Parcel*, *Area* and *Value* are examined. The consistent evidence suggests that the negative effect of TRL policy on developers' land purchasing strengthens with the stringency of the financing constraints imposed.

4.2.2 Pre-TRL debt term structure

To further strengthen the causal interpretation that the TRL policy drove the observed reduction in developer land purchases, we conduct a cross-sectional analysis partitioning developers based on their pre-policy debt term structure. The rationale is that if the TRL

policy truly constrained land purchases by tightening financing conditions, developers that relied more heavily on long-term debt prior to the policy should be disproportionately affected. This is because developers with longer debt maturity structures would face greater difficulty in quickly adjusting their balance sheets to comply with the red-line thresholds. In contrast, if unobserved factors or confounding shocks were driving the results, we would expect no systematic difference in treatment effects across debt term structures.

Table 5 reports the results in Panels A and B for subsamples of developers with high and low (above and below the median) pre-TRL long-term debt reliance, measured as the average ratio of long-term interest-bearing debt to total interest-bearing debt over the two years prior to the policy introduction. The estimates indicate that the coefficient for $Treat \times Post$ is larger in both economic magnitude and statistical significance for the high long-term debt subsample. The results provide additional support that the TRL policy operated as a genuine financial constraint, with its impact amplified by developers' rigid balance sheets and inability to quickly reduce long-term debt.

4.3 LGFVs' land purchases in response to TRL policy

As discussed in Section 2, local governments' fiscal health and their plans to stimulate economic development heavily depend on land sales. In our sample, land sales revenues account for 36.2% of the total fiscal revenues for the average city. While the TRL policy reduces the land purchases of non-SOE developers, would local governments step in to alleviate its negative effects on fiscal revenues and the systemic risks in the real estate and financial markets?

Chang et al. (2025) document that local governments managed land prices through land purchases by LGFVs. Following this line of thoughts, we conjecture that LGFVs may acquire more lands in comparison with other developers in response to the TRL policy, for the purpose of upholding land sales revenues of local governments. Specifically, if the conjecture is true, we should observe that LGFVs have higher propensity to purchase lands than non-

LGFV SOE developers following the TRL policy, although they both may carry implicit government guarantees and are immune to the negative shock of financing constraints.

To empirically test the conjecture, we compare the land purchase of LGFVs and non-LGFV SOEs following the TRL policy within the sample of SOE developers. Table 6 reports the results after controlling for developer characteristics, developer- and quarter-fixed effects. It shows that all the coefficients of $LGFV \times Post$ are positive and statistically significant at 5 or 1 percent, regardless of the dependent variable used. In economic sense, the TRL policy is associated with a 10 percentage point increase in the likelihood for LGFVs to buy lands relative to non-LGFV SOE developers, raising the probability from 20.2% during the pre-policy period to 30.2% following the policy. It corresponds to an increase of approximately 0.17 land parcel (or CNY 100.3 million) purchased by the average LGFV per quarter.

While LGFVs typically buy lands within the cities where they are registered for infrastructure construction purposes, it is possible that they participate in land purchases in other cities for profit-pursuing purpose. Therefore, one could argue that the increase in LGFVs' land purchases may not be driven by their motivation to uphold land sales revenues for their parent local governments, but rather due to their profit-pursuing motives. To rule out this alternative explanation for LGFVs' land purchase activities, we conduct a land-parcel-level analysis to assess whether LGFVs are more likely to purchase land in their parent cities. It is reasonable to expect that profit-driven land purchases would not necessarily result in more acquisitions within the same city where the LGFV is registered. Formally, we estimate the following model,

$$\begin{aligned} LGFV_{i,j,t} = \alpha + \beta_1 Samecity_{i,j} + \beta_2 Samecity_{i,j} \times Post_t + \gamma' \text{FirmControl}_{i,t} \\ \rho' \text{Landprice}_j + \mu_{j,t} + \epsilon_{i,j,t} \end{aligned} \tag{4}$$

where $LGFV_{i,j,t}$ is a dummy indicating that the developer i that purchases a land parcel j at time t is a LGFV. $Samecity_{i,j}$ is a dummy indicating that developer i and the land parcel j it purchases are located in the same city, $Landprice_j$ is the price of a parcel, and $\mu_{j,t}$ is the

City × *Quarter*-fixed effects.

Table 7 reports the results. The first column shows the results when the interaction term of *Post* × *Samecity* is not included in the specification. It suggests that, on average, LGFVs tend to buy lands in the cities where they are registered, which is consistent with their operational objective of stimulating the land and economic development of local governments. The positive coefficient of *Post* suggests that LGFVs' propensity of land purchasing relative to other developers increases post the TRL policy, which is consistent with the findings in Table 6. The second column, with *City* × *Quarter*-fixed effects, further shows that the coefficient of *Post* × *Samecity* is positive and statistically significant at 1 percent, suggesting that the propensity for LGFVs to buy lands in their parent cities becomes stronger post the policy.

To validate the results further, we split the sample by whether the city where a land locates relies heavily on non-SOE developers' land purchases. It is natural to expect that if a LGFV purchases lands in an attempt to uphold its local land market in response to the TRL policy, its intervention should be more prominent (or at least no less prominent) in cities whose fiscal revenues rely more heavily on non-SOE developers' land purchases prior to the policy. Specifically, we define a city relies heavily on non-SOE developers' land purchases if *NSOElanddep* is greater than the median across all cities as of 2019. The results presented in the last two columns in Table 7 suggest that for both groups of cities, LGFVs become more likely to buy lands within their parent cities post the policy, while the effect is slightly greater for the cities with stronger reliance on non-SOE developers (i.e., the coefficient of the interaction term is 0.095 and 0.091 for cities with high and low reliance on non-SOE developers, respectively).

Overall, this section provides consistent evidence in support of the conjecture that LGFVs intensify their land purchases in response to the shrink in the land sales to non-SOE developers triggered by the TRL policy. The evidence highlights the role of LGFVs (and local governments) in supporting the land market in the presence of external financing constraints imposed on developers.

4.4 Impact on the land market structure

So far, we have established that the financing constraints imposed by the TRL policy alter developers' land purchase decisions, which varies among developers with different ownership nature. Would the changes in developer-level land purchase decisions aggregate and re-structure the land market, and to what extent?

To answer this question, panels (a) and (b) of Figure 3 plot the total value (in CNY 10 billion) and the value fraction of land purchases by the non-SOE developers, non-LGFV SOE developers and LGFVs separately in each year between 2016 and 2022, when the developers in the sample are considered. Two key findings are observed: (i) the total transaction value of residential lands slumps from CNY 2,342 billion in 2020 to CNY 1,074 billion in 2022, by over 50%; (ii) the shrink in total transaction value for non-SOE developers over doubles that for non-LGFV SOE developers (i.e. 81% vs. 36%), while the total value of lands purchased by LGFVs increases by 30%; and this corresponds to the fact that the market share of non-SOE developers has shrunk by over half, that of non-LGFV SOEs has grown by 40%, while that of LGFVs has tripled, as shown in plot (b).

Panels (c) - (d) and (e) - (f) of Figure 3 plot analogous statistics for the transactions of lands in cities that have high and low dependence on non-SOE developers' land purchases, respectively. They suggest that the previously observed pattern is more prominent for lands in cities with high *NSOElanddep*. Specifically, while the market share of non-SOE developers reduces by 34 percentage points from 2020 to 2022 in cities of low *NSOElanddep*, the decline for cities of high *NSOElanddep* reaches 45 percentage points. In contrast, the market share of LGFVs in cities of low *NSOElanddep* increases by 21 percentage points, which is only half of the increase in their market share in high *NSOElanddep* cities.

Figure 3 visualizes the statistics based on the developers in our sample (i.e., that have financial data). To rule out the possibility that the sample is not representative, we draw the plots analogous to those in Figure 3 based on all the transactions on the land market, and qualitatively similar patterns are obtained, as shown in Figure 4.

To further assess whether the observed reallocation of land purchases entails efficiency costs, we examine developers' efficiency in converting acquired land into revenue. Specifically, we regress the ratio of revenue to land area (or CNY value amount of lands) acquired on ownership indicators interacted with the post-TRL policy period. The results, reported in Appendix B Table B3, reveal that SOE developers generate lower revenue per unit of land compared with non-SOE developers in the post-policy period (statistically significant at 1% level). Within SOEs, LGFVs exhibit even lower revenue per unit of land than non-LGFV SOEs after the policy.

Combining these estimates with the observed changes in market shares, we provide a back-of-the-envelope assessment of the aggregate efficiency loss. The market share of non-SOE developers declined by approximately 36 percentage points between 2020 and 2022, while SOE developers' revenue per unit of land is roughly 9% lower than that of non-SOEs. Multiplying these numbers suggests that the shift in land market shares corresponds to an approximate 3% reduction in aggregate land-use efficiency, highlighting that the observed reallocation is not only substantial in scale but also costly in terms of revenue-generating efficiency. These findings are consistent with the misallocation framework documented in Hsieh and Klenow (2009).

Overall, this section provides evidence that changes in individual developers' land purchase decisions are significant in aggregate, effectively re-structuring the land market. This reallocation is characterized by a sharp decline in the market share of non-state-owned developers, particularly in cities where fiscal revenues rely heavily on non-SOE land purchases. Moreover, the shift toward SOE and LGFV developers is not without cost: these developers generate lower revenue per unit of land compared with non-SOEs, implying a non-negligible reduction in aggregate land-use efficiency.

5 Mechanism analysis

To deepen the understanding of the relationship between the TRL policy and developers' land purchases, this section explores the underlying mechanisms by exploring the effects of TRL policy on the access to and costs of debts for developers.

5.1 Access to debts

The TRL policy explicitly links allowable debt growth to developers' existing leverage positions, implying that more highly leveraged developers face tighter constraints on incremental borrowing capacity. Accordingly, we expect the policy to reduce the amount of new debt financing accessible to indebted developers. We further conjecture that this negative effect is weaker for SOE developers. This is because SOE developers are typically perceived as having stronger implicit guarantees and better access to credit markets, which may allow them to maintain compliance with regulatory thresholds more effectively and thus sustain higher permissible levels of debt growth, *ceteris paribus*.

To test the conjectures, we estimate the DiD model following Eq.(5) on the sample of all publicly listed firms to examine the impact of the TRL policy on developers' access to debts. Given that bank loans and bond financing constitute the two primary sources of external debt financing for developers, we examine both channels in our analysis. [14]

$$Debt_{i,t} = \alpha + \beta Developer_i \times Post_t + \gamma' FirmControls_{i,t} + \delta_i + \mu_t + \epsilon_{i,t} \tag{5}$$

where $Debt_{i,t}$ could be $LoantoAsset_{i,t}$ (i.e., the ratio of loan to total assets) or $Ln(Bond)_{i,t}$ (i.e., the logarithm of one plus the amount of bonds payable) of firm i at time t, at the end of June or December of a calendar year due to the semi-annual data available. $Developer_i$ is an indicator equal to one if firm i is a developer, and $Post_t$ indicates the period after 2020 Q3. $FirmControls_{i,t}$ indicate a set of variables that may affect firm i's access to debts, including

[14] In this analysis, only listed firms are considered in the sample given the unavailability of bank loan data of unlisted firms.

firms' Tobin's Q ($TobinQ$), return on assets (ROA), sales growth rate ($Salegrowth$), net profit margin ($Netprofitmargin$) and logarithm of total assets ($LnAsset$). δ_i and μ_t indicate firm and semi-annual fixed effects, respectively.

The first and last three columns of Table 8 report the results after estimating Eq.(5) using $LoantoAsset$ and $Ln(Bond)$ as the dependent variable, respectively. The first column shows that the loan to asset ratio of the average developer declines by 3% after the TRL policy relative to non-developer firms, statistically significant at 1 percent. This decline is also economically significant given that the mean loan to asset ratio of the sample firms is 16.2%.

Moreover, the next two columns show that the adverse effect of TRL policy on developers' access to bank loans is stronger for non-SOE developers, both statistically and economically, than for SOE developers. Similarly, the last three columns suggest that while there is a reduction in bonds payable for non-SOE developers in comparison with firms from other sectors (statistically significant at 5 percent), SOE developers issue more bonds post the policy in comparison with non-developer firms, albeit with no statistical significance.

The results confirm our conjecture that the negative impact of the TRL policy on access to debt financing differs between SOE and non-SOE developers. Specifically, SOE developers appear to be better positioned to maintain debt financing capacity and adjust their financing structures following the policy implementation, thereby enabling them to satisfy regulatory requirements associated with higher permissible debt growth for both bank loans and bond issuance.

Next, we restrict the sample to developers only, to examine whether the negative effect on access to debts differs between the treated and control groups of the developers, as specified in Eq.(6).

$$Debt_{i,t} = \alpha + \beta Treat_i \times Post_t + \gamma' FirmControls_{i,t} + \delta_i + \mu_t + \epsilon_{i,t} \tag{6}$$

where $Treat_i$ indicates that developer i comes from the treated group, i.e., violates at least one red line during the pre-TRL period, $Post_t$ indicates the period after 2020 Q3. $FirmControl_{i,t}$

indicates the same set of control variables as in Eq. (5). δ_i and μ_t indicate firm and semi-annual fixed effects, respectively.

Panels A and B of Table 9 present results when *LoantoAsset* and *Ln*(*Bond*) is the dependent variable, respectively. The first three columns of Panel A show that the loan to asset ratio of developers in the treated group declines following the TRL policy (statistically significant at 5 percent), and such effect is more pronounced for non-SOE developers than for SOE developers, both statistically and economically. The unbalanced adverse effect of the TRL policy on access to bank loans between SOE and non-SOE developers leads to higher *LoantoAsset* for SOE developers in comparison with non-SOE developers following the policy (as indicated by the positive coefficient of $SOE \times Post$ in the fourth column, statistically significant at 5 percent).

Qualitatively similar findings are observed for bonds payable as reported in Panel B. While the negative impact of the TRL policy on developers' access to bonds is statistically insignificant for the two sub-groups, the coefficient of $Treat \times Post$ for the non-SOE specification is greater than that for the SOE specification. Consistently, the fourth column suggests that following the policy, the increase in SOE developers' level of bonds payable is statistically higher than that of non-SOE developers.

Overall, this section documents evidence consistent with the hypothesis that SOE developers benefit from implicit government guarantees, which provide advantages over non-SOE developers in maintaining access to debt financing under explicit financing constraints.

5.2 Costs of debts

Building on the evidence that the TRL policy reduces developers' access to debt financing, we next examine whether the policy also increases debt financing costs. Existing literature suggests that tighter financial constraints are associated with higher expected risks and, consequently, a higher cost of capital (Whited and Wu, 2006; Campbell et al., 2012; Carvalho, 2018; Buehlmaier and Whited, 2018). In the context of the TRL policy, higher debt financing

costs may further weaken developers' ability to repay outstanding obligations and adjust their financing structures, thereby making it more difficult for them to satisfy the financing conditions required to qualify for higher permissible debt growth. Consistently, we conjecture that the TRL policy would drive up the costs of debts for developers, especially those carrying less government guarantees.

To test the conjecture, we compile a dataset of bond issuance by both developers and non-developer firms, and measure a firm's cost of debt by its bond yield at issuance.[15] Formally, we estimate the DiD model as shown by Eq.(7) on the sample of bonds issued by all listed firms, consistent with the sample used in Section 5.1.

$$\begin{aligned} Yield_{i,j,t} &= \alpha + \beta Developer_i \times Post_t + \gamma' FirmControls_{i,t} + \lambda' BondControls_{j,t} \\ &+ \delta_i + \theta_k + \mu_t + \epsilon_{i,j,t} \end{aligned} \tag{7}$$

where the dependent variable $Yield_{i,j,t}$ represents the yield at issuance of bond j of issuer i at time t, $Developer_i$ is an indicator equal to one if the issuer i is a developer firm, and $Post_t$ indicates the period after 2020 Q3. $FirmControls_{i,t}$ indicate a set of variables measuring the characteristics of issuer firm i including firms' sales growth rate (*Salegrowth*), the debt-to-asset ratio (*Leverage*) and the credit rating of the issuer (*Rating*).[16] $BondControls_{j,t}$ indicate a set of variables measuring the characteristics of issued bond j: the CNY value of the issuance (*Issuanceamount*), the time to maturity (*Maturity*) and the yield of treasury bonds with a maturity matching that of bond j (*TreasuryYield*). δ_i, θ_k, and μ_t indicate firm, bond market and quarter fixed effects, respectively. For the pre- and post-TRL policy comparison, we restrict the sample to firms that issue bonds in both periods. Table B5 in the Appendix presents the summary statistics of the characteristics of bonds and bond issuers.

Columns (1) and (2) in Table 10 report the results after the estimation of Eq.(7) on the non-SOE firms and the SOE firms, respectively. Column (1) shows that the coefficient of

[15] The bonds in the sample include corporate bonds, enterprise bonds, medium-term notes, and short-term notes, while excluding debt instruments such as bonds issued by financial firms, asset-backed securities, exchangeable bonds, convertible bonds and negotiable certificates of deposit.

[16] We assign values of 1 to 5 to issuers rated "A+" "AA-", "AA" "AA+", and "AAA", respectively.

Developer × *Post* is positive and statistically significant at the 10% level, indicating that, on average, the yield at issuance for bonds issued by non-SOE developers increases by 19 basis points (bps) following the TRL policy, relative to that for non-SOE non-developer issuers. This increase corresponds to 4.7% of the mean yield across all issued bonds. Column (2) suggests that the increase in bond yields is less prominent for SOE developers: the increase following the TRL is minimal with no statistical significance. The results for the control variables are consistent with the existing literature that yields at issuance increase with the issuance amount and the maturity of bonds (Reeb et al., 2001; Walker et al., 2021).

Moreover, Column (3) presents results when the sample is restricted to only developers, in order to compare any different effects of the TRL policy on SOE and non-SOE developers. It suggests that, in comparison with non-SOE developers, the yield at issuance for the average SOE developer declines by 18 bps following the TRL policy (statistically significant at 10 percent), corresponding to 4.8% of the mean yield across all bonds issued by developers.

Overall, the section documents evidence suggesting that while the bond yield at issuance for non-SOE developers surges following the TRL policy, it remains stable for SOE-developer issuers. The evidence supports our conjecture that SOE developers carry implicit government guarantees and therefore enjoy lower perceived risks and bond yields, particularly when exposed to external financing constraints.

6 Additional analysis

6.1 Impact on home sales

The previous sections establish the negative shock of the TRL policy to developers' land purchase decisions. Next, we ask the question of whether the effect of the policy could spill over to the retail market, i.e., developers' home sales. The motivation to ask this question is the dominance of pre-sale transactions in China's new home market. Statistics suggest that, as at the end of 2020, the area (CNY value) of pre-sold new homes accounts for 87.3%

(99.9%) of the total area (CNY value) of new homes sold.[17] Therefore, it is natural to expect that, under the TRL policy, potential home buyers would raise concerns about developers' financing stability and the subsequent delays and worsen quality of home delivery, which would result in shrink in developers' home sales and sale price. Moreover, if home buyers anticipate the "immunity" of SOE developers to the financing constraints, we would observe that any shrink in home sales and price is weaker for SOE developers.

To empirically test the above conjectures, we collect developers' home sales data from CREIS, which reports the total number, area, value, and the average price per square meter of homes transacted during a period of time for individual buildings and the name of the developer a building belongs to. We collect the information for each calendar quarter during the sample period for each of the buildings located in 147 cities of China, where the sample buildings belong to 39,997 developers (including subsidiaries).[18]

Since developers in the home sales dataset may act as subsidiaries of those in the financial performance dataset, we first extract shareholder information for all 39,997 developers from Qiyeyujingtong. The home sales data is then merged with the financial performance data by matching shareholders' names for subsidiaries and the developers' own names for parent companies, yielding a consolidated dataset of 736 developers. For each of the 736 developers in each quarter, we calculate the total number, area and CNY value of homes transacted across all buildings located in the 147 cities (denoted as Num_{Home}, $Area_{Home}$ and $Value_{Home}$, respectively). Moreover, for each developer, we define $Price_{Home}$ as the average price per square meter across homes transacted for all buildings in the 147 cities in a given quarter. Table B6 in the Appendix reports the summary statistics of the four variables. It suggests that, on average, the number (area and value) of home sales by non-SOE developers is over five times that of SOE developers, while the average price of homes per square meter is comparable between the two groups of developers.

[17] See details at https://www.bosidata.com/data/057504SJMI.html.
[18] CREIS provides home sales data for 147 cities, which together account for 29.2% of the total home sales area and 44% of the total home sales value as of 2022.

With developer-level home sales data, we estimate the following DiD model to examine the impact of the TRL policy on the retail market,

$$Homesale_{i,t} = \alpha + \beta Treat_i \times Post_t + \gamma' \text{FirmControls}_{i,t-1} + \delta_i + \mu_t + \epsilon_{i,t} \tag{8}$$

where the dependent variable can be one of the four variables: Num_{Home}, $Area_{Home}$, $Value_{Home}$ and $Price_{Home}$. $Treat_i$ is an indicator for developers in the treated group, and $Post_i$ is an indicator for the period after 2020Q3 when the TRL policy was introduced. Control variables include those used in the specifications for developers' land purchase analysis, plus developer- and quarter-fixed effects. Poisson regressions are adopted in the specifications to account for the large dispersion of the dependent variables.

Panels A and B of Table 11 report the results after the estimation of Eq.(8) on non-SOE and SOE developers, respectively. Panel A suggests that the coefficients of $Treat \times Post$ are all negative, with six out of the eight coefficients being statistically significant at 1-5% percent. The evidence supports our working conjecture that non-SOE developers' home sales decline following the TRL policy, possibly driven by home buyers' expectations of the policy's negative impact on developers' financing conditions. However, Panel B suggests that the statistically significant negative impact of the TRL policy on developers' home sales is not observed for SOE developers.

Figure 5 plots the dynamic effects of the TRL policy on developers' home sales.[19] It confirms that there are no meaningful differences between the treated and control groups of developers in the value of home sales during the pre-TRL period and that the adverse effects of the financing constraints on home sales only exhibit for non-SOE developers.

Overall, the evidence in this section indicates that home buyers can distinguish the differing effects of external financing constraints on the financial conditions of SOE and non-SOE developers, and, as a result, exhibit distinct home purchasing behaviors towards the

[19] Consistent with the parallel trend assumption test in Section 4.1, to conserve space, we present the results for the dynamic effects when $Value_{Home}$ is the dependent variable, which is a more comprehensive measure for a developer's home sales.

two groups of developers under the policy.

6.2 Impact on bank risk

Commercial banks, as loan providers, take a crucial role in the debt accumulation of developers. One major aim of the TRL policy is to mitigate financial risks in the banking sector by constraining debt financing of indebted developers. However, the previous sections have established that the TRL policy worsens developers' capability of renewing debt contracts and subsequently the ability of repaying debts. Therefore, whether the policy objective of reducing bank risk is achieved, or whether developers' financial distress spills over to their lenders and thereby increases bank risk, remains an empirical question.

To shed light on the question, we collect annual financial performance data, including loan loss provision, loan assets, industry exposure of loan assets, total assets, net income, equity, non-interest revenues, total revenues of 380 commercial banks in China. According to our calculations, during the pre-TRL period of 2016-2020, the loans offered to the real estate sector of the average commercial bank account for 6.19% of the total loans it offers in a given year, with the percentage ranging from zero to 41%.

To formally assess the impact of the TRL policy on bank risk, we estimate the following DiD model specified in Eq. (9),

$$LoanExpREsector_{i,t} \text{ or } Bankrisk_{i,t} = \alpha + \beta Treat_{banki} \times Post_t + \gamma' BankControls_{i,t-1} + \delta_i + \mu_t + \epsilon_{i,t} \quad (9)$$

where $LoanExpREsector_{i,t}$ represents bank i's loan exposure to the real estate sector, calculated as the ratio of outstanding loans to the real estate sector for the total loan assets at year t, $Bankrisk_{i,t}$ represents bank i's risk in year t, measured by either the Z-score developed for banks (i.e., the ratio of a bank's return on assets plus equity-to-asset ratio to the standard deviation of the return on assets) following Tan (2016), or the ratio of loan loss provision to total loan assets (*LLPTL*) of a bank. $Treat_{banki}$ is an indicator for banks more exposed to

risks from the real estate sector during the pre-TRL period, i.e., whose average share of loans to the real estate sector exceeds 10% of the total loans during years 2016-2020; $Post_t$ is an indicator for the post-TRL period. $Year2021$ and $Year2022$ are indicator variables for the individual years 2021 and 2022, respectively. $BankControls_{i,t-1}$ indicate a set of bank characteristics variables including logarithm of bank i's total assets (*LnSize*), the ratio of total loans to total assets of a bank (*Liquidity*), the ratio of non-interest revenues to total revenues (*Diversification*) and net profit margin (*Netprofitmargin*) in the previous year. δ_i and μ_t are bank- and year-fixed effects.

Columns (1)–(2) of Table 12 present the results with *LoanExpREsector* as the dependent variable. The coefficients indicate that the three-red-line policy significantly reduces banks' loan exposure to the real estate sector, consistent with prior evidence that the policy constrains developers' access to bank credit.

Columns (3)–(4) and (5)–(6) report the results for *Z*-score and *LLPTL*, respectively. Overall, the DiD coefficients are generally not statistically significant, suggesting that the TRL policy does not have a systematic effect on banks' overall financial risk. However, when we examine the two years following the TRL implementation separately, the coefficient on $Treat \times Year2022$ in column (4) is negative and statistically significant at the 10 percent level. This indicates that, in the short run, banks with greater exposure to the real estate sector experienced an increase in financial risk (a decline in *Zscore*) following the policy introduction, contrary to the policy's intended stabilizing effect.

Overall, this section documents little evidence that the TRL policy affects financial risks in the banking sector. If any, it reduces the financial stability of the banks more exposed to the real estate sector two years after the introduction of the policy. The evidence implies that the the immediate adjustment in banks' loan exposures may have temporarily increased risk for the most exposed banks, highlighting a potential trade-off between credit tightening and short-term financial stability.

6.3 Rule out alternative explanation

One potential concern is that the COVID-19 pandemic, rather than the TRL policy itself, may have driven the observed negative relationship between developer debt burdens and outcomes such as land purchases and home sales. While our DiD estimates suggest a causal impact of the TRL policy, the policy's implementation timeline overlapped significantly with the pandemic onset, raising the possibility that COVID-19 independently tightened developers' financing constraints and depressed market activity—particularly for highly leveraged firms. The pandemic could have amplified the effects of pre-existing debt burdens through disrupted supply chains, weakened housing demand, or reduced credit availability, thereby confounding our DiD results.

To address this concern and rule out the pandemic as an alternative explanation, we employ a Regression Discontinuity Design (RDD). The TRL policy established specific cutoff thresholds for developers' debt burdens, creating a sharp discontinuity in regulatory pressure based on their debt conditions. By comparing developers with debt burdens just above versus just below the regulatory thresholds—whose underlying debt conditions are otherwise similar—and examining their land purchases and home sales around the policy cutoff, we can isolate the policy-induced effect from potential confounding influence of COVID-19.

Table B7 and B8 in Appendix B present the estimates obtained from RDD for developers' land purchase probability and home sales values. Since previous sections establish that the TRL policy effects are driven primarily by non-SOE developers, we perform the RDD analysis on the non-SOE developer sample during the post-TRL period, using each of the three red-line metrics as the cutoff in turn.

The RDD estimates yield results consistent with the main findings: non-SOE developers just above (or below, depending on the threshold) the red-line cutoffs exhibited a lower probability of land purchases and lower home sales than those just on the compliant side of the corresponding thresholds. These results remain robust across higher-order polynomial specifications, donut-hole tests, bandwidth sensitivity checks, and alternative kernel func-

tions. Overall, the RDD estimates support our main DiD findings and rule out COVID-19 as the driver of the observed reductions.

7 Conclusion

The Chinese real estate market, after decades of rapid expansion, has encountered significant shifts in recent years. The implementation of the 'Three-red-line' (TRL) policy in August 2020 marked a pivotal moment aimed at deleveraging the real estate sector by imposing stringent limits on annual debt growth for developers. Exploiting this natural experiment, this paper provides insights into the real effects of external financing constraints on real estate developers' investment decisions.

Using a large proprietary dataset encompassing both publicly listed and unlisted developers, which cover nearly half of the transactions on the residential land market, we observe a significant reduction in the likelihood and scale of land acquisitions by developers subject to more stringent TRL-policy constraints. This reduction is particularly pronounced among non-state-owned developers. Consistently, mechanism analysis reveals that the TRL policy reduces the access to bank loan and bond issuance and escalates costs of debts for developers, particularly for non-SOE developers compared to their SOE counterparts. The evidence supports the conjecture that SOEs carry implicit government guarantees and thereby are in a favored position when it comes to adjusting financial conditions to satisfy regulation requirements for higher cap on allowable debt growth.

Furthermore, our findings illuminate nuanced differences within the group of SOE developers, revealing that LGFVs increase their land purchases post-TRL policy implementation, a strategic move likely aimed at stabilizing land prices and safeguarding local government revenues derived from land sales. This evidence echos Chang et al. (2025) by highlighting the intertwined relationship between LGFVs and local governments, where financial constraints

on (non-SOE) developers may amplify LGFVs' incentives to intervene actively in the land market.

Additionally, our study evaluates broader systemic impacts, including the restructuring of the land market, the impact of the financing constraints on developers' home sales on the retail market, and implications for financial stability of commercial banks. We observe a substantial reduction in overall residential land transaction values post-TRL policy, coupled with a notable shift in market share towards SOE developers. This restructuring highlights the policy's intended and unintended consequences, shaping market dynamics in favor of SOE developers while exposing non-SOE developers to heightened financial vulnerabilities. Overall, our analysis deepens insights into the evolving landscape of China's real estate market, offering timely perspectives on policy-induced market adjustments.

References

Acharya, V. V., Banerjee, R., Crosignani, M., Eisert, T., Spigt, R., 2025. Exorbitant privilege? quantitative easing and the bond market subsidy of prospective fallen angels. Journal of Financial Economics 170, 104084.

Agarwal, S., Fan, M., Ghosh, P., Sarkar, A., Zhang, X., 2024. Developer transparency and mortgage access: Expanding homeownership .

Allen, F., Gu, X., Li, C. W., Qian, Y., et al., 2023. Implicit guarantees and the rise of shadow banking: The case of trust products. Journal of Financial Economics 149, 115–141.

Ambrose, B. W., Deng, Y., Wu, J., 2015. Understanding the risk of China's local government debts and its linkage with property markets, vol. 2557031. SSRN.

Ang, A., Bai, J., Zhou, H., 2023. The great wall of debt: Real estate, political risk, and chinese local government financing cost. The Journal of Finance and Data Science 9, 100098.

Bai, C.-E., Hsieh, C.-T., Song, Z., 2020. Special deals with chinese characteristics. NBER macroeconomics annual 34, 341–379.

Bai, C.-E., Hsieh, C.-T., Song, Z. M., 2016. The long shadow of a fiscal expansion. Tech. rep., National Bureau of Economic Research.

Beraja, M., Fuster, A., Hurst, E., Vavra, J., 2019. Regional heterogeneity and the refinancing channel of monetary policy. The Quarterly Journal of Economics 134, 109–183.

Blickle, K., 2022. Local banks, credit supply, and house prices. Journal of Financial Economics 143, 876–896.

Blickle, K., Brown, M., 2019. Borrowing constraints, home ownership and housing choice: evidence from intra-family wealth transfers. Journal of Money, Credit and Banking 51, 539–580.

Bosshardt, J., Di Maggio, M., Kakhbod, A., Kermani, A., 2024. The credit supply channel of monetary policy tightening and its distributional impacts. Journal of Financial Economics 160, 103914.

Brown, D. T., 2000. Liquidity and liquidation: evidence from real estate investment trusts. The Journal of Finance 55, 469–485.

Buehlmaier, M. M., Whited, T. M., 2018. Are financial constraints priced? Evidence from textual analysis. Review of Financial Studies 31.

Caggese, A., 2007. Financing constraints, irreversibility, and investment dynamics. Journal of Monetary Economics 54.

Caggese, A., Cuñat, V., Metzger, D., 2019. Firing the wrong workers: Financing constraints and labor misallocation. Journal of Financial Economics 133, 589–607.

Cai, H., Henderson, J. V., Zhang, Q., 2013. China's land market auctions: Evidence of corruption? RAND Journal of Economics 44.

Campbell, J. L., Dhaliwal, D. S., Schwartz, W. C., 2012. Financing constraints and the cost of capital: Evidence from the funding of corporate pension plans.

Carvalho, D., 2018. How Do Financing Constraints Affect Firms' Equity Volatility? Journal of Finance 73.

Chang, J., Wang, Y., Xiong, W., 2025. Price and volume divergence in china's real estate markets: the role of local governments. The Review of Financial Studies .

Chen, D., Jiang, D., Ljungqvist, A., Lu, H., Zhou, M., 2015. State capitalism vs. private enterprise. National Bureau of Economic Research.

Chen, K., Du, H., Ma, C., 2024. The spillover effects of real estate. American Economic Association: Washington, DC, USA .

Chen, K., Gao, H., Higgins, P., Waggoner, D. F., Zha, T., 2023. Monetary stimulus amidst the infrastructure investment spree: Evidence from china's loan-level data. The Journal of Finance 78, 1147–1204.

Chen, K., Wen, Y., 2017. The great housing boom of china. American Economic Journal: Macroeconomics 9.

Chen, T., Liu, L. X., Xiong, W., Zhou, L.-A., 2017. Real Estate Boom and Misallocation of Capital in China. Working Paper .

Chen, Y., Shi, S., Tang, Y., 2019. Valuing the urban hukou in China: Evidence from a regression discontinuity design for housing prices. Journal of Development Economics 141.

Chen, Z., He, Z., Liu, C., 2020. The financing of local government in china: Stimulus loan wanes and shadow banking waxes. Journal of Financial Economics 137, 42–71.

Cohn, J. B., Wardlaw, M. I., 2016. Financing constraints and workplace safety. The Journal of Finance 71, 2017–2058.

Cong, L. W., Gao, H., Ponticelli, J., Yang, X., 2019. Credit Allocation under Economic Stimulus: Evidence from China. Review of Financial Studies 32.

Czarnitzki, D., Hottenrott, H., 2011. R&D investment and financing constraints of small and medium-sized firms. Small Business Economics 36.

Deng, Y., Gyourko, J., Wu, J., 2012. Land and house price measurement in china. Tech. rep., National Bureau of Economic Research.

Di Maggio, M., Kermani, A., Palmer, C. J., 2020. How quantitative easing works: Evidence on the refinancing channel. The Review of Economic Studies 87, 1498–1528.

D'Lima, W., Lopez, L. A., Pradhan, A., 2022. Covid-19 and housing market effects: Evidence from us shutdown orders. Real Estate Economics 50, 303–339.

Fan, J., Liu, Y., Zhang, Q., Zhao, P., 2022. Does government debt impede firm innovation? evidence from the rise of lgfvs in china. Journal of Banking & Finance 138, 106475.

Fang, H., Gu, Q., Xiong, W., Zhou, L. A., 2016. Demystifying the Chinese housing boom. NBER Macroeconomics Annual 30.

Favara, G., Imbs, J., 2015. Credit supply and the price of housing. American Economic Review 105, 958–992.

Favilukis, J., Ludvigson, S. C., Van Nieuwerburgh, S., 2017. The macroeconomic effects of housing wealth, housing finance, and limited risk sharing in general equilibrium. Journal of Political Economy 125, 140–223.

Fazzari, S. M., Hubbard, R. G., Petersen, B. C., Blinder, A. S., Poterba, J. M., 1988. Financing Constraints and Corporate Investment. Brookings Papers on Economic Activity 1988.

Fieldhouse, A. J., Mertens, K., Ravn, M. O., 2018. The macroeconomic effects of government asset purchases: Evidence from postwar us housing credit policy. The Quarterly Journal of Economics 133, 1503–1560.

Floetotto, M., Kirker, M., Stroebel, J., 2016. Government intervention in the housing market: Who wins, who loses? Journal of Monetary Economics 80, 106–123.

Geng, Z., Pan, J., 2024. The soe premium and government support in china's credit market. The Journal of Finance 79, 3041–3103.

Gete, P., Zecchetto, F., 2018. Distributional implications of government guarantees in mortgage markets. The Review of Financial Studies 31, 1064–1097.

Glaeser, E., Huang, W., Ma, Y., Shleifer, A., 2017. A real estate boom with chinese characteristics. Journal of Economic Perspectives 31, 93–116.

Greenwald, D. L., Guren, A., 2025. Do credit conditions move house prices? American Economic Review 115, 3559–3596.

Gupta, A., Mittal, V., Peeters, J., Van Nieuwerburgh, S., 2022. Flattening the curve: pandemic-induced revaluation of urban real estate. Journal of Financial Economics 146, 594–636.

Gyourko, J., Shen, Y., Wu, J., Zhang, R., 2022. Land finance in china: Analysis and review. China Economic Review 76, 101868.

He, Z., Nelson, S., Su, Y., Zhang, A. L., Zhang, F., 2023. Land misallocation and local public finance in china. University of Chicago, Becker Friedman Institute for Economics Working Paper .

Hsieh, C.-T., Klenow, P. J., 2009. Misallocation and Manufacturing TFP in China and India*. The Quarterly Journal of Economics 124, 1403–1448, _eprint: https://academic.oup.com/qje/article-pdf/124/4/1403/5407260/124-4-1403.pdf.

Huang, J., Huang, Z., Shao, X., 2023. The risk of implicit guarantees: evidence from shadow banks in china. Review of Finance 27, 1521–1544.

Huang, Y., Pagano, M., Panizza, U., 2020. Local crowding-out in china. The Journal of Finance 75, 2855–2898.

Jeske, K., Krueger, D., Mitman, K., 2013. Housing, mortgage bailout guarantees and the macro economy. Journal of Monetary Economics 60, 917–935.

Jin, S., Wang, W., Zhang, Z., 2023. The real effects of implicit government guarantee: evidence from chinese state-owned enterprise defaults. Management Science 69, 3650–3674.

Jin, Y., Ru, H., Yang, E., Zou, K., 2026. Redefining china's real estate market: Land sale, local government, and policy transformation. Local Government, and Policy Transformation (January 15, 2026) .

Justiniano, A., Primiceri, G. E., Tambalotti, A., 2019. Credit supply and the housing boom. Journal of political economy 127, 1317–1350.

Li, B., Wang, Z., Zhou, H., 2022. China's anti-corruption campaign and credit reallocation from soes to non-soes. PBCSF-NIFR research paper .

Li, D., 2011. Financial constraints, r&d investment, and stock returns. The Review of Financial Studies 24, 2974–3007.

Li, H., Zhou, L.-A., 2005. Political turnover and economic performance: the incentive role of personnel control in china. Journal of Public Economics 89, 1743–1762.

Lin, H., Paravisini, D., 2013. The effect of financing constraints on risk. Review of Finance 17, 229–259.

Liu, C., Xiong, W., 2020. China's real estate market. National Bureau of Economic Research .

Moyen, N., 2004. Investment–cash flow sensitivities: Constrained versus unconstrained firms. The Journal of Finance 59, 2061–2092.

Reeb, D. M., Mansi, S. A., Allee, J. M., 2001. Firm internationalization and the cost of debt financing: Evidence from non-provisional publicly traded debt. Journal of Financial and Quantitative Analysis 36, 395–414.

Rogoff, K., Yang, Y., 2021. Has china's housing production peaked? China & World Economy 29, 1–31.

Song, Z., Storesletten, K., Zilibotti, F., 2011. Growing like china. American Economic Review 101, 196–233.

Song, Z., Xiong, W., 2018. Risks in china's financial system. Annual Review of Financial Economics 10, 261–286.

Tan, Y., 2016. The impacts of risk and competition on bank profitability in china. Journal of international financial Markets, Institutions and Money 40, 85–110.

Van Straelen, E., 2024. Desperate house sellers: Distress among developers. The Review of Financial Studies 37, 802–836.

Walker, T., Zhang, X., Zhang, A., Wang, Y., 2021. Fact or fiction: Implicit government guarantees in China's corporate bond market. Journal of International Money and Finance 116.

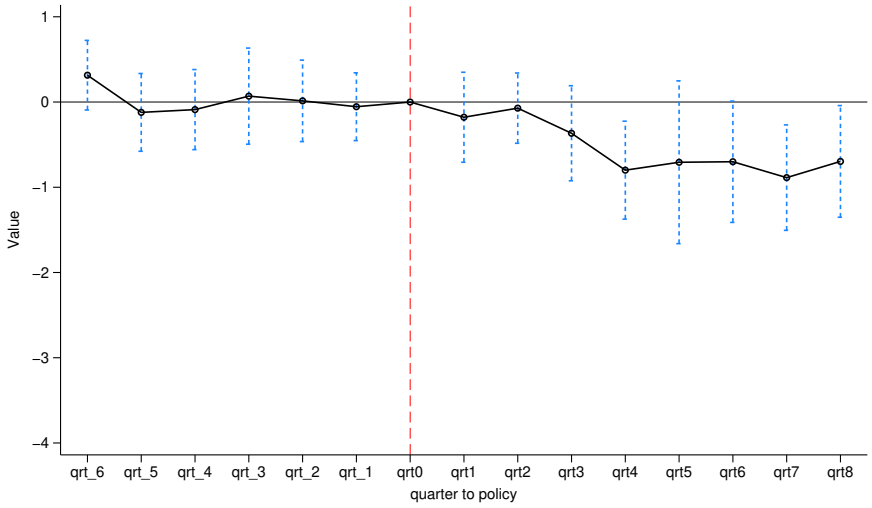
Whited, T. M., 1992. Debt, Liquidity Constraints, and Corporate Investment: Evidence from Panel Data. The Journal of Finance 47.

Whited, T. M., Wu, G., 2006. Financial constraints risk. Review of Financial Studies 19.

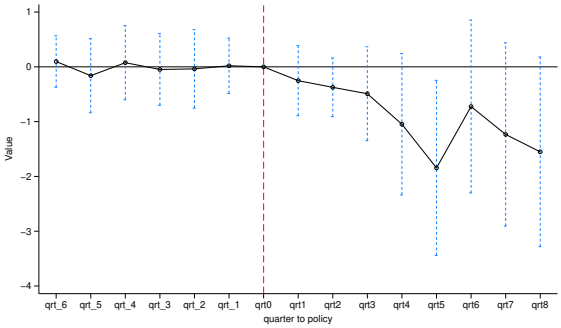
Wu, J., Gyourko, J., Deng, Y., 2012. Evaluating conditions in major chinese housing markets. Regional Science and Urban Economics 42, 531–543.

Xiong, W., 2023. Derisking real estate in china's hybrid economy. Tech. rep., National Bureau of Economic Research.

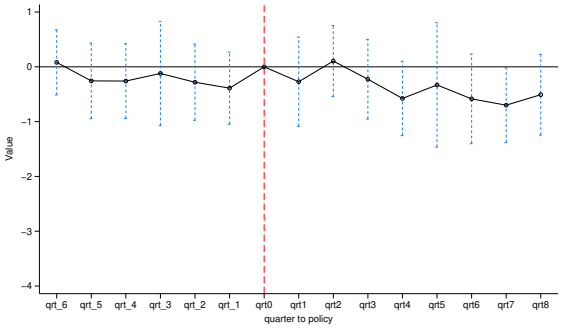
Zhang, Z., Xiong, Y., 2020. Infrastructure financing. The Handbook of China's Financial System pp. 208–226.



(a) Full sample



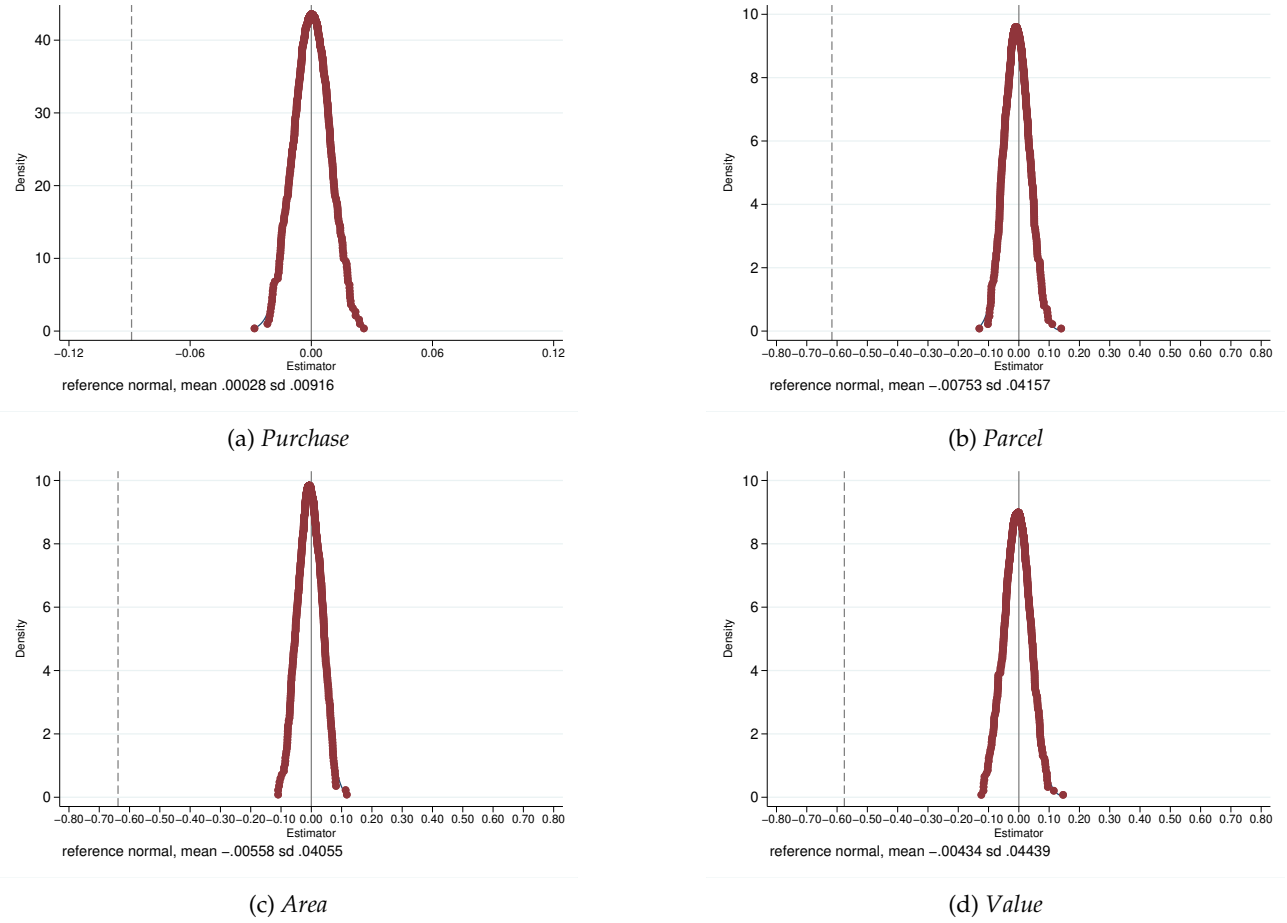
(b) Non-SOE developers



(c) SOE developers

Figure 1: Dynamic effects of the impact of the TRL policy on developers' land purchase value

This figure assesses the parallel trends assumption prior to the introduction of TRL policy. Dynamic effects of the TRL policy are estimated when the dependent variable is *Value*, corresponding to the specification presented in column (8) of Table 3. Panel (a) plots estimated coefficients and 99% confidence intervals based on the full sample. Panel (b) and (c) plots estimated coefficients and 99% confidence intervals based on the non-SOE and SOE developer sample, respectively. The vertical axis displays coefficient estimates, and the horizontal axis indicates event quarter relative to the TRL introduction quarter.



(a) *Purchase*

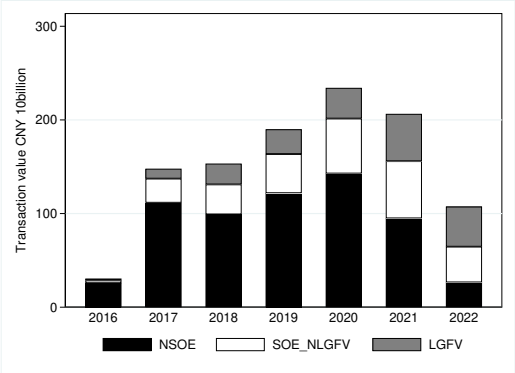
(b) *Parcel*

(c) *Area*

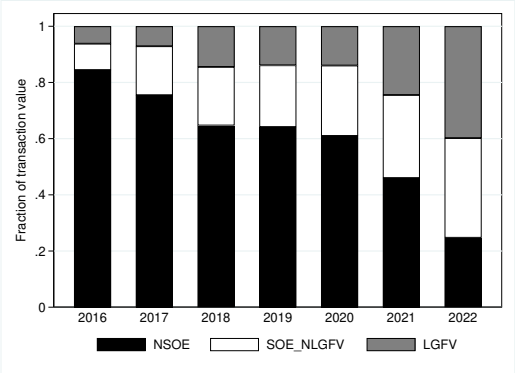
(d) *Value*

Figure 2: Placebo distribution of estimated treatment effects

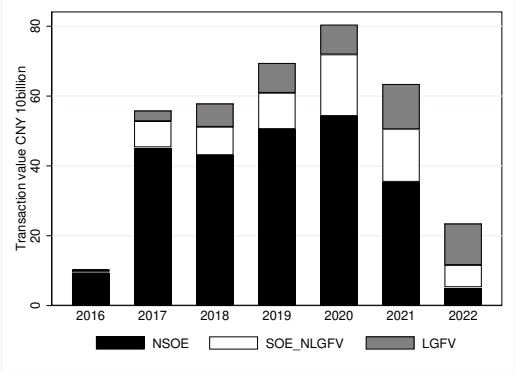
This figure shows the distribution of coefficients for the interaction term *Treat* × *Post* generated using 500 random draws of pseudo *Treat* or *Post*. Panel (a)-(d) plots the distribution of the pseudo *Treat* × *Post* when *Purchase*, *Parcel*, *Area* and *Value* is the dependent variable, respectively. The histogram summarizes the range and frequency of placebo estimates, with the actual estimated coefficient from the main specification (Panel A of Table 3, columns (5)-(8)) marked for reference.



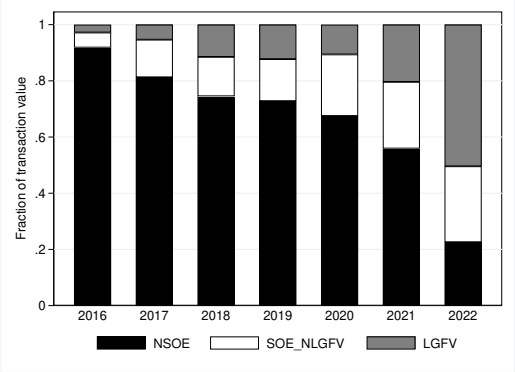
(a) All cities: Transaction value



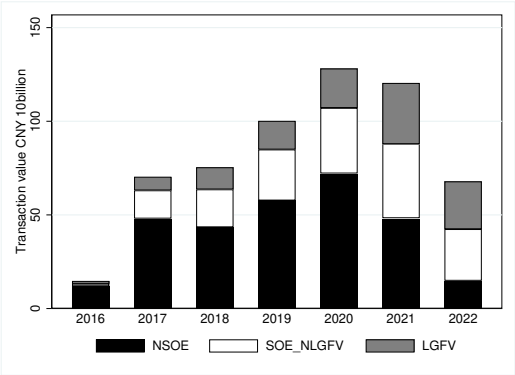
(b) All cities: Transaction value fraction



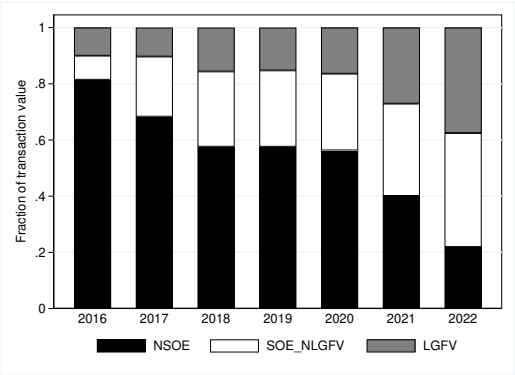
(c) High land sales dependence on non-SOEs: Transaction value



(d) High land sales dependence on non-SOEs: Transaction value fraction



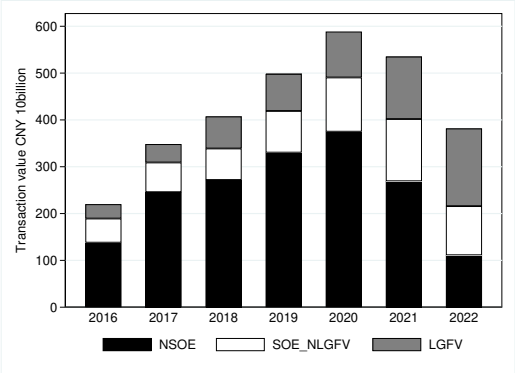
(e) Low land sales dependence on non-SOEs: Transaction value



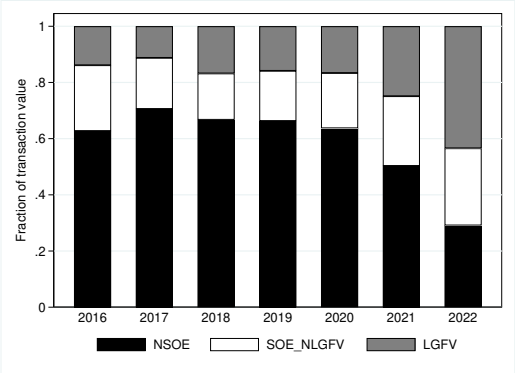
(f) Low land sales dependence on non-SOEs:Transaction value fraction

Figure 3: Land purchases by different groups of developers in the sample

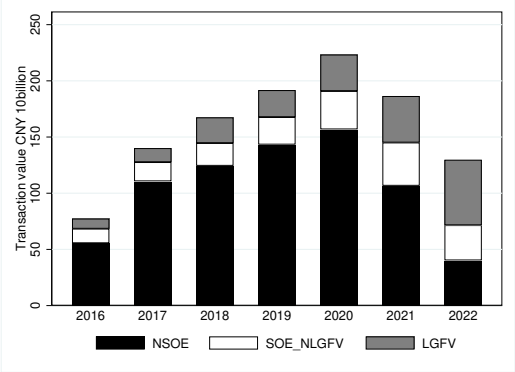
This figure shows the value (panels (a), (c) and (e)) and value fraction (panels (b), (d) and (f)) of land purchases by different groups of developers in the sample, i.e., non-SOE, non-LGFV SOE and LGFV developers. Panels (a)-(b) are based on statistics calculated for all cities, panels (c)-(d) are based on statistics calculated for the cities with high land sales dependence on non-SOE developers, and panels (e)-(f) are based on statistics calculated for the cities with low land sales dependence on non-SOE developers.



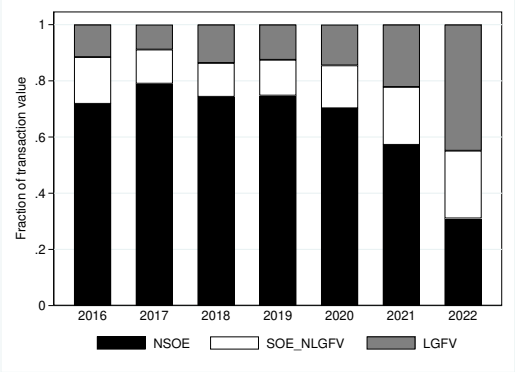
(a) All cities: Transaction value



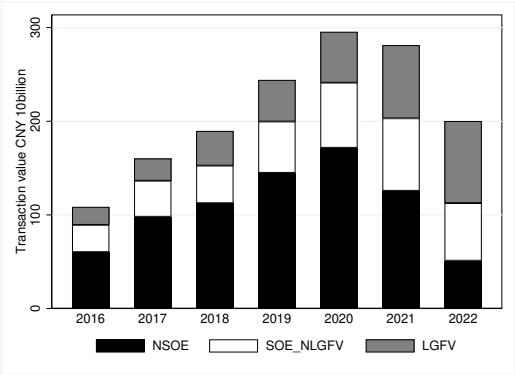
(b) All cities: Transaction value fraction



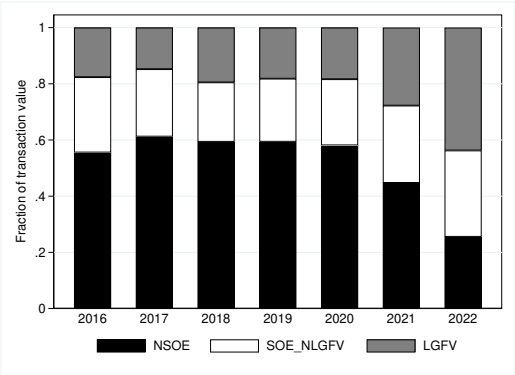
(c) High land sales dependence on non-SOEs: Transaction value



(d) High land sales dependence on non-SOEs: Transaction value fraction



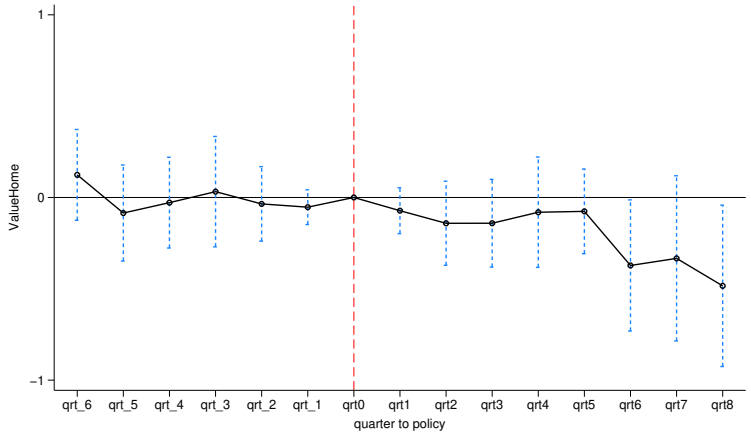
(e) Low land sales dependence on non-SOEs: Transaction value



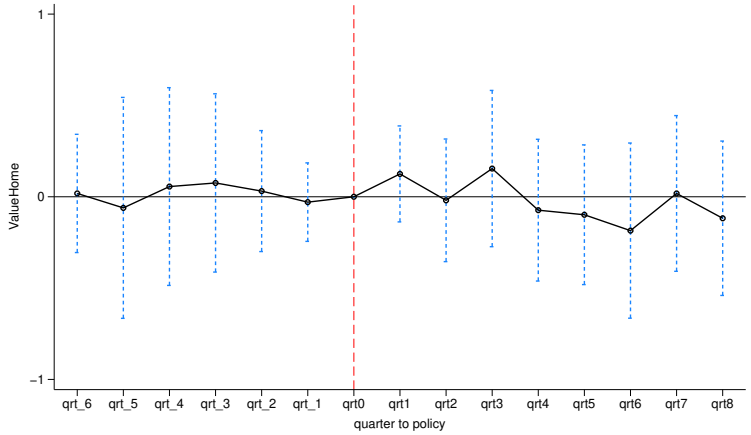
(f) Low land sales dependence on non-SOEs: Transaction value fraction

Figure 4: Land purchases by different groups of developers on the market

This figure shows the value (panels (a), (c) and (e)) and value fraction (panels (b), (d) and (f)) of land purchases by different groups of developers on the whole market, i.e., non-SOE, non-LGFV SOE and LGFV developers. Panels (a)-(b) are based on statistics calculated for all cities, panels (c)-(d) are based on statistics calculated for the cities with high land sales dependence on non-SOE developers, and panels (e)-(f) are based on statistics calculated for the cities with low land sales dependence on non-SOE developers.



(a) Non-SOE developers



(b) SOE developers

Figure 5: Dynamic effects of the impact of the TRL policy on developers' home sales value

This figure assesses the parallel trends assumption prior to the introduction of TRL policy. Dynamic effects of the TRL policy are estimated when the dependent variable is $Value_{Home}$, corresponding to the specification presented in column (8) of Table 11. Panels (a) and (b) plots estimated coefficients and 99% confidence intervals based on the non-SOE and SOE developer sample, respectively. The vertical axis displays coefficient estimates, and the horizontal axis indicates event quarter relative to the TRL introduction quarter.

Table 1: Sample construction

Panel A outlines the step-by-step construction of the final developer-level sample from the initial universe of all developers covered by CREIS between 2016 and 2022. Panel B reports the composition of the developer sample partitioned by the number of red lines violated during the pre-TRL period.

Panel A. Sample construction				
	# Developers	% Land purchase on whole market	# Publicly listed	# Unlisted
Sample from CREIS	2,801	59%	246	2,555
After excl. developers with no financial information	1,323	47%	244	1,079
After excl. developers with no obs. in pre- or post-TRL period	1,203	46%	237	966
After excl. developers with no land purchase activities	861	46%	139	722
Panel B. By # red lines violated				
	# Developers	# Non-SOE	# Non-LGFV SOE	# LGFV
No violation	440	42	47	351
1 red line violation	289	55	29	205
2 red lines violation	101	13	19	69
3 red lines violation	31	12	8	11
Total	861	122	103	636
% Land purchase value in the sample	100%	52%	23%	25%
#Obs.	24,107	3,415	2,884	17,808
#Obs. with control variables	10,530	2,369	1,496	6,665

Table 2: Summary statistics

Panel A reports summary statistics for developer-level variables for the full sample. Panel B and C provides summary statistics for non-SOE and SOE developers, respectively. Variable definitions are provided in Table A1.

	Mean	Median	Std.Dev.	10prct	90prct	Obs
Panel A. All developers						
Treat	0.489	0.000	0.500	0.000	1.000	24,107
Purchase	0.262	0.000	0.440	0.000	1.000	24,107
Parcel	0.888	0.000	2.366	0.000	3.000	24,107
Area (Mil m^2)	0.441	0.000	1.238	0.000	1.301	24,107
Value (10Mil CNY)	43.845	0.000	142.881	0.000	105.866	24,107
Asset (Bil CNY)	130.613	41.699	490.741	12.094	241.061	17,512
ROA (%)	1.328	0.832	1.657	0.050	3.340	16,786
Netprofitmargin	0.103	0.095	0.237	0.009	0.261	14,967
Salegrowth	0.572	0.415	1.490	-0.759	1.537	16,648
Redline1 (%)	60.386	61.710	13.581	42.240	76.730	17,036
Redline2 (%)	85.526	72.580	69.540	16.726	162.490	17,129
Redline3	2.936	0.910	10.595	0.230	3.910	16,920
Panel B. Non-SOE developers						
Treat	0.656	1.000	0.475	0.000	1.000	3,415
Purchase	0.419	0.000	0.494	0.000	1.000	3,415
Parcel	2.239	0.000	4.071	0.000	9.000	3,415
Area (Mil m^2)	1.196	0.000	2.218	0.000	4.994	3,415
Value (10Mil CNY)	140.896	0.000	263.897	0.000	582.773	3,415
Asset (Bil CNY)	266.478	80.224	825.306	13.748	453.631	3,111
ROA (%)	2.513	2.180	2.476	0.020	5.810	2,990
Netprofitmargin	0.050	0.085	0.269	-0.098	0.211	2,688
Salegrowth	0.456	0.402	1.140	-0.751	1.422	2,991
Redline1 (%)	67.979	70.190	13.983	49.380	83.445	3,043
Redline2 (%)	100.473	79.065	87.614	9.895	206.110	3,048
Redline3	3.385	0.980	13.349	0.202	3.244	3,010
Panel C. SOE developers						
Treat	0.461	0.000	0.499	0.000	1.000	20,692
Purchase	0.236	0.000	0.424	0.000	1.000	20,692
Parcel	0.665	0.000	1.853	0.000	2.000	20,692
Area(Mil m^2)	0.316	0.000	0.930	0.000	0.983	20,692
Value (10Mil CNY)	27.828	0.000	102.384	0.000	65.243	20,692
Asset (Bil CNY)	101.262	37.676	375.368	11.882	185.895	14,401
ROA (%)	1.071	0.700	1.282	0.055	2.600	13,796
Netprofitmargin	0.115	0.097	0.228	0.012	0.270	12,279
Salegrowth	0.598	0.419	1.555	-0.761	1.556	13,657
Redline1 (%)	58.735	60.220	12.915	41.360	73.279	13,993
Redline2 (%)	82.290	70.960	64.518	17.520	153.061	14,081
Redline3	2.839	0.899	9.897	0.240	4.080	13,910

Table 3: Impact of TRL policy on land purchases

This table reports the difference-in-differences estimation results examining the effects of the TRL policy on developers' land purchase activities. Panel A presents the estimation results based on the full sample, Panels B and C present the results based on non-SOE and SOE developers, respectively. Columns (1)-(4) and (5)-(8) report the results before and after controlling for firm-level characteristics, respectively. *Purchase* is an indicator for a developer purchasing at least one land parcel in a calendar quarter. *Parcel*, *Area* and *Value* is the number, total area and value of land parcels purchased over a quarter. *Treat* is an indicator for the firms that violated at least one red line during two years prior to the Three Red Line Policy. *Post* is an indicator for the post-policy period, since Q4 of 2020. Control variables include the one-quarter lag for: the natural logarithm of total assets of a developer in a given quarter (*LnAsset*), quarterly return on assets ratio (*ROA*) , quarterly net profit margin (*Netprofitmargin*), and quarterly growth rate of sales(*Salegrowth*). Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Model:	OLS	Poisson			OLS	Poisson		
Dependent variable:	*Purchase*	*Parcel*	*Area*	*Value*	*Purchase*	*Parcel*	*Area*	*Value*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. All developers								
Treat×*Post*	-0.065***	-0.526***	-0.536***	-0.560***	-0.089***	-0.617***	-0.638***	-0.576***
	(0.016)	(0.099)	(0.104)	(0.120)	(0.022)	(0.113)	(0.112)	(0.121)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	24,107	24,107	24,107	24,107	10,530	10,314	10,314	10,314
Adjusted R^2	0.257				0.290			
Pseudo R^2		0.422	0.531	0.606		0.485	0.602	0.626
Panel B. Non-SOE developers								
Treat×*Post*	-0.111**	-0.662***	-0.587***	-0.533***	-0.131***	-0.655***	-0.617***	-0.516***
	(0.043)	(0.140)	(0.137)	(0.154)	(0.043)	(0.133)	(0.132)	(0.151)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	3,415	3,415	3,415	3,415	2,369	2,313	2,313	2,313
Adjusted R^2	0.421				0.464			
Pseudo R^2		0.592	0.713	0.682		0.609	0.747	0.718
Panel C. SOE developers								
Treat×*Post*	-0.024*	-0.125	-0.147	-0.209	-0.023	-0.214	-0.287*	-0.250*
	(0.014)	(0.108)	(0.118)	(0.151)	(0.022)	(0.151)	(0.151)	(0.148)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	20,692	20,692	20,692	20,692	8,161	8,001	8,001	8,001
Adjusted R^2	0.229				0.239			
Pseudo R^2		0.339	0.432	0.533		0.373	0.479	0.558
P-value for test on Diff. in *Treat* × *Post* between Panels B and C	0.010	0.002	0.012	0.084	0.064	0.016	0.082	0.230

Table 4: Impact of TRL policy on land purchases: stringency heterogeneity

This table reports the difference-in-differences estimation results examining the heterogeneous effects of the TRL policy with different caps on debt growth rates on developers' land purchase activities. *Purchase* is an indicator for a developer purchasing at least one land parcel in a calendar quarter. *Parcel*, *Area*, and *Value* denote the number, total area, and value of land parcels purchased in a quarter. *Treat*1 (*Treat*2 and *Treat*3) indicates firms that violated one (two or three) red line(s) during the two years prior to the Three Red Lines policy. *Post* is an indicator for the post-policy period (since Q4 2020). Control variables include the one-quarter lag of the natural logarithm of total assets (*LnAsset*), return on assets (*ROA*), net profit margin (*Netprofitmargin*), and sales growth (*Salegrowth*). Standard errors are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Model:	OLS	Poisson		
Dependent variable:	*Purchase*	*Parcel*	*Area*	*Value*
	(1)	(2)	(3)	(4)
Treat1×*Post*	-0.077***	-0.531***	-0.543***	-0.481***
	(0.027)	(0.125)	(0.124)	(0.124)
Treat2×*Post*	-0.090**	-0.625***	-0.672***	-0.543**
	(0.036)	(0.202)	(0.196)	(0.233)
Treat3×*Post*	-0.176***	-1.206***	-1.200***	-1.418***
	(0.052)	(0.215)	(0.218)	(0.257)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	10,530	10,314	10,314	10,314
Adjusted R^2	0.291			
Pseudo R^2		0.486	0.603	0.629

Table 5: Impact of TRL policy on land purchases: debt term structure heterogeneity

This table reports the difference-in-differences estimation results examining the heterogeneous effects of the TRL policy on the land purchase activities of developers with different pre-TRL debt term structures. Panels A and B present estimation results for subsamples with above-median and below-median long-term debt ratios (long-term interest-bearing debt / total interest-bearing debt), respectively, measured over the two years prior to the TRL policy. *Purchase* is an indicator for a developer purchasing at least one land parcel in a calendar quarter. *Parcel*, *Area* and *Value* is the number, total area and value of land parcels purchased over a quarter. *Treat* is an indicator for the firms that violated at least one red line during two years prior to the Three Red Line Policy. *Post* is an indicator for the post-policy period, since Q4 of 2020. Control variables include the one-quarter lag for: the natural logarithm of total assets of a developer in a given quarter (*LnAsset*), quarterly return on assets ratio (*ROA*) , quarterly net profit margin (*Netprofitmargin*), and quarterly growth rate of sales(*Salegrowth*). Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Model:	OLS	Poisson		
Dependent variable:	*Purchase*	*Parcel*	*Area*	*Value*
	(1)	(2)	(3)	(4)
Panel A. Developers with high reliance on long-term debts				
Treat×*Post*	-0.072*	-0.464***	-0.606***	-0.435**
	(0.040)	(0.169)	(0.173)	(0.176)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	3,978	3,860	3,860	3,860
Adjusted R^2	0.281			
Pseudo R^2		0.416	0.540	0.627
Panel B. Developers with low reliance on long-term debts				
Treat×*Post*	-0.033	-0.409**	-0.301*	-0.297
	(0.038)	(0.175)	(0.170)	(0.192)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	4,086	3,966	3,966	3,966
Adjusted R^2	0.276			
Pseudo R^2		0.531	0.646	0.620
P-value for test on Diff. in *Treat* × *Post* between Panels B and C	0.180	0.368	0.018	0.174

Table 6: Impact of TRL policy on LGFVs' land purchases: developer-level analysis

This table examines the impact of TRL policy on LGFVs' land purchase activities. *Purchase* is an indicator for a developer purchasing at least one land parcel in a calendar quarter. *Parcel*, *Area* and *Value* is the number, total area and value of land parcels purchased over a quarter. *LGFV* is an indicator for LGFVs. *Post* is an indicator for the post-policy period, since Q4 of 2020. Control variables include the one-quarter lag for the natural logarithm of total assets of a developer in a given quarter (*LnAsset*), quarterly return on assets ratio (*ROA*) , quarterly net profit margin (*Netprofitmargin*), and quarterly growth rate of sales(*Salegrowth*).Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Model:	OLS	Poisson		
Dependent variable:	*Purchase*	*Parcel*	*Area*	*Value*
	(1)	(2)	(3)	(4)
LGFV × *Post*	0.100***	0.389**	0.480***	0.657***
	(0.029)	(0.170)	(0.171)	(0.143)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	8,161	8,001	8,001	8,001
Adjusted R^2	0.241			
Pseudo R^2		0.375	0.482	0.563

Table 7: Impact of TRL policy on LGFVs' land purchases: land-level analysis

This table examines the impact of the TRL policy on LGFVs' probability of acquiring land parcels in local cities. Columns (1)-(2) are based on all cities, column (3) and (4) is based on the cities with *NSOElanddep* is above and below the sample median, where *NSOElanddep* indicates a city's fiscal reliance on non-SOE developers' residential land purchases. *Samecity* is an indicator for the land purchaser being registered in the same city of the purchased land parcel. *Landprice* is the price of the land transacted. Definitions of other variables are presented in Table A1 in Appendix A. Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Dependent variable:	*LGFV*			
City group:	All	All	High *NSOElanddep*	Low *NSOElanddep*
	(1)	(2)	(3)	(4)
Post	0.057***			
	(0.012)			
Samecity	0.618***	0.528***	0.593***	0.480***
	(0.049)	(0.060)	(0.084)	(0.084)
Post×Samecity		0.077***	0.095*	0.091**
		(0.029)	(0.052)	(0.037)
LnAsset	-0.043***	-0.039***	-0.028***	-0.047***
	(0.008)	(0.010)	(0.009)	(0.018)
ROA	-0.030***	-0.054***	-0.040***	-0.064***
	(0.004)	(0.006)	(0.010)	(0.007)
Netprofitmargin	0.132**	0.233***	0.043	0.315***
	(0.053)	(0.068)	(0.094)	(0.094)
Salegrowth	0.011***	0.009**	0.016**	0.006
	(0.003)	(0.004)	(0.007)	(0.004)
Landprice	-0.023***	-0.017***	0.005	-0.017***
	(0.006)	(0.006)	(0.009)	(0.006)
City FE	Yes	No	No	No
City×Quarter FE	No	Yes	Yes	Yes
Clustering	City	City	City	City
Observations	15,911	15,087	6,499	6,952
Adjusted R^2	0.735	0.793	0.797	0.795

Table 8: Impact of TRL policy on access to debts: developers vs. non-developer firms

This table reports the results examining the impact of the TRL policy on developers' access to debts. Given data availability, the analysis is based on listed firms. The dependent variable is *LoantoAsset*, the loan to asset ratio in the first three columns, and *Ln*(*Bond*), the logarithm of one plus the amount of bonds payable in the last three columns. *Developer* is an indicator for developer firms. *Post* is an indicator for the post-policy period, since Q4 of 2020. Control variables include firms' Tobin's Q (*TobinQ*), return on assets (*ROA*), sales growth rate (*Salegrowth*), net profit margin ($Netprofitmargin$) and the logarithm of a firm's total assets (*LnAsset*). Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Dependent variable:	*LoantoAsset*			*Ln(Bond)*		
Sample:	All	Non-SOE	SOE	All	Non-SOE	SOE
	(1)	(2)	(3)	(4)	(5)	(6)
Developer×*Post*	-0.027***	-0.044***	-0.009	-0.108**	-0.238***	0.010
	(0.006)	(0.009)	(0.008)	(0.046)	(0.076)	(0.049)
TobinQ	0.001	-0.000	0.003	0.003	0.002	0.012**
	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)	(0.006)
ROA	-0.258***	-0.227***	-0.335***	-0.346***	-0.367***	-0.328***
	(0.017)	(0.020)	(0.037)	(0.065)	(0.083)	(0.100)
Salegrowth	-0.000	0.001	-0.002	-0.014***	-0.013***	-0.018***
	(0.001)	(0.001)	(0.002)	(0.004)	(0.005)	(0.007)
Netprofitmargin	-0.012**	-0.007	-0.020**	0.048**	0.063**	0.015
	(0.005)	(0.006)	(0.008)	(0.019)	(0.026)	(0.019)
LnAsset	0.031***	0.027***	0.037***	0.187***	0.174***	0.214***
	(0.004)	(0.004)	(0.007)	(0.013)	(0.016)	(0.026)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Semi-annual FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	34,433	21,499	12,934	34,433	21,499	12,934
Adjusted R^2	0.764	0.726	0.804	0.843	0.745	0.880

Table 9: Impact of TRL policy on access to debts: treated vs. control groups of developers

This table reports the results comparing the impact of the TRL policy on the access to debts between the treated and control groups of developers. Given data availability, the analysis is based on listed developers. Panel A presents the results when the dependent variable is *LoantoAsset*, the loan to asset ratio. Panel B presents the results when the dependent variable is *Ln*(*Bond*), the logarithm of one plus the amount of bonds payable. *Treat* is an indicator for developers from the treated group, i.e., that violated at least one red line during the pre-TRL period. *Post* is an indicator for the post-policy period, since Q4 of 2020. *SOE* is an indicator for state-owned developers, and *LGFV* is an indicator for LGFVs. Control variables include firms' Tobin's Q (*TobinQ*), return on assets (*ROA*), sales growth rate (*Salegrowth*), net profit margin (*Netprofitmargin*) and the logarithm of a firm's total assets (*LnAsset*). Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Panel A. Impact on access to bank loans				
Dependent variable:	*LoantoAsset*			
Sample:	All	Non-SOE	SOE	All
	(1)	(2)	(3)	(4)
Treat×*Post*	-0.023**	-0.026*	-0.015	
	(0.011)	(0.015)	(0.015)	
SOE× *Post*				0.022**
				(0.011)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Semi-annual FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	1,754	835	919	1,754
Adjusted R^2	0.678	0.664	0.700	0.678
Panel B. Impact on access to bonds				
Dependent variable:	*Ln(Bond)*			
Sample:	All	Non-SOE	SOE	All
	(1)	(2)	(3)	(4)
Treat×*Post*	-0.168**	-0.193	-0.118	
	(0.078)	(0.124)	(0.085)	
SOE×*Post*				0.217**
				(0.084)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Semi-annual FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	1,754	835	919	1,754
Adjusted R^2	0.874	0.860	0.898	0.875

Table 10: Impact of TRL policy on costs of debts

This table reports the results examining the impact of the TRL policy on cost of debts. The first three columns report the results based on the bonds issued by all listed firms and the last column reports the results based on the bonds issued by the listed developers. The dependent variable *Yield* is the yield at issuance of a bond. *Developer* is an indicator for developer firms. *SOE* is an indicator for state-owned firms. *Post* is an indicator for the post-policy period, since Q4 of 2020. Variable definitions are presented in Table A1 in Appendix A. Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Dependent variable:	*Yield*		
Sample:	All		Developers
Sub-sample:	Non-SOE	SOE	All
	(1)	(2)	(3)
Developer× *Post*	0.198*	0.006	
	(0.113)	(0.048)	
SOE× *Post*			-0.177*
			(0.094)
Issuance amount	-0.001	0.007***	0.030
	(0.008)	(0.003)	(0.025)
Maturity	0.165***	0.138***	0.151***
	(0.026)	(0.011)	(0.021)
Rating	0.073	-0.052	0.018
	(0.129)	(0.067)	(0.076)
TreasuryYield	0.062	0.307***	0.224***
	(0.094)	(0.030)	(0.056)
Leverage	-0.005	0.005	-0.004
	(0.006)	(0.004)	(0.006)
Salegrowth	-0.025	-0.019	0.012
	(0.055)	(0.031)	(0.030)
Firm FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Bond Market FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
Observations	1,271	5,959	1,720
Adjusted R^2	0.909	0.876	0.904

Table 11: Impact of TRL policy on home sales

This table reports the difference-in-differences estimation results examining the effects of the TRL policy on developers' home sales. Poisson regressions are used for the estimation. Panel A presents the estimation results based on the non-SOE developer sample, and Panel B presents the results based on SOE developers. Columns (1)-(4) and (5)-(8) present the results before and after controlling for firm characteristics, respectively. Num_{Home} is the total number houses sold by a developer in a calendar quarter. $Area_{Home}$ is the total area of the houses sold by a developer in a calendar quarter (in m^2). $Value_{Home}$ is the total value amount (in 10 thousand CNY) of the houses sold by a developer in a calendar quarter. $Price_{Home}$ is the average price per m^2 of the houses sold by a developer in a calendar quarter(in CNY). *Treat* is an indicator for the firms that violated at least one red line during the pre-TRL period. *Post* is an indicator for the post-policy period, since Q4 of 2020. Control variables include the one-quarter lag for: the logarithm of total assets of a developer in a given quarter (*LnAsset*), quarterly return on assets ratio (*ROA*) , quarterly net profit margin ($Netprofitmargin$), and quarterly growth rate of sales(*Salegrowth*). Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Dependent variable:	Num_{Home} (1)	$Area_{Home}$ (2)	$Value_{Home}$ (3)	$Price_{Home}$ (4)	Num_{Home} (5)	$Area_{Home}$ (6)	$Value_{Home}$ (7)	$Price_{Home}$ (8)
Panel A. Impact on Non-SOE developers								
Treat × *Post*	-0.192***	-0.177***	-0.263***	-0.074	-0.179***	-0.160***	-0.197**	-0.038
	(0.056)	(0.058)	(0.082)	(0.047)	(0.057)	(0.058)	(0.082)	(0.047)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	4,367	4,367	4,367	3,985	3,066	3,066	3,066	2,851
Pseudo R^2	0.947	0.950	0.948	0.640	0.963	0.965	0.961	0.688
Panel B. Impact on SOE developers								
Treat × *Post*	-0.181	-0.172	-0.194	-0.003	0.057	0.048	-0.016	-0.074
	(0.169)	(0.166)	(0.149)	(0.044)	(0.124)	(0.122)	(0.098)	(0.052)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	9,287	9,287	9,287	7,188	4,364	4,364	4,364	3,429
Pseudo R^2	0.866	0.874	0.893	0.634	0.917	0.923	0.929	0.664
P-value for test on Diff. in *Treat* × *Post*	0.432	0.494	0.098	0.008	0.000	0.006	0.012	0.126

Table 12: Impact of TRL policy on bank risk

This table reports the difference-in-differences estimation results examining the effects of the TRL policy on commercial banks' risk. The dependent variable is *LoanExpREsector* (a bank's loan exposure to the real estate sector, i.e., the ratio of a bank's outstanding loans to the real estate sector to its total outstanding loans) in columns (1)-(2), *Zscore* (with higher values indicating lower bank insolvency risk) in columns (3)-(4) and is *LLPTL* (the ratio of loan loss provision to loan assets) in columns (5)-(6). $Treat_{bank}$ is an indicator for banks from the treated group, i.e. for which the loans offered to the real estate sector accounts for over 10% of the total loans offered prior to TRL policy introduction. *Post* is an indicator for the post-policy period, after 2020. *Year*2021 and *Year*2022 is an indicator for year 2021 and 2022, respectively. Control variables include the one-year lag of *LnSize*, the natural logarithm of total assets of a bank, $Liquidity$, the ratio of total loans to total assets of a bank, $Diversification$, the ratio of non-interest revenues to total revenues, and $Netprofitmargin$, the net profit margin of a bank. Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Dependent variable:	*LoanExpREsector*		*Zscore*		*LLPTL*	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{bank} \times Post$	-3.350***		-3.499		-0.034	
	(0.501)		(3.337)		(0.137)	
$Treat_{bank} \times Year2021$		-2.920***		0.718		-0.065
		(0.505)		(3.138)		(0.136)
$Treat_{bank} \times Year2022$		-3.788***		-8.073*		-0.003
		(0.528)		(4.477)		(0.150)
LnSize	-0.389	-0.368	2.419	2.633	-0.087	-0.089
	(0.694)	(0.700)	(5.015)	(5.034)	(0.208)	(0.209)
$Liquidity$	-1.715	-1.691	30.847*	32.053*	-1.912**	-1.919**
	(2.171)	(2.162)	(16.953)	(17.059)	(0.841)	(0.843)
$Diversification$	-0.025	-0.043	-2.931	-2.856	0.421*	0.420*
	(0.679)	(0.683)	(4.877)	(4.891)	(0.227)	(0.228)
$Netprofitmargin$	0.329	0.381	20.863**	21.080**	-0.158	-0.160
	(1.423)	(1.417)	(9.029)	(9.038)	(0.482)	(0.482)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Bank	Bank	Bank	Bank	Bank	Bank
Observations	1,174	1,174	1,566	1,566	1,342	1,342
Adjusted R^2	0.884	0.884	0.206	0.207	0.825	0.825

Appendix A

Table A1: Variable definition

Variable	Definition
Treat	An indicator for the developers that violated at least one red line during the period over two years prior to the TRL policy.
Treat1	An indicator for the developers that violated one red line during the period over two years prior to the TRL policy.
Treat2	An indicator for the developers that violated two red lines during the period over two years prior to the TRL policy.
Treat3	An indicator for the developers that violated three red lines during the period over two years prior to the TRL policy.
Post	An indicator for the post-policy period, i.e. 2020Q4-2022Q4 for Tables 2-11, and years 2021 and 2022 for Table 12.
LGFV	An indicator for LGFV developers.
High NSOElanddep	An indicator for cities of which the fiscal reliance on non-SOE developers residential land purchases (*NSOElanddep*) is greater than the median, where *NSOElanddep* is calculated as the ratio of the total purchase value of residential lands in a city to the sum of general public budget revenue and land sales revenues in that city during the pre-policy period, i.e. 2016-2019.
Samecity	An indicator for a developer and the land parcel it purchases being located in a same city.
Landprice	The inflation-adjusted price of a land parcel (2012 as the base year).
Purchase	An indicator for a developer purchasing at lease one land parcel in a given quarter.
Parcel	The number of land parcels a developer purchases in a given quarter.
Area	The total area of the land parcels a developer purchases in a given quarter.
Value	The total CNY value of the land parcels a developer purchases in a given quarter.
Asset	The total assets of a developer as at the end of a given quarter.
Num_{Home}	The total number of homes transacted across all buildings located in the sample cities for a developer over a calendar quarter.
Area_{Home}	The total area of homes transacted across all buildings located in the sample cities for a developer over a calendar quarter.
Value_{Home}	The total CNY amount of homes transacted across all buildings located in the sample cities for a developer over a calendar quarter.
Price_{Home}	The average price per square meter across homes transacted for all buildings located in the sample cities for a developer over a calendar quarter.
ROA	The return on assets of a developer as at the end of a given quarter.
Netprofitmargin	The net profit margin of a developer as at the end of a given quarter.
Salegrowth	The growth rate of sales of a developer as at the end of a given quarter.

Variable	Definition
Redline1	The debt to asset ratio after excluding advance receipts of a developer as at the end of a given quarter, calculated by: (Debt − advance receipts)/(Asset − advance receipts).
Redline2	The net gearing ratio of a developer as at the end of a given quarter, calculated by: (Interest-bearing Debt − Cash)/Equity.
Redline3	The cash to short-term debt ratio of a developer as at the end of a given quarter, calculated by: Cash/Short-term Interest-bearing Debt.
Yield	The yield at issuance of a bond.
Issuance amount	The bond issuance amount in CNY.
Maturity	The maturity of a bond.
Rating	The rating of a bond issuer, with the value 1 of 5 indicating the rating of A+, AA-, AA, AA+ and AAA.
TreasuryYield	The yield of treasury bonds with maturity matched with individual bonds.
Developer	An indicator for developer firms.
SOE	An indicator for SOE developers/non-developer firms.
TobinQ	A publicly listed firm's Tobin's Q as at the end of a quarter.
Leverage	A firm's debt-to-asset ratio at the end of a quarter.
LoantoAsset	The ratio of the amount of bank loans to the total asset of a publicly listed firm as at the end of June and December of a year.
Bond	The CNY amount of bonds payable of a publicly listed firm as at the end of June and December of a year.
Size	The total assets of a bank as at the end of a year.
Treat_{bank}	An indicator for banks from the treated group, i.e. for which the loans offered to the real estate sector accounts for over 10% of the total loans offered during 2016-2020.
Liquidity	The ratio of total loans to total assets of a bank as at the end of a year.
Diversification	The ratio of non-interest revenues to total revenues of a bank as at the end of a year.
LoanExpREsector	A bank's loan exposure to the real estate sector, i.e., the ratio of a bank's outstanding loans to the real estate sector to its total outstanding loans.
Z-score	The ratio of a bank's return on assets plus equity-to-asset ratio to the standard deviation of the return on assets, following Tan (2016).
LLPTL	The ratio of loan loss provision to loans of a bank as at the end of a year.

Appendix B

Table B2: Impact of TRL policy on land purchases: Alternative dependent variables

This table reports the difference-in-differences estimation results examining the effects of the TRL policy on developers' land purchase activities. Panel A presents the estimation results based on the full sample, Panels B and C present the results based on non-SOE and SOE developers, respectively. Columns (1)-(3) and (4)-(6) report the results before and after controlling for firm-level characteristics, respectively. *Ln*(1 + *Parcel*), *Ln*(1 + *Area*) and *Ln*(1 + *Value*) is the natural logarithm of one plus the number, total area and value of land parcels purchased over a quarter. *Treat* is an indicator for the firms that violated at least one red line during two years prior to the Three Red Line Policy. *Post* is an indicator for the post-policy period, since Q4 of 2020. Control variables include the one-quarter lag for: the natural logarithm of total assets of a developer in a given quarter (*LnAsset*), quarterly return on assets ratio (*ROA*) , quarterly net profit margin (*Netprofitmargin*), and quarterly growth rate of sales(*Salegrowth*). Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Dependent variable:	*Ln(1+Parcel)* (1)	*Ln(1+Area)* (2)	*Lm(1+Value)* (3)	*Ln(1+Parcel)* (4)	*Ln(1+Area)* (5)	*Lm(1+Value)* (6)
Panel A. All developers						
Treat×*Post*	-0.116***	-0.813***	-0.782***	-0.189***	-1.176***	-1.156***
	(0.024)	(0.186)	(0.183)	(0.038)	(0.268)	(0.268)
Controls	No	No	No	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	24,107	24,107	24,107	10,530	10,530	10,530
Adjusted R^2	0.398	0.285	0.299	0.477	0.328	0.339
Panel B. Non-SOE developers						
Treat×*Post*	-0.322***	-1.566***	-1.546***	-0.402***	-1.879***	-1.859***
	(0.074)	(0.517)	(0.519)	(0.082)	(0.516)	(0.521)
Controls	No	No	No	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	3,415	3,415	3,415	2,369	2,369	2,369
Adjusted R^2	0.629	0.472	0.476	0.657	0.517	0.519
Panel C. SOE developers						
Treat×*Post*	-0.031	-0.283*	-0.248	-0.038	-0.310	-0.276
	(0.020)	(0.164)	(0.159)	(0.031)	(0.251)	(0.248)
Controls	No	No	No	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	20,692	20,692	20,692	8,161	8,161	8,161
Adjusted R^2	0.301	0.246	0.255	0.352	0.261	0.274
P-value for test on Diff. in *Treat* × *Post* between Panels B and C	0.000	0.000	0.000	0.000	0.024	0.028

Table B3: Land use efficiency following TRL policy

This table compares the land use efficiency between different groups of developers following the TRL policy. The dependent variable in columns (1) and (3) is a developer's ratio of revenues to the total area of lands acquired in a quarter (*Revenue/land area*). The dependent variable in columns (2) and (4) is a developer's ratio of revenues to the total value of lands acquired in a quarter (*Revenue/land value*). Columns (1)-(2) are based on all developers, columns (3)-(4) are based on SOE developers. *SOE* is an indicator for SOE developers. *LGFV* is an indicator for LGFVs. *Post* is an indicator for the post-TRL period, i.e., since 2020 Q4. Control variables include the one-quarter lag for the natural logarithm of total assets of a developer in a given quarter (*LnAsset*), quarterly return on assets ratio (*ROA*) , quarterly net profit margin (*Netprofitmargin*), and quarterly growth rate of sales(*Salegrowth*).Standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

Sample:	All developers		SOE developers	
	(1)	(2)	(3)	(4)
Dependent variable:	*Revenue/land area*	*Revenue/land value*	*Revenue/land area*	*Revenue/land value*
SOE × *Post*	-0.005***	-0.009***		
	(0.001)	(0.003)		
LGVF × *Post*			-0.004***	-0.004
			(0.001)	(0.003)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	3,147	3,147	2,112	2,112
Adjusted R^2	0.382	0.316	0.365	0.299

Table B4: Summary statistics for access to loans and bond

This table reports summary statistics of variables related to firms' access to debts and firm characteristics. Panels A-F present summary statistics based on all listed firms, non-SOE listed firms, SOE listed firms, all listed developers, listed non-SOE developers and listed SOE developers, respectively. Variable definitions are provided in Table A1 in Appendix A.

	Mean	Median	Std.Dev.	10prct	90prct	Obs
Panel A. All listed firms						
LoantoAsset	0.162	0.143	0.120	0.021	0.327	34,433
Bond (Bil CNY)	0.870	0.000	3.555	0.000	1.317	34,433
Developer	0.051	0.000	0.220	0.000	0.000	34,433
Asset (Bil CNY)	21.523	5.457	54.128	1.380	42.432	34,433
TobinQ	2.007	1.637	1.226	1.038	3.360	34,433
ROA	0.020	0.021	0.057	-0.018	0.073	34,433
Netprofitmargin	0.037	0.059	0.250	-0.081	0.203	34,433
Salegrowth	0.471	0.815	0.971	-0.587	1.541	34,433
#Firms (#Developers)			3,146 (143)			
Panel B. Non-SOE listed firms						
LoantoAsset	0.151	0.134	0.109	0.022	0.301	21,499
Bond (Bil CNY)	0.401	0.000	2.175	0.000	0.557	21,499
Developer	0.039	0.000	0.193	0.000	0.000	21,499
Asset (Bil CNY)	12.747	4.029	36.733	1.197	21.301	21,499
TobinQ	2.189	1.798	1.292	1.152	3.642	21,499
ROA	0.021	0.024	0.063	-0.022	0.079	21,499
Netprofitmargin	0.031	0.066	0.265	-0.097	0.199	21,499
Salegrowth	0.468	0.810	0.958	-0.587	1.537	21,499
#Firms (#Developers)			2,062 (68)			
Panel C. SOE listed firms						
LoantoAsset	0.181	0.160	0.133	0.020	0.371	12,934
Bond (Bil CNY)	1.649	0.000	4.980	0.000	3.500	12,934
Developer	0.071	0.000	0.257	0.000	0.000	12,934
Asset (Bil CNY)	36.109	10.364	72.225	2.089	83.521	12,934
TobinQ	1.705	1.360	1.039	0.968	2.795	12,934
ROA	0.018	0.017	0.047	-0.012	0.061	12,934
Netprofitmargin	0.046	0.050	0.224	-0.054	0.214	12,934
Salegrowth	0.475	0.825	0.992	-0.586	1.548	12,934
#Firms (#Developers)			1,084 (75)			
Panel D. All listed developers						
LoantoAsset	0.187	0.179	0.103	0.059	0.320	1,754
Bond (Bil CNY)	4.792	0.991	7.821	0.000	18.608	1,754
Developer	1.000	1.000	0.000	1.000	1.000	1,754
Asset (Bil CNY)	85.058	30.379	114.931	5.101	324.540	1,754
TobinQ	1.268	1.054	0.672	0.889	1.757	1,754
ROA	0.013	0.012	0.036	-0.009	0.044	1,754
Netprofitmargin	0.046	0.073	0.295	-0.116	0.283	1,754
Salegrowth	0.616	0.613	1.259	-0.705	2.406	1,754
#Developers (#Treated developers)			143 (72)			
Panel E. Listed Non-SOE developers						
LoantoAsset	0.174	0.160	0.101	0.059	0.313	835
Bond (Bil CNY)	4.890	0.000	8.351	0.000	21.246	835
Developer	1.000	1.000	0.000	1.000	1.000	835
Asset (Bil CNY)	94.809	32.842	120.121	4.838	339.223	835
TobinQ	1.393	1.087	0.878	0.907	2.120	835
ROA	0.012	0.013	0.043	-0.020	0.052	835
Netprofitmargin	0.019	0.069	0.336	-0.213	0.262	835
Salegrowth	0.597	0.599	1.230	-0.688	2.317	835
#Developers (#Treated developers)			68 (35)			
Panel F. Listed SOE developers						
LoantoAsset	0.198	0.194	0.103	0.058	0.322	919
Bond (Bil CNY)	4.703	1.684	7.309	0.000	14.795	919
Developer	1.000	1.000	0.000	1.000	1.000	919
Asset (Bil CNY)	76.197	29.852	109.317	5.745	318.853	919
TobinQ	1.155	1.030	0.367	0.880	1.585	919
ROA	0.013	0.012	0.029	-0.003	0.040	919
Netprofitmargin	0.071	0.074	0.249	-0.060	0.302	919
Salegrowth	0.633	0.647	1.285	-0.722	2.580	919
#Developers (#Treated developers)			75 (37)			

Table B5: Summary statistics for bond issuance

This table reports summary statistics of the characteristics of bonds and bond issuers. For the pre- and post-TRL policy comparison, we restrict the sample to firms that issue bonds in both periods. Panels A-F present summary statistics based on all listed firms, non-SOE listed firms, SOE listed firms, all listed developers, listed non-SOE and listed SOE developers, respectively. Variable definitions are provided in Table A1 in Appendix A.

	Mean	Median	Std.Dev.	10prct	90prct	Obs
Panel A. All listed firms						
Yield (%)	3.312	3.030	1.248	2.000	5.000	7,230
Developer	0.238	0.000	0.426	0.000	1.000	7,230
Issuance amount (Bil CNY)	2.007	1.000	3.303	0.500	4.000	7,230
Maturity (year)	1.684	0.750	1.826	0.240	5.000	7,230
Rating	4.715	5.000	0.564	4.000	5.000	7,230
TreasuryYield	2.362	2.290	0.556	1.715	3.190	7,230
Leverage	66.548	69.373	13.431	46.471	81.013	7,230
Salegrowth	0.425	0.513	0.773	-0.771	1.232	7,230
#Firms			319			
Panel B. Non-SOE listed firms						
Yield (%)	4.086	3.600	1.664	2.250	6.800	1,271
Developer	0.348	0.000	0.476	0.000	1.000	1,271
Issuance amount (Bil CNY)	1.501	1.000	3.000	0.400	2.500	1,271
Maturity (year)	1.987	1.000	1.816	0.230	5.000	1,271
Rating	4.543	5.000	0.644	4.000	5.000	1,271
TreasuryYield	2.482	2.418	0.561	1.809	3.300	1,271
Leverage	62.867	63.928	11.298	48.154	75.330	1,271
Salegrowth	0.449	0.527	0.873	-0.794	1.387	1,271
#Firms			67			
Panel C. SOE listed firms						
Yield (%)	3.147	2.960	1.070	1.950	4.700	5,959
Developer	0.214	0.000	0.410	0.000	1.000	5,959
Issuance amount (Bil CNY)	2.115	1.500	3.354	0.500	4.000	5,959
Maturity (year)	1.620	0.750	1.822	0.240	5.000	5,959
Rating	4.752	5.000	0.538	4.000	5.000	5,959
TreasuryYield	2.336	2.260	0.551	1.670	3.170	5,959
Leverage	67.333	71.452	13.717	46.093	81.496	5,959
Salegrowth	0.420	0.513	0.750	-0.768	1.220	5,959
#Firms			252			
Panel D.All listed developers						
Yield (%)	3.756	3.480	1.483	2.100	6.000	1,720
SOE	0.743	1.000	0.437	0.000	1.000	1,720
Issuance amount (Bil CNY)	1.084	1.000	0.747	0.400	2.000	1,720
Maturity (year)	2.431	3.000	2.058	0.250	5.000	1,720
Rating	4.511	5.000	0.691	3.000	5.000	1,720
TreasuryYield	2.475	2.440	0.542	1.793	3.190	1,720
Leverage	63.335	66.719	12.696	45.393	76.927	1,720
Salegrowth	0.581	0.565	0.965	-0.834	1.757	1,720
#Developers			88			
Panel E.Listed non-SOE developers						
Yield (%)	4.846	4.440	1.679	2.950	7.300	442
Issuance amount (Bil CNY)	1.336	1.000	0.809	0.500	2.500	442
Maturity (year)	3.273	3.000	1.864	0.740	5.000	442
Rating	4.572	5.000	0.603	4.000	5.000	442
TreasuryYield	2.688	2.650	0.484	2.200	3.420	442
Leverage	69.740	69.880	8.273	61.530	78.550	442
Salegrowth	0.653	0.661	1.092	-0.862	2.155	442
# non-SOE developers			19			
Panel F. Listed SOE developers						
Yield (%)	3.379	3.195	1.197	2.000	4.990	1,278
Issuance amount (Bil CNY)	0.997	0.900	0.703	0.370	2.000	1,278
Maturity (year)	2.140	0.750	2.042	0.250	5.000	1,278
Rating	4.490	5.000	0.717	3.000	5.000	1,278
TreasuryYield	2.402	2.365	0.541	1.715	3.170	1,278
Leverage	61.121	63.555	13.200	42.920	76.330	1,278
Salegrowth	0.556	0.531	0.916	-0.805	1.619	1,278
#Developers			69			

Table B6: Summary statistics for home sales

Panel A reports summary statistics for developer-level home sales variables for the full sample. Panel B and C provides summary statistics for non-SOE and SOE developers, respectively. Variable definitions are provided in Table A1.

	Mean	Median	Std.Dev.	10prct	90prct	Obs
Panel A. All developers						
Num_{Home}	779.879	45.000	3150.715	0.000	1377.000	14,284
$\text{Area}_{\text{Home}}$ $(1,000m^2)$	90.332	5.403	365.693	0.000	159.478	14,284
$\text{Value}_{\text{Home}}$(Million CNY)	1304.550	55.401	5120.269	0.000	2325.176	14,284
$\text{Price}_{\text{Home}}(1,000CNY/m^2)$	12.981	11.698	7.575	5.852	20.897	11,183
#Developers			736			
Panel B. Non-SOE developers						
Num_{Home}	1796.787	220.000	5174.323	1.000	4162.000	4,411
$\text{Area}_{\text{Home}}$ $(1,000m^2)$	207.779	26.254	600.703	0.089	464.447	4,411
$\text{Value}_{\text{Home}}$(Million CNY)	2953.777	310.241	8232.193	0.607	7121.798	4,411
$\text{Price}_{\text{Home}}(1,000CNY/m^2)$	14.322	13.376	7.456	7.166	21.532	3,986
#Developers			186			
Panel C. SOE developers						
Num_{Home}	325.550	18.000	1316.750	0.000	565.000	9,873
$\text{Area}_{\text{Home}}$ $(1,000m^2)$	37.860	2.318	152.880	0.000	65.977	9,873
$\text{Value}_{\text{Home}}$(Million CNY)	567.718	22.421	2428.900	0.000	929.841	9,873
$\text{Price}_{\text{Home}}(1,000CNY/m^2)$	12.238	10.474	7.538	5.460	20.441	7,197
#Developers			550			

Table B7: Regression Discontinuity estimates on developers' land purchase probability

This table reports the Regression Discontinuity (RD) estimates of the effect of each red-line threshold on non-SOE developers' probability of land purchase during the post-TRL period. All specifications use local polynomial regression with a uniform kernel (unless noted otherwise) and control for developer characteristics (*LnAsset*, *ROA*, *Netprofitmargin*, and *Salegrowth*). Robust bias-corrected inference is applied throughout. Panel A presents baseline estimates using first-order polynomial regression with MSE-optimal bandwidth. Panel B presents higher-order polynomial estimates. Panel C presents donut-hole estimates excluding observations within ±0.5 of the cutoff for debt-to-asset ratio threshold, within ±1 of the cutoff for net gearing ratio threshold and within ±0.05 of the cutoff for Cash-to-short term debt ratio threshold. Panel D present bandwidth sensitivity tests using 0.75× and 1.25× the MSE-optimal bandwidth. Panel E uses alternative kernels for estimations, i.e., Trianguluar and Epanechanikov kernel. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

	Coefficient	Optimal Bandwidth	Observations	Effective observations
Panel A. Baseline test				
T = 1[Debt-to-Asset Ratio >70%]	-0.036	5.220	1,911	532
T = 1[Net gearing ratio >100%]	-0.193***	30.041	1,911	505
T = 1[Cash-to-short term debt ratio <1]	-0.175**	0.192	1,911	280
Panel B. Polynomial order: 2				
T = 1[Debt-to-Asset Ratio >70%]	-0.178	4.343	1,911	445
T = 1[Net gearing ratio >100%]	-0.245***	37.914	1,911	681
T = 1[Cash-to-short term debt ratio <1]	-0.178**	0.297	1,911	454
Panel C. Donut RDD Test				
T = 1[Debt-to-Asset Ratio >70%]	-0.217**	4.091	1,838	363
T = 1[Net gearing ratio >100%]	-0.146**	29.700	1,902	496
T = 1[Cash-to-short term debt ratio <1]	-0.655***	0.141	1,827	134
Panel D. Bandwidth Robustness				
Bandwidth = 0.75 × Optimal bandwidth				
T = 1[Debt-to-Asset Ratio >70%]	-0.216	N/A	1,911	418
T = 1[Net gearing ratio >100%]	-0.176***	N/A	1,911	351
T = 1[Cash-to-short term debt ratio <1]	-0.191	N/A	1,911	218
Bandwidth = 1.25 × Optimal bandwidth				
T = 1[Debt-to-Asset Ratio >70%]	-0.067	N/A	1,911	576
T = 1[Net gearing ratio >100%]	-0.087***	N/A	1,911	639
T = 1[Cash-to-short term debt ratio <1]	-0.057**	N/A	1,911	355
Panel E. Kernel Function Robustness				
Triangular Kernel				
T = 1[Debt-to-Asset Ratio >70%]	-0.088	5.572	1,911	541
T = 1[Net gearing ratio >100%]	-0.218***	25.069	1,911	433
T = 1[Cash-to-short term debt ratio <1]	-0.020	0.452	1,911	684
Epanechnikov Kernel				
T = 1[Debt-to-Asset Ratio >70%]	-0.106	5.000	1,911	516
T = 1[Net gearing ratio >100%]	-0.216***	22.872	1,911	365
T = 1[Cash-to-short term debt ratio <1]	-0.049	0.298	1,911	472

Table B8: Regression Discontinuity estimates on developers' home sales

This table reports the Regression Discontinuity (RD) estimates of the effect of each red-line threshold on non-SOE developers' home sales values (in 10K CNY) during the post-TRL period. All specifications use local polynomial regression with a uniform kernel (unless noted otherwise) and control for developer characteristics (*LnAsset*, *ROA*, *Netprofitmargin*, and *Salegrowth*). Robust bias-corrected inference is applied throughout. Panel A presents baseline estimates using first-order polynomial regression with MSE-optimal bandwidth. Panel B presents higher-order polynomial estimates. Panel C presents donut-hole estimates excluding observations within ±0.5 of the cutoff for debt-to-asset ratio threshold, within ±1 of the cutoff for net gearing ratio threshold and within ±0.05 of the cutoff for Cash-to-short term debt ratio threshold. Panel D present bandwidth sensitivity tests using 0.75× and 1.25× the MSE-optimal bandwidth. Panel E uses alternative kernels for estimations, i.e., Trianguluar and Epanechanikov kernel. ***, ** and * indicate statistical significance at 1%, 5% and 10%, respectively.

	Coefficient	Optimal Bandwidth	Observations	Effective observations
Panel A. Baseline test				
T = 1[Debt-to-Asset Ratio >70%]	-0.544***	3.423	1,340	300
T = 1[Net gearing ratio >100%]	-0.126***	19.008	1,340	192
T = 1[Cash-to-short term debt ratio <1]	-0.768***	0.086	1,340	94
Panel B. Polynomial order: 2				
T = 1[Debt-to-Asset Ratio >70%]	-0.138**	3.106	1,340	273
T = 1[Net gearing ratio >100%]	-0.215***	26.832	1,340	317
T = 1[Cash-to-short term debt ratio <1]	-0.778***	0.118	1,340	111
Panel C. Donut RDD Test				
T = 1[Debt-to-Asset Ratio >70%]	-0.312	2.175	1,281	120
T = 1[Net gearing ratio >100%]	-0.051	18.068	1,236	149
T = 1[Cash-to-short term debt ratio <1]	-1.058***	0.181	1,292	107
Panel D. Bandwidth Robustness				
Bandwidth = 0.75 × Optimal bandwidth				
T = 1[Debt-to-Asset Ratio >70%]	-0.181	N/A	1,340	197
T = 1[Net gearing ratio >100%]	-0.150***	N/A	1,340	137
T = 1[Cash-to-short term debt ratio <1]	-0.719***	N/A	1,340	85
Bandwidth = 1.25 × Optimal bandwidth				
T = 1[Debt-to-Asset Ratio >70%]	-0.154***	N/A	1,340	342
T = 1[Net gearing ratio >100%]	-0.484***	N/A	1,340	272
T = 1[Cash-to-short term debt ratio <1]	-0.193***	N/A	1,340	111
Panel E. Kernel Function Robustness				
Triangular Kernel				
T = 1[Debt-to-Asset Ratio >70%]	-0.131***	2.805	1,340	224
T = 1[Net gearing ratio >100%]	-0.180***	22.279	1,340	235
T = 1[Cash-to-short term debt ratio <1]	-0.315***	0.242	1,340	217
Epanechnikov Kernel				
T = 1[Debt-to-Asset Ratio >70%]	-0.137***	2.419	1,340	197
T = 1[Net gearing ratio >100%]	-0.161***	17.955	1,340	192
T = 1[Cash-to-short term debt ratio <1]	-0.234***	0.135	1,340	111