Can Real Estate Booms Hurt Firms? Evidence on Investment Substitution

Harald Hau*

University of Geneva, CEPR and Swiss Finance Institute

Difei Ouyang**

University of International Business and Economics

May 31, 2024

Abstract

In geographically segmented credit markets, local real estate booms can deteriorate the funding conditions for small manufacturing firms and undermine their growth and competitiveness. Based on exogenous variations in the administrative land supply for residential housing across Chinese cities, we show that real estate price hikes caused by a restrictive land supply reduce bank credit to manufacturing firms, raise their borrowing costs, diminish their investment rate, compromise their output and productivity growth, and increase their exit rates. Such harmful effects are more pronounced among small firms and those located in more bank-dependent regions.

Key words: Factor price externalities, real estate booms, firm growth, financial constraints
JEL codes: D22, D24, R31

*Geneva Finance Research Institute, University of Geneva, 42 Bd du Pont d'Arve, 1211 Genève 4, Switzerland. Tel.: (+41) 22 379 9581. E-mail: `prof@haraldhau.com`.

**School of International Trade and Economics, University of International Business and Economics, Huixindong Street No. 10, Beijing 100029, China. Tel.: (+86) 139 1060 2792. E-mail: `difei.ouyang@gmail.ch`

Acknowledgments

We are grateful to Yongheng Deng, René M. Stulz, Meng Miao, Hongyi Chen, Sebastian Dörr, Fansheng Jia, Chen Lin, Olivier Scaillet, Jin Tao, Fabio Trojani, Zexi Wang, Hao Zhou, Michel Habib and Daragh Clancy for their comments. Our particular thanks goes to Roni Michaely for his most helpful suggestions. We also thank participants at various seminaors and conferences for valuable comments and discussions. Difei Ouyang gratefully acknowledges funding from the National Natural Science Foundation of China (grant no. 72303030). The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

1 Introduction

In geographically segmented credit markets, real estate investments compete with corporate investments for the local household savings. During real estate booms with a strong surge in housing investment, the residual capital available for corporate investments can become expensive and scarce — thus undermining the competitiveness and growth potential of local (bank-dependent) manufacturing firms. Empirically, this potential negative causal effect of housing booms on corporate growth is difficult to establish because of many confounding effects on real estate prices and firm performance.

China's internally segmented credit market and its considerable geographic variation in local real estate price changes make it a fascinating case study to explore such investment crowding-out among financially constrained manufacturing firms. Figure 1 illustrates the potential macroeconomic significance of investment substitution during real estate booms: We select the 50 prefecture-cities with the highest and lowest real estate price change and compare the average annual investment rate and output growth rate for different types of manufacturing firms located in these two groups of cities. Small firms, as the most financially constrained group, show a dramatic average shortfall of both average investment rate and growth rate of 10 and 13 percentage points, respectively, in cities with sizeable real estate booms. By contrast, less constrained large firms show only modest differences in their average investment rate and growth rate across both groups of cities. Such dramatic geographic differences in investment and growth of small firms in the world's largest economy represent a critical economic phenomenon that begs for an explanation.

The main contribution of our paper is to account for these large geographic differences in small firm development. We provide evidence on a negative *causal* effect of real estate booms on small firm performance—a channel operating through increased borrowing costs, bank credit substitution from corporate to real estate lending, and reduced firm investment. We propose a modified Harrod-Balassa-Samuelson framework that guides our identification strategy and theoretically demonstrates why real estate booms can harm the tradeable sector. Such a causal nexus is of great general interest as most countries have experienced episodes with significant housing price increases that can give rise to the credit substitution we document for China. Our ability to show how a systematic increase in housing prices causes a reduction in firms' investment and corporate growth concerns policy makers in general and those overseeing macroprudential regulation on real estate lending in particular.

In real estate booms, various supply and demand factors can work in conjunction and make it difficult to isolate a single exogenous instrument of sufficient relevance to allow causal inference. China is exceptional in the sense that all constructible land for residential housing is supplied monopolistically by the local government and subject to its particular administrative process. Friction-prone intragovernmental coordination, property rights conflicts, and policy conflicts with the regional and central government can produce large and highly variable discrepancies between planned and realized residential land supply unique to each city. We construct quasi-exogenous land supply measures at the city-level, and our identification is based only on the erratic intertemporal land supply shocks in a panel structure. Our IV strategy resembles Waxman *et al.* (2020), who also use cumulative land sales as an instrumental variable and find a negative consumption response to rising Chinese housing prices in 2011–13. As a robustness check, we also execute a strictly cross-sectional identification strategy based on local housing supply elasticities similar to Mian and Sufi (2011, 2014). This type of specification has drawn some criticism in the literature (Davidoff, 2013, 2015). However, we obtain quantitatively similar results under both the intertemporal and cross-sectional identification scheme.

Our main empirical finding is the strong economic effect of land-supply induced variations in real estate prices on corporate capital costs of small firms, their access to bank credit, their investment rate and growth. We show that a 50% relative increase in a city's real estate price due to a shortage in local land supply increases the borrowing costs of firms by an average 0.65 percentage points annually. It reduces the share of firms with bank credit by 3.9 percentage points, which represents a 11.5% reduction relative to the sample mean of 34 percentage points.[1] This local credit crunch lowers the average corporate investment rate by 8.6 percentage points, which represents a large 25% reduction relative to the sample mean of 35 percentage points. The relative output decline amounts to a 37.8% of value-added output and total factor productivity features a relative decline of nearly 11.6% for the average manufacturing firm. Moreover, an increase in the real estate price increases firm's exit rate, suggesting that real estate booms can also hurt the manufacturing sector via the extensive margin.

[1] We note that a 50% change is only slightly smaller than a 1 standard deviation change in housing prices during our sample period.

Because of the uneven firm access to China's national credit market, we find that these real effects are concentrated among small firms. Large and listed firms in the same boom city show no evidence of underinvestment and relative decline. Evidence on this firm-size dependence of the investment crowding-out disqualifies alternative explanations which do not predict such firm size heterogeneity in investment and output growth. Similar investment rate and growth performance among large firms in locations with and without real estate booms suggests that there are no omitted variables accounting for a general (non-financial) nexus between firm growth and the local real estate market price increases. We also find that the real effects concentrate in firms located in more bank-dependent provinces, which strengthens our argument that real estate booms harm firm growth through a credit supply channel.

To provide a deeper theoretical foundation for these real effects, we develop a modified Harrod-Balassa-Samuelson framework in which a real estate sector and a tradable sector compete for a limited local capital supply. Land supply shortages inflate the price and capital demand for housing investment and thus crowd out investment in the tradable sector. Just as wage externalities in the traditional Harrod-Balassa-Samuelson framework afflict the non-traded sector (Balassa, 1964), capital cost externalities can harm the tradable sector in our modified model. But such a capital cost externality is inherently more pernicious for a tradeable sector subject to global competition and therefore — unlike the non-tradable sector — incapable of factor cost pass-through to product prices. Our evidence for strong adverse real effects of real estate booms on small firms acquires its full significance only in this broader theoretical perspective.

The theoretical framework delivers additional insights. Importantly, the model predicts that real estate booms should lower the local real manufacturing wage. The Chinese data strongly confirms this prediction, which also contrasts with a so-called "Dutch disease" setting with increasing labor costs. While the Harrod-Balassa-Samuelson model is widely credited for its explanatory power in the context of real wage externalities between the traded and non-traded sector (Samuelson, 1994), its empirical relevance for capital cost externalities from the real estate sector to the tradable sector is not widely appreciated. Yet, our generalization of the Harrod-Balassa-Samuelson framework provides a good empirical match to the Chinese experience for many different firm variables. The fact that real manufacturing wage decrease in cities with real estate booms also implies that labor cost changes cannot be the cause the competitive decline of small manufacturing firms in these locations. Moreover, our analysis sheds particular light on the role of real estate booms for the development of small manufacturing firms, which are most often subject to financial constraints. Conditional on survival, small firms tend to grow faster than large firms in the US and many other countries (Birch 1979; Davis *et al.* 1996; Neumark *et al.* 2011; Haltiwanger *et al.* 2013; Akcigit and Kerr 2018). Therefore, any financing shortfall among small firms induced by real estate booms can have a particularly harmful effect on job creation and real long-term growth.

Overall, our analysis suggests that real estate booms impaired the development of the private manufacturing sector in many locations of China. As private firms play a vital role in China's technological advance and have been shown to be more agile than state-owned enterprises due to better management practices (Song *et al.*, 2011, Hau, *et al.* 2020), such an effect is likely to have lasting negative consequences for the long-run growth prospect of the regions concerned. The large magnitude of the differential growth effects are grounded in the geographic fragmentation of China's banking system and its many borrowing frictions for small private firms (Hau, *et al.*, 2021). Path dependence of industrial development could imply that such lost growth potentials cannot be recovered even if banking policy changes and banks curb their real estate lending. This exemplifies the limits of China's investment-led growth model and the need for a more integrated and developed capital market if China seeks a broader advance to the technological frontier (Zilibotti, 2017).

While our paper highlights the particularly stark geographic segmentation of the Chinese market for small firm credit, we stress that such geographic segmentation of bank credit to small firms is also widespread in advanced economies. For example, the paucity of information about small firms weakens their borrowing capacity, which is reinforced by bank distance. While Petersen and Rajan (2002) find that improved information technology in US banking reduces the role of distance for small business lending, such distance nevertheless continues to matter. Similarly, Michelacci and Silva (2007) find that Italian and U.S. entrepreneurs often operate and work in their birthplace even more so than employees, which they attribute to the competitive advantage of local entrepreneurs in securing local credit and other financial opportunities. The universally strong role of geography for small firm financing suggests that our findings enjoy external validity beyond the Chinese context.

2 Literature

The real effects of credit supply shocks on corporate investment and growth have always been a key concern for economists. Based on improved identification methods, recent empirical work has highlighted how negative shocks to bank capital compromise the development prospects of bank-dependent firms (Chava and Purnanandam, 2011; Huber 2019). Our new evidence from China shows that a large negative credit supply shock to the corporate sector can alternatively originate in credit substitution to real estate finance. An extensive new literature has focused on the real effects of the 2008 banking crisis, which originated in excessive real estate credit.[2] But there is only limited evidence that real estate booms themselves can have direct negative real effects on firm investment and competitiveness even if bank distress is completely absent. Yet, the Chinese evidence suggests that such effects can be quantitatively large.

Recent research has examined the relationship between real estate booms and corporate investment by U.S. firms. For firms with real estate property, local property booms can improve business collateral, relax borrowing constraints, and thus increase capital investment, expand employment, or generally foster entrepreneurship (Chaney *et al.*, 2012; Adelino *et al.*, 2015; Corradin and Popov, 2015; Harding and Rosenthal,2017; Schmalz *et al.*, 2017; Bahaj *et al.*, 2020; Jiménez *et al.*, 2020; Ersahin and Irani, 2020). However, some recent work has challenged the importance of this collateral channel. Kerr *et al.* (2022) study the impact of the dramatic run-up in US house prices prior to the 2007 financial crisis on local entrepreneurship, but find primary evidence for incremental activity related directly to real estate businesses than to entrepreneurship and business growth in general. Similarly, Bracke et al. (2018) question the positive effect of housing booms on entrepreneurship as mortgage debt levels also tend to rise, and higher mortgage rates and leverage tend to amplify risk aversion. Our evidence on the Chinese real estate booms suggests that such negative externalities appear to dominate any limited benefits of higher collateral values; hence policymakers should be cautious in promoting real estate prosperity. Meanwhile, for Chinese firms this balance sheet effect is unlikely to matter much because of a lack of real estate assets on firms' balance sheets. For example, among Chinese listed firms in 2007, only 35% report positive real estate assets and their aggregate

[2] Amiti and Weinstein, 2011; Chodorow-Reich, 2013; Paravisini *et al.*, 2014; Cingano *et al.*, 2016; Bentolila *et al.*, 2017; Acharya *et al.*, 2018; Huber, 2018.

value accounts for only 2.6% of aggregate assets.[3] Accordingly, we find that real estate holdings play only a negligible role in attenuating the strong negative investment effect of real estate booms when we account separately for non-operating assets on the firms' balance sheets. This confirms results by Wu *et al.* (2015) that the collateral channel is of only minor macroeconomic significance in China.

An exception here is the evidence by Chakraborty *et al.* (2018) showing that bank-level exposure to real estate booms adversely affect banks' volume and cost of business loans in the U.S. A one standard deviation increase in normalized U.S. housing price index relative to the base year 2000 (corresponding to 104%) increases the corporate borrowing costs of financially constrained U.S. firms by 0.53 percentage points and reduces the corporate investment rate by an average of 6.2 percentage points. Our evidence for China suggests a much larger real effect: A similar increase of the residential real estate price by 104% implies an average increase of corporate borrowing costs by 1.1 percentage points and reduces the average investment rate by 15.2 percentage points. In both cases, part of the credit supply adjustment is at the external margin and takes on the from of credit rationing primarily to small firms. Also closely related to our paper is Chen *et al.* (2017), who explores the effect of (commercial) land appreciation on firm behavior in China. They find land price appreciation increases investments by firms with significant land holdings, but reduces investment by all other firms. These authors focus on publicly listed firms for which credit constraints tend to be less severe. By contrast, our study considers all manufacturing firms and analyzes more comprehensively how *local* real estate booms affect different dimensions of *local* firm performance.

3 Theoretical Motivation

One of the best documented stylized facts about relative competitiveness is the Harrod-Balassa-Samuelson effect: Productivity growth in tradables drives local real wage growth across sectors (Obstfeld and Rogoff, 1996). This makes non-tradable labor-intensive service sectors expensive and non-competitive by international comparison; yet their very non-tradability implies that high wage costs can be passed through to high prices for non-tradables. We briefly present a

[3] For all firms, including those that do not hold real estate assets, the real estate value share is lower at 1.1% of aggregate assets — suggesting that smaller non-listed manufacturing firms own only negligible amounts of real estate assets.

similar two-sector framework with factor price externalities in capital costs (rather than labor costs) to explore the impact of real estate booms. The following section provides the key insights and outlines several testable propositions; Appendices A and B describe the theoretical model in full detail.

Our analysis is predicated on a high degree of geographic credit market segmentation for small and medium-size firms in China. A number of previous studies document evidences of low interregional capital mobility in China using the Feldstein-Horioka saving-investment or the Campbell-Mankiw consumption-smoothing framework (Boyreau-Debray and Wei, 2004; Chan *et al.*, 2011). Although large (state-owned) national banks form internal markets that facilitate free capital flows, their lending policies are strongly tilted towards large (state-owned) companies. By contrast, province, city, or local banks usually operate within the geographic perimeter of the respective territorial entities, which generates a geographically highly segmented credit market for small and medium size private firms. For example, Huang *et al.* (2018, 2020) still finds extensive private investment crowding-out by local government borrowing after our sample period.[4]

While there are no *explicit* restrictions for firms to borrow from banks in other cities, the observed share of out-of-city corporate borrowing is very small — suggesting important non-regulatory barriers. Gao *et al.* (2019) documents that the share of out-of-city bank loans accounts for only 12% of total loans based on 7 million loan contracts granted by the 19 largest Chinese banks between October 2006 and June 2013; this share is likely to be even smaller when smaller city banks are considered. Government policies also impose numerous restrictions on mortgage credit and credit to real estate developers. Personal provident housing loans and mortgages can only be invested in local real estate; commercial bank lending to developers can only be used for local construction. Finally, shadow banking can alleviate local credit constrains only to a limited extent as bank lending still represents almost 7/8th of outstanding credit in 2008 (Elliott *et al.* 2015). In the Internet Appendix B1, we estimate an error correction model and find a mean reversion of only 13.8% between a city's median loan rate and those of firms in the neighboring cities, illustrating the strong geographic segmentation of China's corporate

[4] We note that the firm investment crowding out due to high local government borrowing reported in Huang, Pagano, and Panizza (2018, 2020) is independent from the crowding-out effects related to real estate booms; both show only a small and statistically insignificant correlation. Moreover, extensive local government debts only emerged after the end of our sample period.

credit markets.[5]

Consider a close economy (a city) with a real estate sector and a tradable sector (i.e. the manufacturing). While tradable sector features a Cobb-Douglas production function in capital and labor, the real estate production requires both capital and governmental land supply S as complementary inputs. Under a price elastic housing demand[6], real estate inflation can be shown to be proportional to changes in the local land supply, namely

$$\widehat{P} = \widehat{S} \times \eta_c \ , \tag{1}$$

where $\widehat{X} = dX/\overline{X}$ represents variables in percentage changes relative to steady state log values. The parameter $\eta_c = -1/\gamma_c$ equals the (negative) inverse of the housing demand elasticity γ_c in city c; it governs the local housing price sensitivity to land supply.

If a real estate price boom occurs in a Chinese city due to limited land supply, local capital is predominantly channeled into real estate investment, where rapid price increases promise a high return. But unlike the non-tradable sector in the Harrod-Balassa-Samuelson world, the manufacturing sector cannot pass on a higher factor cost (in capital) to a competitive international market price and therefore faces stagnating growth prospects. Meanwhile, underinvestment depresses firm's real wages via changed labor productivity. Therefore, our theoretical framework delivers the following two testable propositions:

Proposition 1: Firm Adjustments to the Real Estate Boom

Under a limited supply of constructible land $\widehat{S}$, real estate inflation $\widehat{P}$ reduces investment rate I/K, output $\widehat{Y}$, and labor productivity $\widehat{Y/L}$ in the manufacturing sector.

Proposition 2: Wages and Interest Rates Adjustments to the Real Estate Boom

[5] The variation in the median corporate bank loan rate across cities ranges from 3.8% for a city at the 10% quantile to 6.4% for a city at the 90% quantile.

[6] We note that housing price inflation can be further accelerated by speculative buying of housing in view of future capital gains; yet we do not explicitly model any additional speculative housing demand (unlike Chen *et al.*, 2016; Shi Yu, 2017).

Under a limited supply of constructible land $\widehat{S}$, real estate inflation $\widehat{P}$ pushes up interest rate $\widehat{i}$ but depresses real wage $\widehat{w}$.

We do not model bank intermediation and assume competitive capital allocation. Unlike Chakraborty *et al.* (2019), we do not dispose of disaggregate bank data to document banks' real estate lending bias under real estate booms. However, aggregate data suggests that the banking sector allocated an increasing proportion of credit to housing development.[7]

Our simple two-sector model does not allow for firm heterogeneity in capital access. Naturally, some firms are exposed to local capital scarcity more than others. In particular, firms with large fixed assets (available as collateral) and state-owned enterprises (SOEs) with political support should find it much easier to maintain credit access even under local capital scarcity. We therefore add the following testable hypothesis:

Hypothesis 1: Heterogeneous Credit Access

In cities with real estate booms, firms with large fixed assets or SOEs find it easier to maintain credit access and ceteris paribus experience higher investment rates, and larger growth in output and labor productivity compared to small private firms.

Our competitive model also ignores the additional consequences of higher capital costs and underinvestment on (long-term) firm profitability, factor productivity, and exit. However, firm performance measures are likely to decline if real estate booms increase the capital costs of local manufacturing firms (Dörr *et al.* 2017; Manaresi and Pierri 2018). We summarize these effects in a second testable hypothesis:

Hypothesis 2: Firm Profitability, TFP, and Exit

For tradable producers, increased local capital costs in a real estate boom imply reduced profitability. The credit supply constraint adversely affects total factor productivity (TFP) growth because of underinvestment. Moreover, firms become more

[7] The outstanding individual housing loans increased fivefold from 560 billion Yuan in 2001 to 3 trillion Yuan in 2007. In the last sample year 2007, roughly 13.8% of all new medium and long term bank loans were allocated to the real estate companies compared to only 7.5% for the entire manufacturing (People's Bank of China, 2007).

likely to exit due to capital chain rupture. Within a city, these effects should be less pronounced for SOEs or firms with large fixed assets because of easier access to credit.

4 Data Issues

4.1 Data Sources

We use firm data from the annual survey of all industrial firms (ASIF) conducted by China's National Bureau of Statistics over the period of 1998–2007. The ASIF data cover state-owned and private-owned enterprises in the mining, manufacturing, and utility sectors. Private enterprises are covered if their annual operating income exceeds RMB 5 million.[8] The ASIF data accounts for over 70% of industrial employment and 90% of industrial output. It is the most frequently used dataset for studying micro-firm behaviors in China. The survey consists of a stratified firm sample for more than 300 cities and 43 two-digit industries. The survey reports detail accounting data, allowing us to construct measures of firm investment, productivity, and financial performance. The location of the firm's headquarters can be identified at the prefecture-level cities so that we can match additional city-level statistics–particularly on the local real estate market. The prefecture-level city is the third level of China's public administration, below the province level, but above the county/district level. The real estate market becomes more homogeneous with geographic disaggregation as we move from province to city to county level. Yet, data availability issues force us to undertake the analysis at the city level rather than the county level.

Three main shortcomings of the data source should be highlighted. First, the firm sample is unbalanced, smaller firms in particular are typically covered only for less than three consecutive years. Second, the survey contains data errors and must be filtered for implausible data points. We provide details of our data cleaning procedure in the Internet Appendix B2, which produces a final sample of around 1 million firm-year observations for the period 2002–2007. Third, the survey data do not report any plant-level information. Multi-plant firms can produce in multiple cities with diverging real estate environments. However, the city-level represents a relatively

[8] RMB 5 million was equivalent to US$ 603,930 in 1998 and US$ 657,549 in 2007.

large administrative unit with an average population of 3.5 million. Only very large corporations are likely to operate in multiple cities, and eliminating large firms from the sample does not appear to influence our main estimation results. Untabulated results also show that the more pronounced negative effect of the real estate booms on small firms is robust to the elimination of large firms from the sample which are likely to feature multiple establishments.

Table 1 gives the statistical description of the firm-level variables. We denote as $I/K_{j,t}$ the real gross investment rate. The ASIF only reports the book value of fixed assets so that nominal investments are not comparable across firms and reporting years due to inflation. Following Brandt *et al.* (2012), we assume that firms start purchasing fixed assets from the starting periods with a certain pattern so we can deflate the book value to obtain the real terms. Internet Appendix B3 reports in detail the procedure we use to calculate the real investment rate (Rudai, 2015). For most manufacturing firms, long-term debt consists almost exclusively of bank credit. The dummy variable $Loan_{j,t}$ marks as 1 all firms that have long-term debt on their balance sheet. The end of the year (log) output $\ln Y_{j,t}$ is measured as value-added output deflated at industry output prices and labor productivity follows as the log ratio $\ln(Y/L)_{j,t}$. The two important factor prices of a firm are the (log) average real annual employee salary $\ln w_{j,t}$ and bank loan rate $i_{j,t}$ measured by the ratio of interest payments to the sum of long-term bank and short-term bank credit, where the latter term is interpolated from the more comprehensive reporting of listed firms.[9] A firm's return on assets $ROA_{j,t}$ is net profits divided by total firm assets. The (log) revenue-based total factor productivity $\ln TFP_{j,t}$ is calculated (log) TFP using the Olley and Pakes (1996) method. $Exit_{j,t}$ is a dummy variable for firms exiting from the sample in period $t+1$. In addition, we define as $\ln \mathit{Fixed\ Assets}_j$ the firm's (log) fixed assets and a dummy SOE_j of whether the firm represents a state-owned enterprise in the year a firm enters the survey.

A supplementary panel of city-level data comes from the China City Statistical Yearbook (CSY) and China's Regional Economic Statistical Yearbook (RESY). The RESY reports the total sales value and total floor space of so-called "commercial housing." This term refers to residential housing sold at market prices by a "qualified real estate development company.",

[9] For listed manufacturing companies, we calculate the ratio of short-term bank credit to short term liability annually between 2002 and 2007. For example, in 2002, this ratio is 44.9%. For any sample firm j in the ASIF in 2002, we add short-term bank credit, $\mathit{ST\ Credit}_{j,t} = 44.9\% \times \mathit{ST\ Liability}_{j,t}$, to the reported long-term bank credit to obtain a firm's total bank credit.

which is the predominant type of housing transaction. The latter acquires land usage rights via land leasing, develops the real estate, and then sells it at a profit. The ratio of the sales value of commercial housing to its total floor space represents our local (city-level) real estate price index, which is the only index available for most cities during our sample period. Table 1, Panel B, reports the (log) real price level $\ln P_{c,t}$ of residential housing calculated as an average of annual transaction prices in city c and deflated by the consumer price index. Our sample is dominated by boom years: We find annual price declines for only 24% of all city-year observation. Figure 2 conveys the large overall variation in real estate prices across China's prefecture-level cities. We sort cities by their initial real estate price index in 2003 (blue spikes) and shows the large variation of the same price index in 2010 (red spikes).

We instrument local real estate price change by the local land supply for residential housing $L_{c,t}$ at the city-level for the period 2002–2007. Unfortunately, the annual land supply for residential housing is reported only at the province level as $L_{p,t}$. However, we know the city-level supply of non-industrial land, which is composed mostly of residential land and some commercial (non-industrial) land supply. To infer the component of the city-level land supply for residential housing, we calculate the ratio $L^{NI}_{c,t}/L^{NI}_{p,t}$ city's share of non-industrial land supply and use this ratio to allocate province-level residential land supply to the city level according to

$$L_{c,t} = \frac{L^{NI}_{c,t}}{L^{NI}_{p,t}} L_{p,t}. \qquad (2)$$

Underlying this approximation is the assumption that the shares of commercial and residential land supply are constant across cities in the same province.[10]

Important to our identification strategy is that variation in the residential land supply does not directly influence firm investment and performance through channels other than the residential housing price and the credit diversion it causes. In this context we highlight that land supply policies for industrial land do not correlate at economically significant magnitudes with non-industrial or residential land supply. The correlation between the (log) non-industrial land supply $\ln L^{NI}_{c,t}$ and the industrial land supply $\ln L^{I}_{c,t}$ is very low at 0.03. In addition, industrial land prices feature constantly low prices during our sample period; with industrial land prices being on average only 20% of non-industrial land prices. The correlation between the (log)

[10]Using city's initial share of non-industrial land (L^{NI}_{c}/L^{NI}_{p}) to infer city-level land supply for residential housing does not change the main results, as shown in Internet Appendix Table B3.

price of non-industrial land and the (log) price of industrial land is negligible at 0.008, which is consistent with the observation that real estate booms for residential property generally do not spill over into higher rental income for industrial property. The most plausible explanation is that local governments in China usually charge very low prices on industrial land to attract investment in the manufacturing sector. This in conjunction with the general non-convertibility of industrial land creates the persistent segmentation between residential property and industrial property..

4.2 Land Supply Variations as Instrument

Recent work on the determinants of U.S. growth before and during the Great Recession has used housing supply constraints as instruments for housing price inflation to explore causal effects on household debt and consumption (Mian and Sufi, 2011; Mian *et al.,* 2013). We apply a similar logic to China's housing market by showing that the local housing price depends inversely on the supply of new constructible land for residential housing in a particular city c. Since housing supply relative to the existing housing stock (measured as total floor space) rather than the absolute land supply should affect the dynamics of housing prices, we define (the log of) this ratio as our normalized supply measure. That is, we normalize the new constructible gross land supply $L_{c,t}$ by the size of the existing housing stock $Stock_{c,t}$ (measured as total floor space) and define the *Relative Land Supply* in city c and year t as

$$Relative\ Land\ Supply_{c,t} = \frac{L_{c,t}}{Stock_{c,t}}. \tag{3}$$

Our econometric strategy allows for unobservable economic factors to influence the cross-sectional pattern of land supply as we include city fixed effects in all 2SLS regressions. Hence, our identification relies on *intertemporal variation* in the land supply only.

We note that real estate markets are highly segmented and that both existing and new housing supply in different districts is heterogeneous. It differs in the commuting time to employment locations and surrounding amenities. The prefecture-level city represents a large geographic area that aggregates existing housing and new land supply of different qualities. As our analysis draws on the average transaction price for housing in a prefecture-level city and aggregate land supply without adjustment for location quality or construction quality, we expect to have measurement errors in our instrument.

The intertemporal variation of the residential land supply is subject to many exogenous uncertainties of the bureaucratic and administrative approval process depicted in Figure 3. The land supply process starts with a land supply plan created by the city-level urban planning bureau in cooperation with the land resource bureau. It is the basis for any project development plan which can receive inputs from other local, provincial, and central government agencies. The second stage consists in the land acquisition either through conversion of farm land (approved by the provincial or central government) or by expropriation of existing local residents. In stage three, the land is developed through demolition and/or new infrastructure creation before commercialization occurs through land auctions and/or direct land sales to residential housing developers.

The elaborate bureaucratic process creates considerable exogenous uncertainty in the supply of residential (constructible) land as can be illustrated for the case of Beijing. For the years 2005 to 2011, the Beijing city government was able to deliver 33%, 49%, 84%, 50%, 124%, 95%, and 49%, respectively, of the planned land supply to its residential housing developers. The standard deviation in realized percentage land supply (relative to the plan) is therefore very large at 32.6%.[11] Such (random) housing supply variation can be traced to a variety of institutional features:

1. **Friction prone intragovernmental coordination:** Implementation of the residential land supply plan relies on the coordination of various city-level government departments (e.g. Land and Resources, Urban Planning, Development and Reform) and county-level institutions. Implementation of the land supply plans therefore depends on successful intragovernmental bargaining and faces many bureaucratic contingencies that can delay supply (Qu, 2008).

2. **Property right conflicts:** The land supply requires (often conflictious) negotiations over incumbent usage rights and local protest can hold up land acquisition. For example, China's Central Television received 15,312 letters on such land conflicts in 2004 (Hui and Bao, 2013). Even if local government can ultimately prevail, legal conflict can inflict considerable delays in implementation.

[11] For details, see the Beijing Planning and Land Resources Yearbook.

3. **Policy conflicts:** The central government occasionally interferes with city-level development plans by stipulating particular quotas for the types and sizes of housing units that city governments are allowed to approve. Imposed revisions to local land supply policies can also result in supply delay (Qu, 2008).

These three institutional features explain why actual and planned land supply show large discrepancy and justifies why the intertemporal pattern of land supply is a plausible exogenous source of variation. Figure B2 in the Internet Appendix shows the large intertemporal variation in the land supply across Chinese cities between 2002 and 2007. Governed by autonomous administrative processes, such variations in land supply outcomes are independent of local economic developments as studied in the following section.

Despite the aforementioned institutional arrangements that cause a high degree of randomness in the realized land and housing supply, a skeptical reader could still maintain that a city's land supply policy is at least partially boosted by expectations of high future housing prices. First, we note that an endogenous supply response is not very likely as local land supply initiatives originate mostly in fiscal incentives rather than concerns about future real estate prices (Waxman *et al.*, 2020). This motivates the use of various city characteristics as baseline controls to reduce estimation bias.

Second, if there is any endogenous forward-looking supply policy, its effect on parameter estimates depends on how it correlates with the error term in the second stage regression for corporate outcomes. We can distinguish three cases. First, if government anticipations are incorrect and only cause land supply to fluctuate more, then this should make the relationship between housing prices and land supply weaker. However, this does not induce any biases in the 2SLS estimates. Second, if endogenous supply positively correlates with corporate outcomes (e.g., positive demand shocks), then we overestimate the harmful effect of housing prices. Third, if endogenous supply negatively correlates with corporate outcomes, then we underestimate the harmful effect of housing prices.[12] While we think that the residential housing supply is generally not influenced by expectations about future corporate outcomes, we consider a positive

[12] Assume a negative relationship between the outcome variable y and the real estate housing price P given by $y = P\beta_P + \varepsilon$, where $\beta_P < 0$. If the land S responds endogenously to the price level P, the resulting equilibrium supply is still a valid instrument as long as exclusion restriction is fulfilled. We obtain a biased inference only if $Cov(S, \varepsilon) \neq 0$. The IV estimator then has a negative bias (towards more negative outcomes) given by $\widehat{\beta}_P - \beta_P = \frac{Cov(S,\varepsilon)}{Cov(S,P)} < 0$, or has a positive bias (towards more positive outcomes) given by $\widehat{\beta}_P - \beta_P = \frac{Cov(S,\varepsilon)}{Cov(S,P)} > 0$.

correlation in principle more likely than a negative correlation. Such a violation of the exclusion restriction makes our estimates an upper bound for the negative effect of real estate booms.

4.3 Endogeneity Concerns about the Land Supply

As argued above, the observed land supply for residential housing by local governments is subject to numerous intra-governmental administrative frictions and can be regarded as exogenous to the performance of local manufacturing firms. In Table 2, we nevertheless explore various potential endogeneity concerns. Specifically, we regress different city-level economic variables on the log relative land supply ($\ln$ *Relative Land Supply*$_{c,t}$) conditioning a set of city-level macroeconomic control variables $X_{c,t-1}$; namely the city-level (log) gross domestic product ($\ln GDP$), (log) city population ($\ln$ *Population*), the annual (log) expenditure ($\ln$ *Gov. Expenditure*) by the city government, its annual (log) revenue ($\ln$ *Gov. Revenue*), the share of government budget deficit to GDP (*Gov. Deficit*) and the percentage of park area within the urban area (*Park Share*) in year $t-1$ that could affect government's land supply in year t. We include these baseline controls because a city's land supply could plausibly vary with city size and fiscal conditions. Failing to control for these variations could invalidate the (conditional) exogeneity restriction for a valid instrument.

The first endogeneity concern is that the city's residential land supply depends on the growth of the local manufacturing sector. For example, cities with a declining manufacturing sector but growing government expenditure could seek to sell more land for extrabudgetary revenue due to the difficulty of gathering tax revenues. This could cause a spurious relationship between land supply and manufacturing firm performance. However, Table 2, Panel A shows that lagged variables of city-level government expenditures growth ($\Delta \ln$ *Gov. Exp.*$_{c,t-1}$), government revenue growth ($\Delta \ln$ *Gov. Revenue*$_{c,t-1}$), GDP growth ($\Delta \ln GDP_{c,t-1}$), and the growth in the share of employment in the secondary sector (Δ*Sec. Ind.*$_{c,t-1}$) are not significantly correlated with the (log) relative land supply, which should alleviate such endogeneity concerns.

In Table 2, Panel B, we consider a number of contemporaneous city variables as potentially related to the relative land supply. Since land sales gradually become an important source of local governments' extrabudgetary revenues (Fang *et al.*, 2016), a potential endogeneity concern is that local governments finance local infrastructure construction through land sales and that such expenditure influences the performance of local manufacturing firms indirectly though demand effects. Yet, Column (1) shows that the relationship between the (log) relative land supply and contemporaneous government infrastructure expenditure (ln $Infrastr.\ Exp._{c,t}$) is statistically insignificant.

A third potential endogeneity issue concerns the political economy of city development. Previous work by Li and Zhou (2005) and Hsu *et al.* (2017) suggest that the tenure (years in office) of the local party leader influences local development policies through promotion incentives, whereby local government officials push new projects at the beginning of their tenure. Table 2, Panel B, Column (2), shows that such a political business cycle does not extend to the residential land supply as the variable ($Leader\ Tenure_{c,t}$) measuring years in power for the local party leadership shows no statistically significant covariance with our instrument. Another endogeneity concern is that residential land supply could substitute for or be complementary to a city's industrial land supply (ln $Industrial\ Land_{c,t}$). Column (3) explores such a relationship, but again no systematic link emerges.

Ambitious future city development projects could also be linked to land sales as a source of revenue (Tian and Ma, 2009; Lichtenberg and Deng, 2009; Chen and Kung, 2016). Table 2, Panel C, Columns (1)-(4), proposes a various proxies for future infrastructure developments such as the (log) growth in the length of city highways ($\Delta \ln Road\ Length_{c,t+1}$), (log) growth in city public bus lines ($\Delta \ln Bus_{c,t+1}$), log growth in the number of hospitals ($\Delta \ln Hospital_{c,t+1}$), or theatres ($\Delta \ln Theatre_{c,t+1}$) all measured in year t+1, respectively. We find no evidence that the relative land supply for residential construction by local government is related to a city's future infrastructure improvements.

Overall, Table 2 supports the assertion that the timing of the residential land supply in a city is governed by internal administrative frictions that do not bear any meaningful relationship to the economic growth of the local manufacturing sectors. This justifies using the (log) relative land supply as a suitable instrument for the local housing price level and its impact on manufacturing firms through the credit substitution channel

4.4 Land Supply and Housing Price Inflation

Variations in the land supply for residential housing construction translate into proportional variations in the housing supply in the following one to two years. Empirically, we use a one year lag for the *Relative Land Supply* to characterize the price effect of land supply variations. As demonstrated in Section 2, the (negative) price effect $\eta_c = -\frac{1}{\gamma_c}$ of land supply on the (log) residential housing price is parameterized in the inverse of the local housing demand elasticity γ_c. In 1998, the Chinese government largely seized to provide social housing and instead allowed the development of a private residential housing market, the so-called commodity housing market. This new housing market has specialized real estate developers as its suppliers and provides a higher quality standard for residential housing than the pre-existing housing stock. A lack of suitable substitutes to commodity housing can explain a more price inelastic demand (i.e. a low γ_c and a large $|\eta_c|$). Empirically, cities with a higher population density and higher per capita income feature lower substitutability between new commodity and older non-commodity housing, which in turn makes the housing demand for commodity housing more price inelastic. Figure B3 in the Internet Appendix contrasts two cities with an elastic and inelastic housing demand. A more price inelastic demand implies that any given supply shock has ceteris paribus a more pronounced effect on the equilibrium housing price.

Compared with more price inelastic cities, cities for which the housing demand elasticities is large and approaches $\gamma_c = 1$, variation in the *Relative Land Supply* is *not* a suitable instrument for local capital scarcity and its effects on firm outcomes. We seek to incorporate this city-level heterogeneity into the construction of our instrument for the (intertemporal) variation of city-level residential housing prices. It is straightforward to estimate the elasticity parameter η_c using a random coefficient model. The latter regroups the panel data for the relative land supply into column vectors containing only the (log) relative land supply for a specific city (and zero otherwise). Formally, we have for the $N = 202$ cities

$$\ln P_{c,t} = \mu_c + \sum_{c=1}^{N=202} \eta_c \ln \mathit{Relative\ Land\ Supply}_{c,t-1} + \mu_X X_{c,t-1} + \nu_t + \varepsilon_{c,t} \quad , \tag{4}$$

where μ_c denotes a city fixed effects, η_c the city-specific elasticities, and ν_t the year fixed effects. $X_{c,t-1}$ represents a set of city-level macroeconomic control variables including annual city-level statistics for the (log) gross domestic product ($\ln GDP$), (log) city population (ln *Population*), the annual (log) expenditure (ln *Gov. Expenditure*) by the city government, its annual (log) revenue (ln *Gov. Revenue*), the share of government budget deficit to GDP (*Gov. Deficit*) and the percentage of park area within the urban area (*Park Share*) in year $t-1$ that could affect government's land supply in year t. To improve the estimation quality of this random effect model, we extend the sample period in length to the period 2001-10. But as the city-specific elasticity $\widehat{\eta}_c$ is itself estimated before we combine it into the interacted instrument $\ln$*Relative Land Supply*$_{c,t-1} \times \widehat{\eta}_c$, the conventional standard errors of the 2SLS procedure no longer apply. We check robustness by (block) bootstrapping the standard errors at the city-level and report bootstrapped standard errors in Internet Appendix Table B2.

Estimating a large number of 202 city-specific elasticity parameters $\widehat{\eta}_c$ could amount to "overfitting" the first stage regression and overstate the actual strength of our instruments. We dispose of only 10 annual observations to estimate a city-specific elasticity and this implies considerable estimation error. We therefore propose two alternative specifications. First, we imposes the restriction that the price elasticity of housing supply is identical across all cities, hence $\widehat{\eta}_c = \widehat{\eta}$. In this "pooled elasticity" specification, we use the (log) relative land supply directly as our instrument.[13] Second, we sort city elasticities $\widehat{\eta}_c$ into four quartiles, and estimate a joint "quartile elasticity" $\widehat{\eta}_{Q(c)}$ for each of the four (sorted) city groups. Replacing the city-specific elasticity $\widehat{\eta}_c$ by the quartile elasticity $\widehat{\eta}_{Q(c)}$ provides a specification that is both flexible and parsimonious. We use this latter approach to construct our preferred instrument called the *Adjusted Land Supply*; formally

$$Adjusted\ Land\ Supply_{c,t-1} \equiv \ln Relative\ Land\ Supply_{c,t-1} \times \widehat{\eta}_{Q(c)} \tag{5}$$

The first stage regression then follows as

$$\ln P_{c,t} = \mu_c + \mu_{IV}\ Adjusted\ Land\ Supply_{c,t-1} + \mu_X X_{c,t-1} + \nu_t + \varepsilon_{c,t} \tag{6}$$

Figure 4 compares the fit of the first stage regression under the "pooled elasticity" (Panel A), the "city-specific elasticity" (Panel B), and the "quartile elasticity" (Panel C). Table 3 reports

[13] Here, standard errors do not require any bootstrapping for the correct inference. Note that the coefficient μ_{IV} directly identifies the pooled elasticity parameter $\widehat{\eta}$.

the corresponding three regression specifications. The data rejects the pooling assumption that the price effect of land supply variations is identical across Chinese cities. The convention F-value of the city-specific elasticity" specification in Column (2) is 165.6 compared to only 15.0 for the "pooled elasticity" specification in Column (1). However, pooling over only four quartiles in the "quartile elasticity" specification generates an even higher F-value of 199.6. This is our preferred specification for the first stage regression as it accounts for city heterogeneity based on only four free parameters.

All three instrumental variable strategies are based on the *Relative Land Supply*, which measures the land surface of the new residential housing supply, but does not capture potential quality differences of housing, the density of construction, or the attractiveness of the location. Moreover, housing demand may also depend on speculative buying in anticipation of future capital gains. Finally, the transformation of constructible land into sold housing units can take more or less than one year. All this (unobserved) supply heterogeneity should enter the unit price $\ln P_{c,t}$, and can generate a more "noisy" estimates $\widehat{\eta}_c$.

In our data, roughly 25% of the city-level elasticity estimates are positive, which is a not plausible result as a rising land supply is expected to decrease housing prices. However, several aspects might account for the positive estimates. First, we dispose only of approximately ten yearly observations to estimate the demand elasticity of any particular city; hence, estimation error can account for positive point estimates. Only by pooling observations over similar cities can we obtain a more efficient point estimate. Second, our housing prices measure is not quality-adjusted. For example, if average housing quality increases because new commodity housing is of higher quality, then land supply shocks can produce higher average prices if this quality upgrading is ignored. Positive elasticity estimates then reflect a measurement error with respect to housing quality. Similarly, if local governments supply more constructible land in particularly attractive locations or provide better neighborhood amenities, average city-level housing transaction prices can increase along with land supply. Third, some cities experience housing speculation, where investors acquire residential property without using or renting it. Such speculative activity increases prices without alleviating supply shortages. In additional robustness tests, we present more results that do not involve any elasticity estimates and also separately examine the effect of real estate booms in cities with negative and positive elasticity estimates.

When using the pooled elasticity instrument, the 2SLS estimates will particularly exploit firms in cities with more negative elasticity estimates. However, when using city-specific or quartile-specific elasticity instruments, the 2SLS estimates with exploit firms in these cities relatively less, as the explanatory power of the first stage from cities with less negative elasticity estimates becomes larger. As discussed, positive elasticity estimates may mainly come from measurement issues, which may produce 2SLS estimates close to zero and statistically insignificant. Table A2 provides the corresponding comparison and confirms that the effect of real estate booms on firm outcomes is larger among cities with negative elasticity estimates

5 Empirical Analysis

5.1 Baseline Results for Firm Outcomes

The first step in the empirical analysis is to verify the negative effect of the real estate price level $\ln P_{c,t}$ in city c on the local firm outcomes as stated in Proposition 1. We use a linear panel regression

$$y_{j,t} = \beta_0 + \beta_p \ln P_{c,t} + \beta_X X_{c,t-1} + \lambda_j + \nu_t + \epsilon_{j,t}, \tag{7}$$

where $y_{j,t}$ represents a set of firm-level outcome variables. We control macroeconomic variables $X_{c,t-1}$ at the city-level used in Table 3. The firm fixed effects λ_j absorb time-invariant firm or city features; time fixed effects ν_t absorb yearly shocks to all firms. The error term $\epsilon_{i,t}$ is clustered at city-level to address the concern that standard errors among manufacturing firms within the same city are positively correlated.

Local capital scarcity induced by real estate booms implies lower firm investment, lower levels of bank lending to firms, less output, and lower labor productivity. The corresponding panel regressions are reported in Table 4. Panel A provides the OLS results. Panel B reports the simple 2SLS regressions that instrument the (log) real estate price level $\ln P_{c,t}$ with the pooled elasticity instrument (see Column (1) in Table 3). Panel C instead uses the city-specific elasticity instrument (see Column (2) in Table 3). Panel D then uses the quartile elasticity instrument-Adjusted Land Supply (see Column (3) in Table 3), and Panel E extends 2SLS regression with additional industry-year fixed effects with this instrument. It is worth mentioning that all three instrumental variables give similar qualitative results. Only the pooled elasticity instrument is weaker and leads to larger 2SLS estimates.

The higher real estate price $\ln P_{c,t}$ has a strong negative effect on gross investment rates $(I/K)_{j,t}$ in all 2SLS regressions. The magnitude of coefficient in the 2SLS regression using the pooled elasticity instrument triples compared to the OLS estimate. This difference between OLS and 2SLS estimates is plausibly explained by the following two effects: First, unobserved positive technology and demand shocks can stimulate corporate investment and housing price inflation simultaneously and bias OLS estimates upwardly. Second, better manufacturing firm performance can contribute to a local real estate boom—thus also delivering a higher OLS estimate. Both endogeneity concerns apply equally to the OLS estimates for other firm outcomes.

The low Kleibergen-Paap F-statistics of around 7.0 in Panel B raises concerns about a weak instrument problems. Panels C and D avoid this problem by using the city-specific and quartile elasticity instruments, respectively. This yield much stronger instruments. The point estimates are smaller, but statistical significant at the 1% level. A 50% higher real estate price implies a decrease in the average firm investment rate by 8.6 percentage points $[= -0.213 \times \ln(1.5)]$ in Panel D, which is large compared to a mean sample value of 35 percentage points.[14] In Panel E, we include additional industry-year fixed effects. Here, we then compare firms within the same industries, but are subject to differential local real estate booms. The point estimates decreases only slightly. The strong negative investment effect remains if we consider the net investment rate which accounts for depreciation.

Column (2) provides direct evidence that booming real estate markets curtail local bank lending to manufacturing firms. A point estimate of -0.096 in Panel D implies that a 50% higher real estate price reduces the percentage of firms with bank credit by 3.9 percentage points $[= -0.096 \times \ln(1.5)]$ relative to a sample mean of 34.0 percentage point of firms with bank credit. Real estate investment booms therefore increase the share of credit constrained firms.

Columns (3) and (4) show the effect of real estate prices on (log) value added output $\ln Y_{j,t}$ and (log) labor productivity $\ln(Y/L)_{j,t}$, respectively. All 2SLS estimations in Panels B to E document a dramatic decrease in both value added output and labor productivity under higher

[14] One standard deviation in the log housing price index $\ln P_{c,t}$ is 0.466 and corresponds to a 59% $[= e^{0.466} - 1]$ change to the housing price level. Thus, a 50% price variation is slightly smaller than a one standard deviation of the log housing price index.

(instrumented) real estate prices $\ln P_{c,t}$. A 50% higher real estate price induces an output decrease of approximately 39% [$= -0.963 \times \ln(1.5)$] in Panel D. Labor productivity $\ln(Y/L)_{j,t}$ decreases by a similar magnitude.

5.2 Factor Price Response to Housing Price Inflation

Having confirmed the predicted firm responses to real estate booms qualitatively, we test the responses of factor prices articulated in Proposition 2 using the baseline specification. Table 5, Column (1) shows the OLS estimates for the interest rate on bank loans. The point estimate is positive at 0.008 and marginally significant at the 1% level. Yet, various economic channels may simultaneously influence local interest rates and the real estate price level. For example, local productivity shocks could increase local interest rates and higher interest rates could moderate local housing price inflation. Column (2) therefore proceeds to the 2SLS regression that instruments variations in the local real estate price with the *Adjusted Land Supply* based on the "quartile-elasticity". Under this 2SLS specification, the point estimate increases to 0.016 and is statistically significant at the 5% level. This coefficient implies that an increase of the local real estate price by 50% increases the capital costs of local firms by approximately 0.65 percentage points [$= 0.016 \times \ln(1.5)$], which is large compared to a mean sample value of 6.3 percentage points ($0.65/6.3 = 10.3\%$ of the sample mean). This represents an economically highly significant factor price effect that should deter capital investment. We also estimate an extended specification that controls for the industry-year fixed effects which absorb time-variant industry-specific shocks in Column (3), the coefficient for the interest rate effect of real estate inflation is similar at 0.015. The main transmission channel of underinvestment is therefore both the capital cost increase and the economically significant increase of manufacturing firms without bank credit access.

The factor price effect of real estate prices on real wages is documented in Table 5, Columns (4)–(6). The OLS estimate in Column (4) is negative at -0.131 and statistically significant. Various economic channels can bias the OLS estimate upward. First, higher local wages can increase household income and also push up real estate demand and prices. Second, (omitted) economic shock can produce a positive correlation between local wages and local housing prices. To address these issues, we once again use the 2SLS estimator reported in Columns (5)–(6), which features much more negative point estimates at -0.468 and -0.454, respectively. Here, a 50% increase in real estate prices is associated with 19% [$= -0.468 \times \ln(1.5)$] decrease in real wages.

The percentage wage effects of local capital scarcity is quantitatively larger than the percentage interest rate effect, even though Proposition 2 (see Appendix A) predicts the opposite as

$$\frac{\widehat{i}}{\widehat{w}} = -\frac{\mu}{1-\mu} \gtrsim 1, \tag{8}$$

where $\mu \gtrsim 0.5$ corresponds to the (average) labor share of production. We note that credit substitution away from small firms could be reflected more in credit rationing than in higher firm loan rates — something not captured by the neoclassical model. We also highlight that some firms benefited from regulated bank loan rates during our sample period, which should also attenuate the measured ratio.

The negative effect on firm's labor productivity can rationalize the real wage decline. Quantitatively, the (percentage) real wage changes ($\widehat{w}$) should roughly match the change in labor productivity of tradables. Yet we find in Table 4 a larger coefficient for the reduction in labor productivity, $(\widehat{Y/L})_T = -0.96 \times \widehat{P}$, compared to real wage decline $\widehat{w} = -0.47 \times \widehat{P}$ reported in Table 5. This discrepancy could be explained by wage rigidity and other labor market frictions outside our neoclassical model framework. But we highlight the correct sign of both effects consistent with the model.

5.3 Robustness checks: alternative specifications

In this subsection, we conduct several robustness checks for the effect of real estate booms on firm performance and factor prices. In Panel A, Table A1, we use the variable ln*Relative Land Supply*$_{c,t-1}$ as the direct explanatory variable, which is often referred to as the reduced form effect. Although these estimates are OLS, they should be unbiased, suppose (lag) residential land supply is exogenous to manufacturing firm performance. Meanwhile, this does not require interacting with housing demand elasticities. The results show an opposite effect of increasing (residential) land supply compared with increasing housing prices, complementing the 2SLS results from the structural form estimation.

Our theoretical framework predicts how different outcomes respond to the change in house prices $\widehat{P}$ instead of the absolute level P, motivating us to include individual (firm or city) fixed effects. While our baseline regressions control firm fixed effects, we instead control city fixed effects in Panel B which is less demanding. The effect of real estate booms on most outcomes remains, but becomes slightly weaker.

In addition, we seek to control for additional industry- and city-specific time-variation. Specifically, we define three dummies for the local housing market, namely $D\_HM^{H}_{c,t} = 1$ (and zero otherwise) if a city's yearly house price growth is larger than 15%; $D\_HM^{L}_{c,t} = 1$ (and zero otherwise) if a city's yearly house price growth is negative; and $D\_HM^{M}_{c,t} = 1$ (and zero otherwise) if the city's housing market status is between two extremes. In Panel C, we further control the interaction of city-year-specific housing market dummies with industry dummies, which thus absorb all industry-specific shocks when cities are in different stages of housing market cycles. While the instrument becomes weaker, the results show even larger economic magnitudes for most outcomes.

Furthermore, roughly 25% of cities have implausible positive housing demand elasticity estimates, and such noisy elasticity estimates are not suitable for instrument construction. What happens if we discard them? Table A2 separates firms in cities with negative and positive elasticity estimates and undertakes two separate regressions. We find that the negative effect of real estate booms is most pronounced in cities with negative elasticity estimates across all estimation methods—namely the reduced form approach, OLS estimation, 2SLS estimation with pooled elasticity, and 2SLS with quartile-specific elasticity instruments. Moreover, the point estimates for cities with negative demand elasticities are more negative than for the full sample.

We note that estimating the effect of aggregate variables on micro units could raise a number of heteroscedasticity issues as discussed in Moulton (1990). We thus implement a two-stage estimation method proposed by Combes *et al.* (2008, 2019), which can mitigate such concerns. Specifically, we regress firm-level outcomes on city-year fixed effects in the first stage. In the second stage, we regress estimated city-year fixed effects from the first stage on housing prices conditional on city observables, city and year fixed effects. Table A3 shows that the obtained 2SLS estimates under this strategy give similar point estimates for the key outcome variables, namely -0.206 (instead of -0.213) for the investment share, -0.131 (instead of -0.096) for loans, and -0.662 (instead of -0.932) for (log) output. However, the standard errors are now larger due to the lower number of city-year observations relative to the firm-year panel.

The standard error increases most for the investment share from 0.053 to 0.137. Hence, the 2SLS estimate of the negative investment effect is no longer statistically significant at the five percent level, even though the point estimate is almost unchanged. The reduction of statistical power due to the projection of firm-year observations into a city-year panel can explain a lower statistical significance level of the coefficients. Overall, we conclude that these results are still broadly supportive of our main hypotheses, even if conventional levels of statistical significance cannot be obtained for all outcome variables.

Finally, we consider two alternative measures of land supply to construct quartile-specific elasticity instruments, and Internet Appendix Table B3 provides the 2SLS results in Panels A and B, respectively. In Panel A, we use the city's initial share of non-industrial land supply to infer its residential land supply and find robust results. In Panel B, we use the (log) absolute land supply without normalization by the housing stock. While the harmful effects of real estate booms remain, the economic magnitudes decrease, which means that the precise specification of the instrument affects our estimation results quantitatively. We think that the normalized land supply is a more appropriate choice because the land supply affects housing prices through housing supply, which is best quantified relative to an existing housing stock. The same new housing supply creates less price pressure if the housing stock in a city is large. Aligned with this reasoning, we find a stronger price decreases if we use the relative land supply as our instrument.

5.4 Firm Heterogeneity in Credit Access

A major market friction in China consists in unequal firm access to credit. Hypothesis 1 argues that firms with larger fixed assets and SOEs should be less affected by local capital shortages brought about by real estate booms. Previous research has highlighted the privileged capital market access of SOEs in China (Allen *et al.* 2005). Access to credit from the "big five" national banks should greatly reduce the dependence of large (asset rich) firms and SOEs on local credit market conditions, which in turn eliminates their capital cost exposure to local real estate booms.

Table 6 provides evidence to support this hypothesis. In Panel A, we interact the real estate price $\ln P_{c,t}$ with a firm's log fixed assets ($\ln Fixed\ Assets_j$) at the beginning of the sample. Panel B interacts the real estate price with a dummy variable marking SOEs (SOE_j). We expect to find higher investment rates for less financially constrained firms as well as lower output and labor productivity decline. Column (2) confirms that firms with more fixed assets (Panel A) and SOEs (Panel B) do indeed face a smaller or no decline in access to bank loans. Accordingly, their investment rates $(I/K)_{j,t}$ hold up much better under local real estate booms than their more financially constrained peers in the same industry. For example, the average SOE shows a reduction in the investment rate of only 2 percentage points $[= (-0.232 + 0.182) \times \ln(1.5)]$ for a 50% higher local real estate price relative to an investment shortfall of 9.4 percentage points $[= -0.232 \times \ln(1.5)]$ observed for privately-owned firms. We also note that firms with better financial market access feature lower output and labor productivity decline. It is likely that fixed assets capture other firm characteristics related to size instead of financial constraints (e.g., larger firms receive more government support). In Table A4, we also include the interaction of housing prices with the firm's initial value-added output size. We find that the harmful effects of real estate booms are only weaker among firms with more fixed assets, which supports the credit substitution channel.

It is interesting to show the long-run differential performance of privately-owned firms and SOEs as a function of predicted local real estate inflation. Figure 5 shows the average (log) value added output change from 2002 to 2007 at the city-level for all privately-owned firms (blue crosses) in Panel A, and all SOEs (red squares) in Panel B. Formally, we define city-level aggregates

$$\ln Y_{c,2007}^{type} - \ln Y_{c,2002}^{type} = \frac{1}{N_c^{type}} \sum_{j \in C,\ j \in Type} \ln Y_{j,2007} - \ln Y_{j,2002}\ , \tag{9}$$

where C represents the set of all firms headquartered in city c, N_c^{type} the number of firms in city c of a particular type, and firm *type* can be a private-owned firm or a SOE. The x-axis represents the instrumented log real estate inflation index relative to the initial (log) real estate price in 2002, i.e. $\Delta \ln \widehat{P} = \ln \widehat{P}_{2007} - \ln \widehat{P}_{2002}$. Subtracted from the (log) output change are firm and year fixed effects. The growth experience of privately-owned firms in Panel A shows a strong negative dependence on relative real estate price growth. The growth of SOEs in a city is visibly less affected by the overall change in real estate prices over the five year period.

To further show that large firms and particularly listed companies with access to the national credit market are generally shielded from the credit substitution effect, we undertake asset-weighted 2SLS regressions and also repeat the baseline regressions for listed companies only.

Internet Appendix Table B4 reports the respective results. As expected, the asset-weighted regressions in Columns (1b)-(6b) yield point estimates near zero that are statistically insignificant. Similarly, Columns (1c)-(6c) find no real effects of real estate booms on listed companies headquartered in the respective location as one might expects if these firms can access the national capital market. This implies that the underinvestment problem is concentrated in small manufacturing firms. As a consequence, any alternative channel linking real estate booms to firm underinvestment also has to explain why the latter is limited to small firms.

In addition to firm-level financial constrains, we also consider variations in regional bank-dependence in Panel C, Table 6. We calculate each province's ratio of firm fixed investment financed by bank loans to total fixed investment in the year 2000, and define a dummy variable $Bank\ dependence_p$ equal to one (and zero otherwise) for those provinces above the sample median.[15] Intuitively, if real estate booms harm firm growth through credit substitution, firms in more bank-dependent provinces should be more affected. Combined results show that the negative effects of the real estate booms on investment, credit access, and output are indeed more pronounced for firms in bank-dependent provinces. These findings are also consistent with a bank credit supply channel that is disrupted by credit diversion in a real estate booms. Additional untabuluated results find that the large negative investment sensitivity of non-SOEs relative to SOEs is particularly pronounced in the most bank-dependent locations. Higher bank dependence of the local economy increases the competitive advantage of SOEs in obtaining bank credit via the national banking system and thus increases the differential investment sensitivity between the private and public sector.

Additionally, we explore the role of the so-called collateral channel on firm investment, which concerns firms with real estate assets (Chaney *et al.*, 2012). The ASIF data do not report real estate property on the firm balance sheet, but provide a larger category of non-operating assets which subsumes the former. We define a dummy variable $D\_\ Collateral_j$ equal to one (and zero otherwise) if a firm at the start of the sample period has more than 50% of its total fixed assets invested in non-operating assets. In Internet Appendix Table B5, Columns (1)-(2), we show that the 7.4% of firms marked by the dummy $D\_\ Collateral_j = 1$ show indeed a 30.9%

[15] This measure ranges from 10.2% for Shandong province to 30.6% for Guizhou province with a median value of 19.9%. A few provinces report the funding sources of local fixed investment only after the year 2000. However, this data shortcoming should not affect our sample split as these cases are not close to the sample median and can be sorted into one of the two groups with high confidence.

[$= 0.067/0.217$] smaller decline in their gross investment rate than the rest of the sample during real estate booms. However, this point estimate is not statistically significant. Moreover, the rarity of real estate assets on corporate balance sheets greatly diminishes the macroeconomic significance of the collateral effect. Consistent with our results, Wu *et al.* (2015) find no collateral channel for Chinese listed companies, unlike what has been reported for the United States and Japan. They also provide suggestive evidence that state-owned banks have strong enforcement channels so that pledging real estate as a security is of secondary importance in China's unique financial system.

5.5 Additional Firm Performance Measures

Higher capital costs and underinvestment for firms in locations with real estate booms predict additional negative effects on firm performance measures. Hypothesis 2 conjectures lower firm profitability (ROA), lower (log) total factor productivity, and higher likelihood of exit. Table 7 reports panel regressions for all both performance measures. The OLS coefficients are provided in Columns (1), (4), and (7). The 2SLS results for the baseline specification are given in Columns (2), (5), and (8), whereas Columns (3), (6), and (9) add interaction effects for financially unconstrained (asset rich) firms and SOEs.[16]

Columns (1)–(3) show a negative effect of real state investment booms on firm profitability measured by the return on assets (ROA). The 2SLS point estimate of -0.141 in Column (2) implies that a 50% higher real estate price reduces ROA by 5.7 percentage points [$= -0.141 \times \ln(1.5)$] relative to the sample mean of only 7.4 percentage points. Column (3) shows that asset rich firms and SOEs are less affected as indicated by the positive coefficient of 0.009 and 0.045 for the interaction terms $\ln P_{c,t} \times \ln Fixed\ Assets_j$ and $\ln P_{c,t} \times SOE_j$, respectively.

The effects of high capital costs and relative underinvestment on TFP levels are also economically significant. The average manufacturing firm features a 2SLS coefficient of -0.285 in Column (5), which implies that a 50% increase in real estate prices translates into a TFP shortfall of 11.6% [$= -0.285 \times \ln(1.5)$]. Hence, firms in locations with real estate booms suffer a considerable decline in industrial competitiveness. The positive interaction coefficients of 0.018

[16] For expositional simplicity, we include all three interaction terms in the same regression in Table 7. Alternatively, one can run separate regressions including only on interaction term at the time. Yet, this gives quantitatively similar results and confirms the finding of more harmful effects on smaller firms, non-SOEs, and firms in more bank-dependent regions.

and 0.020 for asset rich firms and SOEs in Column (6) imply that this average effect varies considerably with firm characteristics. Meanwhile, the harmful TFP effect is much stronger among firms located in bank-dependent provinces.

Our results on the adverse effect of local credit constrains on relative productivity growth are related to recent findings by Manaresi and Pierri (2018), who trace a quarter of the productivity slowdown in Italian firms in 2007–2009 to worsening credit conditions which imply slower IT-adoption, lower export growth, and slower managerial improvements.

The real effects of capital scarcity induced by local real estate booms for local manufacturing firms are therefore dramatic in their economic magnitude — causing a substantial (relative) industrial decline for firms located in cities with real estate booms. Also, we expect such industrial decline to be reflected in firm exit rates. These exiting firms tend to have lower productivity and profitability compared with non-exiting firms. The 2SLS estimate in Column (8) shows that this effect is statistically significant at 5% level and shows a significant economic magnitude: A 50% increase in the real estate price increases the probability of firm exit by approximately 3.5 percentage points [$= 0.087 \times \ln(1.5)$].[17] This implies that real estate booms hurt manufacturing sector also via the extensive margin. Finally, we show in Column (9) that market *Exit* for firms located in booming real estate markets is considerably more likely for firms with fewer fixed assets, which is consistent with the credit supply channel.

6 Instrument Choice and Robustness

6.1 Credit Supply versus Credit Demand Shocks

Local real estate booms could change the demand for locally produced manufacturing goods either positively through a wealth effect or negatively by substituting product demand for housing demand (Cloyne *et al.*, 2019). For example, Waxman *et al.* (2020) find that rising housing prices cause Chinese households to reduce non-housing spending—possibly due to tighter household budget constraints resulting from higher mortgage payments. However, the positive effect on corporate interest rates and the stronger harmful effect on financially constrained small firms cannot be attributed to such a demand channel. While adverse demand effects can also depress

[17] The overall annual firm exit rate in the sample is high at 9%. Firms exit from the ASIF data whenever their sales drops below a threshold of 5 million RMB and this may not imply firm closure.

local firm growth, they should spread across firms serving the local economy, but not concentrate in financially constrained manufacturing firms.

To further explore the demand channel, we carry out additional investigations. First, we extend our analysis by considering firm exports as a measure of firm performance. The identifying assumption is that credit supply shocks adversely affect output independently from the product destination, whereas demand effects are local and do not extend to exports. The Chinese customs authorities collect a comprehensive product-level data set on firm exports that account separately for product price and quantity of exporting firms. We aggregate similar products (in the same measurement units) into a single product category by value and unit price. For pure exporters with reported gross sales equal to exports, we consider the export statistics as a suitable (real) performance measure devoid of any local demand effect. On average, these firms export in 3.9 different product categories.

Table A5 analyzes Chinese firms' (real) export performance as a function of local real estate prices. Column (1) first confirms a significant negative output effect for the subsample of pure exporters. The magnitude is smaller than the full sample, but remains large, which might be somewhat surprising as exporters are usually larger than non-exporters and should be less harmed. Untabulated results find that exporters are larger only in employment but not in fixed assets, suggesting they are not necessarily less financially constrained. Meanwhile, exporters face global competition. Hence credit cost externalities originating in the real estate sector might be more difficult to passed on to the respective (global) output market. Column (2) then estimates a firm's (log) export value ($\ln ExpValue_{i,j,t}$) as a function of the local housing price. The estimated export elasticity is large at -0.448. The relative decline in export value amounts to 18.2% [$= -0.448 \times \ln(1.5)$] for a 50% increase in local housing prices.[18] A relatively high estimate for the export elasticity supports the credit supply channel because export demand is presumably unrelated to local Chinese real estate prices. Columns (3) and (4) decompose the export value ($\ln ExpValue_{i,j,t}$) into the export quantity ($\ln ExpQuantity_{i,j,t}$) and the export price ($\ln ExpPrice_{i,j,t}$), respectively. While both estimates are statistically insignificant, the coefficient on the export price is close to zero, implying that most of the export effect comes from quantity decline rather than pass-through.

[18] The export is the gross output sales, but the value-added output is the gross output net of intermediate inputs. This difference explains the discrepancy between the output and export effects.

Second, we alternatively measure the tradability of firm output at the industry level by the industry-specific export share at the beginning of the sample. Firms in more tradable industries face a national or international product demand rather than a local one. As a consequence, they are less subject to local demand changes related to a local real estate boom. Internet Appendix Table B6 shows that a high export share comes with a stronger investment and credit decline as such firms suffer from the saving displacement to real estate investment without benefiting much from the local housing boom. In contrast to the adverse investment and credit effects, harmful output effects appear less contingent on firms' financial constraints and industry tradability. This suggests that even firms with sufficient collateral assets in non-tradable industries suffer from local real estate booms regarding their output growth, which points to the demand channel. Unfortunately, it is not possible to identify more precisely the firms that rely most on local demand due to limited information on the sales destination in the ASIF. In conclusion, we do not exclude an economically significant parallel negative demand channel. But we also highlight the critical insight from our modified Harrod-Balassa-Samuelson framework that real estate booms can hurt firm growth by creating local capital shortages.

Third, one might worry that increasing house prices could trigger labor cost inflation and further hurt firm growth. However, Table 5 finds a positive effect on corporate interest rates and a negative wage effect rather than the opposite. Unlike in the so-called "Dutch Disease" phenomenon, we can exclude adverse labor cost effects as empirically important for the decline of the local manufacturing sector.

Lastly, we investigate heterogeneous effects across firms with differential wages and labor productivity in Internet Appendix Table B7. The results indicate a stronger harmful effect on labor productivity on firms with higher initial wages or labor productivity but this pattern does not apply to corporate investment and output. Therefore, it is unlikely that the heterogenous responses across financial constraints are driven by differential ex-ante labor costs or labor productivity. However, it seems that firms with more outputs are more harmed in investments and output, but not in credit access, implying channels other than the credit substitution, such as adverse demand shocks.

6.2 Housing Supply Elasticity as an Alternative Instrument

Following Mian and Sufi (2011, 2014), Mian *et al.* (2013) and Adelino *et al.* (2015), we also undertake a specification with time-invariant local housing supply elasticity as instrument. The basic idea is that cities with an elastic housing supply experience only modest housing price changes as they can quickly absorb housing demand shocks through new housing construction, while cities with an inelastic housing supply encounter stronger price increases. As a first-stage regression, we use

$$\Delta^{02-07} \ln P_c = \mu_0 + \mu_1 SupplyElast_c + \varepsilon_c \ . \tag{10}$$

The dependent variable is no longer the yearly log housing price index, but its change over the entire period 2002–2007. As the city-specific elasticity is time-invariant, this specification dispenses with city and year fixed effects. For data on the housing supply elasticity $SupplyElast_c$, we draw on Wang *et al.* (2012), who estimate the response of new housing construction to price shock for 35 major cities in China for the period 1998–2008. Internet Appendix Table B8 reports some their elasticity estimates for the five locations with the largest (*Top 5*) and lowest (*Bottom 5*) values. The cities with lower elasticity (inelastic construction supply), such as Shenzhen and Beijing, experience a greater increase in housing prices in 2003–2010 and feature the highest overall price levels in 2010, as shown in Columns (2) and (3), respectively. By contrast, cities with a higher supply elasticity, such as Yinchuan and Changsha, experience a modest increase in housing prices over the same period, and show much lower price levels as of 2010. Figure B4 in the Internet Appendix shows a strong negative relationship between the (log) change of housing prices in the period 2002–2007 and the respective housing supply elasticities. The t-value for the (first-stage) regression line is above 5 and R-squared is above 40%, indicating a reasonably strong instrument.

The second-stage regression is also reduced to a pure cross-sectional specification given by

$$\Delta^{02-07} y_j = \beta_0 + \beta_p \Delta^{02-07} \ln P_c + \beta_X X_c + \xi_{ind} + \epsilon_j \ \ , \tag{11}$$

where outcome variables $\Delta^{02-07} y_j$ are the gross investment rate change $[\Delta^{02-07}(I/K)_j]$; the change in firm share with bank loans $[\Delta^{02-07} Loan_j]$, in (log) value-added output $[\Delta^{02-07} \ln Y_j]$, in (log) labor productivity $[\Delta^{02-07} \ln(Y/L)_j]$; factor price changes given by the firm bank loan rate change $[\Delta^{02-07} i_j]$ and the (log) wage change $[\Delta^{02-07} \ln w_j]$; the change in firm profitability $[\Delta^{02-07} ROA_j]$, and the change in (log) TFP $[\Delta^{02-07} \ln TFP_j]$.

The city-level controls X_c include the GDP per capita, population density, employment share of the secondary sector and GDP share of the secondary sector in 2002 to capture differences across cities at the starting date of the sample period. We also control for two-digit industry fixed effects ξ_{ind} to capture heterogeneity by industry. Table A6 reports the results for this alternative specification with different (time invariant) instruments. The number of (cross-sectional) observations decreases considerably because local housing supply elasticities are available for only 32 cities and a smaller number of firms operate in these locations for the full period 2002–2007.

Column (1) is consistent with the result in Table 4: housing price inflation lowers firms' gross investment rate at high levels of economic and statistical significance. The point estimate of -0.255 is slightly larger than the comparable coefficient of -0.198 in Table 4, Panel E, Column (1). Column (2) confirms the negative relationship between (instrumented) housing prices and firms' bank loan acquisition even though the coefficient is statistically insignificant. Columns (3) and (4) confirm the negative effect of housing inflation on firm output and labor productivity with similar magnitudes as results in Table 4. Column (5) confirms that firms in cities with greater housing price increase $[\Delta^{02-07} \ln P_c]$ experience an increase in their bank loan rate $\Delta^{02-07} i_j$ with a similar magnitude as in Table 5, Column (3). For wage growth $[\Delta^{02-07} \ln w_j]$ in Column (6) we confirm the negative coefficient of similar magnitude as in Table 5, Column (6). Very similar economic effects are obtained for ROA and TFP, as shown in Columns (7) and (8). Overall, the pure cross-sectional specification confirms the baseline estimates using the quartile elasticity instrument and firm fixed effects.

7 Conclusion

This paper addresses the important question of whether real estate investment booms can crowd out corporate investment and thus impact long-run corporate competitiveness and growth. We argue that China's state monopoly in residential land supply and its geographically segmented market for small and medium-size firm credit represent an ideal empirical setting to address this question: quasi-exogenous variations in local land supply policies provide an instrument that can partially account for the large (intertemporal) variation of real estate prices across Chinese cities in the period 2002–2007. Real estate price increases traced to exogenous land supply variation can proxy for local capital scarcity as more local credits are channeled into real estate investment rather than corporate investment.

Based on a sample of 202 prefecture-level cities in China, we show that local real estate booms constrain bank credit for small manufacturing firms and cause strong underinvestment relative to industry peers located in cities with lesser real estate price growth. The initial lack of capital in China's private sector combined with large investment opportunities after China's WTO accession made local funding condition a particularly important determinant for small firm growth: For a 50% higher real estate price, the corporate investment rate drops by 8.6 percentage points (relative to a mean of 35 percentage points) and value-added firm output is lower by a large 37.8%. These findings highlight that heterogenous firm funding conditions give rise to very different real firm outcomes.

Our paper contributes to a new macroeconomic literature on the effects of depressed bank borrowing on firm competitiveness and economic growth (Amiti and Weinstein, 2011; Chodorow-Reich, 2013; Paravisini *et al.*, 2014; Cingano *et al.*, 2016; Bentolila *et al.*, 2017; Acharya *et al.*, 2018; Huber, 2018). Much of this literature has relied on bank distress in the recent financial crisis as the source of identification. We add an entirely different experience to this literature by showing that corporate investment can be depressed due to a rival use of local savings in the absence of any bank distress. Here, we build on recent work on bank credit substitution caused by real estate booms (Chakraborty *et al.*, 2018).

From a welfare perspective, capital allocation to the investment of highest return is certainly a desirable outcome unless this (temporarily) high return is itself a consequence of 'irrational exuberance'. But even a locally optimal capital allocation between corporate and real estate investment is globally distorted if manufacturing firms face very heterogenous capital costs due to capital market segmentation while competing in the same product market. Such distorted product market competition seems potentially more pernicious than distorted real wages for non-tradable products in the traditional Harrod-Balassa-Samuelson world.

Some of the previous finance literature highlights the benefit of real estate booms on entrepreneurial activity through a collateral channel that alleviates credit constraints (Corradin and Popov, 2015; Harding and Rosenthal, 2017; Schmalz *et al.*, 2017; Bahaj *et al.*, 2020). Our evidence from the Chinese experience with real estate booms casts serious doubts on the empirical relevance of such a positive effect. We find that in geographically segmented credit markets, local real estate booms tend to crowd out private entrepreneurial investment with long-lasting negative consequences for the development of the local manufacturing sector. Particularly in highly competitive export industries like China's manufacturing sector, local capital cost externalities cannot be externalized into higher product prices and can therefore be particularly pernicious. From a policy perspective, our evidence points to substantial benefits from credit market integration and a more efficient market-based capital allocation in China.

References

[1] Acharya, V. V., Eisert, T., Eufinger, C., and Hirsch, C. 2018. Real effects of the sovereign debt crisis in Europe: Evidence from syndicated loans. *The Review of Financial Studies*, 31(8), 2855-2896.

[2] Adelino, M., Schoar, A., and Severino, F. 2015. House prices, collateral and self-employment. *Journal of Financial Economics,* 117(2), 288-306.

[3] Allen, F., Qian, J., and Qian, M., 2005. Law, finance, and economic growth in China. *Journal of Financial Economics,* 77(1), 57-116.

[4] Amiti, M., and Weinstein, D. E. 2011. Exports and financial shocks. *The Quarterly Journal of Economics*, 126(4), 1841-1877.

[5] Akcigit, U., and Kerr, W. R. 2018. Growth through heterogeneous innovations. *Journal of Political Economy*, 126(4), 1374-1443.

[6] Balassa, B., 1964. The purchasing power parity doctrine: A reappraisal. *Journal of Political Economy*, 72 (6), 584–596.

[7] Bahaj, S., Foulis, A., and Pinter, G. 2020. Home values and firm behavior. *American Economic Review*, 110(7), 2225-2270.

[8] Bentolila, S., Jansen, M., and Jiménez, G. 2017. When credit dries up: Job losses in the great recession. *Journal of the European Economic Association*, 16(3), 650-695.

[9] Birch, D. L., Mass. 1979. The job generation process (Vol. 302, p. 1979). Cambridge, MA: MIT program on neighborhood and regional change.

[10] Boyreau-Debray, G. and Wei, S. J. 2004. Can China grow faster? A diagnosis on the fragmentation of the domestic capital market. IMF Working Paper 04/76. International Monetary Fund.

[11] Bracke, P., Hilber, C. A., and Silva, O. 2018. Mortgage debt and entrepreneurship. *Journal of Urban Economics*, 103, 52-66.

[12] Brandt, L., Van Biesebroeck, J., and Zhang, Y. 2012. Creative accounting or creative destruction? Firm-level productivity growth in Chinese manufacturing. *Journal of Development Economics*, 97(2), 339-351.

[13] Chakraborty, I., Goldstein, I., and MacKinlay, A. 2018. Housing price booms and crowding-out fffects in bank lending, *Review of Financial Studies*, 31(7), 2806-2853.

[14] Chaney, T., Sraer, D., and Thesmar, D. 2012. The collateral channel: How real rstate shocks affect corporate investment. *American Economic Review,* 102(6), 2181-2409.

[15] Chan, K. S., Dang, V. Q. T., Jiang, M., and Yan, I. K. 2011. On China's domestic capital mobility and the role of the Government: empirical evidence over 1970–2006. *The World Economy*, 34(7), 1216-1236.

[16] Chen, T., Liu, L., Xiong, W., and Zhou, L. A, 2017. Real estate boom and misallocation of capital in China, working paper.

[17] Chen, T., and Kung, J. S. 2016. Do land revenue windfalls create a political resource curse? Evidence from China. *Journal of Development Economics*, 123, 86-106.

[18] Chava, S., and A. Purnanandam. 2011. The effect of banking crisis on bank-dependent borrowers. *Journal of Financial Economics*, 99, 116-135.

[19] Chodorow-Reich, G. 2013. The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *The Quarterly Journal of Economics*, 129(1), 1-59.

[20] Cingano, F., Manaresi, F., and Sette, E. 2016. Does credit crunch investment down? New evidence on the real effects of the bank-lending channel. *The Review of Financial Studies*, 29(10), 2737-2773.

[21] Cloyne, J., Huber, K., Ilzetzki, E., and Kleven, H. 2019. The effect of house prices on household borrowing: a new approach. *American Economic Review*, 109(6), 2024-36.

[22] Corradin, S., and Popov, A. 2015. House prices, home equity borrowing, and entrepreneurship. *The Review of Financial Studies*, 28(8), 2399-2428.

[23] Davidoff, T. 2013. Supply Elasticity and the Housing Cycle of the 2000s. *Real Estate Economics*, 41(4), 793-813.

[24] Davidoff, T. 2015. Supply constraints are not valid instrumental variables for home prices because they are correlated with many demand factors. Forthcoming at *Critical Finance Review.*

[25] Davis, S. J., Haltiwanger, J., and Schuh, S. 1996. Small business and job creation: Dissecting the myth and reassessing the facts. *Small Business Economics*, 8, 297-315.

[26] Ding, C. 2003. Land policy reform in China: assessment and prospects. *Land Use Policy*, 20(2), 109-120.

[27] Döerr, S., Raissi, M. M., and Weber, A. 2018. Credit-supply shocks and firm productivity in Italy. *Journal of International Money and Finance*, 87, 155-171.

[28] Elliott, D., Kroeber, A., and Qiao, Y. 2015. Shadow banking in China: A primer. *Economic Studies at Brookings*, 3, 1-7.

[29] Ersahin, N., and Irani, R. M. 2020. Collateral shocks and corporate employment. *Review of Finance*, 24(1), 163-187.

[30] Fang, H., Gu, Q., Xiong, W., and Zhou, L. A. 2016. Demystifying the Chinese housing boom. *NBER macroeconomics annual*, 30(1), 105-166.

[31] Gao, H., Ru, H., and Yang, X. 2019. What do a billion observations say about distance and relationship lending? SSRN working paper.

[32] Haltiwanger, J., Jarmin, R. S., and Miranda, J. 2013. Who creates jobs? Small versus large versus young. *Review of Economics and Statistics*, 95(2), 347-361.

[33] Hau, H., Huang, Y., and Wang, G. 2020. Firm Response to Competitive Shocks: Evidence from China's Minimum Wage Policy. *Review of Economic Studies*, 87(6), 2639-2671.

[34] Hau, H., Huang, Y., Shan, H., and Sheng, Z. 2021. FinTech Credit and Entrepreneurial Growth, Swiss Finance Institute Research Paper No. 21-47.

[35] Harding, J. P., and Rosenthal, S. S. 2017. Homeownership, housing capital gains and self-employment. *Journal of Urban Economics*, 99, 120-135.

[36] Huang, Y., Pagano, M., and Panizza, U. 2020. Local Crowding-Out in China. *The Journal of Finance*, 75(6), 2855-2898.

[37] Hsu, W. T., Li, X., Tang, Y., and Wu, J. 2017. Determinants of urban land supply in China: How do political factors matter?. *Asian Development Review*, 34(2), 152-183.

[38] Huber, K. 2018. Disentangling the effects of a banking crisis: evidence from German firms and counties. *American Economic Review*, 108(3), 868-98.

[39] Hui, E. C., and Bao, H. 2013. The logic behind conflicts in land acquisitions in contemporary China: A framework based upon game theory. *Land Use Policy*, 30(1), 373-380.

[40] Jiménez, G., Mian, A., Peydró J. L., and Saurina, J. 2020. The real effects of the bank lending channel. *Journal of Monetary Economics*, 115, 162-179.

[41] Kerr, S. P., Kerr, W. R., and Nanda, R. 2022. House prices, home equity and entrepreneurship: Evidence from US census micro data. *Journal of Monetary Economics*, 130, 103-119.

[42] Khwaja, A.I., and A. Mian, 2008, Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market, *American Economic Review* 98, 1413-42.

[43] Li, H., and Zhou, L. A. 2005. Political turnover and economic performance: the incentive role of personnel control in China. *Journal of Public Economics*, 89(9-10), 1743-1762.

[44] Lichtenberg, E., and Ding, C. 2009. Local officials as land developers: Urban spatial expansion in China. *Journal of Urban Economics*, 66(1), 57-64.

[45] Manaresi, F., and Pierri, N. 2018. Credit supply and productivity growth. Bank of Italy working paper.

[46] Mian, A., and Sufi , A. 2011. House prices, home equity-based borrowing, and the US household leverage crisis. *American Economic Review,* 101(5), 2132-56.

[47] Mian, A., Rao, K., and Sufi, A. 2013. Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics*, 128(4), 1687-2026.

[48] Mian, A., and Sufi , A. 2014. What explains the 2007-2009 drop in employment? *Econometrica,* 82(6), 2197-2223.

[49] Michelacci, C., and Silva, O. 2007. Why so many local entrepreneurs? *The Review of Economics and Statistics*, 89(4), 615-633.

[50] Moulton, B. R. 1990. An illustration of a pitfall in estimating the effects of aggregate variables on micro units. *The review of Economics and Statistics*, 334-338.

[51] Neumark, D., Wall, B., and Zhang, J. 2011. Do small businesses create more jobs? New evidence for the United States from the National Establishment Time Series. *The Review of Economics and Statistics*, 93(1), 16-29.

[52] Obstfeld, M., and K. Rogoff. 1996. *Foundations of International Macroeconomics*, MIT Press.

[53] Olley, G. S., and Pakes A. 1996. The dynamics of productivity in the telecommunications equipment industry. *Econometrica,* 64(6), 1263-1297.

[54] Paravisini, D., Rappoport, V., Schnabl, P., and Wolfenzon, D. 2014. Dissecting the effect of credit supply on trade: Evidence from matched credit-export data. *The Review of Economic Studies*, 82(1), 333-359.

[55] People's Bank of China, 2007. Fourth quarter report of China's monetary policy implementation.

[56] Petersen, M. A., and Rajan, R. G. 2002. Does distance still matter? The information revolution in small business lending. *The Journal of Finance*, 57(6), 2533-2570.

[57] Bo, Q. 2008. Preparation and implementation for a land supply plan in Beijing: Problems and countermeasures. *China Land Science*, 6, 51-56.

[58] Rudai, Y. 2015. Study on the total factor productivity of Chinese manufacturing enterprises. *Economic Research Journal*, 2, 61-74.

[59] Samuelson, P. A., 1994. Facets of Balassa-Samuelson thirty years Later, *Review of International Economics*, 2 (3), 201–226

[60] Schmalz, M. C., Sraer, D. A., and Thesmar, D. 2017. Housing collateral and entrepreneurship. *The Journal of Finance*, 72(1), 99-132.

[61] Shi, Y. 2017. Real estate booms and endogenous productivity growth. working paper.

[62] Song, Z., Storesletten, K., and Zilibotti, F. 2011. Growing like china. *American Economic Review*, 101(1), 196-233.

[63] Tian, L., and Ma, W. 2009. Government intervention in city development of China: A tool of land supply. *Land Use Policy*, 26(3), 599-609.

[64] Wang, S., Chan, S. H., and Xu, B. 2012. The estimation and determinants of the price elasticity of housing supply: Evidence from China. *Journal of Real Estate Research,* 34(3), 311-344.

[65] Waxman, A., Liang, Y., Li, S., Barwick, P. J., and Zhao, M. 2020. Tightening belts to buy a home: Consumption responses to rising housing prices in urban China. *Journal of Urban Economics*, 115, 103190.

[66] Wu, J., Gyourko, J., and Deng, Y. 2015. Real estate collateral value and investment: The case of China. *Journal of Urban Economics*, 86, 43-53.

[67] Zilibotti, F. 2017. Growing and slowing down like China. *Journal of the European Economic Association*, 15(5), 943-988.

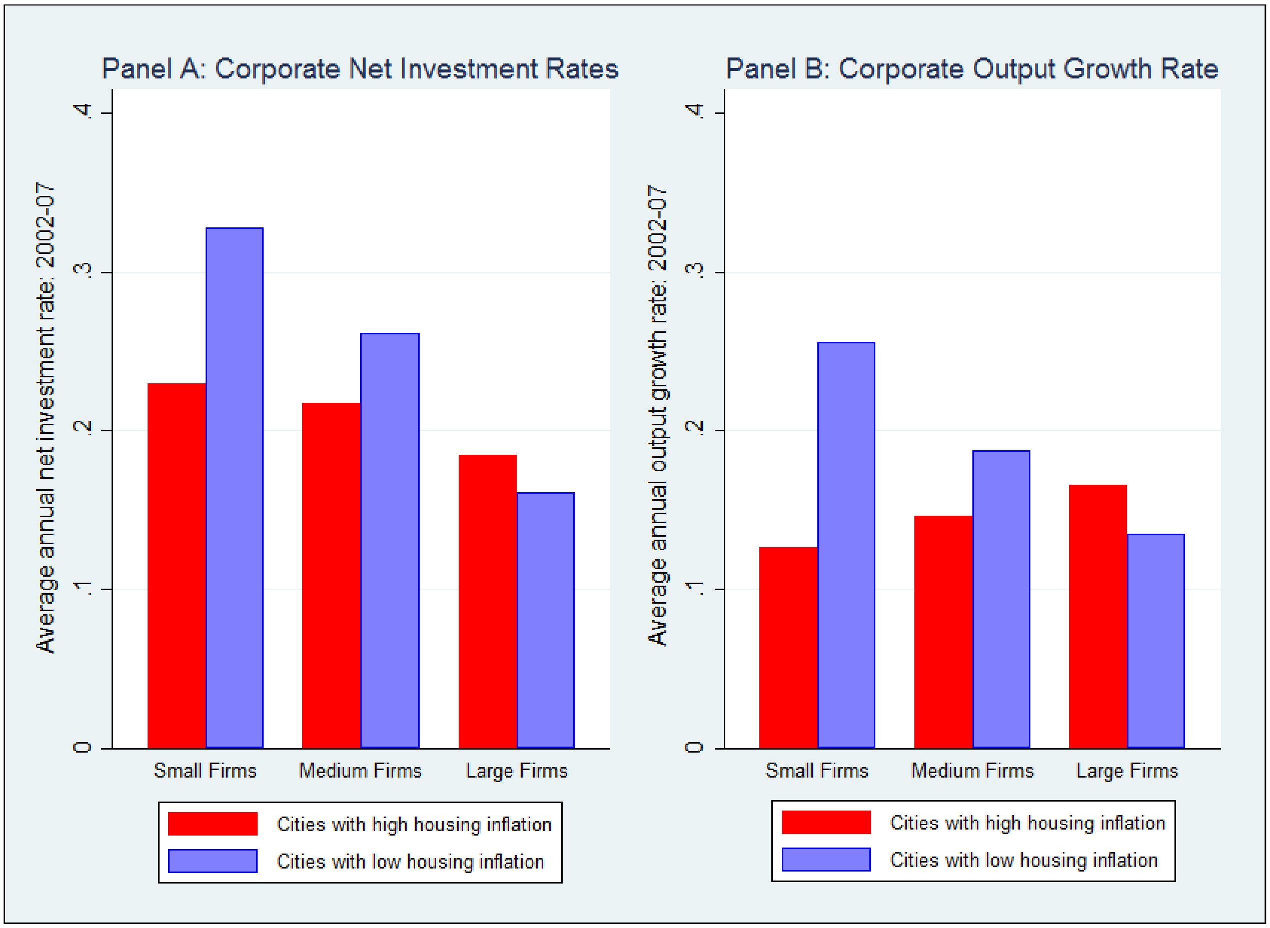

Figure 1: Among China's prefecture-cities, we identify the 50 cities with the highest and 50 cities with the lowest real estate price increase in the period 2002-07, which features an average real estate price increase of 144% and 30%, respectively. For manufacturing firm located in one of the two city groups, we report in Panel A the average annual gross investment rate and in Panel B the average annual output growth rate during the period 2002-07, where we sort firms by size into small firms with up to 50 employees, medium firms with 51 to 500 employees and large firms with more than 500 employees. The sample comprises 83,349 (21,070) firm-year observations for small firms, 331,189 (68,894) firm-year observations for medium firms, and 51,564 (15,139) firm-year observations for large firms in the cities with high (low) real estate inflation.

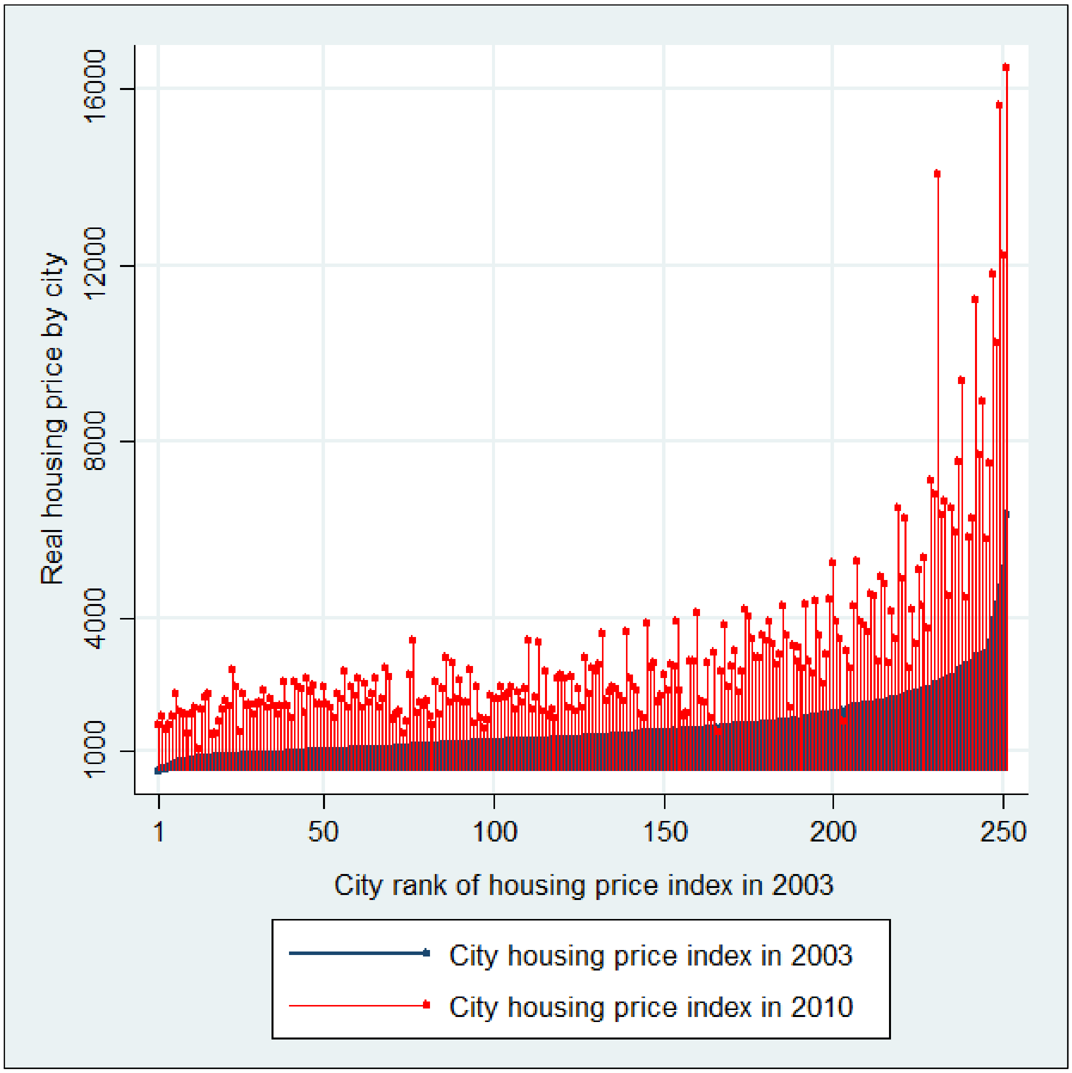

Figure 2: We rank 251 Chinese cities by their local housing price index in 2003 (blue spikes) and compare them the house price index in 2010 (red spikes).

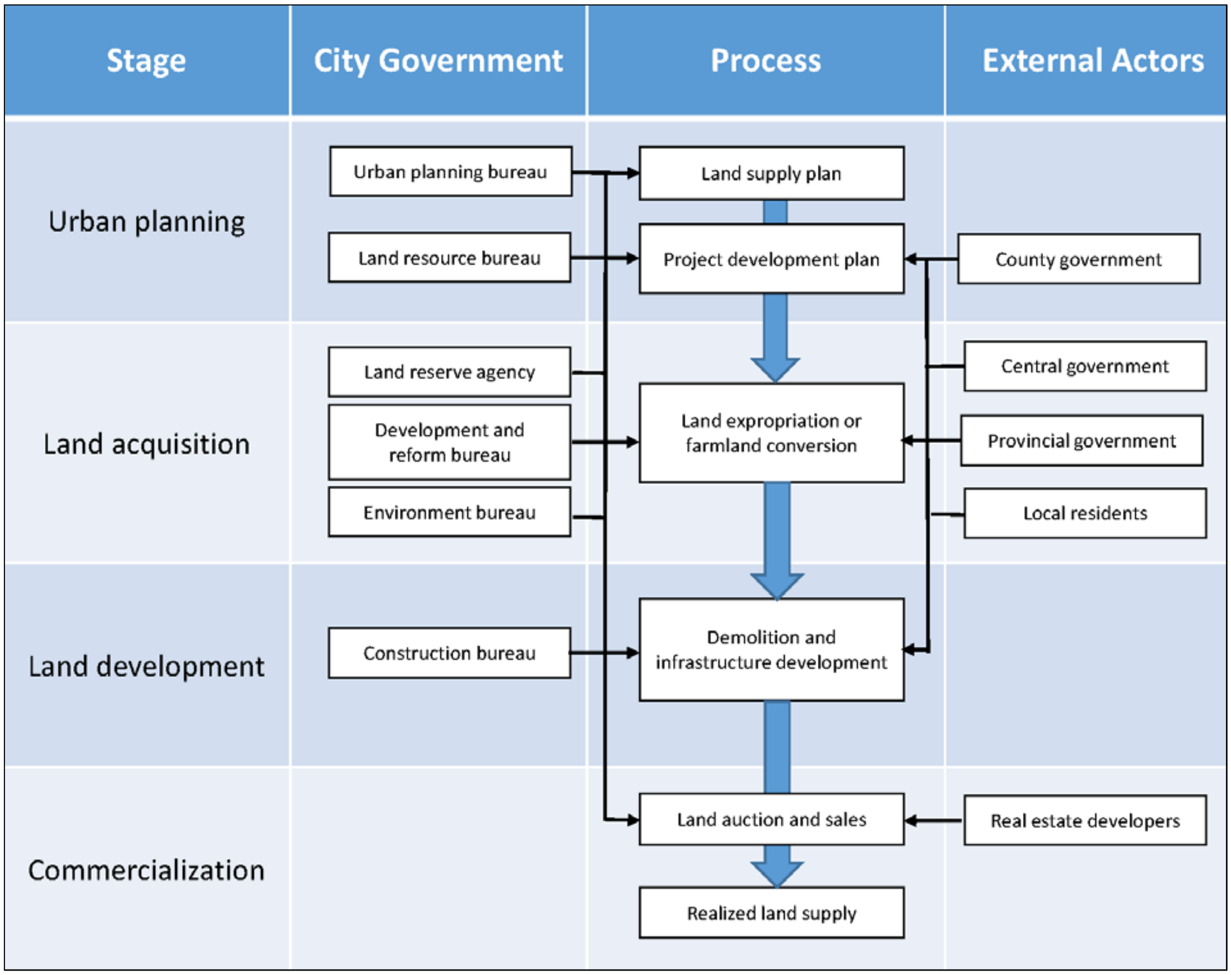

Figure 3: Stages of the bureaucratic land supply process for residential housing in China. Listed are major city-level agencies involved and external actors interfering in the process. Friction prone intragovernmental coordination, property rights, and policy conflicts generate significant discrepancies between planned and realized land supply.

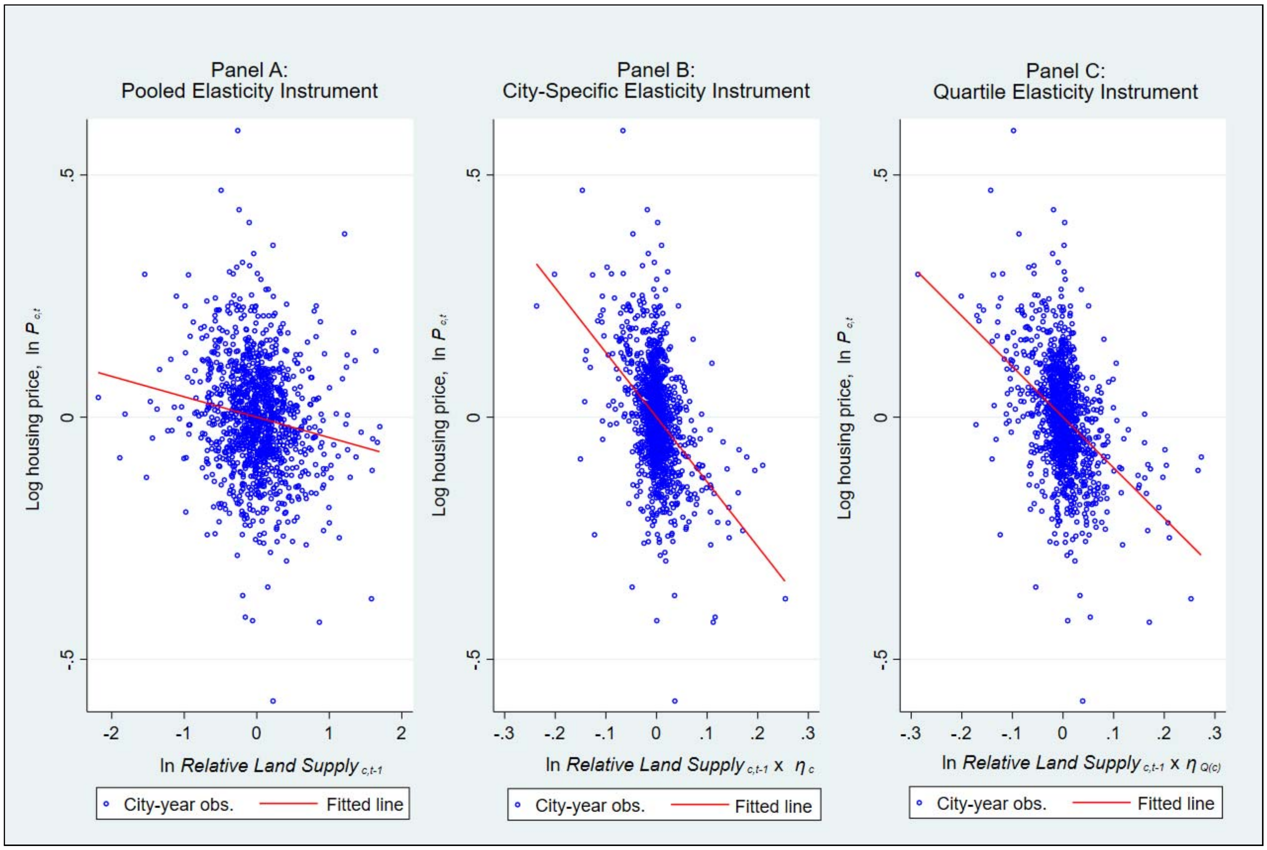

Figure 4: We plot city-year observation for the period 2001-10 showing the (log) housing prices ($\ln P_{c,t}$) on the y-axis against three instruments: Panels A plots as the x-axis the pooled elastiticity instrument (i.e. $\ln Relative\ Land\ Supply_{c,t-1}$) by imposing the restriction $\eta_c = 1$, Panel B plots as the x-axis the city-specific elastictiy instrument (i.e. $\ln Relative\ Land\ Supply_{c,t-1} \times \eta_c$), and Panel C plots as the x-axis the quartile elasticity insturment (i.e. $\ln Relative\ Land\ Supply_{c,t-1} \times \eta_{Q(c)}$) a for which we sort cities into four quartiles (based on η_c) and estimate four quartile-specific parameters $\eta_{Q(c)}$. City and year fixed effects are filtered out in all three graphs.

Figure 5: We graph the city-level average (log) value-added output change of all privately-owned firms (Panel A) and SOEs (Panel B) in each of 202 cities against the instrumented change in the (log) real estate price index from 2002 to 2007. Subtracted from the value-added output growth are firm and year fixed effects.

Appendix A1
A Modified Harrod-Balassa-Samuelson Model

We start from the two-sector structure of the Harrod-Balassa-Samuelson model and replace the non-tradable sector with a real estate sector.

Assumption 1: Real Estate and Tradable Sector

Consider a competitive real estate sector (R) producing housing Y_R and a competitive manufacturing sector (T) producing tradables Y_T. Both sectors compete for capital with inputs K_R and K_T, respectively. The real estate sector requires a governmental land supply S as a complementary factor and a high real estate price P requires proportionately more capital to produce the same amount of housing. The production function for real estate is given by

$$Y_R = A_R \min(S, K_R/P) \tag{A1}$$

where land supply S and real capital K_R/P are strictly complementary. The tradable sector features a Cobb-Douglas production function with labor input L (capital input K_T) and labor (capital) elasticity μ $(1-\mu)$ given by

$$Y_T = A_T L^{\mu} K_T^{1-\mu}. \tag{A2}$$

For simplicity, we assume real estate production does not require any labor input. This assumption can be easily relaxed and is not critical for our analysis. More important is the assumption that the capital requirements for real estate production increase linearly in the price of real estate P. This assumption is motivated by the monopolistic land supply S, where local government rations land supply and increases land prices in line with the real estate price. Hence, the same real housing production requires an increasing amount of private capital as real estate prices increase. This implies that a real estate boom in our model does not require that more real resources are allocated to housing. Yet, inflated costs of new housing reduce the share of private savings available for corporate investment.

We assume that the revenue from land sales is consumed by the government (or invested otherwise) and does not relax the limited supply of local (private) capital. If local government does not consume (or invest) its gains from land sales, but instead deposits these revenues in local banks, then we do not obtain a local capital scarcity effect under real estate price inflation. A general equilibrium model therefore needs to model government expenditure decisions in addition to private saving decisions.

In particular, we assume a fixed local factor supply for both labor and capital.

Assumption 2: Factor Supplies

The local capital and labor supply are both price inelastic (i.e. fixed); hence

$$K_R + K_T = \overline{K} \tag{A3}$$
$$L = \overline{L}. \tag{A4}$$

The local capital supply $\overline{K}$ generally depends on the local saving rate, which in turn could depends on real estate prices. But we find no evidence in Chinese data that the local household saving rate correlates with local real estate prices and sidestep such issues in the interest of simplicity.[19]

We close the model with a housing demand function of low price elasticity.

Assumption 3: Housing Demand

The (log) housing demand in city c is price elastic and for strictly positive parameters γ_0, γ_c *with* $0 \lesssim \gamma_c < 1$, *total housing demand follows as*

$$\ln Y_R^D(P) = \gamma_0 - \gamma_c \ln P. \tag{A5}$$

Under $0 \lesssim \gamma_c < 1$ housing demand features a low price elasticity and this institutional feature is crucial for our qualitative results. We assume that the local housing production is constrained by the land supply S. The equilibrium real estate price follows directly as

$$\ln P = \frac{1}{\gamma_c}\left[\gamma_0 - \ln A_R - \ln S\right], \tag{A6}$$

and the capital demand of the real estate sector is given by

$$\begin{aligned} \ln K_R &= \gamma_0 + (1-\gamma_c)\ln P - \ln A_R = \\ &= \frac{1}{\gamma_c}(\gamma_0 - \ln A_R) - \frac{1-\gamma_c}{\gamma_c}\ln S. \end{aligned} \tag{A7}$$

An insufficient land supply by local government therefore inflates the real estate price P, and at the same time increases the capital demand $\ln K_R$ by the real estate sector. Moreover, land supply shocks $\Delta \ln S$ translate into proportional capital demand shocks $\Delta \ln K_R$ according to the factor $-(1-\gamma_c)/\gamma_c < 0$. Thus, a low elasticity parameter $\gamma_c \gtrsim 0$ matters for the success of

[19] Using household data from China's Urban Household Survey over the period of 2002–2007, we regress the household-level saving rate on local real estate prices in a regression with household and time fixed effects. The positive coefficient for the local real estate price in this panel regression is economically small and statistically insignificant.

the proposed instrumental variable strategy based on measurement of the land supply S. The more inelastic the housing demand, the more capital scarcity in the tradable sector is created by any undersupply of constructible land.

To simplify notation, we express all variables in percentage changes relative to steady state log values, that is $\widehat{X} = dX/\overline{X}$.[20] The zero profit condition in the tradable sector implies the following relationship for changes in the equilibrium wage $\widehat{w}$ and the local interest rate $\widehat{i}$

$$\widehat{A}_T = \mu\widehat{w} + (1-\mu)\widehat{i}, \tag{A8}$$

where we abstract from any productivity growth by assuming $\widehat{A}_T = \widehat{A}_R = 0$. Profit maximization in the tradable sector also implies

$$\widehat{Y}_T = \widehat{w} + \widehat{L} = \widehat{i} + \widehat{K}_T, \tag{A9}$$

and the factor supply conditions give $\widehat{L} = 0$ and $\widehat{K}_T\overline{K}_T + \widehat{K}_R\overline{K}_R = 0$. Combining these relationships implies the following proposition.

The first part of our empirical analysis consists in showing that local firm adjustments across Chinese cities are indeed related to local real estate prices changes $\widehat{P}$ and constructible land supply $\widehat{S}$ as predicted in Proposition 1. The second part of our analysis explores the role of the implied factor prices variation for the manufacturing sector summarized in Proposition 2.

Proposition 1: Firm Adjustment to the Real Estate Boom

Under Assumptions 1–3 and a limited supply of constructible land S, the local production response in the manufacturing sector to real estate inflation $\widehat{P}$ is characterized by a relative (percentage) adjustment in capital $\widehat{K}_T$, the investment rate $\widehat{(I/K)}_T$, labor input $\widehat{L}_T$, manufacturing output $\widehat{Y}_T$, and labor productivity $\widehat{(Y/L)}_T$ given by

$$\widehat{(I/K)}_T = \widehat{K}_T = -\frac{\overline{K}_R}{\overline{K}_T}(1-\gamma_c)\widehat{P} \tag{A10}$$

$$\widehat{Y}_T = \widehat{(Y/L)}_T = -(1-\mu)\frac{\overline{K}_R}{\overline{K}_T}(1-\gamma_c)\widehat{P} \tag{A11}$$

where a low price elasticity of housing demand implies $0 \lesssim \gamma_c < 1$. Real estate inflation itself is proportional to changes in the local land supply $\widehat{S}$ as

$$\widehat{P} = \widehat{S} \times \eta_c, \tag{A12}$$

with a housing price sensitivity to land supply $\eta_c = -1/\gamma_c$ equal to the (negative) inverse of the demand elasticity γ_c in city c.

[20] We adopt the notation in Obstfeld and Rogoff (1996), chapter 4.

The model predicts the direct real effects of real estate booms on firm investment, output, and labor productivity. Firm effects are again scaled in the term $1-\gamma_c$, and are stronger effects for cities with a low housing demand elasticity $\gamma_c \approx 0$.

The linear relationship between the real estate price and the land supply in Eq. (A12) suggests that land supply should be a good instrument for local real estate inflation. This is particularly so if the housing demand in a city is very inelastic (i.e., γ_c is low), in which case the factor η_c is very negative and large in absolute terms. Generally, cities in China feature a low housing demand elasticity, hence $\gamma_c \gtrsim 0$ or $1-\gamma_c \lesssim 1$.[21] But these theoretical considerations suggest that any empirical inference based on exogenous land supply shocks should ideally account for city-level difference in the parameter γ_c (or η_c). In Section 4.3, we describe an empirical strategy that refines the instrument in order to do achieve this.

Proposition 2: Wages and Interest Rates

Under Assumptions 1–3, and a limited supply of constructible land S, the local interest rate change $\widehat{i}$ (real wage changes $\widehat{w}$) is proportional (is inversely proportional) to real estate prices inflation $\widehat{P}$ with percentage changes characterized as

$$\widehat{i} = \mu \frac{\overline{K}_R}{\overline{K}_T}(1-\gamma_c)\widehat{P} \tag{A12}$$

$$\widehat{w} = -(1-\mu)\frac{\overline{K}_R}{\overline{K}_T}(1-\gamma_c)\widehat{P}. \tag{A13}$$

A low city-level demand elasticity γ_c also implies that instrumented variation in the real estate price $\widehat{P}$ generate substantial interest rate and wage externalities captured in Eqs. (12)-(13). However, this relationship between (instrumented) local housing price variation and local capital scarcity breaks down for cities with a large housing demand elasticity (i.e. $\gamma_c \approx 1$). The negative effect of the real estate boom on wages distinguishes our model from a so-called "Dutch Disease" scenario, where an investment boom (often in natural resource industries) increases real labor costs and exercises competitive pressures on other firms through a higher local wage level. By contrast, our model predicts a decrease in the real wage level because of corporate underinvestment under high interest rates.

[21] The low price elasticity of housing demand is confirmed by a linear regression of housing sales value $\widehat{HS} = \widehat{P} + \widehat{Y}_R$ on the housing price level $\widehat{P}$ which produces a coefficient $(1-\gamma_p) \lesssim 1$ as shown in Figure B1 in the Internet Appendix.

Appendix A2
Model Generalization to Price Elastic Factor Supplies

The benchmark model presented in Appendix A assumes a fully price inelastic capital and labor supply. Here we relax this assumption and allow for a price elastic supply in both factors with positive elasticity parameters λ_i and λ_w, respectively. The factor supply constraints in Eqs. (3) and (4) generalize to

$$K_R + K_T = \overline{K}(1 + \lambda_i i) \tag{B1}$$
$$L = \overline{L}(1 + \lambda_w w), \tag{B2}$$

where $\lambda_i = \lambda_w = 0$ represents the benchmark case of fully price inelastic factor supplies. Linearizing eqs. (B1) and (B2) implies

$$\overline{K}_T\widehat{K}_T + \overline{K}_R\widehat{K}_R = (\frac{\lambda_i\bar{i}}{1+\lambda_i\bar{i}})\widehat{i} \tag{B3}$$
$$\widehat{L} = (\frac{\lambda_w\overline{w}}{1+\lambda_w\overline{w}})\widehat{w}, \tag{B4}$$

where $\overline{X}$ represents the steady state value and $\widehat{X} = dX/\overline{X}$ the percentage change of any variable.

The zero-profit condition for tradeable sector implies

$$A_T L^{\mu} K^{1-\mu} - wL - iK = 0.$$

and the Taylor expansion gives

$$\widehat{A}_T = 0 = \mu\widehat{w} + (1-\mu)\widehat{i}. \tag{B5}$$

Profit maximization in the tradable sector and constant factor shares further implies

$$\widehat{w} + \widehat{L} = \widehat{i} + \widehat{K}_T = \widehat{Y}_T \tag{B6}$$

For the generalized supply structure and $\widehat{K}_R = \widehat{P} + \widehat{S} = (1-\gamma_p)\widehat{P}$, we directly obtain Proposition 1:

$$\widehat{i} = \mu\frac{\overline{K}_R}{(1+B_0)\overline{K}_T}(1-\gamma_p)\widehat{P} \tag{B7}$$
$$\widehat{w} = -(1-\mu)\frac{\overline{K}_R}{(1+B_0)\overline{K}_T}(1-\gamma_p)\widehat{P}, \tag{B8}$$

where we define

$$B_0 = (1-\mu)\frac{\lambda_w \overline{w}}{1+\lambda_w \overline{w}} + \mu(1+\lambda_i \overline{i})\frac{\lambda_i}{1+\lambda_i}\frac{\overline{K}}{\overline{K}_T} \geq 0. \tag{B9}$$

The variables $\overline{K}_R$, $\overline{K}_T$, $\overline{i}$, and $\overline{w}$ represent the steady state values for capital in the two sectors and for the factor prices. For $\lambda_i = \lambda_w = 0$, we obtain $B_0 = 0$. Because $B_0 \geq 0$, local interest rate changes $\widehat{i}$ (real wage changes $\widehat{w}$) are again proportional (inversely proportional) to real estate prices inflation $\widehat{P}$.

Proposition 2 generalizes to the following expressions:

$$\widehat{(I/K)}_T = \widehat{K}_T = -\frac{(1+B_1)\overline{K}_R}{(1+B_0)\overline{K}_T}(1-\gamma_p)\widehat{P} \tag{B10}$$

$$\widehat{Y}_T = -(1-\mu)\frac{(1+B_2)\overline{K}_R}{(1+B_0)\overline{K}_T}(1-\gamma_p)\widehat{P} \tag{B11}$$

$$\widehat{(Y/L)}_T = -(1-\mu)\frac{\overline{K}_R}{(1+B_0)\overline{K}_T}(1-\gamma_p)\widehat{P}, \tag{B12}$$

where we define

$$B_1 = (1-\mu)\frac{\lambda_w \overline{w}}{1+\lambda_w \overline{w}} \geq 0 \tag{B13}$$

$$B_2 = \frac{\lambda_w \overline{w}}{1+\lambda_w \overline{w}} \geq 0. \tag{B14}$$

For $\lambda_i = \lambda_w = 0$, we obtain $B_1 = 0$ and $B_2 = 0$. The capital stock change $\widehat{K}_T$, the investment change $\widehat{(I/K)}_T$, output change $\widehat{Y}_T$, and labor productivity change $\widehat{(Y/L)}_T$ of the manufacturing sector are still negative for a positive local housing price inflation $\widehat{P} > 0$.

Table 1: Summary Statistics

Summary statistics at the firm level are the gross investment rate ($I/K_{j,t}$), a dummy for whether a firm has long-run borrowing ($Loan_{j,t}$), the (log) value-added output ($\ln Y_{j,t}$), the (log) labor productivity ($\ln(Y/L)_{j,t}$), the (log) average employees' wage ($\ln w_{j,t}$), the firm bank loan rate ($i_{j,t}$), the return on assets ($ROA_{j,t}$), the (log) total factor productivity ($\ln TFP_{j,t}$), a dummy variable for firms exiting from the sample in period $t+1$ ($Exit_{j,t}$), the demean firm's (log) fixed assets ($\ln Fixed\ Assets_j$) at the beginning of the sample, and a dummy for whether a firm is a state-owned-enterprises (SOE_j) at the beginning of the sample. Summary statistics at the city level (indicated by subscript c) are the (log) average real house price ($\ln P_{c,t}$) for residential housing. The $Relative\ Land\ Supply_{c,t-1}$ is the constructable surface for new residential housing relative to the existing housing stock. The $Adjusted\ Land\ Supply_{c,t-1}$ is defined as the product of the one-year lag (log) $Relative\ Land\ Supply_{c,t-1}$ and the quartile-specific (inverse) housing demand elasticity $\widehat{\eta}_{Q(c)}$. We also report city-level summary statistics for the (log) gross domestic product ($\ln\ GDP_{c,t-1}$), the (log) city population ($\ln\ Population_{c,t-1}$), the annual (log) expenditure ($\ln\ Gov.\ Exp._{c,t-1}$) by the city government, its annual (log) revenue ($\ln\ Gov.\ Revenue_{c,t-1}$), the share of government budget deficit (expenditure minus revenue) to GDP ($Gov.\ Deficit_{c,t-1}$) and the percentage of park area within the urban area ($Park\ Share_{c,t-1}$).

	Obs.	Mean	SD	Q25	Q50	Q75
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Firm-level variables						
$I/K_{j,t}$	752,504	0.350	1.138	0	0.051	0.279
$Loan_{j,t}$ (dummy)	1,010,600	0.340	0.474	0	0	1
$\ln Y_{j,t}$	983,388	8.740	1.188	7.860	8.600	9.487
$\ln(Y/L)_{j,t}$	984,282	3.983	0.964	3.311	3.920	4.614
Firm wage: $\ln w_{j,t}$	989,950	2.581	0.566	2.223	2.550	2.907
Firm bank loan rate: $i_{j,t}$	458,802	0.063	0.046	0.028	0.052	0.087
$Exit_{j,t}$	1,010,600	0.097	0.296	0	0	0
$ROA_{j,t}$	990,755	0.074	0.125	0.006	0.036	0.101
$\ln\ TFP_{j,t}$	925,785	1.134	0.341	0.972	1.188	1.365
$\ln\ Fixed\ Assets_j$	253,762	0.081	1.808	−0.956	0.054	1.133
SOE_j (Dummy)	253,762	0.075	0.264	0	0	0
Panel B: City-level variables						
$\ln P_{c,t}$	1,212	7.399	0.466	7.082	7.316	7.665
$\ln\ Relative\ Land\ Supply_{c,t-1}$	1,212	−3.895	0.992	−4.518	−3.870	−3.183
$Adjusted\ Land\ Supply_{c,t-1}$	1,212	−0.206	0.346	−0.384	−0.106	0.074
$\ln\ GDP_{c,t}$	1,212	15.18	0.896	14.58	15.04	15.69
$\ln\ Population_{c,t-1}$	1,212	5.895	0.650	5.531	5.939	6.363
$\ln\ Gov.\ Exp._{c,t-1}$	1,212	12.54	1.094	11.87	12.60	13.11
$\ln\ Gov.\ Revenue_{c,t-1}$	1,212	11.89	1.265	11.11	12.80	12.57
$Gov.\ Deficit_{c,t-1}$	1,212	0.048	0.035	0.024	0.041	0.066
$Park\ Share_{c,t-1}$	1,212	3.720	5.176	0.846	2.052	4.690

Table 2: Balance Tests

We define the *Relative Land Supply*$_{c,t}$ the ratio of the land purchases by the real estate sector for residential housing development scaled by the housing stock in the same city c in year t. We explore whether the log of the *Relative Land Supply*$_{c,t}$ is correlated with potential confounding factors at the city level. In Panel A, the outcome variables are the yearly growth in government expenditure ($\Delta \ln$ *Gov. Exp.*$_{c,t-1}$), government revenue ($\Delta \ln$ *Gov. Revenue*$_{c,t-1}$), gross domestic output/GDP ($\Delta \ln$ $GDP_{c,t-1}$), and the share of employment in the secondary sector (Δ*Sec. Ind.*$_{c,t-1}$) in year $t-1$. In Panel B, the outcome variables are the contemparaneous (log) government expenditure on infrastructure (ln *Infrastr. Exp.*$_{c,t}$), the tenure in years of the local communist party leader (*Leader Tenure*$_{c,t}$), and the (log) supply of industrial land (ln *Industrial Land*$_{c,t}$). In Panel C, the outcome variables are the yearly growth in road length ($\Delta \ln$ *Road Length*$_{c,t+1}$), public buses ($\Delta \ln$ $Bus_{c,t+1}$), hospitals ($\Delta \ln$ *Hospital*$_{c,t+1}$), and theatre ($\Delta \ln$ *Theatre*$_{c,t+1}$) in a forward period. All regressions control city-level macroeconomics variables (in Table 2), year and firm fixed effects. Robust standard errors are in parentheses and are clustered at city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: OLS with lagged variables

Dep. variables:	$\Delta \ln$ *Gov. Exp.*$_{c,t-1}$ (1)	$\Delta \ln$ *Gov. Revenue*$_{c,t-1}$ (2)	$\Delta \ln$ $GDP_{c,t-1}$ (3)	Δ*Sec. Ind.*$_{c,t-1}$ (4)
ln *Relative Land Supply*$_{c.t}$	0.016	0.017	0.002	−0.127
	(0.020)	(0.024)	(0.006)	(0.278)
Observations	1,212	1,212	1,212	1,212
R-squared	0.608	0.530	0.237	0.039

Panel B: OLS with contremporaneous variables

Dep. variables:	ln *Infrastr. Exp.*$_{c,t}$ (1)	*Leader Tenure*$_{c,t}$ (2)	ln *Industrial Land*$_{c,t}$ (3)
ln *Relative Land Supply*$_{c.t}$	−0.037	0.192	0.045
	(0.055)	(0.129)	(0.065)
Observations	960	1,121	795
R-squared	0.284	0.044	0.095

Panel C: OLS with future variables

Dep. variables:	$\Delta \ln$ *Road Length*$_{c,t+1}$ (1)	$\Delta \ln$ $Bus_{c,t+1}$ (2)	$\Delta \ln$ *Hospital*$_{c,t+1}$ (3)	$\Delta \ln$ *Theatre*$_{c,t+1}$ (4)
ln *Relative Land Supply*$_{c.t}$	0.014	0.020	−0.011	−0.010
	(0.015)	(0.013)	(0.013)	(0.029)
Observations	1,208	1,209	1,210	1,204
R-squared	0.018	0.008	0.047	0.016
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes

Table 3: Housing Prices and Adjusted Land Supply

The (log) housing price $\ln P_{c,t}$ is regressed in three different instruments. The pooled elasticity specification in Columns (1) uses the (log) *Relative Land Supply*$_{c,t-1}$ as the instrument for residential housing price variations, whereas the city-specific elasticity specification in Column (2) interacts the ln *Relative Land Supply*$_{c,t-1}$ with the (inverse) of the city-specific local housing demand elasticity $\widehat{\eta}_c$, and the quartile specification in Column (3) interacts the ln *Relative Land Supply*$_{c,t-1}$ with the (inverse) of the quartile-specific housing demand elasticity $\widehat{\eta}_{Q(c)}$. We refer to this product ln *Relative Land Supply*$_{c,t-1}$ $\times$ $\widehat{\eta}_{Q(c)}$ as the *Adjusted Land Supply*$_{c,t-1}$ in city c and year $t-1$. The control variables in both columns include annual city-level statistics for the (log) gross domestic product ($\ln GDP_{c,t-1}$), (log) population (ln*Population*$_{c,t-1}$), the annual (log) expenditure (ln*Gov. Exp.*$_{c,t-1}$) of the city government, its annual (log) revenue (ln *Revenue*$_{c,t-1}$), the ratio of the local government budget deficit (expenditure minus revenue) to GDP (*Gov. Deficit*$_{c,t-1}$), and the percentage of park area within the urban area (*Park Share*$_{c,t-1}$) in city c and year $t-1$. Robust standard errors are in parentheses and are clustered at city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	$\ln P_{c,t}$		
Specification	Pooled elasticity	City-specific elasticity	Quartile elasticity
	OLS	OLS	OLS
	(1)	(2)	(3)
ln *Relative Land Supply*$_{c,t-1}$	-0.042^{***}		
	(0.011)		
ln*Relative Land Supply*$_{c,t-1}$ $\times$ $\widehat{\eta}_c$		-1.335^{***}	
		(0.105)	
ln *Relative Land Supply*$_{c,t-1}$ $\times$ $\widehat{\eta}_{Q(c)}$			-1.044^{***}
			(0.074)
ln $GDP_{c,t-1}$	-0.079	0.006	0.001
	(0.056)	(0.048)	(0.048)
ln *Population*$_{c,t-1}$	0.018	-0.048	-0.036
	(0.052)	(0.036)	(0.037)
ln *Gov. Exp.*$_{c,t-1}$	-0.043	-0.018	-0.022
	(0.032)	(0.029)	(0.029)
ln *Gov. Revenue*$_{c,t-1}$	0.052^{*}	0.054^{**}	0.059^{**}
	(0.031)	(0.026)	(0.027)
Gov. Deficit$_{c,t-1}$	-0.689^{*}	-0.557^{*}	-0.561
	(0.410)	(0.337)	(0.356)
Park Share$_{c,t-1}$	0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
F-statistics	15.0	165.6	199.6
R-squared	0.660	0.715	0.718
Observations	1,212	1,212	1,212
Number of cities	202	202	202
Year fixed effects	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes

Table 4: Housing Prices and Firm Outcomes

Different measures of firm production are regressed on the local housing price level $\ln P_{c,t}$. Panel A reports the OLS regression; Panel B reports the 2SLS estimates using the pooled elasticity instrument ln *Relative Land Supply*$_{c,t-1}$; Panel C reports the 2SLS estimates using the city-specific elasticity instrument, namely the interaction of ln*Relative Land Supply*$_{c,t-1}$ with the (inverse) of the city-specific local housing demand elasticity $\widehat{\eta}_c$; Panel D reports the 2SLS estimates using the quartile-specific elasticity instrument *Adjusted Land Supply*$_{c,t-1}$, namely the interaction of ln*Relative Land Supply*$_{c,t-1}$ with the (inverse) of the quartile-specific housing demand elasticity $\widehat{\eta}_{Q(c)}$, and Panel E includes additional industry×year fixed effects. All regressions include city-level macroeconomic controls (see Table 3), as well as firm and year fixed effects. The dependent variables are the firm's gross investment rate ($I/K_{j,t}$) in Column (1), a dummy variable of whether firm $_j$ has long-run bank lending ($Loan_{j,t}$) in Column (2), the (log) value-added firm output ($\ln Y_{j,t}$) in Column (3), and the (log) labor productivity ($\ln(Y/L)_{j,t}$) in Column (4). Standard errors are in parentheses and are clustered at the city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$
	(1)	(2)	(3)	(4)
Panel A: OLS				
ln $P_{c,t}$	-0.129^{***}	-0.021	-0.280^{***}	-0.315^{***}
	(0.043)	(0.020)	(0.076)	(0.068)
Observations	752,504	1,010,600	983,388	984,282
Panel B: 2SLS with pooled elasticity instrument				
ln $P_{c,t}$	-0.348^{*}	-0.243^{**}	-1.863^{***}	-2.030^{***}
	(0.206)	(0.123)	(0.645)	(0.658)
Kleibergen-Paap *F-stat*	5.0	7.0	7.0	7.0
Observations	752,504	1,010,600	983,388	984,282
Panel C: 2SLS with city-specific elasticity instrument				
ln $P_{c,t}$	-0.186^{***}	-0.117^{**}	-0.884^{***}	-0.947^{***}
	(0.049)	(0.057)	(0.150)	(0.110)
Kleibergen-Paap *F-stat*	47.9	42.0	40.8	40.8
Observations	752,504	1,010,600	983,388	984,282
Panel D: 2SLS with quartile-specific elasticity instrument				
ln $P_{c,t}$	-0.213^{***}	-0.096^{**}	-0.932^{***}	-0.963^{***}
	(0.053)	(0.049)	(0.185)	(0.137)
Kleibergen-Paap *F-stat*	124.4	135.5	135.6	135.2
Observations	752,504	1,010,600	983,388	984,282
Panel E: 2SLS like in Panel D with additional industry-year fixed effects				
ln $P_{c,t}$	-0.198^{***}	-0.084^{**}	-0.954^{***}	-0.967^{***}
	(0.052)	(0.041)	(0.189)	(0.139)
Kleibergen-Paap *F-stat*	132.7	143.9	143.9	143.8
Observations	752,504	1,010,600	983,388	984,282

Table 5: Factor Price Response to Housing Price Inflation

We regress local factor prices on the log of the local real housing price ($\ln P_{c,t}$). The dependent variables are the firm bank loan rate ($i_{j,t}$) in Columns (1)–(3), and the log average firm wage ($\ln w_{j,t}$) in Columns (4)–(6). Columns (1) and (4) report OLS results, Columns (2) and (5) the corresponding 2SLS results. Our instrument is the *Adjusted Land Supply*$_{c,t-1}$ defined as the product of the ln *Relative Land Supply*$_{c,t-1}$ and the (inverse) of the quartile-specific housing demand elasticity $\widehat{\eta}_{Q(c)}$. Columns (3) and (6) report 2SLS results with additional industry × year fixed effects. Robust standard errors are provided in parentheses and are clustered at city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	*Firm bank loan rate:* $i_{j.t}$			*Firm wage:* $\ln w_{j.t}$		
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln P_{c,t}$	0.008***	0.016**	0.015**	−0.131***	−0.468***	−0.454***
	(0.002)	(0.007)	(0.007)	(0.048)	(0.132)	(0.135)
Kleibergen-Paap *F-stat*		145.2	153.3		134.8	143.3
Observations	458,802	458,802	458,802	989,950	989,950	989,950
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	Yes	Yes	No
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year fixed effects	No	No	Yes	No	No	Yes

Table 6: Housing Prices and Firm Heterogeneity in Credit Access

Different measures of firm outcomes are regressed on the (log) local housing price level $\ln P_{c,t}$ and the interaction terms of the housing price level with a proxy for bank access. Panel A uses the demean of ex-ante (log) fixed assets (ln *Fixed Assets*$_j$) as a measure of collateral availability. Panel B creates interaction terms with the state-ownership dummy (SOE_j), because Chinese SOEs enjoy privileged bank access. Panel C uses a dummy of bank dependence if province's share of fixed investment financed by loans in 2000 is above the sample median (*Bank dependence*$_p$). All regressions control city-level macroeconomics variables, year and firm fixed effects. Standard errors are in parentheses and are clustered at the city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$
	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Panel A: Interaction with firm's fixed assets				
$\ln P_{c,t}$	-0.230^{***}	-0.096^{**}	-0.932^{***}	-0.962^{***}
	(0.057)	(0.047)	(0.186)	(0.137)
$\ln P_{c,t} \times \ln$ *Fixed Assets*$_j$	0.089^{***}	0.026^{***}	0.027	0.025^{*}
	(0.024)	(0.005)	(0.021)	(0.013)
Kleibergen-Paap *F-stat*	53.6	45.3	42.3	47.8
Observations	752,504	1,010,600	983,388	984,282
Panel B: Interaction with SOE_j dummy				
$\ln P_{c,t}$	-0.232^{***}	-0.102^{**}	-0.944^{***}	-0.980^{***}
	(0.056)	(0.047)	(0.181)	(0.138)
$\ln P_{c,t} \times SOE_j$	0.182^{***}	0.075^{*}	0.151	0.228^{**}
	(0.048)	(0.040)	(0.207)	(0.111)
Kleibergen-Paap *F-stat*	27.2	14.4	14.3	14.7
Observations	752,504	1,010,600	983,388	984,282
Panel C: Interaction with *Bank dependence*$_p$ dummy				
$\ln P_{c,t}$	0.032	0.121	-0.404	-0.392
	(0.122)	(0.102)	(0.284)	(0.239)
$\ln P_{c,t} \times$ *Bank dependence*$_p$	-0.190^{**}	-0.165^{**}	-0.398^{*}	-0.430^{**}
	(0.087)	(0.073)	(0.220)	(0.169)
Kleibergen-Paap *F-stat*	7.9	7.2	7.3	7.4
Observations	752,504	1,010,600	983,388	984,282
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes

Table 7: Housing Prices and Firm Performance

Three different measures of firm performance, namely return on assets ($ROA_{j,t}$) in Columns (1)-(3), the (log) total factor productivity $\ln TFP_{j,t}$ in Columns (4)-(6), and a dummy variable for firms exiting from the sample in period $t+1$ ($Exit_{j,t}$) in Columns (7)-(9) are regressed on the local (log) housing price level $\ln P_{c,t}$ and interaction terms of $\ln P_{c,t}$ with three different proxies for firm bank access, namely (log) fixed assets ($\ln$ *Fixed Assets*$_j$) as a measure of collateral availability, a dummy for state-owned enterprises (SOE_j), and a dummy for bank-dependent regions (*Bank Dependence*$_p$). All regressions control macroeconomic variables, year and firm fixed effects. Standard errors are in parentheses and are clustered at the city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dep. variables:	$ROA_{j,t}$			$\ln TFP_{j,t}$		
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln P_{c,t}$	-0.044^{***}	-0.141^{***}	-0.072	-0.101^{***}	-0.285^{***}	-0.106
	(0.012)	(0.029)	(0.058)	(0.021)	(0.041)	(0.069)
$\ln P_{c,t} \times \ln$ *Fixed Assets*$_j$			0.009^{***}			0.018^{***}
			(0.002)			(0.005)
$\ln P_{c,t} \times SOE_j$			0.045^{**}			0.020
			(0.022)			(0.029)
$\ln P_{c,t} \times$ *Bank Dependence*$_p$			-0.055			-0.134^{***}
			(0.039)			(0.047)
Kleibergen-Paap *F-stat*		135.3	3.5		134.8	3.9
Observations	990,755	990,755	990,755	925,785	925,785	925,785
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes	Yes	Yes	Yes

Dep. variables:	$Exit_{j,t}$		
	OLS	2SLS	2SLS
	(7)	(8)	(9)
$\ln P_{c,t}$	-0.028	0.087^{*}	0.016
	(0.030)	(0.046)	(0.152)
$\ln P_{c,t} \times \ln$ *Fixed Assets*$_j$			-0.035^{***}
			(0.006)
$\ln P_{c,t} \times SOE_j$			-0.020
			(0.036)
$\ln P_{c,t} \times$ *Bank dependence*$_p$			0.055
			(0.099)
Kleibergen-Paap *F-stat*		135.1	3.5
Observations	1,010,600	1,010,600	1,010,600
Macroeconomic controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes

Table A1: Robustness Check using Alternative Specifications

We check the robustness of the benchmark results in Tables 3 and 4 using alternative specifications. Panel A provides the reduced form specification with macroeconomic controls, firm and year fixed effects. In Panels B and C we report 2SLS regressions. The instrument is the quartile-specific elasticity instrument $Adjusted\ Land\ Supply_{c,t-1}$, namely the interaction of $\ln Relative\ Land\ Supply_{c,t-1}$ with the (inverse) of the quartile-specific housing demand elasticity $\widehat{\eta}_{Q(c)}$. In Panel B we replace firm with city fixed effects. In Panel C, we define three dummies for the local housing market status, namely $D\_HM^H_{c,t} = 1$ (and zero otherwise) if a city's yearly house price growth is larger than 15%; $D\_HM^L_{c,t} = 1$ (and zero otherwise) if a city's yearly house price growth is negative; and $D\_HM^M_{c,t} = 1$ (and zero otherwise) if the city's housing market status is between the two extremes. Roughly 30 percent of city-year observations concern a boom period, 25% a bust period; and 45% are in the middle group. We interact these three housing market status dummies with industry-year dummies and add these interactions terms to the baseline regressions. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$	$i_{j.t}$	$\ln w_{j.t}$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reduced form using the pooled elasticity instrument as the explanatory variable						
$\ln Relative\ Land\ Supply_{c,t-1}$	0.014^{*}	0.012^{***}	0.091^{***}	0.099^{***}	-0.002^{**}	0.038^{***}
	(0.008)	(0.004)	(0.018)	(0.016)	(0.001)	(0.011)
Observations	752,517	1,010,620	983,388	984,282	458,808	989,950
Panel B: city rather than firm fixed effects						
$\ln P_{c,t}$	-0.196^{***}	-0.063^{*}	-0.811^{***}	-0.845^{***}	0.008	-0.421^{***}
	(0.067)	(0.034)	(0.172)	(0.125)	(0.006)	(0.115)
Kleibergen-Paap *F-stat*	122.8	137.3	137.9	137.4	154.9	136.1
Observations	810,724	1,097,510	1,070,556	1,071,749	531,113	1,077,601
Panel C: housing market status dummies interacted with industry-year fixed effects						
$\ln P_{c,t}$	-0.244^{***}	-0.089^{**}	-0.962^{***}	-1.007^{***}	0.018^{***}	-0.573^{***}
	(0.089)	(0.041)	(0.173)	(0.183)	(0.007)	(0.192)
Kleibergen-Paap *F-stat*	64.9	68.2	68.7	68.3	72.3	67.0
Observations	751,504	1,009,801	982,537	983,424	457,476	989,130
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm (or city) fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Table A2: Robustness Check for Cities with Negative and Positive Elasticity Estimates

We check the robustness of the benchmark results in Tables 3 and 4 for the subsample of firms in cities with negative and positive elasticity estimates. We separate firm observations for cities with negative and positive city-level elasticity estimates in Panels A to D and E to H, respectively. The instrument is the pooled elasticity instrument ln*Relative Land Supply*$_{c,t-1}$in Panels C and G, and the quartile-elasticity instrument *Adjusted Land Supply*$_{c,t-1}$ in Panels D and H. All regressions control for macroeconomic variables, year and firm fixed effects. Standard errors are in parentheses and are clustered at the city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$	$i_{j.t}$	$\ln w_{j.t}$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cities with negative elasticity estimates ($\widehat{\eta}_c < 0$), reduced form						
ln*Relative Land Supply*$_{c,t-1}$	0.016^{*}	0.014^{***}	0.113^{***}	0.121^{***}	-0.002^{***}	0.053^{***}
	(0.009)	(0.005)	(0.020)	(0.016)	(0.001)	(0.012)
Observations	549,682	738,048	719,500	720,121	342,564	722,912
Panel B: Cities with negative elasticity estimates ($\widehat{\eta}_c < 0$), OLS						
$\ln P_{c,t}$	-0.169^{***}	-0.040^{*}	-0.310^{***}	-0.380^{***}	0.010^{***}	-0.145^{**}
	(0.052)	(0.024)	(0.087)	(0.076)	(0.002)	(0.058)
Observations	549,682	738,048	719,500	720,121	342,564	722,912
Panel C: Cities with negative elasticity estimates ($\widehat{\eta}_c < 0$), 2SLS with pooled elasticity						
$\ln P_{c,t}$	-0.213^{*}	-0.167^{**}	-1.319^{***}	-1.401^{***}	0.021^{***}	-0.613^{***}
	(0.057)	(0.055)	(0.150)	(0.102)	(0.006)	(0.119)
Kleibergen-Paap *F-stat*	14.2	17.7	17.5	17.5	22.2	17.8
Observations	549,682	738,048	719,500	720,121	342,564	722,912
Panel D: Cities with negative elasticity estimates ($\widehat{\eta}_c < 0$), 2SLS with quartile-specific elasticity						
$\ln P_{c,t}$	-0.221^{***}	-0.117^{**}	-1.001^{***}	-0.982^{***}	0.019^{***}	-0.462^{***}
	(0.059)	(0.049)	(0.180)	(0.124)	(0.007)	(0.131)
Kleibergen-Paap *F-stat*	136.1	149.1	147.2	146.9	154.9	148.4
Observations	549,682	738,048	719,500	720,121	342,564	722,912
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Table A2 continued

Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$	$i_{j,t}$	$\ln w_{j,t}$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel E: Cities with positive elasticity estimates ($\widehat{\eta}_c > 0$), reduced form						
$\ln Relative\ Land\ Supply_{c,t-1}$	-0.004	0.000	0.045^{**}	0.041^{**}	-0.000	-0.010
	(0.019)	(0.008)	(0.021)	(0.019)	(0.001)	(0.019)
Observations	202, 822	272, 552	263, 888	264, 161	116, 238	267, 038
Panel F: Cities with positive elasticity estimates ($\widehat{\eta}_c > 0$), OLS						
$\ln P_{c,t}$	-0.025	0.038	-0.165	-0.081	0.001	-0.099
	(0.058)	(0.028)	(0.133)	(0.082)	(0.004)	(0.071)
Observations	202, 822	272, 552	263, 888	264, 161	116, 238	267, 038
Panel G: Cities with positive elasticity estimates ($\widehat{\eta}_c > 0$), 2SLS with pooled elasticity						
$\ln P_{c,t}$	-0.109	0.001	1.074	0.981	-0.006	-0.249
	(0.430)	(0.186)	(0.737)	(0.610)	(0.023)	(0.440)
Kleibergen-Paap *F-stat*	3.7	4.1	4.1	4.1	2.5	4.1
Observations	202, 822	272, 552	263, 888	264, 161	116, 238	267, 038
Panel H: Cities with positive elasticity estimates ($\widehat{\eta}_c > 0$), 2SLS with quartile-specific elasticity						
$\ln P_{c,t}$	-0.201	0.001	1.114	1.006	-0.003	-0.294
	(0.415)	(0.189)	(0.748)	(0.617)	(0.024)	(0.441)
Kleibergen-Paap *F-stat*	3.7	4.1	4.1	4.1	2.4	4.1
Observations	202, 822	272, 552	263, 888	264, 161	116, 238	267, 038
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Table A3: Robustness Check using a Two-Stage Estimation Strategy

We check the robustness of the benchmark results in Tables 3 and 4 using a two-stage estimation strategy proposed by Combes *et al.* (2008, 2019) and Combes and Gobillon (2015). Panel A reports the OLS estimates and Panel B reports the 2SLS estimates using the quartile-specific elasticity instrument *Adjusted Land Supply*$_{c,t-1}$. All regressions control for macroeconomic variables, year and city fixed effects. Standard errors are in parentheses and are clustered at the city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	$I/K_{c,t}$	$Loan_{c,t}$	$\ln Y_{c,t}$	$\ln(Y/L)_{c,t}$	$i_{c.t}$	$\ln w_{c.t}$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
$\ln P_{c,t}$	−0.086	−0.014	−0.183**	−0.145***	0.004	−0.045
	(0.057)	(0.024)	(0.090)	(0.054)	(0.003)	(0.036)
Observations	1,182	1,197	1,188	1,197	1,197	1,197
Panel B: 2SLS						
$\ln P_{c,t}$	−0.206	−0.131**	−0.662**	−0.322**	0.009**	−0.214*
	(0.137)	(0.066)	(0.263)	(0.154)	(0.004)	(0.121)
Kleibergen-Paap *F-stat*	228.7	244.1	259.6	244.1	244.1	244.1
Observations	1,182	1,197	1,188	1,197	1,197	1,197
Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$	$i_{j.t}$	$\ln w_{j.t}$
Panel C: 2SLS (firm baseline results)						
$\ln P_{c,t}$	−0.213***	−0.096**	−0.932***	−0.963***	0.015**	−0.454***
	(0.053)	(0.049)	(0.185)	(0.137)	(0.007)	(0.135)
Kleibergen-Paap *F-stat*	124.4	135.5	135.6	135.2	153.3	143.3
Observations	752,504	1,010,600	983,388	984,282	458,802	989,950
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City or firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Table A4: Firm Heterogeneity: Fixed Assets or Output Matters?

Different measures of firm outcomes are regressed on the (log) local housing price level $\ln P_{c,t}$ and the interaction terms of the housing price level with different measures of firm size. All regressions control city-level macroeconomics variables, year and firm fixed effects. Standard errors are in parentheses and are clustered at the city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$
	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
$\ln P_{c,t}$	-0.225^{***}	-0.096^{**}	-0.903^{***}	-0.915^{***}
	(0.057)	(0.046)	(0.174)	(0.125)
$\ln P_{c,t} \times \ln \textit{Fixed Assets}_j$	0.127^{***}	0.017^{***}	0.127^{***}	0.108^{***}
	(0.034)	(0.004)	(0.021)	(0.016)
$\ln P_{c,t} \times \ln Y_j$	-0.078^{***}	0.020^{***}	-0.280^{***}	-0.222^{***}
	(0.029)	(0.006)	(0.062)	(0.058)
Kleibergen-Paap *F-stat*	18.0	16.7	16.6	16.8
Observations	752,504	1,010,600	983,388	984,282
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes

Table A5: Export Performance Measured at the Product Level

For a subsample of pure-exporters, we use product-level export statistics from the Chinese customs authorities to decompose the yearly (log) export value of a firm's exported products ($\ln ExpValue_{j,t}$) into (directly reported) export quantity ($\ln ExpQuantity_{j,t}$) and export (unit) price ($\ln ExpPrice_{j,t}$). We repeat the 2SLS regression in Table 3, for this subsample in Columns (1), and the new export performance measure in Columns (2)–(4). All regressions control for city level macroeconomic variables, year fixed effects and firm fixed effects. Standard errors are in parenthesis and are clustered at the city level. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

	Export firms	Product level firm performance		
Dependent variables:	$\ln Y_{j,t}$	$\ln ExpValue_{j,t}$	$\ln ExpQuantity_{j,t}$	$\ln ExpPrice_{j,t}$
	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
$\ln P_{c,t}$	-0.680^{**}	-0.448^{*}	-0.412	-0.037
	(0.263)	(0.245)	(0.272)	(0.140)
Kleibergen-Paap *F-stat*	36.9	34.0	34.0	34.0
Observations	34,597	108,717	108,717	108,717
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes

Table A6: Using Housing Supply Elasticities as an Alternative Instrument

As a robustness check, we use the housing supply elasticities reported by Wang *et al.* (2012) as an alternative instrument in a cross-sectional specification at the firm level with two-digit industry fixed effects. All regressions control city-level (log) GDP per capita, (log) population density, employment share of the secondary sector, and GDP share of the secondary sector in 2002. Robust standard errors are in parentheses and robust standard errors clustered at city level are in brackets. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variables:	$\Delta^{02-07}(I/K)_j$	$\Delta^{02-07}Loan_j$	$\Delta^{02-07}\ln Y_j$	$\Delta^{02-07}\ln(Y/L)_j$
	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
$\Delta^{02-07}\ln P_c$	-0.255	-0.013	-1.278	-0.968
	(0.094)***	(0.050)	(0.114)***	(0.102)***
	[0.102]**	[0.117]	[0.601]**	[0.581]*
Kleibergen-Paap *F-stat*	3.7	5.4	5.3	5.3
Observations	20,390	28,359	26,921	25,486
Macroeconomic controls	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes

Dependent variables:	$\Delta^{02-07}i_j$	$\Delta^{02-07}\ln w_j$	$\Delta^{02-07}ROA_j$	$\Delta^{02-07}\ln TFP_j$
	2SLS	2SLS	2SLS	2SLS
	(5)	(6)	(7)	(8)
$\Delta^{02-07}\ln P_c$	0.022	-0.217	-0.344	-0.333
	(0.009)**	(0.061)***	(0.020)***	(0.042)***
	[0.015]***	[0.253]	[0.228]	[0.145]**
Kleibergen-Paap *F-stat*	7.1	5.4	5.3	5.3
Observations	9,717	27,339	27,471	25,066
Macroeconomic controls	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes