Can Real Estate Booms Hurt Small Firms? Evidence on Investment Substitution

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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 small firms, raise their borrowing costs, diminish their investment rate, compromise their output and productivity growth, and increase their exit rates. Such harmful effects are negligible among large firms due to weaker financial constraints. Using matched firm and product-level export data, we are able to discard local demand effects as an alternative explanation to the credit supply channel.

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

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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 large geographic variation in local real estate price changes make it a particularly interesting 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 city-prefectures with the highest and lowest real estate price change in the period 2002-7 with an average 154% and 33% real estate inflation, respectively; and compare the average annual investment rate[1] and output growth rate for small, medium, and large manufacturing firms located in these two groups of city-prefectures. Small firms as the most financially constrained group show a dramatic average shortfall of both their average investment rate and growth rate of 10 and 13 percentage points, respectively, in cities with large 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 for investment and growth of small firms in the World's largest economy represent a very important 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. Such a causal nexus is of great general interest as most countries have experienced episodes with large housing price increases. Our ability to show how a systematic increase in housing prices causes a reduction in firms' investment and corporate growth concerns policy makers in

[1] We measure firm's net investment rate as net investment relative to the real capital stock.

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. This is why China provides a particularly valuable inference opportunity. We are able to construct quasi-exogenous land supply measures at the city-level that have considerable explanatory power for local real estate price. Importantly, this land supply for residential housing does not correlate with local business cycle measures as it is strongly influenced by frictions in and between particular branches of the local public administration.

A second fundamental inference problem concerns the role of local demand factors that affect firm performance. Real estate booms can produce wealth effects and changes in local household consumption which also influence local product demand for the manufacturing sector. Thus, confounding demand effects could also influence firm outcomes — though in a manner quite distinct from the credit substitution channel. Again, Chinese data provide a special opportunity to filter such confounding demand effects. We match the firm data with detailed product-level export statistics from the Chinese custom authorities. Export demand is arguably independent of the demand conditions in the manufacturing location. If firm export performance nevertheless covaries strongly with the local financial conditions and local real estate price increases, the evidence for the credit and investment substitution channel becomes very strong. In addition, product data allow us to infer export performance based on *real* variables like the number of products shipped. This means that potential city-level mismeasurement of firm investment, output and productivity (based on incorrect industry-level product price deflators) can be excluded as an alternative explanation.

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.[2] This local credit crunch lowers the average corporate investment rate by 8.6 percentage points, which represents a large 24% reduction relative to the sample mean of 35.6 percentage points. The relative output decline amounts to a 37.4% 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.

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 more bank-dependent provinces with a higher initial share of external finance in firm investments, which strengthens our argument that real estate booms harm firm growth through a credit supply channel.

We also show that local product demand effects (related to local wealth or consumption switching effects) cannot account for the differential small firm development related to real estate booms. The analysis of firm-level custom data (for firms that export more than 75% of their output) show a parallel 20.2% shortfall in exports for firms in cities with a 50% higher real estate price index. The latter estimate for the effect of real estate prices on export performance identifies the *pure* credit substitution effect if we assume that the international product demand is independent of local real estate boom in China.[3] In addition, export prices show no pass-through effect of real estate prices and thus confirm that output deflators are not subject to any

[2]We note that a 50% relative increase in the annual real estate price [$\ln P_{c,t}/P_{c,t-1} = \ln 1.5 = 0.405$] corresponds to roughly three standard deviations of the annual variation in the real estate price index.

[3]In a related result we find that firms in more tradable industries (proxied by a larger export share) show a stronger investment and output decline during local housing boom. They suffer from the credit substitution associated with the real estate boom without the benefit of higher local demand from the same boom (Internet Appendix, Table A5).

systematic measurement biases across cities. The analysis of export data allow us to exclude local demand effects more convincingly than previous research.

Previous research on credit substitution used disaggregate loan-level credit data to document the crowding out of corporate loans (Khawaja and Mian, 2008; Chakraborty *et al.,* 2018). We do not dispose of loan-level bank data, and our empirical focus is instead on the real growth effects of credit substitution. Our "reduced form approach" relies on China's many institutional features (like high growth and large investment needs, strong geographic banking market segmentation, and fierce product market competition) in which credit substitution towards real estate investment can potentially generate large real effects (as illustrated in Figure 1). Observed in the world's largest economy, the real consequences of credit substitution represent an important topic for macroeconomic research separate from the documentation credit substitution itself.

To provide a deeper theoretical foundation for these real effects, we develop a modified Harrod-Balassa-Samuelson framework in which a construction 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 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.

An extensive new literature has focused on the real effects of the 2008 banking crisis, which originated in excessive real estate credit (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). But there is only limited evidence that real estate booms 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.

For our identification strategy we rely mainly on panel data, which characterizes the monopolistic land supply by local government for new residential housing relative to the existing housing stock. The government monopoly on land supply for residential property construction is a unique feature of the Chinese economy. Such land supply is very erratic due to frictions in the administrative approval process. The panel structure of the data permits the use firm fixed effects in the regressions. These neuter time-invariant cross-sectional influences on both firm outcomes and real estate inflation so that identification is based only on the erratic intertemporal land supply shocks specific to each city. We accept that the intertemporal land supply process may not be entirely dominated by exogenous frictions and local policy responses could also matter. However, if local governments react to high real estate prices with a larger or accelerated land supply, land supply variation is attenuated, real estate booms are flattened, and credit substitution less pronounced, which simply biases the estimated real effects downwards. Moreover, endogenous government response cannot easily account for triple differences such as the more pronounced negative real effects on financially constrained small firms and firms in more bank-dependent regions.

Moreover, the main results are robust for firms without linkage to the infrastructure sectors, which could be stimulated by local governments spending. 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.

2 Literature

Our analysis is predicated on a high degree of geographic capital 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, Pagano, and Panizza (2018, 2019) 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).

The real effects of credit supply shocks on corporate capital expenditure and growth have always been a key concern for financial 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). The new evidence from China shows that a large negative credit supply shock to the 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.

sector can alternatively originate in credit substitution to real estate finance. This channel also highlights that the structure and institutional features of the banking system matter for the collateral effects of real estate booms.

The macroeconomic literature has recognized that real estate markets and mortgage institutions can have an influence on the savings rate of households (Deaton and Laroque, 2001) and possibly growth. For example, cross-country variations in the loan to value ratios in mortgage markets affect the liquidity constraints of households, influence household saving rates and appear to correlate negatively with corporate investment rates and growth rates (Jappelli and Pagano, 1994). The substitution channel we highlight in this paper focuses not so much on the equilibrium saving rate *per se*, but more directly on savings that are diverted from corporate to housing investments if the latter promises higher returns during real estate booms.

Recent finance research has examined the relationship between real estate booms and corporate investment by U.S. firms. For firms with real estate property, a local property price increase can relax borrowing constraints and increase firm investment or employment expenditures (Chaney *et al.*, 2012; Jiménez *et al.*, 2020; Ersahin and Irani, 2020). For Chinese firms this balance sheet effect is unlikely to matter much because of a lack of real estate assets on firms' balance sheets and the state's monopoly of land development. Among Chinese listed firms in 2007, only 35% report positive real estate assets and their aggregate value accounts for only 2.6% of aggregate assets. 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. 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.

Unlike a collateral channel, the credit substitution channel concerns all bank-dependent firms with potentially wider economic ramifications. The literature on financial stability has often highlighted real estate booms as a precursor of financial crisis through imprudent bank lending (IMF, 2011). The negative effects of such booms on the real sector through reduced credit and a loss of competitiveness are apparently important features of recent financial crises in southern Europe (Sinn, 2014; Martín *et al.*, 2018) — yet identifying a clear causal link between real estate booms and deteriorating firm performance has generally been difficult.

An exception here is the evidence by Chakraborty *et al.* (2018) showing that local real estate booms adversely affect the volume and cost of business loans from U.S. banks. 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 an even 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 8.6 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. But only our paper undertakes a comprehensive analysis of the real effects of bank credit substitution on firm performance. Its key contribution is to document the large economic significance for China.

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 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.

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[5], real estate inflation can be

[5]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).

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.

We treat each city as a closed economy with a fixed factor supply of capital and constructible land. The traditional Harrod-Balassa-Samuelson literature generally assumes perfect capital market integration. However, a constrained local capital supply provides a better empirical benchmark for China. Its internal capital market appears to be segmented with only limited capital flows compensating for capital demand shocks across cities (Huang *et al.*, 2020). Many restrictions on banking across various administrative units contribute to the regional segmentation of the corporate credit market. In the Appendix C, 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 credit markets.[6]

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

[6] 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.

Boom

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 Capital Access Within Cities

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 likely to exit due to capital chain rupture. Within a city, these effects should be

[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).

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 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 firm's headquarters is identified so that we can match additional city-level statistics—in particular to the local real estate market.

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 Appendix D, 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 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

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

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. Appendix E 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.

Productivity research generally infers real quantities by applying industry-specific price deflators to revenue statistics. These deflators are not firm-specific and could potentially introduce a measurement bias if firm-specific output prices and industry-wide averages systematically diverge as a function of local real estate prices. To address this concern, we match the ASIF data with additional Chinese customs data that provide quantity and price information at the firm and product level for the period of 2002–2006. Specifically, we retain all firms that export more than 75% of their output and track their various exported items in time-consistent measurement units, i.e. in number of units, weight, volume, etc. The product-level data (at the six-digit product code) is aggregated for each firm into a maximum of 49 different product categories by quantity and unit price. The aggregate quantity is the sum of items in the same measurement units, and the unit price is the ratio of aggregate value to aggregate quantity. This procedure provides a direct real measure of export quantity that is not subject to any price mismeasure-

[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.

ment. For export-oriented firms, such a quantity measure should be a good substitute for real output and informative about firm performance. In conclusion, we define for each firm j one or more product categories i and measure the annual (log) export value ($\ln ExpValue_{i,j,t}$), the (log) export quantity ($\ln ExpQuantity_{i,j,t}$) and the (log) export (unit) price ($\ln ExpPrice_{i,j,t}$). Focusing on export quantity allows a robust analysis without any price distortions.

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." , 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. The annul real price inflation (of residential housing) $\ln P_{c,t}/P_{c,t-1}$ has an average annual (log) growth rate of 8.5% with a large standard deviation of 14.1%. Our sample is dominated by boom years: We find annual price declines for only 24.1% 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 $L_{p,t}/L_{p,t}^{NI}$ of residential to non-industrial land supply at the province level and use this ratio to adjust the city-level non-industrial land supply according to

$$L_{c,t} = \frac{L_{p,t}}{L_{p,t}^{NI}} L_{c,t}^{NI}. \tag{2}$$

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

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 $\ln L_{c,t}^{NI}$ and the industrial land supply $\ln L_{c,t}^{I}$ 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) 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. 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.

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%.[10] 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.

[10] 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 A2 in 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 shown in the Section 6.1.

4.3 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. Empirically, cities with a lower per capita income and a lower population density tend to have a more price inelastic demand (i.e. a low γ_c and a large $|\eta_c|$) — presumably because real estate acquisitions are accessible to a smaller population. This makes poorer cities particularly useful observations for the purpose of our analysis (compared to richer cities), because a shock to the *Relative Land Supply* triggers a larger change of the real estate price, $\widehat{P}$, as shown in Eq. (12), and also a quantitatively larger effect on the tradable firm variables in Eqs. (10)-(11), and (13)-(14). By contrast, 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

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

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 $\ln \textit{Relative Land Supply}_{c,t-1} \times \widehat{\eta}_c$, 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 Appendix Table A9.

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.[11] 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 flexibile and parsimonious. We use this latter approach to construct our preferred instrument called the *Adjusted Land Supply*; formally

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

[11] 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 first stage regression then follows as

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

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 2 reports 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 $\widehat{\eta}_c$ are positive. In additional robustness tests, we discard such cities, and focus only on those cities with a strictly negative elasticity estimate. We find quantitatively similar results, but obtain slightly stronger point estimates relative to the full sample. This is not surprising if the truncated sample features cities with a lower average housing demand elasticities γ_c, and thus more negative parameters η_c. In the following analysis, we proceed with the full sample of 202 cities, and report the subsample results in Table A3 of the Appendix.

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 2, namely lagged local (log) GDP, (log) population, local (log) government expenditure and revenue, the ratio of the local government budget deficit to GDP, and share of park area. 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 3. 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 2). Panel C instead uses the city-specific elasticity instrument (see Column (2) in Table 2). 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 tripples 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 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 [$= -0.211 \times \ln(1.5)$] in Panel D, which is large compared to a mean sample value of 35.6 percentage points. 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 -0.097 in Panel D implies that a 50% higher real estate price reduces the percentage of firms with bank credit by 3.9 percentage points [$= -0.097 \times \ln(1.5)$] 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 (instrumented) real estate prices $\ln P_{c,t}$. A 50% higher real estate price induces an output decrease of approximately 37.4% [$= -0.923 \times \ln(1.5)$] 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 benchline specification. Table 4, Column (1) shows the OLS estimates for the interest rate on bank loans. The point estimate is positive at 0.009 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 4, 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 -0.470 and -0.455, respectively. Here, a 50% increase in real estate prices is associated with 19.1% [$= -0.470 \times \ln(1.5)$] 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 3 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 4. 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 Credit Substitution versus Local 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). We can eliminate such endogenous demand effects from our analysis by considering firm exports as a measure of firm performance. The identifying assumption is that credit supply shocks adversely effect 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 accounts 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 firms that export more than 75% of their output, we consider the export statistics as a good (real) performance measure devoid of any local demand effect. One average, these firms export in 3.8 different product categories.

Table 5 analyzes the (real) export performance of Chinese firms as a function of local real estate prices. Column (1) replicate the 2SLS regression of Table 3, Panel D, Column (5) for the subsample of exporting firms to establish the benchmark results. At a coefficient of -0.852, local real estate inflation shows a similar negative effect on firm output for exporting firms than in the full sample (-0.923). Columns (2) of Table 5 estimates a firm's (log) export value

($\ln ExpValue_{i,j,t}$) as a function of the real estate price. The estimated export elasticity is at -0.499 large: the relative decline in export value amounts to 20.2% [$= -0.499 \times \ln(1.5)$] for a 50% increase in local real estate prices. However, the overall output elasticity is still larger at -0.852 and the discrepancy could be explained by local demand effects. Nevertheless, a fairly high estimate for the export elasticity supports the credit supply channel because export demand is presumably unrelated to local Chinese real estate prices.

The Chinese export data also allow us to address an important measurement issue. Both the investment rate measure and the (revenue-based) value-added output rely on industry-level output and intermediate input prices that might be systematically biased downward for cities with higher real estate prices. Any incorrect inflation adjustment could imply that the residuals of the second-stage regression correlate with our instrument. Columns (3) and (4) of Table 5 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. We see clearly that higher real estate prices covary with lower export quantities, but not with product prices. This implies that there is no price pass-through from local real estate inflation to export prices. Using industry level price deflators rather than firm level price deflators should therefore not pose a major inference issue.

Rather than concentrating the analysis on the subsample of firms with exports, we can alternatively measure the tradability of firm output at the industry level by the industry-specific export share at the beginning of the sample. Firm's characterized by high output tradability 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. Table A4 in the Appendix shows that the local demand effect in boom cities indeed decreases with the tradability of industrial output. A high export share comes with a stronger investment and output decline as such firm suffer from the saving displacement to real estate investment without benefiting much from the local housing boom.

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 1.9 percentage points $[= (-0.222 + 0.175) \times \ln(1.5)]$ for a 50% higher local real estate price relative to an investment shortfall of 9.0 percentage points $[= -0.222 \times \ln(1.5)]$ observed for privately-owned firms. We also note that firms with better financial market access feature lower output and labor productivity decline.

It is 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 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. Table A5 in the Appendix 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.

Further, 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 the Appendix Table A6, Columns (1)-(2), we show that the 7.4% of firms marked by the dummy $D\_\ Collateral_j = 1$ show indeed a 25.1% [= 0.055/0.219] 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.

Finally, we construct a measure of bank dependence at the province level based on the ratio of firm fixed investment financed by loans to total fixed investment in the year 2000. This measure ranges from 10.2% for Shandong province to 30.6% for Guizhou province with a median value of 19.9%. Intuitively, if real estate booms harm firm growth through credit substitution, firms in more bank-dependent provinces should be more affected. Table A7 in the Appendix splits the sample at the median into bank-dependent and non-bank-dependent provinces.[12] 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 results are also consistent with a bank credit supply channel that is disrupted by credit diversion in a real estate booms.

[12] 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.

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.

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. The negative effect on firm profitability is even stronger for financially constrained firms as shown in Column (3) with a baseline coefficient of -0.225. Asset rich firms and SOEs are less affected as indicated by the positive coefficient of 0.010 and 0.068 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 0.021 and 0.071 for asset rich firms and SOEs in Column (6) imply that this average effect varies considerably with firm characteristics. But even for a large SOE, the average loss of competitiveness is economically significant: At the 75% quantile of fixed asset size ($\ln Fixed\ Assets_{j,t} = 8.84$), the relative loss in TFP is still 7.9% [$= (-0.451 + 0.021 \times 8.84 + 0.071) \times \ln(1.5)$] for a 50% higher local real estate price.

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 3.8 percentage points [$= 0.093 \times \ln(1.5)$].[13] 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 Endogeneity Concerns about the Land Supply

In Table 8, we explore some specific endogeneity concerns with respect to the *Relative Land Supply*$_{c,t}$ by regressing it on a variety of city-level economic variables. Panel A, Column (1), shows that it does not covary with a higher GDP or population growth. Neither is the *Relative Land Supply*$_{c,t}$ significantly correlated with city-level government expenditure, revenue and deficit. This suggests that a city's financial situation does not influence land supply for our data period.[14] Since land sales gradually become an important source of local government revenues (Fang *et al.*, 2016), a potential concern is that local governments finance local infrastructure construction through land sales, which might influences local manufacturing firms indirectly.

Column (2) shows that the relationship between the *Relative Land Supply*$_{c,t}$ and contemporaneous government infrastructure expenditure is statistically insignificant. Local governments might use revenue from land sales to finance local infrastructure investments, which in turn could create demand effects for local manufacturing firm related to the construction sector. We highlight that our main results in Table 3 extend to the 50 percent of manufacturing firms

[13]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.

[14]The regressions in Table 8, Column (1), pool all these variables. We note that regressions including each variable separately lead to the same conclusion.

least related to the infrastructure sector in terms of input-output linkages (see Table A8 in Appendix).

Another potential endogeneity concern is that residential land supply might substitute or be complementary to a city's industrial land supply policy. Column (3) explores such a relationship by using the industrial land supply as an explanatory variable for the (residential) *Relative Land Supply.* Again, no systematic relationship of statistical significance appears. Previous work by Li and Zhou (2005) and Hsu *et al.* (2017) suggest that the age of the local party leader and his tenure (years in office) influences local policies through promotion incentives. Panel A, Column (4) shows that these variables do not covary with our instrument.

Panel B first investigates if a city's past GDP growth predicts the *Relative Land Supply.* Also past growth in a city's college population ($\Delta \ln College\ Students$) may create incentives for local government to improve living conditions for high-skill workers who might (in the future) able to afford new housing. Yet, none of these (lagged) variable has any explanatory power for our instrument. Furthermore, land supply could also plausibly correlate with a transformation of industrial structure from a manufacturing to a service oriented economy. Hence, we include in Column (2) the change in the ratio of output in the secondary sector relative to total GDP ($\Delta Secondary\ Industry\ Share_{c,t-k}$) at lags $k = 0$ and $k = 1$. Again, this variable features no explanatory power.

Ambitious city development projects — motivated by the career and promotion concerns of top city officials — could also be linked to land sales as a source of revenue (Tian and Ma, 2009; Lichtenberg and Deng, 2009; Chen and Kung, 2016). Panel B further proposes a variety of proxies for future infrastructure development such as (log) growth of local government expenditure ($\ln Gov.\ Expenditure_{c,t+k}$) in Column (3), or growth in highway mileage ($\Delta \ln Highways_{c,t+k}$), and growth in the number of public buses ($\Delta \ln Bus_{c,t+k}$) in Column (4) with $k = 1$ and $k = 2$. Overall, we find no evidence that the realized residential land supply by local government is related to a city's infrastructure upgrades. Improved local infrastructure could benefit the local manufacturing sector, whereas it is hard to see how the latter could benefit directly from a higher residential land supply.

Table 8 supports our instrument choice because the *Relative Land Supply* is uncorrelated with meaningful measures of past, contemporaneous, and future city development that could influence simultaneously local factor prices and manufacturing firm performance. It is difficult to rule out a limited scope for reverse causality: city governments could try to supply more residential land in direct response to high local real estate inflation.[15] But this particular endogeneity has an attenuating influence on the cross-sectional variation of real estate inflation documented in Figure 2, and biases the 2SLS estimates of all real effects towards zero. It cannot *per se* generate false positive results.

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. Table A10 in the Appendix 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 A3 in the 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

[15] However, various commentators note that local government reacted to increasing real estate prices by imposing eligibility restriction on purchasers, raising down payment requirement, and increasing indemnificatory housing (only for low income families) rather than increase land supply for residential commodity housing.

strong instrument.

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

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

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 A11 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 3: 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 -0.195 in Table 3, 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 3. 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 4, 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 4, 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 household savings 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 net investment rate drops by 5.5 percentage points (relative to a mean of 23 percentage points) and value-added firm output is lower by a large 37.4%. 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. In this sense, our evidence points to substantial benefits from credit market integration and a more efficient market-based capital allocation in China.

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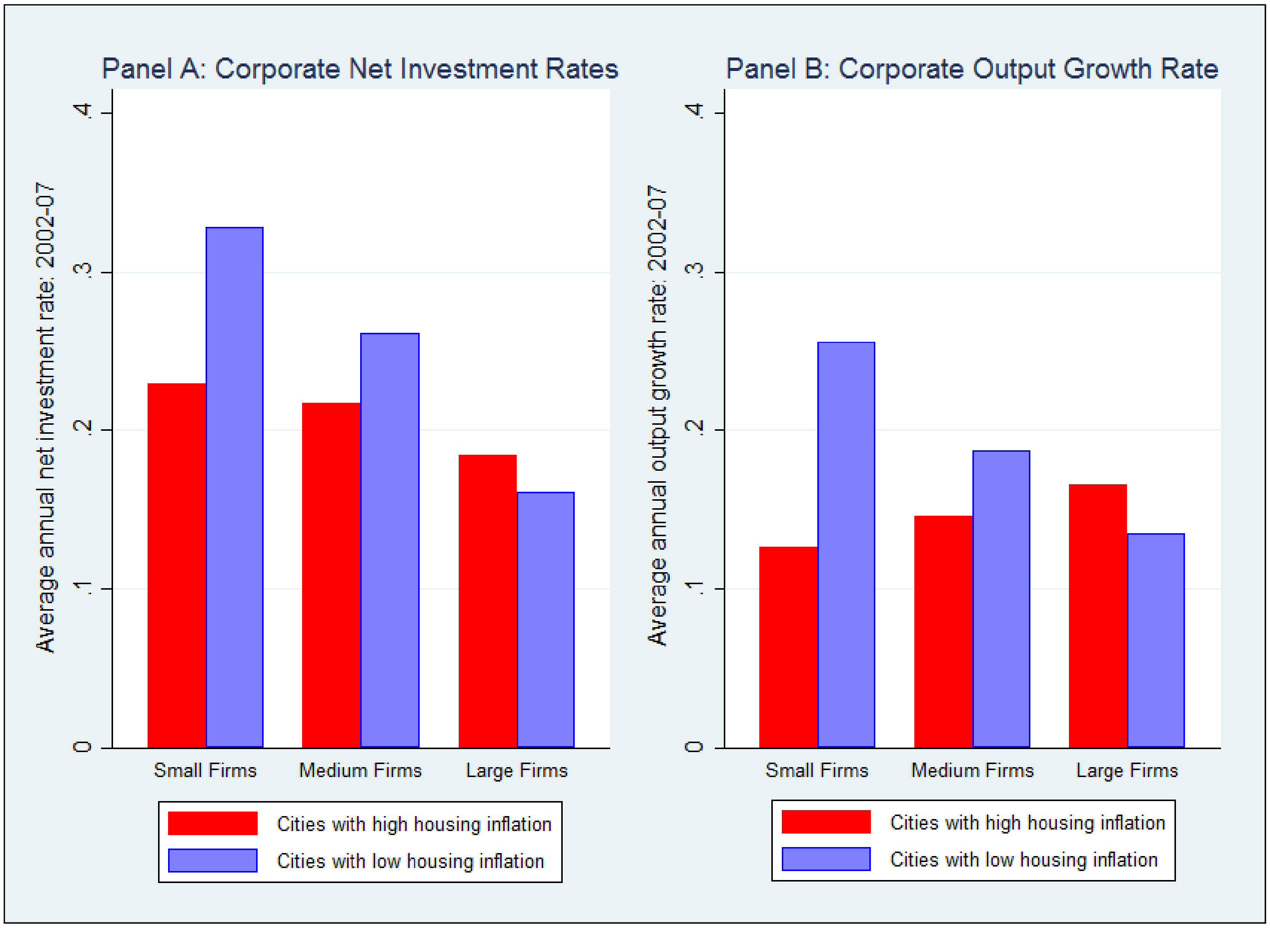

Figure 1: Among China's city-prefectures, we identify the 50 cities with the highest and 50 cities with the lowest real estate price increase in the period 2002-07, which features an average real estate price increase of 144% and 30%, respectively. For manufacturing firm located in one of the two city 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.

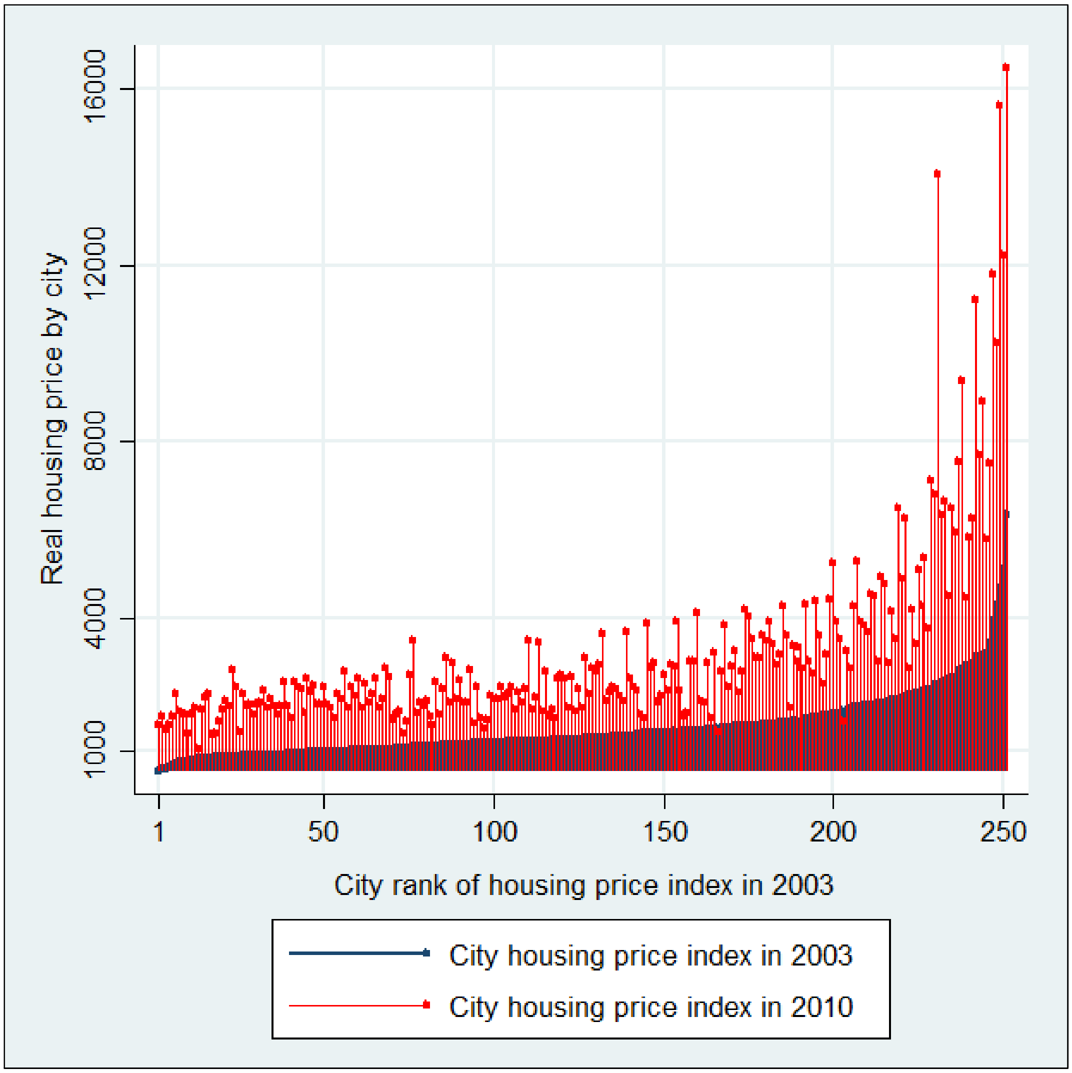

Figure 2: We rank 251 Chinese cities by their local housing price index in 2003 (blue spikes) and compare them the house price index in 2010 (red spikes).

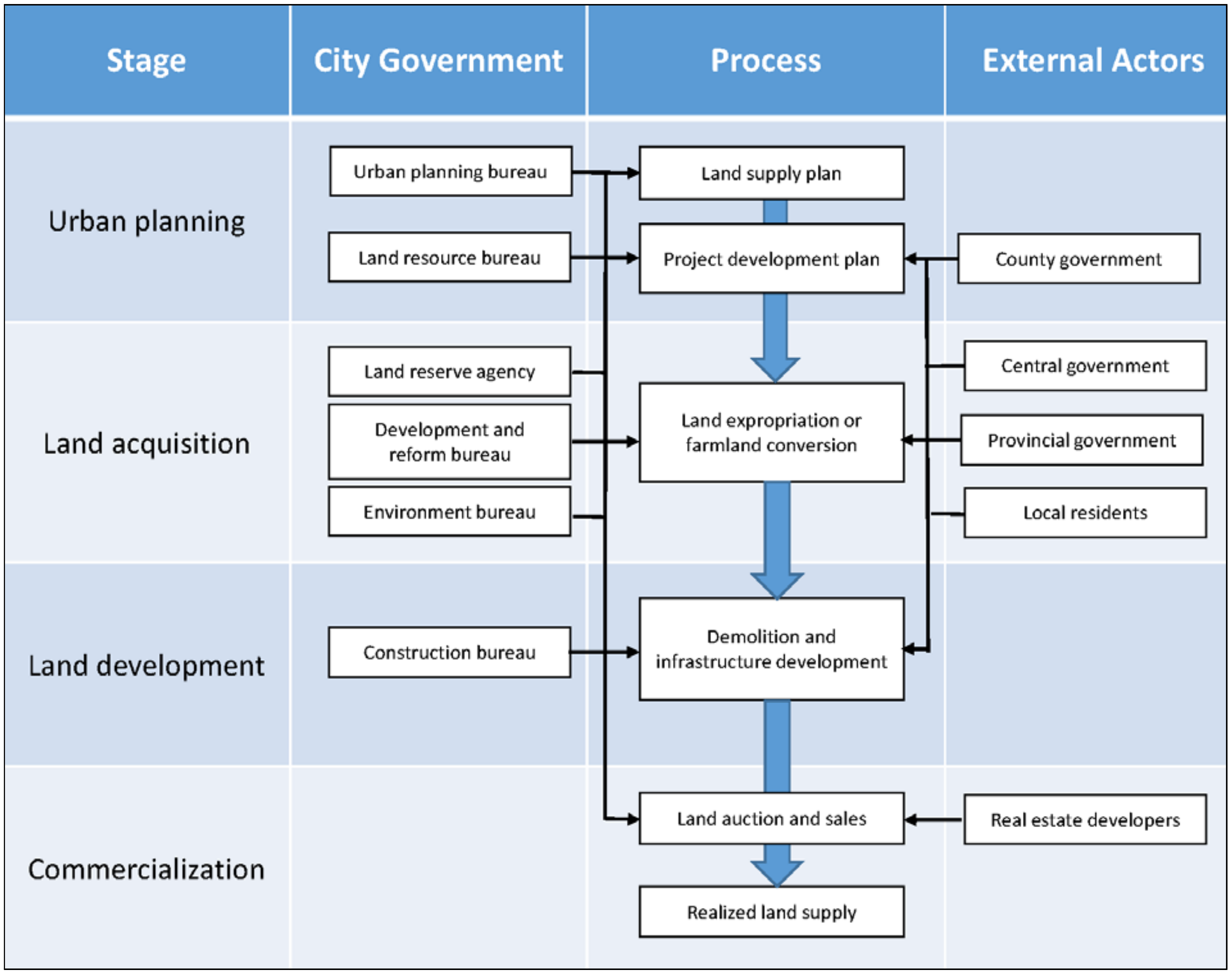

Figure 3: Stages of the bureaucratic land supply process for residential housing in China. Listed are major city-level agencies involved and external actors interfering in the process. Friction prone intragovernmental coordination, property rights, and policy conflicts generate significant discrepancies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

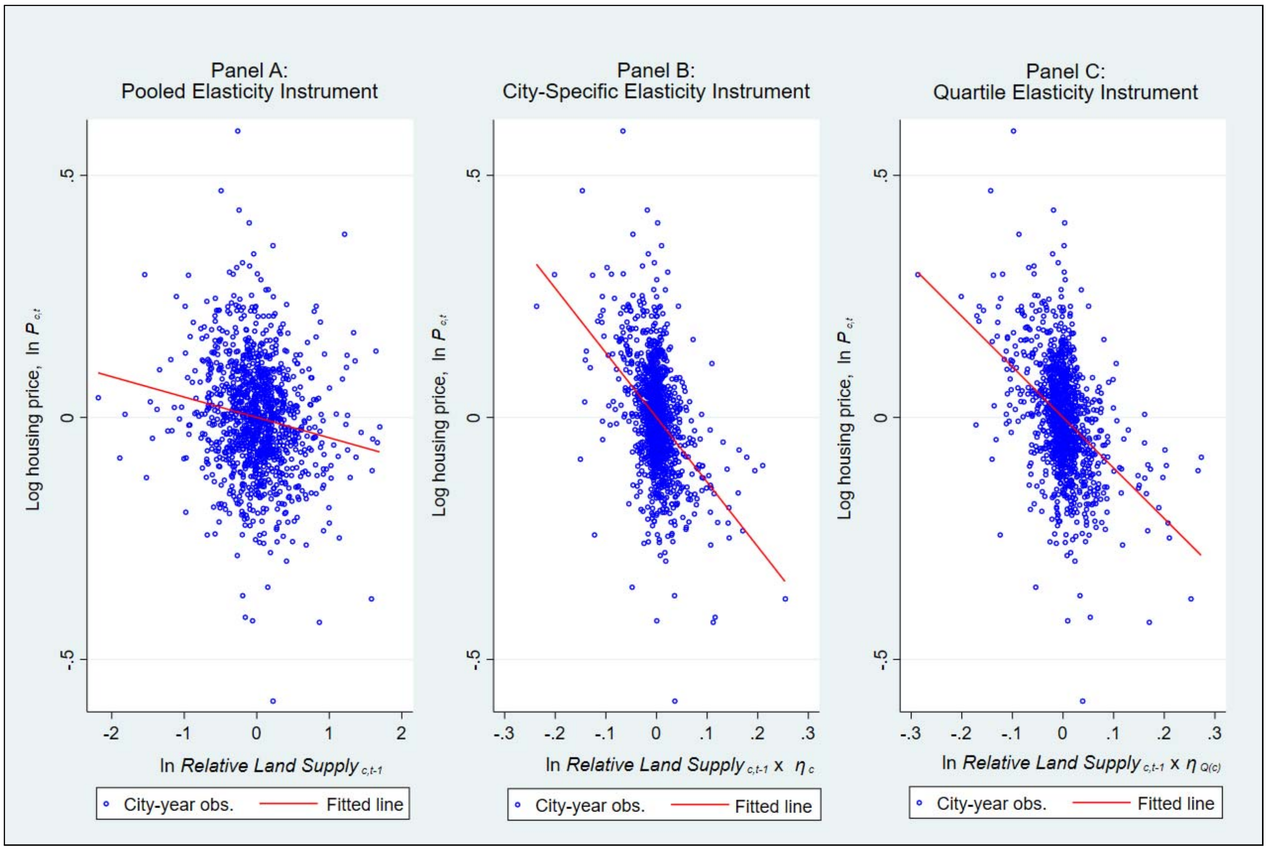

Figure 4: We plot city-year observation for the period 2001-10 showing the (log) housing prices ($\ln P_{c,t}$) on the y-axis against three instruments: Panels A plots as the x-axis the pooled elastiticity instrument (i.e. $\ln$*Relative Land Supply*$_{c,t-1}$) by imposing the restriction $\eta_c = 1$, 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.

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 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. We match additional product-level information from the Chinese customs authorities, which decomposes the annual (log) export value ($\ln ExpValue_{j,t}$) into firm-level export quantity ($\ln ExpQuantity_{j,t}$) and unit price ($\ln ExpPrice_{j,t}$). Summary statistics at the city level (indicated by subscript c) are the (log) average real house price ($\ln P_{c,t}$) for residential housing, annual change of (log) average real house price ($\ln P_{c,t}/P_{c,t-1}$) 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 $\widehat{\eta}_{Q(c)}$.

	Obs.	Mean	SD	Q25	Q50	Q75
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Firm-level variables						
$I/K_{j,t}$	741,098	0.356	1.133	0	0.053	0.283
$Loan_{j,t}$ (dummy)	1,007,944	0.340	0.474	0	0	1
$\ln Y_{j,t}$	985,420	8.724	1.187	7.846	8.584	9.470
$\ln(Y/L)_{j,t}$	985,993	3.966	0.958	3.298	3.902	4.592
Firm wage: $\ln w_{j.t}$	987,357	2.579	0.564	2.223	2.550	2.906
Firm bank loan rate: $i_{j,t}$	458,104	0.063	0.046	0.028	0.052	0.087
$Exit_{j,t}$	1,007,944	0.098	0.297	0	0	0
$ROA_{j,t}$	988,136	0.074	0.125	0.006	0.036	0.101
$\ln TFP_{j,t}$	923,684	1.133	0.341	0.972	1.187	1.364
$\ln Fixed\ Assets_j$	339,808	7.788	1.801	6.750	7.779	8.843
SOE_j (Dummy)	339,808	0.056	0.230	0	0	0
$\ln ExpValue_{j,t}$	191,972	11.70	3.320	9.359	12.14	14.29
$\ln ExpQuantity_{j,t}$	191,972	10.45	3.561	7.982	10.78	13.18
$\ln ExpPrice_{j,t}$	191,972	1.254	1.782	0.262	1.107	2.083
Panel B: City-level variables						
$\ln P_{c,t}$	1,212	7.399	0.466	7.082	7.316	7.665
$\ln P_{c,t}/P_{c,t-1}$	1,010	0.085	0.141	0.006	0.084	0.172
$\ln$*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

Table 2: Housing Prices and Adjusted Land Supply

The (log) real estate 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. Expenditure*$_{c,t-1}$) 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. Expenditure*$_{c,t-1}$	-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
Year fixed effects	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes
Observations	1,212	1,212	1,212
R-squared	0.660	0.715	0.718
Number of cities	202	202	202

Table 3: House 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, 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*$_{c,t-1}$, 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. 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				
$\ln P_{c,t}$	-0.102^{***}	-0.018	-0.298^{***}	-0.334^{***}
	(0.037)	(0.020)	(0.076)	(0.069)
Panel B: 2SLS with pooled elasticity instrument				
$\ln P_{c,t}$	-0.360^{*}	-0.244^{**}	-1.852^{***}	-2.021^{***}
	(0.209)	(0.123)	(0.640)	(0.652)
Kleibergen-Paap *F-stat*	5.1	7.0	7.1	7.0
Panel C: 2SLS with city-specific elasticity instrument				
$\ln P_{c,t}$	-0.187^{***}	-0.117^{**}	-0.872^{***}	-0.942^{***}
	(0.049)	(0.057)	(0.149)	(0.110)
Kleibergen-Paap *F-stat*	47.4	41.8	40.8	40.9
Panel D: 2SLS with quartile-specific elasticity instrument				
$\ln P_{c,t}$	-0.211^{***}	-0.097^{**}	-0.923^{***}	-0.961^{***}
	(0.052)	(0.057)	(0.184)	(0.137)
Kleibergen-Paap *F-stat*	125.4	135.1	135.9	135.5
Panel E: 2SLS like in Panel D with additional industry×year fixed effects				
$\ln P_{c,t}$	-0.195^{***}	-0.085^{**}	-0.958^{***}	-0.978^{***}
	(0.052)	(0.041)	(0.188)	(0.141)
Kleibergen-Paap *F-stat*	133.8	143.5	144.3	144.2
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Observations	740,900	1,007,944	985,420	985,993

Table 4: 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 $\times$ 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***	−0.470***	−0.455***
	(0.002)	(0.007)	(0.007)	(0.048)	(0.129)	(0.132)
Kleibergen-Paap *F-stat*		145.2	153.3		134.8	143.3
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
Observations	458,104	458,104	458,104	987,357	987,357	987,357
Number of cities	202	202	202	202	202	202

Table 5: Export Performance Measured at the Product Level

For a subsample of exporting firms with an export share larger than 75% of output, 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)
$\ln P_{c,t}$	-0.852^{***}	-0.499^{**}	-0.473^{**}	-0.026
	(0.233)	(0.197)	(0.192)	(0.093)
Kleibergen-Paap *F-stat*	35.9	30.0	30.0	30.0
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Observations	68,820	190,008	190,008	190,008

Table 6: House Prices and Firm Heterogeneity in Bank Access

Different measures of firm production 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 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. 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				
$\ln P_{c,t}$	-0.953^{***}	-0.303^{***}	-1.148^{***}	-1.153^{***}
	(0.205)	(0.059)	(0.164)	(0.135)
$\ln P_{c,t} \times \ln Fixed\ Assets_j$	0.092^{***}	0.026^{***}	0.029	0.024^{*}
	(0.023)	(0.005)	(0.021)	(0.013)
Kleibergen-Paap *F-stat*	53.4	42.7	38.2	43.4
Panel B: Interaction with *SOE* dummy				
$\ln P_{c,t}$	-0.222^{***}	-0.102^{**}	-0.936^{***}	-0.976^{***}
	(0.054)	(0.047)	(0.183)	(0.139)
$\ln P_{c,t} \times SOE_j$	0.175^{***}	0.096^{**}	0.274	0.299^{*}
	(0.058)	(0.048)	(0.285)	(0.160)
Kleibergen-Paap *F-stat*	15.6	10.2	9.9	10.3
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Observations	741,098	1,007,944	985,420	985,993

Table 7: House Prices and Other 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 two different proxies for firm bank access, namely (log) fixed assets (ln $Fixed\ Assets_j$) as a measure of collateral availability and a dummy for state-owned enterprises (SOE_j). 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^{***}	-0.225^{***}	-0.101^{***}	-0.285^{***}	-0.451^{***}
	(0.012)	(0.029)	(0.044)	(0.021)	(0.041)	(0.060)
$\ln P_{c,t} \times \ln$ $Fixed\ Assets_j$			0.010^{***}			0.021^{***}
			(0.003)			(0.005)
$\ln P_{c,t} \times SOE_j$			0.068^{*}			0.071^{**}
			(0.035)			(0.034)
Kleibergen-Paap *F-stat*		134.9	7.0		134.8	7.3
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
Observations	988,136	988,136	988,136	923,684	923,684	923,684

Dep. variables:	$Exit_{j,t}$		
	OLS	2SLS	2SLS
	(7)	(8)	(9)
$\ln P_{c,t}$	-0.028	0.093^{**}	0.399^{***}
	(0.030)	(0.046)	(0.079)
$\ln P_{c,t} \times \ln$ $Fixed\ Assets_j$			-0.038^{***}
			(0.005)
$\ln P_{c,t} \times SOE_j$			-0.035
			(0.038)
Kleibergen-Paap *F-stat*		135.1	7.0
Macroeconomic controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes
Observations	1,007,944	1,007,944	1,007,944

Table 8: Determinants of Land Supply

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 various potential determinants of the *Relative Land Supply*$_{c,t}$, namely city-level variables in Panel A, measures of past and future city development in Panels B. In Panel A, the explanatory variables in Column (1) include annual city-level statistics for the (log) gross domestic product ($\ln GDP_{c,t}$), (log) city population ($\ln Population_{c,t}$), the annual (log) expenditure ($\ln$ *Gov. Expenditure*$_{c,t}$) by the city government, its annual (log) revenue ($\ln$*Gov. Revenue*$_{c,t}$), the ratio of the local government budget deficit (expenditure minus revenue) to GDP (*Gov. Deficit*$_{c,t}$) and the percentage of park area within the urban area (*Park Share*$_{c,t}$). The explanatory variable in Column (2) is the (log) government's direct expenditure on local infrastructure construction ($\ln$*Infrastructure Expenditure*$_{c,t}$). The explanatory variable in Column (3) is the (log) land supply for industrial purpose ($\ln$*Industrial Land*$_{c,t}$). In Column (4), the explanatory variables are the age of local communist party secretary (*Party Leader Age*$_{c,t}$) and his or her tenure year (*Party Leader Tenure*$_{c,t}$). In Panel B, the explanatory variables in Column (1) include (log) growth of GDP and number of college students ($\Delta \ln$*College Student*$_{c,t-k}$) at lags $k = 0$ and $k = 1$. In Column (2), the explanatory variable is the change in the ratio of employment in the secondary sector relative to total employment (Δ*Secondary Industry Share*$_{c,t-k}$) at lags $k = 0$ and $k = 1$. The explanatory variables in Column (3) are the (log) growth of government expenditure ($\Delta \ln$*Gov. Expenditure*$_{c,t+k}$), and in Column (4) measures of future infrastructure growth, namely the (log) growth in highway mileage ($\Delta \ln$*Highway*$_{c,t+k}$), and the number of public buses ($\Delta \ln Bus_{c,t+k}$) with $k = 1$ and $k = 2$. All regressions control for the city and year 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.

Dependent variable:	$\ln$*Relative Land Supply*$_{c.t}$			
	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Panel A: City characteristics				
$\ln GDP_{c,t}$	−0.361			
	(0.328)			
$\ln Population_{c,t}$	−0.413			
	(0.344)			
$\ln$*Gov. Expenditure*$_{c,t}$	0.018			
	(0.125)			
$\ln$*Gov. Revenue*$_{c,t}$	0.026			
	(0.111)			
Gov. Deficit$_{c,t}$	0.240			
	(1.984)			
Park Share$_{c,t}$	−0.008			
	(0.009)			
$\ln$*Infrastructure Expenditure*$_{c,t}$		0.031		
		(0.026)		
$\ln$*Industrial Land*$_{c,t}$			0.023	
			(0.035)	
Party Leader Age$_{c,t}$				0.004
				(0.008)
Party Leader Tenure$_{c,t}$				0.021
				(0.013)
Observations	1,212	1,152	795	1,102
R-squared	0.218	0.209	0.025	0.209

Table 8 continued

Dependent variable:	ln $Relative\ Land\ Supply_{c.t}$			
	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Panel B: Past and future city development				
$\Delta \ln GDP_{c,t}$	0.281			
	(0.321)			
$\Delta \ln GDP_{c,t-1}$	−0.025			
	(0.216)			
$\Delta \ln\ College\ Student_{c,t}$	0.022			
	(0.024)			
$\Delta \ln College\ Student_{c,t-1}$	−0.015			
	(0.020)			
$\Delta Secondary\ Industry\ Share_{c,t}$		0.003		
		(0.005)		
$\Delta Secondary\ Industry\ Share_{c,t-1}$		0.0002		
		(0.004)		
$\Delta \ln Gov.\ Expenditure_{c,t+1}$			−0.029	
			(0.052)	
$\Delta \ln Gov.\ Expenditure_{c,t+2}$			0.009	
			(0.047)	
$\Delta \ln Highway_{c,t+1}$				−0.045
				(0.044)
$\Delta \ln\ Highway_{c,t+2}$				−0.065
				(0.077)
$\Delta \ln Bus_{c,t+1}$				0.020
				(0.100)
$\Delta \ln Bus_{c,t+2}$				−0.120
				(0.085)
Observations	1,159	1,212	1,211	1,203
R-squared	0.218	0.206	0.205	0.210

Internet Appendix

Can Real Estate Booms Hurt Small Firms? Evidence on Investment Substituion

Not for Journal Publication

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Internet Appendix

A. 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.[1]

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 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.

[1] 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.

To simplify notation, we express all variables in percentage changes relative to steady state log values, that is $\widehat{X} = dX/\overline{X}$.[2] 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.

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

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

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 $1 - \gamma_c \lesssim 1$.[3] 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.

B. 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

[3] 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 A1 in the Internet Appendix.

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 $\overline{X}$ repesents 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

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

The variables $\overline{K}_R$, $\overline{K}_T$, $\bar{i}$, 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 $\widehat{P} > 0$.

C. Persistence of Corporate Loans Rate Differences Across Cities

We identify the loan rate of corporate bank debt from balance sheet data: the annual interest expenses are divided by the outstanding (bank) debt at the beginning of the year. For all firms with bank debt in a given city we calculate the median city-level bank loan rate $i_{c,t}$ and compare it to the median bank loan rate of all firms in the neighbouring cities/prefectures denoted by $i_{c\_nb,t}$. Summary statistics are reported in Table 1 of the paper. To evaluate persistence of loan rate differences across cities we estimate an error correction model

$$\Delta i_{c,t+1} = i_{c,t+1} - i_{c,t} = \alpha + \beta\left(i_{c,t} - i_{c\_nb,t}\right) + \epsilon_{c,t}, \tag{C1}$$

by pooling the data across 202 cities. We report the results in Table A1.

D. Sample Construction

Our data source is the Annual Survey of Industrial Firms (ASIF) over the period 1998–2008. This survey covers industrial firms in the three sectors of mining, manufacturing, and utilities (water, electricity, gas production and supply). This section describes the data-cleaning procedures:

1. We drop firm-year observations before 2001 and after 2007 to focus on the period 2002–2007. Only in the calculation of net investment rates in 2002 do we use a firm's capital stock in 2001.

2. We drop all firms in the mining and utility sector from the sample and focus on manufacturing firms. The corresponding two-digit industry codes are 13–43 according to GB/T 4574–2002.

3. We discard firms in Tibet because of data quality concerns.

4. We drop firm-year observations reported not to have an "operation status."

5. We drop firm-year observations whenever the gross operating income is below RMB 5 million.

6. We discard all firm-year observations reporting fewer than eight employees.

7. We drop firm-year observations reporting negative value-added.

8. We drop firm-year observations reporting a firm age of more than 100 years.

9. We do not use firm-year observations where the corresponding city-year information on housing prices and other macroeconomic control variables is not available.

10. We drop firms in the (i) tobacco and (ii) waste resources and waste materials recycling and processing industry. These two industries have too few observations to allow us to control for industry-year fixed effect in the panel regression.

The raw data comprise 2,635,787 firm-year observations, corresponding to 689,010 distinct firms. After these cleaning procedures, the gross sample has around 1 million firm-year observations belonging to 339,808 distinct firms. To mitigate the role of reporting errors, we also discard firm-year observations in the top 1% and bottom 1% percentiles for each variable. For the real interest rate i we only use firm-year observations in the 10% to 90% percentiles because this variable is estimated as interest cost over outstanding debt. Since a firm-year observation represents an outlier in one regression specification, but does not feature in another, the sample size can vary depending on regression specification.

E. Real Investment and Capital Stock Calculation

Under price inflation, the purchasing year of new capital matters for the real book value of a firm's asset. To obtain the real value of capital, net new investment should be deflated with the deflator corresponding to the investment year. Formally, the real book value follows as

$$K_T = \sum_{s=t}^{T} Deflator_s \times \widetilde{NI}_s + K_t \tag{E1}$$

where K_t is the (begining-of-year) initial real book value at the year of entering survey, $\widetilde{NI}_s$ the nominal net new investment in period s, and $Deflator_s$ the price deflator for investment goods in period s. The initial real book value is unobservable and can at best be proxied by an adjustment to the nominal book value $\widetilde{K}_t$. We assume that a firm's (unobservable) nominal book value before entering the survey (prior to t) follows the growth rate of nominal fixed assets in the firm's the two-digit industry, hence

$$\widetilde{K}_t = \widetilde{K}_0 \prod_{\tau=0}^{t} (1 + r_\tau), \tag{E2}$$

where $\widetilde{K}_0$ is the nominal book value when the firm starts operation in period 0, and r_τ the industry-level growth rate of nominal fixed asset in period τ. For the evolution of the nominal book value $\widetilde{K}_0, \widetilde{K}_1, ..., \widetilde{K}_{t-1}, \widetilde{K}_t$ in the industry, the initial real book value K_t can be approximated as

$$K_t = Deflator_0 \times \widetilde{K}_0 + \sum_{\tau=0}^{t} Deflator_\tau \times [\widetilde{K}_{\tau+1} - (1-\delta)\widetilde{K}_\tau], \tag{E3}$$

where δ is the depreciation rate of fixed assets.

The real net investment rate NI_s/K_s in period s then follows as

$$NI_s/K_s = \frac{Deflator_s \times \widetilde{NI}_s}{K_s} \tag{E4}$$

where $\widetilde{NI}_s = \widetilde{K}_{s+1} - (1-\delta)\widetilde{K}_s$. A simpler way to calculate the real net investment rate is to divide the real net investment by the total nominal assets so that we do not need to estimate the firm's real initial capital stock. This simplification gives similar estimation results.

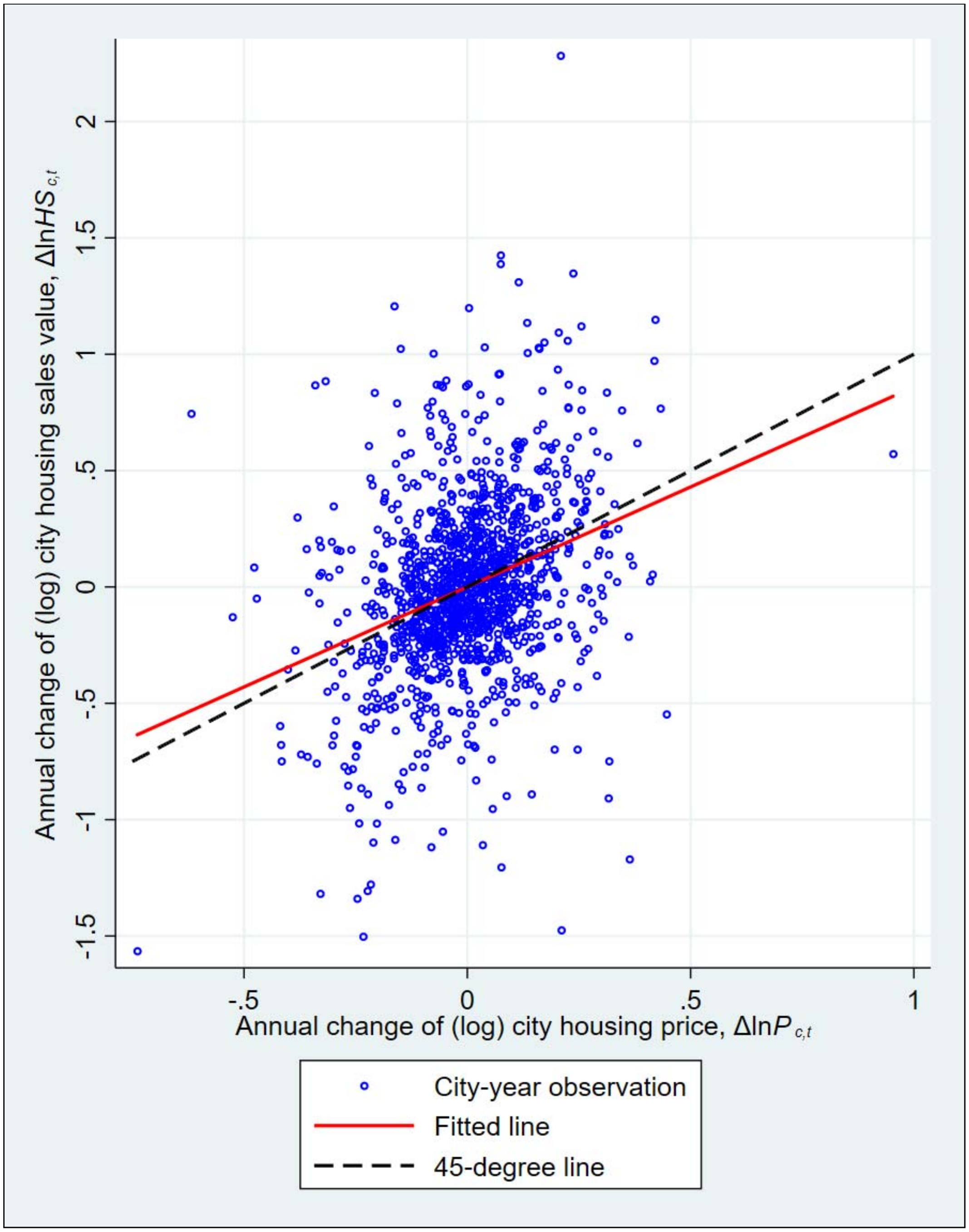

Figure 1: Relationship between the (log) change of housing prices $\widehat{P}$ and the (log) change of housing sales $\widehat{HS} = \widehat{S} + \widehat{P} = (1-\gamma)\widehat{P}$ over the period 2002–2007. The dashed black line represents the 45-degree line. The red line represents the fitted line with $\gamma \gtrsim 0$.

Figure 2: Annual changes in the (log) *Relative Land Supply* between 2002 and 2007 for 202 cities sorted into different deciles based on the rank of the initial real estate price level in 2002. The graphs for decile n include all cities with an initial real estate price in 2002 between the percentiles $10(n-1)$ and $10n$.

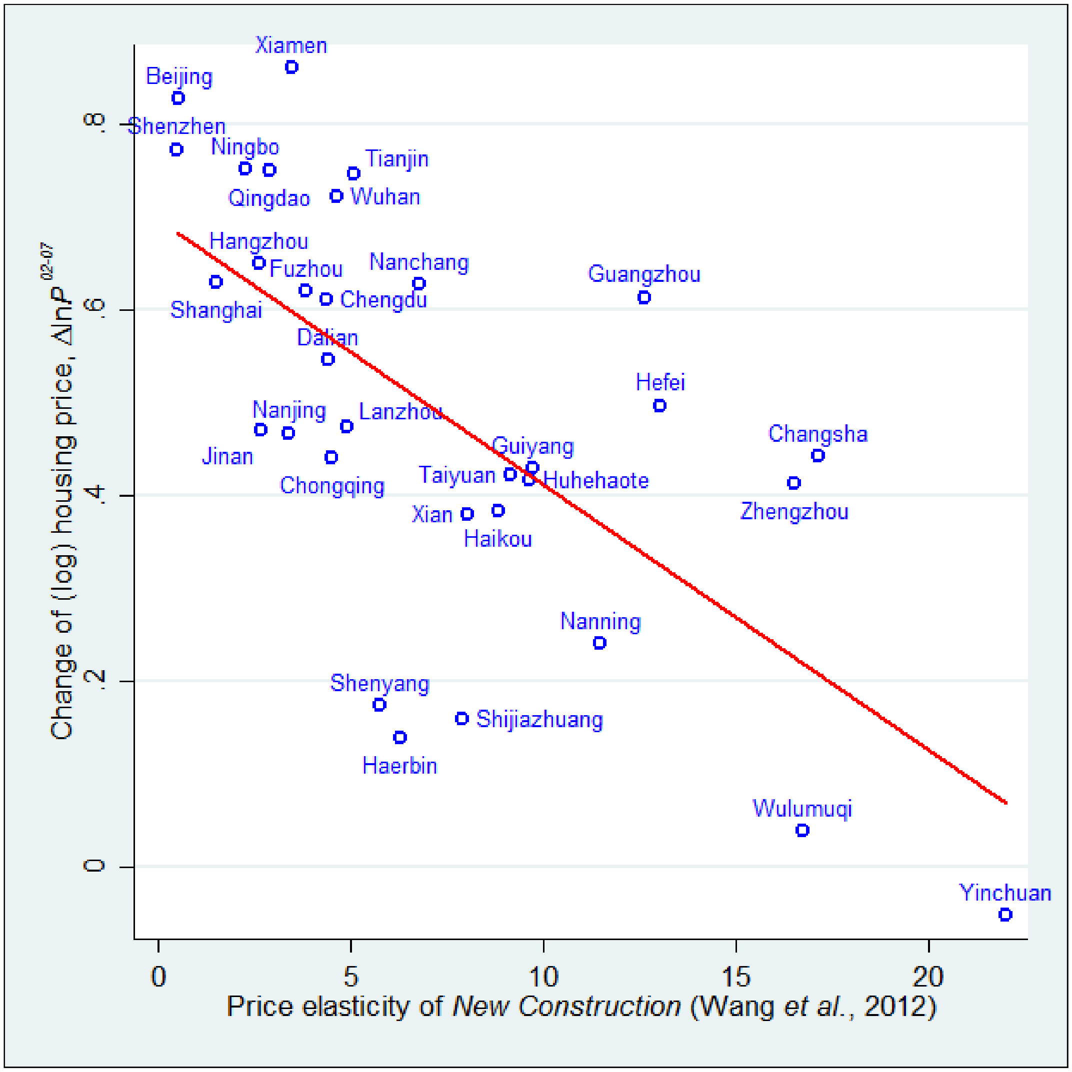

Figure 3: Relationship between the (log) change of housing prices between 2002 and 2007 and housing supply elasticity estimated by Wang *et al.* (2012).

Table A1: Persistence of the Corporate Loan Rate

We estimate an error correction model for the average corporate loan rate $i_{c,t}$ in each city relative to same rate $i_{c\_nb,t}$ for firms in neighboring cities. We use ***, **, and * to denote statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	$\Delta i_{c,t+1}$		
	(1)	(2)	(3)
$i_{c,t} - i_{c\_nb,t}$	-0.105^{***}	-0.138^{***}	-0.138^{***}
	(0.022)	(0.032)	(0.032)
City fixed effects	No	Yes	Yes
Year fixed effects	No	No	Yes
R-squared	0.047	0.133	0.203
Observations	846	846	846

Table A2: Summary Statistics of Other City-level Variables

We report the median bank loan rate of all firms in a city as $i_{c,t}$, its annual change $i_{c,t} = i_{c,t} - i_{c,t-1}$, and its difference to all firms located in neighboring cities/prefectures $i_{c,t} - i_{c\_nb,t}$. We also report city-level summary statistics for the (log) gross domestic product ($\ln GDP_{c,t}$), the (log) city population ($\ln$ $Population_{c,t}$), the annual (log) expenditure ($\ln$*Gov. Expenditure*$_{c,t}$) by the city government, its annual (log) revenue ($\ln$*Gov. Revenue*$_{c,t}$), 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*$_{c,t}$).

	Obs.	Mean	SD	Q25	Q50	Q75
	(1)	(2)	(3)	(4)	(5)	(6)
$i_{c,t}$	846	0.050	0.011	0.043	0.049	0.056
$\Delta i_{c,t}$	846	0.001	0.008	-0.002	0.001	0.005
$i_{c,t} - i_{c\_nb,t}$	846	-0.002	0.017	-0.009	-0.001	0.006
$\ln GDP_{c,t}$	1,212	15.18	0.896	14.58	15.04	15.69
$\ln Population_{c,t}$	1,212	5.895	0.650	5.531	5.939	6.363
$\ln$*Gov. Expenditure*$_{c,t}$	1,212	12.54	1.094	11.87	12.60	13.11
$\ln$*Gov. Revenue*$_{c,t}$	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$_{c,t}$	1,212	3.720	5.176	0.846	2.052	4.690

Table A3: Robustness Check for Cities with Strictly Negative Elasticity Estimates

We check the robustness of the benchmark results in Tables 3 and 4 for the subsample of firms in cities with strictly negative elasticity estimates, $\widehat{\eta}_c < 0$. The instrument is the city-specific elasticity instrument ln*Relative Land Supply*$_{c,t-1} \times \widehat{\eta}_c$ is the interaction of ln *Relative Land Supply*$_{c,t-1}$ with the (inverse) of the city-specific local housing demand elasticity $\widehat{\eta}_c$.We separate firm observations for cities with negative and postive city-level elasticity estimates in Panels A and B, respectively. 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: Firms in cities with negative city-level elasticity estimates, $\widehat{\eta}_c < 0$						
$\ln P_{c,t}$	-0.116^{**}	-0.136^{**}	-0.939^{***}	-0.963^{***}	0.020^{***}	-0.460^{***}
	(0.051)	(0.055)	(0.149)	(0.104)	(0.006)	(0.116)
Kleibergen-Paap *F-stat*	43.1	38.8	38.0	37.9	27.2	37.9
Observations	541, 758	736, 240	721, 033	721, 462	342, 083	721, 144
Panel B: Firms in cities with positive city-level elasticity estimates, $\widehat{\eta}_c > 0$						
$\ln P_{c,t}$	−0.114	−0.062	1.036	0.725	0.009	−0.084
	(0.291)	(0.163)	(0.650)	(0.584)	(0.020)	(0.278)
Kleibergen-Paap *F-stat*	13.9	15.2	15.2	15.2	9.8	15.3
Observations	199, 142	271, 704	264, 387	264, 531	116, 021	266, 213
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 A4: Firm Response by Output Tradability

As a robustness check, we study whether the negative effect of local real estate booms on firm outcomes is stronger in industries producing more tradable products. In this case the positive local consumption demand effect of real estate booms is attenuated. We proxy the $Tradability_s$ of firm output by the export share (in the year 2000) of the two-digit industry s to which firm j belongs. 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.

Dependent variables:	$I/K_{j,t}$	$Loan_{j,t}$	$\ln Y_{j,t}$	$\ln(Y/L)_{j,t}$	$i_{j.t}$	$\ln w_{j.t}$
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln P_{c,t}$	-0.119^{*}	-0.044	-0.885^{***}	-0.932^{***}	0.012^{**}	-0.382^{***}
	(0.063)	(0.039)	(0.213)	(0.156)	(0.006)	(0.125)
$\ln P_{c,t} \times Tradability_s$	-0.328^{**}	-0.186^{***}	-0.131	-0.101	0.013^{**}	-0.308^{***}
	(0.144)	(0.040)	(0.174)	(0.171)	(0.006)	(0.088)
Kleibergen-Paap *F-stat*	80.0	86.3	86.8	84.8	83.7	84.1
Observations	741,098	1,007,944	985,420	985,993	458,104	987,357
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 A5: Equal versus Asset Weighted Regressions and Listed Companies

Due to their access to the national capital/banking market, large firms (by assets) and listed companies should not be exposed to local capital scarcity associated with real estate booms and thus represent a suitable placebo group. To document heterogeneous real outcomes by firm type, we compare equal and asset weighted 2SLS regressions and also repeat the regressions in Tables 3 and 4 for the sample of listed comanies. The dependent variables are a firm's gross investment to capital share ($I/K_{j,t}$) in Columns (1a)–(1c); a dummy variable of whether firm j has a bank loan ($Loan_{j,t}$) in Columns (2a)–(2c); the the (log) value-added firm output ($\ln Y_{j,t}$) in Columns (3a)–(3c); and the (log) labor productivity ($\ln(Y/L)_{j,t}$) in Columns (4a)–(4c), bank loan rate ($i_{j,t}$) in Columns (5a)–(5c); and its log average firm wage ($\ln w_{j,t}$) in Columns (6a)–(6c). All regressions control macroeconomic variables, year and firm 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.

Sample	Equal weighted 2SLS	Assets weighted W2SLS	Listed companies 2SLS	Equal weighted 2SLS	Assets weighted W2SLS	Listed companies 2SLS
Dependent variables:		$I/K_{j,t}$			$Loan_{j,t}$	
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
$\ln P_{c,t}$	-0.211^{***}	0.056	0.514^{*}	-0.097^{**}	-0.097	0.283^{**}
	(0.052)	(0.242)	(0.280)	(0.057)	(0.084)	(0.127)
Kleibergen-Paap *F-stat*	125.4	129.9	90.8	135.1	131.1	92.5
Observations	741,098	741,083	2,489	1,007,944	1,007,924	2,824
Dependent variables:		$\ln Y_{j,t}$			$\ln(Y/L)_{j,t}$	
	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)
$\ln P_{c,t}$	-0.923^{***}	-0.452^{**}	0.069	-0.961^{***}	-0.573^{***}	-0.009
	(0.184)	(0.206)	(0.354)	(0.137)	(0.132)	(0.226)
Kleibergen-Paap *F-stat*	135.9	141.2	101.0	135.5	143.0	102.8
Observations	985,420	985,401	2,070	985,993	985,972	2,751
Dependent variables:		*Firm bank loan rate:* $i_{j.t}$			*Firm wage:* $\ln w_{j.t}$	
	(5a)	(5b)	(5c)	(6a)	(6b)	(6c)
$\ln P_{c,t}$	0.016^{**}	-0.002	-0.002	-0.470^{***}	-0.422^{***}	-0.113
	(0.007)	(0.010)	(0.011)	(0.129)	(0.102)	(0.137)
Kleibergen-Paap *F-stat*	145.2	145.3	87.9	134.8	143.7	92.7
Observations	458,104	458,104	1,958	987,357	987,341	2,750
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 A6: The Collateral Effect of Real Estate Booms on Corporate Investment

As a robustness check, we explore whether local real estate booms affect corporate investment through a collateral channel. 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. $D\_\ Collateral_j = 1$ applies to 7.4% of all firms. The dependent variables are is the gross investment rate in total fixed assets ($I/K_{j,t}$) in Columns (1)-(3), the gross investment rate in operating fixed assets ($I^P/K_{j,t}$) in Columns (4)-(5), and the gross investment rate in non-operating fixed assets ($I^{NP}/K_{j,t}$) in Columns (6)-(7). 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.

Dependent variables:	$I/K_{j,t}$			$I^P/K_{j,t}$		$I^{NP}/K_{j,t}$	
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\ln P_{c,t}$	−0.211***	−0.219***	−0.962***	−0.270**	−1.220***	0.092	0.215
	(0.052)	(0.053)	(0.205)	(0.134)	(0.322)	(0.120)	(0.201)
$\ln P_{c,t} \times D\_\ Collateral_j$		0.055	0.046		−0.005		−0.082
		(0.102)	(0.097)		(0.205)		(0.159)
$\ln P_{c,t} \times \ln\ Fixed\ Assets_j$			0.092***		0.116***		−0.014
			(0.023)		(0.026)		(0.013)
Kleibergen-Paap *F-stat*	125.4	24.5	16.1	126.6	15.2	126.2	15.2
Observations	741,098	741,098	741,098	739,075	739,075	739,343	739,343
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A7: Bank Dependent Provinces vs. Non-Bank Dependent Provinces

As a robustness check, we split the sample into firms in bank dependent provinces and firms in non-bank dependent provinces. The bank dependence is measured by the share of fixed investment financed by loans in 2000. 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.

Sample	Bank-dependent 2SLS	Non-bank-dependent 2SLS	Bank-Dependent 2SLS	Non-bank-dependent 2SLS
Dependent variables:	$I/K_{j,t}$		$Loan_{j,t}$	
	(1a)	(1b)	(2a)	(2b)
$\ln P_{c,t}$	-0.254^{***}	0.109	-0.130^{**}	0.077
	(0.062)	(0.146)	(0.056)	(0.082)
Kleibergen-Paap *F-stat*	107.4	26.6	107.7	34.1
Observations	480, 446	260, 652	648, 801	359, 143
Dependent variables:	$\ln Y_{j,t}$		$\ln(Y/L)_{j,t}$	
	(3a)	(3b)	(4a)	(4b)
$\ln P_{c,t}$	-0.832^{***}	-0.396	-0.876^{***}	-0.347^{*}
	(0.189)	(0.256)	(0.123)	(0.195)
Kleibergen-Paap *F-stat*	109.5	34.1	108.8	34.1
Observations	633, 370	352, 050	634, 937	351, 056
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes

Table A8: Firm Sorting by Output Linkage to the Infrastructure Sectors

As a robustness check, we use input-output tables for 42 different industries and devide firms by industry at the median by their industry output linkages to the sector comprising (i) the production of electricity and heat (in Panels A) or (ii) transportation and storage (in Panels B). Under the null hypothesis that the capital supply externality is exclusively relevant for the differential small firm developement between cities, such firm output linkages to infrastructure expenditure should not matter. 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.

Dependent variables:	$I/K_{j,t}$		$\ln Y_{j,t}$	
Firm sample:	Weak linkage	Strong linkage	Weak linkage	Strong linkage
	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Panel A: Sorting on firm output linkage to the production of electricity and heat				
$\ln P_{c,t}$	-0.247^{***}	-0.183^{**}	-1.000^{***}	-0.847^{***}
	(0.079)	(0.055)	(0.204)	(0.175)
Kleibergen-Paap *F-stat*	130.9	114.0	144.6	120.3
Observations	375, 336	365, 762	503, 093	482, 327
Panel B: Sorting on firm output linkages to transportation and storage				
$\ln P_{c,t}$	-0.270^{***}	-0.149^{***}	-0.990^{***}	-0.852^{***}
	(0.084)	(0.047)	(0.198)	(0.184)
Kleibergen-Paap *F-stat*	113.3	129.3	124.3	137.1
Observations	381, 065	360, 033	508, 733	476, 687
Macroeconomic controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes

Table A9: Bootstrapping Standard Errors

We check the robustness of the benchmark results in Tables 3 and 4 with bootstrapping standard errors based on 500 draws of city level observations. 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)
$\ln P_{c,t}$	-0.211^{***}	-0.097^{*}	-0.923^{***}	-0.961^{***}	0.016^{**}	-0.470^{***}
	(0.066)	(0.052)	(0.201)	(0.157)	(0.007)	(0.145)
Kleibergen-Paap *F-stat*	125.4	135.1	135.9	135.5	145.2	134.8
Observations	741,098	1,007,944	985,420	985,993	458,104	987,357
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 A10: Housing Supply Elasticity of Major Cities

This table reports the local housing supply elasticity of major cities in China provided by Wang *et al.* (2012) in the top 5 and bottom 5 (Column 1), their corresponding increase of house prices over the period 2003–2010 (Column 2), and the level of house prices in 2010 (Column 3).

City	Housing supply elasticity (1)	Housing prices growth (2003–2010) (2)	Housing price level (in 2010) (3)
Top 5			
Shenzhen	0.49	201.78%	19,169.89
Beijing	0.53	280.83%	17,782.01
Shanghai	1.52	183.01%	14,400.10
Ningbo	2.27	294.74%	11,223.62
Hangzhou	2.65	267.61%	14,133.06
Bottom 5			
Hefei	13.3	185.06%	5,904.47
Zhengzhou	16.5	143.71%	4,957.34
Wulumuqi	16.71	95.40%	4,443.26
Changsha	17.14	118.71%	4,418.11
Yinchuan	21.98	88.79%	3,928.93

Table A11: Using Housing Supply Elastcities 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