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ABSTRACT

Previous research on the United States and Japan finds economically large impacts of changing real estate collateral value on firm investment. Working with unique data on land values in 35 major Chinese markets and a panel of firms outside the real estate industry, we estimate investment equations that yield no evidence of a collateral channel effect. One reason for this stark difference appears to be that some of the most dominant firms in China are state-owned enterprises (SOEs) which are unconstrained in the sense that they do not need to rely on rising underlying property collateral values to obtain all the financing necessary to carry out their desired investment programs. However, we also find no collateral channel effect for non-SOEs when we perform our analysis on disaggregated sets of firms. Norms and regulation in the Chinese capital markets and banking sector can account for why there is no collateral channel effect operating among these firms. We caution that our results do not mean that there will be no negative fallout from a potential real estate bust on the Chinese economy. There are good reasons to believe there would be, just not through a standard collateral channel effect on firm investment.

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I. Introduction

In a world without complete contracting, economists long ago realized that pledging collateral such as owned real estate can allow firms to borrow more, and thus, to invest more (Barro (1976), Stiglitz and Weiss (1981) and Hart and Moore (1994)). Macroeconomists quickly realized the implication this insight had for amplifying the business cycle via a collateral channel effect (Bernanke and Gertler (1987); Kiyotaki and Moore (1997)). Falling asset values reduce the debt capacity of credit constrained firms, which depresses their investment on the downside of the cycle. An analogous impact occurs on the upside of the cycle when collateral values are increasing for these firms.

Empirically, recent research on the United States and Japan supports this theory and has shown that rises and declines in property values substantially amplify the volatility of investment by non-real estate firms (Chaney, et. al. (2012), Cvijanovic (2011), Gan (2007a, 2007b), and Lin, Wang and Zhu (2011)). That these effects are large economically is evident from Chaney, et. al.'s (2012) finding that a one standard deviation increase in underlying real estate collateral value is associated with over one-quarter of a standard deviation higher level of corporate investment. This implies about six cents added investment for every dollar increase in collateral value. Earlier research by Bernanke (1983) concludes that this factor helps account for the extraordinarily large variation in output during America's Great Depression.

The remarkable boom and recent cresting of China's property markets, as well as that country's growing importance in the global economy, naturally raises the question of whether its cycle is being magnified by a collateral channel effect on investment. This is an empirical question that cannot be solved by theory. As will be discussed more fully below, two conditions are necessary (but not sufficient) for a collateral channel effect to operate: (a) the existence of financially constrained firms; and (b) their ability to pledge collateral to increase debt capacity so that that investment can be ramped up when desired.

While the aforementioned research indicates these conditions exist in the United States and Japan, it is not clear this is so in China. Some of the most important Chinese firms are state-owned enterprises (SOEs), which are claimed to be politically favored and to have special access to capital (e.g., Lin and Tan (1999), Allen, Qian and Qian (2005), and Poncet, Steingress and Vandenbussche (2010)). Thus, they could be financially unconstrained in the sense that their ability to borrow to fund investment programs is not dependent upon underlying collateral value. However, non-SOEs seem much more likely to be financially constrained. We document below that Chinese firms' properties can be pledged as collateral for loans and that they do get taken back in the event of default, so that the two aforementioned conditions appear to be met for at least a subset of Chinese firms.[1]

This still leaves the possibility that other Chinese financial market regulations and practices shut down any collateral channel effect. For example, binding loan quotas could do this. To see this more clearly, consider a situation like that depicted in Figure 1, in which the government imposes a rule that aggregate loans may not exceed L1 in amount. This quota binds when property values exceed P1. In the absence of such a regulation, increases in underlying property collateral value from (say) P1 to P2 would allow the amount of debt to increase from L1 to L2 (i.e., the movement from point A to point B along the line relating property value to debt capacity in Figure 1). This is the graphical version of the collateral channel in operation: higher

[1] Even though local governments technically own all urban land as we describe more fully in the next section, the so-called "Guarantee Law" and "Property Law" provide the legal underpinning for firms to use their leasehold estate interests in land as collateral for borrowing. There is much collateralized lending in China (although not all collateral is property-based). According to the financial reports of sixteen commercial banks listed on the Shanghai or Shenzhen exchanges, at the end of 2011 their total loan balance was 38.13 trillion yuan RMB, of which collateralized loans accounted for 19.75 trillion yuan (or 51.8%).

quality collateral supports more debt, which enables greater investment by the firm. However, this effect is turned off when the quota binds. Even if property values increase from P1 to P2, debt cannot increase from L1 to L2, and firms do not move from point A to point B. Loan quotas clearly exist in China (He and Wang (2012), Allen, et. al. (2012)), but whether they bind is unknown. Plus, they did get materially raised in that country's recent stimulus period (Deng, et. al., 2011).

The only way to tell whether there is an economically meaningful collateral channel effect is with an empirical test. That this issue has not yet been studied for China probably is due to data limitations. Because of the likely endogenous relationship between aggregate investment and property prices, convincing tests rely on firm panels. At a minimum, this requires high quality data on individual companies, especially their property holdings and investment programs, in addition to property market prices themselves. An important contribution of this paper is to construct two unique data sets which can be used to estimate a potential collateral channel effect in China.

The first is a panel on constant quality land prices from 2003-2011 for the 35 major Chinese cities depicted in Figure 2. This is the first comprehensive series on land values outside the capital of Beijing. Our measure is constructed from sales of vacant land by local governments to residential property developers and builds on previous work we have done (Deng, Gyourko and Wu, 2012). We are able to control for site quality, which does change over time in important ways. As is discussed more fully in the next section, this makes our series far superior to alternatives such as those on house prices reported by the Chinese government.

The second data set constructed is a panel of 444 firms from outside the real estate industry that were continuously listed on the Shanghai and Shenzhen exchanges between 2003-

2011. Accounting variables typically used in standard collateral channel effect estimation are collected from Wind Info, which provides the equivalent of Compustat data on Chinese publicly-listed firms. We then manually collected and merged information on firms' real estate assets from their annual financial reports. Market values of their property holdings, adjusting for depreciation, were recovered via procedures described more fully below. This provides us a unique data set on listed firms' real estate asset holdings in China, which is then merged with the land price index data. The variation in land prices over time within a market is used to impute changes in underlying real estate collateral for firms located in the relevant market.

These data sources are combined to provide the first estimate of the impact of changing real estate collateral values on the investment behavior of Chinese firms outside the real estate sector. In stark contrast to the recent findings referenced above for America and Japan, we find no evidence of a collateral channel effect among non-real estate firms in China. This conclusion is robust to a wide range of permutations. For example, there is no evidence of asymmetry in the collateral channel effect depending upon whether property markets are rising or falling. We also do not see heterogeneity in impact by measures of likely financial constraint. For example, there is no difference in our baseline results depending upon whether the firm is a low versus high dividend payer. Nor is there any meaningful effect (or difference in impact) depending upon whether or not the firm is a state-owned enterprise (SOE). We can also rule out the possibility that our results might be driven by financially constrained firms tending to be located in markets without good investment opportunities (so that they rationally would not want to invest even if collateral value increased substantially to lessen the degree to which they are constrained). Actual growth rates of local GDP were high in absolute terms among the slowest growing of our 35 markets during our sample period, so it seems likely that there are profitable investment opportunities even in those places. And, there is no evidence of a larger (or non-zero) collateral channel effect among firms headquartered in the markets with the strongest growth trends.

Naturally, this important difference in impacts between China and the U.S. (or Japan) requires explanation, and we investigate a number of possibilities. First, we rule out specification bias from a defect in our empirical strategy. As is discussed more fully in Section III, the typical concern when running reduced form neoclassical investment equations of the type done by us and others in this literature is that the estimates are biased up, not down, due to endogeneity. Hence, it is unlikely that our results are being driven by standard specification error concerns about this type of work. Data quality issues involving measurement error also seem highly unlikely to be able to account for our findings.

We argue that what can do so are specific features of Chinese firms and the capital markets in which they operate. For example, state-owned enterprises, which tend to be the largest firms in China, represent about three quarters of our sample of publicly-traded firms. Our findings are consistent with the claims by others noted above that these firms are financially unconstrained and do not need to pledge their underlying real estate holdings as collateral to secure all the funding needed to finance their desired investment programs. However, we also find no collateral channel impact on investment for purely private firms who are not SOEs. These private firms are highly likely to be financially constrained, so that collateral value should be a relevant factor for the financing of their investment programs. Regulation in the Chinese lending market can account for the absence of a collateral channel impact among this group of firms. The presence of binding loan quotas imposed by the central government is one such example.

While this could account for the absence of a collateral channel effect as long as the quota is binding, it also suggests that we are more likely to observe a collateral channel effect for financially constrained private firms when loan quotas have been lifted and no longer bind. The 2009-2010 stimulus period following the global financial crisis is likely to have been just such a time, but we find no evidence of a collateral channel effect during those years either. Other norms in the Chinese lending market can account for the absence of an impact in that case. Even in the absence of a binding quota, purely private firms find it very difficult, if not impossible, to renegotiate loans no matter how underlying collateral quality might have improved. One piece of evidence consistent with this is the nearly complete absence of loan prepayments by corporations in China. If existing loan terms could be restructured, one would expect to see prepayments and new loans taken out when collateral quality improves. That we do not see such behavior among the Chinese corporations most likely to be financially constrained appears due to financial and banking sector regulation that prohibits or is very cautious about permitting firms to leverage increased asset values. While purely private firms did not invest more during the recent stimulus period, we do find evidence they borrowed more and hired a few more employees, lengthened the maturity of their debt, and built cash reserves.

This still leaves the question of whether these results are representative of the nation as a whole. Our sample of publicly-listed firms includes the very large manufacturing sector which is responsible for nearly half of aggregate national investment outside the real estate sector in recent years.[2] Hence, what happens in this sector is likely to be fairly representative of the country as a whole. However, listed firms are not as numerous or as representative of the

[2] There was about 10.3 trillion yuan of investment undertaken with the manufacturing sector in 2011. Given total aggregate investment that year of 31.1 trillion yuan, with 8.2 trillion yuan within the real estate sector, the manufacturing sector was responsible for 45% of all non-real estate sector investment that year (10.3/22.9~0.45). All figures are from the National Bureau of Statistics of China.

corporate universe in China as they are in the United States and Japan which have more developed equity markets.[3] Data limitations prevent us from replicating our analysis on non-traded firms.[4] That said, there is no reason to suspect the results would be different for non-traded firms. The SOEs among that group are no more likely to be financially constrained than those that are listed. And, the regulations and rules that turn off the collateral channel effect for publicly-traded private firms are even more likely to bind for those that are not listed.[5]

Our empirical work concludes by analyzing whether there is any evidence of a collateral channel effect on investment among local SOEs involved in providing infrastructure. While this sector is not nearly as large as manufacturing,[6] if there is an economically meaningful impact to be found, it is likely to be here for the following reason. Local governments in China that want infrastructure created typically capitalize a locally-controlled state-owned enterprise with land grants. That land serves as collateral for loans from banks which provide the financial resources needed to build the infrastructure. Thus, this sector contains a very special type of SOE in which the collateral channel is most likely to operate precisely because the 'equity' component of its capital structure is made up of local land.

[3] A recent report by the consulting firm Grant Thornton reports over 5,091 companies listed on major American stock exchanges as of early 2011. [These numbers include foreign firms listed in the U.S., and thus are not all domestic in nature.] This compares with little more than 1,000 such firms listed on the Shanghai and Shenzhen exchanges in China. The Central Intelligence Agency's *World Factbook* reports that U.S. publicly traded companies had a market value of $17.1 trillion at the end of 2011, which was over three times the $4.8 trillion for China. These figures are in U.S. dollars and the relevant table can be accessed at https://www.cia.gov/library/publications/the-world-factbook/rankorder/2200rank.html. On a per dollar of output basis, the difference is larger, as U.S. GDP is estimated by the same source to have been only 1.34 larger than China's on a purchasing power parity basis at the same time (the relevant data may be accessed: https://www.cia.gov/library/publications/the-world-factbook/rankorder/2001rank.html).

[4] There are lists of such firms and some balance sheet information on them (which is completely unaudited, and therefore of very uncertain quality), but it does not include data on their property holdings (whether buildings or land). Hence, there is no way for us to investigate a relationship between real estate collateral value and investment for this group.

[5] For example, a World Bank survey on the financing patterns of over 1,300 Chinese firms in 2003 suggested that on average the non-listed privately held firms found it more difficult to access commercial bank loans compared to their publicly-listed counterparts (Ayyagari, Demirguc-Kunt and Maksimovic, 2010).

[6] According to the statistics published by the Ministry of Housing and Urban-Rural Development, in 2011 the total investment on urban infrastructure was 1.4 trillion yuan RMB, or about 18% of the investment undertaken by the manufacturing sector.

Unfortunately, there is no individual firm-level information available for this sector, so our empirical strategy using listed firms cannot be replicated. However, we do observe the aggregate amount of investment across local SOEs in the infrastructure provision business plus whatever the local government itself directly provides. This sum is a city-level measure of such investment. We then use a city-level measure of land available for sale by the city to private developers at the beginning of our sample period and apply our land price index series to measure the changing value of each city's real estate collateral value. We find a consistently positive correlation between changing local real estate collateral value and investment in local infrastructure (transportation infrastructure especially), but the correlation is not always statistically significant at conventional levels. The most optimistic case for a large collateral channel case in this sector is that a one standard deviation increase in local land values is associated with about one-fifth of a standard deviation greater transportation infrastructure investment. However, this estimate should be interpreted with caution, as it is likely to be biased upward because it is from a specification clearly subject to endogeneity concerns.

Before getting to the results, the next section describes the unique real estate and firm data we bring to bear in our estimation of the collateral channel effect. Section III then discusses our estimation strategy and reports results. There is a brief conclusion.

II. Data on Land Values and Listed Firms

We bring two new data sources to bear on the question of whether there is a collateral channel effect on Chinese firm investment. Both are unique to the study of the Chinese economy. The first is a panel on land prices across 35 Chinese cities, and the second is a panel on firms not directly involved in the real estate industry.

II.A. Land Value Data

Our land price series is based on sales of raw land by local governments, and is described more fully in Deng, Gyourko and Wu (2012). While raw land sales are rarely observed in most countries, this is not the case in China. Local governments own all the urban land in the country and allow private parties to purchase use rights of up to 70 years for residential purposes (i.e., technically, this is a leasehold estate).[7] We treat the upfront lump sum payment as the transactions price for land because there are no further rental payments required.

Our data series begins in 2003 because of an important 2002 ruling by the Ministry of Land and Resources that required local governments to sell land via some type of public auction process and to publicly report the winning bidder along with the transactions price.[8] Because of this new process, these prices can reasonably be treated as free market values. We also typically observe the land parcel's precise address, designated usage, land conditions upon delivery, and certain planning indicators such as the floor-to-area ratio.

Building upon prior research on the city of Beijing in Wu, Gyourko and Deng (2012), we worked with a leading residential real estate data vendor in China (Soufun) to collect data on all residential usage land sales to private parties from 2003-2011 in the 35 major markets mapped above in Figure 2. The geographic breadth of our sample is noteworthy. We are not limited to a

[7] Not only does Chinese law facilitate the use of such leasehold estates in urban areas as collateral for borrowing, but the data confirm that they can and will be transferred to the lender if the borrower defaults. For example, 14 of the 16 commercial banks listed on the Shanghai or Shenzhen exchanges regularly report the value and breakdown of repossessed assets seized because of defaulted loans. At the end of 2011, the total book value of their repossessed assets was 10.79 billion yuan RMB, of which the leasehold estates associated with properties accounted for 8.79 billion yuan RMB (or 81.4%). The remainder was comprised of plant and equipment, securities, etc.

[8] Prior to this ruling called the 11th Provision, most transactions of urban land parcels were done by negotiation between a developer and a local government. This process was criticized for being opaque and open to corruption. For our purposes, the prices that resulted seem likely to be below free market levels, with the degree unknown and possibly changing over time depending upon local circumstances. Currently, all transactions must be done via public auctions, including regular English auctions (*pai mai)*, two-stage auctions (*gua pai)*, and sealed bids (*zhao biao*). See Cai, Henderson and Zhang (2009) for a comparison of these three types of auctions.

few coastal-region markets that allegedly had the biggest booms. Table 1 reports summary statistics on the sample. We have complete data dating back to 2003 for 15 markets, with the rest entering the sample in subsequent years. The number of transactions per market ranges from 25-50 depending upon the year.

Land parcels in China are priced in terms of the floor area of housing permitted to be built on the parcel, instead of in terms of the land area. Real prices in constant 2009 yuan per square meter of permitted space are computed by deflating with the relevant monthly CPI series for each city. Figure 3's plot of the simple means of these real values over time for each market illustrates the substantial heterogeneity in real price levels across markets. That said, the growing dispersion apparent in this figure is somewhat misleading, as it is partially due to the inclusion of added markets over time. If we restrict the analysis to the 15 markets with complete data throughout our sample period, land was nearly eight times more costly in the most expensive market in 2003 (2,693 yuan per square meter of floor area in Hangzhou) than in the least expensive city that year (352 yuan in Chongqing). By 2011, the gap was just over five times (i.e., with Shanghai at 5,470 yuan and Nanning at 997 yuan). This is not a small absolute difference by any means, but still pales in comparison to the nearly 25 times gap between Shanghai and Wulumuqi (222 yuan) in 2011 for the full sample.

We do not work with these unadjusted transactions prices because the raw means may be driven by quality changes over time. For example, a city government might decide to sell the best locations first, in which case the change in the unadjusted mean values would understate the true constant quality rate of land price appreciation. On the other hand, local governments might reserve the good parcels and only list them during the more recent booming periods of the stimulus years, which would result in an overestimation of price growth in the simple average series. In addition, some land parcels in a few cities were not leveled on delivery in the early years of our sample. Not controlling for that would result in the overestimation of true, constant quality price growth.[9] It also is possible that sales of high quality parcels occur whenever the local government has the greatest need for revenue.[10] Thus, the bias could go in either direction for different markets. Consequently, we follow Wu, Gyourko and Deng (2012) in creating constant quality land price indexes for each market.

Our city-level hedonic is estimated via ordinary least squares (OLS), with the log of the real transaction price in constant 2009 yuan as the dependent variable.[11] Quality controls on the right-hand side include: (a) the parcel's distance to the center of the corresponding city, which is measured after mapping the precise location of each site with GIS software; (b) the distance to the nearest subway station; this variable is relevant in 10 of the 35 cities with operating subway systems during our sample period; (c) district dummies which control for local/neighborhood-level fixed effects not captured by the two previous location controls[12]; (d) a set of physical attributes including the size of the parcel (in land area), the density permitted on the site when built, and whether the parcel is leveled on delivery; (e) in some cases, a small portion of a residential land parcel is designated for affiliated commercial properties, public establishments, or public housing units; we control for such conditions via a set of dummies; (f) the parcel's

[9] If a land parcel is not leveled on delivery, the purchaser has to pay additional costs for relocating previous occupiers of the parcel, removing the existing buildings, and so forth, which would negatively affect the transaction price of the parcel. Before 2003, whether a parcel was leveled upon delivery was a key part of the negotiation between the developer and the local government. After that, most land parcels sold via public auctions are leveled on delivery, although there were a few exceptions in some cities, especially during the early years. We directly control for this in the hedonic estimation as described below.

[10] Our 2012 paper reports evidence consistent with this hypothesis.

[11] We also conducted a two-stage Heckman estimation to control for potential bias arising from the fact that there were a total of 614 parcels listed that failed to result in transactions (either because there were no bidders if there was an auction or the bid prices were lower than the local governments' reserve prices, which is relevant for cases involving sealed bids). If these failures are disproportionately concentrated in certain periods such as the financial crisis, selection bias would result in an overestimation of the price index for that period. That said, we could not find any statistically significant impact for the inverse Mills ratio estimated from our first-stage probit model.

[12] These controls serve much the same purpose as grid dummies do in the creation of recent land price series using U.S. data. See Nichols, et. al. (2013) for a recent example.

transaction form as reflected in whether it was purchased via sealed bidding, regular English auction, or two-stage auction; and (g) year dummies, whose coefficients are used to create the constant quality price index.[13]

Figure 4 plots real constant quality land price appreciation rates based on the results of our hedonic estimation. There clearly was a boom in land prices in most cities in China, although it is incorrect to claim that there is a single national land market, as there is substantial variation in land price appreciation across and within cities over time. Many cities, not just the big east coast region markets of Beijing, Shanghai and Shenzhen, have experienced considerable booms and busts in land values over time. There are only two in which constant quality land price growth has been appreciating at an average per annum compound rate below 10% (Nanchang and Qingdao, at 7.8% and 5.9%, respectively). Nine have experienced average compound annual growth rates above 20%, with the rest of our sample cities in between. Naturally, this implies large increases in real estate values, as a 10% compound annual average rate of price appreciation implies more than a doubling in real value between 2003 and 2011 (i.e., 1.1^8=2.14); analogously, a 20% compound annual rate implies that prices were over three times greater at the end of 2011 than at the beginning in 2003 (i.e., 1.2^8=4.30).

Table 2, which is taken from Deng, Gyourko and Wu (2012), reports summary statistics on average annual land price growth over time. Table 3, also from Deng, Gyourko and Wu (2012), highlights that land values are much more volatile than house prices (and other factors of production in housing) as theory predicts for the residual claimant on property value. Standard

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[13] Our land price hedonic works well in each of the 35 cities. The city-level coefficients are almost always consistent with expectations, and the null that there is no explanatory power for our right-hand side variables always is easily rejected. See Deng, Gyourko and Wu (2012) for more on the hedonic model. All underlying results are available upon request.

deviations in land prices typically are in the 20%-40% range, which is about four times that of any other variable reported in Table 3.

While there clearly is substantial volatility in land values over time across markets, there is no doubt that constant quality land prices are higher now in every market than they were in 2003. Hence, if Chinese firms are financially constrained and collateral value is important in obtaining the desired amount of debt, there has been a huge increase in those values over time on average, with economically large variation across markets. It is the cross sectional and time series variation in these constant quality land price series which we use to impute the change in firms' real estate collateral value.[14]

We believe these data are far superior to any alternative, the most prominent of which are two government-provided series on house prices. One is called the Average Selling Price of Newly-Built Residential Units. This reports the simple average of transactions prices as measured by total sales values divided by the total amount of housing square footage permitted on the land. This series makes no attempt to control for quality differences across markets or drift over time. As our results and those of Wu, Deng and Liu (2011) show, not effectively controlling for quality leads to substantial biases.[15] The other government-provided house price series, officially termed "Price Indices in 70 Large and Medium-Sized Cities", is a measure of the change in average prices on unit sales within individual housing complexes over time. More specifically, this index is calculated by first computing the average sales price of new units each

[14] We use residential land prices because it is not yet feasible to create an analogous index for commercial properties. Theory suggests these two series should be highly correlated, since land is substitutable between these two uses on the margin. As a robustness check, we calculated the correlation coefficient between the average transaction prices of newly-built residential buildings and commercial properties as reported by the National Bureau of Statistics of China in our 35 city sample. It was a strongly positive 0.64. Chaney, et. al. (2012) report similar findings in their robustness checks using U.S. data.

[15] Average annual appreciation in our hedonic index is about five percentage points higher than in the unadjusted price series, which suggests that parcel quality has been falling over time on average. The declining quality of location with more sites being located in outlying areas as China has rapidly urbanized is an important factor, but this does vary by time and market. See Deng, Gyourko and Wu (2012) for more on this.

month in each distinct housing complex. The series reported by the government then is the transactions-volume weighted average of each complex's average price changes over time. As Deng, Gyourko and Wu (2012) show, this series has very little variation over time in most markets, including the big coastal region cities that are thought to have had the biggest booms. That makes this data source highly suspect on its face, and Wu, Deng and Liu (2011) explain why it produces downwardly biased estimates of price growth, with much lower price volatility than exists in reality.

Beyond having a better empirical measure of price change in the property market that can control for quality drift in parcel sites, it also makes good sense to measure price changes of land in the first place. As long as structure is in anything approaching elastic supply, it is land value which is the residual claimant on overall property value and which will change in response to shocks to fundamentals. Land prices are not directly measured in most other countries because of data limitations—namely, land parcel sales independent of the building (or property improvements) situated on it are rarely observed.[16] For purely historical reasons associated with the Chinese Communist Party's victory in its civil war, local governments literally own all the surrounding urban land and sell it (via a long-term leasehold estate) to private developers before structures are built. Hence, we routinely observe vacant land parcel sales in China, although data quality prior to 2003 is highly suspect for reasons discussed above.

II.B. Firm Data

We next collected data on publicly-traded firms listed on the Shanghai or Shenzhen stock exchanges. There are 1,291 firms listed during or before 2003 on these two exchanges. We get

[16] This is beginning to change in the U.S. as land transactions data bases become available to academics. See Nichols, Oliner and Mulhall (2013), Sirmans and Slade (2012) and Haughwout, Orr and Bedoll (2008) for examples.

to our final sample size of 444 firms as follows. First, we exclude firms delisted during or before 2011. We also drop those with measured negative equity and those involved in a major takeover operation during the sample period, as we suspect either data error or some other aspect of firm strategy is likely to complicate any potential relationship between real estate collateral value and investment and other spending behavior. Next, firms operating in the industries of “finance”, “real estate”, and “construction” are dropped because it seems likely that the relationship between firm investment behavior and real estate price fluctuations may be determined by a mechanism other than the collateral channel effect in these business sectors. Firms in the industries of “agriculture”, “mining”, “production and supply of electricity, gas and water” and “transportation and storage” also are excluded because they tend to own properties outside of urban areas, and we cannot impute property value price changes outside of the 35 major markets listed above. Thus, our final sample is restricted to firms in the industries of “manufacturing”, “information technology”, “wholesale and retail”, “social service”, and “media and culture”.[17] We also restrict our focus to firms with headquarters in 35 major cities for which we have a reliable land price series that is used to impute the value of real estate collateral over time. This leaves us with a balanced panel of 444 firms with 3,996 firm-year observations during 2003-2011.

Determining the market value of these firms’ real estate asset holdings obviously is a critical task for our estimation. The nature of Chinese accounting and reporting practices is such that three major categories of assets on the balance sheet are involved in constructing our measure. One is “Buildings” (a sub-entry of “Fixed Assets”, the equivalent of “Property, Plant and Equipment” in China’s accounting codes); a second is “Land Ownership” (a sub-entry of

[17] According to the official industry codes issued by the China Security Regulatory Commission, there are also a few firms defined as in “Multiple Industries”. These companies are grouped based on their largest sales sector.

"Intangible Assets"); and the third is "Investment Properties". Table 4 provides more detail on related accounting codes, including a minor adjustment in 2007. Unfortunately, none of the available listed firm electronic databases in China presently provides full information on all three categories of property assets. Consequently, we manually collected this information from the original version of the firms' annual financial reports.

While we believe this is the first systematic collection of non-real estate firms' property holdings, this is only the starting point for our analysis. As with the Compustat data on U.S. corporations, Chinese company financial reports include values based on historic cost, not current market values. We follow the procedures adopted by Chaney, et. al. (2012) to translate these book values into market values. From the financial reports, we know both the original book value and the current book value after depreciation. Then, following certain rules on depreciation, the average age of properties can be computed.[18] Finally, the book value is updated to the market value using the city-level residential land price index described above after 2003, a newly-built house price index between 2000 and 2002[19], and the city-level CPI index before 2000. Because we do not know the exact address of each property in a firm's portfolio, we follow Chaney, et. al. (2012) and Cvijanovic (2011) in presuming that a firm's properties are concentrated in the city of its headquarters.[20]

[18] We use the following strategy to guide us in these calculations. If a firm explicitly describes its depreciation method in the appendix of its financial reports, we adopt that specific rule for that firm. Otherwise, we follow conventional rules on depreciation for China, which reflect an average of the different rules published by the listed firms in our sample: for the items of "Building" and "Investment Properties", it is assumed that the properties are straight-line depreciated with 25 years of depreciable life and 5% remains; for "Land Ownership", the corresponding assumption is straight-line depreciation method, 40 years of depreciable life, and 0 remains. Western readers may be surprised by the depreciation of land, but it does make sense because this technically is a leasehold estate position.

[19] This house price index is provided by Institute of Real Estate Studies at Tsinghua University. It is a hedonic series that is not subject to the biases discussed earlier regarding the official government series.

[20] Both Chaney, et. al. (2012) and Cvijanovic (2011) investigate the robustness of this assumption using added information from firm 10-K filings. Unfortunately, similar documents and data are not available in China. We addressed the robustness of this assumption as follows. First, we pared down the sample to firms headquartered in the 32 cities outside of the three national financial centers of Beijing, Shanghai and Shenzhen on the presumption that firms located in the other 32 cities are less likely to be geographically dispersed in their business and, hence, in

We next develop an estimate of annual change in the value of firm real estate asset holdings. Our preferred measure is one that reflects changes in the market value over time of real estate assets owned by the firm in the reference year of 2002 at the very beginning of our sample period. Chaney, et. al. (2012) and Cvijanovic (2011) both do something similar to guard against bias arising from the potentially endogenous decisions of firms to alter real estate holdings in response to (or in conjunction with) market price changes. Thus, our collateral value measure is defined as:

$$RATIO\_REV1_{i,t} = [REV_{i,2002} \times \prod_{j=2003}^{t-1} (1 + LPG_{c,j}) \times LPG_{c,t}]/ASSET_{i,t-1}$$

where $REV_{i,2002}$ is the market value of real estate assets owned by firm *i* at the end of 2002 computed based on the procedures described above, $LPG_{c,j}$ is the annual growth rate in the local land price index for firm *i*'s headquarters city *c* in year *j*, and $ASSET_{i,t-1}$ is the total assets of firm *i* at the beginning of year *t* (i.e., at the end of the previous year).

As part of our robustness checks described more fully below, we also used a second proxy, which measures the market value change in real estate assets held by the firm at the beginning of each year:

$$RATIO\_REV2_{i,t} = [REV_{i,t-1} \times LPG_{c,t}]/ASSET_{i,t-1}$$

where $REV_{i,t-1}$ is the market value of real estate assets owned by firm *i* at the beginning of year *t* (i.e., at the end of the previous year). We experiment with both measures because it is not obvious *a priori* what the optimal balance is between potential endogeneity bias and measurement error.

their real estate asset holdings. All our key results reported below still hold in this "geographically concentrated" group.

In addition to our measures of changes in underlying real estate collateral, we also use a number of variables describing other firm characteristics. These are from Wind Info, which is a supplier of 'Compustat-type' data on Chinese companies.[21] These include the ratio of net investment on fixed assets (property, plant and equipment) to firm asset value (*RATIO_INV*), where the numerator is defined as expenditures on fixed assets less cash inflows from disposing of existing fixed assets over the year and the denominator reflects total assets at the beginning of the relevant year (*ASSET*), *RATIO_LOAN* which is defined as the net change in loan balances scaled by firm assets, *RATIO_EBITDA*, which reflects earnings before interest tax, depreciation and amortization (again scaled by firm assets), *RATIO_CASH* which reflects the net change in cash holdings divided by assets, *RATIO_SALARY* which is defined as total salary payments divided by firm size, *RATIO_DIVIDEND*, which is total dividend payments scaled by firm assets, *RATIO_FAINV* which is the ratio of net investment on financial assets such as stocks and bonds to firm asset value, and *EMP*, which is the number of employees per million yuan of firm assets. Other variables include the market-to-book ratio (*MBR*) and leverage level at the beginning of the year (*LEVERAGE*, defined as total debt on the balance sheet divided by asset value), and the change in share of long-term loans (i.e., loans with a term exceeding one year) in total loan balance during the year (*LONGLOAN*).

We also collected information on the total amount of government subsidies received by a firm during each year. This also comes from Wind Info which includes this as a sub-entry of "non-operating income" in each firm's income statement. This includes both explicit monetary subsidies and implicit subsidies from discounted tax rates. As with the other variables, this also is normalized by the total assets of the firm (*RATIO_SUBSIDY*). It serves as a proxy of government support in some of the robustness checks discussed below.

[21] See their website at www.wind.com.cn for more details.

Table 5 reports the summary statistics on the variables, with each having been winsorized at the 5th percentile to eliminate extreme outliers in the data series.[22] One noteworthy feature is the large magnitude of the annual market value change of the listed firms' real estate assets. On average, it is equivalent to about 6% of a firm's total assets if we only take properties owned in the reference year into account, and to about 7.5% if all real estate assets are included.[23]

Table 6 then reports the number of firms in our sample broken down by whether or not they are state-owned enterprises (SOEs). This firm characteristic also comes from the Wind Info data source.[24] SOEs account for about three quarters of these 444 firms, although that proportion declines over time due. Table 7 then compares the values of these variables across the two types of firms. SOEs and non-SOEs differ in several aspects. SOEs tend to be much larger than non-SOEs. They also pay out more in salaries to their employees, but they are less profitable, pay fewer dividends, and have lower market-to-book ratios. However, both these groups experience almost the same degree of change in the market values of their real estate assets during the sample period. And, the difference in their fixed assets investment or borrowing characteristics also is statistically insignificant.

III. Empirical Implementation and Results

Collateral channel effects typically are estimated with reduced form investment

[22] We also experimented with winsorizing the top and bottom 1% of values, as well as not dropping any outliers at all. The results were not materially affected, so we can be sure our conclusions are not driven by the presence of a few outliers.

[23] The fact that the average value of *RATIO_REV2* is larger than *RATIO_REV1* implies that the listed firms generally are expanding their real estate holdings over our sample period.

[24] More detailed information is available, as we can tell whether a SOE is directly controlled by the central government or by some type of sub-national government. See Deng, et. al. (2011) for more details on the distinction between these two groups. Because all our key conclusions are robust to this breakdown, we only report results for all SOEs versus non-SOEs.

specifications as given by equation (1) just below. The underlying model and assumptions from which they derive are derived and well described in the on-line theory appendix to Chaney, et. al. (2012).[25] That model leads those authors (and others, including us, working in this literature) to estimate investment equations of the following type:

$$(1)\ RATIO\_INV_{i,c,t} = \alpha + \beta * RATIO\_REV_{i,t} + \gamma * LP_{c,t} + \delta_t + \eta_i + \varphi * OtherControls_{i,t} + \epsilon_{i,t}\ ,$$

where *i* indexes the firms, *c* denotes the city of their headquarters, and *t* reflects the year of the observation. *RATIO_INV* and *RATIO_REV* are as defined above, *LP* is the land price index in the city where the firm is located, δ and η capture year and firm fixed effects, respectively, and *OtherControls* includes standard firm measures of leverage (total debt/asset value), firm value (market-to-book value) and cash flow (EBITDA more specifically) used in these types of regressions. The investment and collateral value measures are scaled to control for firm size differences.[26] Time and firm fixed effects are included so that identification effectively comes from variation over time within firm. One of the two real estate variables is the log of the land price index (*LP*) in the city where the firm is headquartered. This variable is intended to control for broader real estate market changes that could influence investment behavior independent of the value of an individual firm's collateral.

The coefficient of interest in this regression is β which captures how changes in the value of a firm's real estate collateral (*RATIO_REV*) affect investment (*RATIO_INV*). Theory does not sign this coefficient. Controlling for firm value (which is done via the market-to-book variable

[25] It is entitled "A Simple Model of Real Estate Prices and Investment" and is available at www.princeton.edu/dsraer/theoryRE.pdf for this material.

[26] Note that we use asset value in the denominator rather than the more typical measure in the literature of overall property, plant and equipment (which are called "fixed assets" in China). This is due to the nature of the available Chinese balance sheet data. As depicted earlier in Table 4, part of a firm's real estate holdings are not included in the item "Fixed Assets" on its balance sheet. In particular, the 2007 adjustment of accounting codes separated the leased-out properties from "Fixed Assets", and put them as part of a new, independent item called "Investment Properties" on the balance sheet. This makes the fixed assets series inconsistent over our sample period. Hence, we scale by total assets. The 2007 adjustment did not apply to the cash flows, so it does not affect our measure of *RATIO_INV*.

discussed above), investment and collateral value are negatively correlated for unconstrained firms and positively correlated for constrained firms.[27] Overall, the estimate of β using a panel of firms reflects the combination of how many of them are financially constrained, how binding those constraints are, and how easy it is to pledge the underlying collateral to increase debt capacity.

While it is relatively straightforward to generate a specification such as equation (1) from a model of investment with financial constraints, it is more challenging to obtain unbiased estimates of β. The typical worry is that OLS yields upwardly biased results on the collateral channel effect. The productivity of a firm is inherently unobservable, and if it is correlated with initial collateral value, the estimate of β will be biased. Reverse causality is perhaps the most obvious problem if property prices and productivity are correlated. Consider the case of a firm that is large enough in its market for its own investment program to affect factor prices, including local land values. Similar effects could occur in markets in which multiple firms from the same industry co-locate. In that case, common shifts in investment patterns not actually driven by collateral value could be captured in the estimate of β from equation (1) if the firms' investment behavior bids up local land values. Upwardly biased estimates of β also would result if large land-holding firms are especially sensitive to local demand shocks (for whatever reason) and our real estate variables proxy for local demand to any significant extent (which seems likely).

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[27] The reasons, which are discussed more fully in the proof of Proposition 1.2 in the on-line theory appendix to Chaney, et. al. (2012) referenced above, are as follows. If two unconstrained firms have identical market values, but the first has higher collateral value, then it must also have lower productivity and investment than the second firm because the greater collateral value raises liquidation value. Thus, productivity and investment are lower in the first firm to compensate. Next, consider a completely constrained firm. Its investment is independent of its productivity because it is determined by a binding budget constraint set by collateral value (by assumption). However, this constrained firm's productivity must be lower to hold firm value constant, even though this does not affect its investment program. Hence, there still is a positive correlation between such value and investment for this type of firm, even when firm value is controlled for in the regression.

The recent literature on collateral channel effects on U.S. corporate investment exploits differences in local market supply elasticity to deal with this bias (Chaney, et. al. (2012), Cvijanovic (2011)). The basic strategy is to instrument for real estate values using the interaction of a demand shifter (e.g., mortgage rates) with the local supply elasticity, along with city and time fixed effects. The underlying logic is as follows. Demand shifters should show up in higher prices the more inelastic is local supply. If supply were perfectly elastic, prices should not change at all. The measure of supply elasticity used (typically from Saiz (2010)) is based on fixed geographic factors such as the amount of water and the slopes of land plots in the market area, so it provides plausibly exogenous variation in real estate values due to changes in demand. As noted in the Introduction, that type of instrumental variables estimator yields economically large collateral channel effects on investment among U.S. corporations.

We cannot implement that strategy here due to the absence of an appropriate instrument.[28] Hence, we estimate a version of (1) that does not instrument for real estate value, keeping in mind that the reported estimate of β is likely to be biased upward. That said, if we do not find a statistically and economically large collateral channel effect, we can be reasonably sure that it does not exist, as the biases from such a specification work in the direction of finding one.

Table 8 reports our baseline findings. The precise specification estimated is very similar to equation (1), except that it includes interaction terms of the initial firm controls with local land

[28] Wang, Chan and Xu (2012) do provide a recent estimate of housing supply elasticity for a select group of major Chinese cities. However, our experimentation with their data showed that it cannot explain much of the variance in our land price index growth (or that of the official government house price metrics). Not surprisingly, trying to use it to help instrument for collateral value growth as in Chaney, et. al. (2012) and Cvijanovic (2011) does not generate results significantly different from those reported here using OLS. Given that it does not appear to be a strong instrument, we prefer to report the simpler OLS results.

prices.[29] The first column reports estimates using the full sample of firms, regardless of type. The estimate of β on our measure of the real estate collateral variable, *RATIO_REV1*, is a very small and statistically insignificant 0.0045. Thus, there is no evidence of a collateral channel effect in the Chinese data, as firm investment behavior is uncorrelated with changes in the value of the real estate they own.

This average could be masking important heterogeneity across types of firms, as state-owned enterprises, which constitute the bulk of our firm sample well could be unconstrained. If so, they would not be expected to exhibit any collateral channel effect on their investment behavior. Hence, columns 2 and 3 of Table 8 report estimates of the same specification on subsamples of SOEs and non-SOEs. The results are not meaningfully different, and we cannot reliably discriminate between the coefficients across the two types of firms.

This conclusion about the absence of a collateral channel effect is robust to many alternative specifications investigated. For example, it could be that our desire to minimize upward bias due to endogeneity by measuring real estate exposure with the quantity of firm holdings at the end of 2002 ends up generating attenuation bias in our estimate of β because of measurement error. That this is not the case is evident from the fact that we obtain very similar results if we use *RATIO_REV2* instead of *RATIO_REV1* as our measure of real estate collateral, as illustrated in the top panel of Table 9.

We also investigated whether there were differences in the relationship between firm investment behavior and real estate collateral value depending upon whether the property market

[29] This helps control for another source of potential upward bias. As discussed in Chaney, et. al. (2012), upward bias in β might also result from potential endogeneity arising from the decision to own real estate in the first place. If firms that are more likely to own real estate also are especially sensitive to local demand shocks, equation (1) will overestimate the collateral channel effect. Our inclusion of the firm traits and their interaction with local land prices helps control for any fixed firm-level correlation between investment and real estate values. We have no good instrument to deal with variation that may not be fixed, but this is not costly for us, as we do not find a meaningful collateral channel effect in any event.

was improving or declining. The second panel of Table 9 reports results from adding an interaction term of *RATIO_REV1* with a dummy variable that takes a value of one if the relevant underlying land market was declining in value (i.e., *DECREASE*=1 if so, and =0 otherwise). There is no evidence of any important asymmetry in impacts on investment behavior, as the interaction term is never statistically significant at anything close to conventional confidence levels. While not reported here for space reasons, including this added term does not affect the coefficients on the other right-hand side regressors in any material way.

Another robustness check that was performed arose out of a concern that unobserved firm-level default risk could be biasing down our estimate of β. This could occur if the firms with the largest real estate holdings were also perceived by lenders as being very risky so that they could not borrow to finance additional investment even in the face of rising property values. To investigate this, we began by estimating a corporate default risk instrument at the company level. We then included the inverse Mill's ratio from that corporate default model as a proxy to control for unobserved heterogeneity in default risk in our investment equation. The appendix goes into the details behind the creation of this variable. The bottom panel of Table 9 reports estimates of our collateral channel effect when a corporate default risk factor is added to our baseline model. Note that there is virtually no change in the estimated impact for non-SOEs. That for SOEs does increase a bit, but it still remains immaterial in economic and statistical terms. Including added controls for potentially asymmetric collateral effects (as in the middle panel) does not alter the results. In sum, the absence of a collateral channel effect in China does not appear to be due to some type of specification bias arising from an omitted firm trait such as default risk.[30]

[30] While our focus is on the estimate of β as a measure of the collateral channel effect, the estimated coefficient for corporate default risk is interesting in its own right. That for SOEs is positive and statistically significant (point

We also investigated whether the small and insignificant estimates of β might be due to a lack of investment opportunities for some firms. The answer is no. First, there is no evidence that amassing more real estate is negatively correlated with growth in EBITDA or asset accumulation in general. We also reestimated our baseline model on subsets of firms broken down by whether they were headquartered in high, average or low growth cities. These classifications were based on local market GDP growth rates computed by the Chinese government. The results for each group were statistically and economically indistinguishable from those for the combined sample reported in Table 8. It turns out that the average local GDP growth rate in the group of lowest growth markets is quite high (at about 11.7% annually), so there are plenty of investment opportunities in those places, too. We also estimated the baseline model on subsets of firms based on their own asset growth rates. The point estimates for β were larger for both SOEs and non-SOEs among the highest third in terms of asset growth, but they were never statistically significantly different from zero themselves or from the point estimates for the lowest third of firms in terms of annual asset growth.

Another possible explanation for the absence of any estimated collateral channel effect could be that lenders recognize the strong mean reversion in Chinese land prices plotted in Figure 4 and only respond to longer-run, not annual, changes. Deng, Gyourko and Wu (2012) report that the mean reversion the annual land price growth data is on the order of 35%.

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estimate of 0.0426, with a standard error of 0.0131), but that for non-SOEs is not (point estimate of 0.0286, with as a standard error of 0.0202). We also estimated the analogous regression using borrowing as the dependent variable and found that riskier SOEs also borrowed more (but not riskier non-SOEs). Thus, this type of firm was allowed to borrow and invest more. It is possible that these firms are viewed as 'too big to fail', but that is the subject for another paper. More relevant for the present paper work is that there is no such effect for non-SOEs. Finally, including this default risk proxy did not materially change the coefficients on the other regressors.

However, our estimates using 2- and 3-year moving averages for both the land price and investment variables never yield a statistically significant positive relationship either.[31]

The robustness of our conclusion that there is no meaningful collateral channel effect in China is heightened by the fact that the standard errors about our estimates of β are small enough so that even the implied economic impact from a coefficient two standard deviations above the reported point estimate still is small.[32] Returning to our baseline results in Table 8, if we consider the estimates for non-SOEs, the group of firms for which a sizeable collateral channel effect is most plausible, a coefficient of 0.0243, which is two standard deviations above the -0.0003 value reported in column 3 of Table 8, implies only six percent of a standard deviation increase in investment per dollar of assets among those firms. Given that virtually any endogeneity-driven bias arising from our simple OLS specification is to raise β above its true value[33], this implies a fairly tight estimate around zero for β and strongly suggests that there is no collateral channel effect operating among listed, non-real estate firms in China.

That there is no collateral channel effect operating for SOEs is readily explainable in terms of their not being financially constrained. Indeed, our findings support the claims by many

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[31] We do find a significantly negative relationship between collateral value and investment for SOEs, but as discussed above, that is consistent with theory when firm value is held constant in the regression. All these results are available upon request.

[32] This argument obviously presumes that the standard errors are not biased downward. Classical measurement error in the dependent investment variable would lead to standard errors being underestimated. Ours is a created variable, so we cannot know for sure whether this is happening. However, the techniques used to construct it are virtually identical to those employed by researchers using U.S. data, which typically find large collateral channel effects. Hence, we do not believe this to be an important enough statistical problem that could change our key conclusion.

[33] Thus far, we have abstracted from the possibility that measurement error in our land price index variable could result in countervailing attenuation bias. The direction of the bias in that case is uncertain because it depends upon how any such measurement error associated with the land price index variable is correlated with other regressors. We suspect any measurement error is orthogonal to the firm characteristics on the right-hand side of (1), but we cannot know for sure. In addition, we believe the signal-to-noise ratio of this variable is high. Deng, Gyourko and Wu (2012), which describes the creation of the land price index variable in greater detail, shows there is a very strong common trend across properties within any given market. This suggests that the measurement error associated with any individual property value (which is the actual collateral) is small compared to the 'signal' of the market-level land price index itself. Hence, the attenuation bias should be less of a concern here given the likely strong signal-to-noise ratio. We did experiment with regressions that put more weight on city-year index observations with more underlying land transactions, but always got very similar results.

that SOEs are specially favored within the Chinese economy (e.g., see Lin and Tan (1999), Allen, Qian and Qian (2005), and Poncet, Steingress and Vandenbussche (2010)), and have no need to rely on increasing collateral value to secure financing. However, that cannot be the story for non-SOEs, who are much more likely to be financially constrained. To explain the absence of any meaningful collateral channel effect for this group, we must appeal to features of the Chinese banking system and capital markets. The aggregate volume of credit is tightly controlled in China, with the government often imposing quotas on lenders in different cities (e.g., see He and Wang (2012) and Allen, et. al. (2012) for recent discussions on this). Even if underlying collateral value owned by the firm increases, a binding quota implies that no further lending or borrowing can or will take place, as illustrated above in Figure 1.

Other regulatory norms also could help explain why we do not observe a collateral channel effect among non-SOEs, even if loan quotas are not binding. Common practice in the Chinese lending markets does not allow firms that experience positive appreciation on existing assets pledged as collateral against current loans to use that enhanced value to obtain new loans or better terms on the existing loans. This fixity in loan terms cuts off any possibility for a collateral channel effect.[34] Added properties could be used as collateral for additional debt, of course, but that is very different from being able to exploit increased value on existing assets.

The prospect of a binding loan quota constraint suggests that the best case for observing a collateral channel effect among non-SOEs would be in the recent 2009-2010 stimulus period, when those quotas were raised. This would be the equivalent of raising the loan quota from L_1 to L_2 in Figure 1. Table 10 reports regression results analogous to those in Table 8, except that they

[34] In most cases, non-SOEs also cannot obtain better loan terms simply by prepaying their existing loans. There is no guarantee that private firms can get new loans after prepaying unless loan quota amounts have been relaxed. This is one reason the prepayment of corporate loans is rare in China, although there are no accurate statistics we could find on this issue (which is another indication of how rare this practice is).

include the interaction of our firm-level real estate collateral variable with a dummy for the stimulus period. Even in the stimulus period, Table 10 shows no evidence that non-SOEs were engaged in investment booms that are correlated with higher owned real estate values, and we know from Figure 3 that values were increasing markedly in most markets during this period.[35]

The absence of any meaningful economic or statistical relationship between real estate collateral value and firm investment naturally raises the question of whether firms are even borrowing more at all when real estate values are higher. Table 11 reports results using the annual change in total debt per dollar of asset value as the dependent variable for the full sample of firms and then the breakdowns for SOEs and non-SOEs. Here, we find that real estate collateral values are positively correlated with firm borrowing for non-SOEs, but the impact is small in both economic and statistical terms.

Table 12 then reports results on the impact on borrowing from a specification including an interaction of the collateral value control with the stimulus period. Interestingly, the results in column 3 show a statistically significantly higher amount of borrowing by non-SOEs (but not SOEs, as expected) during the stimulus period that is associated with their appreciated real estate assets. The impact of higher collateral value during the stimulus period (which equals 0.0538 as the sum of -0.0243+0.0781) suggests that a $1 increase in real estate collateral value will raise non-SOEs' loan balances by just over five cents, controlling for other factors. How large this is in economic terms can be seen via the following calculation. The standard deviations of *RATIO_REV1* and *RATIO_LOAN* for non-SOEs during the stimulus period are 0.144 and 0.079, respectively, which implies that a one standard deviation increase in collateral values is

[35] Another possibility is that non-SOEs operate in industries that the central or local governments want to discourage. In that case, it could be that they cannot expand for other regulatory reasons independent of the collateral channel effect. To investigate this, we collected data on the level of subsidy received by the firm, as described for the *RATIO_SUBSIDY* variable in the previous section. We found no evidence that non-SOE investment behavior varied by the degree of subsidy.

associated with about 10% of a standard deviation increase in loan balance (i.e., $0.144 \times 0.0538 / 0.079 \approx 0.0981$).

If non-SOEs were borrowing a bit more, but not investing more in property, plant and equipment, what were they doing with the funds? Table 13 provides some answers. Six potential spending outcomes (other than fixed asset investments) are investigated. These include hiring more employees (*EMP*), increasing salary payments (*RATIO_SALARY*), expanding equity payouts (*RATIO_DIVIDEND*), investing in financial assets such as stocks or bonds (*RATIO_FAINV*), replacing short-term debt with long-term debt (*LONGLOAN*), or doing nothing but simply holding more cash (*RATIO_CASH*). The results show statistically significant correlations of increasing real estate collateral value during the stimulus period only with firm employment levels, with a marginally significant correlation with respect to adjusting debt structure to take on more longer-term loans. Cash holdings are also higher, but the point estimate of 0.0505 (column 1) only has a t-statistic of 1.5.

While statistically significant, the impact on employment is modest in economic terms. A one standard deviation of increase in collateral value only results in about three percent of a standard deviation increase in employment ($0.144 \times 0.2275 / 0.987 \approx 0.0332$) for non-SOEs during the stimulus period. The case for adjusting debt structure is similar, as a one standard deviation of increase in collateral value is associated with about seven percent of a standard deviation increase in increase of long-term debt share ($0.144 \times 0.0822 / 0.131 \approx 0.0726$). While not statistically significant, the point estimates for cash reserves imply that a one standard deviation increase in real estate collateral value is associated with one-seventh of a standard deviation increase in cash holdings.

We cannot tell why non-SOEs did not 'splurge' more with their added debt, but it could be they understood the change was not sustainable. With the stimulus package, the Chinese government only temporarily relaxed the loan quota, but did not unfold any fundamental reform in the financial system which could be expected to systematically ease non-SOEs' access to bank loans over the long term. If the entrepreneurs running the non-SOEs expected the loan quota to decrease back to old levels after the stimulus (i.e., from L_2 to L_1 in terms of our Figure 1), it would be unwise of them to spend the added funds on 'irreversible' uses such as fixed assets or equity payouts.[36] Instead, they appear to have chosen to expand their business by hiring a few more employees and taken out a few more longer-term loans (and possibly increased cash reserves).

While we have established that there is no amplification of investment among listed companies, whether purely private or primarily state-owned, that does not necessarily mean that no collateral channel effect exists throughout the Chinese economy. There are two other sectors to consider. One is the set of non-listed firms that are not traded on any stock exchange. According to the latest Economic Census in 2008, the reported total assets of all manufacturing firms were 35.87 trillion yuan RMB, while the listed manufacturing firms in our sample account for only 1.78 trillion, or 4.95% of the total (or about 6.57% if we exclude the FDI component). Thus, there are many such firms, but the information on them is completely unaudited, so that data quality is higher uncertain. Moreover, information on their real estate holdings is not available. Hence, the empirical analysis performed above cannot be replicated. That said, we

[36] This may be investing too much foresight and discipline in the non-SOE managements, but it is the case that the debt markets soon reverted back to their formerly difficult conditions for most purely private enterprises. They became so constrained that the Chinese government has begun to seriously consider undertaking a major reform in China's financial system in early 2012 to provide more secure financing to these firms. See reports from the *Wall Street Journal* ("China Tests Financial Relaxation in Wenzhou", March 28, 2012; "Chinese Premier Blasts Banks", April 3, 2012) for examples of this.

know of no reason why the results obtained for the listed companies would not apply to this group of firms. With respect to the SOEs, there is no reason to believe they are not financially unconstrained, just like their publicly-listed brethren. Hence, we would not expect to find a collateral channel effect among this group of firms either. The same holds for the non-SOEs that are not publicly traded. They do access the commercial loan market according to the survey results in Ayyagari, Demirguc-Kunt and Maksimovic (2010). And, there is no reason to suspect that the loan quotas and financial market norms that prevent easy prepayment and refinancing of existing debt do not apply with at least equal (and perhaps more) force for them. Thus, there is no reason to believe that there would be a larger collateral channel effect for non-SOEs that are not publicly traded compared to those that are.

Perhaps the most likely exception to our finding of no collateral channel effect in China is among local government-sponsored enterprises (local SOEs) charged with developing urban infrastructure. Local governments needing (say) a road or a subway constructed usually establish a local SOE to build the infrastructure. These entities typically are capitalized with land grants from the local government. The land, which essentially serves as the local SOE's equity capital, can be used to help raise debt from banks to complete the financing of the infrastructure provision.[37] These entities are not publicly traded on stock exchanges, so detailed firm-level data of the type used in the empirical analysis above are not available. However, we can aggregate data on infrastructure provision at the city level. This includes investment by local SOEs plus whatever the city itself directly provides. We proxied city collateral value first by

[37] A recent example of this type of entity is illustrated in the April 23, 2012, *Wall Street Journal* article entitled "Behind a Chinese City's Growth, Heavy Debt". It describes the city of Chongqing's (which is one of our 35 markets) use of this structure.

summing the total amount of land supplied in each city from 2001-2010, and then calculating its market price at the beginning of 2003 (scaled by GDP) using our land price data.[38]

We then ran a number of simple regressions of annual infrastructure investment (scaled by city GDP) on the collateral value variable plus city and year fixed effects. Higher city-level collateral values generally were positively correlated with aggregate city infrastructure investment, but the coefficients often were not significantly different from zero. We did typically find a significantly positive correlation with transportation infrastructure investment, when we experimented with disaggregated data.[39] One admittedly naïve specification that regressed the log change in transportation infrastructure investment per dollar of city GDP on the log change in our land value collateral variable implied that a one standard deviation higher amount of collateral was associated with one-fifth of a standard deviation increase in transportation infrastructure (controlling for city and year fixed effects). Naturally, this result should be interpreted with caution given the obvious potential endogeneity concerns. That said, we are not surprised to find this relationship in a special type of vehicle used by local governments in China in which land grants serve as equity. Even so, this impact occurs in a sector responsible for a relatively small share of overall investment in the country. In 2011, for example, local governments spent 901.6 billion yuan RMB on urban transportation infrastructure investment, accounting for about 3% of the total investment volume in that year.

IV. Conclusions

Research in macroeconomics and financial economics reports substantial collateral

[38] We also experimented with another measure of total potential developable land in the urban area calculated by Wang, Chan and Xu (2012).
[39] We can break overall infrastructure into transportation (e.g., roads, subways), environmental (e.g., parks, sewerage), and other.

channel effects on firm investment that amplified the business cycles of the United States and Japan. We provide the first analysis of whether something similar can be expected for China, which also has experienced an extraordinary property market boom that appears to have crested recently. Working with unique data on land values in 35 major Chinese markets and a panel of firms outside the real estate industry, we estimate investment equations that yield no evidence of a collateral channel effect.

This is perhaps not surprising for state-owned enterprises which probably are not financially constrained in the sense required by the underlying theory for a collateral channel effect to operate. However, we also find no such effect for non-SOEs, so our average results are not masking important heterogeneity across different types of firms. Norms and regulation in the Chinese capital markets and banking sectors can explain why the collateral channel effect does not operate among these firms in China. Various robustness analyses that range from investigating whether there is an asymmetry in the effect based on whether property market prices are increasing or decreasing to whether there is specification error from omitted firm-level default risk confirm this conclusion.

While this indicates that a real estate bust in China is unlikely to cause a material decline in investment by non-real estate firms because of a decline in underlying property collateral values, this does not imply that a bust will not materially harm the economy. It could, and we strongly suspect it will. The direct impact of a major decline in property values on hiring in the construction industry and on demand for raw and processed materials is likely to be quite large, even without amplification from the collateral channel. More indirect impacts via a wealth effect on household consumption also could be important (as in Gan 2010). However, we leave investigation of those potential mechanisms to future work.

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Figure 1: Loan Quotas and Collateral Channel Effects

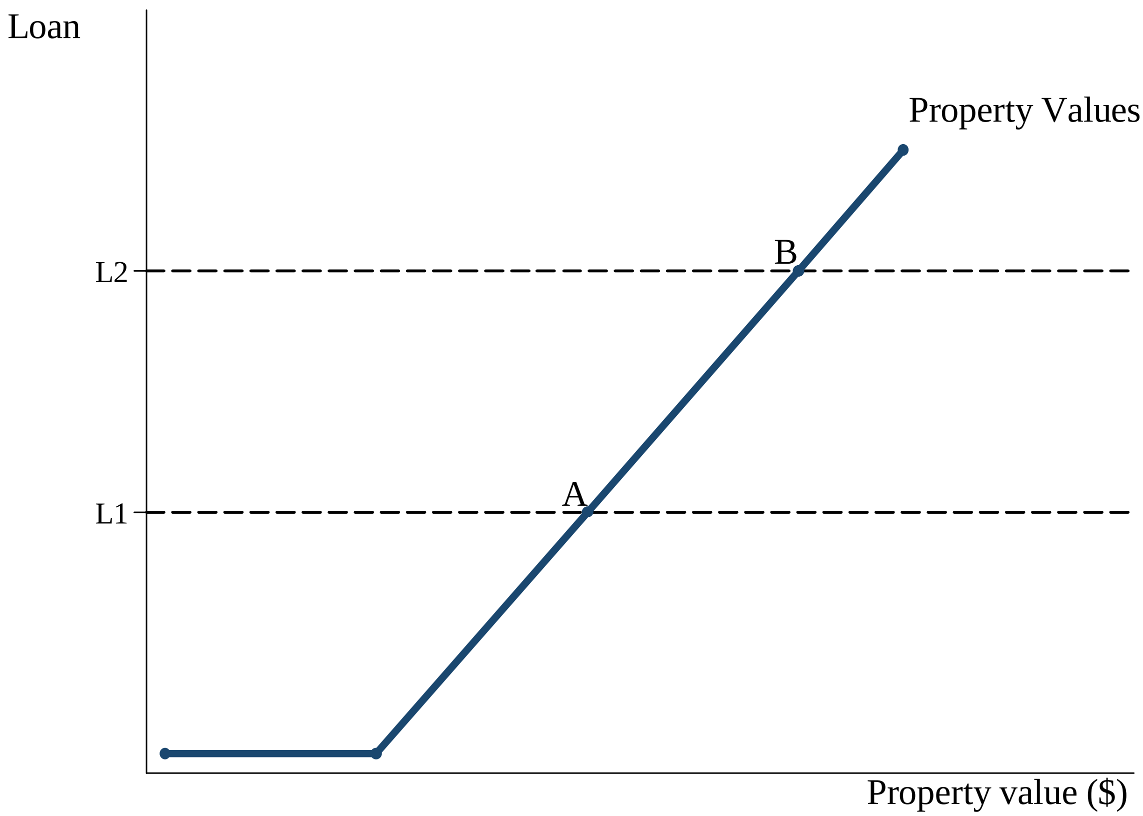
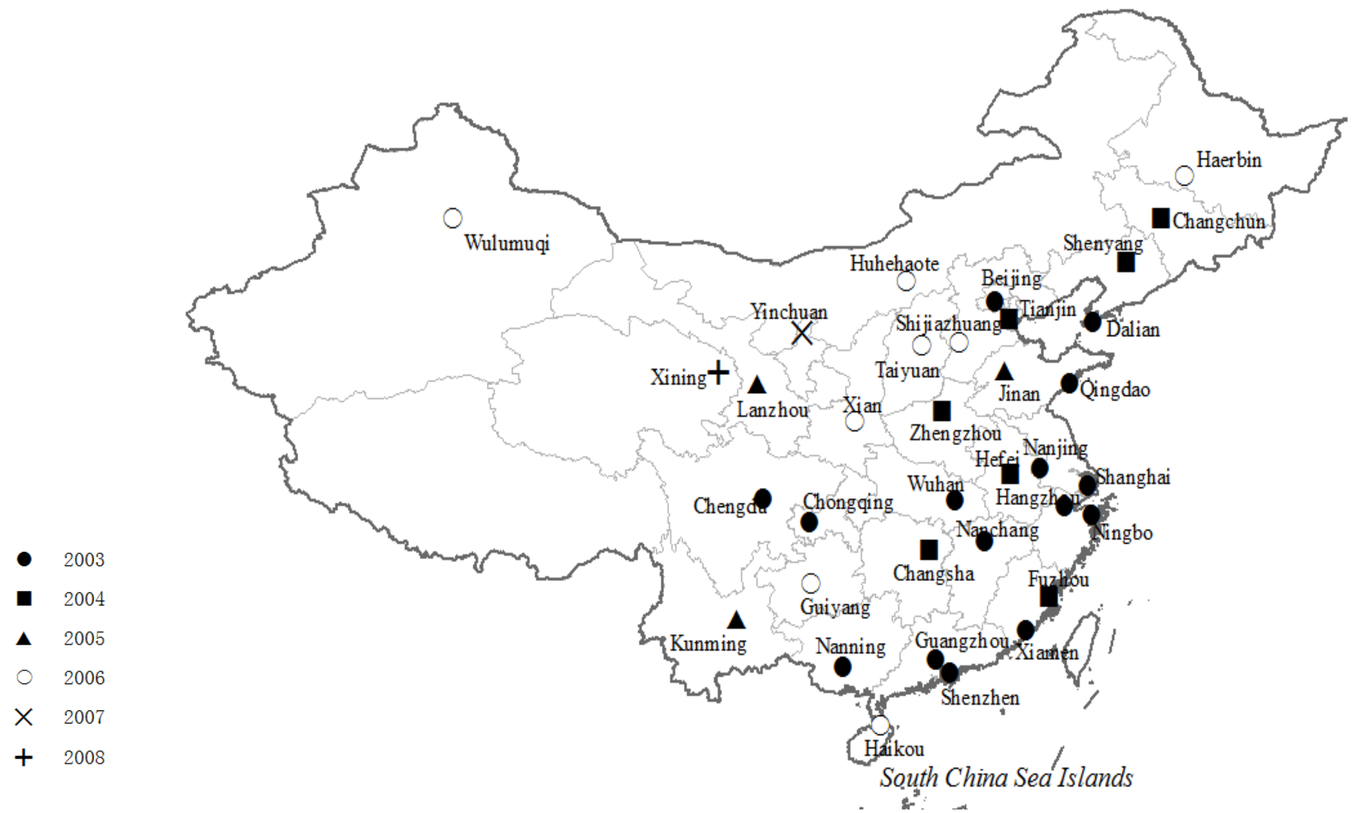


Figure 2: Land Market Dataset Coverage



Note: the cities are labeled by the starting year of the land data in the dataset.

Figure 3: Real Average Residential Land Prices in 35 Major Chinese Markets (2009 Yuan)

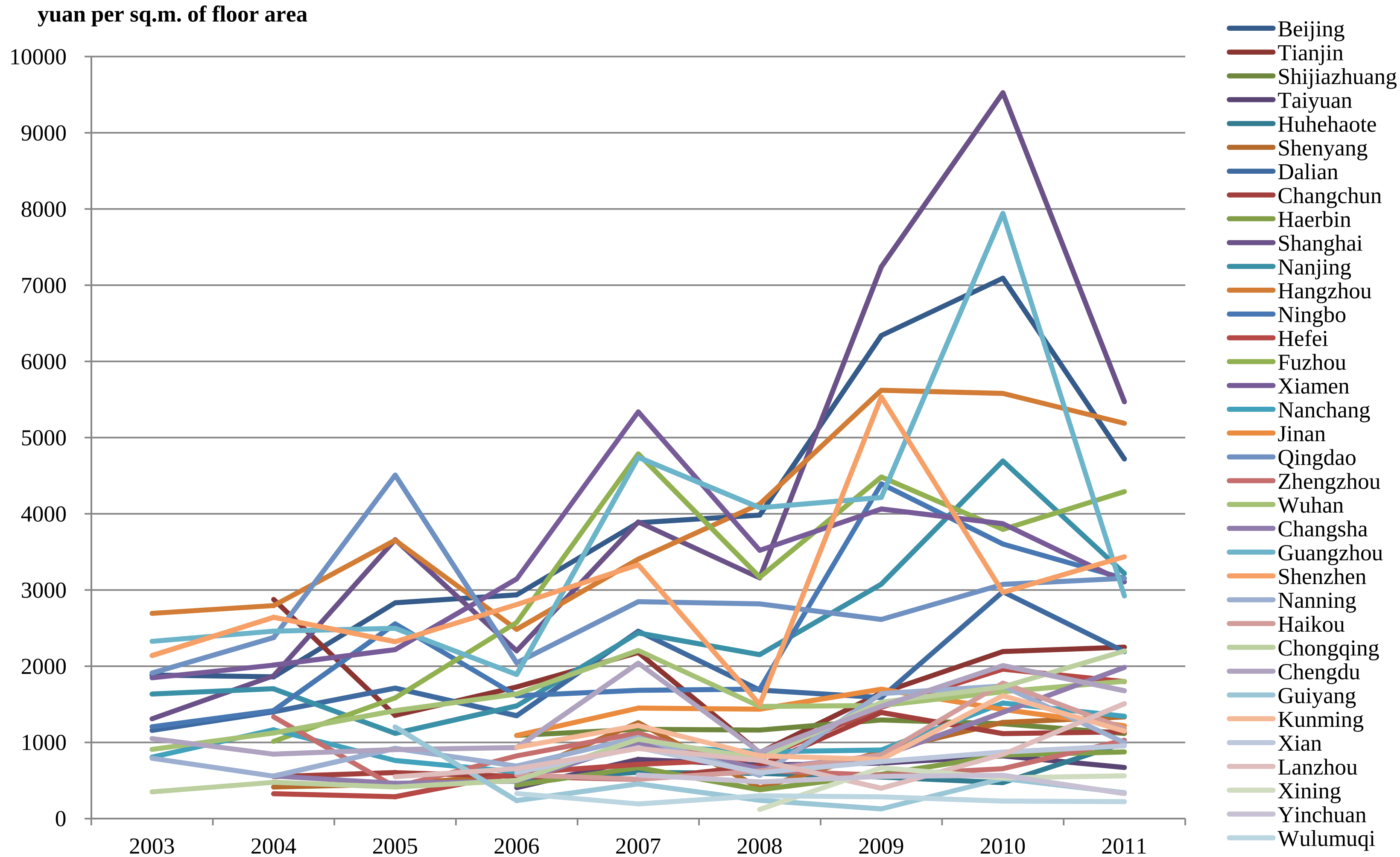


Figure 4: Real Constant Quality Land Price Appreciation by Year, 35 Chinese Cities, 2003-2011

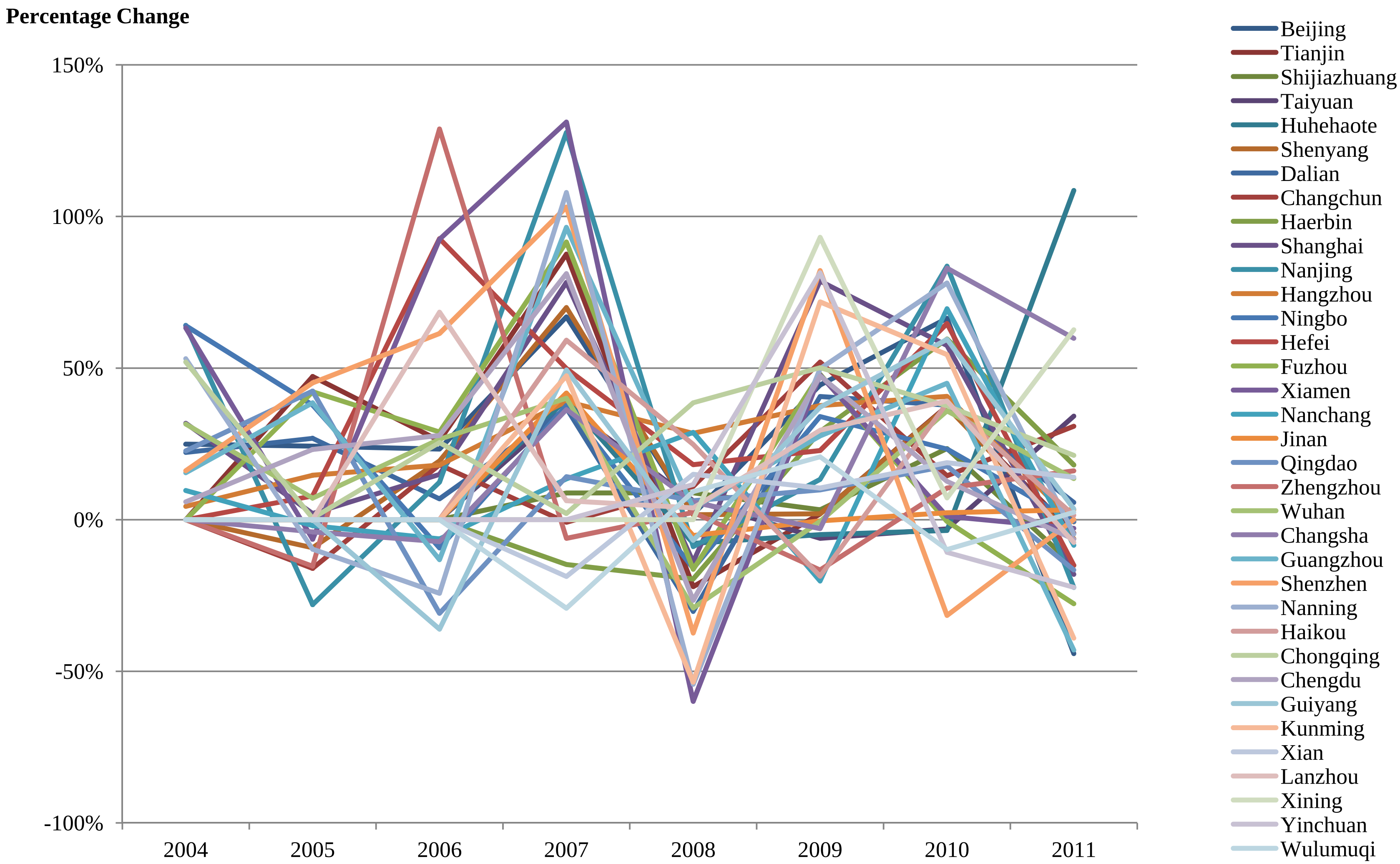


Table 1 Sample Sizes in the Land Transaction Dataset

	Number of Cities Covered	Number of Land Parcels Sold
2003	15	378
2004	22	681
2005	24	773
2006	33	1133
2007	34	1413
2008	35	963
2009	35	1564
2010	35	1759
2011	35	1749
Aggregated	-	10413

Table 2: Annual Real Land Price Appreciation, Summary Statistics, 35 Major Chinese Markets

	2003-2004	2004-2005	2005-2006	2006-2007	2007-2008	2008-2009	2009-2010	2010-2011
Mean	32.1%	12.2%	23.5%	46.4%	-5.3%	28.5%	31.4%	2.57%
Standard Deviation	21.7%	23.1%	40.5%	42.1%	24.0%	30.7%	29.4%	30.22%
Max	64.1%	47.2%	128.8%	131.2%	38.6%	93.1%	83.6%	108.58%
Min	4.4%	-28.0%	-36.1%	-29.2%	-59.9%	-20.2%	-31.6%	-44.2%
Number of Cities	15	22	24	33	34	35	35	35
Number with Positive Appreciation	15	15	17	28	18	27	29	16
Number with Negative Appreciation	0	7	7	5	16	8	6	19

Table 3: Average Annual Growth Rates and Standard Deviations: Land Prices, House Prices, Construction Costs and Wages

A. Mean of Annual Real Growth Rate								
	2004	2005	2006	2007	2008	2009	2010	2011
House Price	4.14%	5.56%	6.72%	13.60%	8.18%	9.17%	23.31%	8.47%
Land Price	32.07%	12.22%	23.51%	46.39%	-5.34%	28.46%	31.36%	2.57%
Construction Cost	6.26%	0.12%	0.22%	1.26%	6.77%	-1.87%	1.76%	-
Construction Industry Wage	8.24%	12.38%	14.19%	10.73%	8.56%	14.62%	10.26%	-
Number of Cities Included	15	22	25	33	34	35	35	35
B. Standard Deviation of Annual Real Growth Rate								
	2004	2005	2006	2007	2008	2009	2010	2011
House Price	4.91%	3.64%	6.13%	12.41%	8.31%	6.39%	11.60%	7.82%
Land Price	21.68%	23.06%	40.52%	42.11%	23.98%	30.72%	29.44%	30.22%
Construction Cost	2.23%	1.68%	1.39%	1.38%	2.49%	1.31%	1.29%	-
Construction Industry Wage	5.78%	4.29%	4.61%	5.07%	4.33%	9.01%	4.89%	-
Number of Cities Included	15	22	25	33	34	35	35	35

Table 4: Real Estate Asset Information in the Financial Reports

(A) Before 2006

<table>
<tr><th></th><th></th><th>Self-Occupied & Lease-Out Properties</th></tr>
<tr><td rowspan="2">Self-Built Properties</td><td>Construction in Progress</td><td>• The lands are listed in the item of “Land Ownerships” as a subentry of “Intangible Assets”.
• The construction materials, affiliated plants and equipment are listed in the item of “Construction in Progress”.</td></tr>
<tr><td>Completed Properties</td><td>• The lands are listed in the item of “Land Ownerships” as a subentry of “Intangible Assets”.
• The structures are listed in the item of “Buildings” as a subentry of “Fixed Assets”. The plants and equipment are excluded.</td></tr>
<tr><td colspan="2">Purchased Properties</td><td>• Both lands and structures are listed in the item of “Buildings” as a subentry of “Fixed Assets”. The plants and equipment are excluded.</td></tr>
</table>

(B) Since 2007

<table>
<tr><th></th><th></th><th>Self-Occupied Properties</th><th>Lease-Out Properties</th></tr>
<tr><td rowspan="2">Self-Built Properties</td><td>Construction in Progress</td><td colspan="2">• The lands are listed in the item of “Land Ownerships” as a subentry of “Intangible Assets”.
• The construction materials, affiliated plants and equipment are listed in the item of “Construction in Progress”.</td></tr>
<tr><td>Completed Properties</td><td>• The lands are listed in the item of “Land Ownerships” as a subentry of “Intangible Assets”.
• The structures are listed in the item of “Buildings” as a subentry of “Fixed Assets”. The plants and equipment are excluded.</td><td rowspan="2">• Both lands and buildings are listed in the item of “Investment Properties”. The plants and equipment are excluded.</td></tr>
<tr><td colspan="2">Purchased Properties</td><td>• Both lands and structures are listed in the item of “Buildings” as a subentry of “Fixed Assets”. The plants and equipment are excluded.</td></tr>
</table>

Table 5: Definition and Summary Statistics of Variables

Variable	Definition	Average	Std. Dev
ASSET	Total asset at the beginning of the year; in billion yuan RMB.	4.882	17.598
RATIO_REV1	Market value change of real estate assets held in 2002 (see the text for more details); normalized by *ASSET*.	0.060	0.151
RATIO_REV2	Market value change of real estate assets held at the beginning of the year (see the text for more details); normalized by *ASSET*.	0.075	0.170
RATIO_INV	Net change in investment on fixed assets; normalized by *ASSET*.	0.056	0.056
RATIO_LOAN	Net change in loan balance; normalized by *ASSET*.	0.019	0.074
RATIO_EBITDA	Earnings before interest, taxes, depreciation and amortization; normalized by *ASSET*.	0.089	0.059
RATIO_CASH	Net change in cash holdings; normalized by *ASSET*.	0.020	0.073
RATIO_SALARY	Total salary payments; normalized by *ASSET*.	0.065	0.034
RATIO_DIVIDEND	Total dividend payments; normalized by *ASSET*.	0.018	0.028
RATIO_FAINV	Net change in investment on financial assets; normalized by *ASSET*.	0.006	0.031
EMP	Number of employee per million of *ASSET*.	1.381	1.012
RATIO_SUBSIDY	Total amount of government subsidies received; normalized by *ASSET*.	0.003	0.005
MBR	Market-to-book ratio at the beginning of the year.	1.627	1.043
LEVERAGE	Leverage level at the beginning of the year.	0.501	0.159
LOANLOAN	Net change in share of long-term loan in total loan balance.	0.005	0.128
RISK	The ratio of the probability density function to the cumulative distribution function of corporate default model at the beginning of the year (see the text for more details).	3.069	0.105

Table 6: Distribution of Sample by Ownership Structure

Year	Number of SOEs	Number of Non-SOEs
2003	353	91
2004	347	97
2005	343	101
2006	329	115
2007	323	121
2008	325	119
2009	320	114
2010	318	126
2011	318	126

Table 7: Summary Statistics of Variables by Ownership Structure Groups

	SOEs		Non-SOEs		t stat.
	Average	Std. Dev	Average	Std. Dev	
ASSET	5.477	20.089	3.147	5.650	3.655***
RATIO_REV1	0.060	0.153	0.060	0.144	0.018
RATIO_REV2	0.076	0.173	0.075	0.163	0.062
RATIO_INV	0.057	0.055	0.055	0.057	1.052
RATIO_LOAN	0.019	0.073	0.022	0.079	1.109
RATIO_EBITDA	0.086	0.057	0.096	0.065	4.442***
RATIO_CASH	0.019	0.071	0.023	0.078	1.704*
RATIO_SALARY	0.066	0.033	0.061	0.035	4.807***
RATIO_DIVIDEND	0.017	0.026	0.023	0.034	5.464***
RATIO_FAINV	0.005	0.029	0.008	0.035	3.295**
EMP	1.377	1.020	1.390	0.987	0.331
RATIO_SUBSIDY	0.003	0.005	0.004	0.006	3.191***
MBR	1.552	0.972	1.847	1.201	7.817***
LEVERAGE	0.502	0.158	0.499	0.163	0.455
LOANLOAN	0.004	0.127	0.008	0.131	0.855
RISK	3.075	0.102	3.053	0.113	5.119***

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Do Chinese Firms Invest More When Collateral Value Increases?

Dependent Variable: *RATIO_INV*

	Full Sample	SOE's	Non-SOE's
$RATIO\_REV1_{i,t}$	0.0045	0.0062	-0.0003
	(0.0061)	(0.0072)	(0.0123)
Log(LP_{it})	-0.0148	-0.0154	0.0217
	(0.0110)	(0.0122)	(0.0245)
$MBR_{i,t}$	0.0032***	0.0030**	0.0050**
	(0.0011)	(0.0013)	(0.0023)
$RATIO\_EBITDA_{i,t}$	0.1743***	0.1761***	0.1776***
	(0.0186)	(0.0237)	(0.0375)
$LEVERAGE_{i,t}$	-0.0624***	-0.0662***	-0.0629***
	(0.0108)	(0.0123)	(0.0192)
Initial Controls * Log($LP_{i,t}$)	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Number of Observations	3974	2962	1012
R2	0.473	0.488	0.499

Note: (1) the observations are clustered by city-year.
(2) standard errors in parentheses.
(3) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Collateral Channel Effects under Alternative Specifications

Dependent Variable: *RATIO_INV*

	Full Sample	SOE's	Non-SOE's
A. Real Estate Collateral Value Measured Continuously (*RATIO_REV2* instead of *RATIO_REV1*)			
$RATIO\_REV2_{i,t}$	-0.0026	-0.0035	-0.0010
	(0.0064)	(0.0078)	(0.0109)
B. Asymmetry in Collateral Channel Effects			
$RATIO\_REV1_{i,t}$	0.0083	0.0110	0.0019
	(0.0079)	(0.0102)	(0.0170)
$RATIO\_REV1_{i,t}$ $*DECREASE_{i,t}$ (*DECREASE*=1 if land values are declining; =0, otherwise)	-0.0148	-0.0184	-0.0095
	(0.0211)	(0.0260)	(0.0486)
C. Real Estate Collateral Effects Controlling for Corporate Default Risk			
$RATIO\_REV1_{i,t}$	0.0093	0.0111	0.0002
	(0.0083)	(0.0095)	(0.0210)

Note: (1) all models are estimated with additional control variables (see Table 8 for the full specification).
(2) the observations are clustered by city-year.
(3) standard errors in parentheses.
(4) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Is There Evidence of a Collateral Channel Effect During the Stimulus Period?

Dependent Variable: *RATIO_INV*

	Full Sample	SOE's	Non-SOE's
$RATIO\_REV1_{i,t}$	0.0068	0.0083	-0.0056
	(0.0089)	(0.0102)	(0.0176)
$RATIO\_REV1_{i,t}$ * 2009/10	-0.0043	-0.0042	0.0095
	(0.0110)	(0.0141)	(0.0226)
Log(LP_{it})	-0.0150	-0.0156	0.0219
	(0.0110)	(0.0121)	(0.0245)
$MBR_{i,t}$	0.0032***	0.0030**	0.0050**
	(0.0011)	(0.0013)	(0.0023)
$RATIO\_EBITDA_{i,t}$	0.1743***	0.1762***	0.1778***
	(0.0186)	(0.0237)	(0.0376)
$LEVERAGE_{i,t}$	-0.0625***	-0.0662***	-0.0629***
	(0.0108)	(0.0123)	(0.0192)
Initial Controls * Log($LP_{i,t}$)	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Number of Observations	3974	2962	1012
R2	0.473	0.488	0.499

Note: (1) the observations are clustered by city-year.
(2) standard errors in parentheses.
(3) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Do Chinese Firms Borrow More When Land Values Rise?

Dependent Variable: *RATIO_LOAN*

	Full Sample	SOE's	Non-SOE's
$RATIO\_REV1_{i,t}$	-0.0000	-0.0052	0.0189
	(0.0104)	(0.0116)	(0.0203)
$\text{Log}(LP_{it})$	0.0110	0.0141	0.0456
	(0.0162)	(0.0172)	(0.0460)
$MBR_{i,t}$	0.0002	-0.0011	0.0069*
	(0.0020)	(0.0025)	(0.0037)
$RATIO\_EBITDA_{i,t}$	0.0329	0.0306	0.0184
	(0.0299)	(0.0376)	(0.0571)
$LEVERAGE_{i,t}$	-0.1467***	-0.1497***	-0.1737***
	(0.0175)	(0.0196)	(0.0356)
Initial Controls * $\text{Log}(LP_{i,t})$	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Number of Observations	3974	2962	1012
R2	0.232	0.248	0.285

Note: (1) the observations are clustered by city-year.
(2) standard errors in parentheses.
(3) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Did Chinese Firms Borrow More During the Stimulus Period?

Dependent Variable: *RATIO_LOAN*

	Full Sample	SOE's	Non-SOE's
$RATIO\_REV1_{i,t}$	-0.0105	-0.0093	-0.0243
	(0.0130)	(0.0165)	(0.0297)
$RATIO\_REV1_{i,t}$ * 2009/10	0.0197	0.0079	0.0781**
	(0.0199)	(0.0209)	(0.0371)
Log(LP_{it})	0.0118	0.0147	0.0465
	(0.0162)	(0.0173)	(0.0451)
$MBR_{i,t}$	0.0000	-0.0011	0.0066*
	(0.0020)	(0.0025)	(0.0036)
$RATIO\_EBITDA_{i,t}$	0.0329	0.0304	0.0201
	(0.0299)	(0.0376)	(0.0570)
$LEVERAGE_{i,t}$	-0.1466***	-0.1496***	-0.1742***
	(0.0176)	(0.0196)	(0.0356)
Initial Controls * Log($LP_{i,t}$)	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Number of Observations	3974	2962	1012
R2	0.232	0.248	0.288

Note: (1) the observations are clustered by city-year.
(2) standard errors in parentheses.
(3) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: How did Non-SOEs Spend the Money During the Stimulus Period?

	RATIO_CASH	*EMP*	*RATIO_SALARY*	*RATIO_FAINV*	*RATIO_DIVIDEND*	*LONGLOAN*
$RATIO\_REV1_{i,t}$	0.0248	-0.2525	0.0108*	0.0141	0.0066	-0.0368
	(0.0253)	(0.1785)	(0.0057)	(0.0134)	(0.0091)	(0.0504)
$RATIO\_REV1_{i,t}$ * 2009/10	0.0505	0.4800**	0.0004	-0.0177	-0.0106	0.1190*
	(0.0347)	(0.2241)	(0.0078)	(0.0189)	(0.0120)	(0.0647)
Log(LP_{it})	0.0010	-0.2323	-0.0067	-0.0232	-0.0128	0.0540
	(0.0461)	(0.2912)	(0.0100)	(0.0201)	(0.0168)	(0.0701)
$MBR_{i,t}$	0.0051	0.0053	0.0013*	0.0030	-0.0008	0.0053
	(0.0047)	(0.0246)	(0.0007)	(0.0019)	(0.0014)	(0.0075)
$RATIO\_EBITDA_{i,t}$	0.4168***	1.5708***	0.0780***	-0.0103	0.2028***	0.1022
	(0.0625)	(0.4122)	(0.0139)	(0.0265)	(0.0259)	(0.0984)
$LEVERAGE_{i,t}$	-0.0132	-0.0660	-0.0017	-0.0523***	-0.0152	0.0832
	(0.0358)	(0.2046)	(0.0063)	(0.0141)	(0.0103)	(0.0670)
Initial Controls * Log($LP_{i,t}$)	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1012	1011	1012	1012	1012	1012
R2	0.239	0.795	0.847	0.224	0.503	0.117

Note: (1) the observations are clustered by city-year.
(2) standard errors in parentheses.
(3) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 1: Estimation of Firm-Level Default Risk

We adopt a popular proportional hazard modeling framework (Deng (1997), Deng, Quigley and Van Order (2000)) to estimate the firm-level default risk. The empirical estimation is based on a large sample of 2,061 publicly-traded firms in China. We used data from 1995-2010 on these firms, which were traded on the Shenzhen or Shanghai stock exchanges. Our corporate default model specification follows the existing literature on firm bankruptcy analysis (see, for example, Duffie, Saita and Wang, 2007). Key variables of the default risk model include firm-specific attributes, such as distance to default (*DTD*), which is defined as the logarithm of total assets over total liabilities, weighted by the idiosyncratic risk of firm's stock return. Hillegeist et. al. (2004), Bharath and Shumway (2008), and Duan, Sun and Wang (2012) each report that distance-to-default is a highly significant measure that helps capture heterogeneous firm level credit risk. Another variable used is Net Income/Total Asset (*NI/TA*), which measures firm profitability. Bharath and Shumway (2008) and Duan, Sun and Wang (2012) find it provides significant predictive power for bankruptcy, controlling for the distance to default measure. A third variable is Earnings before Interest and Taxes/ Total Asset (*EBIT/TA*), which Altman (1968) include in his classic work. We also control for firm size (*SIZE*) based on the hypothesis that large firms are more likely to have more diversified business lines and financial flexibility than smaller firms. Other variables included in our model control for the type of firm, specifically whether the entity is a state-owned enterprise. This variable is called *OWNERSHIP* in our model. We also include various market level attributes such as real GDP growth (*GDP*), stock index return (*STOCK*), and the inflation rate (*INFLATION*).

Our results, which are presented below in Appendix Table A1, generally are consistent with findings in the existing literature (Hillegeist et. al. (2004), Bharath and Shumway (2008), and Duan, Sun and Wang (2012)). The estimated inverse Mill's ratio obtained from this firm bankruptcy default model is then added to our baseline investment regression, with results on the collateral channel effect reported at the bottom of Table 9. Note that this estimate is based on a slightly smaller sample of 376 firms (versus 444 firms in the baseline regression) because of missing credit risk data for some firms.

Appendix Table A1. Firm Level Bankruptcy Default Estimation

	Whether the Corporate Defaults
DTD	-0.2559***
	(0.0873)
NET INCOME	-0.4262***
	(0.1536)
EBIT/TA	-0.3682*
	(0.1967)
SIZE	-0.1385**
	(0.0697)
OWNERSHIP	-0.0976
	(0.0718)
GDP	-0.7328***
	(0.2529)
STOCK	0.0521
	(0.0727)
INFLATION	-0.3100***
	(0.1017)
Industry Fixed Effects	Yes
Number of Observations	2,061

Note: standard errors in parentheses.