The Financial Transmission of Housing Booms: Evidence from Spain*

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Abstract

How does a housing boom affect credit to non-housing firms? Using bank, firm and loan-level micro-data, we show that the Spanish housing boom reduced non-housing credit growth during its first years, but stimulated it later on. These patterns can be rationalized by financial constraints for banks. Constrained banks initially accommodated higher housing credit demand by reducing non-housing credit. Eventually, however, the housing boom increased bank net worth and expanded credit supply. A quantitative model, disciplined by our cross-sectional estimates, indicates that the crowding-out effect was substantial but temporary, and had been fully absorbed by the end of the boom.

Keywords: Housing boom, Credit, Investment, Financial Frictions, Financial Transmission, Spain.

JEL Codes: E32, E44, G21.

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1 Introduction

During the last two decades, many countries (including the United States, China, the United Kingdom, Spain and Ireland) experienced large run-ups in house prices. These housing booms are widely believed to have had important spillovers on the non-housing sector, and understanding their transmission channels has become a key concern for economists and policymakers (see Zhu, 2014 and Jordà et al., 2015).

In this paper, we analyze the role of banks for the transmission of housing booms. Despite its economic importance, the role of the banking system as a transmission channel is a priori unclear. On the one hand, some studies argue that housing booms crowd out credit to firms in the non-housing sector, as banks reallocate credit to mortgages, real estate and construction firms (e.g., Chakraborty et al., 2018; Hau and Ouyang, 2018). On the other hand, there is also evidence that housing booms stimulate credit growth for all sectors of the economy (e.g., Chaney et al., 2012; Jiménez et al., 2019).

Using bank, firm and loan-level microdata, we show that both effects operated during the Spanish housing boom of the early 2000s. Crucially, however, their strength varied over time. In its first years, the housing boom reduced credit growth for firms in the non-housing sector, but this eventually reverted and the boom ended up increasing credit growth for firms in the non-housing sector. We argue that financial constraints for banks can rationalize both the initial crowding-out effect and its later reversal, and provide empirical support for this hypothesis. Finally, we use a calibrated model, partly disciplined by our cross-sectional empirical estimates, to quantify the aggregate importance of financial transmission. We find that the Spanish housing boom had a substantial crowding-out effect, but that this effect was short-lived and had been fully absorbed by the end of the boom.

Spain's housing boom was massive. Between 2000 and 2008, nominal house prices increased by 135%. Real housing credit increased by 243% between 2000 and 2007, and the share of housing in overall credit increased from 46.6% to 61.9% in the same time period.[1] This makes Spain an ideal case study for the effects of a housing boom on credit to firms in the non-housing sector.

For our analysis, we combine the Spanish Credit Registry (which contains virtually all bank loans to firms) and the Commercial Registry (which contains balance sheet information on virtually all firms). Our empirical strategy exploits bank and firm-level heterogeneity in exposure to the housing boom. We measure a bank's exposure by its share of credit allocated to housing in 2000. This reflects the idea that banks with housing-centered business models or pre-existing ties to housing were more affected by the boom. We restrict our firm sample to non-housing firms (firms that operate neither in the construction nor in the real estate

[1] Section 2.1 lists the sources for these figures and provides further background information on the Spanish housing boom.

sector), and measure firm exposure as a weighted average of the exposure of the banks a firm borrows from.

Using this dataset, we first regress loan-level credit growth (i.e., credit growth for any bank-firm pair) on bank exposure to the housing boom. Following Khwaja and Mian (2008), we use firm fixed effects to control for firm-level credit demand shocks. Thus, coefficients are identified by differences in the credit growth of the same non-housing firm at banks with different levels of exposure to the boom. We find that for the average non-housing firm, credit growth was significantly lower at more exposed banks during the first years of the boom (between 2001 and 2003), but became significantly higher at these banks during the final years of the boom (between 2004 and 2007).

In principle, these results may be due to firms reallocating their credit between different banks, without any effect on overall firm credit growth. To dispel this concern, we regress firm-level credit growth on our measure of firm exposure. We find that the boom did affect firm-level credit growth: non-housing firms that borrowed more from more exposed banks experienced lower credit growth during the first years of the boom, but higher credit growth during its final years. These results are confirmed when we consider value added or investment instead of credit.

In sum, we find that the Spanish credit boom crowded out non-housing credit in its first years. However, crowding-out eventually faded and gave way to a crowding-in effect. What could be the mechanism driving this pattern? In the literature, a standard explanation for crowding-out is that financial constraints make it costly for banks to raise external capital (Chakraborty et al., 2018). Thus, when facing rising credit demand from a booming housing sector, banks react by reducing the supply of credit to non-housing firms. We point out that this explanation has dynamic implications that have so far been overlooked. Indeed, rising demand for housing credit eventually also raises banks' profits and net worth. As banks are constrained, higher net worth is not neutral, but allows them to increase credit supply to all sectors of the economy. Thus, the crowding-out and crowding-in effects of a housing boom may be driven by the exact same mechanism.

The empirical evidence supports this conjecture. We show that the crowding-out and crowding-in effects identified in our baseline regressions are driven by a subsample of constrained banks (defined as banks with high leverage ratios). Moreover, there is a significant positive correlation between a bank's exposure to housing and its net worth growth during the boom years. Finally, we show that alternative explanations for why crowding-out gives way to crowding-in (e.g., a fall in the demand for housing credit, or a decision by exposed banks to diversify away from housing) are not supported by the data.

Our cross-sectional empirical results do not directly speak to the aggregate magnitude of financial transmission. On the one hand, if financial transmission affected all non-housing firms in a similar way, firm-level differences in credit growth would be smaller than aggregate effects. On the other hand, if there were large substitution effects between firms, firm-level differences would overstate aggregate effects.

To assess aggregate effects, we therefore need to rely on a model. This does not imply that our cross-sectional estimates are uninformative. Indeed, we show that using them as calibration targets allows us to identify two crucial model parameters. To the best of our knowledge, we are among the first to use cross-sectional evidence to discipline an aggregate model in the macroeconomic literature on credit markets.[2]

Our model considers a small open economy which is populated by overlapping generations of housing firms, non-housing firms, and banks. Firms in both sectors borrow from banks in order to finance capital investment, and banks borrow from an international financial market. Credit from different banks is imperfectly substitutable, so that firms borrow from multiple banks in equilibrium. Crucially, we assume that bank borrowing from the international financial market is limited by a leverage constraint. This captures the fact that raising funds is costly for banks, and implies that their credit supply is increasing in their net worth.[3] Net worth is persistent over time, as the profits of the old generation of banks are partly transferred to the young generation. Finally, the model reproduces cross-sectional heterogeneity through firm-specific preferences for credit from different banks. Just as in the data, we consider banks to be more exposed to a housing boom if a higher share of their pre-boom credit is allocated to housing (i.e., if housing firms have a relatively high preference for them). Likewise, non-housing firms are more exposed if they have a relatively higher preference for exposed banks.

We model a housing boom as a series of preference shocks raising the relative price of housing. Our model then qualitatively reproduces our cross-sectional empirical findings, neatly illustrating how financial constraints for banks generate successive crowding-out and crowding-in effects. A housing boom raises the credit demand of housing firms. However, it has initially only a small effect on bank net worth, which depends mainly on profits from past loans. As credit supply hardly moves, higher demand leads to an increase in domestic interest rates and a reduction in non-housing credit. These effects are stronger for more exposed banks: since housing makes up a larger share of their overall lending, they face the strongest increases in credit demand. As the boom progresses, however, the crowding-out effect is gradually reversed. Indeed, higher interest rates and a higher loan volume boost the net worth of banks, allowing them to expand their

[2] The Online Appendix of Chodorow-Reich (2014) contains one of the first model-based discussions of this issue. Subsequently, Catherine et al. (2018) and Herreño (2020) both use cross-sectional estimates to structurally estimate a model. The first paper aims to quantify the output losses from financial frictions, while the second aims to quantify the effect of bank lending cuts on output. Our own approach is methodologically most similar to Acemoglu and Restrepo (2020), who study the impact of robotization on employment and wages.

[3] In line with our assumption, the leverage ratio of the Spanish banking system was relatively stable during the housing boom (see Section 4 for details). Begenau et al. (2019) show that key features of bank behavior can be rationalized by financial constraints, reflecting either regulations or market discipline.

credit supply. Thus, interest rates fall and non-housing credit starts rising. Again, this effect is stronger for more exposed banks, as their net worth rises more.

We calibrate our model's parameters by matching aggregate and cross-sectional statistics (e.g., the share of housing in aggregate credit, or our empirical exposure measures for banks and firms). Most importantly, we show that - taking all other parameter values as given - our cross-sectional loan and firm-level estimates identify two key parameters: the elasticity of substitution of non-housing credit across banks, and a parameter that governs the speed at which banks accumulate net worth. Intuitively, if credit is more substitutable across banks, loan-level differences in credit growth are relatively larger than firm-level differences. Moreover, if bank net worth accumulation is fast, more exposed banks compensate their initial crowding-out effect more quickly, and cross-sectional differences in both loan and firm-level credit growth are small.

Using the calibrated model, we compute aggregate non-housing credit and compare it to a counterfactual path that would have prevailed if there had been no financial transmission of the housing boom. We find that the housing boom had a substantial crowding-out effect in its early years: by 2004, it lowered non-housing credit by 7.7% with respect to the counterfactual without financial transmission. However, in the later stages of the boom, this was more than offset by the crowding-in effect. In 2008, when house prices peaked, non-housing credit was 1.8% higher than it would have been without financial transmission. Thus, our findings indicate that the aggregate crowding-out effect of the Spanish housing boom was substantial but transitory.

Our paper is related to a large empirical literature studying the effect of house prices on credit and investment. Several studies provide evidence for a positive effect of house prices on firm credit through a collateral channel (Chaney et al., 2012; Adelino et al., 2015; Bahaj et al., 2020). Our focus is distinct: instead of studying the direct effect of real estate collateral, we analyze the spillovers of a housing boom on non-housing credit arising through the banking system. This issue has been studied by a limited number of papers. Jiménez et al. (2019) argue that the Spanish housing boom allowed banks to increase credit supply through mortgage securitization. Other studies find a negative effect. Chakraborty et al. (2018) show that banks which were more exposed to the US housing boom reduced their loans to firms, as mortgages crowded out corporate credit. Hau and Ouyang (2018) document a similar finding for China. Our analysis suggests that these seemingly conflicting findings may just capture different phases of the financial transmission of housing booms, as crowding-out eventually gives way to crowding-in.[4]

Beyond housing, we speak to a growing literature emphasizing the role of the banking system for the transmission of sectoral shocks. For instance, Dell'Ariccia et al. (2018) argue that falling collateral values

[4]While our crowding-in effect is in line with the results of Jiménez et al. (2019), we argue that it is driven by increases in bank net worth rather than by securitization. Section 3 provides evidence for this claim.

of commercial firms induced banks to increase their real estate lending. Bustos et al. (2020) show that Brazilian banks that were more exposed to regions experiencing an agricultural boom expanded their lending to non-agricultural firms elsewhere.[5] Gilje et al. (2016) and Cortés and Strahan (2017) show other instances of banks transmitting sectoral shocks through space. More generally, there is a vast literature studying the implications of shocks to banks for lending and firm outcomes (Khwaja and Mian, 2008; Paravisini, 2008; Chodorow-Reich, 2014; Amiti and Weinstein, 2018; Huber, 2018). Our contribution with respect to these studies is twofold. First, we emphasize that the direction of financial transmission may change over time. Second, while this literature has been largely empirical, relying on cross-sectional regressions, our paper shows how such estimates can be used to discipline a quantitative model, and thus to assess aggregate magnitudes.

Finally, our paper is related to the literature on the macroeconomic role of housing (see Iacoviello (2010), Guerrieri and Uhlig (2016) or Piazzesi and Schneider (2016) for an overview). Most of this literature analyzes consumption dynamics. We focus instead on firm credit, and analyze how housing booms are transmitted to other sectors. There is also a large literature on the Spanish boom-bust cycle (Fernández-Villaverde et al., 2013; Akin et al., 2014; Santos, 2017a,b). While we build on some of the insights of these studies, we do not aim to provide a unified narrative for Spain's economic development during the period. Instead, we take the housing boom as given and focus on its transmission to the rest of the economy. We also largely abstract from the banking crisis that followed the boom (although we provide some results for it in Appendix A).

The remainder of the paper is organized as follows. Section 2 provides background information and describes our empirical evidence on crowding-out and crowding-in at the loan and firm-level. Section 3 argues that these effects are driven by financial constraints for banks. Section 4 lays out our model, and Section 5 discusses its calibration and our quantitative results. Section 6 concludes.

2 Financial transmission during the Spanish housing boom

2.1 The Spanish housing boom

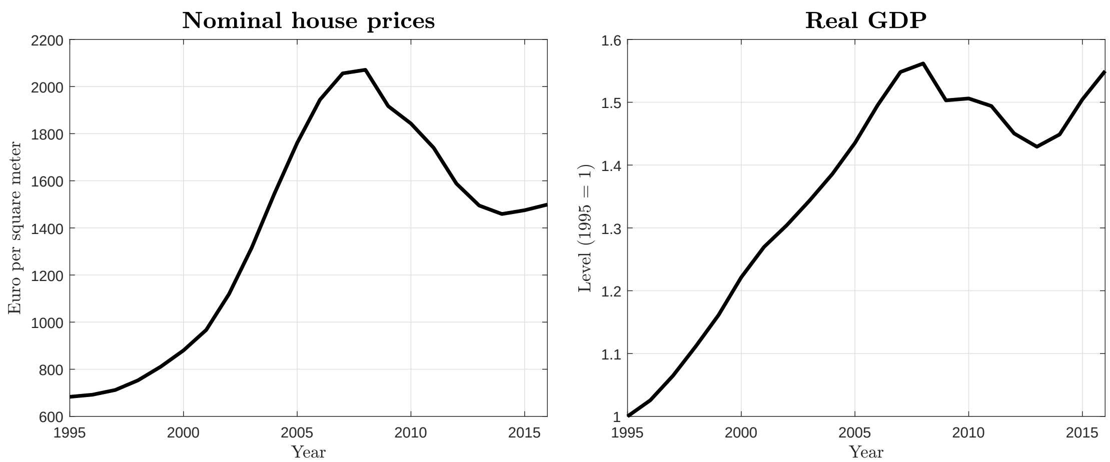
Towards the end of the 1990s, Spanish house prices started rising massively. Between 1995 and 2008, nominal house prices tripled, with the bulk of the increase occurring during the 2000s. At the same time, Spain experienced an economic boom, with real GDP increasing on average by 3.5% per year (see Figure 1).

The origins of the housing boom are still debated. Review articles by Jimeno and Santos (2014) and Santos (2017a) list many contributing factors, including population growth, changes in zoning and land use

[5] While their finding is reminiscent of our crowding-in effect, it is driven by a somewhat different mechanism, namely higher deposits in booming regions (rather than higher bank profits due to the boom).

regulations in 1997 and 1998 (which decentralized and liberalized the granting of housing permits), the decline in interest rates after the creation of the euro, a loosening of bank lending standards (especially in regional banks subject to capture by local politicians), and a speculative bubble on house prices. We do not take a stance on the relative importance of these factors.[6] Instead, we start from the premise that there were some developments in the housing sector which caused a boom, and made it attractive for Spanish banks to increase housing credit. We then study the implications of this boom for non-housing credit.

Figure 1: House prices and real GDP, 1995-2016



Source: Ministry of Construction (house prices), Eurostat (GDP). See Appendix A.1 for further details.

Spain's boom was also a credit boom: as shown in the left panel of Figure 2, the ratio of credit to GDP almost tripled. Credit was mainly provided by domestic banks, which channeled capital inflows to firms and households.[7] Most importantly, the credit boom was driven by housing. Between 2000 and 2007, housing credit (defined as the sum of mortgage credit and credit to construction and real estate firms) increased three times as fast as non-housing credit. As a result, the housing share of total credit increased from 46.6% in 2000 to 61.9% in 2007, as shown in the right panel of Figure 2.[8] This fact provides the main motivation for our paper: we want to understand whether the massive increase in housing credit slowed down credit growth in other sectors, or on the contrary stimulated it.

At this point, it is useful to make two further clarifications. First, non-housing credit during this period was obviously affected by many other factors besides the housing boom (e.g., population growth or falling

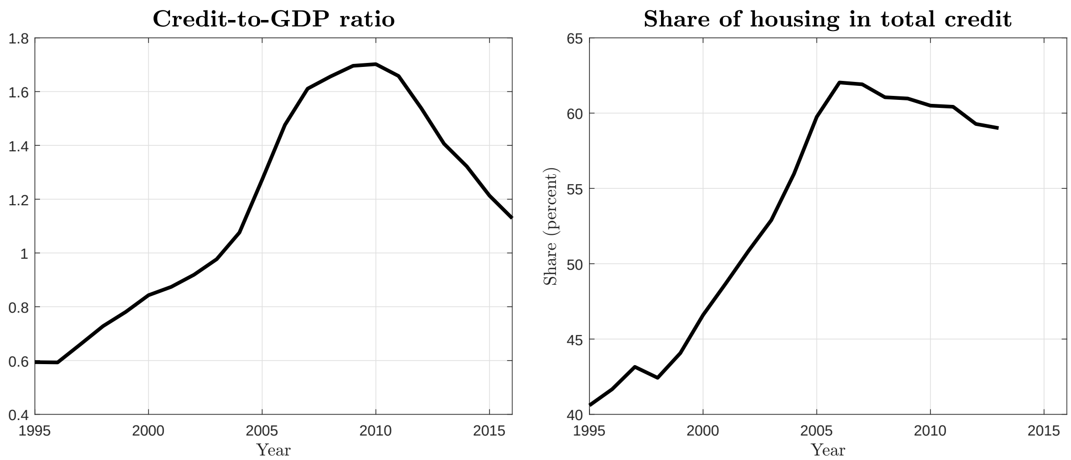
[6]There is an important debate about the origins of the contemporary housing boom in the United States. Violante et al. (2020) summarize the debate, and argue that the most important driver of the boom were beliefs about future house prices.

[7]The external debt of Spanish banks almost tripled between 2002 and 2007 (Statistical bulletin of the Bank of Spain, Series 17.31, https://www.bde.es/webbde/es/estadis/infoest/bolest17.html).

[8]Real housing credit increased by 243% between 2000 and 2007, while real non-housing credit increased by 81%. Note also that firm credit experienced an even larger composition change than overall credit. Credit to construction and real estate firms made up 25.2% of total credit to firms in 2000, but rose to 48.5% in 2007.

interest rates after the creation of the euro).[9] We largely abstract from these alternative factors, and instead try to identify the part of non-housing credit growth due to the financial transmission of the housing boom.

Figure 2: Credit and credit composition, 1995-2016



Source: Eurostat (GDP) and Bank of Spain (credit). See Appendix A.1 for further details.

Second, as indicated in Figures 1 and 2, the Spanish boom eventually ended with a collapse in house prices, credit and GDP. In particular, the accumulation of non-performing housing loans on bank balance sheets triggered a severe banking crisis. We largely abstract from the crisis period in our analysis (although we do provide some empirical results for the period after 2008 in Appendix A.3). The crisis has been extensively studied (see Hernando and Villanueva, 2014; Bentolila et al., 2017 or Santos, 2017b), and our results for it are very much in line with the existing literature. Instead, our main focus is on financial transmission during the housing boom, a topic which has received much less attention.

To study financial transmission, we exploit cross-sectional heterogeneity in the exposure of Spanish banks and firms to the housing boom. The next section describes the data that we use for our analysis.

2.2 Data

Our empirical analysis combines data from two different sources.

1. Credit registry data. The Spanish credit registry (*Central de Información de Riesgos* (CIR) in Spanish) is maintained by the Bank of Spain in its role as primary banking supervisory agency. It contains detailed monthly information on all outstanding loans over 6,000 euros to non-financial firms granted by all

[9]Productivity, on the other hand, was a drag on growth and declined in virtually all sectors (Fernández-Villaverde et al., 2013; Gopinath et al., 2017; García-Santana et al., 2020).

banks operating in Spain since 1984.[10] Given the low reporting threshold, virtually all firms with outstanding bank debt appear in the CIR. For each month, we define a loan by aggregating all outstanding loans for a bank-firm pair. From 1991 onward, the CIR also contains information on banks' balance sheets. This data allow us to compute bank-specific measures of exposure to the housing boom, and to control for bank characteristics such as size, capital, liquidity ratios and default rates.

The Spanish banking system underwent a major consolidation wave in the late 1990s, which makes it difficult to consistently define bank identities over this period. Thus, following Jiménez et al. (2019), we start our analysis in the year 2000. Our raw sample then has 11,870,542 loan-level observations, for 193 different banks and 1,197,038 different firms.

2. Firm-level data. For firm-level outcomes besides credit, we use the Spanish Commercial Registry. This dataset a priori covers the universe of Spanish firms, as they have a legal obligation to deposit their balance sheets at the Registry. For each firm, among other variables, it includes the firm's name, fiscal identifier, sector of activity (4-digit NACE Rev. 2 code), location (5-digit zip code), net operating revenue, material expenditures, number of employees, labor expenditures and total fixed assets. Furthermore, it can be matched to the credit registry. Almunia et al. (2018) describe the dataset in greater detail, and show that it closely matches the movements of aggregate variables such as employment over the period 2003-2013.

Our final sample contains 1,801,955 firms with an average of 993,876 firms per year. This corresponds to around 85-90% of the firms in the non-financial market economy, for all size categories. We mainly focus on non-housing firms, which we define as firms that do not belong to the construction sector (NACE codes 411 to 439) or the real estate sector (NACE codes 681 to 683).

Appendix A.2 provides summary statistics for all variables used in our analysis.

2.3 Empirical strategy

Which effect, if any, did the housing boom have on non-housing credit? To answer this question, we exploit cross-sectional heterogeneity. In particular, we rely on the fact that not all banks were equally exposed to housing when the boom started. Following Jiménez et al. (2019), we define the housing boom exposure of bank b as its ratio of housing loans (residential mortgages and loans to construction and real estate firms) to total loans in the first year of our sample:

$$\mathrm{E}^{b}_{2000} = \frac{\text{Housing loans}^{b}_{2000}}{\text{Total loans}^{b}_{2000}}. \tag{1}$$

[10] We use total credit (the sum of promised and drawn credit lines) and deflate credit levels with the EU KLEMS GDP deflator for the market economy (see https://euklems.eu/). Results are unchanged if we use drawn and/or nominal credit.

This measure captures differences in the business model of banks, or differences in pre-boom ties to the housing sector. As it predates the bulk of the housing boom, it can be considered as exogenous to the extent that the boom was unanticipated.[11] As we discuss later, our results do not change if we use alternative exposure measures, including one based on the geographic location of bank clients.

If the boom had an effect, one would expect it to trigger differences in non-housing credit growth between more and less exposed banks. We investigate this issue in our first series of regressions, using loan-level data.

Loan-level regressions Comparing non-housing credit growth between banks with different levels of exposure is not straightforward. Indeed, banks may have different groups of clients, experiencing different shocks to credit demand. To isolate changes in credit growth due to bank-level (supply) rather than to firm-level (demand) factors, we estimate a series of regressions

$$\text{Credit\_growth}^b_{f,t_0,t_1} = \beta_{t_0,t_1}\mathrm{E}^b_{2000} + \theta_{t_0,t_1}\boldsymbol{X}^b_{2000} + \delta_{t_0,t_1}\boldsymbol{Z}^b_{f,t_0} + \mu_f + u^b_f, \tag{2}$$

where $\text{Credit\_growth}^b_{f,t_0,t_1}$ is the growth rate of the credit of non-housing firm f at bank b between year t_0 and year t_1.[12] $\boldsymbol{X}^b_{2000}$ is a vector of bank controls measured in the year 2000, including the natural logarithm of total assets, capital ratio, liquidity ratio, default rate and a dummy for public savings banks. $\boldsymbol{Z}^b_{f,t_0}$ is a vector of firm-bank controls in year t_0, including the length of the firm-bank relationship and a dummy for past defaults. Most importantly, following Khwaja and Mian (2008), Equation (2) includes firm fixed effects μ_f. This addresses the identification challenge mentioned above by controlling for all factors affecting credit demand. Formally, coefficients in Equation (2) are only identified through differences in the credit growth of the same non-housing firm across different banks.[13] We estimate Equation (2) by Weighted Least Squares (WLS), weighting by credit in year t_0, and cluster standard errors at the bank and at the firm level.

While our estimates control for credit demand, bank exposure to the housing boom may be correlated with other bank characteristics affecting credit supply. To address this issue, we introduce the bank controls $\boldsymbol{X}^b_{2000}$. Furthermore, we will show that our results are robust to an alternative specification of Equation (2) with bank fixed effects, and to using different measures of banks' housing exposure.

[11] While house prices began to rise before 2000, the bulk of their increase was concentrated between 2000 and 2008. We explore developments before 2000 in Appendix A.3.2, using a sample of banks that were unaffected by the merger wave of the late 1990s.

[12] The growth rate is defined as $100 \cdot \left(q^b_{f,t_1} - q^b_{f,t_0}\right)/q^b_{f,t_0}$, where $q^b_{f,t}$ is the yearly average of outstanding credit of firm f at bank b in year t. To reduce the impact of outliers, we winsorize throughout growth rates at +200%.

[13] This identification strategy relies on the assumption that there are no firm-bank specific shocks to credit demand or credit supply. Paravisini et al. (2017) suggest that this assumption may be violated in the presence of bank specialization. However, three points alleviate this concern in our case. First, we include bank-firm covariates in our regressions and thus control for relationship lending to some extent. Second, if bank exposure is exogenous with respect to the omitted factors subsumed in the error term, the β estimates are unbiased even in the presence of bank specialization (see Amiti and Weinstein, 2018). Third, we find a change in the sign of the β estimates during the housing boom, which is difficult to rationalize through bank specialization.

Firm-level regressions Equation (2) identifies differences in the credit growth of a given non-housing firm at more or less exposed banks. However, even if these differences are statistically and economically significant, they may be irrelevant for firm-level credit growth. Indeed, it may be that the housing boom causes only a reallocation of credit across banks, without affecting overall firm credit growth.

To investigate whether the housing boom also had a firm-level effect, we exploit heterogeneity in the links of firms to different banks. We then ask whether the credit growth of a non-housing firm linked to more exposed banks is different from the credit growth of a non-housing firm linked to less exposed banks. To that extent, we define a firm-level exposure measure as

$$\mathrm{E}_{f,t_0} = \sum_b \frac{q^b_{f,t_0}}{q_{f,t_0}} \mathrm{E}^b_{2000}. \tag{3}$$

Thus, the exposure of firm f is a weighted average of the exposure measures of the banks from which the firm borrows in year t_0. Using this statistic, we estimate a series of regressions

$$\text{Credit\_growth}_{f,t_0,t_1} = \gamma_{t_0,t_1}\mathrm{E}_{f,t_0} + \theta_{t_0,t_1}\boldsymbol{X}_{f,t_0} + \upsilon_f, \tag{4}$$

for the sample of non-housing firms. $\text{Credit\_growth}_{f,t_0,t_1}$ stands for credit growth of firm f between year t_0 and year t_1. $\boldsymbol{X}_{f,t_0}$ is a vector of firm controls (including balance sheet items such as the number of employees and total assets, industry-municipality fixed effects and a measure of credit demand shocks computed with loan-level data).[14] We estimate Equation (4) by WLS, weighting by credit in year t_0, and cluster standard errors at the industry-municipality and the main bank level.[15]

This sums up our empirical strategy. The next sections discuss our results, starting at the loan level.

2.4 Loan-level regression results

Baseline results We first estimate Equation (2) for year-on-year credit growth rates between 2001 and 2008 (that is, we estimate the regression seven times, for the periods 2001-2002, 2002-2003, etc.). Figure 3 plots the estimated β coefficients for these regressions. As emphasized above, firm fixed effects imply that we compare, for the same non-housing firm, differences in credit growth across banks with different exposure.

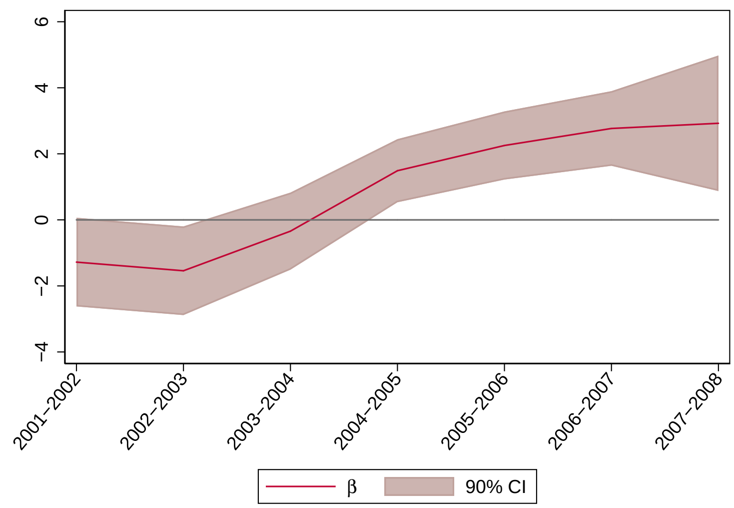
Figure 3 illustrates our first main result: the effect of bank exposure on non-housing credit growth is negative in the first years and positive in the last years of the housing boom. In other words, the housing

[14] For further details on this measure of credit demand, see Cingano et al. (2016) and Alfaro et al. (2018).

[15] For every firm, the main bank is defined as the bank from which the firm borrows most in year t_0.

boom appears to have had a crowding-out effect on the supply of credit to non-housing firms during its first years. However, this effect gradually disappeared, and gave way to a crowding-in effect. According to Figure 3, crowding-out dominated between 2001 and 2003. In 2003-2004, exposure had essentially no effect, and then crowding-in took over between 2004 and 2008.

Figure 3: Bank exposure and loan-level non-housing credit growth, year-on-year estimates



Notes: This plot shows the WLS estimates of β_{t_0,t_1} in Equation (2), estimated for a sample of non-housing firms.

In order to streamline the exposition and to smooth out noise in year-to-year credit growth rates, we henceforth focus on two subperiods, grouping the years with positive and negative estimates shown in Figure 3. Thus, we consider the periods 2001-2003 and 2004-2007 and reestimate Equation (2) for these two longer subperiods.[16] Columns (1) and (2) of Table 1 report the results from these regressions.

In line with Figure 3, we find that crowding-out dominated between 2001 and 2003: for the average non-housing firm, credit growth was lower at more exposed banks. Precisely, a one standard deviation increase in bank exposure reduced credit growth by 2.29 percentage points (around 19% of the average growth rate in this period). Between 2004 and 2007, instead, crowding-in dominated, and a one standard deviation increase in bank exposure raised credit growth by 4.82 percentage points (around 28% of the average growth rate in this period). Thus, cross-sectional effects appear to be both statistically and economically significant.

In columns (3)-(4) of Table 1, we substitute firm fixed effects by a rich set of firm controls and industry-municipality fixed effects, as in Bentolila et al. (2017). Firm controls include total assets, number of employees,

[16] Crowding-in estimates would be even larger if we considered the period 2004-2008. However, as the Great Recession started in this year, one may be worried that exposed banks anticipated the crisis and diversified towards non-housing loans. Although we do not find empirical evidence for this (see Section 3), we focus on the period 2004-2007 in order to be conservative.

Table 1: Bank exposure and loan-level non-housing credit growth, baseline results

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	-2.29^{**}	4.82^{***}	-2.05^{**}	4.52^{***}	-2.21^{**}	4.62^{***}
(s.e.)	(0.92)	(1.09)	(0.90)	(1.05)	(0.96)	(1.11)
Average dep. variable	11.80	17.48	15.94	20.90	17.01	21.98
Firm fixed effects	YES	YES	NO	NO	NO	NO
Firm controls	NO	NO	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind.×munic. FE	NO	NO	YES	YES	YES	YES
Balance-sheet data	NO	NO	YES	YES	YES	YES
R-sq	0.48	0.50	0.34	0.34	0.35	0.35
# observations	276,782	247,022	243,329	247,326	202,766	201,431
# firms	97,322	85,825	124,489	130,400	83,926	84,505
# banks	135	129	135	129	135	129

Notes: Regressions are based on Equation (2), estimated by WLS. Bank exposure (E^b_{2000}) is measured by the share of housing loans in total bank loans in 2000, and normalized to have zero mean and unit variance. Columns (1)-(2) and (5)-(6) are estimated for a sample of firms which borrow from at least two banks (multibank firms). Bank controls include the natural logarithm of total assets, capital ratio, liquidity ratio, default rate and a dummy for public savings banks. Firm-bank controls include the length of firm-bank relationship in months and a dummy for past defaults. Firm controls are total assets, number of employees, own funds over total assets, return on assets, a dummy for firms younger than three years, and a dummy for exporters. Standard errors multi-clustered at the bank and firm level are shown in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

own funds over total assets, return on assets, a dummy for firms younger than three years, and a dummy for exporters. This allows us to consider all non-housing firms rather than just those borrowing from at least two banks (multibank firms), with the firm-level variables controlling for credit demand. Finally, columns (5)-(6) report estimates from the specification with firm controls but using the same sample of multibank firms as in columns (1)-(2). In both cases, the results remain virtually unchanged.

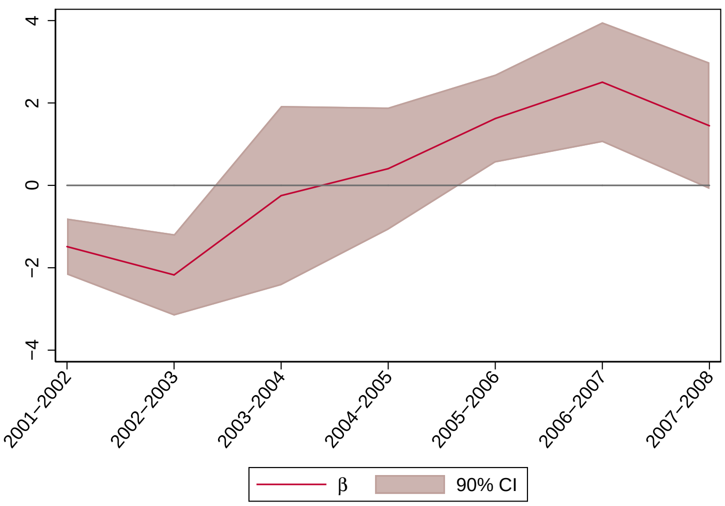
Controlling for collateral effects A first potential concern regarding the results reported so far is that they may be contaminated by collateral effects. Indeed, if real estate is an important source of collateral for non-housing firms (see Chaney et al., 2012), and if more exposed banks are better able to lend against this collateral, then our coefficients may be biased upwards due to the fact that non-housing firms with real estate collateral increased their borrowing from more exposed banks during the boom. To mitigate these concerns, Figure 4 and Table 2 reproduce our results for a restricted sample of non-collateralized "cash-flow" loans.[17]

The pattern of crowding-out and crowding-in depicted in Figure 4 is essentially the same as in Figure 3.

[17] Ivashina et al. (2020) provide an in-depth discussion of the properties of different loan types available in credit registries.

Furthermore, Table 2 shows that the estimated effects of bank exposure by subperiods and the basic patterns of statistical and economic significance reported in Table 1 are preserved. Note, however, that the crowding-out effect is now somewhat stronger and the crowding-in effect somewhat weaker. That is, estimates are shifted down with respect to the baseline, which is exactly what one would expect in the presence of collateral effects. As differences are small, we will always report results based on the sample of all loans in the remainder of the paper. However, Appendix A.3 shows some additional results for the sample of cash-flow loans.

Figure 4: Bank exposure and loan-level non-housing credit growth, year-on-year estimates (cashflow loans)



Notes: This plot shows the WLS estimates of β_{t_0,t_1} in Equation (2), estimated for a sample of cash-flow loans.

Other robustness checks Another potential concern regarding our results is that exposure to the housing boom may be correlated with some unobserved bank characteristics, and that it is the latter which are truly driving our results. Although Tables 1 and 2 include bank-level controls, these may not capture all relevant bank characteristics. Therefore, in Appendix A.3.1, we estimate a pooled version of Equation (2) for our entire sample, including bank fixed effects (capturing all time-invariant differences across banks) and a series of interactions of bank exposure with subperiod dummies. This does not affect our results.

We also consider many further robustness checks and extensions. In Appendix A.3.2, we use a sample of banks that were unaffected by mergers to show that exposure had no effect on credit growth before 2000. In Appendix A.3.3, we explore two alternative measures of bank exposure to the housing boom: a measure based on the geography of bank activity (assuming that banks are more exposed if they operate in municipalities prone to stronger housing booms) and the ratio of banks' mortgage-backed credit over total credit in 2000.

Table 2: Bank exposure and loan-level non-housing credit growth: cash-flow loans

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	-3.00^{***}	3.33^{**}	-2.70^{***}	3.21^{*}	-3.00^{***}	3.54^{*}
(s.e.)	(0.70)	(1.61)	(0.79)	(1.91)	(0.84)	(1.99)
Average dep. variable	5.37	16.10	9.27	19.88	10.12	20.76
Firm fixed effects	YES	YES	NO	NO	NO	NO
Firm controls	NO	NO	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind.×munic. FE	NO	NO	YES	YES	YES	YES
Balance-sheet data	NO	NO	YES	YES	YES	YES
R-sq	0.49	0.54	0.39	0.41	0.40	0.42
# observations	139,904	131,887	129,355	144,945	96,283	108,172
# firms	52,442	48,756	75,395	85,679	42,323	48,906
# banks	133	128	135	128	134	128

Notes: See Table 1. Regressions are estimated on a sample of cash-flow loans. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

In both cases, our results are preserved. Appendix A.3.4 shows that our estimates are robust to the exclusion of public savings banks (*cajas*, operating under a different institutional framework than commercial banks) from the sample. Appendix A.3.5 studies geographical clustering, as the housing boom was not uniform across Spain. If non-housing firms relied more on local banks to satisfy higher credit demand, our baseline estimates could be biased upwards. To address this, we consider subsamples of nationally operating banks and of non-housing firms located in provinces with large or small housing booms. In all three samples, our results are unchanged. Appendix A.3.6 analyzes the creation and termination of loan relationships during the boom. Finally, Appendix A.3.7 explores the role of bank exposure during the banking crisis that followed the housing boom, and shows that non-housing credit contracted more at more exposed banks.

Summing up, the evidence presented so far suggests that non-housing firms had slower credit growth at more exposed banks in the first years of the housing boom, and faster credit growth at more exposed banks in the last years of the housing boom. However, did this have any effect on firm-level credit growth?

2.5 Firm-level regression results

Table 3 presents the estimated coefficients for our firm-level regression specified in Equation (4), for the two usual subperiods. Columns (1)-(2) refer to the sample of all non-housing firms, while columns (3)-(4) are based on a sample of multibank firms.

In both cases, we find strong evidence that the successive crowding-out and crowding-in effects also operated at the firm level. Magnitudes remain economically significant, although estimates are lower than those at the bank-firm level shown in Table 1 (when normalizing point estimates by the mean of the dependent variable). For the sample of all firms, the 2001-2003 crowding-out effect of a one-standard-deviation increase in exposure represented a fall in credit growth of approximately 13% of the sample average. Crowding-in during 2004-2007 was of a similar magnitude, representing approximately 11% of the sample average credit growth. This suggests that firms were partially able to compensate for the effect of exposure by switching to other banks, but could not fully undo it.

Table 3: Boom exposure and credit growth at the firm level

	All firms		Multibank firms	
	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)
Firm exposure (E_{f,t_0})	-2.89^{***}	3.34^{**}	-3.50^{***}	3.09^{*}
(s.e.)	(0.96)	(1.53)	(1.16)	(1.65)
Average dep. variable	23.05	31.07	32.94	43.39
Firm controls	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES
Industry $\times$ municipality FE	YES	YES	YES	YES
Balance-sheet data	YES	YES	YES	YES
R-sq	0.57	0.55	0.58	0.55
# observations	82,344	96,734	48,944	54,950

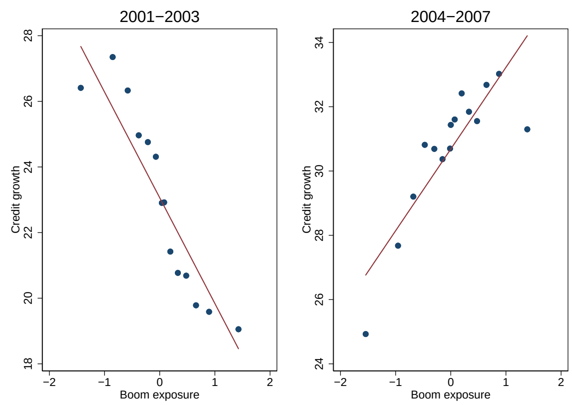
Notes: All regressions are based on Equation (4). Firm exposure is standardized to have zero mean and unit variance. Firm controls are total assets, number of employees, own funds over total assets, return on assets, a dummy for firms younger than three years, a dummy for exporters, and a measure of firm credit demand. Standard errors multi-clustered at the main bank and industry-municipality level in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

We also perform robustness checks for our firm-level results. Appendix A.3.8 shows that we obtain similar results when considering our alternative geographical measure of boom exposure. Figure 5 depicts firm-level results graphically, by plotting the partial correlation of firms' credit growth and their boom exposure on a binned scatterplot. The correlation is negative for the period 2001-2003, but positive for the period 2004-2007.

Finally, we analyze whether changes in firm credit had any implications for real outcomes. To do so, we estimate Equation (4) again, using value added growth or investment growth as the dependent variable.[18] Columns (1)-(2) of Table 4 show that firm boom exposure also had an effect on value added growth: the

[18]Value added is defined as the difference between sales (net operating revenue) and material expenditures. Results are unchanged if we consider sales. Our measure of firm investment is the change in the book value of fixed assets.

Figure 5: Boom exposure and credit growth for non-housing firms



Notes: To generate these plots, we group firms by boom exposure into equally-sized bins. We then compute the mean of exposure and credit growth (after controlling for the regressors and fixed effects included in Table 3) in each bin.

value added of non-housing firms linked to more exposed banks grew less than that of their peers in the 2001-2003 period, but more in the 2004-2007 period. This pattern also emerges when we consider investment growth, as shown in columns (3)-(4), even though results are statistically weaker.

Table 4: Boom exposure and real outcomes at the firm level

	Dep. variable: VA growth		Dep. variable: Investment	
	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)
Firm exposure (E_{f,t_0})	-0.38^{**}	0.94^{*}	-0.32^{*}	1.89^{*}
(s.e.)	(0.15)	(0.53)	(0.18)	(0.97)
Average dep. variable	8.86	15.82	6.02	10.86
Firm controls	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES
Industry × municipality FE	YES	YES	YES	YES
Balance-sheet data	YES	YES	YES	YES
R-sq	0.32	0.52	0.27	0.64
# observations	94,105	112,482	99,271	113,352

Notes: All regressions are based on Equation (4), but consider value added or investment growth as the dependent variable. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Real effects are smaller and less significant than credit effects, but still relevant. For instance, a one standard deviation increase in boom exposure results in a 0.38 percentage point reduction in value added growth between 2001 and 2003, and a 0.94 percentage point increase in value added growth between 2004 and 2007.

3 What drives financial transmission?

3.1 A unified narrative

Our empirical results show that the financial transmission of housing booms changes over time: booms initially reduce non-housing credit growth, but eventually, they raise it again. This finding reconciles seemingly conflicting results in the literature. For instance, Chakraborty et al. (2018) show that in the United States, banks that were more exposed to house price appreciations between 1998 and 2006 reduced corporate credit.[19] Hau and Ouyang (2018) find a similar crowding-out effect for real estate booms in China. Instead, Jiménez et al. (2019) document a crowding-in effect of the Spanish housing boom between 2004 and 2007, which they attribute to securitization.[20] Similarly, Bustos et al. (2020) show that the agricultural boom generated by the introduction of transgenic soy in Brazil had a crowding-in effect on manufacturing credit. Our results suggest that these crowding-out and crowding-in effects are not incompatible, but rather that they operate at different time horizons.

However, what is the economic reason for crowding-out, and why does it eventually give way to crowding-in? Previous papers emphasizing the crowding-out effect explain it by some form of financial constraint, which makes it costly for banks to raise external capital.[21] Therefore, banks respond to higher credit demand from a booming sector by reducing credit to other sectors. Note that this narrative does not require banks to be literally constrained by regulations or markets during the boom: even the prospect of being constrained in the future may suffice to make them wary of raising external capital.[22]

Whatever the underlying nature of bank constraints, we point out that they also have dynamic implications that have been overlooked. Indeed, the booming sector raises interest rates and loan volumes, and therefore eventually increases bank profits and net worth. However, if banks are constrained (or potentially constrained), higher net worth is not neutral, but allows banks to increase their credit supply to all sectors of the economy. Thus, we conjecture that the crowding-out and crowding-in effects of a housing boom may be due to the exact same mechanism. In the next sections, we provide evidence for this conjecture.

[19]Their analysis restricts the coefficient in a regression of credit growth on housing exposure to be time-invariant. Therefore, they estimate the average effect of exposure, ignoring potential changes over time.

[20]We use a similar dataset and the same measure of bank exposure as Jiménez et al. (2019). Their results are similar to our estimates reported in Table 1, i.e., a negative effect of bank exposure on the credit growth of non-housing firms for the period 2001-2004 and a positive effect for the period 2004-2007. As they find that the initial negative effect is not statistically significant (see column 8 of Table 3 in their paper), they disregard it. However, it is worth emphasizing that, apart from differences in the sample selection, their baseline specification differs from ours by neither including bank nor bank-firm controls.

[21]Chakraborty et al. (2018) write: "*The premise underlying this crowding-out behavior is that banks are constrained in raising new capital or selling their loans, and so when highly profitable lending opportunities arise in one sector (mortgage lending), they choose to pursue them by cutting their lending in another sector (commercial lending)*" (P. 2807).

[22]For instance, Begenau et al. (2019) show that banks in the United States tend to stabilize their leverage around a "target" level (so that bank leverage was roughly constant during the 2000-2007 housing boom), and provide a model in which potentially binding regulatory and market constraints make such a behavior optimal.

3.2 Evidence for banks' financial constraints

The empirical banking literature has long recognized that banks may face financial constraints, and devised a number of tests to identify them. Here, we follow Chakraborty et al. (2018) and assume that banks with high leverage ratios (i.e., low ratios of net worth to assets) are more constrained and should therefore be more sensitive to shocks. Thus, we split our sample according to banks' leverage ratios, considering banks in the lowest quartile of leverage ratios as unconstrained, and the remaining banks as constrained.[23] We then estimate our baseline loan-level specification, given by Equation (2), for both subsamples.

Table 5: Bank exposure and loan-level credit growth: constrained and unconstrained banks

	2001-2003		2004-2007	
	Constrained	Unconstrained	Constrained	Unconstrained
	(1)	(2)	(3)	(4)
Bank exposure (E^b_{2000})	-2.38^{***}	0.45	3.19^{***}	1.52
(s.e.)	(0.89)	(2.67)	(1.06)	(3.53)
Average dep. variable	11.07	14.34	17.97	18.22
Firm fixed effects	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES
R-sq	0.49	0.64	0.52	0.63
# observations	200,929	12,603	188,286	10,181
# firms	73,723	6,061	68,052	4,870
# banks	67	22	65	21

Notes: All regressions are based on Equation (2). Unconstrained banks are banks in the lowest quartile of the bank leverage ratio in the first year of the period, constrained banks are all others. Standard errors multi-clustered at the bank and firm level are shown in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 5 shows that the results from these regressions are in line with the narrative outlined above. In particular, the crowding-out effect between 2001 and 2003 is only present among constrained banks. Likewise, our estimates suggest that constraints are also key for the crowding-in effect between 2004 and 2007, which is larger for constrained banks (and insignificant for unconstrained ones).

However, is the link between crowding-in and exposure really driven by the effect of exposure on bank net worth? To address this question, Table 6 reports the results of a regression of the growth rate of bank net worth on our measure of housing exposure. During 2001-2003, there is almost no correlation between exposure and net worth growth. However, in the peak period of the housing boom, during 2004-2007, net worth growth is positively and significantly correlated with exposure. This suggests that the effect of the

[23] Leverage is defined as the ratio of total assets to net worth (the sum of capital, reserves and profits at book values).

boom on net worth takes time to materialize (which is in line with our narrative, as it implies that the crowding-in effect is not immediate either). Interestingly, the net worth effect appears to be driven by constrained banks, as shown in Column (5).

Table 6: Bank exposure and net worth growth

Dep. variable is growth in bank net worth						
	2001-2003			2004-2007		
	All banks	Constr.	Unconstr.	All banks	Constr.	Unconstr.
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	0.07	0.04	-0.16	0.63^{***}	0.64^{***}	-0.04
(s.e.)	(0.09)	(0.11)	(0.14)	(0.12)	(0.14)	(0.27)
R-sq	0.45	0.46	0.73	0.62	0.62	0.62
# observations	140	116	24	136	113	23

Notes: This table reports the results of the regression $\text{Net\_worth\_growth}^b_{t_0,t_1} = \gamma_{t_0,t_1} \mathrm{E}^b_{2000} + \theta_{t_0,t_1} \boldsymbol{X}^b_{2000} + v_b$. $\text{Net\_worth\_growth}^b_{t_0,t_1}$ is the growth rate of net worth (the sum of capital, reserves and profits at book values) between year t_0 and year t_1. $\boldsymbol{X}^b_{2000}$ is a vector of bank controls, listed in the notes to Table 1. Unconstrained banks are banks in the lowest quartile of the bank leverage ratio in the first year of the period, constrained banks are all others. Standard errors in parentheses. $^+$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

The results reported in Tables 5 and 6 support the view that financial constraints are key for the crowding-out effect of housing booms, in line with the literature. Moreover, they suggest that constraints also drive the crowding-in effect: the boom raises bank net worth, and as banks are constrained, higher net worth stimulates credit supply. However, while this explanation is intuitive and consistent with the data, it is in principle possible that crowding-in is driven by other factors. In the next section, we discuss alternative explanations and argue that they are not in line with the empirical evidence.

3.3 Alternative explanations for the crowding-in effect

A first alternative interpretation of our crowding-in estimates is that they reflect a slowdown of housing credit demand towards the end of the boom. At first glance, this seems unlikely, as house prices and the share of housing in total credit were still rising until 2007. Nonetheless, we test this possibility by estimating a variation of Equation (2). We consider a sample of both non-housing and housing firms, and instead of firm fixed effects, we include bank fixed effects, a set of firm controls and a dummy variable for housing firms. Bank fixed effects control for bank supply factors, and therefore the housing dummy should capture housing credit demand (that is not accounted for by the other firm controls).

Columns (1)-(2) of Table 7 show that our estimates for the coefficient of the housing dummy are positive and significant for the periods 2001-2003 and 2004-2007: all else equal, a housing firm obtained more credit than a non-housing firm from the same bank, which can be interpreted as evidence for higher housing credit demand. Crucially, our estimate is higher in the later period, indicating that housing credit demand did not fall in the last years of the boom. Results are unchanged for a sample of multibank firms.

Table 7: Alternative explanations for crowding-in: a drop in housing credit demand?

	All firms		Multibank firms	
	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)
Housing dummy	5.33***	9.12***	4.67***	9.71***
(s.e.)	(0.65)	(1.25)	(0.75)	(1.28)
Average dep. variable	16.71	22.80	17.90	24.26
Bank fixed effects	YES	YES	YES	YES
Firm fixed effects	NO	NO	NO	NO
Firm controls	YES	YES	YES	YES
Bank controls	NO	NO	NO	NO
Firm-bank controls	YES	YES	YES	YES
Industry × municipality FE	YES	YES	YES	YES
Balance-sheet data	YES	YES	YES	YES
R-sq	0.06	0.09	0.06	0.09
# observations	283,361	388,902	211,190	287,200
# firms	170,362	230,651	98,191	128,951
# banks	154	154	156	150

Notes: The table reports estimates from Equation (2) - substituting firm fixed effects with bank fixed effects, firm controls and a dummy for housing firms - for a sample of housing and non-housing firms. Firm controls are listed in Table 3. Standard errors multi-clustered at the bank and firm level in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

A second alternative explanation of our crowding-in estimates is that they reflect a fall in the relative supply of housing credit. Indeed, more exposed banks may have chosen to reduce their exposure to housing in the late stages of the boom, recognizing the risks of their position. Again, given the evolution of housing prices and housing credit until 2007 (and indeed until 2008), this explanation seems unlikely. To test it more formally, we estimate Equation (2) for a sample of housing firms. In case more exposed banks tried to diversify away from housing at the end of the boom, we would expect a negative coefficient of bank exposure for this period. Table 8 shows that this is not the case. Just as in our baseline results, the coefficient on bank exposure is positive and significant for the period 2004-2007, implying that credit to housing firms grew more, and not less, at more exposed banks. This is consistent with the narrative outlined above (which implies that the higher net worth of exposed banks increased credit supply to all sectors), but it is hard to square with the idea that more exposed banks tried to diversify away from housing.

Table 8: Alternative explanations for crowding-in: Diversification?

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	0.55	4.58***	0.21	3.14*	0.15	3.08*
(s.e.)	(0.99)	(1.28)	(0.75)	(1.68)	(0.83)	(1.72)
Average dep. variable	20.95	30.85	20.53	27.23	21.47	28.98
Firm fixed effects	YES	YES	YES	YES	YES	YES
Firm controls	NO	NO	NO	NO	NO	NO
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind.×munic. FE	NO	NO	NO	NO	NO	NO
Balance-sheet data	NO	NO	NO	NO	NO	NO
R-sq	0.52	0.56	0.31	0.31	0.32	0.33
# observations	87,349	103,087	83,399	109,086	68,295	86,358
# firms	32,084	37,653	46,680	63,281	31,576	40,553
# banks	134	129	135	128	135	128

Notes: The table reports estimates from Equation (2) for a sample of housing firms. See notes to Table 1 for further details. $^+$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A third alternative interpretation of our crowding-in estimates is that they reflect securitization, as argued by Jiménez et al. (2019). Indeed, banks that are more exposed to housing are likely to have more real estate assets to securitize, and thus higher liquidity and credit supply. We deal with this concern through two exercises. First, we analyze the strength of the crowding-in effect in subsamples of banks which are more or less active in the securitization market, measured in terms of the issuance of asset backed securities (ABS) and covered bonds over total assets. Table 9 shows that crowding-in is positive and significant regardless of securitization activity. Indeed, if anything, the effect is slightly stronger for banks with low securitization activity. This holds regardless of whether we split the sample based on the level of securitization in 2004, its level in 2007, or the increase in securitization between 2004 and 2007.

Second, we compute a measure of credit supply shocks. Following Amiti and Weinstein (2018), we regress non-housing credit growth at the bank-firm level on a set of bank and firm fixed effects (for our two subperiods) and interpret bank fixed effects as credit supply shocks. We then regress the estimated supply shocks on our measure of boom exposure, a measure of securitization and the standard set of bank controls. Appendix A.3.10 shows the results of these regressions. They replicate the crowding-out and

Table 9: Alternative explanations for crowding-in: Securitization?

	Level 2004		Level 2007		Change 2004-2007	
	High secur.	Low secur.	High secur.	Low secur.	High secur.	Low secur.
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	3.87***	5.96**	4.78***	5.08**	2.33$^+$	6.37***
(s.e.)	(1.15)	(2.68)	(1.07)	(2.38)	(1.48)	(1.87)
Average dep. variable	17.80	15.75	17.54	20.06	17.49	18.68
Firm fixed effects	YES	YES	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
R-sq	0.52	0.62	0.53	0.64	0.57	0.57
# observations	200,891	5,908	177,927	16,361	69,588	106,848
# firms	72,495	2,851	65,661	7,822	28,557	42,635
# banks	64	62	64	63	64	65

Notes: The table reports estimates from Equation (2) for the period 2004-2007 for different subsamples. Securitization is measured by the ratio of ABS and covered bonds over total assets. High (low) securitization refers to banks above (below) the median of each securitization measure. The change in securitization between 2004 and 2007 is the percentage increase in the securitization ratio. $^+$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

crowding-in patterns shown above: housing boom exposure reduces non-housing credit supply in 2001-2003, but stimulates it during 2004-2007. In contrast, securitization is not or only marginally significant during the period 2004-2007. Overall, these results suggest that our crowding-in effect is not driven by securitization.[24]

Summing up, our empirical results suggest that banks transmitted the Spanish housing boom to the non-housing sector: initially, non-housing credit was crowded out, but eventually, this effect was undone and non-housing credit was crowded in. We have also shown that these effects can be rationalized by financial constraints for banks. We now develop a macroeconomic model that formalizes this view, and use it to assess the aggregate importance of crowding-out and crowding-in.

4 A two-sector model of housing booms and financial transmission

4.1 Assumptions

Agents, preferences and technologies Time is discrete ($t \in \mathbb{N}$), and the economy is populated by generations of agents that live for two periods. Agents are risk-neutral and derive utility from their old-age consumption of a housing good H and a non-housing good N. The utility of agent i born in period t is

[24]Appendix A.3.10 contains one additional robustness test, introducing securitization as a control variable in our regression of net worth growth on exposure shown in Table 6. Securitization is not significantly correlated with net worth growth.

$$U_t^i = \mathbb{E}_t \left(C_{N,t+1}^i + \xi_{t+1} C_{H,t+1}^i \right), \tag{5}$$

where $C_{j,t+1}^i$ denotes agent i's consumption of good $j \in \{N, H\}$ in period $t+1$. The non-housing good is tradable and used as a numeraire, so that we normalize its price to 1. The housing good is instead non-tradable and its price, denoted by $P_{H,t}$, is determined endogenously. We assume that the weight of housing in the utility function, ξ_{t+1}, follows an exogenous stochastic process. This stochastic process is the main driving force in our model, and we model a housing boom as a succession of positive shocks to ξ_{t+1}.[25]

Both goods are produced by perfectly competitive firms with the production function

$$Y_{j,t} = A_{j,t} \left(K_{j,t} \right)^{\alpha_j} \left(L_{j,t} \right)^{1-\alpha_j}, \text{ with } \alpha_j \in (0,1), \tag{6}$$

where $K_{j,t}$ stands for the capital stock and $L_{j,t}$ for the labor employed by sector j at time t. We assume that both capital and labor are sector-specific. Thus, in our model, a housing boom has no spillover effects through factor or goods markets: it only affects the non-housing sector because of financial transmission (i.e., because of spillovers through the credit market).

In each sector, labor is supplied inelastically by workers who work during youth and consume during old age. The economy's total labor endowment satisfies $L_{H,t} = L_{N,t} = 1$ in every period t. In each sector, the capital stock is a constant elasticity of substitution (CES) aggregate of a continuum of heterogeneous capital goods, holding

$$K_{j,t} = \left(\int_0^1 \left(k_{j,t}(\omega) \right)^{\frac{\varepsilon_j - 1}{\varepsilon_j}} d\omega \right)^{\frac{\varepsilon_j}{\varepsilon_j - 1}}, \tag{7}$$

where $k_{j,t}(\omega)$ is the amount of capital good ω of sector j available at time t, and $\varepsilon_j > 0$ is the elasticity of substitution across different capital goods in sector j.

Capital goods are produced by heterogeneous entrepreneurs. The generation of entrepreneurs born in period t invests during their youth in order to generate capital during their old age (i.e., in period $t+1$) and rent it out to final producers. To invest, entrepreneurs need credit, supplied by bankers. Entrepreneurs and bankers are the crucial actors of our model, and their behavior is described in the next sections.

Investment and credit demand Each generation of agents contains a continuum of heterogeneous entrepreneurs. An individual entrepreneur is characterized by her ability to produce a certain capital good ω of

[25] In an earlier version, we considered booms generated by rational bubbles and found qualitatively similar results.

sector j: for each unit of the tradable good invested in period t, she can generate one unit of her capital good in period $t+1$. We assume that there is a continuum of entrepreneurs of each "type" $(j,\,\omega)$, and without loss of generality, we focus on the representative entrepreneur of each type.

Entrepreneurs are born without resources. To invest, they therefore need to borrow from banks (which will be described later). We assume that young entrepreneurs trade state-contingent credit contracts with banks, promising them a fraction of their future income as a repayment for their credit. We refer to the expected return on such a credit contract as the interest rate charged by the bank. Note, moreover, that entrepreneurs in our model correspond to firms in the data. Thus, we henceforth refer to them as firms.

The capital produced by firm ω in sector j is given by

$$k_{j,t+1}\left(\omega\right)=\left(\sum_{b=1}^{B}\left(\pi_{j}^{b}\left(\omega\right)\right)^{\frac{1}{\eta_j}}\left(q_{j,t}^{b}\left(\omega\right)\right)^{\frac{\eta_j}{\eta_j-1}}\right)^{\frac{\eta_j}{\eta_j-1}}, \tag{8}$$

where $q_{j,t}^{b}\left(\omega\right)$ is the amount of credit that the firm receives from bank b in period t, and η_j is the elasticity of substitution of credit across different banks for firms of sector j. $\pi_{j}^{b}\left(\omega\right)$ are weights governing the preferences of each firm across the B banks in the economy. We normalize weights such that $\sum_{b=1}^{B}\pi_{j,t}^{b}\left(\omega\right)=1$. Equation (8) implies that credits from different banks are imperfect substitutes. This is a common way of modeling the empirical reality of firms borrowing from more than one bank (Paravisini et al., 2017; Herreño, 2020).

Firms operate under perfect competition, taking as given the expected price of their capital good in period $t+1$ and the interest rates charged by banks. We also assume that capital depreciates fully in production.

Credit supply Our small open economy is embedded in an International Financial Market (IFM), which is risk-neutral and willing to borrow or lend at an exogenous interest rate R^{*}. However, only bankers have the know-how to collect payments from domestic firms, making them necessary intermediaries between these firms and the IFM.

Each generation of bankers is composed of B different types. Without loss of generality, we focus on the representative banker of each type. During youth, the representative banker of type b receives a fraction $\phi\in(0,1)$ of the profits of the old generation of type-b bankers. Thus, bank profits are persistent: instead of being fully consumed by old bankers, a fraction of them is transferred to young bankers and forms their net worth.[26] Young bankers use this net worth, as well as additional resources borrowed from the IFM, to

[26]We could microfound this income by assuming that old bankers need to hire young bankers to perform some productive services (e.g., loan collection) against a fraction ϕ of their profits (see Song et al., 2011), or that old bankers leave bequests. More generally, our assumption is a simple way to generate persistence in an economy in which bankers live only two periods. Alternatively, we could assume that bankers live longer than two periods and die stochastically (see Gertler and Karadi, 2011).

extend credit to firms. When they are old, they collect repayments from firms, repay the IFM, and consume. Note that all credit contracts are denominated in the tradable good. Furthermore, bankers operate under perfect competition. That is, they take all interest rates (including the one of their own type) as given.

While bankers can borrow from the IFM at interest rate R^*, they face a financial constraint that limits their leverage (see Moll, 2014): the banker of type b cannot borrow more than a multiple $\lambda - 1$ of her net worth, where $\lambda > 1$ is a constant parameter. This is a simple way to capture the fact that it is costly for banks to raise outside funds beyond a certain threshold. However, it is not out of line with the data: indeed, the aggregate leverage ratio of the Spanish banking system hardly increased during the housing boom.[27] Crucially, we will focus on equilibria where the leverage constraint is always binding (i.e., in which the equilibrium interest rate of each bank is higher than the international interest rate R^*).

This completes our model's assumptions. In the next section, we solve for the equilibrium.

4.2 Equilibrium

Final and capital goods prices Market clearing for the non-tradable housing good implies $C_{H,t} = Y_{H,t}$ in every period t. Throughout, we focus on equilibria in which domestic agents consume both goods, implying $P_{H,t} = \xi_t$. A necessary and sufficient condition for this is that the income of old agents in period t exceeds $\xi_t Y_{H,t}$. We impose parameter restrictions ensuring that this always holds (see Appendix B.1.2).

In each sector j, the cost minimization problem of the final goods firms implies that in period t, demand for capital of type ω holds

$$k_{j,t}(\omega) = \left(\frac{p^K_{j,t}(\omega)}{P^K_{j,t}}\right)^{-\varepsilon_j} K_{j,t}, \tag{9}$$

where $p^K_{j,t}(\omega)$ is the price of capital good ω and $P^K_{j,t} \equiv \left(\int_0^1 \left(p^K_{j,t}(\omega)\right)^{1-\varepsilon_j} d\omega\right)^{\frac{1}{1-\varepsilon_j}}$ is the price of one unit of sector-j capital.

Cost minimization and perfect competition imply that capital is paid a fraction α_j of each sector's final sales:

$$P^K_{j,t} K_{j,t} = \alpha_j A_{j,t} P_{j,t} K^{\alpha_j}_{j,t}. \tag{10}$$

Combining Equations (9) and (10), we get that the price of each capital good in period t is

$$p^K_{j,t}(\omega) = \alpha_j A_{j,t} P_{j,t} K^{\alpha_j - 1}_{j,t} \left(\frac{k_{j,t}(\omega)}{K_{j,t}}\right)^{-\frac{1}{\varepsilon_j}}. \tag{11}$$

[27] In our data, the aggregate ratio of total assets to net worth increased by only 10% between 2000 and 2007 (from 15.08 to 16.55). Bedayo et al. (2018) show that the ratio of total assets to equity was also stable, and Begenau et al. (2019) document the same fact for the United States. In Appendix B.3.4, we discuss a robustness check in which we allow leverage to increase.

Investment and credit demand Each firm ω in sector j demands credit from different banks, promising bank b a fraction of their income in period $t+1$. Writing R^b_{t+1} to denote the expected return on credit contracts with bank b (i.e., the interest rate of bank b), the expected repayment for borrowing $q^b_{j,t}(\omega)$ from bank b is $R^b_{t+1}q^b_{j,t}(\omega)$. As we show in greater detail in Appendix B.1, this implies that each firm solves the cost minimization problem

$$\begin{gathered}\min_{q^b_{j,t}(\omega)}\sum_{b=1}^{B}R^b_{t+1}q^b_{j,t}(\omega)\\ \text{s.t.}\qquad k_{j,t+1}(\omega)=\left(\sum_{b=1}^{B}\left(\pi^b_j(\omega)\right)^{\frac{1}{\eta_j}}\left(q^b_{j,t}(\omega)\right)^{\frac{\eta_j}{\eta_j-1}}\right)^{\frac{\eta_j}{\eta_j-1}}.\end{gathered}\tag{12}$$

Accordingly, its credit demand at bank b is given by

$$q^b_{j,t}(\omega)=\pi^b_j(\omega)\left(\frac{R^b_{t+1}}{R_{j,t+1}(\omega)}\right)^{-\eta_j}k_{j,t+1}(\omega),\tag{13}$$

where $R_{j,t+1}(\omega)\equiv\left(\sum_{b=1}^{B}\pi^b_j(\omega)\left(R^b_{t+1}\right)^{1-\eta_j}\right)^{\frac{1}{1-\eta_j}}$ is the (constant) marginal cost of producing one unit of capital, a weighted average of the interest rates of the banks that the firm borrows from. Perfect competition and risk neutrality imply that the firm invests up to the point where this marginal cost equals the expected price of its capital variety tomorrow, $\mathbb{E}_t\left(p^K_{j,t+1}(\omega)\right)$.

Combining this condition with Equation (11), it is easy to determine the credit demand of firm ω at each bank b, overall investment of firm ω, and aggregate investment:

$$q^b_{j,t}(\omega)=\pi^b_j(\omega)\left(\frac{R^b_{t+1}}{R_{j,t+1}(\omega)}\right)^{-\eta_j}\left(\frac{R_{j,t+1}(\omega)}{R_{j,t+1}}\right)^{-\varepsilon_j}\left(\frac{\alpha_j\mathbb{E}_t\left(A_{j,t+1}P_{j,t+1}\right)}{R_{j,t+1}}\right)^{\frac{1}{1-\alpha_j}},\tag{14}$$

$$k_{j,t+1}(\omega)=\left(\frac{R_{j,t+1}(\omega)}{R_{j,t+1}}\right)^{-\varepsilon_j}\left(\frac{\alpha_j\mathbb{E}_t\left(A_{j,t+1}P_{j,t+1}\right)}{R_{j,t+1}}\right)^{\frac{1}{1-\alpha_j}},\tag{15}$$

$$\text{and}\qquad K_{j,t+1}=\left(\frac{\alpha_j\mathbb{E}_t\left(A_{j,t+1}P_{j,t+1}\right)}{R_{j,t+1}}\right)^{\frac{1}{1-\alpha_j}},\tag{16}$$

where $R_{j,t+1}\equiv\left(\int_0^1\left(R_{j,t+1}(\omega)\right)^{1-\varepsilon_j}d\omega\right)^{\frac{1}{1-\varepsilon_j}}$ is the aggregate cost of capital in sector j.

Equations (14) to (16) summarize the credit demand side, expressing all credit demands and investment levels as a function of bank interest rates. Aggregate investment is increasing in expected future TFP and in the expected future price of the sector's final good, and decreasing in the aggregate cost of capital of the sector, $R_{j,t+1}$ (which is an average of the cost of capital of individual firms). For each firm, investment is proportional to aggregate investment, and depends on the ratio of the firm's cost of capital $R_{j,t+1}(\omega)$ to the aggregate cost of capital of the sector. Finally, credit demand at the bank-firm level is proportional to the total investment of the firm, and also depends on its preference weight for bank b and on the ratio of the interest rate of bank b to the firm's cost of capital.

To determine the equilibrium levels of credit and investment, we therefore need to solve for the interest rates charged by banks. To do so, we now characterize the supply of credit.

Credit supply and credit market clearing As mentioned before, we focus on equilibria in which the leverage constraint of bankers is binding. Formally, this means that the interest rate at which bankers of type b lend to firms, R^b_{t+1}, exceeds the interest rate at which bankers can borrow from the IFM, R^*. Thus, all bankers want to borrow as much as possible in equilibrium, i.e., a multiple $(\lambda - 1)$ of their net worth. Accordingly, denoting by W^b_t the net worth of bank b in period t, credit supply by bank b is λW^b_t.

In equilibrium, credit demand must be equal to credit supply at every bank b. That is,

$$\forall b \in \{1, .., B\}, \quad Q^b_{N,t} + Q^b_{H,t} = \lambda W^b_t, \tag{17}$$

where $Q^b_{j,t} \equiv \int_0^1 q^b_{j,t}(\omega)\, d\omega$ stands for the total credit demand of firms of sector j at bank b. In any period t, once the preference shock ξ_t is realized, bank net worth is fully determined. Thus, (17) forms a system of B equations with B unknowns (the interest rates of the B banks), which can be solved numerically.

With this, we have now solved for the equilibrium interest rates in any period t, given the net worth of young bankers. The last step left to characterize the dynamic solution of the model is to specify the law of motion of bank net worth. As we show in Appendix B.1, for any bank b, net worth in period t is given by

$$W^b_t = \phi \left(R^b_t \left(\sum_{j \in \{N,H\}} \left(\frac{A_{j,t} P_{j,t}}{\mathbb{E}_{t-1}\left(A_{j,t} P_{j,t}\right)} Q^b_{j,t-1} \right) \right) - R^* \left(\lambda - 1\right) W^b_{t-1} \right). \tag{18}$$

Equation (18) has a natural interpretation. The net worth of banks of type b in period t is a fraction ϕ of their type's profits. On the income side, banks expect to be repaid their bank-specific interest rate on their credit to each sector, i.e., $R^b_t \cdot Q^b_{j,t-1}$. Ex post, however, actual repayments may be higher or lower than expected, depending on whether TFP and/or final goods prices turn out to be higher or lower than expected. On the cost side, banks must repay the international interest rate R^* on their borrowing from the IFM, $(\lambda - 1) W^b_{t-1}$.

This completes the characterization of the equilibrium. In the next section, we analyze our model's implications for financial transmission, and show that it can replicate the patterns of crowding-out and crowding-in uncovered by the empirical analysis of Section 2.

4.3 Crowding-out and crowding-in in the model

To illustrate our model's predictions, Figure 6 shows a simple example. We assume that there are $B = 2$ banks, that productivity is constant ($A_{j,t} = A_j$), and that the relative price of housing has been constant for a long time, so that the economy is initially in a steady state. From period 4 onward, a housing boom starts: a series of positive shocks to ξ raise the relative price of housing, until it finally stabilizes at a new, higher level (see Panel i). For simplicity, we assume that agents have perfect foresight on the path of housing prices.

What are the consequences of this housing boom? As shown by Equation (14), the increase in the expected future price of housing shifts up the credit demand curves of all housing firms. As a result, housing credit increases throughout (see Panel ii). However, while housing credit demand shifts up, credit supply – which depends on bank net worth, and thus on the repayment of last period's steady-state level loans – changes little initially. Thus, market-clearing interest rates increase, as shown in Panel iii. As a result, non-housing credit is crowded out, as shown in Panel ii.[28]

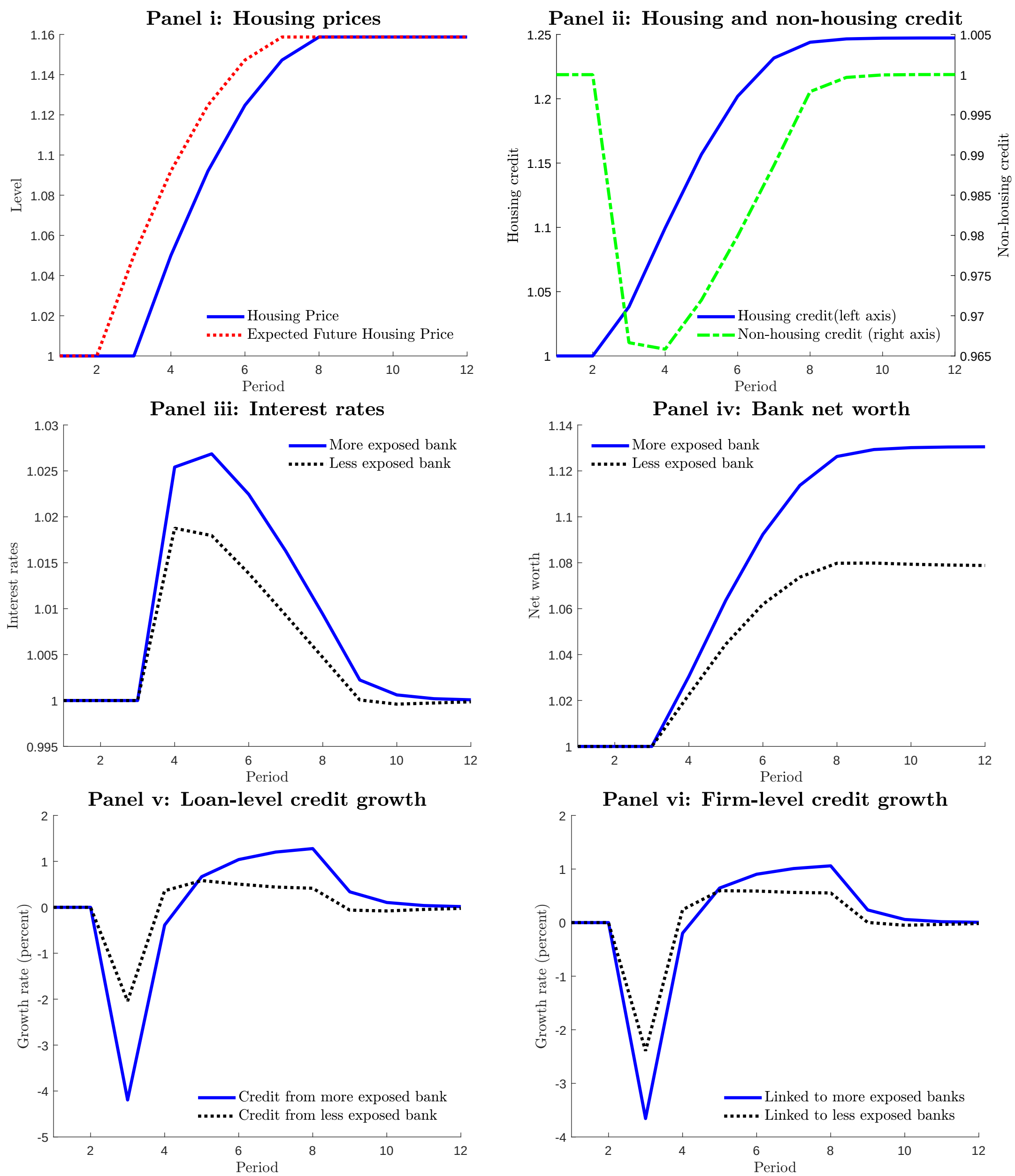
Eventually, however, higher interest rates and a higher loan volume increase the net worth of banks (see Panel iv), and this increases credit supply. Thus, interest rates fall and non-housing credit starts growing again (see Panels ii and iii): crowding-out gets reversed by a crowding-in effect.

While crowding-in is always present in our model, its strength depends on the characteristics of the housing boom. In Figure 6, where housing prices eventually converge to a new (higher) steady-state level, crowding-in exactly compensates the initial crowding-out. Formally, Equations (14)-(18) imply that in any steady state holding $P_{j,t} = \mathbb{E}_{t-1}(P_{j,t})$, the interest rate is independent of housing prices and equalized across banks at $R^b = \frac{1+\phi R^*(\lambda-1)}{\lambda\phi}$. Intuitively, while credit demand increases with the expected price of housing, bank net worth (and thus credit supply) increases with the realized price of housing. In a steady state, both effects exactly cancel out. However, real-world housing booms may not have this feature. Thus, it is also possible that crowding-in does not fully compensate the initial crowding-out (e.g., if the boom does not last long enough for bank net worth to rise significantly) or on the contrary that crowding-in more than

[28] In this example, perfect foresight implies that the first period of the housing boom raises only the expected future price of housing, but not its current price. In general, both may rise jointly. As banks' loan revenue is state-contingent, this increases net worth and thus credit supply on impact. However, supply generally moves less than demand initially, for two reasons. First, while higher current prices increase repayments per loan, higher expected future prices increase both the interest rate per loan and the loan volume. Second, it seems reasonable to suppose that housing booms unfold gradually, and that agents initially (correctly) anticipate that the best is yet to come. Thus, while we can construct examples in which current prices increase much more than future prices, causing supply to shift more than demand on impact, these do not seem empirically relevant.

compensates the initial crowding-out (e.g., if the late stages of the boom are characterized by a low ratio of expected to realized housing prices).[29]

Figure 6: Illustration of the main mechanisms



Notes: Parameter values are chosen for illustrative purposes, and listed in Appendix B.1.4. With the exception of growth rates, all time series are normalized to 1 in the steady state. The fifth panel plots the credit growth rates for a non-housing firm with $\pi_N^1(\omega) = 0.75$ at both banks, while the sixth panel plots the total credit growth of a non-housing firm with $\pi_N^1(\omega) = 0.75$ (blue straight line) and a non-housing firm with $\pi_N^1(\omega) = 0.25$ (black dotted line).

[29]The latter case may occur in a stochastic housing boom, where agents' realization that the boom may end lowers the expected price of housing below the current price. Appendix B.1.5 provides further details on this issue.

The model also provides a clear structure to think about bank exposure and cross-sectional heterogeneity. In our empirical analysis, we measured a bank's exposure to the housing boom by the pre-boom ("steady-state") share of housing loans in the bank's loan portfolio. In the model, this share depends on the banking preferences of housing and non-housing firms. Formally, the steady-state ratio of housing to non-housing credit of bank b is proportional to $\int_0^1 \pi_H^b(\omega)d\omega / \int_0^1 \pi_N^b(\omega)d\omega$ (see Appendix B.1.3). Thus, in our two-bank example, one bank has a higher share of housing loans than the other if housing firms prefer that bank relatively more than non-housing firms.

As Figure 6 shows, this heterogeneity in exposure plays a key role in shaping the response of banks to the housing boom. Higher expected housing prices uniformly shift up the credit demand curves of all housing firms. However, for the more exposed bank (bank 1), housing represents a larger fraction of credit. Thus, the boom entails a larger increase in credit demand relative to net worth for the more exposed bank, and its interest rate therefore rises relative to the less exposed bank (see Panel iii).

This divergence in interest rates has consequences at the loan and at the firm-level. First, it leads non-housing firms to reallocate credit towards the less exposed bank. Thus, for a given non-housing firm, credit growth is lower at more exposed banks (see Panel v), exactly as we found in our loan-level regressions. Second, consider two different types of non-housing firms, A and B, with one type relying more on the exposed bank than the other (i.e., $\pi^1_{N,A} > \pi^1_{N,B}$). It is easy to see that as the relative interest rate of bank 1 increases, the relative funding cost of type-A firms increases too (as the interest rate of type-1 banks has a higher weight in the funding cost of type-A firms). Consequently, the relative credit of type-A firms falls. In other words, credit growth for more exposed firms is lower than credit growth for less exposed firms (see Panel vi), exactly as we found in our firm-level regressions.

Eventually, the net worth effect sets in and shifts out banks' credit supply curves. This effect is also stronger at the more exposed bank, precisely because housing – which is responsible for the increase in net worth – represents a larger share of its loans (see Panel iv). The relative interest rate of the more exposed bank now falls (see Panel iii), which prompts non-housing firms to redirect their borrowing back to them (see Panel v). Thus, in the late stages of the boom, credit growth for a given non-housing firm is higher at more exposed banks, as we found in our loan-level regressions. Furthermore, the decline in the relative interest rate of more exposed banks implies a decline in the relative funding costs of type-A firms. Therefore, the credit growth rate of more exposed firms eventually exceeds the one of less exposed firms (see Panel vi), as we found in our firm-level regressions.

This discussion shows that our model can provide a simple and consistent explanation for the empirical regularities shown in Section 2. Moreover, it does so without introducing new ingredients: its driving mechanism – financial constraints – is already invoked in the literature on the crowding-out effect of housing booms and is consistent with the empirical evidence. The model has other important implications for financial transmission, e.g. during housing busts, which we do not pursue here. Instead, we use it to assess the aggregate importance of our cross-sectional empirical findings. We turn to this next.

5 Estimating the aggregate impact of financial transmission

5.1 Cross-sectional estimates and aggregate magnitudes

The empirical results of Section 2 show that the Spanish housing boom affected the credit growth of non-housing firms: initially, firms shifted their borrowing away from banks that were more exposed to the boom, and firms with stronger links to these banks had lower credit growth. These effects were reverted in the later stages of the boom, as firms switched back to more exposed banks, and firms with stronger links to these banks had faster credit growth. However, while these results indicate that there was financial transmission, they are silent about its aggregate importance. That is, they cannot tell us how much higher or lower aggregate non-housing credit would have been without financial transmission.

The empirical estimates that come closest to answering this question are the firm-level regression results shown in Table 3.[30] Given these estimates, one could imagine the following back-of-the-envelope calculation (for simplicity, we focus on the crowding-out phase, but the same arguments apply to crowding-in). A one-standard-deviation increase in firm exposure during 2001-2003 was associated with a 2.85 percentage points lower credit growth rate. The average firm's exposure was 4.72 standard deviations higher than that of a (hypothetical) zero-exposure firm, which could be thought of as being unaffected by the boom.[31] Thus, financial transmission lowered the credit growth of the average firm by $2.85 \cdot 4.72 = 13.45$ percentage points.

Such a naive estimate is obviously flawed. Indeed, our cross-sectional regressions identify differences in credit growth between firms, not the overall effect of financial transmission on non-housing credit growth. In fact, aggregate magnitudes could in principle be either larger or smaller than the numbers suggested by the naive extrapolation described above.

This can easily be illustrated using our model. Assume, for instance, that non-housing firms can perfectly substitute credit from different banks. Then, their initial exposure is irrelevant for credit growth, and our

[30] Firm-level results are more relevant than loan-level results in this context, as they apply to a higher level of aggregation. Indeed, it is in principle possible that the boom triggers a large reallocation of credit within firms, but does not affect firm-level credit growth, as firms just substitute one source of credit for another.

[31] The average value of firm exposure is 45.8%, and the standard deviation is 9.7%.

firm-level regression coefficients equal zero. However, there could still be large aggregate crowding-out and crowding-in effects, affecting all non-housing firms in the exact same way.[32] This example is extreme, but it conveys a general principle: if there is a common effect of financial transmission on all non-housing firms, firm-level differences underestimate aggregate magnitudes.

Conversely, the aggregate effect of financial transmission could also be smaller than the one suggested by our firm-level estimates. Indeed, if non-housing firms are heterogeneous, our model shows that there is bound to be substitution between them. As the funding costs of more exposed firms rise, less exposed firms gain market share and – all else equal – increase their credit demand. Thus, it is easy to construct a parametrization of the model in which large firm-level differences in credit growth just reflect reallocation, while aggregate credit barely changes.[33]

This discussion shows that our empirical estimates alone are insufficient to assess the aggregate magnitude of financial transmission. Instead, we have to rely on our model. However, this does not imply that our empirical estimates are uninformative: as we show next, they will be crucial inputs in the model's calibration.

5.2 Calibration strategy

We calibrate all but two parameters by using external evidence and matching key features of the Spanish data. We then show that, conditional on our model being the data-generating process and all other parameters being fixed, our cross-sectional (loan and firm-level) estimates identify the two remaining parameters. We consider robustness checks for our most important choices, and discuss these in Section 5.5 and in the Appendix.

Basics We assume that one period in the model corresponds to one year in the data. As in our empirical analysis, we focus on the period 2000-2008. We set the capital shares in the housing and non-housing sectors to $\alpha_N = \alpha_H = \frac{1}{3}$, and assume that the international interest rate is equal to 3% ($R^* = 1.03$). Moreover, we assume that the leverage ratio of banks λ is equal to 11.56, the median leverage ratio for Spanish banks in 2000. Finally, we set the elasticity of substitution between different firms to $\varepsilon_H = \varepsilon_N = 4$, a standard value in the literature (see, e.g., Galí and Monacelli, 2016; Aghion et al., 2019).

Growth and the housing boom We assume that in both sectors, productivity $A_{j,t}$ grows at a constant rate g_A, and set g_A by targeting the growth rate of real non-housing credit between 2000 and 2007, which was 81%. Productivity growth serves a limited purpose for our calibration. As we will explain in greater

[32] This common effect is absorbed by the intercept of our regressions. However, it is obviously impossible to distinguish the part of the intercept due to financial transmission and the part due to other common shocks affecting all non-housing firms.

[33] This could be achieved by assuming a high elasticity of substitution ε_N, large differences in exposure, and a small boom.

detail below, we run the equivalent of our empirical loan and firm-level regressions with model-generated data. Without growth, the scale of these regressions would not be comparable to their data counterparts: a 3 percentage-point difference in credit growth does not represent the same magnitude against the backdrop of increasing or falling aggregate non-housing credit (recall that in the absence of productivity growth, our model predicts that aggregate non-housing credit falls in the first years of the boom). Otherwise, the parameter g_A is irrelevant. Indeed, keeping all other parameter values fixed, our estimate for the aggregate magnitude of financial transmission does not change when setting $g_A = 0$.[34]

We assume that, up to the year 2000, the Spanish economy was on a Balanced Growth Path (BGP) with a constant relative price of housing. For convenience, we normalize $\xi_{2000} = 1$, implying $P_{H,2000} = 1$. As we show in Appendix B.1.3, all banks charge the same interest rate on the BGP. Using Equation (14), it is then easy to show that the share of housing in total credit is $1/\left(1+\left(\frac{A_N}{A_H}\right)^{\frac{1}{1-\alpha}}\right)$. That is, conditional on the capital share α (calibrated to $\frac{1}{3}$), the BGP share of housing in total credit only depends on the (constant) relative productivity of the housing sector. We normalize $A_{H,2000} = 1$, and choose $A_{N,2000}$ to match the share of housing in total credit in 2000 (46.6%, as shown in Figure 2). This implies $A_{N,2000} = 1.095$.

Starting from 2001, the economy is hit by a housing boom. We assume that the increase in relative housing prices in the model is proportional to the one observed in the data. That is, for $t \geq 2001$, $\Delta \ln P_{H,t} = \zeta \cdot \Delta \ln P_{H,t}^{\text{Data}}$, where $\Delta \ln P_{H,t} \equiv \ln P_{H,t} - \ln P_{H,t-1}$ and ζ is a positive scaling parameter.[35] We set ζ in order to match the observed increase in the housing share of aggregate credit, from 46.6% in 2000 (which, as explained above, we already match) to 61.9% in 2007. Thus, the parameter ζ reflects the fact that the available data on house prices may not capture all dimensions of the Spanish housing boom.[36] Furthermore, to discipline agents' expectations of future housing prices, we assume that housing price growth was generated by a stochastic AR(1) process and that agents have rational expectations. Thus, agents' expectations for future housing price growth satisfy $\mathbb{E}_t \left(\Delta \ln P_{H,t+1}\right) = \rho \cdot \Delta \ln P_{H,t}$, where ρ is the persistence of the AR(1) process. We estimate this parameter using the time series on relative housing price growth between 1996 and 2016, finding $\rho = 0.909$. Summing up, we discipline the size of the housing boom by matching the change in aggregate credit composition, its time profile by using the data on relative price increases, and expectations by imposing an AR(1) structure and rational expectations.

[34]In the data, Spain experienced negative productivity growth during the boom years. However, g_A need not be interpreted as actual productivity growth. Indeed, we could introduce growth just as well through rising labor endowments.

[35]Formally, we set a path of housing preference shocks. However, as $P_{H,t} = \xi_t$, we refer directly to housing prices. We compute the growth rate of relative housing prices using the housing price index from the Ministry of Construction (shown in Figure 1), deflated with Spain's Harmonized Index of Consumer Prices (excluding housing and fuels) from Eurostat.

[36]Without introducing ζ, our model would overpredict the increase in the housing share of aggregate credit. A contemporaneous evolution that may have limited housing credit growth was the decrease in the relative TFP of the construction sector. EU KLEMS data (see https://euklems.eu/) shows that annual TFP growth in the construction sector was 3.5 percentage points lower than in the rest of the market economy.

Bank and firm-level heterogeneity In our baseline calibration, we assume that there are $B = 2$ types of banks, which differ in their exposure to the housing boom (measured, as in the data, by the share of housing in bank credit in 2000). Type-1 banks in the model correspond to banks with an above-median exposure in the data, while type-2 banks correspond to banks with below-median exposure.

Since we are interested in the evolution of the non-housing sector, we keep the housing sector as simple as possible. Thus, we assume that all housing firms are identical, with preference weights given by π_H^1 and $\pi_H^2 = 1 - \pi_H^1$. We also set $\eta_H = 0$, implying that housing firms have Leontief preferences across banks. Therefore, housing credit grows at the same rate at both types of banks during the boom.[37] Nevertheless, type-1 banks are more affected by the boom: as housing represents a larger share of their loan portfolio, a uniform increase in housing credit demand implies a stronger increase in overall credit demand for them.

For the non-housing sector, we approximate the distribution of preferences across firms by reducing it to four points. Precisely, we assume that a mass $\theta_{N,A}$ of firms borrow only from type-1 banks ($\pi_{N,A}^1 = 1$), while a mass $\theta_{N,D}$ of firms borrow only from type-2 banks ($\pi_{N,D}^1 = 0$). Furthermore, a mass $\theta_{N,B}$ of firms obtain 75% of their BGP credit from type-1 banks ($\pi_{N,B}^1 = 0.75$), and a mass $\theta_{N,C}$ obtain 25% of their BGP credit from type-1 banks ($\pi_{N,B}^1 = 0.25$).

Table 10: Bank and firm-level heterogeneity, calibrated parameters

Parameter	Meaning	Value	
π_H^1	BGP share of housing credit obtained from type-1 banks	0.799	
$\theta_{N,A}$	Share of non-housing firms of type A	0.566	
$\theta_{N,B}$	Share of non-housing firms of type B	0.057	
$\theta_{N,C}$	Share of non-housing firms of type C	0.058	
Target	Meaning	Model	Data
E_{2000}^1	Share of housing in total credit, type-1 banks	52.8%	52.8%
E_{2000}^2	Share of housing in total credit, type-2 banks	31.8%	31.8%
$\overline{\mathrm{E}}_{f,2000}$	Average value of firm exposure	44.9%	45.8%
$\sigma\left(\mathrm{E}_{f,2000}\right)$	Standard deviation of firm exposure	9.7%	9.7%

These assumptions introduce four parameters that need to be calibrated: π_H^1, $\theta_{N,A}$, $\theta_{N,B}$ and $\theta_{N,C}$ (obviously, $\theta_{N,D} = 1 - \theta_{N,A} - \theta_{N,B} - \theta_{N,C}$). We set these parameters to match four BGP moments. First, we target our empirical exposure measures for both types of banks. In the data, the share of housing in the aggregate credit given by banks with above-median exposure is $\mathrm{E}_{2000}^1 = 52.8\%$, while the corresponding

[37] Assuming $\eta_H > 0$ would imply that housing credit initially grows less at more exposed banks. As Table 8 shows, this is counterfactual. Thus, setting $\eta_H = 0$ comes as close as possible to matching the data in the context of our model.

number for banks with below-median exposure is $\mathrm{E}^2_{2000} = 31.8\%$. Second, we match the average and the standard deviation of our empirical measure of firm exposure. As shown in Equation (3), this measure is a weighted average of bank exposure. Using the same formula, the BGP exposure of a non-housing firm of type f in our model is $\pi^1_{Nf}\mathrm{E}^1_{2000} + \left(1 - \pi^1_{Nf}\right)\mathrm{E}^2_{2000}$. The average firm exposure in the data is 45.8%, and the standard deviation across firms is 9.7%. These four moments identify the four parameters. Note, moreover, that these moments only depend on the aforementioned parameters and on the (previously calibrated) relative productivity of housing. Therefore, this part of the calibration is independent of the rest. Table 10 shows the chosen parameter values, which closely match the targeted moments.[38]

Cross-sectional estimates The choices described so far leave us with two free parameters: η_N, the elasticity of substitution of non-housing credit across banks, and ϕ, the fraction of bank profits passed on to young bankers. We rely on our cross-sectional estimates from Section 2 for the calibration of these parameters.

To do so, we run the equivalent of our empirical loan and firm-level regressions with model-generated data. For the loan-level regressions, we estimate for each time period (t_0, t_1),

$$100 \cdot \frac{q^b_{Nf,t_1} - q^b_{Nf,t_0}}{q^b_{Nf,t_0}} = \mu_f + \beta^{\text{Model}}_{t_0,t_1}\mathrm{E}^{b,\text{Model}}_{2000} + u^b_f, \tag{19}$$

where $q^b_{Nf,t}$ is the credit of the representative non-housing firm of type f at banks of type b in year t, μ_f is a firm-type fixed effect, and $\mathrm{E}^{b,\text{Model}}_{2000}$ is our measure of exposure for type-b banks (standardized to have mean zero and unit standard deviation, as in the empirical regressions). We estimate this regression for the periods 2001-2003 and 2004-2007 in the model and compare the results to the ones obtained in the data, stated in Columns (1) and (2) of Table 1. Likewise, at the firm-level, we estimate

$$100 \cdot \frac{q_{Nf,t_1} - q_{Nf,t_0}}{q_{Nf,t_0}} = \mu + \gamma^{\text{Model}}_{t_0,t_1}\mathrm{E}^{\text{Model}}_{f,2000} + u_f, \tag{20}$$

where μ is a constant and $\mathrm{E}^{\text{Model}}_{f,2000}$ is the exposure of the representative non-housing firm of type f (again standardized as in the empirical regressions).[39] We compare our estimates for $\gamma^{\text{Model}}_{t_0,t_1}$ for the periods 2001-2003 and 2004-2007 to the ones obtained in the data, stated in Columns (1) and (2) of Table 3.

[38] The calibration delivers a high share of non-housing firms which borrow only from one bank. This is necessary to match the standard deviation of firm exposure: considering only two banks reduces the range of bank exposure with respect to the empirical one, so there needs to be substantial mass in the tails. However, our results do not depend on this. Appendix B.3.1 shows that in a three-bank model, we get very similar results in a calibration without any single-bank firm. Indeed, we find that as long as we match the mean and standard deviation of firm exposure, the distribution of preferences does not matter.

[39] Furthermore, as in the empirical regressions, we estimate coefficients with WLS, weighting by BGP credit. Note that each regression is estimated on four model observations: there are two types of multibank non-housing firms (types B and C) borrowing from two banks for the loan-level regression, and four types of non-housing firms for the firm-level regression.

Given the importance of this step, it is worth discussing in detail how the regression coefficients identify the parameters of interest. First, consider ϕ, which determines how fast banks accumulate net worth. This parameter is crucial for the magnitude of both series of regression coefficients. In our model, more exposed banks receive a relatively larger credit demand shock, which initially increases their relative interest rate. Eventually, however, they also accumulate more net worth than less exposed banks, and relative interest rates converge again. When ϕ is low, the rate of net worth accumulation is slow. This leads to a large divergence of interest rates between more and less exposed banks, resulting in large differences in loan- and firm-level credit growth.[40] When ϕ is high, more exposed banks accumulate net worth quickly, there is little divergence in interest rates across banks, and there are only small differences in loan- and firm-level credit growth.

Second, consider the elasticity of substitution η_N. When η_N is zero, loan-level estimates are zero, as non-housing firms cannot substitute between banks. Instead, when η_N is high, loan-level estimates are also high, as even small differences in interest rates lead multibank firms to shift most of their credit from one bank to another. Thus, roughly speaking, ϕ determines the magnitude of both series of regression coefficients, while η_N determines their relative size. We will return to these issues in Section 5.5.

5.3 Model fit

Our model has four internally calibrated parameters, ϕ, η_N, g_A and ζ.[41] We use a non-linear solver to find the values of these parameters that minimize the distance between model moments and their data equivalents (see Appendix B.2 for details). Table 11 lists the estimated parameter values and illustrates the model's fit.

Jointly, productivity growth g_A and the size of the boom ζ allow us to exactly match the overall credit growth and the shift in aggregate credit composition between 2000 and 2007. The last four rows of Table 11 show that we also closely match our cross-sectional regression coefficients. The estimation delivers a value of 0.019 for ϕ, the speed of net worth accumulation, and a value of 3.4 for η_N, the elasticity of substitution of non-housing credit across banks. The latter estimate is in line with the (limited) existing literature.[42]

Figure 7 illustrates the behavior of the calibrated model. Panel i shows the series for relative housing prices and expectations of future prices. Relative housing prices increase substantially during the boom, and between 2001 and 2007, agents consistently expect them to keep increasing. Panel ii shows that the housing boom triggers a change in aggregate credit composition that mirrors the one observed in the data, with the

[40] In this context, it is important to note that in our model, larger crowding-out estimates go hand in hand with larger crowding-in estimates: if non-housing credit falls more initially, higher subsequent growth is needed to undo this effect.

[41] Strictly speaking, the parameters π_H^1, $\theta_{N,A}$, $\theta_{N,B}$ and $\theta_{N,C}$ are internally calibrated as well. However, they only depend on BGP moments and are independent of the rest of the calibration. Thus, their values - given in Table 10 - are unchanged throughout (except in Appendix B.3.1, which considers three bank types), and we abstract from them in the following discussion.

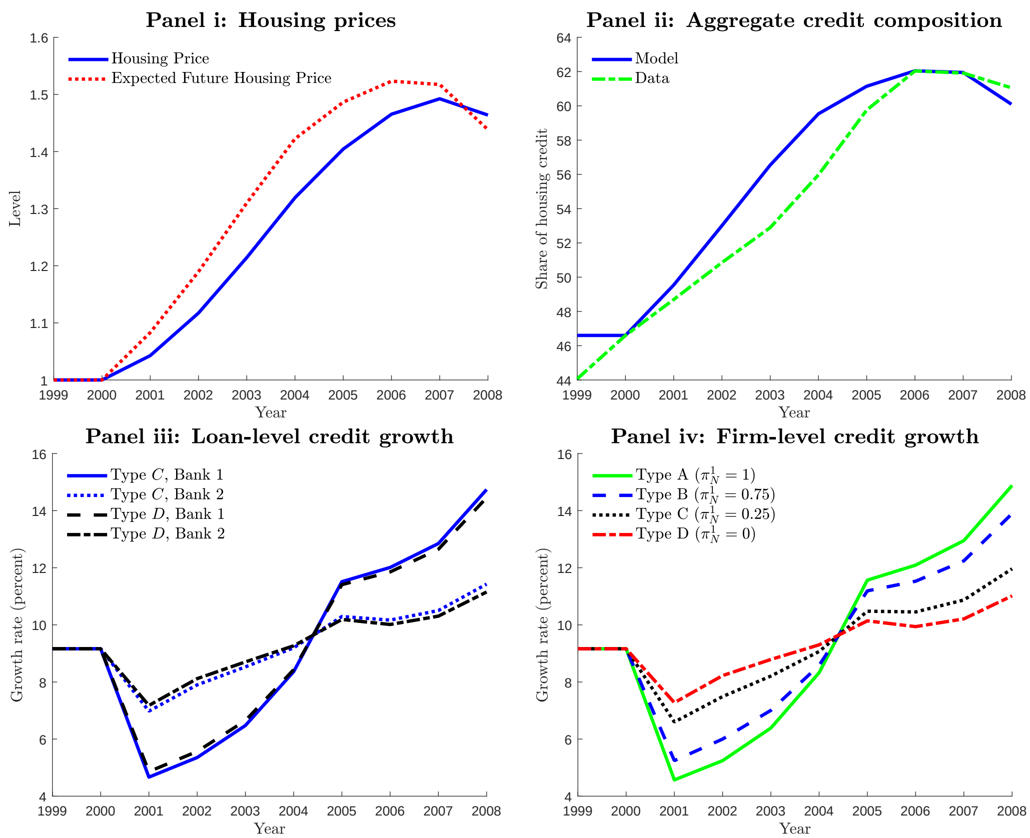
[42] Herreño (2020) finds an elasticity of substitution corresponding to $\eta_N = 2.5$ in his baseline estimation.

housing share increasing from 46.6% in 2000 to 61.9% in 2007.

Table 11: Parameter estimates and model fit

Parameter	Meaning	Value	
g_A	Background productivity growth	0.060	
ζ	Magnitude of the housing boom	0.632	
ϕ	Speed of net worth accumulation	0.019	
η_N	Elasticity of substitution across banks for N-firms	3.421	
Target	Meaning	Model	Data
$Q_{H,2007}/(Q_{H,2007}+Q_{N,2007})$	Share of housing in total credit, 2007	61.9%	61.9%
$(Q_{N,2007}-Q_{N,2000})/Q_{N,2000}$	Non-housing credit growth, 2000-2007	81.0%	81.0%
$\beta_{2001-2003}$	Loan-level regression coefficient, 2001-2003	-2.85	-2.29
$\beta_{2004-2007}$	Loan-level regression coefficient, 2004-2007	3.85	4.82
$\gamma_{2001-2003}$	Firm-level regression coefficient, 2001-2003	-2.67	-2.89
$\gamma_{2004-2007}$	Firm-level regression coefficient, 2004-2007	3.61	3.34

Figure 7: Quantitative model, time series



Notes: This figure plots some key outcomes for our baseline calibration. The chosen parameter values are listed in Section 5.2.

Panels iii and iv illustrate cross-sectional differences in loan and firm-level credit growth. The third panel plots credit growth rates of multibank non-housing firms at different banks, showing that credit growth is first lower and then higher at more exposed (type-1) banks. The fourth panel plots overall credit growth rates for the four types of non-housing firms, showing that more exposed firms have first lower and then higher credit growth rates.[43]

Untargeted moments To further assess the model's performance, we consider some untargeted moments. A key quantity in our model is the increase in bank net worth triggered by the housing boom, which drives the transition from crowding-out to crowding-in. To assess our model's predictions for net worth growth, we estimate our empirical regression of net worth growth on exposure (shown in Table 6) with model-generated data. We find positive coefficients of 0.23 for the period 2001-2003 and 0.34 for the period 2004-2007 (against coefficients of 0.07 and 0.63 in the data). Thus, even though we did not target these moments, our model comes relatively close to matching them: differences in net worth growth between banks are roughly similar in the model and in the data.

We are now ready to turn to the main question: how much higher or lower would non-housing credit have been without the financial transmission of the housing boom?

5.4 Quantifying financial transmission

To measure the impact of financial transmission, we compare the path of aggregate non-housing credit in our baseline calibration to a counterfactual path without financial transmission, obtained by assuming that interest rates remain at their BGP level throughout. In our model, this counterfactual would be an equilibrium outcome if the housing boom did not occur, or equivalently, if banks did not face financial constraints (without constraints, there is no financial transmission, and non-housing sector outcomes are independent of housing-sector developments).[44] Figure 8 illustrates our results, expressing the baseline levels of non-housing credit and output as a fraction of their counterfactual levels without financial transmission.

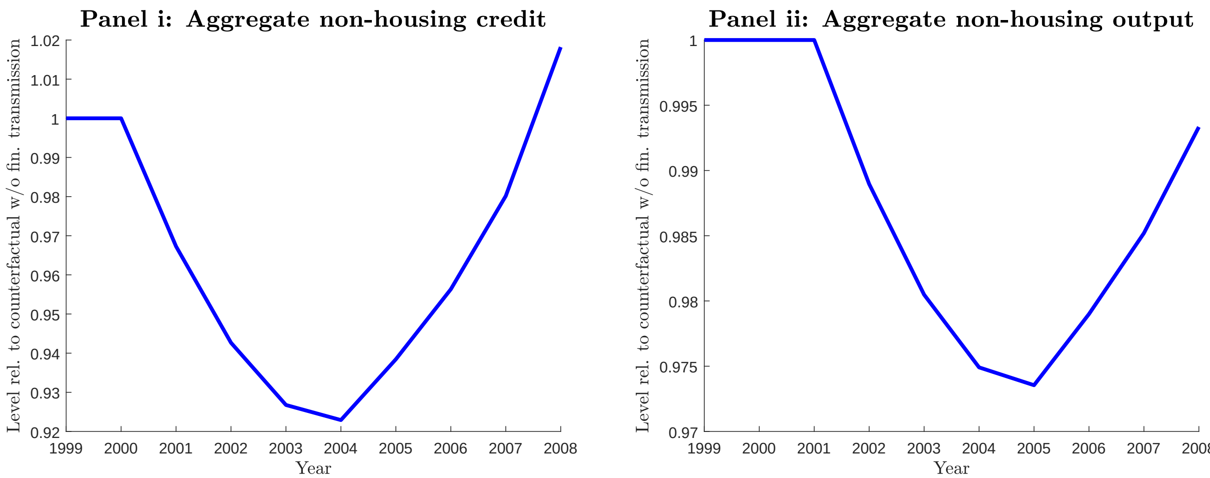
Our model indicates that although non-housing credit grows by 31% between 2000 and 2004, it would have grown by 42% in the absence of financial transmission. Thus, as shown in Figure 8, by 2004 non-housing credit was 7.7% lower than it would have been without financial transmission. Then, crowding-in kicked in and turned the situation around: by 2007, the shortfall was reduced to 2.0%, and in 2008 (when housing prices plateaued), financial transmission had raised non-housing credit by 1.8% relative to what it would

[43] A closer look reveals that credit growth is approximately linear in exposure, supporting our empirical specifications.

[44] To be precise, without financial constraints, interest rates are constant and equal to R^*. Thus, there is a permanent level difference with respect to a world with financial constraints, but this does not matter for the economy's reaction to a boom.

have been otherwise. The time series of output effects is delayed by one period, as credit in year t finances capital in year $t+1$. Furthermore, output effects are smaller, as crowding-out only applies to capital and not to labor. As the aggregate production function is log-linear and the capital share is $1/3$, output effects are roughly one third as large as credit effects.

Figure 8: The aggregate effects of financial transmission on non-housing credit and output



Notes: The figure plots the ratio between aggregate non-housing credit/output in our baseline calibration and in a counterfactual in which all interest rates are equal to their BGP values throughout. All other parameters are always at their baseline calibration values.

Summing up, our estimates for financial transmission imply that the Spanish housing boom had a substantial crowding-out effect until the mid-2000s, slowing down the expansion of non-housing credit. This echoes the frequently voiced fears about the negative effect of housing booms on other economic sectors. However, we also find that this crowding-out effect was temporary: the housing-induced accumulation of net worth by the banking sector undid the entire negative effect by the time the boom ended.

5.5 Robustness checks

As we have argued above, our empirical estimates are crucial inputs for the calibration of our model. To further illustrate their role, it is useful to investigate how aggregate conclusions would change if our empirical estimates had been different. Table 12 provides the answer to this question. Column (1) reproduces our baseline results, while the other columns list the results obtained when targeting loan-level and/or firm-level estimates that are only half as large as the point estimates shown in Tables 1 and 3. For these alternatives, we recalibrate the internal parameters g_A, ζ, ϕ and η_N in order to target the new moments. All other parameters

Table 12: Cross-sectional estimates and aggregate implications

	Baseline	Lower loan targets	Lower firm targets	Lower loan&firm targets
	(1)	(2)	(3)	(4)
Parameters				
g_A	0.060	0.060	0.059	0.059
ζ	0.632	0.630	0.630	0.627
ϕ	0.019	0.022	0.133	0.135
η_N	3.421	1.703	6.029	3.109
Targets (model)				
$\beta_{2001-2003}$	-2.85	-1.40	-3.09	-1.58
$\beta_{2004-2007}$	3.85	1.96	3.38	1.75
$\gamma_{2001-2003}$	-2.67	-2.63	-1.64	-1.63
$\gamma_{2004-2007}$	3.61	3.69	1.80	1.80
Level of non-housing credit relative to counterfactual w/o financial transmission				
2004	-7.7%	-7.5%	-4.0%	-3.9%
2007	-2.0%	-1.9%	-1.2%	-1.2%
2008	$+1.8\%$	$+1.8\%$	$+0.4\%$	$+0.4\%$

Notes: Column (1) is the baseline calibration, shown in Table 11. In column (2), targets for loan-level coefficients are multiplied by 0.5, and the internal parameters g_A, ζ, ϕ and η_N are recalibrated. In column (3), targets for firm-level coefficients are multiplied by 0.5, and in column (4), targets for both coefficients are multiplied by 0.5. All other parameters or targets are unchanged throughout.

are set to their baseline values.

The first result worth noting is that our estimates for g_A and ζ are virtually unchanged throughout, as these parameters are not identified by cross-sectional estimates. Second, Table 12 neatly illustrates how cross-sectional estimates identify ϕ and η_N. When targeting firm-level coefficients that are only half as large as the ones actually estimated (Columns (3) and (4)), we find a substantially higher value for the parameter ϕ. For our model, all else equal, lower firm-level coefficients imply a smaller divergence in firm funding costs. This must mean that more exposed banks catch up more quickly with less exposed ones, i.e., that net worth accumulation is faster. The three last rows of Table 12 show that this has crucial aggregate implications: with faster net worth accumulation, aggregate crowding-out is also substantially smaller.

Loan-level targets play a less important role. The fourth column shows that when we scale down both loan and firm-level targets, our estimate for η_N is roughly the same as in the baseline. This is consistent with the intuition that η_N is identified by the relative size of loan and firm-level coefficients. Accordingly, η_N decreases when we only scale down loan-level targets (see Column (2)), and increases when we only scale down firm-level targets (see Column (3)). However, these changes have a limited aggregate impact.

This analysis clearly illustrates the link between our empirical results and our estimates for the aggregate impact of financial transmission: keeping all other parameters fixed, the amount of cross-sectional divergence pins down the speed of net worth accumulation, and that speed of net worth accumulation is key for the depth and the persistence of the crowding-out effect.

In the Appendix, we discuss a range of further robustness checks, including a calibration with 3 rather than 2 banks (see Appendix B.3.1), a different starting year for the housing boom (see Appendix B.3.2), perfect foresight for future housing prices (see Appendix B.3.3), a time-varying bank leverage ratio (see Appendix B.3.4) and different elasticities of substitution among non-housing firms (see Appendix B.3.5). Our results are roughly unchanged across these alternatives.

6 Conclusion

Housing booms have spillover effects to the non-housing sector through the banking system. Our analysis shows that the direction of this financial transmission varies over time: a housing boom first slows down non-housing credit growth, only to eventually stimulate it again. We provide cross-sectional evidence that these crowding-out and crowding-in effects were at work during the Spanish housing boom, and argue that they can be rationalized by appealing to one single mechanism, financial constraints for banks. Finally, our quantitative analysis, model-based but disciplined by our cross-sectional estimates, suggests that crowding-out was substantial in Spain, lowering non-housing credit by around 8%. However, crowding-out was also transitory, and had been fully undone by the end of the boom. Of course, these precise figures refer to Spain, a relatively small economy that was very open to capital inflows. Crowding-out may well be more important for larger, less open economies like the United States or China.

Our analysis provides a comprehensive view of the role of the banking system for the transmission of housing booms. However, our findings are also relevant for other sectoral or geographically concentrated shocks. Fears that a boom in one sector may slow down the development of others have a long history in economics, reaching back at least to the vast literature on the Dutch disease. Our findings suggest that, as far as the banking system is concerned, these worries may not be fully warranted: crowding-out can be a short-lived phenomenon, as booms (even in a sector as credit-intensive as housing) eventually raise credit to all sectors. Likewise, recent build-ups in public debt have raised concerns that credit demand by the public sector may crowd out private lending (Acharya et al., 2018; Broner et al., 2020). However, our paper suggests that profits from government bond purchases may eventually increase bank net worth and thus credit supply, compensating for initial crowding-out effects.

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A (Online) Data Appendix

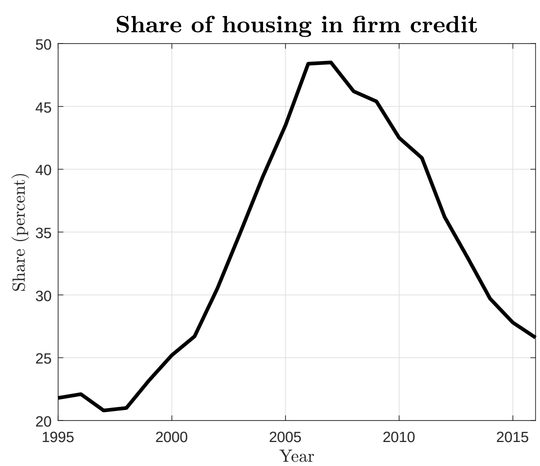
A.1 Data sources for Section 2.1

Nominal house prices are taken from the Spanish Ministry of Construction (http://www.fomento.gob.es/MFOM/LANG_CASTELLANO/ATENCION_CIUDADANO/INFORMACION_ESTADISTICA/Vivienda/Estadisticas). We use the series "valor tasado de vivienda libre" (Table 1). Prices are defined as the average price per square meter of free (that is, non-subsidized) housing, and estimated every trimester by the ministry on the basis of data provided by valuation experts. We take a simple average to aggregate this data to a yearly series. The ministry also provides an estimate of the number of new housing construction projects started in a given year ("Numero de viviendas libres iniciadas", Table 3.1). New construction projects also explode during the boom, going from 250'000 in 1997 to around 660'000 in 2006. They then collapse spectacularly, falling below 100'000 in 2009, and below 50'000 in 2012, 2013 and 2014.

Annual GDP data is taken from Eurostat. Finally, data on credit and credit composition is taken from Table 8.9 of the Bank of Spain's Economic Bulletin (https://www.bde.es/webbde/es/estadis/infoest/bolest.html). Throughout, we abstract from credit to non-profits (*Crédito para financiación a instituciones privadas sin fines de lucro*) and "other not elsewhere classified" credit (*Otros sectores residentes sin clasificar*), which represent only a tiny fraction of overall credit. We define housing credit as the sum of credit to construction firms (*Construcción*), credit to real estate firms (*Actividades inmobiliarias*) and mortgage and home improvement credit (*Adquisición y rehabilitación de viviendas*). Deflating credit growth with the EU KLEMS GDP deflator for the market economy, we obtain the growth rates cited in Footnote 8.[45]

Finally, Figure A.1 plots the share of housing in firm credit (i.e., the share of firm credit going to construction and real estate firms). As stated in Footnote 8, the composition change for firm credit during the housing boom is even more striking than the one for overall credit.

Figure A.1: Composition of firm credit, 1995-2016



Source: Bank of Spain.

A.2 Summary statistics

Tables A.1 and A.2 contain summary statistics for most variables used in our analysis. For greater clarity, we present summary statistics only for three years of our sample, 2001, 2004 and 2008.

[45] To be consistent with our micro-level analysis, we construct the credit composition series shown in Figure 2 using CIR data, and not the data from the Bank of Spain's Economic Bulletin (EB). Both series are very close to each other: the share of housing in total credit in 2000 is 46.6% in the CIR and 45.2% in the EB. In 2007, it is 61.9% in the CIR and 61.4% in the EB.

Table A.1: Summary statistics

Panel A: Year 2001	Mean	Std. Dev.	25th pctile	Median	75th pctile	# obs.
Bank-firm variables						
Credit_growth$^b_{f,2001,2000}$	10.42	54.86	-17.33	-3.50	18.72	682,767
Length of firm-bank relat. (months)	31.69	23.70	12.00	36.00	60.00	845,975
Past defaults	0.13	0.33	0.00	0.00	0.00	845,975
Bank variables						
Boom exposure (E^b_{2001})	0.44	0.15	0.35	0.43	0.55	153
log total assets	13.30	2.30	11.60	13.21	15.09	194
Capital ratio	0.13	0.20	0.05	0.08	0.10	194
Liquidity ratio	0.12	0.10	0.05	0.12	0.17	194
Default rate	0.009	0.012	0.003	0.006	0.010	194
Firm variables						
Credit_growth$_{f,t}$	8.91	63.99	-25.01	-5.88	18.72	401,022
Demand shock	-0.15	1.02	-0.57	-0.38	-0.14	391,143
Total assets (thousands euros)	1596	4460	155	383	1072	226,966
Number employees	20.24	266.60	3.00	6.00	14.00	226,966
Own funds over total assets	0.38	0.28	0.14	0.33	0.59	226,966
Return on assets	0.02	0.14	0.00	0.02	0.07	226,955
Young firm dummy (age$<$ 3 years)	0.06	0.23	0.00	0.00	0.00	226,966
Exporter dummy	0.07	0.25	0.00	0.00	0.00	226,966
Panel B: Year 2004						
Bank-firm variables						
Credit_growth$^b_{f,2004,2003}$	12.36	56.41	-16.02	-2.90	20.53	810,791
Length of firm-bank relat. (months)	43.76	35.69	12.00	36.00	84.00	1,013,000
Past defaults	0.10	0.30	0.00	0.00	0.00	1,013,000
Bank variables						
Boom exposure (E^b_{2004})	0.47	0.17	0.35	0.48	0.60	146
log total assets	13.69	2.32	11.91	13.56	15.61	181
Capital ratio	0.11	0.16	0.05	0.07	0.10	181
Liquidity ratio	0.09	0.08	0.03	0.08	0.13	181
Default rate	0.007	0.010	0.003	0.005	0.008	181
Firm variables						
Credit_growth$_{f,t}$	14.69	67.95	-21.56	-2.90	27.53	470,254
Demand shock	0.83	1.03	0.39	0.59	0.84	460,804
Total assets (thousands euros)	1680	4511	161	410	1171	318,025
Number employees	17.24	210.90	3.00	6.00	12.00	318,025
Own funds over total assets	0.39	0.29	0.14	0.34	0.61	318,025
Return on assets	0.02	0.15	-0.01	0.02	0.07	318,016
Young firm dummy (age$<$ 3 years)	0.06	0.24	0.00	0.00	0.00	318,025
Exporter dummy	0.05	0.22	0.00	0.00	0.00	318,025

Table A.2: Summary statistics - continued

Panel C: Year 2008	Mean	Std. Dev.	25th pctile	Median	75th pctile	# obs.
Bank-firm variables						
$\text{Credit\_growth}^{b}_{f,2008,2007}$	10.09	48.28	−11.91	2.64	14.72	1,074,000
Length of firm-bank relat. (months)	57.50	48.37	12.00	48.00	96.00	1,251,000
Past defaults	0.10	0.30	0.00	0.00	0.00	1,251,000
Bank variables						
Boom exposure (E^{b}_{2008})	0.54	0.15	0.46	0.53	0.66	149
log total assets	14.16	2.45	12.18	14.03	16.16	175
Capital ratio	0.12	0.18	0.05	0.07	0.10	175
Liquidity ratio	0.09	0.08	0.02	0.07	0.13	175
Default rate	0.018	0.015	0.006	0.016	0.027	175
Firm variables						
$\text{Credit\_growth}_{f,t}$	5.94	56.83	−20.47	−0.25	7.10	616,226
Demand shock	−0.24	0.84	−0.54	−0.38	−0.22	606,924
Total assets (thousands euros)	2048	5085	199	519	1497	387,673
Number employees	16.78	215.00	3.00	5.00	12.00	387,673
Own funds over total assets	0.41	0.30	0.15	0.36	0.66	387,673
Return on assets	0.00	0.16	−0.02	0.01	0.05	387,643
Young firm dummy (age< 3 years)	0.03	0.17	0.00	0.00	0.00	387,673
Exporter dummy	0.05	0.22	0.00	0.00	0.00	387,673

A.3 Additional results and robustness checks

A.3.1 Bank fixed effects

In this section, we introduce bank fixed effects in our loan-level regressions, by estimating

$$\text{Credit\_growth}^b_{f,t} = \beta_1 D_{2002-2003,t}\mathrm{E}^b_{2000} + \beta_2 D_{2005-2008,t}\mathrm{E}^b_{2000} + \mu_b + \mu_{f,t} + u^b_{f,t}, \tag{21}$$

where $\text{Credit\_growth}^b_{f,t}$ stands for the growth rate of the credit of non-housing firm f with bank b between year $t-1$ and year t. μ_b are bank fixed effects and $\mu_{f,t}$ are firm-time fixed effects. Finally, $D_{2002-2003,t}$ is a dummy equal to one if the year t is between 2002 and 2003, and $D_{2005-2008,t}$ is defined analogously. We estimate this equation for $t \in \{2002, 2003, ..., 2008\}$, i.e., for annual credit growth rates between 2001 and 2008 (the same time period as in Figures 3 and 4).

Column (1) of Table A.3 shows that crowding-out and crowding-in patterns are preserved: in 2001-2002 and 2002-2003, non-housing credit growth is lower at more exposed banks (with respect to the average non-housing credit growth at the bank over the entire period 2001-2008). Conversely, during 2004-2008, non-housing credit growth is higher at more exposed banks.[46] Column (2) shows that results are preserved when introducing bank controls interacted with the same subperiod dummies. Finally, Columns (3)-(6) replace firm-time fixed effects by firm controls, both for the full sample and for the sample of multibank firms.

Table A.3: Bank exposure and loan-level non-housing credit growth, bank fixed effects

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	(1)	(2)	(3)	(4)	(5)	(6)
$D_{2002-2003}\mathrm{E}^b_{2000}$	-1.43^{**}	-1.58^{**}	-1.90^{***}	-1.77^{**}	-1.79^{***}	-1.70^{**}
(s.e.)	(0.52)	(0.58)	(0.34)	(0.55)	(0.42)	(0.59)
$D_{2005-2008}\mathrm{E}^b_{2000}$	2.49^{**}	2.27^{***}	1.75^{*}	1.67^{**}	1.96^{**}	1.88^{**}
(s.e.)	(0.74)	(0.61)	(0.73)	(0.62)	(0.72)	(0.59)
Average dep. variable	7.84	7.84	9.52	9.52	10.24	10.24
Firm-time FE	YES	YES	NO	NO	NO	NO
Bank FE	YES	YES	YES	YES	YES	YES
Firm controls	NO	NO	YES	YES	YES	YES
Bank controls	NO	YES	NO	YES	NO	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind.×munic. FE	NO	NO	YES	YES	YES	YES
Balance-sheet data	NO	NO	YES	YES	YES	YES
R-sq	0.40	0.41	0.26	0.26	0.27	0.27
# observations	2,621,396	2,621,396	2,437,477	2,437,477	1,977,966	1,977,966
# firms	251,646	251,646	366,560	366,560	233,662	233,662
# banks	137	137	137	137	137	137

Notes: All regressions are based on Equation (21), estimated by WLS. Bank exposure (E^b_{2000}) is measured by the share of housing loans in total bank loans in 2000, and normalized to have zero mean and unit variance. Columns (1)-(2) and (5)-(6) are estimated for a sample of firms which borrow from at least two banks (multibank firms). Bank, firm and firm-bank controls are listed in Table 1. Standard errors multi-clustered at the bank and firm level are shown in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

[46] Our results are unchanged if we instead consider a dummy for the period 2004-2007.

A.3.2 Pretrend regressions

As discussed in the main text, our sample starts in the year 2000, because of the merger wave in the Spanish banking system in the late 1990s. Even though the bulk of the increase in house prices took place after 2000 (see Figure 1), one could still worry that the housing boom had already started earlier. In this section, we examine this concern by reducing our sample to banks which were unaffected by mergers and acquisitions, and estimating our basic loan-level regression given in Equation (2) for the periods 1996-1998 and 1998-2000. As Table A.4 shows, our point estimates for these regressions are close to zero and insignificant. This indicates that there were no pretrends: exposure had no effect on non-housing credit growth before the housing boom started in earnest.

Table A.4: Bank exposure and loan-level non-housing credit growth, pretrend regressions

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	1996-1998	1998-2000	1996-1998	1998-2000	1996-1998	1998-2000
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	−0.14	1.11	0.01	0.05	−0.03	0.25
(s.e.)	(1.38)	(1.19)	(1.39)	(1.06)	(1.45)	(1.13)
Average dep. variable	19.39	13.76	27.49	18.13	27.82	19.07
Firm fixed effects	YES	YES	NO	NO	NO	NO
Firm controls	NO	NO	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind. × munic. FE	NO	NO	YES	YES	YES	YES
Balance-sheet data	NO	NO	YES	YES	YES	YES
R-sq	0.55	0.53	0.45	0.43	0.45	0.43
# observations	76,621	95,051	56,627	84,023	50,377	71,993
# firms	31,719	39,134	33,674	51,758	27,424	39,728
# banks	103	103	103	103	103	103

Notes: All results are based on Equation (2), and consider a sample of banks that were not affected by the merger wave of the late 1990s. See Table 1 for further details. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A.3.3 Alternative measures of housing exposure

Our baseline results use the share of bank credit going to housing in 2000 as a proxy for their exposure to the housing boom. In this section, we consider two alternative proxies.

First, we follow Chakraborty et al. (2018) and consider the geographical distribution of bank activity, assuming that banks are more exposed if they operate in municipalities that are prone to stronger housing booms. To generate an exogenous source of variation in housing prices, we rely on municipal housing supply elasticities (HSEs), which were first introduced by Saiz (2010) for the United States, and by Basco and Lopez-Rodriguez (2017) for Spain. More precisely, we measure land unavailability (which can be seen as the inverse of HSE), defined as the ratio of built urban surface over the potential plot surface, and computed using census data from the Spanish Cadastre (Catastro) in the year 2000.[47] We then define a bank-specific exposure measure as

$$\mathrm{E}_{2000}^{b,LU} = \sum_{m} \omega_{m,2000}^{b} LU_{m,2000}, \tag{22}$$

where $\omega_{m,2000}^{b}$ refers to the share of total credit of bank b in municipality m[48] and $LU_{m,2000}$ is the land unavailability ratio for municipality m in 2000. This measure is expected to be positively associated with the housing boom, as municipalities with less available land should have higher housing price increases. The average value of the land availability measure is 0.053, and its standard deviation is 0.204.

Table A.5 reports the estimates for this alternative exposure measure, using our baseline specification given by Equation (2). The structure of Table A.5 is analogous to that of Table 1 and the estimated effects are also similar, albeit smaller in magnitude and somewhat less significant.

Table A.5: Bank exposure and loan-level non-housing credit growth, geographical exposure

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure ($\mathrm{E}_{2000}^{b,LU}$)	-1.28^{+}	2.92^{**}	-1.34^{+}	2.71^{**}	-1.48^{*}	2.66^{**}
(s.e.)	(0.81)	(1.23)	(0.81)	(1.13)	(0.87)	(1.22)
Average dep. variable	11.80	17.46	15.96	20.89	17.02	21.97
Firm fixed effects	YES	YES	NO	NO	NO	NO
Firm controls	NO	NO	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind. × munic. FE	NO	NO	YES	YES	YES	YES
Balance-sheet data	NO	NO	YES	YES	YES	YES
R-sq	0.48	0.50	0.34	0.34	0.35	0.35
# observations	276,839	247,153	243,452	247,529	202,801	201,523
# firms	97,353	85,878	124,594	130,552	83,943	84,546
# banks	132	127	132	127	132	127

Notes: See notes to Table 1. Bank exposure is measured by the land unavailability ratio defined in Equation (22). $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Second, we can also measure banks' exposure by their ratio of mortgage-backed credit over total credit in 2000. Table A.6 shows that our results are preserved under this alternative measure.

[47] Potential plot surface includes all available land for construction. It excludes protected non-urban areas (e.g. rivers or natural parks) and public goods land (e.g. local surface covered by transport infrastructure and utilities).

[48] This share can be constructed by matching the CIR to our firm-level data, which includes zipcodes of firms' headquarters.

Table A.6: Bank exposure and loan-level non-housing credit growth, Mortgage-backed exposure

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure ($E_{2000}^{b,MG}$)	-2.00^{**}	4.02^{***}	-1.69^{*}	3.89^{***}	-1.85^{*}	3.86^{***}
(s.e.)	(0.95)	(1.20)	(0.90)	(0.99)	(0.97)	(1.07)
Average dep. variable	11.79	17.46	15.94	20.88	17.00	21.97
Firm fixed effects	YES	YES	NO	NO	NO	NO
Firm controls	NO	NO	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind. × munic. FE	NO	NO	YES	YES	YES	YES
Balance-sheet data	NO	NO	YES	YES	YES	YES
R-sq	0.48	0.50	0.34	0.34	0.35	0.35
# observations	277,280	247,578	243,867	247,971	203,106	201,813
# firms	97,501	86,027	124,795	130,798	84,034	84,640
# banks	152	145	137	137	137	137

Notes: See notes to Table 1. Bank exposure is measured by the ratio of mortgage-backed credit to total credit in 2000. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A.3.4 Sample without public savings banks

Public savings banks (*cajas*) represented a large share of overall credit in Spain during the housing boom, and expanded substantially during the period. However, they operated under a different institutional framework than "regular" commercial banks, and were often controlled by local politicians (see Santos, 2017a). Moreover, they were also on average more exposed to housing than commercial banks. Table A.7 presents our baseline estimates in a sample without public savings banks. Results are even stronger than in the full sample, showing that public savings banks do not drive our results.

Table A.7: Bank exposure and loan-level non-housing credit growth, sample without savings banks

	Firm fixed effects		Firm controls		Firm controls (multib.)	
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	-3.95^{***}	6.12^{***}	-3.47^{***}	6.17^{***}	-3.72^{***}	6.23^{***}
(s.e.)	(1.30)	(1.33)	(1.21)	(1.28)	(1.29)	(1.29)
Average dep. variable	13.51	19.15	17.74	21.80	18.94	22.82
Firm fixed effects	YES	YES	NO	NO	NO	NO
Firm controls	NO	NO	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
Ind. × munic. FE	NO	NO	YES	YES	YES	YES
Balance-sheet data	NO	NO	YES	YES	YES	YES
R-sq	0.53	0.54	0.39	0.38	0.40	0.39
# observations	159,300	139,322	153,559	159,089	127,982	129,176
# firms	60,361	52,228	88,100	93,911	62,523	63,998
# banks	33	30	33	30	33	30

Notes: See notes to Table 1. We exclude public savings banks (*cajas*) from the sample. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A.3.5 Geographical clustering

Table A.8 shows our results when estimating Equation (2) for three different subsamples: a sample of national banks (defined as banks operating in at least 15 of Spain's 50 provinces), a sample of non-housing firms located in the 25 provinces that experienced the highest growth in house prices between 2000 and 2007, and a sample of non-housing firms located in the remaining 25 provinces. Our baseline results hold in all three subsamples.

Table A.8: The role of geographical clustering

	National banks		High housing price growth		Low housing price growth	
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	-3.12^{***}	5.21^{***}	-2.66^{**}	3.66^{***}	-1.97^{*}	7.25^{***}
(s.e.)	(0.97)	(1.26)	(1.28)	(1.41)	(1.15)	(1.45)
Average dep. variable	11.90	17.68	18.08	21.56	15.34	21.64
Firm fixed effects	YES	YES	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES	YES	YES
R-sq	0.49	0.51	0.46	0.49	0.47	0.50
# observations	252,613	225,303	101,545	97,566	55,443	60,636
# firms	89,549	78,943	33,906	32,354	19,303	20,680
# banks	53	54	129	123	104	96

Notes: The table reports estimates from Equation (2) for different subsamples. National banks are those operating in more than 15 provinces. Columns (3) and (4) limit the sample to firms located in the 25 provinces with the highest housing price growth between 2000 and 2007, while columns (5) and (6) limit the sample to the remaining 25 provinces. See notes to Table 1 for further details. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A.3.6 The extensive margin of credit

To account for the extensive margin of credit growth, we first follow Chodorow-Reich (2014) and consider a measure of credit growth of firm f with bank b between year t_0 and year t_1 that incorporates the creation of new lending relationships and the termination of existent loans:

$$\text{Extensive\_Credit\_growth}^b_{f,t_0,t_1} = 100 \cdot \frac{q^b_{f,t_1} - q^b_{f,t_0}}{0.5 \cdot (q^b_{f,t_1} + q^b_{f,t_0})} \tag{23}$$

This definition yields a growth measure that is symmetric around zero and bounded between -200 and 200, providing an integrated treatment of new loans, ended loans, and continuing loans. Second, we analyze how banks' boom exposure affects the probability of creating a new credit relationship by considering as dependent variable a dummy that takes the value one if a given bank-firm (loan) pair was not active in year t_0 but it is active in year t_1 ($\text{New\_loan}^b_{f,t_0,t_1}$).

Table A.9 presents the results for the two subperiods 2001-2003 and 2004-2007, using our baseline exposure measure. Columns (1)-(2) consider the extensive-margin growth rate defined in Equation (23) as dependent variable. The crowding-out estimate in column (1) is similar to that of Table 1, albeit less significant. On the other hand, the crowding-in estimate for the 2004-2007 period is somewhat larger than in the baseline. Columns (3)-(4) in Table A.9 consider New_loan as the dependent variable. Banks more exposed to the boom are less likely to start a new lending relationship with non-housing firms in the 2001-2003 period, even though the point estimate is only marginally significant. In contrast, those banks are significantly more likely to do so between 2004 and 2007.

Table A.9: Bank exposure and loan-level non-housing credit growth, the extensive margin

	Extensive_Credit_growth		New_loan	
	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)
Bank exposure (E^b_{2000})	-2.63^{+}	9.58^{***}	-0.006^{+}	0.02^{***}
(s.e.)	(-1.83)	(1.26)	(0.004)	(0.004)
Average dep. variable	6.79	18.08	0.27	0.37
Firm fixed effects	YES	YES	YES	YES
Firm controls	NO	NO	NO	NO
Bank controls	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES
Ind. × munic. FE	NO	NO	NO	NO
Balance-sheet data	NO	NO	NO	NO
R-sq	0.51	0.68	0.58	0.69
# observations	610,413	803,718	610,413	803,718
# firms	196,644	251,971	196,644	251,971
# banks	135	129	135	129

Notes: See notes to Table 1. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A.3.7 The Spanish banking crisis

In this section, we present some results for the period 2008-2011, when the Spanish housing boom had given way to a severe banking crisis. We end our analysis in 2011, as the resolution of the crisis lead to a large amount of mergers and bank failures (see Santos, 2017b) which make it difficult to track bank identities beyond that year.

Table A.10 shows the results of our baseline loan-level regressions for the banking crisis period. It is the equivalent of Table 1 in the main text, for the time period 2008-2011. The results indicate a large negative effect of housing exposure: during the banking crisis, the same non-housing firm had substantially lower credit growth at more exposed banks.

Table A.10: Bank exposure and loan-level non-housing credit growth, banking crisis

	Firm fixed effects 2008-2011	Firm controls 2008-2011	Firm controls (multib.) 2008-2011
	(1)	(2)	(3)
Bank exposure (E^b_{2000})	-7.51^{***}	-7.59^{***}	-7.63^{***}
(s.e.)	(1.97)	(1.85)	(1.89)
Average dep. variable	5.74	8.53	9.02
Firm fixed effects	YES	NO	NO
Firm controls	NO	YES	YES
Bank controls	YES	YES	YES
Firm-bank controls	YES	YES	YES
Ind.×munic. FE	NO	YES	YES
Balance-sheet data	NO	YES	YES
R-sq	0.46	0.34	0.35
# observations	217,793	205,180	163,274
# banks	85	85	85

Notes: see Table 1. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table A.11 shows the results of our baseline firm-level regression for the banking crisis period. It is the equivalent of Table 3 in the main text, for the time period 2008-2011. Again, we find a large negative effect of exposure: firms that are more exposed to exposed banks see substantially larger contractions in credit during the banking crisis years.

Overall, these results indicate that the end of the housing boom disproportionately affected banks that were more exposed to housing, and that firms with stronger links to these banks suffered. These results are very much in line with the existing evidence on the Spanish banking crisis (see Bentolila et al., 2017; Santos, 2017b). They are also very much in line with our emphasis on financial transmission. Indeed, the negative effect of exposure during the crisis is consistent with our model, assuming that the banking crisis triggered a large fall in the net worth of banks that were more exposed to housing. However, studying this effect more comprehensively (and quantifying it) is beyond the scope of our paper.

Table A.11: Boom exposure and credit growth at the firm level, banking crisis

	All firms 2008-2011	Multibank firms 2008-2011
	(1)	(2)
Firm exposure (E_{f,t_0})	-5.33^{***}	-5.87^{***}
(s.e.)	(0.98)	(1.31)
Average dep. variable	11.20	16.03
Firm controls	YES	YES
Firm-bank controls	YES	YES
Industry × municipality FE	YES	YES
Balance-sheet data	YES	YES
R-sq	0.53	0.53
# observations	96,776	53,773

Notes: See Table 3. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A.3.8 Robustness of firm-level results to alternative boom exposure measures

Table A.12 reports the results for our baseline firm-level regression (specified in Equation (4)) when using the geographical exposure measure defined in Appendix A.3.3. That is, we still compute firm exposure according to Equation (3), but the bank exposure measure E^{b}_{2000} is substituted by $E^{b,LU}_{2000}$. Estimates are similar to our baseline results shown in the main text.

Table A.12: Boom exposure and credit growth at the firm level, alternative exposure measure

	All firms		Multibank firms	
	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)
Firm exposure (E^{LU}_{f,t_0})	-1.94^{+}	3.00^{**}	-2.94^{***}	2.69^{+}
(s.e.)	(1.22)	(1.56)	(1.41)	(1.75)
Average dep. variable	23.04	31.05	32.94	43.39
Firm controls	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES
Industry × municipality FE	YES	YES	YES	YES
Balance-sheet data	YES	YES	YES	YES
R-sq	0.57	0.55	0.58	0.55
# observations	82,401	96,799	48,944	54,950

Notes: All regressions are based on Equation (4). Firm exposure is standardized to have zero mean and unit variance. Firm controls are total assets, number of employees, own funds over total assets, return on assets, a dummy for firms younger than three years, and a dummy for exporters. Standard errors multi-clustered at the main bank and industry-municipality level in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

A.3.9 Further results for cash-flow loans

Table A.13 reproduces Table 5 for the sample of cash-flow loans.

Table A.13: Bank exposure, constrained versus unconstrained banks, cash-flow loans

	2001-2003		2004-2007	
	Constrained	Unconstrained	Constrained	Unconstrained
	(1)	(2)	(3)	(4)
Bank exposure (E^b_{2000})	-3.43^{***}	5.93^{*}	2.20^{+}	1.89
(s.e.)	(0.71)	(2.90)	(1.53)	(4.27)
Average dep. variable	6.47	3.49	17.08	13.75
Firm fixed effects	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES
Firm-bank controls	YES	YES	YES	YES
Multiple banks per firm	YES	YES	YES	YES
R-sq	0.51	0.62	0.55	0.64
# observations	96,833	5,323	100,697	3,798
# firms	37,582	2,584	38,370	1,839
# banks	58	19	56	18

Notes: All regressions are based on Equation (2). Unconstrained banks are banks in the lowest quartile of the bank leverage ratio in the first year of the period, constrained banks are all others. Standard errors in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

A.3.10 Further alternative explanations for the crowding-in effect

Table A.14 shows the results of our analysis using credit supply shocks identified with the Amiti and Weinstein (2018) methodology. Table A.15 introduces securitization controls into our regression of net worth growth on bank exposure to the housing boom.

Table A.14: Bank exposure and securitization: Credit supply analysis

Dep. variable is bank credit supply identified based on Amiti and Weinstein (2018)						
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Bank exposure (E^b_{2000})	-0.30^{**}	0.38^{***}	-0.30^{**}	0.32^{***}	-0.30^{**}	0.29^{**}
(s.e.)	(0.12)	(0.10)	(0.13)	(0.12)	(0.13)	(0.11)
Securitization level			0.03	0.25		
(s.e.)			(0.53)	(0.25)		
Securitization change					-0.12	0.77^{*}
(s.e.)					(0.96)	(0.40)
R-sq	0.15	0.23	0.15	0.24	0.15	0.26
# observations	136	130	136	130	136	130

Notes: Bank exposure (E^b_{2000}) is measured by the share of housing loans in total bank loans in 2000. Securitization is measured as the ratio of asset backed securities (ABS) and covered bonds over total assets in the first year of the period. Bank controls are the natural logarithm of total assets, capital ratio, liquidity ratio, and default rate. Standard errors in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table A.15: Bank exposure and net worth growth: controlling for securitization

Dep. variable is growth in bank net worth						
	2001-2003	2004-2007	2001-2003	2004-2007	2001-2003	2004-2007
	(1)	(2)	(4)	(5)	(7)	(8)
Bank exposure (E^b_{2000})	0.07	0.63^{***}	0.10	0.59^{***}	0.07	0.58^{***}
(s.e.)	(0.09)	(0.12)	(0.10)	(0.15)	(0.10)	(0.14)
Securitization level			-0.56	0.12		
(s.e.)			(0.43)	(0.29)		
Securitization change					-0.33	0.38
(s.e.)					(0.78)	(0.48)
R-sq	0.45	0.62	0.45	0.62	0.45	0.62
# observations	140	136	140	136	140	136

Notes: Bank boom exposure (E^b_{2000}) is measured by the share of housing loans in total bank loans in 2000. Securitization is measured as the ratio of asset backed securities (ABS) and covered bonds over total assets in the first year of the period. Bank controls are the natural logarithm of total assets, capital ratio, liquidity ratio, and default rate, measured in the initial year of the period. Standard errors in parentheses. $^{+}$ $p < 0.15$; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

B (Online) Model Appendix

B.1 Further details on the model and the calibration

B.1.1 Additional derivations

Credit demand In every period t, firm ω from sector j demands credit from the B banks in the economy. To pay for this credit, the firm promises each bank b a fraction $m^b_{j,t+1}(\omega)$ of its capital income in period $t+1$. Therefore, the cost minimization problem of the firm is given by

$$\begin{aligned}
&\min_{m^b_{j,t+1}(\omega)} \left(\mathbb{E}_t\left(\textstyle\sum_{b=1}^{B} m^b_{j,t+1}(\omega)\, p^K_{j,t+1}(\omega)\, k_{j,t+1}(\omega)\right)\right) \\
&\text{such that} \quad k_{j,t+1}(\omega) = \left(\textstyle\sum_{b=1}^{B} \left(\pi^b_j(\omega)\right)^{\frac{1}{\eta_j}} \left(q^b_{j,t}(\omega)\right)^{\frac{\eta_j}{\eta_j-1}}\right)^{\frac{\eta_j}{\eta_j-1}}, \\
&\forall b,\ \mathbb{E}_t\left(\frac{m^b_{j,t+1}(\omega) p^K_{j,t+1}(\omega) k_{j,t+1}(\omega)}{q^b_{j,t}(\omega)}\right) = R^b_{t+1}, \\
&\textstyle\sum_{b=1}^{B} m^b_{j,t+1}(\omega) \leq 1.
\end{aligned} \tag{24}$$

That is, firms minimize the expected amount of resources they need to pay to banks tomorrow, subject to three constraints: their capital tomorrow is given by the production function, the expected return on a credit contract with bank b must be equal to R^b_{t+1}, and they cannot promise more than their income. Note that because of perfect competition, firms take the expected future price of their product and the expected returns requested by banks as given.

The constraint on required returns for each bank b implies that $\mathbb{E}_t\left(m^b_{j,t+1}(\omega)\, p^K_{j,t+1}(\omega)\, k_{j,t+1}(\omega)\right) = R^b_{t+1} q^b_{j,t}(\omega)$. Therefore, we can substitute this constraint into the objective function, and obtain the problem shown in Equation (12) in the main text. That problem omits the constraint that firms cannot promise more than their income, but it is easy to verify that this always holds (see next paragraph).

Credit repayments From the above, it is easy to see that the fraction of future income promised by firm ω of sector j to bank b holds

$$m^b_{j,t+1}(\omega) = \frac{R^b_{t+1} q^b_{j,t}(\omega)}{\mathbb{E}_t\left(p^K_{j,t+1}(\omega)\right) k_{j,t+1}(\omega)}. \tag{25}$$

Summing across all banks, we get

$$\sum_{b=1}^{B} m^b_{j,t+1}(\omega) = \frac{\sum_{b=1}^{B} R^b_{t+1} q^b_{j,t}(\omega)}{\mathbb{E}_t\left(p^K_{j,t+1}(\omega)\right) k_{j,t+1}(\omega)}. \tag{26}$$

The numerator of this expression is the total cost of credit of firm ω of sector j. By definition of the firm's marginal cost (the firm's ideal price index for credit), it is equal to $R_{j,t+1}(\omega)\, k_{j,t+1}(\omega)$. Furthermore, as discussed in the main text, perfect competition implies that in equilibrium, $R_{j,t+1}(\omega) = \mathbb{E}_t\left(p^K_{j,t+1}(\omega)\right)$. Thus, we finally have $\sum_{b=1}^{B} m^b_{j,t+1}(\omega) = 1$. This result is intuitive: because of perfect competition, firms promise their entire future capital income to banks and make no profits.

Finally, using Equation (25), it is easy to show that the actual repayment received by bank b in period $t+1$ holds

$$m^b_{j,t+1}(\omega)\, p^K_{j,t+1}(\omega)\, k_{j,t+1}(\omega) = R^b_{t+1} q^b_{j,t}(\omega) \frac{p^K_{j,t+1}(\omega)}{\mathbb{E}_t\left(p^K_{j,t+1}(\omega)\right)} = R^b_{t+1} q^b_{j,t}(\omega) \frac{A_{j,t+1} P_{j,t+1}}{\mathbb{E}_t\left(A_{j,t+1} P_{j,t+1}\right)}, \tag{27}$$

where we have used Equation (11) and the fact that capital stocks at period $t+1$ are known in period t.

Law of motion of bank net worth Lagging Equation (27) by one period, and aggregating across all firms of a given sector, we get that the total repayments of bank b from firms of sector j at time t are given by $R_t^b Q_{j,t-1}^b \frac{A_{j,t}P_{j,t}}{\mathbb{E}_{t-1}(A_{j,t}P_{j,t})}$. Old bankers collect these credit repayments, and pay back the credit that they obtained from the rest of the world. As the financial constraint of bankers is always binding, their total borrowing from the IFM in period $t-1$ is equal to $(\lambda - 1) W_{t-1}^b$. These considerations directly yield the law of motion of bank net worth given in Equation (18) in the main text.

B.1.2 Housing prices and the income of the old

The income of old agents is given by

$$\sum_{j \in \{N,H\}} \left((1-\phi)\left(\alpha_j P_{j,t} Y_{j,t} - R^* Q_{j,t-1}\right) + R^* (1-\alpha_j) P_{j,t-1} Y_{j,t-1} \right). \tag{28}$$

This expression is intuitive. The only old agents with a positive income are bankers and workers (as entrepreneurs do not make profits, they have no old-age income). Old bankers collect the economy's entire capital income, repay their loans to the IFM, and keep a fraction $1-\phi$ of their profits (the remainder being paid out to young bankers). Workers save their entire labor income at the international interest rate R^* and collect the proceeds when old.

Throughout the paper, we consider equilibria in which the income of old agents exceeds the value of housing output ($P_{H,t}Y_{H,t}$) in every possible state of the world, implying that $P_{H,t} = \xi_t$ in every state of the world. To impose this condition, we assume that there is an upper bound for housing price increases. Then, it is sufficient to check that for every period t, even if housing prices were to reach their highest possible value, the income of the old would still be higher than the value of housing output.

Note that we have assumed that housing price growth follows an AR(1) process, which implies that housing prices are in principle unbounded. However, we can approximate the stochastic process for housing price growth by a finite-state Markov chain, bounded by definition. We consider an upper bound of 25% per year, substantially higher than the highest realization of housing price growth in our baseline calibration (which is 8.3%).[49]

B.1.3 Balanced growth path solution

We define the balanced growth path (BGP) of our model as the equilibrium that applies when productivity in both sectors grows at a constant rate g_A (i.e., $\frac{A_{H,t}}{A_{H,t-1}} = \frac{A_{N,t}}{A_{N,t-1}} = 1 + g_A$ for every t), and housing preferences are constant over time and normalized to 1 for convenience (i.e., $\xi_t = 1$ for every t). In this section, we show that on the BGP, credit, investment and net worth grow at a constant rate g, while interest rates are constant. For simplicity, we assume $\alpha_N = \alpha_H = \alpha$, as in our calibration.

The BGP equilibrium Using Equation (18), we get that the net worth of bank b on the BGP holds

$$\widehat{W}_t^b = \frac{\phi \widehat{R}^b \left(\widehat{Q}_{N,t-1}^b + \widehat{Q}_{H,t-1}^b \right)}{1 + \frac{\phi R^* (\lambda - 1)}{1+g}},$$

where $\widehat{X}_t$ stands for the BGP value of variable X in period t. Combining this expression with the credit market clearing condition in Equation (17), we get

$$\widehat{R}^b = \frac{1 + g + \phi R^* (\lambda - 1)}{\lambda \phi}. \tag{29}$$

[49] An even simpler alternative would be to assume that there is an additional category of agents in the economy that have the same preferences as all others, but receive their income from abroad (e.g., pensioners from Northern Europe). This would not affect any of our results, but by making the income of these agents arbitrarily large, it would be assured from the outset that income is always sufficient to buy housing output.

That is, every bank charges the same interest rate on the BGP. This is a consequence of the linearity of our model: each bank's credit supply is linear in net worth, and net worth is linear in credit demand. As interest rates are constant, Equations (14) to (16) immediately imply that credit for each bank-firm pair, the overall credit of each firm, and aggregate capital grow at rate $g = (1 + g_A)^{\frac{1}{1-\alpha}} - 1$.

Finally, Equation (29) also provides a necessary and sufficient condition for banks' financial constraints to be binding on the BGP. Indeed, it is straightforward to see that $\widehat{R} > R^*$ iff $\phi < \frac{1+g}{R^*}$. We impose this condition throughout.

Credit shares on the BGP Using Equation (14), we can show that on the BGP, total credit to sector j holds $\widehat{Q}_{j,t} = \left(\frac{\alpha \widehat{A}_{j,t+1}}{\widehat{R}}\right)^{\frac{1}{1-\alpha}}$. Accordingly, the BGP ratio of housing to non-housing credit holds

$$\frac{\widehat{Q}_{H,t}}{\widehat{Q}_{N,t}} = \widehat{\left(\frac{A_H}{A_N}\right)}^{\frac{1}{1-\alpha}}, \tag{30}$$

where $\widehat{\frac{A_H}{A_N}}$ is the relative productivity of the non-housing sector (constant on the BGP). For each bank b, the BGP ratio of housing to non-housing credit is

$$\frac{\widehat{Q}^b_{H,t}}{\widehat{Q}^b_{N,t}} = \widehat{\left(\frac{A_H}{A_N}\right)}^{\frac{1}{1-\alpha}} \cdot \frac{\int_0^1 \pi^b_H(\omega)\, d\omega}{\int_0^1 \pi^b_N(\omega)\, d\omega}. \tag{31}$$

This expression shows that a bank is more exposed if it is relatively preferred by housing firms with respect to non-housing firms.

Finally, the BGP share of credit of firm ω of sector j coming from bank b simply holds

$$\frac{\widehat{q}^b_{j,t}(\omega)}{\widehat{q}_{j,t}(\omega)} = \pi^b_j(\omega). \tag{32}$$

These expressions show that credit shares and exposure measures on the BGP only depend on the relative productivity of both sectors and on the distribution of preference weights $\pi^b_j(\omega)$ across firms.

B.1.4 Parameter values for illustrations

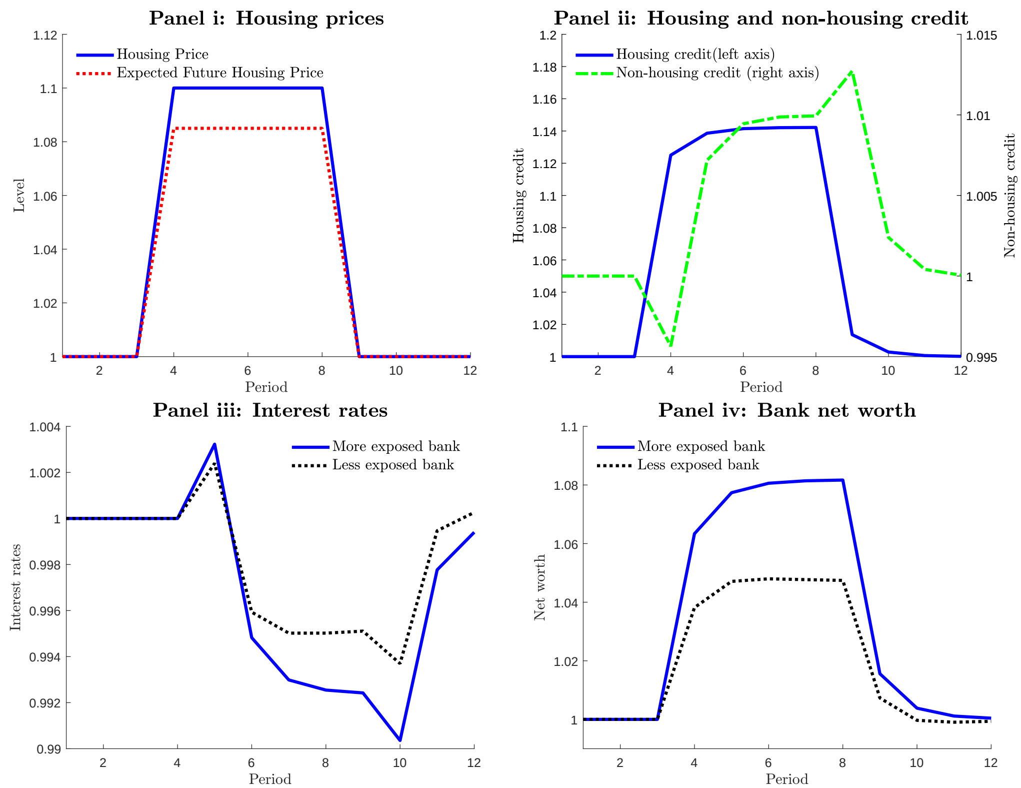
Figure 6 mostly uses the same parameter values as in our baseline calibration, listed in the main text. The only differences with respect to the baseline calibration are that we set $g_A = 0$, and that we consider an arbitrary housing boom. The housing boom shown in Figure 6 consists of five successive periods of increases in the relative price of housing: in the first period, the relative price of housing increases by 5%, and in subsequent periods, it increases by 4%, 3%, 2% and 1%. As stated in the main text, agents perfectly foresee these increases.

B.1.5 A stochastic housing boom

In this section, we briefly illustrate how a stochastic housing boom can trigger a strong crowding-in effect, increasing non-housing credit above its level in the absence of a housing boom.

To do so, we assume that the relative price of housing follows a Markov chain with two possible values, a high and a low one. Figure A.2 illustrates the consequences of a housing boom (a number of periods spent in the state with high housing prices) in this model.

Figure A.2: Illustration: a stochastic housing boom



Notes: These figures illustrate our model's main qualitative predictions for a stochastic housing boom. With the exception of the housing price process, all parameter values are the same as those used for Figure 6 (see Appendix B.1.4). We assume that housing prices are 1 in the low state of the world and 1.1 in the high state of the world, that there is a very small probability to transition from the low to the high state of the world, and a 15% probability to transition from the high to the low state of the world.

Figure A.2 shows that this stochastic boom first crowds out non-housing credit and then crowds it in again, exactly as in the illustration discussed in the main text. However, the crowding-in effect of the stochastic boom is stronger: interest rates eventually fall below their pre-boom level (see Panel iii) and non-housing credit rises above its pre-boom level (see Panel ii). Indeed, as the boom has a positive probability of ending every period, expected future housing prices rise less than realized housing prices. However, credit demand is proportional to expected prices, while net worth (and therefore credit supply) is proportional to realized prices. Thus, the credit supply curve eventually shifts out more than the credit demand curve, lowering the equilibrium interest rate and triggering the strong crowding-in effect.

B.2 Details on the calibration

BGP heterogeneity We set the parameters π_H^1, $\theta_{N,A}$, $\theta_{N,B}$ and $\theta_{N,C}$ in order to match the four BGP moments described in the main text (the exposure of both types of banks, and the average and standard deviation of firm exposure). Precisely, we choose the parameter values which minimize the distance function

$$D = \sum_{m=1}^{4} w_m \left(\frac{\text{Moment}_m\,(\text{Data}) - \text{Moment}_m\,(\text{Model})}{0.5 \cdot (\text{Moment}_m\,(\text{Data}) + \text{Moment}_m\,(\text{Model}))} \right)^2, \tag{33}$$

where w_m are weights for each moment in the distance function. Given the importance of bank exposure, we set $w_m = 100$ for the two bank exposure measures, and $w_m = 1$ for the remaining two moments. Note, however, that Table 10 shows that we match all four moments almost perfectly, so that these weights do not matter much. Note as well that this part of the calibration is independent of the remainder (as the targeted moments only depend on the four parameters to be calibrated, and the predetermined relative productivity of housing) and can therefore be carried out separately.

Main calibration As described in the main text, our model has four internally calibrated parameters: g_A, ζ, ϕ and η_N. To estimate these parameters, we proceed as follows. First, we impose that the share of housing in total credit in 2007 (which identifies ζ) and the increase in non-housing credit between 2000 and 2007 (which identifies g_A) are matched exactly. That is, we only consider parameter combinations (g_A, ζ, ϕ , η_N) for which the model values of these two moments are within 0.05 percentage points of the data targets.[50] For this subset of the parameter space, we minimize the distance function

$$D = \sum_{m=1}^{4} w_m \left(\frac{|\text{Moment}_m\,(\text{Data}) - \text{Moment}_m\,(\text{Model})|}{0.5 \cdot (|\text{Moment}_m\,(\text{Data})| + |\text{Moment}_m\,(\text{Model})|)} \right)^2, \tag{34}$$

where the four moments considered are our cross-sectional regression coefficients (as described in the main text) and w_m are weights for each moment in the distance function.[51] We set $w_m = 1$ for the two loan-level coefficients, and $w_m = 2$ for the two firm-level coefficients, reflecting the fact that firm-level results are more informative about aggregate outcomes than bank-level coefficients, and should therefore be matched more closely. We solve this minimization problem using a Differential Evolution algorithm for MATLAB. The algorithm was developed by Markus Buehren and is available for download at https://it.mathworks.com/matlabcentral/fileexchange/18593-differential-evolution. In order to speed up computations, we impose bounds for all 4 parameters to be calibrated, listed in Table A.16. As can be verified from the results, these bounds are not binding, with the exception of one robustness check (see Appendix B.3.5), in which the lower bound on ϕ is binding. However, note that this lower bound is not arbitrary: it is essentially zero, for a parameter that conceptually needs to be positive.

Table A.16: Bounds for the numerical calibration

Parameter	Lower bound	Upper bound
g_A	0.045	0.065
ζ	0.6	0.7
ϕ	0.001	0.15
η_N	0	8

B.3 Robustness checks and additional results

B.3.1 A calibration with three bank types

In the main text, we assume that there are $B = 2$ bank types. In this section, we consider instead the case with $B = 3$ bank types. We assume that the data equivalents of these bank types are banks above the 66th percentile of exposure (type 1), banks between the 33rd and the 66th percentile of exposure (type 2) and banks below the 33rd percentile of exposure (type 3).

Our calibration of the three-bank model closely follows the one of the two-bank model. The only changes apply to the way in which we model firm heterogeneity. We keep assuming that there is only one type of housing firm, with a preference profile $\left(\pi_H^1, \pi_H^2, 1 - \pi_H^1 - \pi_H^2\right)$. However, we now consider seven different

[50] Imposing such a condition is necessary to prevent the estimation procedure from trading off fit across different moments in a situation in which we have more targets (6) than parameters (4). Precisely, we want to avoid that the algorithm chooses e.g. a higher shock size ζ to match the cross-sectional regressions by overpredicting the increase in the housing share of total credit.

[51] Note that absolute values in Equation (34) are needed because moments may be either negative or positive.

types of non-housing firms, with preference profiles $(0.95, 0.05, 0)$, $(0, 0.95, 0.05)$, $(0.05, 0, 0.95)$, $(1/2, 1/2, 0)$, $(0, 1/2, 1/2)$, $(1/2, 0, 1/2)$ and $(1/3, 1/3, 1/3)$.[52] We calibrate the parameters π_H^1, π_H^2 and the vector $\boldsymbol{\theta}_N$ (giving the mass of each type of non-housing entrepreneur) in order to match the same moments as in the main text: our boom exposure measure for the three types of banks and the average and standard deviation of our non-housing firm exposure measure.

There are now eight free parameters and five moments to match. Thus, without further restrictions, parameters are not identified. To deal with this, we exogenously set the mass of firms with preferences $(1/2, 1/2, 0)$, $(0, 1/2, 1/2)$ and $(1/2, 0, 1/2)$ to 0.05. Table A.17 summarizes the other parameter values.

Table A.17: Calibrated parameters: bank and firm-level heterogeneity, three-bank model

Parameter	Meaning	Value	
π_H^1	BGP share of housing credit obtained from type-1 banks	0.575	
π_H^2	BGP share of housing credit obtained from type-2 banks	0.250	
	Share of non-housing firms with pref. $(0.95, 0.05, 0)$	0.336	
	Share of non-housing firms with pref. $(0, 0.95, 0.05)$	0.212	
	Share of non-housing firms with pref. $(0.05, 0, 0.95)$	0.297	
	Share of non-housing firms with pref. $(1/3, 1/3, 1/3)$	0.005	
Target	**Meaning**	**Model**	**Data**
E_{2000}^1	Share of housing in total credit, type-1 banks	56.6%	56.6%
E_{2000}^2	Share of housing in total credit, type-2 banks	44.4%	44.4%
E_{2000}^3	Share of housing in total credit, type-3 banks	30.9%	30.9%
$\overline{\mathrm{E}}_{f,2000}$	Average value of firm exposure	44.5%	45.8%
$\sigma(\mathrm{E}_{f,2000})$	Standard deviation of firm exposure	9.7%	9.7%

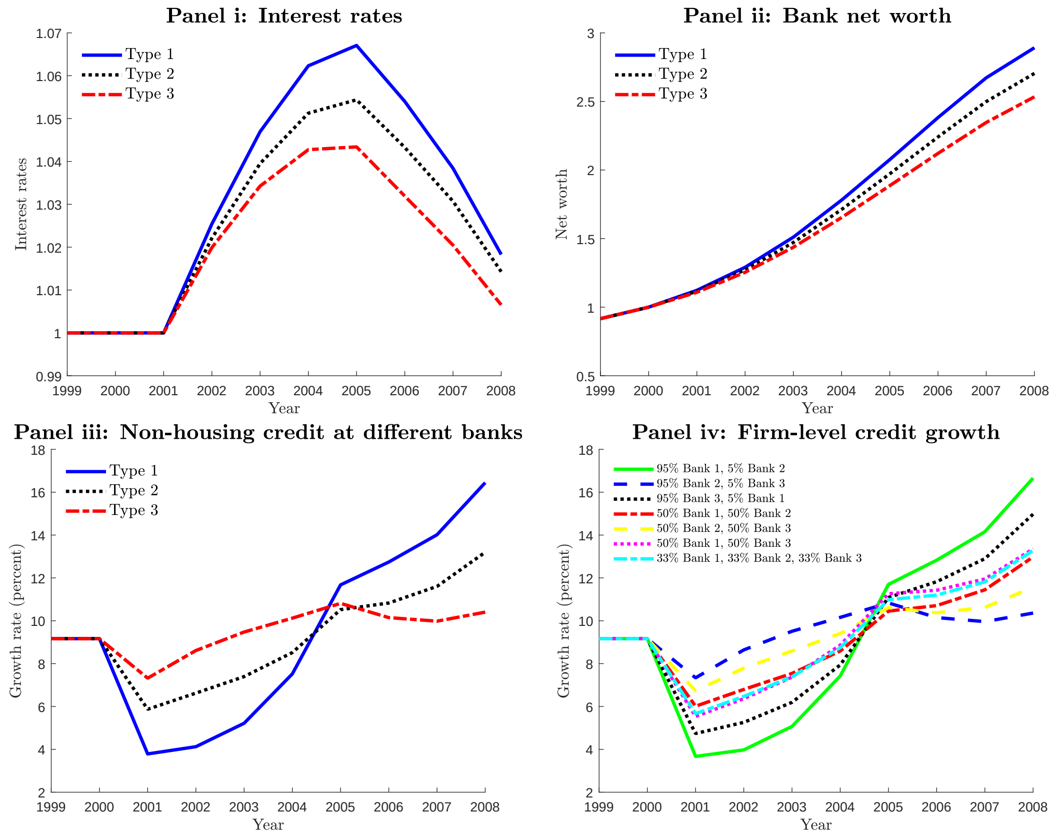
Given this structure, we first consider the predictions of the three-bank model when leaving all other parameters at their baseline values. Figure A.3 plots some key outcomes for this calibration. It shows that all predictions of the baseline model continue to hold: more exposed banks experience a greater initial increase in the interest rate, but also higher net worth accumulation. This triggers subsequent crowding-out and crowding-in effects at the loan and at the firm-level, illustrated in the two lower panels of the figure.

Table A.18 summarizes the quantitative predictions of the model with three bank types. The first column lists the baseline results. The second column instead lists the results for the three-bank model, leaving all parameter values except π_H^1, π_H^2 and $\boldsymbol{\theta}_N$ at their baseline values. It shows that the three-bank model also fits the data well, and that its aggregate implications are virtually identical to the two-bank model. Finally, the third column of Table A.18 shows results when we recalibrate the internal parameters g_A, ζ, ϕ and η_N in the three-bank model. Again, results remain very similar to the ones obtained with the two-bank model.

The slight dampening effect observed in Table A.18 suggests that bank heterogeneity is beneficial: non-housing credit falls less in an economy with more banks. This is a a bit more striking if we consider the results obtained in an economy with one bank (with an exposure equal to the aggregate value). In this single-bank economy, the crowding-out effect increases to -8.1% (full results are available on request). Indeed, with more banks, some non-housing firms (the ones with strong links to less exposed banks) are partly shielded from crowding-out. As the elasticity of substitution between non-housing firms is higher than 1, these shielded firms can make up partly for the lost output and credit of their peers linked to more exposed banks. However, the discussion in this section suggests that the dampening effect of bank heterogeneity is small.

[52] These assumptions imply that all non-housing firms are multibank firms, while in our baseline calibration, 89% of non-housing firms were single-bank firms. Nevertheless, as we show below, results are virtually unaffected, demonstrating that the high fraction of single-bank firms in the baseline calibration was not crucial for our results. Likewise, if we were to allow for single-bank firms in the model with three bank types, our results would not change either.

Figure A.3: The calibrated model with three types of banks



Notes: These figures illustrate some features of our calibrated model with three bank types. Calibrated parameters are listed in Table A.17 and in the main text.

Table A.18: Quantitative results: calibration with three banks

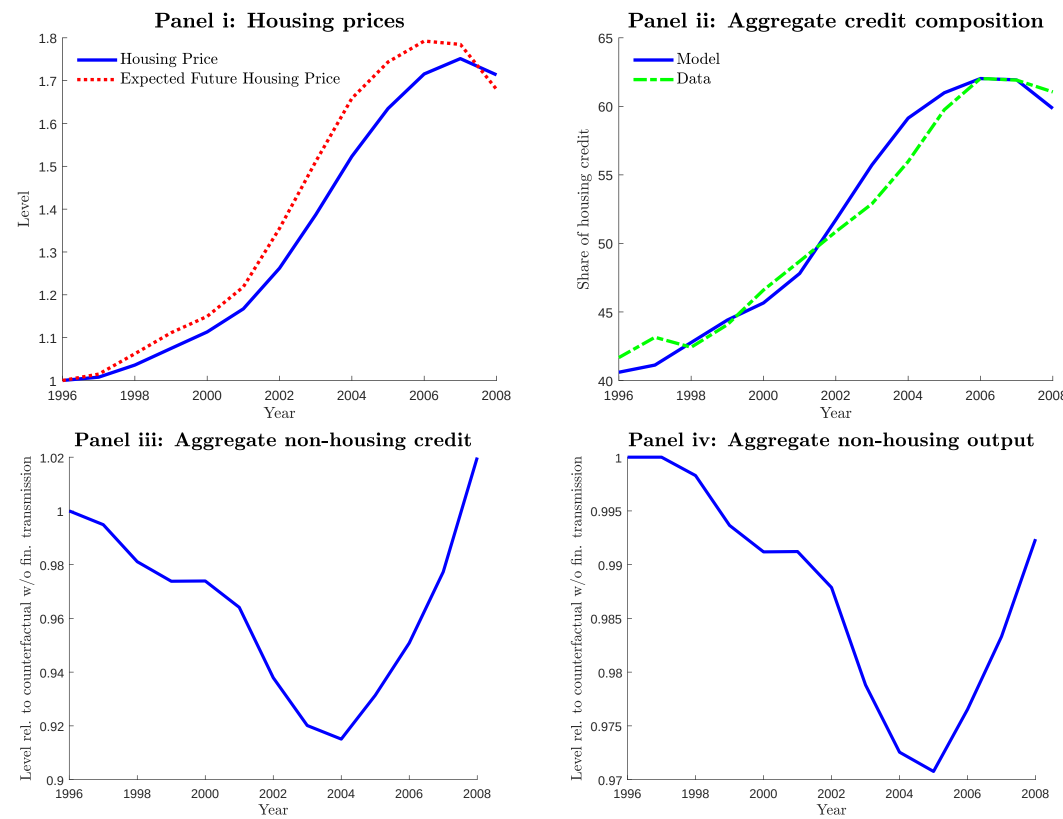
	Baseline	$B = 3$, baseline par.	$B = 3$, recalib.
Parameters			
ϕ	0.019	0.019	0.021
η_N	3.421	3.421	3.857
Targets (model)			
$\beta_{2001-2003}$	-2.85	-2.53	-2.79
$\beta_{2004-2007}$	3.85	3.67	3.99
$\gamma_{2001-2003}$	-2.67	-2.66	-2.60
$\gamma_{2004-2007}$	3.61	3.86	3.71
Level of non-housing credit rel. to counterfactual w/o financial transmission			
2004	-7.7%	-7.6%	-7.5%
2007	-2.0%	-2.0%	-1.9%
2008	$+1.8\%$	$+1.8\%$	$+1.8\%$

B.3.2 A longer housing boom

In our baseline calibration, we consider the time period 2000-2008, just as in our empirical analysis. However, as shown in Figures 1 and 2, housing prices and the housing share of aggregate credit already started to increase in the mid-1990s (even though the bulk of their increase came after 2000). Therefore, this section discusses a robustness check in which we consider the year 1995, and not the year 2000, as representing the pre-boom BGP.

In this robustness check, we use the time series of housing price changes between 1995 and 2008 (rather than the one between 2000 and 2008) as the shock hitting the economy. Furthermore, we reset the BGP relative productivity of housing to match the housing share in aggregate credit in 1995 (40.6%), and the scaling parameter ζ to match the increase in the housing share until 2007 (to 61.9%).[53] The remaining parameters are kept at their baseline calibration values. Figure A.4 illustrates the key results of this calibration. In particular, the third panel shows that the crowding-out effect still reaches its apex in 2004, lowering non-housing credit by 8.5% with respect to its level without financial transmission (rather than 7.7% in the baseline). In 2007, the shortfall is reduced to 2.3% (rather than 2.0% in the baseline), and in 2008, non-housing credit is 2.0% (rather than 1.8% in the baseline) higher than it would have been without financial transmission.

Figure A.4: Calibration with a long housing boom



Notes: These figures illustrate our model's predictions when we consider 1995 as the pre-boom BGP equilibrium and use the data series for housing price increases between 1995 and 2008. A_N and ζ are recalibrated as described in the text, all other parameters are set to their baseline values.

Thus, our estimates for the crowding-out effect of the boom until 2004 and for its net effect are very

[53]The implied parameter values are $A_N = 1.289$ and $\zeta = 0.715$.

similar to our baseline estimates. Indeed, there are two offsetting effects: considering a housing price boom starting in 1995 makes the overall shock larger (which, all else equal, increases the crowding-out effect), but it also lets the boom start with some years of relatively low housing price growth (which, all else equal, lowers the crowding-out effect, as banks accumulate some net worth before the years with the steepest price increases). The effect of a larger shock dominates, but our conclusions are not substantially altered.

B.3.3 Perfect foresight for housing prices

Our baseline calibration assumes that agents have rational expectations for housing price growth (and that housing price growth follows an AR(1) process). As there is - to the best of our knowledge - no systematic data on house price expectations in Spain during the housing boom, we cannot test these assumptions.[54]

To examine the robustness of our conclusions with respect to different assumptions on expectation formation, we assume in this section that agents have perfect foresight with respect to future housing prices. Note that in this case, we do not need to make any assumptions on the stochastic process generating the observed path of house prices. We recalibrate the internal parameters to match the same baseline targets, and keep all other parameters at their baseline values.

Table A.19: Quantitative results: perfect foresight

	Baseline (Rat. Expectations)	Perfect foresight
Parameters		
ϕ	0.019	0.042
η_N	3.421	3.325
Targets (model)		
$\beta_{2001-2003}$	-2.85	-2.24
$\beta_{2004-2007}$	3.85	5.33
$\gamma_{2001-2003}$	-2.67	-2.16
$\gamma_{2004-2007}$	3.61	5.14
Level of non-housing credit relative to counterfactual w/o financial transmission		
2003	-7.3%	-7.4%
2004	-7.7%	-6.0%
2007	-2.0%	$+1.8\%$
2008	$+1.8\%$	$+4.6\%$

Notes: The baseline (first column) corresponds to Table 11. In the second column, we assume that agents have perfect foresight for future housing prices, and recalibrate the parameters g_A, ζ, ϕ and η_N to match the baseline targets.

Table A.19 summarizes the results. It shows that with perfect foresight, we get a crowding-out effect of very similar magnitude than in the baseline calibration (but the trough is reached one year earlier, in 2003 rather than in 2004). However, at the end of the boom, there is a stronger crowding-in effect. This difference is due to the fact that with perfect foresight, agents anticipate a fall in housing prices at the very end of the boom, which lowers housing credit demand and boosts non-housing credit. Overall, however, results remain similar to the ones obtained with our baseline calibration.

[54] García-Montalvo (2006) contains the only survey evidence that we are aware of, but it is limited to the year 2005 and to five cities.

B.3.4 Changes in bank leverage

In our baseline model, we assume that the leverage ratio of any bank is fixed over time. In reality, bank leverage increased during the housing boom, albeit modestly: the median leverage ratio of a bank in our sample increased from 11.56 in 2000 to 12.72 in 2007.[55] In this section, we examine whether this increase matters for our results. To do so, we make the parameter λ time-varying, and let it increase gradually from 11.56 in 2000 to 12.72 in 2007. This can be interpreted as Spanish banks being hit by a series of shocks that progressively loosened their financial constraints over the course of the boom.

Table A.20 illustrates the results for this robustness check. The second column shows our model's prediction with a time-varying leverage ratio, leaving all other parameters at their baseline calibration values. This shows that there is no direct interaction between the changes in the bank leverage ratio and the crowding-out and crowding-in effects: aggregate implications are virtually identical to the baseline.

Table A.20: Quantitative results: increase in leverage

	Baseline (fixed λ)	Increasing λ, baseline par.	Increasing λ, recalib.
Parameters			
ϕ	0.019	0.019	0.019
η_N	3.421	3.421	3.431
Targets (model)			
$\beta_{2001-2003}$	-2.85	-2.93	-2.84
$\beta_{2004-2007}$	3.85	4.06	3.87
$\gamma_{2001-2003}$	-2.67	-2.74	-2.65
$\gamma_{2004-2007}$	3.61	3.80	3.62
Level of non-housing credit relative to counterfactual w/o financial transmission			
2004	-7.7%	-7.7%	-7.6%
2007	-2.0%	-2.0%	-1.9%
2008	$+1.8\%$	$+1.8\%$	$+1.8\%$

Notes: The baseline (first column) corresponds to Table 11. Column (2) and (3) assume that λ increases at a constant rate from 11.56 in 2000 to 12.72 in 2007. In Column (2), all other parameters are at their baseline values, in Column (3), the internally calibrated parameters are recalibrated to match the baseline targets.

The third column of Table A.20 instead shows the results obtained when recalibrating the internal parameters g_A, ζ, ϕ and η_N for the model with increasing leverage. The main effect of this recalibration is to reduce the productivity growth rate g_A (as part of aggregate credit growth is now explained by the increase in bank leverage). However, this hardly affects our estimates for ϕ and η_N, or our aggregate conclusions.

B.3.5 Different elasticities of substitution across firms

In our baseline calibration, we set the elasticity of substitution among non-housing firms to $\varepsilon_N = 4$. In this section, we explore how results change for alternative values. Table A.21 summarizes the results. Columns (2) and (4) show our model's predictions when only changing the value of ε_N, keeping all other parameter values fixed. Columns (3) and (5), on the other hand, show the predictions when we recalibrate the internal parameters g_A, ζ, ϕ and η_N in order to again match our cross-sectional estimation results.

[55] Note that this figure corresponds to book leverage. As most banks in our sample are not publicly traded, we do not have measures of market leverage for them. Begenau et al. (2019) show that in the United States, book and market leverage behaved in the same way during the 2000-2007 housing boom, both increasing very slightly.

As shown in Column (2), all else equal, a lower value for ε_N leads to smaller divergence at the firm-level, and higher divergence at the bank-level. This is intuitive. As Equation (15) shows, ε_N is the elasticity of the relative credit of non-housing firms with respect to their relative funding costs. Thus, with a lower value of ε_N, the same differences in funding costs lead to smaller divergence in firm credit. As there is less substitution across firms, there is more substitution within firms. Indeed, substitution across firms dampens the divergence of interest rates across banks (as firms linked to low-exposure banks increase their credit demand to gain market share from firms linked to high-exposure banks). Thus, with a lower value of ε_N, interest rates diverge more and firms substitute more between different banks. However, Column (2) also shows that on its own, this change in ε_N hardly affects our model's aggregate predictions.

This changes when we recalibrate the model in order to again fit our cross-sectional estimates more closely. As shown in Column (3), the recalibrated model manages to match again the firm-level divergence observed in the data by setting ϕ to a lower level. This implies that it takes longer for more exposed banks to accumulate net worth: they therefore diverge more from less exposed banks, and there is a greater divergence in firm funding costs. On its own, of course, this increases the model's loan-level predictions even more, and so the calibration also selects a lower elasticity of substitution across banks η_N. Overall, in this recalibrated model, the slower net worth accumulation implies a larger aggregate crowding-out effect, as shown in the last three rows of Column (3).

Increasing ε_N, as shown in Columns (4) and (5), has the exact opposite effect: all else equal, a higher elasticity of substitution between firms increases firm-level divergence, and so the calibration selects a higher speed of net worth accumulation to compensate for this and keep matching the same data targets. This faster net worth accumulation implies that the aggregate crowding-out effect is smaller.

Table A.21: Robustness: different elasticities of substitution ε_N

	Baseline ($\varepsilon_N = 4$)	$\varepsilon_N = 3$, base. par.	$\varepsilon_N = 3$, recal.	$\varepsilon_N = 5$, base. par.	$\varepsilon_N = 5$, recal.
	(1)	(2)	(3)	(4)	(5)
Parameters					
ϕ	0.019	0.019	0.001	0.019	0.037
η_N	3.421	3.421	2.681	3.421	4.353
Targets (model)					
$\beta_{2001-2003}$	-2.85	-2.93	-2.66	-2.66	-3.07
$\beta_{2004-2007}$	3.85	4.70	4.20	3.04	3.60
$\gamma_{2001-2003}$	-2.67	-2.06	-2.38	-3.11	-2.82
$\gamma_{2004-2007}$	3.61	3.30	3.76	3.56	3.31
Level of non-housing credit relative to counterfactual w/o financial transmission					
2004	-7.7%	-7.7%	-8.9%	-7.7%	-6.8%
2007	-2.0%	-2.0%	-2.7%	-2.0%	-1.6%
2008	$+1.8\%$	$+1.8\%$	$+1.7\%$	$+1.9\%$	$+1.7\%$

To sum up, Table A.21 shows that our results do depend on our assumption for the elasticity of substitution across non-housing firms, mainly because this assumption influences our estimate for the crucial parameter ϕ. However, the magnitude of our results does not change much for reasonable values of this elasticity.