Housing Price Booms and Crowding-Out Effects in Bank Lending

Indraneel Chakraborty
University of Miami

Itay Goldstein
University of Pennsylvania

Andrew MacKinlay
Virginia Tech

Analyzing the period 1988–2006, we document that banks that are active in strong housing markets increase mortgage lending and decrease commercial lending. Firms that borrow from these banks have significantly lower investment. This is especially pronounced for firms that are more capital constrained or borrow from more-constrained banks. Various extensions and robustness analyses are consistent with the interpretation that commercial loans were crowded out by banks responding to profitable opportunities in mortgage lending, rather than with a demand-based interpretation. The results suggest that housing prices appreciations have negative spillovers to the real economy, which were overlooked thus far. (*JEL* G21, G31, G32)

Received November 29, 2016; editorial decision January 12, 2018 by Editor David Hirshleifer. Authors have furnished an Internet Appendix, which is available on the Oxford University Press Web Site next to the link to the final published paper online.

We thank David Hirshleifer (the editor), two anonymous referees, Jean-Noël Barrot, Sudheer Chava, Ing-Haw Cheng, Mark Flannery, Kathleen Hanley, Victoria Ivashina, Borja Larrain, Carlos Madeira, Vikas Mehrotra, Amiyatosh Purnanandam, André Sapir, Philipp Schnabl, and Philip Strahan for helpful comments. We also thank seminar participants at the Bank of Canada, Boston College, Boston Federal Reserve, Federal Reserve Board, George Washington University, International Monetary Fund, Lehigh University, Louisiana State University, Southern Methodist University, University of Alberta, University of Colorado, University of Georgia, University of Melbourne, University of Miami, University of New South Wales, University of Texas Austin, and Virginia Tech; and conference participants at the 2013 Finance Cavalcade, the Rothschild Caesarea 10th Annual Conference, the 2013 Western Finance Association Meetings, the 2013 National Bureau of Economic Research Summer Institute (joint Corporate Finance and Risks of Financial Institutions session), the 2013 European Finance Association Meetings, the 2013 Federal Deposit Insurance Corporation-Journal of Financial Services Research Fall Banking Research Conference, the 2014 Adam Smith Corporate Finance Workshop, the 2014 Financial Intermediation Research Society Conference, the 2014 European Central Bank Conference on the Optimal Size of the Financial Sector, the First Bank for International Settlements Research Network Meeting on Banking and Asset Management, and the Central Bank of Chile's Financial Markets and Macroeconomic Fluctuations Workshop for valuable discussions. This paper previously circulated under the titles "Do Asset Price Booms Have Negative Real Effects?" and "Do Asset Price Bubbles Have Negative Real Effects?" Supplementary data can be found on *The Review of Financial Studies* web site. Send correspondence to Itay Goldstein, Department of Finance, Wharton School, University of Pennsylvania, 3620 Locust Walk, Philadelphia, PA 19104; telephone: (215) 746-0499. Email: itayg@wharton.upenn.edu.

doi:10.1093/rfs/hhy033 Advance Access publication March 21, 2018

The years leading up to the 2007–08 financial crisis were characterized by a significant boom in real estate prices. A similar pattern has been observed in previous episodes, in which real estate prices increase leading up to a crisis and then crash at the onset of the crisis. Much has been written about the negative real effects of asset price crashes (see, e.g., Gan 2007a,b; and Peek and Rosengren 2000). The logic behind this effect is that firms that own real estate can borrow less and invest less following the decline in the value of their assets (the collateral channel). In addition, banks exposed to real estate prices decrease their lending following the crash, causing further deterioration in firms' access to capital and investment (the lending channel).

Much less is known, however, about the real effects of the boom phase in asset prices. We explore these effects, focusing on the bank lending channel, in this paper. Specifically, we study the effect of housing prices on bank commercial lending and firm investment in the United States in the period between 1988 and 2006. We document a crowding-out effect, whereby the lending opportunities in the real estate market, following the boom in real estate prices, have led banks to reduce commercial lending. This has caused firms that depend on these loans to reduce investment, hence having a negative real effect.[1] Our empirical analysis hinges on the differences across banks in their exposure to the real estate market. We use the location of banks' deposit branches to proxy for the location of mortgage activity, since banks are more likely to do mortgage lending if there is larger price appreciation in the areas where they have branches. We then compare the behavior of banks that are more exposed with that of banks that are less exposed to housing price booms, and explore the implications for firms related to them.

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). Consistent with this argument, we find that across different specifications, our crowding-out results hold much more strongly and significantly for constrained banks; these are the banks that are smaller, more levered, and less active in securitization markets. We also explore a personnel-based constraint and find similar results, which suggests that some of the crowding-out effect can also be attributed to the difficulty banks face in expanding their workforce and increasing the overall volume of their lending activities. Similarly, the results about a decrease in firms' investment following the substitution in lending made by banks rely on the idea that firms are constrained and cannot easily substitute bank lending for new sources of capital. Indeed, we find that our results hold much more strongly for constrained firms across different measures.

[1] Thus, in combination with the previous literature on supply-side effects of price declines, our results suggest that the real effects of housing price changes through the bank lending channel are asymmetric.

An important issue in interpreting our empirical results, as in most papers in empirical corporate finance, is endogeneity. Is the reduction in commercial loans and firm investment a result of a decrease in the supply of loans from banks due to their opportunities in real estate markets, as we argue, or does it stem from a decrease in the demand for loans due to a decrease in firms' investment opportunities? A demand-based story could emerge if the housing prices that a bank is exposed to, based on its location, are correlated with the demand for loans by firms related to this bank. This could be argued most reasonably in cases where the firms are located near the banks that they borrow from.[2] It should be noted, however, that endogeneity here is more likely to work against the crowding-out story and makes it more difficult to find this result. This is because increased housing prices usually coincide with economic growth, and so one would expect a positive relation between housing prices and firm investment opportunities. This implies that, if anything, the basic regressions possibly underestimate the reduction in lending and investment due to a positive real estate price shock that is unrelated to firm demand for capital.

To address the endogeneity issue and estimate the direct effect of a shock to real estate prices in the location of the bank on lending and investment, we start by using the instrumental variable that was developed by Saiz (2010) and applied extensively in the literature. The instrument measures the availability of developable land in terms of topographic restrictions. To introduce time variation in the instrument, we also include the national 30-year fixed mortgage interest rate. This average mortgage interest rate is interacted with the land unavailability measure. These instruments are motivated by the idea that for a given decrease in mortgage rates, there will be an increase in housing demand. In areas where land cannot be easily developed into new housing, this increase in housing demand should translate to higher housing prices, compared with areas that can easily accommodate more housing. Further, the assumption is that housing elasticity differences due to the presence of undevelopable land are exogenous to any underlying economic activity. Thus, the instruments provide a component of housing price appreciation in the bank's region that is not related to firm financing and investment choices except through its effect on housing prices. Our approach is similar to that taken by Adelino, Schoar, and Severino (2015); Chaney, Sraer, and Thesmar (2012); and Loutskina and Strahan (2015), among others.

Using these instruments, we find that firm-level lending growth decreases by 42.3% from a given bank for a one-standard-deviation increase in housing prices. At the same time, firms that borrow from banks exposed to these appreciations also face large effects: a one-standard-deviation increase in

[2] Our data contains observations of firms borrowing from nearby banks as well as firms borrowing from banks in different locations. As we explain below, we use this property further in our identification approach.

housing prices decreases firm investment by 20.9% as a fraction of investment.[3] As expected, these results are more statistically and economically significant than those obtained without instrumentation: the potential endogeneity between loan supply and firm demand makes it more difficult to find the crowding-out effect, and once it is addressed, the reduction in commercial lending that translates into a reduction in firm investment is clearly observed. Comparing the magnitudes of our results suggests that for a 1% reduction in loan growth, firms reduce investment by about 0.49%. Thus, firms are able to internalize a fraction of the loss in credit supply, but a large piece of the reduction translates into real effects. We also document the effects of bank exposure to housing prices on other real activities of the firms, as well as their payout policies and capital structures.

In evaluating the results with instrumentation, it is important to consider the recent critique of Davidoff (2016). He argues that the elasticity of supply is a problematic instrument for housing prices because it is correlated with housing desirability and therefore with unobserved demand factors. His critique seems less pertinent to our setting than to previous ones for two reasons. First, the bias he documents implies that lower supply elasticity is correlated with high demand and economic activity, and so this works against our results. This suggests that, if anything, even our instrumented results may be understating the negative effect of housing prices on real investment via the bank lending channel. Second, a fundamental distinction of our setting versus those that Davidoff (2016) critiques is that we can separate housing prices at the bank's location from those at the firm's location, given that firms do not always borrow from nearby banks. Hence, we can address the concerns of Davidoff (2016) by controlling for firms' local demand shocks in our firm-related regressions.

Specifically, in our firm-related regressions we include specifications with a firm's state-year fixed effect (or a firm's county-year fixed effect). This control removes the omitted demand factors that Davidoff (2016) is concerned about. Moreover, in further robustness tests, we also run the analysis on a subsample that requires the firm and bank to have geographically separate footprints. Our results remain the same. Finally, for firms' loan growth, we include a specification in the style of Khwaja and Mian (2008) that uses firm-year fixed effects to compare loan growth changes for a given firm in a given year across different lenders with different exposures to housing price booms. We show that the same firm is borrowing less from banks that have greater exposure to housing price increases than from banks that have lower exposure. These findings provide strong evidence against the firm-demand explanation and provide further support for the supply-based explanation in which loans

[3] The number 42.3% is from Column 6 of Table 2. The analogous estimate for firm investment is 6.2 percentage points as a fraction of lagged property, plant, and equipment (PP&E, Column 2 of Table 4). Given the average investment rate of 29.7 percentage points, this estimate translates to 20.9% as a fraction of investment.

decrease as a result of crowding-out due to the bank's more attractive lending opportunities.

In further results, we show that an increase in housing prices in the bank's location leads to an increase in the interest rate for commercial loans provided by the bank, particularly for constrained firms and constrained banks. This effect is again consistent with a decrease-in-supply story and not with a decrease-in-demand story. We also investigate the profitability of different types of loans. Consistent with a supply effect, the commercial and industrial (C&I) loan profitability of banks is sensitive to increases in housing prices. That is, as housing prices increase, banks cut more C&I loans, and so the loans they continue to extend have higher average profitability. Still, we show that while both commercial lending and mortgage lending profitability increase in response to increasing housing prices, mortgage lending profitability increases more, supporting the basic claim that housing price increases make lending opportunities in the housing market more lucrative and trigger the crowding-out of C&I loans.

Another possible issue of endogeneity arises with the matching between banks and firms. If firms with poor investment opportunities borrow from more-constrained banks, it could contribute to our results.[4] To address this concern, we use firm-bank fixed effects in our firm-level regressions to control for persistent differences across lending relationships. If a firm with consistently poor investment opportunities matches with a financially constrained bank, the average level of the firm's investment will be controlled for. Therefore, any reduction in investment related to the bank restricting capital due to increasing housing prices would be a deviation from the firm's average investment levels over the course of their relationship, and not a cross-sectional difference between firms with different investment opportunities. We also conduct additional analysis using bank branching deregulations as shocks to bank-level constraints to confirm that the crowding-out effect is not due to endogenous matching concerns.

The channel we explore in this paper is an extension of the bank lending channel, whereby shocks to banks affect their ability to lend and end up affecting the firms that borrow from them. Many empirical papers have indeed provided evidence consistent with this view and demonstrating the bank lending channel. Examples include Kashyap and Stein (1995, 2000), Kishan and Opiela (2000), and Ashcraft (2006).[5] At the heart of this channel stands the premise that banks are financially constrained, motivated by a large theoretical literature. Stein (1998), for example, provides a model in which banks have inside information about the quality of their assets, limiting their ability to raise uninsured external

---

4 Note that Schwert (forthcoming) finds that constrained firms typically borrow from less-constrained banks, suggesting typical firm-bank matching goes against such an effect.

5 See also Bernanke (1983), Ashcraft (2005), Khwaja and Mian (2008), Paravisini (2008), Ivashina and Scharfstein (2010), and Schnabl (2012) for empirical evidence on banks' financial constraints and their effect on lending.

funds.[6] A novel feature of our empirical analysis is that the shock to the bank is not a typical negative shock to capital, but rather a positive shock to the bank's other lending opportunities that leads to substitution away from commercial loans. This bears resemblance to the discussion in the internal-capital markets literature in which constrained headquarters have to decide how to allocate resources among competing projects, as in Stein (1997) and Scharfstein and Stein (2000), and so will allocate less to some projects when other projects appear more profitable. Banks may face similar decisions and allocate resources to real estate loans at the expense of commercial loans in the face of real estate price appreciations.

An important question in evaluating the role of banks' constraints is why they cannot be overcome by securitization or loan sales. The key point here is that securitization and loan sales are subject to the same problems of incentives and asymmetric information that create financial constraints to begin with.[7] Hence, there are barriers to their widespread use. For example, risk retention is a common feature of securitization by banks, whereby banks keep some of the risk associated with the securitized product on the books to alleviate information frictions, implying that they still need to hold significant capital (see Acharya, Schnabl, and Suarez 2013; and Begley and Purnanandam 2017).[8] Indeed, looking at our sample period, securitization is limited. Moreover, it is used mostly by more reputable and larger banks. We explore this dimension in the paper to show that our results come more from banks that are not active in securitization, as one should expect.

The real effect that we document in the paper builds on a long line of literature establishing the dependence of firms on banks and the fact that many firms cannot easily substitute bank financing for other sources of financing. Hence, if their banks cut back on commercial lending, they will see real negative consequences in their investment activities. Papers in this line of work include Faulkender and Petersen (2006), Sufi (2009), Leary (2009), Lemmon and Roberts (2010), and Chava and Purnanandam (2011).

Our results on the effect of housing price booms bring a very new angle to the empirical literature, which argues that asset prices have a positive relation to lending and real investment. The papers by Gan (2007a,b) and

[6] See also Thakor (1996) and Bolton and Freixas (2006) for models of banks' financial constraints. Classic theories on financial constraints, originating from asymmetric information and incentives, outside the context of banks include Stiglitz and Weiss (1981) and Holmstrom and Tirole (1997).

[7] Several papers analyze this theoretically, such as Pennacchi (1988), Gorton and Pennacchi (1995), Morrison (2005), and Parlour and Plantin (2008). Indeed, empirically, Keys et al. (2010) find that securitization practices adversely affected the screening incentives of lenders. Loutskina and Strahan (2009) show that while securitization breaks the link between bank funding costs and credit supplied to the non-jumbo mortgage market, this connection is still there for the less-securitized jumbo residential mortgage market.

[8] In addition to retaining risk, MBS securities have additional clauses to protect investors. These clauses require banks to maintain reserves for loss provisions on their balance sheet. An example is the "put-back" clause, which allows the investors to sell the securities back to the originator at par in certain circumstances, such as if the appraised value of the property is misrepresented.

Peek and Rosengren (2000) mentioned above show how decreases in asset prices tighten financial constraints of banks and firms, decreasing lending, borrowing, and investment.[9] In a similar vein, a recent paper by Chaney, Sraer, and Thesmar (2012) documents that U.S. firms owning real estate benefited from the increase in real estate prices during the period of our study due to the collateral channel. While we confirm their results in our data, we document an additional effect operating in the opposite direction: Firms that depend on bank loans are harmed by the appreciation in real estate prices if their banks had a large exposure to real estate markets. This empirical result is related to the model of Farhi and Tirole (2012), which produces a similar substitution effect. To the best of our knowledge, our paper is the first to show a negative real effect of housing price appreciation.

This result has important implications for models in macroeconomics. Such models (e.g., Bernanke and Gertler 1989; and Kiyotaki and Moore 1997) often emphasize the positive effect of an increase in asset prices on real investments. Hence, they generate amplification of shocks—a positive shock in the economy leads to an increase in asset prices, enabling firms to borrow and invest more and thus magnifying the initial shock.[10] However, we show that the opposite also occurs: positive shocks to asset prices sometimes discourage real investment, leading to a dampening of the initial shock. We discuss some basic calculations regarding the size of the macroeconomic effect in Section 5. In particular, we show that the bank lending channel we highlight generates an effect that is similar in magnitude to the collateral channel in Chaney, Sraer, and Thesmar (2012).

There are also important implications for policy, as policymakers often attempt to support real estate prices in the hope that this will help boost the real economy. Our results demonstrate that this may not be the case. Our results do not say directly whether the decrease in lending and real investment following real estate price appreciation is bad for welfare and efficiency. Making such a statement would require us to know at least whether the appreciation is the result of a bubble or not. Second, the real estate market boom supported the construction sector, which may have been distortionary, but still created jobs. Further, one could argue that the policies supporting the real estate sector in the United States are driven by social goals of higher homeownership and not purely economic goals. Instead, we just document the negative relation in our setting and argue that macroeconomists and policymakers should not assume that asset price booms translate to a boost in economic activity, as the opposite occurs in some cases. This finding is consistent with the theoretical analysis of Bleck and Liu (2013), who show that in an economy with two sectors, the

---

9 See also Cuñat, Cvijanović, and Yuan (2013).

10 More recently, Gertler and Kiyotaki (2010) and Rampini and Viswanathan (2017) add a financial intermediary into such models and analyze additional amplification that may arise due to the lending channel.

injection of liquidity by the government may hurt the more constrained sector, due to a crowding-out effect that we capture in our empirical analysis.

Finally, our paper is related to the quickly growing literature studying the impact of the U.S. real estate boom on the larger economy. One paper in this literature is Chaney, Sraer, and Thesmar (2012), which we discussed already. In a related paper, Cvijanović (2014) investigates the impact of the collateral channel on the firm's capital structure decisions and finds results consistent with the firm's real estate collateral alleviating credit frictions. Adelino, Schoar, and Severino (2015) find increases in small business starts and self-employment in areas with large housing price appreciations. Not finding the same effects for larger firms in the same industries, they conclude that individual homes serve as an important source of collateral. Mian and Sufi (2011) find a housing-credit effect of consumers increasing consumption from rising home equity values. Loutskina and Strahan (2015) consider the role of financial integration among banks in amplifying housing price shocks during this period. They find that banks move mortgage capital out of low-appreciating housing markets and into high-appreciating housing markets within their own branch networks. Taken together, these papers suggest banks had an active role in the housing boom, and serve as a complement to our finding of the movement of bank capital away from commercial lending and into mortgage lending.

1. Data and Identification Strategy

This paper traces the crowding-out effects due to housing price booms from lending banks to borrowing firms. Our main analysis is conducted at three levels: at the firm-bank relationship level, at the bank level, and at the firm level. For this analysis, we use loan-level data from DealScan to identify firm-bank relationships. We combine this loan-level data with firm-level data from Compustat and additional bank-level data from the Call Reports. To measure the effect of housing prices on banks, we create a bank-specific housing price index that uses Summary of Deposits data from the Federal Deposit Insurance Corporation (FDIC) and housing price data from the Federal Housing Finance Agency (FHFA). We instrument housing prices with land unavailability data from Saiz (2010) and national 30-year mortgage interest rate data from the St. Louis Federal Reserve Economic Database (FRED). Our sample period is from 1988 through 2006. Since we use lagged data in many specifications, our earliest data goes back to 1987.

1.1 Firm-bank relationships and loan data

We rely on DealScan to conduct analysis on firm-bank relationships and bank-level commercial lending at a granular level.[11] DealScan provides origination

[11] As we discuss later, some additional bank-level analysis focuses on dependent variables that are not available from DealScan (for example, consumer lending) but are available from the Call Reports. In these cases, the analysis is at the bank holding company (BHC) level, which is not as granular.

information on syndicated and sole-lender loans. We consider the presence of any loan between the bank and borrowing firm to be evidence of a relationship. In the case of syndicated loans with multiple lenders, we consider the relationship bank to be the one that serves as lead agent on the loan. The length of the relationship is defined as follows: it begins in the first year that we observe an originated loan between the firm and bank and ends when the last loan observed between the firm and bank matures. Firms and banks are considered in an active relationship for each year of this period, including years when a new loan is not originated.

Beyond determining firm-bank relationships, we use DealScan for data on firm-bank level loan growth, the total amount of commercial lending from lenders, and loan interest rates and other contract terms. We link DealScan with additional data sources for the firms and banks. Following Chava and Roberts (2008), we link the DealScan borrowers to Compustat for firm-specific information using their link table. To obtain additional information regarding the lending banks, we create our own link table, which matches DealScan lenders to their bank holding companies in the Call Report data. We are able to match 753 DealScan lenders to 120 bank holding companies (BHCs) in the Call Report data.[12] These matches are determined by hand using the FDIC's Summary of Deposits data and other available data on historical BHC structures. Additional details on how we construct relationships are in Online Appendix A.1. We present the statistics on the number of relationships between borrowers, DealScan lenders, and BHCs in panel A of Table 1.

To investigate firm-bank relationships, we follow Khwaja and Mian (2008) to create a loan growth variable. However, in our case, we do not observe credit registry–level data. Hence, similar to Lin and Paravisini (2013), we create a panel that emulates a credit registry by aggregating DealScan lending data at the firm-bank relationship level. Given that loan originations can be infrequent, we compare lending between individual firms and their relationship banks over subsequent five-year windows to get a better picture of the firm-bank relationship. DealScan data also allows us to measure the amount of commercial lending at the bank level. Since there are sufficient originations per year by each bank, we consider the lender's total loan amount on an annual basis. This creates a balance sheet panel of the bank's commercial loans. The advantage of this approach compared to using annual C&I data from the Call Reports is that we are able to focus on the lending to the firms that are relevant to our analysis. To fully capture the crowding-out effects for a bank, we create the commercial loan balance sheet of all DealScan loan amounts held by the bank. This includes loans for which the bank is the lead agent and loans for which the bank is a syndicate member. For robustness tests, we create an alternative

[12] As the DealScan lending data is for individual bank or financial companies, there can be multiple DealScan lenders to each BHC. Of the 753 lenders, 654 lenders (and 106 BHCs) have borrowers that can be matched to Compustat and are included in our main sample.

Table 1
Summary statistics

Panel A: Relationship, loan, and firm variable statistics

	Mean	Std. dev.	25th pctile	Median	75th pctile	# obs.
Length/Frequency of Relationships						
Length of Relationship	5.17	3.65	3	5	7	14,377
Number of Loan Packages	2.33	1.87	1	2	3	19,116
Loan Facilities per Loan Package	1.40	0.75	1	1	2	19,116
Number of Relationships						
DealScan Lenders per Borrower	2.87	1.80	1	2	4	14,377
Bank Holding Companies per Borrower	2.44	1.50	1	2	3	12,880
Borrowers per DealScan Lender	319.6	380.8	62	179	463	14,377
Borrowers per Bank Holding Company	755.3	689.5	218	465	1811	12,880
DealScan Lenders per Bank Holding Company	21.9	18.2	7	15	33	655
Loan Characteristics						
All In Drawn Spread (bps)	181.7	131.5	75	162.5	262.0	21,523
Loan Amount	281.1	761.0	26.0	78.8	211.8	19,831
Maturity (months)	41.7	27.2	18	36	60	21,523
Takeover Loan	0.16	0.36	0	0	0	21,523
Revolving Credit Line	0.85	0.36	1	1	1	21,523
Firm Variables						
Loan Growth	0.052	0.22	−0.081	0.037	0.18	5,823
Investment	29.7	45.3	10.5	18.5	32.7	60,995
Market to Book	1.68	1.45	1.05	1.33	1.83	53,404
Cash Flow	38.5	99.4	9.76	24.9	54.7	61,523
Firm Size	6.50	2.04	5.10	6.44	7.85	62,947
Altman's Z-Score	1.28	3.19	0.69	1.49	2.35	59,155
Acquisitions	33.2	115.8	0	0	8.70	58,650
R&D Expense	51.0	482.8	0	6.15	26.0	28,999
Dividend Payout	21.1	2948.4	0	0	2.60	66,411
Book Leverage	34.6	27.1	17.2	31.4	45.9	62,780
Change in Leverage	−0.063	16.5	−4.38	−0.39	4.00	61,822
Change in Debt	4.94	22.4	−3.68	0.019	7.41	61,829
Change in Equity	11.9	27.5	0	0.82	6.11	60,374
Industry Land Intensity	6.62	7.60	2.40	3.30	8.80	62,538
Market Value of Buildings	1.28	2.27	0.30	0.69	1.31	19,436

(continued)

sample that aggregates only the loan amounts for which the bank is the lead agent. We also include a variable based on the number of loans originated by a lender. To calculate the interest rates and maturities of loan packages (which can contain multiple loan facilities), we average the individual facility values by their respective dollar amounts. In our interest rate analysis, we also include indicators if the loan package is designated for takeover purposes or contains a revolving credit line. The summary statistics for these variables are included in panel A of Table 1, and exact variable definitions are included in Table A.1 in the Online Appendix.

1.2 Firm and bank data

As we are focusing on how financial intermediaries affect borrowing firms' real activity, we exclude any borrowing firms that are financial companies. We consider several dimensions of firm activity using Compustat data in our analysis, including investment, acquisitions, research and development (R&D) expenses, dividend payout, and changes in leverage, debt, and equity. We use

Table 1
Continued

Panel B: Bank, housing, and macroeconomic variable statistics

	Mean	Std. dev.	25th pctile	Median	75th pctile	# obs.
Bank Variables						
Log(Dollar Outstanding Loans)	4.95	2.47	3.22	4.87	6.38	6,184
Log(Number Outstanding Loans)	2.05	1.74	0.69	1.79	3.04	6,184
Real Estate Loans	20.0	10.4	13.1	19.2	25.9	1,498
MBS	8.60	7.81	2.38	6.86	12.3	1,498
Commercial Mortgages	8.97	5.90	4.82	8.04	11.7	1,498
C&I Loans	16.4	7.52	11.3	15.7	20.2	1,498
Consumer Loans	9.17	5.90	4.33	8.97	13.1	1,498
C&I Loan Profitability	7.21	2.40	5.72	7.35	8.67	1,497
Real Estate Loan Profitability	7.14	1.76	5.88	7.33	8.30	1,472
Profitability Ratio	1.05	0.40	0.88	1.00	1.15	1,472
Bank's Size	16.3	1.62	15.2	16.2	17.5	1,498
Bank's Equity Ratio	8.19	2.10	6.92	7.88	8.99	1,498
Bank's Net Income	1.08	0.49	0.90	1.12	1.33	1,498
Bank's Cost of Deposits	3.29	1.50	2.35	3.13	4.10	1,498
Securitization Activity	0.24	0.43	0	0	0	1,241
Employee Growth	8.81	40.5	−5.85	0.78	9.84	1,298
Deregulation Measure	0.71	0.36	0.31	0.90	1	6,184
Housing Variables						
Housing Price Index, Bank's State(s)	298.5	104.0	230.2	272.2	349.1	66,443
Return on Housing, Bank's State(s)	6.25	7.59	2.23	5.48	10.00	65,477
Land Unavailability, Bank's State(s)	24.5	8.46	19.8	23.0	28.9	66,425
Office Price Index, Firm's State	166.3	77.6	120.9	141.2	190.2	70,578
Macroeconomic Variables						
Change in Unemp. Rate, Firm's State	−0.075	0.82	−0.60	−0.30	0.30	63,903
Change in Unemp. Rate, Bank's State(s)	−0.062	0.79	−0.58	−0.20	0.20	66,443
National 30-Year Mortgage Rate	7.15	1.12	6.14	7.10	7.60	66,443

This table presents summary statistics of the merged sample of banks and borrowing firms as obtained from the Dealscan, Compustat, and Call Report databases. Panel A presents the summary statistics for the borrower-lender relationships, the loan characteristics, and the firm variables. Panel B presents the summary statistics for the bank balance sheet variables, housing price variables, and other macroeconomic variables used in the analysis. All firm, loan, and bank ratio variables are scaled by 100.

market-to-book ratio, cash flow, firm size, book leverage, and Altman's z-score as control variables in many of our specifications. We also include a measure of the market value of the firm's buildings (following Chaney, Sraer, and Thesmar 2012) and an industry-level measure of the share of capital income that is attributable to land (*Industry Land Intensity*) for some of our additional analysis. Panel A of Table 1 includes the summary statistics for these variables.

On the bank side, we supplement our loan information from DealScan with Call Report data at the BHC level. In our analysis, we consider the following additional asset classes: unsecuritized noncommercial real estate loans, mortgage-backed securities (MBS), commercial mortgages, and consumer loans. The summary statistics of these bank loan variables, all scaled by the bank's total assets, are reported in panel B of Table 1. We include measures of C&I and mortgage loan profitability, which are the interest and fee income divided by the total amount of loans for each type. We also include four additional bank control variables: the bank's size, equity ratio, net income, and cost of deposits. We use measures of securitization activity and employee growth at the bank level in tests of bank-level constraints. As regional economic controls, we include changes in unemployment rates in the firm's state and in the bank's states of operation.

Beyond the inclusion of various controls, in the cross-section of bank holding companies, it is likely that the largest bank holding companies are still significantly less constrained than the smaller bank holding companies. In much of our analysis, we allow the three largest bank holding companies—Citigroup, Bank of America, and JPMorgan Chase—to have a differential effect when it comes to the bank lending channel.[13]

1.3 Housing exposure of banks

At the core of our analysis is the weighted index of housing prices per bank holding company. We use the state-level House Price Index (HPI) from the FHFA as the basis for this variable. To determine the exposure of each bank holding company to different state-level housing prices, we use the Summary of Deposits data from June of the prior year, aggregated to the BHC level. Using the percentage of deposits in each state as weights, we create a measure of housing prices that is specific to each bank and each year.[14] Our bank-specific housing price index is scaled such that an index value of 100 corresponds to $50,000 in year 2000 dollars. Additional details of the variable's construction are provided in Online Appendix A.3.

Figure 1 presents both the level of our index and the annual changes in our index for each bank. The figure shows an upward trend in housing prices over our sample period, but also substantial cross-sectional variation across bank holding companies. In Figure 2, we plot the relation between banks' real estate–related lending, commercial and industrial lending, and housing prices, using a local polynomial regression. We focus on the effect of changes in housing prices on a given bank's holdings by considering within-bank variation only, using the sample of the 106 BHCs we match to Compustat borrowers. We plot one standard deviation above and below each bank's average housing price index level. Figure 2 suggests banks are, on average, increasing real estate lending and decreasing commercial lending as housing prices increase in the bank's deposit area. In the remainder of the paper, we investigate how housing prices affect bank-level, firm-level, and loan-level outcomes more formally in a multivariate setting.

1.4 Identification strategy

There are a few identification concerns that we address in our empirical approach. The first concern is that housing prices are likely correlated with

13 In Online Appendix A.2, we provide additional discussion as to why we chose to separate these three bank holding companies.

14 For example, a bank that in June 2003 had 75% of its deposits in California and 25% of its deposits in Arizona would have a 2003 price index that is a combination of 75% of California's 2003 fourth-quarter state-level price and 25% of Arizona's 2003 fourth-quarter state-level price.

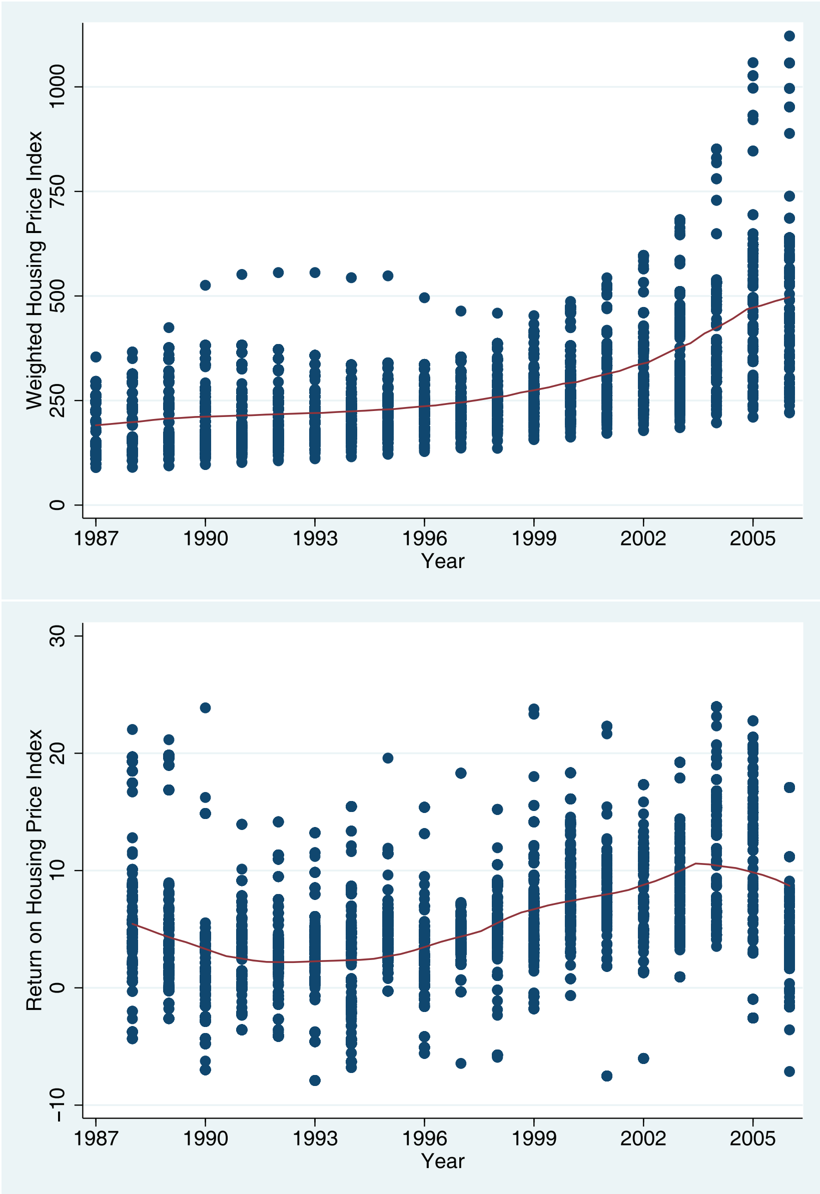

Figure 1
Housing prices in banks' deposit areas
This figure plots the weighted housing price index (top) and return on the weighted housing price index (bottom) in the locations where the bank has depository branches.

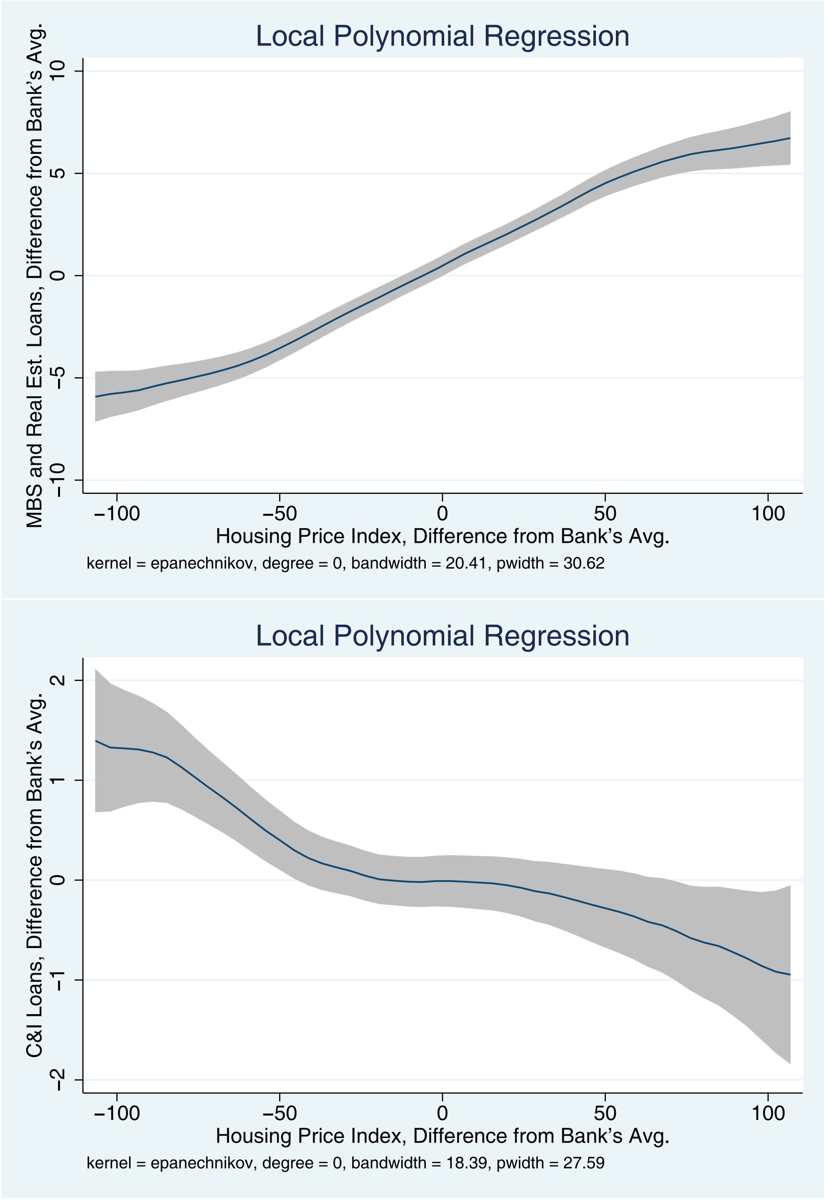

Figure 2
Relation between the housing price index and either MBS and real estate loans or C&I loans
The top figure plots the fraction of the bank's total assets that are MBS and real estate loans (excluding commercial mortgages) against the prior year's housing prices where the bank has depository branches, relative to the bank's average levels. The bottom figure plots the fraction of the bank's total assets that are C&I loans against the prior year's housing price index where the bank has depository branches, relative to the bank's average levels. We demean each variable at the bank level. Both loan variables are scaled by 100. Confidence intervals of 95% are provided for the local polynomial regression estimates.

unobserved economic shocks. The omitted economic shocks, which may affect firm demand for loans as well as housing prices, would bias our estimates. The next issue is whether the instrumental variables approach that we employ fully addresses the concerns regarding unobserved demand-side factors. A final concern is that the mechanism that causes certain firms to match with certain banks could be contributing to our results. We discuss each of these issues in turn.

To address the first concern of an omitted variable bias, we use an instrumental variables approach. Our instrument set is a measure of land area that is unavailable for residential or commercial real estate development (Saiz 2010), the national-level 30-year mortgage rate, which measures housing and mortgage demand for consumers, and the interaction of the land unavailability and mortgage rate measures.[15] Using the deposit weights for each bank's exposure to different states, we calculate the percentage of unavailable land in each bank's region of operation. The instruments are designed to capture variation in housing prices that is not correlated with local economic conditions. For similar housing demand shocks, areas with less available land will experience larger price increases since additional housing construction is more costly. Interacting this unavailability measure with the mortgage rate captures the housing price dynamics further. As mortgage rates decrease (and housing demand increases), areas with less available land will see a relatively higher increase in housing prices than areas with more available land. We provide additional discussion of the instrumental variables and confirm they affect housing prices in the expected manner in Online Appendix B.

There are two related concerns about this instrumental variables approach. First, Davidoff (2016) argues that the elasticity of supply is not a valid instrument for housing prices because it is correlated with housing desirability and therefore unobserved demand factors. As this argument implies that lower elasticity is positively correlated with economic activity and firm investment, this bias would go against our results. Second, the possibility that housing prices and real estate costs directly influence firm decisions (e.g., as an input cost for production) is not addressed by our instrumental variables approach. These concerns are not unique to our paper, as they apply to prior papers that use similar instrument sets, whether for firm investment (Chaney, Sraer, and Thesmar 2012) or employment growth (Adelino, Schoar, and Severino 2015; Loutskina and Strahan 2015).

We address these concerns in a few ways. First, we stress that our housing price variable is calculated at the bank, rather than firm, location. The majority

15 Saiz (2010) calculates slope maps for the continental United States using U.S. Geological Survey (USGS) data. The measure is the share of land within 50 kilometers of each Metropolitan Statistical Area (MSA) that has a slope of more than 15% or is covered by lakes, ocean, wetlands, or other internal water bodies. We use a version that is averaged to the state level by using population figures (from the 2000 Census data) to determine the appropriate weights for different MSAs.

of bank holding companies in our main sample operate across multiple states.[16] Further, the inclusion of a firm's state-year fixed effects removes any potentially time-varying unobserved demand factors at the firm's location. These factors would include differences in housing desirability (as raised by Davidoff 2016) and the cost of land as an input in the firm's production decision. For firms with multiple lenders, we go further and use firm-time fixed effects to determine the effect of housing prices on loan growth. These specifications not only remove local demand concerns, but any demand factors specific to a firm at a given point in time.

Besides a firm's state-year fixed effects, we confirm our investment findings by: directly including the firm's state HPI as a separate control, using firm county-year fixed effects as a finer local demand control, and considering a subsample in which we require firms and banks to have geographically separate footprints. These tests address the two related concerns regarding the instrumental variables approach. As an additional test for the concern that housing prices directly affect firm investment decisions, we use a subsample in which we exclude firms from the most land-intensive industries. We also incorporate the price of commercial real estate in the firm's state and the importance of land for different firms to directly consider the economic importance of the cost of real estate affecting firm investment demand. These results are provided in Online Appendix C.2.

A different source of potential endogeneity is that the matches between firms and banks are not random: more constrained firms with potentially fewer investment opportunities may tend to borrow from weaker, more constrained banks. If the investment of firms that borrow from constrained banks is more negatively affected by housing price booms, and these firms also have fewer investment opportunities, then their larger investment declines may be driven by fewer investment opportunities and not necessarily a larger credit supply shock. A related but distinct concern is that firms that borrow from constrained banks are more bank-dependent than those that borrow from unconstrained banks. If this is the case, then firms that borrow from constrained banks would be more affected by bank credit supply shocks. However, Schwert (forthcoming) finds evidence that constrained firms borrow from well-capitalized banks. These findings would imply that, if anything, any differences from matching are likely to go against finding our crowding-out result.

Nevertheless, to address concerns that nonrandom firm-bank matches are driving investment results, we include firm-bank fixed effects when we consider firm-level outcome variables. Any negative effects in firm outcomes from reductions in bank capital are identified from deviations in the average level of that firm's investment over its relationship with the given bank, and not from cross-sectional differences between firms with stronger or weaker investment

[16] The median number of states is four, with less than 18% of bank holding company observations operating in only one state.

opportunities. As an additional strategy, in Section 3.4 we exploit intrastate branching deregulation as an exogenous shock to the cross-sectional variation in banks' constraints. Supporting the argument that the results are driven by bank credit supply changes rather than firm demand or endogenous matching, we find that following state-level deregulation, less capital is crowded out from C&I lending than before deregulation when the banking sector was more constrained.

2. Housing Prices, Bank Lending, and Firm Investment

2.1 Relationship lending

If the housing boom is crowding out commercial borrowing and investment through the lending channel, we expect a decrease in lending to firms in response to higher housing prices. To test if this is the case, we first consider loan growth at the firm-bank level. We follow Lin and Paravisini (2013) for a modified approach of Khwaja and Mian (2008) that is applicable in our setting. The approach by Khwaja and Mian (2008) relies on credit registry data, where the firm-bank pair's loan balances are observed continuously. To create a panel that is similar to a credit registry, we aggregate DealScan lending data at the relationship level between each firm and bank. Specifically, we sum the total amount of lending between a firm and bank over subsequent five-year periods and use these aggregated loan amounts to compute the loan growth. Thus, when a new loan is initiated between a firm and bank, we can compare the amount borrowed that year (and the following four years) to the amount borrowed in the five years prior to the new loan. Aggregating the loan data over multiple years is helpful as new loans are not initiated every single year between each bank and firm. In this framework, identification is based on changes in lending for a firm-bank pair as housing prices change in the bank's geographic footprint.

We run specifications for firm i, bank j pairs for which time period t represents a five-year window. For our fixed effects, we utilize information about the firm's industry (ind), size quintile ($size$), and state (s). The initial specification is as follows:

$$\text{Loan Growth}_{ijt} = \alpha_{ind,size} + \gamma_{st} + \delta_j + \vartheta_1 \text{Housing Prices}_{jt-1} + \vartheta_2 \text{Bank Vars.}_{jt-1} + \vartheta_3 \text{Macro Vars.}_{jt-1} + \varepsilon_{ijt}. \quad (1)$$

Table 2 reports the results with annualized loan growth at the firm-bank level as the dependent variable. Across all our analysis, we include the following bank-level variables—the bank's size, equity ratio, net income, and cost of deposits—to control for differences in the condition of banks. We also include changes in the unemployment rate in the bank's states as a regional macroeconomic control. All control variables are from the final year of the prior five-year window, and continuous control variables are scaled by their sample standard deviations to aid in interpreting their economic importance. As in Lin and Paravisini (2013),

Table 2
Loan growth regression

	Loan growth									
	(OLS) (1)	(IV) (2)	(OLS) (3)	(IV) (4)	(OLS) (5)	(IV) (6)	(OLS) (7)	(IV) (8)	(OLS) (9)	(IV) (10)
Housing Price Index, Bank's State(s)	−0.0396***	−0.143**	−0.0491***	−0.219***	−0.134***	−0.423***	−0.263***	−0.694***	−0.350***	−0.888***
	(0.00614)	(0.0598)	(0.0147)	(0.0778)	(0.0251)	(0.152)	(0.0450)	(0.244)	(0.0635)	(0.322)
Top-3 × HPI, Bank's State(s)			0.0124	0.132			0.155***	0.218	0.257***	0.798**
			(0.0170)	(0.0837)			(0.0358)	(0.247)	(0.0659)	(0.320)
Bank's Size	−0.0250	0.0725	−0.0269	0.0207	−0.184**	0.127	−0.209**	0.187	−0.472***	−0.398**
	(0.0207)	(0.0558)	(0.0210)	(0.0447)	(0.0760)	(0.187)	(0.0850)	(0.155)	(0.0944)	(0.170)
Bank's Cost of Deposits	−0.00554	−0.0167	−0.00554	−0.0204	0.00721	−0.0243	0.00633	−0.0353	0.00964	0.0215
	(0.0127)	(0.0192)	(0.0131)	(0.0216)	(0.0248)	(0.0328)	(0.0246)	(0.0369)	(0.0376)	(0.0433)
Bank's Equity Ratio	−0.0281***	−0.0110	−0.0269**	−0.0117	−0.0586***	−0.0522***	−0.0531***	−0.0424***	−0.0250	−0.00499
	(0.0108)	(0.00948)	(0.0112)	(0.0142)	(0.0122)	(0.0133)	(0.0125)	(0.0143)	(0.0209)	(0.0297)
Bank's Net Income	0.00685	0.00807	0.00625	−0.00130	−0.00516	−0.00925	−0.0107	−0.0182	−0.00359	−0.00634
	(0.00532)	(0.00610)	(0.00560)	(0.00972)	(0.00838)	(0.0114)	(0.0101)	(0.0187)	(0.0178)	(0.0215)
Change in Unemp. Rate, Bank's State(s)	−0.0100*	−0.00887*	−0.00953	−0.00657	−0.0101	−0.0182	−0.00761	−0.0173	−0.0234	−0.00291
	(0.00603)	(0.00504)	(0.00604)	(0.00503)	(0.0144)	(0.0151)	(0.0138)	(0.0163)	(0.0203)	(0.0270)
Industry and Size Quintile Fixed Effects	Yes	Yes	Yes	Yes	No	No	No	No	No	No
Bank Fixed Effects	Yes	Yes	Yes	Yes	No	No	No	No	Yes	Yes
Firm-Bank Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes	No	No
Firm-Year Fixed Effects	No	No	No	No	No	No	No	No	Yes	Yes
Firm's State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Observations	5,191	5,191	5,191	5,191	3,323	3,323	3,323	3,323	3,163	3,163
Firms	2,449	2,449	2,449	2,449	1,054	1,054	1,054	1,054	1,026	1,026
Banks	155	155	155	155	106	106	106	106	105	105
Adjusted R^2	0.0687	0.0376	0.0687	0.0318	0.500	0.379	0.512	0.303	0.580	0.499

Standard errors in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$

Columns (1) through (10) are panel fixed effect regressions. *Loan Growth* is the loan growth for a firm-bank pair, aggregated over five years and annualized. *Housing Price Index, Bank's State(s)* is the bank holding company's housing price index in a given year. *Top-3* is an indicator for the three largest banks in our sample. Columns (2), (4), (6), (8), and (10) use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. All continuous independent variables are scaled by their respective standard deviations. Standard errors are clustered by firm, bank, and year. In this and following tables, OLS refers to ordinary least squares and IV refers to instrumental variables specifications.

Columns 1–4 are not estimated within firm but across SIC-2 level and size quintiles ($\alpha_{ind,size}$). We also include bank fixed effects (δ_j) and firms' state-time fixed effects (γ_{st}), which capture both persistent and time-varying differences across firm locations. These state-time fixed effects address the concern that housing prices or other economic forces in the firm's state (and not the bank's states) are yielding our results. Columns 5–8 include firm-bank fixed effects (instead of $\alpha_{ind,size}$ and δ_j) to control for any persistent differences in a firm's relation with a particular bank. Columns 9 and 10 include bank and firm-time fixed effects. The latter controls for any firm-specific demand-side factors that might affect loan growth.

Column 1 shows that, after controlling for industry, size quintile, bank, and firms' state-time fixed effects, loan growth decreases when housing prices increase in the bank's location. As discussed in Section 1.4, it is plausible that housing prices may be endogenous to the firm's borrowing and investment decisions. Specifically, if the bank's regional housing prices are correlated with any omitted variables related to the commercial lending of the bank or the investment opportunities of the borrowing firm, the estimate of the effect of the bank's housing exposure may be biased. We believe the source of the bias is likely positive, as housing prices are generally positively correlated with economic growth. A positive bias works against finding the result that an increase in housing prices crowds out commercial lending. Indeed, Column 2 shows that after instrumenting housing prices, the negative effect remains statistically and economically significant and is stronger.[17] For a one-standard-deviation increase in housing prices, loan growth falls by 14.3% per year.

Column 3 includes an interaction term (*Top-3* $\times$ *HPI, Bank's State(s)*) to separately capture the effect of increasing housing prices for the three largest banks by deposits in the United States in our sample.[18] The decision to separate these three banks (Citigroup, Bank of America, and JPMorgan Chase), which are likely the least constrained, is discussed in more detail in Section 1.2. When we separate the top-three banks, the remaining banks still have a statistically significant negative estimate. Column 4 runs the same specification as Column 3 but uses instrumental variables and finds stronger negative results.

Columns 5 through 8 include firm-bank fixed effects to control for differences in firm-bank relationships that may affect loan growth. Such differences could be related to the investment opportunities of specific firms or possible endogenous matches between banks and firms, as discussed in Section 1.4.

[17] The instruments are the measure of land unavailability in the bank's region and its interaction with the prevailing average national-level 30-year fixed mortgage rate. (The mortgage rate as a separate variable is absorbed by the firm's state-time fixed effects.) We split the land unavailability measure (and therefore the interaction term as well) into two periods: 1988–99 and 2000–2006. This is sufficient to capture the differences in housing price growth across the two periods.

[18] Although the *Top-3* indicator variable is included in these specifications as well, it is absorbed by the bank fixed effects or the firm-bank fixed effects.

Columns 5 and 6 show that within a firm-bank pair, the negative effect of housing price increases on loan growth is statistically significant and large in magnitude. Comparing these estimates to Columns 1 and 2, the persistent firm-demand and matching effects controlled for in Columns 5 and 6 appear to have a positive bias on the effect of housing prices on loan growth. In Column 6, a one-standard-deviation increase in housing prices is associated with a 42.3% decrease in loan growth. Columns 7 and 8 include an interaction term for housing prices with the top-three banks. In Column 7, we find that the three largest banks have a smaller but still negative crowding-out effect, which is significant at the 1% level. Column 8 presents the results of the instrumented specification.

Alternatively, Columns 9 and 10 include firm-time fixed effects to control for any possible, potentially time-varying, firm demand-side factors. The estimates in these columns are based on comparing the loan growth of different banks lending to the same firm in the same time period. The results remain similar in this case to Columns 7 and 8. The fact that the estimates in Columns 9 and 10 are uniformly more negative than the estimates in Columns 3 and 4 again suggest that omitted loan demand factors of the firm likely bias our estimates in a positive direction.

2.2 Bank-level lending

Section 2.1 shows evidence of loan growth being reduced for individual firm-bank relationships. Next, we analyze how commercial lending at the bank level is affected by housing price booms. One approach is to utilize balance sheet data for BHCs available from the Call Reports. This approach does not focus on the loans originated to the relevant firms analyzed in our paper. Since we have access to more granular information regarding commercial lending from DealScan, we can improve on the approach by creating a bank-level panel of commercial loans to firms in the DealScan sample. Bank observations in this panel are grouped at the DealScan-lender level by aggregating all new and outstanding lending to the set of firms in the DealScan data set.[19] This creates the balance sheet of a bank's commercial lending for the relevant sample of firms. In contrast to loan originations at a firm-bank relationship level, there is frequent lending in each year at the bank level. Therefore, we are able to create a panel at the annual level. We create the commercial loan balance sheet for each bank by including all loans extended by the bank as a lead agent and as a syndicate participant. We do this to get a complete picture of the lending

[19] This panel includes loans that were previously originated but have not yet matured and includes lending by banks both as a lead bank and as a non-lead member of a syndicate. To determine each lender's loan amount, we do the following: for those loans that have allocation information, we use the provided data. For those loans without allocation data, we estimate the average allotment given the lender's position in the syndicate and the syndicate size and use that to calculate the allotment. We get similar results if we simply divide the loan amount by the number of syndicate members. We assume the loan amount remains with the syndicate member until its stated maturity.

by the bank to DealScan borrowers. However, as a robustness test in Online Appendix C.1, we also create a panel by only aggregating loans where the bank is a lead lender.

To investigate how housing prices affect a bank's commercial lending across its borrower firms, we use the following regression specification for bank j in year t:

$$\text{Comm. Lending}_{jt} = \alpha_j + \gamma_t + \lambda_1 \text{Housing Prices}_{jt-1} + \lambda_2 \text{Bank Vars.}_{jt-1} + \lambda_3 \text{Macro Vars.}_{jt-1} + \varepsilon_{ijt}. \quad (2)$$

We include bank fixed effects (α_j), year fixed effects (γ_t), and the same bank-specific controls as in the prior specifications. Table 3 focuses on commercial loans in terms of dollar amounts and the number of outstanding loans to provide alternative measures of lending at the bank level. Columns 1 and 2 present the effect of housing prices on the amount of loans without and with instrumentation, respectively. Column 1 finds that for a one-standard-deviation increase in housing prices in a bank's states, the dollar amount of commercial loans decreases by 19% ($e^{-0.208}-1$). The instrumented specification suggests an even larger effect. Column 3 includes the interaction of an indicator variable for the three largest banks with housing prices in the banks' states. The positive and statistically significant coefficient suggests that the top-three banks reduce their commercial lending less in response to higher housing prices.[20] This result supports the main argument of our paper that constraints at the bank level are driving the crowding-out result.

Columns 1–3 show that the aggregate amount of loans outstanding declines in the presence of higher housing prices. Columns 4–6 repeat these specifications but focus on the number of loans outstanding rather than the amount. Similar to our amount results, we find that the number of loans outstanding is reduced by a statistically and economically significant magnitude when housing prices increase in a bank's states of operation.

2.3 Firm investment

Together, Sections 2.1 and 2.2 find that banks curtailed lending to firms in response to increasing housing prices. Table 4 considers if this reduction affects the investment of firms that borrow from these banks. The specification below estimates the impact of an increase in housing prices at the level of the lending bank j on the investment of the borrowing firm i at time t:

$$\text{Investment}_{ijt} = \alpha_{ij} + \gamma_{st} + \beta_1 \text{Housing Prices}_{jt-1} + \beta_2 \text{Firm Vars.}_{it-1} + \beta_3 \text{Bank Vars.}_{jt-1} + \beta_4 \text{Macro Vars.}_{jt-1} + \varepsilon_{ijt}. \quad (3)$$

The unit of observation is at the firm-bank-year level and includes observations for each year of the firm-bank relationship. This panel structure allows us to

20 The total effect for the top-three banks ($-0.964+0.475=-0.489$) is statistically significant at the 5% level, indicating that they still reduce lending to some extent.

Table 3
Outstanding loans regression

	Log(dollar outstanding loans)			Log(number outstanding loans)		
	(OLS) (1)	(IV) (2)	(IV) (3)	(OLS) (4)	(IV) (5)	(IV) (6)
Housing Price Index,	−0.208*	−0.735***	−0.964***	−0.208*	−0.583***	−0.729***
Bank's State(s)	(0.125)	(0.241)	(0.262)	(0.123)	(0.216)	(0.244)
Top-3 × HPI, Bank's State(s)			0.475**			0.391**
			(0.185)			(0.169)
Bank Size	0.145	0.223	0.244	0.113	0.167	0.206
	(0.205)	(0.207)	(0.203)	(0.187)	(0.187)	(0.183)
Bank Equity to Assets	−0.127***	−0.119**	−0.0732	−0.0798**	−0.0708*	−0.0324
	(0.0477)	(0.0538)	(0.0466)	(0.0391)	(0.0419)	(0.0414)
Bank Income to Assets	−0.0375	−0.0147	−0.0257	0.00147	0.0216	−0.00330
	(0.0392)	(0.0387)	(0.0369)	(0.0275)	(0.0286)	(0.0271)
Bank Cost of Deposits	−0.234*	−0.281**	−0.202	−0.174*	−0.213**	−0.105
	(0.142)	(0.141)	(0.145)	(0.103)	(0.104)	(0.114)
Change in Unemp. Rate,	0.0158	0.0426	0.0555	−0.0377	−0.0106	−0.0175
Bank's State(s)	(0.0470)	(0.0469)	(0.0493)	(0.0378)	(0.0339)	(0.0364)
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,215	5,215	5,215	5,215	5,215	5,215
Banks	617	617	617	617	617	617
Adjusted R^2	0.748	0.745	0.743	0.727	0.724	0.724

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Columns (1) through (6) are panel fixed effect regressions. *Log(Dollar Outstanding Loans)* is the log amount of outstanding DealScan loans with each bank in a given year. *Log(Number Outstanding Loans)* is calculated by taking the log-transform of the number of firms that have outstanding DealScan loans with each bank. *Housing Price Index, Bank's State(s)* is the bank holding company's housing price index in a given year. *Top-3* is an indicator for the three largest banks in our sample. Columns (2), (3), (5), and (6) use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. All continuous independent variables are scaled by their respective standard deviations. Standard errors are clustered by bank and year.

observe firm investment policy in the years when a new loan is originated and in the years when there is an ongoing lending relationship.

To help control for firm-level determinants of investment, we include lagged market-to-book, lagged firm size, and contemporaneous cash flow as control variables. We continue to include the following bank-level variables—the bank's size, equity ratio, net income, and cost of deposits—to control for differences in the financial condition of banks. Firm-bank fixed effects, α_{ij}, capture any persistent differences among firms and more specifically a firm's relationship with a particular bank. All results include firm's state-year fixed effects (γ_{st}), which control for any local economic conditions in the firm's state. Finally, to capture any more localized economic effects for the bank, the change in the unemployment rate in the bank's states of operation is also included. The dependent variable, the ratio of investment to lagged net property, plant, and equipment (PP&E), is scaled by 100.

As mentioned before, an endogeneity concern is that housing prices are likely positively correlated with firm investment opportunities. In such a case, any bias in our estimates is likely positive. Columns 1 and 2 of Table 4 confirm this supposition. In Column 1, which runs the regression without instrumentation, the effect of housing prices in the bank's state is not significantly different from

Table 4
Investment regression

	Investment			
	(OLS) (1)	(IV) (2)	(OLS) (3)	(IV) (4)
Housing Price Index, Bank's State(s)	−1.386	−6.199***	−4.248**	−8.830***
	(1.162)	(1.931)	(1.926)	(2.340)
Top-3 × HPI, Bank's State(s)			3.166***	4.940***
			(1.102)	(1.201)
Lagged Market to Book	9.097***	9.140***	9.093***	8.818***
	(0.662)	(0.603)	(0.657)	(0.549)
Cash Flow	8.509***	8.942***	8.506***	9.182***
	(1.123)	(1.044)	(1.125)	(1.008)
Lagged Firm Size	−19.39***	−17.72***	−19.33***	−17.42***
	(2.930)	(2.660)	(2.922)	(2.430)
Bank's Size	−2.287	−1.350	−2.500	−1.718
	(1.860)	(1.712)	(1.700)	(1.447)
Bank's Equity Ratio	−0.518	−0.373	−0.491	−0.358
	(0.415)	(0.404)	(0.402)	(0.386)
Bank's Net Income	0.368	0.315	0.382	0.327
	(0.313)	(0.324)	(0.315)	(0.314)
Bank's Cost of Deposits	0.0659	−0.114	0.143	0.416
	(0.628)	(0.619)	(0.534)	(0.402)
Change in Unemp. Rate, Bank's State(s)	0.649	0.563	0.820	1.035**
	(0.572)	(0.550)	(0.565)	(0.503)
Firm-Bank Fixed Effects	Yes	Yes	Yes	Yes
Firm's State-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	38,807	38,807	38,807	38,807
Firms	4,827	4,827	4,827	4,827
Banks	436	436	436	436
Adjusted R^2	0.464	0.464	0.465	0.464

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$
Columns (1) through (4) are panel fixed effect regressions. *Investment* is the firm's capital expenditures divided by the firm's lagged net PP&E and scaled by 100. *Housing Price Index, Bank's State(s)* is the bank holding company's housing price index in a given year. *Top-3* is an indicator for the three largest banks in our sample. Columns (2) and (4) use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. All continuous independent variables are scaled by their respective standard deviations. Standard errors are clustered by firm, bank, and year.

zero. Column 2 uses our instrumental variables and shows that a one-standard-deviation increase in housing prices (about $52,000 in year 2000 dollars) is associated with a 6.2 percentage point decrease in investment.[21] This effect is statistically significant at the 1% level.

The evidence in Column 2 of Table 4 shows that firms are on average negatively affected by the housing price boom through the lending channel. This result suggests that for many banks, capital constraints are such that some credit rationing occurs for the borrowing firms. If it occurs, we should expect this effect to be weaker for banks that are not significantly constrained.

21 The first-stage regressions for Table 4 are presented in Table B.4 in Online Appendix B. The Kleibergen-Paap Wald F-statistics show that weak instruments are not a concern in these regressions.

In Columns 3 and 4 of Table 4, we allow for housing prices for the three largest banks to have a differential effect on firm investment.[22]

For the set of banks that do not include the top three, we indeed find a stronger effect. In our instrumented specification (Column 4), a one-standard-deviation increase in housing prices corresponds to a 8.8 percentage point decrease in investment. At the same time, for the largest three banks, the difference in the housing effect is a positive and statistically significant 4.9 percentage points. Combining the housing price estimate and the interaction term, the net effect of housing prices on firm investment is a negative 3.9 percentage points (statistically significant at the 5% level) for the three largest banks. In sum, the lending channel works in a significantly negative direction during this housing boom and carries substantial economic significance for the borrowing firms.

A remaining question is what is the relative sensitivity of firm investment in response to a reduction in lending. Column 6 of Table 2 shows that for a one-standard-deviation increase in housing prices, loan growth falls by 42.3%. Column 2 of Table 4, which is the analogous column for our investment results, shows that for a one-standard-deviation increase in housing prices, firm investment falls by 6.2 percentage points. Scaling this coefficient by the sample mean (29.7%) suggests a 20.9% relative change in investment for a one-standard-deviation increase in housing prices. Comparing these two percentages allows us to compare the sensitivity of firm investment to loan growth. Specifically, for a 1% decrease in loan growth, we find a 0.49% reduction in investment. Although firms internalize or replace a fraction of their reduction in financing, we still find an economically meaningful reduction in investment.

2.4 Constraints at the bank and firm level

The driving mechanism for our results is the presence of constraints that: (i) prevent banks from meeting commercial loan demand in the face of strong mortgage demand, and (ii) prevent firms from being able to raise capital from sources other than their constrained bank. Separating the largest three banks is one approach to test for the differential impact of housing price booms based on constraints at the bank level, but this section investigates the differential impact of housing price booms on bank and firm constraints further.

2.4.1 Bank capital constraints. Table 5 splits the firm investment panel into constrained and unconstrained groups of banks, and uses the instrumented specification with the full set of controls, firm-bank fixed effects, and firm's state-year fixed effects as described in Section 2.3. Since we are dividing the sample of banks based on alternative dimensions of constraints, specifications

[22] Although these banks have about twice as many deposits as the next largest banks at the end of our sample, we get similar results if we include the fourth and fifth largest banks in our *Top-3* indicator as well. We also get similar results if we exclude the U.S. subsidiaries of foreign BHCs from our sample (see Online Appendix C.3).

Table 5
Investment regression for constrained and unconstrained banks

	Investment							
	Bank size		Bank leverage		Securitization activity		Employee growth	
	(Constrained) (1)	(Unconstrained) (2)	(Constrained) (3)	(Unconstrained) (4)	(Constrained) (5)	(Unconstrained) (6)	(Constrained) (7)	(Unconstrained) (8)
Housing Price Index,	−23.77**	−4.623	−14.44**	1.487	−11.12**	−7.454***	−24.91***	−6.976*
Bank's State(s)	(9.249)	(2.970)	(7.175)	(3.348)	(4.506)	(2.322)	(9.459)	(3.919)
Lagged Market to Book	8.113***	9.165***	8.567***	10.75***	8.021***	10.60***	9.254***	10.16***
	(1.549)	(0.717)	(1.550)	(0.979)	(1.501)	(0.847)	(1.993)	(0.656)
Cash Flow	9.859***	7.474***	11.53***	4.313***	7.908***	7.316***	8.354***	6.412***
	(1.424)	(1.244)	(1.196)	(1.250)	(1.415)	(1.552)	(1.887)	(1.217)
Lagged Firm Size	−23.61***	−17.32***	−24.73***	−10.92***	−21.11***	−17.20***	−15.53***	−22.13***
	(4.567)	(3.613)	(3.772)	(3.064)	(5.168)	(3.997)	(5.240)	(5.242)
Bank's Size	−4.430	−2.038	−0.142	−0.879	−3.468	−2.090	−0.748	0.228
	(4.344)	(2.715)	(2.307)	(2.797)	(3.313)	(2.381)	(2.840)	(2.599)
Bank's Equity Ratio	−0.335	−0.565	0.817	−0.497	−0.228	−0.223	1.315	−0.364
	(1.442)	(0.601)	(0.702)	(0.531)	(0.926)	(0.678)	(1.281)	(0.668)
Bank's Net Income	0.127	0.0380	0.0619	0.766**	0.830	1.078	1.545*	0.861
	(0.350)	(0.959)	(0.327)	(0.337)	(0.508)	(0.676)	(0.812)	(0.915)
Bank's Cost of Deposits	0.932**	−1.255	0.754	−0.370	0.265	−2.861**	−4.323***	0.585
	(0.393)	(1.151)	(0.599)	(1.212)	(0.696)	(1.382)	(0.980)	(0.728)
Change in Unemp. Rate,	0.551	−7.153	0.782	0.575	1.509	−0.0475	1.246	0.411
Bank's State(s)	(0.793)	(6.319)	(0.749)	(1.011)	(0.986)	(0.979)	(2.199)	(1.270)
Wald Test:								
(Constrained Banks = Unconstrained Banks)	3.89**		4.05**		0.52		3.07*	
Firm-Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm's State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,018	29,041	18,696	17,038	10,933	23,892	10,447	18,194
Firms	1,864	3,892	3,296	2,967	2,329	3,607	2,507	3,416
Banks	227	294	348	228	226	293	267	327
Adjusted R^2	0.512	0.480	0.511	0.545	0.515	0.492	0.611	0.511

Standard errors in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$
Columns (1) through (8) are panel fixed effect regressions. All columns use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. Banks in the largest quintile by deposits are designated as unconstrained, and banks outside the largest quintile are designated as constrained (Columns (1) and (2)). Banks in the largest quintile by equity to assets are designated as unconstrained, and banks outside the largest quintile are designated as constrained (Columns (3) and (4)). Banks without securitization activity are designated as constrained, and banks with securitization activity are designated as unconstrained (Columns (5) and (6)). Banks in the bottom two quintiles of employee growth are designated as constrained, and all other banks are designated as unconstrained (Columns (7) and (8)). All independent variables are scaled by their sample standard deviations. Standard errors are clustered by firm, bank, and year. The Wald Test provides the χ^2 statistic on whether the *Housing Price Index, Bank's State(s)* coefficient is statistically different across the constrained and unconstrained samples.

reported in Table 5 do not include interaction terms with the three largest banks. Given the skewed distribution of banks by deposits, we divide banks into quintiles, and classify banks in the largest quintile by deposits as unconstrained and the remaining banks as constrained. We find significant differences in the effect of housing prices on firm investment. Firms borrowing from the constrained banks have a marginal effect of –23.8 percentage points (Column 1), compared with –4.6 percentage points for the unconstrained banks (Column 2). The difference between the estimates is statistically significant.

Using bank leverage as the measure of constraints, in Columns 3 and 4 we again find significant differences between banks. We consider banks in the highest quintile by equity ratios (lowest leverage) as unconstrained and the remaining banks as constrained. Firms that borrow from constrained banks have a marginal effect of housing prices on investment of –14.4 percentage points, compared with an insignificant coefficient for unconstrained banks. The difference between the coefficients is statistically significant. Columns 5 and 6 divide our sample using our securitization activity indicator. We find that the marginal effect of housing prices for the nonsecuritizing banks is a statistically significant –11.1 percentage points, compared with –7.5 percentage points for the less constrained securitizing banks. Thus, even the securitizing banks exhibit crowding-out effects. As the difference between the point estimates is not statistically significant, it seems that in our sample, the presence of securitization activity does not alleviate bank constraints sufficiently.

Another potential friction driving the effect could be organizational or personnel constraints. In Columns 7 and 8 of Table 5, we divide banks by employee growth rates.[23] To make sure that we are not capturing differences in economic conditions in the firm's location, we exclude the individual firm's state from the bank's employee growth calculation and use a one-year lag. We find that the bottom two quintiles have an average negative employee growth rate, while the top three quintiles have a positive employee growth rate. Hence, we classify the bottom two quintiles of banks as constrained banks. For this group, housing prices have a marginal effect of –24.9 percentage points on firm investment. For the remaining, relatively unconstrained banks, we find a marginal effect of –7 percentage points, with the difference between the two coefficients being statistically significant. This finding is consistent with personnel constraints playing a role in crowding-out effects.

2.4.2 Firm capital constraints. The mechanism behind our effect also requires binding capital constraints at the firm level. This section uses a few different variables to capture differences in constraints: firm size, bond ratings, and the presence of a credit line. Table 6 presents the instrumental variables specification with the full set of controls (as in Table 4) for each subsample.

[23] Additional details of how employee growth is measured are given in Online Appendix A.2.

Table 6
Investment regression for constrained and unconstrained firms

	Investment					
	Firm size		Bond ratings		Credit line	
	(Constrained) (1)	(Unconstrained) (2)	(Constrained) (3)	(Unconstrained) (4)	(Constrained) (5)	(Unconstrained) (6)
Housing Price Index,	−19.47***	0.0121	−14.39***	−5.022	−40.89*	−9.345***
Bank's State(s)	(5.398)	(4.017)	(4.005)	(4.484)	(21.89)	(2.696)
Top-3 × HPI,	5.662	1.632	6.615*	1.244	14.33	5.436***
Bank's State(s)	(7.794)	(2.796)	(3.553)	(2.495)	(14.83)	(1.912)
Lagged Market to Book	8.506***	4.637***	9.481***	5.894***	5.581***	9.363***
	(1.342)	(1.540)	(0.675)	(1.424)	(1.349)	(0.686)
Cash Flow	6.252***	8.868***	8.179***	7.392***	8.314***	8.058***
	(1.262)	(1.783)	(1.316)	(1.401)	(3.154)	(1.098)
Lagged Firm Size	−27.52***	−11.29***	−20.97***	−19.29***	−9.813	−20.05***
	(5.459)	(2.123)	(3.816)	(4.367)	(7.832)	(2.775)
Bank's Size	−3.336	−0.665	−2.705	−1.698	−11.94	−0.228
	(3.324)	(2.215)	(2.434)	(2.214)	(7.340)	(1.387)
Bank's Equity Ratio	−0.762	−0.459	−0.743	−0.228	−4.438***	−0.113
	(0.946)	(0.431)	(0.612)	(0.498)	(1.440)	(0.487)
Bank's Net Income	0.219	−0.193	0.415	0.111	2.648**	0.430
	(0.731)	(0.231)	(0.474)	(0.273)	(1.269)	(0.367)
Bank's Cost of Deposits	0.551	−0.264	0.533	−0.195	−3.410*	0.344
	(0.931)	(0.557)	(0.586)	(0.704)	(1.983)	(0.488)
Change in Unemp. Rate,	0.826	1.058	1.354	0.384	3.345	1.102**
Bank's State(s)	(1.203)	(0.873)	(0.899)	(0.744)	(2.381)	(0.532)
Wald Test: (Constrained = Unconstrained)	8.39***		2.43		2.05	
Firm-Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm's State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,609	11,085	21,436	16,481	4,189	34,677
Firms	2,559	1,180	3,499	1,819	856	4,585
Banks	363	156	400	245	228	389
Adjusted R^2	0.448	0.730	0.454	0.535	0.435	0.479

Standard errors in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$
Columns (1) through (6) are panel fixed effect regressions. All columns use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. Splitting the sample into terciles by firm size, constrained firms are in the bottom tercile and unconstrained firms are in the top tercile (Columns (1) and (2)). In Columns (3) and (4), firms with any public bond rating are designated as unconstrained and firms without any rating are designated as constrained. Firm-bank observations that contain a credit line are designated as unconstrained and firm-bank observations without a credit line are designated as constrained (Columns (5) and (6)). All continuous independent variables are scaled by their sample standard deviations. Standard errors are clustered by firm, bank, and year. The Wald Test provides the χ^2 statistic on whether the *Housing Price Index, Bank's State(s)* coefficient is statistically different across the constrained and unconstrained samples.

Hadlock and Pierce (2010) show that firm size is an important predictor of financial constraints. As the size distribution of firms is not as skewed as that of banks, we divide firms into size terciles. Columns 1 and 2 classify firms that are in the lowest tercile by firm size (as measured by book assets) as constrained and firms in the highest size tercile as unconstrained. For constrained firms that do not borrow from the three largest banks (Column 1), the marginal effect is a 19.5 percentage point decrease in investment, compared with an insignificant 0.01 percentage point increase for unconstrained firms.

Columns 3 and 4 of Table 6 split the sample according to whether a firm has a bond rating, including a speculative or investment-grade one. The constrained subsample (no rating) in Column 3 has a large negative coefficient associated with housing prices in the bank's states (–14.4 percentage points). The coefficient for the relatively unconstrained subsample (bond rating present) in Column 4 is –5.0 percentage points, which is not statistically significant.

One additional channel that may mitigate financial constraints would be the presence of a credit line. To the extent that firms do not use all of their credit line immediately, its presence may weaken the rationing they receive when housing prices increase. We therefore use the DealScan loan package data to see if an existing package contains a credit line.[24] We find that for firms without a credit line (Column 5), the marginal effect of housing prices on investment is large, at –41 percentage points. For the firms with a credit line (Column 6), the marginal effect is still negative but smaller in terms of economic magnitude, at –9.3 percentage points. Even for the firms that have access to credit lines, housing prices have a negative effect on investment. Hence, though the point estimate is higher for the constrained sample, the test for differences between the estimated coefficients is not statistically significant.

2.5 Evolution of financial frictions

Our crowding-out result is the combination of two forces. First, it requires a strong increase in housing prices, which incentivizes additional mortgage lending. Second, sufficient frictions also need to be present that force banks to reduce commercial lending and prevent firms from fully substituting to alternative sources of financing. In Section 2.4, we show that the effect is strongest for those banks and firms for which these frictions are most prevalent. Over our sample period, securitization became more widespread and regulatory changes allowed for more bank-branch expansion. Even with these developments, certain fundamental frictions, such as moral hazard and adverse selection, will remain. As a result, banks can be capital constrained even in relatively good economic times. The question becomes to what extent these financial frictions have lessened over time.

To consider this question, we reestimate the effect of housing prices on bank commercial lending over our sample, allowing this effect to vary from year to year.[25] Figure 3 plots the annual coefficients. While the estimates for the effect are always negative, the effect on commercial lending has lessened over time. Although the reduction in commercial lending per dollar increase of housing prices has attenuated, housing prices boomed in the later part of our sample. Using the coefficients from Figure 3 and the average change in housing prices each year, we estimate an average annual decrease in commercial lending growth of 3.33% for 1988–99 and an average annual decrease in commercial

[24] While undrawn credit lines are a better measure of constraints, we have data only on the presence but not the utilization of credit lines.

[25] Specifically, we reestimate Column 1 of Table 3 allowing for differential effects for housing prices in each year.

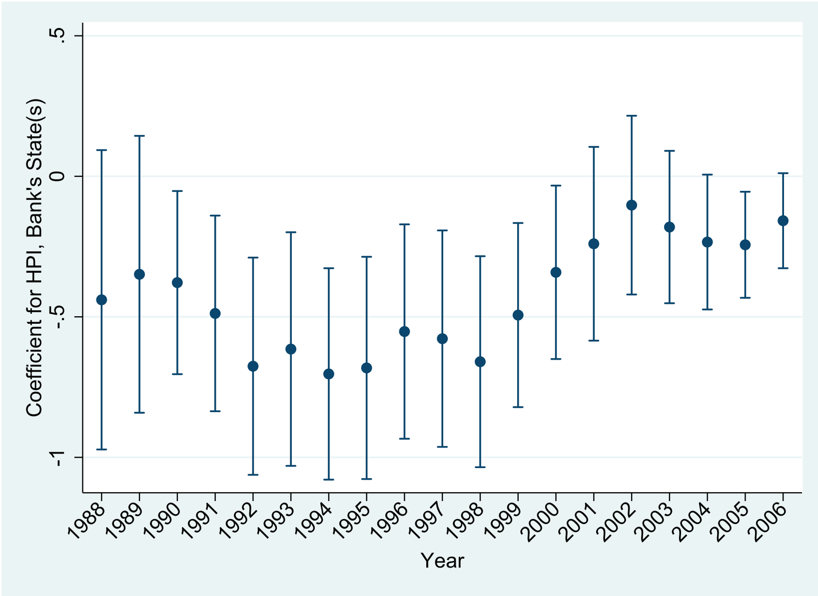

Figure 3
Effect of crowding-out on C&I lending over time
This figure plots annual estimates of the coefficient for the effect of *Housing Price Index, Bank's State(s)* on *Log(Dollar Outstanding Loans)* (similar to Column 1, Table 3). Confidence intervals of 95% are provided for the estimated coefficients.

lending growth of 5.97% for 2000–2006. In our sample period, the decrease in financial frictions is dominated by the strong increase in housing prices, leading to a larger combined effect during the peak period of the housing price boom.

3. Further Evidence in Support of Crowding-Out

3.1 Additional tests to rule out firm demand-side explanations

Throughout Section 2, we use our instrumental variables to address the concern that housing prices are picking up omitted economic factors that affect lending and firm investment. An additional concern discussed in Section 1.4 is that our estimates may still be affected by differences in housing demand or that housing prices may directly affect firm investment decisions. These concerns are specific examples of the broader argument that demand-side factors might be contributing to our results.

In general, we believe these demand-side factors are addressed in our empirical approach. We measure housing prices at the bank, rather than firm, level. When possible, we use firms' state-year fixed effects, which control for any local economic factors in the firm's location. These fixed effects account for a wide range of demand factors, which would include local housing demand and the cost of land for the firm. In addition, we use firm-time fixed effects in some of our loan growth results (Section 2.1), which control not only for local economic conditions in general but more specifically any demand factors related to the firm. Across all these specifications, we find evidence of crowding-out on the part of banks.

Nevertheless, to further check that our results are not driven by omitted firm demand factors related to housing prices, we run additional specifications presented in Table 7. Columns 1 and 2 of Table 7 include the housing price index in the firm's state as an additional control.[26] The estimated effect of housing prices in the bank's states on firm investment remains significantly negative and similar in magnitude to the results in Table 4. This result suggests that our main housing price variable is indeed capturing the lending bank's activity and not the firm's local real estate conditions. Alternatively, Columns 3 and 4 use fixed effects at the firm's county-year level as a finer control of local demand. We find results similar to our main specifications in Table 4.

As a different approach, we consider a subsample for which the state location of the borrowing firm does not overlap with any of the top five states for the bank, as measured by the concentration of its deposits. The results are presented in Columns 5 and 6 of Table 7. The geographic segmentation in this sample limits the likelihood that firm demand factors are correlated with housing prices in the bank's states. The ordinary least squares (OLS) estimates in Column 5 are similar in magnitude to those in Table 4 but the effect of housing prices on firm investment is not statistically significant for the non top-three banks. The instrumented specification in Column 6 is similar in magnitude and statistically significant.

Finally, to address the concern that land availability and real estate prices affect firm investment due to their effect on the firm's production cost, we exclude firms that rank in the top tercile by land intensity. Our land intensity variable captures the share of capital income that is attributable to land and is discussed in more detail in Online Appendix A.2. If a reduction in firm demand due to higher real estate prices is a major driver of our results, the effect should be concentrated in the land-intensive firms. The results, presented in Columns 7 and 8 of Table 7, suggest that a demand-side explanation involving higher cost of land as an input cannot explain away the crowding-out effect.[27]

3.2 Loan interest rate

If the results in Section 2 are driven by a reduction in the bank credit supply, we may expect an increase in the price of credit. A decrease in the price of credit, alternatively, is consistent with firms decreasing demand in response

[26] Because the index varies at the firm-state level, we use year fixed effects rather than firms' state-year fixed effects for these specifications.

[27] We explore this channel further using commercial real estate prices and land intensity in Online Appendix C.2. While we find some evidence of the cost of land affecting firm investment, it is much smaller in economic magnitude than our main crowding-out effect.

Table 7
Investment regression: Robustness checks

	Investment							
	Include firm's state HPI		County-level fixed effects		Exclude overlapping states		Exclude land intensive industries	
	(OLS) (1)	(IV) (2)	(OLS) (3)	(IV) (4)	(OLS) (5)	(IV) (6)	(OLS) (7)	(IV) (8)
Housing Price Index, Bank's State(s)	−3.877**	−7.347**	−4.497***	−10.86***	−3.146	−8.415**	−3.134	−4.829**
	(1.903)	(2.912)	(1.665)	(1.515)	(2.522)	(3.275)	(1.981)	(2.375)
Top-3 × HPI, Bank's State(s)	2.922***	3.782**	3.285***	5.186***	2.374*	6.561*	2.615**	3.354**
	(1.096)	(1.914)	(0.990)	(1.395)	(1.401)	(3.404)	(1.137)	(1.392)
Housing Price Index, Firm's State	−0.470	−0.238						
	(1.051)	(1.018)						
Lagged Market to Book	9.187***	9.180***	8.993***	9.350***	9.051***	8.807***	7.483***	7.612***
	(0.594)	(0.589)	(0.931)	(0.853)	(1.333)	(1.265)	(0.829)	(0.810)
Cash Flow	8.555***	8.555***	8.452***	8.865***	6.833***	7.039***	6.987***	7.354***
	(1.121)	(1.120)	(1.184)	(1.083)	(1.317)	(1.296)	(1.099)	(1.020)
Lagged Firm Size	−19.35***	−19.22***	−19.88***	−20.06***	−22.37***	−23.90***	−14.59***	−14.07***
	(2.793)	(2.781)	(2.962)	(2.534)	(4.748)	(4.042)	(2.936)	(2.682)
Bank's Size	−2.956*	−2.978*	−0.350	0.521	−3.281	−4.431**	−0.448	−1.725
	(1.641)	(1.664)	(1.532)	(1.287)	(2.303)	(2.062)	(1.800)	(1.644)
Bank's Equity Ratio	−0.463	−0.444	−0.712	−0.616	−0.884	−0.582	−0.0446	0.211
	(0.375)	(0.379)	(0.458)	(0.425)	(0.681)	(0.638)	(0.484)	(0.448)
Bank's Net Income	0.321	0.412	0.355	0.355*	0.293	0.218	0.382	0.0732
	(0.301)	(0.318)	(0.298)	(0.209)	(0.336)	(0.323)	(0.333)	(0.269)
Bank's Cost of Deposits	0.0482	−0.0146	−0.550	−0.363	1.346*	1.394**	0.139	0.507
	(0.680)	(0.670)	(0.588)	(0.539)	(0.749)	(0.651)	(0.737)	(0.702)
Change in Unemp. Rate, Firm's State	−0.862***	−0.837***						
	(0.283)	(0.283)						
Change in Unemp. Rate, Bank's State(s)	1.050*	1.220**	0.791*	1.057***	1.157*	0.919*	0.797	0.650
	(0.551)	(0.551)	(0.481)	(0.402)	(0.663)	(0.548)	(0.660)	(0.591)

(continued)

Table 7
Continued

	Investment							
	Include firm's state HPI		County-level fixed effects		Exclude overlapping states		Exclude land intensive industries	
	(OLS) (1)	(IV) (2)	(OLS) (3)	(IV) (4)	(OLS) (5)	(IV) (6)	(OLS) (7)	(IV) (8)
Firm-Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No	No	No	No	No
Firm's State-Year Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes
Firm's County-Year Fixed Effects	No	No	Yes	Yes	No	No	No	No
Observations	38,851	38,851	36,120	36,120	20,906	20,906	25,491	25,491
Firms	4,831	4,831	4,530	4,530	2,947	2,947	3,268	3,268
Banks	437	437	424	424	320	320	359	359
Adjusted R^2	0.463	0.463	0.501	0.501	0.480	0.480	0.495	0.495

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Columns (1) through (8) are panel fixed effect regressions. Columns (2), (4), (6), and (8) use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. Columns (1) and (2) include the housing price index of the firm's state as an additional control. Columns (3) and (4) include fixed effects at the firm's county-year level. Columns (5) and (6) exclude firm-bank-year observations where the firm's state matches one of the bank's five largest deposit states. Columns (7) and (8) exclude firm-bank-year observations in the top tercile by land intensity. All continuous independent variables are scaled by their respective standard deviations. Standard errors are clustered by firm, bank, and year.

to higher housing prices. We investigate the effect of housing prices on loan interest rates using the *All In Drawn Spread* variable from DealScan, which is a standardized spread over LIBOR, inclusive of annual fees. Observations are included in this panel when firms originate a new loan package with a specific bank. The specification estimated is as follows:

$$\begin{aligned}\text{All In Drawn Spread}_{ijt} = &\alpha_j + \gamma_{st} + \delta_1 \text{Housing Prices}_{jt-1}\\ &+ \delta_2 \text{Firm Vars.}_{it-1} + \delta_3 \text{Bank Vars.}_{jt-1}\\ &+ \delta_4 \text{Macro Vars.}_{ijt-1} + \delta_5 \text{Loan Chars.}_{ijt} + \varepsilon_{ijt}. \qquad (4)\end{aligned}$$

The control variables in this regression are similar to those in our investment regression specifications with a few additions: we include lagged Altman's z-score and lagged book leverage to control for differences in firm credit quality, and include the amount and maturity of the loan package to control for observable differences in loan terms. We also include indicators for whether the loan package is for the purpose of a takeover or acquisition and if the package contains a revolving credit line, given these loan types may entail different amounts and pricing. Across all specifications, we include bank fixed effects (α_j) and firms' state-year fixed effects (γ_{st}).[28]

We suspect that the effect of housing prices on interest rates will be concentrated in the constrained banks and firms in our sample. Constrained banks, which will be reducing commercial credit in response to increased housing prices, will be more likely to keep or offer higher interest rate commercial loans. Constrained firms, to the extent that they receive new credit, will likely face higher borrowing costs as they do not have as many alternative sources of external capital. In Table 8, we use the same bank and firm constraint splits as in Section 2.4. Columns 1 and 2 of Table 8 consider the effect of an increase in housing prices on loan pricing by dividing banks into constrained and unconstrained banks by size. We find that constrained banks on average charge 15.4 basis points more on loans for a one-standard-deviation increase in housing prices. The point estimate is positive but statistically insignificant for unconstrained banks. Columns 3 and 4 divide banks by equity ratios. The constrained (more levered) banks increase loan spreads as compared with the unconstrained (less levered) banks when housing prices increase. The difference in interest rate sensitivity to housing prices is not statistically significant when banks are divided by size but is significant when banks are divided by leverage. This may be because even among the top quintile of banks by size, some banks may still be meaningfully constrained.

28 In our sample, 1,987 of the 4,812 firms do not have more than one observed loan package across all banks. The firms with one loan package tend to be smaller (average asset size of $1.1 billion versus $3.8 billion for the other firms) and are less likely to have an investment-grade bond rating (6% are investment grade versus 25% for the other firms). To avoid excluding these firms from the analysis, we do not include firm fixed effects. Similarly, to avoid losing observations from DealScan lenders that issue only one package, we choose to have bank fixed effects at the bank holding company level rather than the DealScan lender level.

Table 8
Interest rate regression

	All in drawn spread							
	Bank size		Bank leverage		Firm size		Bond ratings	
	(Constrained) (1)	(Unconstrained) (2)	(Constrained) (3)	(Unconstrained) (4)	(Constrained) (5)	(Unconstrained) (6)	(Constrained) (7)	(Unconstrained) (8)
Housing Price Index, Bank's State(s)	15.30**	4.122	14.50***	1.344	51.17***	1.238	17.59*	12.47
	(7.334)	(6.887)	(5.623)	(3.629)	(16.23)	(6.333)	(10.61)	(8.523)
Top-3 × HPI, Bank's State(s)					7.641	−0.855	−0.183	−4.102
					(7.536)	(4.599)	(5.254)	(5.925)
Lagged Book Leverage	18.78***	25.54***	23.62***	24.21***	19.30***	20.62***	23.50***	20.21***
	(2.868)	(2.214)	(2.593)	(2.835)	(2.867)	(2.475)	(2.204)	(3.117)
Lagged Market to Book	−8.897***	−14.11***	−11.33***	−14.58***	−11.51***	−10.47**	−11.41***	−18.37***
	(1.954)	(2.620)	(2.398)	(2.491)	(1.274)	(4.378)	(1.850)	(5.141)
Lagged Altman's Z-Score	−9.752**	−14.39***	−9.894***	−16.77***	−7.142**	−27.03***	−11.38***	−22.06***
	(4.309)	(3.706)	(3.274)	(5.649)	(3.023)	(3.912)	(2.666)	(4.267)
Lagged Firm Size	−69.27***	−64.71***	−70.15***	−60.73***	−60.88***	−40.42***	−65.74***	−66.08***
	(5.241)	(2.115)	(1.829)	(2.776)	(7.813)	(3.922)	(3.494)	(3.283)
Loan Amount to Lagged PPE	1.941	2.987**	1.601	6.269***	−0.349	18.18***	2.015	7.479***
	(2.873)	(1.491)	(1.970)	(1.508)	(2.128)	(3.529)	(1.788)	(2.391)
Log(Maturity)	−25.86***	−8.950**	−20.90***	−2.139	−23.60***	6.801*	−22.22***	−0.0560
	(2.691)	(3.851)	(3.234)	(4.685)	(2.509)	(3.913)	(2.629)	(4.326)
Takeover Loan	43.98***	32.11***	38.54***	28.99***	29.00***	27.42***	36.24***	32.14***
	(7.352)	(3.781)	(6.190)	(4.395)	(4.079)	(5.513)	(4.304)	(5.104)
Revolving Credit Line	20.45**	−12.12	13.27*	−24.35*	23.45**	−32.66***	19.80**	−31.05***
	(9.180)	(10.43)	(7.519)	(14.62)	(9.236)	(8.079)	(8.804)	(10.16)
Bank's Size	−4.758	−0.689	−9.102***	−4.153	−33.34**	4.538	−5.214	−17.62
	(4.358)	(4.313)	(2.445)	(3.245)	(13.30)	(10.75)	(10.08)	(14.15)
Bank's Equity Ratio	7.481	4.553**	7.548*	0.202	2.179	5.154	0.557	1.487
	(5.158)	(2.076)	(4.224)	(2.316)	(4.123)	(3.754)	(4.271)	(3.445)

(continued)

Table 8
Continued

	All in drawn spread							
	Bank size		Bank leverage		Firm size		Bond ratings	
	(Constrained) (1)	(Unconstrained) (2)	(Constrained) (3)	(Unconstrained) (4)	(Constrained) (5)	(Unconstrained) (6)	(Constrained) (7)	(Unconstrained) (8)
Bank's Net Income	−3.228***	3.645	−1.170	0.258	0.630	−1.253	0.251	0.0179
	(1.182)	(2.956)	(1.892)	(3.060)	(2.693)	(2.636)	(2.738)	(1.999)
Bank's Cost of Deposits	3.340*	−2.540	1.803	−1.150	−4.256	−2.078	−2.714	5.649*
	(1.933)	(2.755)	(1.983)	(5.320)	(5.771)	(7.110)	(3.157)	(3.163)
Change in Unemp. Rate,	−2.807	3.164	−1.731	8.996**	−5.018	10.83**	−4.459	4.600
Bank's State(s)	(3.649)	(4.841)	(4.060)	(4.571)	(5.826)	(4.269)	(3.831)	(4.805)
Wald Test:								
(Constrained = Unconstrained)	1.23		3.86**		8.21***		0.14	
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm's State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,789	9,318	6,734	5,408	3,974	3,538	6,989	5,112
Firms	1,739	3,697	3,469	2,539	2,358	1,050	3,517	1,643
Banks	228	238	342	161	320	117	369	199
Adjusted R^2	0.426	0.397	0.426	0.396	0.285	0.352	0.381	0.425

Standard errors in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$

Columns (1) through (8) are panel fixed effect regressions. All columns use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. Banks in the largest quintile by deposits are designated as unconstrained, and banks outside the largest quintile are designated as constrained (Columns (1) and (2)). Banks in the largest quintile by equity to assets are designated as unconstrained, and banks outside the largest quintile are designated as constrained (Columns (3) and (4)). Splitting the sample into terciles by firm size, constrained firms are in the bottom tercile, and unconstrained firms are in the top tercile (Columns (5) and (6)). In Columns (7) and (8), firms with any public bond rating are designated as unconstrained, and firms without any rating are designated as constrained. All continuous independent variables are scaled by their sample standard deviations. Standard errors are clustered by firm, bank, and year. The Wald Test provides the χ^2 statistic on whether the *Housing Price Index, Bank's State(s)* coefficient is statistically different across the constrained and unconstrained samples.

Columns 5 and 6 consider the effect of housing prices on loan spreads for the smallest and largest terciles of firms in our sample. We find a large and significant increase in loan spreads for the smallest firms and an insignificant change for the largest firms. The difference in the point estimates in Columns 5 and 6 is statistically significant. We do not see meaningful differences between the three largest banks and the other banks as far as loan pricing is concerned. Columns 7 and 8 use the presence of public bond ratings as a measure of constraints and find that firms without a bond rating pay higher interest rates when housing prices increase. However, the difference in point estimates for the two groups is not statistically significant, suggesting that even firms with bond ratings may be constrained. Overall, our results are consistent with a reduction of credit supply by banks rather than a reduction in loan demand.

3.3 Relative profitability of mortgage and commercial loans

Our paper argues that mortgage lending crowds out commercial lending during housing booms. This argument requires that when housing prices increase, the profitability of the opportunity set of mortgage loans should increase relative to the profitability of the opportunity set of commercial loans. Although we cannot observe the full opportunity sets of loans for banks, we can compare the profitability and amounts of the loans extended by banks. If mortgage loans are becoming relatively more profitable as housing prices increase, we should expect increases in the loan volume and the average loan profitability. We should also expect that while extended commercial loans may become more profitable as banks ration low-profitability borrowers, the amount of commercial loans extended should be decreasing relative to mortgage lending.

To establish the presence of crowding-out effects, we have shown that when housing prices increase, commercial lending amounts decrease and the interest rates on loans increase. To test this further, we use two measures of loan profitability based on the bank's Call Report data: one for mortgage lending and an analogous measure for their commercial (C&I) lending. While we expect the profitability of both loan types to increase in response to higher housing prices, here we check the relative increases and whether they are consistent with the crowding-out mechanism.

Panel A of Table 9 shows the sensitivity of the profitability of the two types of loans with respect to housing prices. Since this analysis relies only on Call Report data, we use the panel of all reporting bank holding companies.[29] Column 1 shows that the profitability of C&I loans goes up as housing prices go up. Column 2 shows that real estate loan profitability also increases with increasing housing prices. Since we are comparing the same change in housing

---

29 This sample is all bank holding companies that have nonzero C&I loan profitability and nonzero real estate loan profitability. Because banks with less than $300 million in total assets did not have to report income separately for C&I loans until 2001, the sample size is smaller than the sample used for the bank asset analysis in Section 4.1. Average C&I loan profitability is 7.98%, and its standard deviation is 2.50%. Average real estate loan profitability is 7.02%, and its standard deviation is 1.41%.

Table 9
Loan profitability regressions

Panel A: Relative loan profitability

	C&I loan profitability (IV) (1)	Real estate loan profitability (IV) (2)	Profitability ratio (IV) (3)
Housing Price Index, Bank's State(s)	0.200***	0.218***	0.0272**
	(0.0470)	(0.0546)	(0.0130)
Bank's Size	−0.544***	−0.252***	0.0260***
	(0.0475)	(0.0215)	(0.00830)
Bank's Equity Ratio	−0.00832	0.00937	0.0115**
	(0.0390)	(0.0223)	(0.00559)
Bank's Net Income	0.134***	0.119***	−0.00924**
	(0.0303)	(0.0366)	(0.00414)
Bank's Cost of Deposits	0.0181	0.207***	0.0536***
	(0.0823)	(0.0560)	(0.0170)
Change in Unemp. Rate, Bank's State(s)	−0.0109	0.0177	0.00234
	(0.0351)	(0.0586)	(0.00671)
Year Fixed Effects	Yes	Yes	Yes
Observations	34,685	34,685	34,685
Banks	6,129	6,129	6,129
Adjusted R^2	0.184	0.201	0.0105

(continued)

prices across both columns, the larger estimate for real estate profitability is evidence that real estate loan profitability increases more than commercial loan profitability on average.

To directly address the relative sensitivity of profits in the two types of lending markets for the same bank, Column 3 compares the profitability of the two types of loans by dividing real estate loan profitability by commercial loan profitability for a bank in a year. We find that a one-standard-deviation increase in housing prices increases the profitability of real estate loans by 2.7% more than commercial loans for a given bank in a given year. This result helps reassure us that the profitability of C&I loans is not going up faster with housing prices than the profitability of mortgage loans.

Rather than comparing mortgage and C&I loan profitability, panel B of Table 9 compares average C&I loan profitability across banks. Another implication of the crowding-out mechanism is that the credit rationing and subsequent increase in average C&I profitability should be most concentrated in the more constrained banks. We find that, as expected, the three largest bank holding companies, which are likely the least constrained, have on average 3.05% lower loan profitability than the remaining banks (Column 1). Columns 2–4 include the housing price variable and show that as housing prices increase, the C&I loan profitability of banks increases even after controlling for the top-three banks.

The instrumented specification in Column 3 shows that a one-standard-deviation increase in housing prices leads to an increase of C&I loan profitability of 20 basis points. In Column 4, the housing price variable is interacted with the top-three banks indicator to test if these banks ration commercial lending at

Table 9
Continued

Panel B: C&I loan profitability and constrained banks

	C&I loan profitability			
	(OLS) (1)	(OLS) (2)	(IV) (3)	(IV) (4)
Top-3	−3.047***	−3.051***	−3.056***	−3.010**
	(0.798)	(0.812)	(0.830)	(1.318)
Housing Price Index,		0.0934***	0.202***	0.201***
Bank's State(s)		(0.0236)	(0.0475)	(0.0475)
Top-3 × HPI,				−0.0625
Bank's State(s)				(0.683)
Bank's Size	−0.482***	−0.498***	−0.516***	−0.516***
	(0.0463)	(0.0438)	(0.0417)	(0.0418)
Bank's Equity Ratio	−0.0113	−0.00802	−0.00427	−0.00426
	(0.0370)	(0.0374)	(0.0381)	(0.0381)
Bank's Net Income	0.110***	0.119***	0.130***	0.130***
	(0.0330)	(0.0313)	(0.0308)	(0.0308)
Bank's Cost of Deposits	−0.0363	0.000302	0.0427	0.0424
	(0.0775)	(0.0758)	(0.0758)	(0.0762)
Change in Unemp. Rate,	−0.0175	−0.0137	−0.00930	−0.00933
Bank's State(s)	(0.0409)	(0.0339)	(0.0354)	(0.0353)
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	34,685	34,685	34,685	34,685
Banks	6,129	6,129	6,129	6,129
Adjusted R^2	0.187	0.189	0.187	0.187

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Panel A compares the relative profitability of C&I and mortgage loans, and panel B looks at the effects of housing prices on C&I loan profitability for constrained and unconstrained banks. *C&I Loan Profitability* is defined as interest and fee income on C&I loans divided by the amount of C&I loans, scaled as a percentage. *Real Estate Loan Profitability* is defined as interest and fee income on real estate loans divided by the amount of real estate loans, scaled as a percentage. *Profitability Ratio* is defined as *Real Estate Loan Profitability* divided by *C&I Loan Profitability*. *Top-3* is an indicator for the three largest banks in our sample. Columns (1) through (3) in panel A and Columns (3) and (4) in panel B use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. All continuous independent variables are scaled by their sample standard deviations. Standard errors are clustered by bank holding company and year.

a different rate than other banks. Although the point estimate for the top-three banks interaction term is negative, it is not statistically significant.

3.4 Deregulation and crowding-out

As discussed in Section 1.4, a concern is that more financially constrained banks are more likely to be matched with firms with fewer investment opportunities, which could affect our results. We use firm-bank fixed effects to control for this issue, but as a further step, we exploit an exogenous shock to banks' financial constraints. Following Jayaratne and Strahan (1996), Demyanyk, Ostergaard, and Sørensen (2007), and Becker (2007), among others, we use the deregulation of intrastate branching as an exogenous shock to the cross-sectional variation in banks' constraints. Specifically, we test if after deregulation less capital is crowded out from commercial lending than before deregulation when the banking sector was more constrained.

Using the same sample as in Section 2.2, Table 10 reports the impact of deregulation on commercial lending in terms of the dollar amount and the number of loans. The dependent variable *Deregulation Measure* is the weighted

Table 10
Outstanding loans regression: Branch deregulation

	Log(dollar outstanding loans)		Log(number outstanding loans)	
	(OLS) (1)	(IV) (2)	(OLS) (3)	(IV) (4)
Housing Price Index, Bank's State(s)	−0.220*	−0.997***	−0.217*	−0.782***
	(0.125)	(0.257)	(0.123)	(0.233)
HPI × Deregulation	0.118*	0.663**	0.0743	0.594**
	(0.0637)	(0.324)	(0.0522)	(0.290)
Deregulation Measure	0.0621	0.606*	−0.0243	0.491*
	(0.114)	(0.328)	(0.0957)	(0.294)
Bank Size	0.120	0.208	0.0925	0.123
	(0.206)	(0.225)	(0.190)	(0.199)
Bank Equity to Assets	−0.126***	−0.0460	−0.0831**	−0.0147
	(0.0481)	(0.0550)	(0.0397)	(0.0426)
Bank Income to Assets	−0.0485	−0.0871*	−0.00657	−0.0339
	(0.0395)	(0.0518)	(0.0277)	(0.0420)
Bank Cost of Deposits	−0.302**	−0.649**	−0.213*	−0.506**
	(0.150)	(0.257)	(0.111)	(0.227)
Change in Unemp. Rate, Bank's State(s)	0.00339	0.00773	−0.0465	−0.0573
	(0.0431)	(0.0693)	(0.0345)	(0.0587)
Bank Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	5,215	5,215	5,215	5,215
Banks	617	617	617	617
Adjusted R^2	0.748	0.727	0.728	0.695

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$
Columns (1) through (4) are panel fixed effect regressions. *Log(Dollar Outstanding Loans)* is the log amount of outstanding DealScan loans with each bank in a given year. *Log(Number Outstanding Loans)* is calculated by taking the log-transform of the number of firms that have outstanding DealScan loans with each bank. *Deregulation Measure* is the share of states where intrastate bank branch expansion is deregulated for the bank, as weighted by its share of the bank's deposits. Columns (2) and (4) use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. All continuous independent variables are scaled by their sample standard deviations. Standard errors are clustered by bank and year.

average of the number of states in which statewide de novo branching is permitted, where the weighting is given by the bank's relative amount of deposits in each state. This exposure measure to deregulation is in effect similar to the housing price index measure, but goes from 0, where no state has been deregulated, to 1, where all the top 15 states in which the bank operates have been deregulated.

The baseline effect for housing prices across all specifications is negative and statistically significant. The interaction term between the deregulation measure and housing price index is consistently positive, showing that the negative effect of housing prices on loan amounts is mitigated as states become more deregulated. This is true for both the noninstrumented (Column 1) and instrumented specifications (Column 2). Columns 3 and 4 consider the number of loans instead of the amount of commercial loans. The results are similar to those in Columns 1 and 2. As these regulatory changes lessen bank-level constraints but are not related to the bank's current borrowers, these results add additional support to the argument that the crowding-out is driven by supply-side constraints rather than a demand-side or a matching-driven explanation.

4. Evidence on Other Bank and Firm Activity

4.1 Other bank activity

Section 2.2 investigates the impact of housing prices on commercial lending using DealScan data. However, such data is not available for other types of bank lending that we have not analyzed so far: real estate loans, MBS, commercial mortgages, and nonmortgage consumer loans. Hence, in this section, we use Call Report data at the BHC level to investigate how housing prices affect these dimensions of the bank's balance sheet.

Columns 1 and 2 of Table 11 present the marginal effect of housing prices on the amount of real estate loans without and with instrumentation, respectively.[30] We find that for a one-standard-deviation increase in housing prices in a bank's states, the amount of nonsecuritized real estate loans increases by 8.5% ($e^{0.0812}-1$). The instrumented specification suggests an even larger effect, although less precisely estimated. The estimates for MBS holdings (Columns 3 and 4) are positive and similar in magnitude to those of real estate loans, but not statistically significant. The effects for commercial mortgages (Columns 5 and 6) are also similar in magnitude to residential mortgage lending, and are statistically significant. This finding is consistent with the collateral channel presented in Chaney, Sraer, and Thesmar (2012), which we explore further in Online Appendix C.4.

In Columns 7 and 8, we consider the effect of housing prices on the fraction of non–real estate consumer lending. This asset category includes auto loans, student loans, credit card debt, and other forms of personal loans. Because any positive omitted economic shocks likely increase other forms of consumer loan demand, we expect a similar positive bias in the noninstrumented specification. In both specifications, consumer loans significantly decrease when housing prices in the bank's region increase. Rather than increasing all types of consumer loan activity with higher housing prices, banks appear to shift into mortgage lending at the expense of other forms of consumer debt.

4.2 Other firm outcomes

To better understand the crowding-out effect on firm activity, this section considers a wider set of firm policies. We consider additional firm real outcomes, namely acquisitions and R&D expenses. We also investigate the financing policy of the firm by considering dividend policy and capital structure changes.

Table 12 reports the results for specifications similar to those in Section 2.3. Column 1 shows that the size of a firm's acquisitions decline in magnitude

[30] For IV specifications involving real estate loans, MBS, or commercial mortgages, we only rely on the land unavailability instrument to be conservative. The reason is that the exclusion restriction may not hold for the mortgage interest rate instrument for these assets. Mortgage interest rates may directly affect the prices and thus the holding of these three asset classes outside the channel of housing prices.

Table 11
Other bank assets

	Log(loans)							
	All banks							
	Real estate loans		MBS		Commercial mortgages		Consumer loans	
	(OLS) (1)	(IV) (2)	(OLS) (3)	(IV) (4)	(OLS) (5)	(IV) (6)	(OLS) (7)	(IV) (8)
Housing Price Index,	0.0812***	0.544**	0.0564	0.626	0.0604***	0.436**	−0.0391**	−0.356***
Bank's State(s)	(0.0177)	(0.223)	(0.0369)	(0.401)	(0.0213)	(0.218)	(0.0175)	(0.134)
Bank's Size	0.508***	0.445***	0.491***	0.415***	0.604***	0.553***	0.422***	0.466***
	(0.0332)	(0.0368)	(0.0709)	(0.0847)	(0.0371)	(0.0408)	(0.0286)	(0.0360)
Bank's Equity Ratio	−0.0911***	−0.0960***	−0.108***	−0.113***	−0.108***	−0.112***	−0.0798***	−0.0757***
	(0.00863)	(0.00879)	(0.0214)	(0.0213)	(0.0118)	(0.0116)	(0.00863)	(0.00902)
Bank's Net Income	0.0540***	0.0428***	0.00328	−0.00979	0.0230***	0.0137	0.0443***	0.0518***
	(0.00706)	(0.00827)	(0.0135)	(0.0160)	(0.00697)	(0.00868)	(0.00545)	(0.00567)
Bank's Cost of Deposits	0.0685**	0.0494*	0.0327	0.0109	0.0807**	0.0639*	−0.0153	−0.00109
	(0.0319)	(0.0286)	(0.0338)	(0.0361)	(0.0360)	(0.0338)	(0.0106)	(0.0133)
Change in Unemp. Rate,	0.0123*	0.00364	−0.0281	−0.0370	0.0197*	0.0128	−0.00696	−0.000966
Bank's State(s)	(0.00711)	(0.0119)	(0.0180)	(0.0255)	(0.0113)	(0.0122)	(0.00521)	(0.00989)
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,040	82,040	66,005	66,005	80,957	80,957	81,949	81,949
Banks	8,231	8,231	7,187	7,187	8,166	8,166	8,217	8,217
Adjusted R^2	0.952	0.943	0.771	0.765	0.923	0.920	0.928	0.923

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$
Columns (1) through (8) are panel fixed effect regressions using the entire sample of bank holding companies. The loan variables (*Real Estate Loans*, *MBS*, *Commercial Mortgages*, *Consumer Loans*) are log transformations of their dollar amounts. Columns (2), (4), and (6) use the unavailable land measure as an instrument. Column (8) uses the unavailable land measure and its interaction with the national-level 30-year mortgage rate as instruments. All independent variables are scaled by their respective standard deviations. Standard errors are clustered by bank holding company and year.

Table 12
Effects on other firm variables

	Acquisitions		R&D expense		Dividend payout		Change in leverage		Change in debt		Change in equity	
	(IV) (1)	(IV) (2)	(IV) (3)	(IV) (4)	(IV) (5)	(IV) (6)	(IV) (7)	(IV) (8)	(IV) (9)	(IV) (10)	(IV) (11)	(IV) (12)
Housing Price Index,	−21.08**	−24.30***	−3.648	−8.117	−4.048*	−5.736**	−1.322	−1.946**	−3.502**	−5.225**	1.324	1.711
Bank's State(s)	(9.773)	(7.731)	(14.05)	(19.25)	(2.109)	(2.677)	(0.825)	(0.957)	(1.591)	(2.032)	(1.308)	(1.415)
Top-3 × HPI,		−5.140		2.557		3.497**		1.179		1.299		−2.246*
Bank's State(s)		(5.379)		(12.54)		(1.667)		(0.749)		(1.256)		(1.181)
Lagged Market to Book	8.586***	7.741***	10.57***	10.55***	0.397*	0.394*	−0.458	−0.421	1.957***	1.954***	1.336***	1.785***
	(1.530)	(1.342)	(3.434)	(3.470)	(0.214)	(0.216)	(0.342)	(0.329)	(0.598)	(0.598)	(0.380)	(0.469)
Cash Flow	15.73***	12.40***	−4.991*	−4.988*	0.872	0.870	−2.565***	−2.599***	−0.0759	−0.0771	2.648***	2.458***
	(3.527)	(3.105)	(2.578)	(2.590)	(0.905)	(0.902)	(0.341)	(0.335)	(0.315)	(0.314)	(0.366)	(0.388)
Lagged Firm Size	−93.46***	−80.83***	−16.21*	−16.03*	2.470***	2.491***	−2.319*	−2.343*	−30.92***	−30.88***	−16.64***	−18.39***
	(11.38)	(9.619)	(8.349)	(8.327)	(0.561)	(0.537)	(1.372)	(1.367)	(2.856)	(2.858)	(1.690)	(2.055)
Bank's Size	−4.594	−1.894	−19.20	−19.36	−0.160	−0.407	0.157	0.122	−0.275	−0.357	1.700**	1.327
	(5.754)	(5.287)	(15.11)	(15.68)	(1.198)	(1.089)	(0.360)	(0.347)	(1.217)	(1.188)	(0.841)	(0.936)
Bank's Equity Ratio	−2.200	−3.082**	−3.250*	−3.207*	−0.717**	−0.690*	−0.0833	−0.0503	−0.108	−0.0967	−0.584	−0.477
	(1.503)	(1.567)	(1.900)	(1.819)	(0.365)	(0.415)	(0.186)	(0.177)	(0.372)	(0.359)	(0.403)	(0.465)
Bank's Net Income	−0.483	−0.283	1.032	1.123	−0.159	−0.187	−0.0176	−0.0434	0.226	0.248	0.352	0.525**
	(1.021)	(1.036)	(1.376)	(1.382)	(0.537)	(0.526)	(0.109)	(0.103)	(0.171)	(0.174)	(0.229)	(0.244)
Bank's Cost of Deposits	2.771	3.267*	−2.839	−2.822	−0.625***	−0.496***	0.467*	0.388	0.382	0.397	0.649	0.363
	(2.408)	(1.896)	(5.456)	(5.003)	(0.187)	(0.184)	(0.283)	(0.239)	(0.566)	(0.521)	(0.466)	(0.548)
Change in Unemp. Rate,	−1.789	−1.718	7.438**	7.670**	−0.471	−0.343	−0.432	−0.372	−0.232	−0.140	−0.294	−0.215
Bank's State(s)	(2.074)	(1.907)	(3.392)	(3.464)	(0.628)	(0.631)	(0.301)	(0.288)	(0.369)	(0.357)	(0.454)	(0.497)
Firm-Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm's State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36,960	36,960	18,444	18,444	38,932	38,932	38,799	38,799	38,802	38,802	38,850	38,850
Firms	4,720	4,720	2,475	2,475	4,838	4,838	4,821	4,821	4,822	4,822	4,833	4,833
Banks	433	433	332	332	437	437	437	437	437	437	437	437
Adjusted R^2	0.372	0.371	0.538	0.538	0.405	0.405	0.0774	0.0775	0.246	0.245	0.147	0.147

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$

Columns (1) through (12) are panel fixed effect regressions. *Acquisitions* is the firm's acquisitions divided by lagged net PP&E and scaled by 100. *R&D Expense* is the firm's R&D expenses divided by lagged net PP&E and scaled by 100. *Dividend Payout* is the firm's annual dividend payout divided by lagged net PP&E and scaled by 100. *Change in Leverage* is the annual change in book leverage, scaled by 100. *Change in Debt* is the annual change in total debt outstanding divided by lagged total assets and scaled by 100. *Change in Equity* is the percentage change in shares of outstanding common equity. All columns use the unavailable land measure and its interaction with the national 30-year mortgage rate as instruments. All continuous independent variables are scaled by their sample standard deviations. Standard errors are clustered by firm, bank, and year.

by 21 percentage points (as scaled by lagged PP&E) in response to a one-standard-deviation increase in housing prices of the lending bank. Column 2 does not find a statistical difference for firms borrowing from top-three banks. Columns 3 and 4 have negative estimates for the impact of a housing price increase on firm R&D, but the estimates are not statistically significant. Thus we find evidence of crowding-out effects for acquisitions, but not for R&D. This may be because acquisitions, similar to capital expenditures, are financed by banks but R&D, is generally not financed by banks (Brown, Fazzari, and Petersen 2009).

Columns 5 and 6 consider the impact of increasing housing prices on firm dividend policy. We find that firms reduce their dividend payout when banks experience higher housing prices. Column 5 shows that for a one-standard-deviation increase in housing prices, an average firm that borrows from such a bank reduces its dividend payout by 4 percentage points (scaled by lagged PP&E for comparability). Column 6 shows that the effect is weaker for the three largest banks, which are less constrained. The decrease in dividends in response to higher housing prices is consistent with firms facing constraints in raising additional capital.

If banks reduce lending to firms, and firms are unable to substitute other sources of debt financing, we should expect a reduction in firm leverage. Columns 7 and 8 show that this is indeed the case. Column 7, which does not separate the top three from the other banks, shows a statistically insignificant negative effect. Column 8, which separately controls for the interaction of housing prices for the top-three banks, suggests that book leverage falls by approximately 2 percentage points in response to a one-standard-deviation increase in housing prices for the lending bank. We check whether this decrease in leverage is driven by a reduction in debt rather than an increase in equity issuance. Columns 9 and 10 report a statistically and economically significant negative relationship between debt and housing prices. In sum, when housing prices increase, we find that firms decrease debt levels and leverage. This is consistent with banks crowding-out lending and firms not being able to fully replace it with alternative sources of debt.

Columns 11 and 12 test whether equity repurchases decrease and new equity issuances increase in response to housing prices. Although we find positive estimates for the effect of housing prices on the change in equity, they are not significant. So while we find that firms lose debt financing, they do not replace it by raising new equity. This is presumably because equity issuance is a costly way to substitute for the lost financing (Hennessy and Whited 2007).

5. Macroeconomic Implications

The results so far suggest that banks move capital away from commercial lending and toward mortgage lending when situated in stronger housing markets. Firms are unable to replace the capital that banks reallocated to the housing sector from the commercial and industrial sector, leading to a net negative impact on firm investment levels. This section discusses the macroeconomic importance of our mechanism, taking into account the potential offsetting effect of the collateral channel. Online Appendix C.4 considers the role of the collateral channel as documented in Chaney, Sraer, and Thesmar (2012) in detail.

Column 4 of Table 4 shows that a one-standard-deviation increase in housing prices within a bank, which is about $52,000 in year 2000 prices, reduces firm investment by 3.89 percentage points for firms that obtain loans from the three largest banks and by 8.83 percentage points for firms borrowing from the smaller banks. The three largest banks have 40% of the firm-bank-year observations in our data set. Putting a weight of 0.6 on –8.83 and a weight of 0.4 on –3.89, we obtain an average reduction in investment of 6.85 percentage points as a fraction of lagged net PP&E. Since the mean investment is 29.7 percentage points as a fraction of PP&E in our sample, this translates into a 23.1% reduction in investment.

To measure the impact of the increase in housing prices on an aggregate level, we next conduct some simple back-of-the-envelope calculations. We obtain aggregate nonfinancial corporate business capital expenditure from the Fed Flow of Funds. We use the gross domestic product (GDP) implicit price deflator (GDPDEF) series to adjust for inflation, where the index is 100 for year 2009 (GDP used is $14,418 billion). For the whole sample period of 1988–2006, the average annual capital expenditure of nonfinancial corporate business is U.S.$1,063 billion. However, not all nonfinancial firms borrow from banks and are affected by the bank lending channel. To get some measure of how prevalent our effect is, we calculate that our sample covers about 23% of the broader nonfinancial Compustat universe. This likely underestimates the number of firms affected, as only the subset of firms that are linked to banks through the DealScan loan data are captured. Nevertheless, 23% of U.S.$1,063 billion gives us about U.S.$244.5 billion of capital expenditure affected by the bank lending channel. Using the average reduction in investment of 23.1% and the affected subset of aggregate capital expenditure data, our results suggest that a one-standard-deviation increase in housing prices reduces annual firm investment by about U.S.$56.5 billion. Using the average GDP during our sample (U.S.$11,224 billion), this reduction in investment is equivalent to 0.50% of the GDP, which is significant given average capital expenditure as a fraction of GDP is 9.47%.

Chaney, Sraer, and Thesmar (2012) show that firms are able to obtain more financing if they have collateral available to pledge, and this helps firms invest more. However, the ability to obtain financing in this case is not driven by the value of the project, but by the value of the collateral. In Column 2 of Table C.8 in the Online Appendix, we find that a one-standard-deviation increase in the market value of buildings leads to a 2.25 percentage point increase in investment in our sample. This number compares to a larger (in absolute value)

–6.85 percentage point estimate of the crowding-out effect based on Column 4 of Table 4.

Similar to the bank lending channel effect, we calculate that about 23% of the nonfinancial Compustat universe has some real estate collateral. Combining the crowding-out effect with the collateral effect, we obtain an estimate of –4.6 percentage points as a fraction of PP&E. This translates to a 15.5% reduction in investment given the mean investment level of 29.7% as a fraction of PP&E in our sample. Using the same U.S.$244.5 billion subset of capital expenditure data as above, this is approximately a $38 billion reduction in investment or 0.34% of the average GDP during the sample period. In addition to the collateral channel, construction activity as a result of real estate appreciation would provide an additional positive effect to GDP—measuring the exact magnitude of such an effect is beyond the scope of this paper. Overall, our results are of high enough magnitude to be taken into consideration.

6. Conclusion

There is an established literature that considers the effect of crashes in asset prices (and real estate prices in particular) on the broader economy. One channel of particular importance is the bank lending channel, through which asset price crashes affect the economy further through a contraction in bank activity. The role of the bank lending channel in the presence of an asset price boom has not been empirically documented, to the best of our knowledge. This gap is what our paper seeks to fill.

We find negative real effects for firms through the bank lending channel. In the presence of a housing boom, banks increase mortgage lending. For many banks, capital constraints are sufficiently binding that this increase comes at the expense of other activity, such as commercial lending. Firms that borrow from these banks are crowded out as a result. Ultimately, we find these firms reduce their investment levels as compared with their peers. This is especially true for those firms without ready access to other sources of capital. Consistent with a crowding-out effect, we find that these firms pay higher interest rates on their loans and have lower loan growth. We also rule out that our findings are being driven by the changes in real estate prices directly affecting the firms' demand for capital and investment.

The positive spillover effects of strong housing prices on the larger economy are often discussed. Policymakers argue that supporting such markets will increase consumer wealth, consumer demand, and real economic activity. While some positive effects are certainly present, it is important to consider the potential negative effects on real activity. If banks are interested in capitalizing on these supported markets at the expense of commercial lending, firms may be unable to increase investment and real activity may suffer. Therefore, the direction and magnitude of bank lending channel effects should be an important consideration for policy action.

References

Acharya, V. V., P. Schnabl, and G. Suarez. 2013. Securitization without risk transfer. *Journal of Financial Economics* 107:515–36.

Adelino, M., A. Schoar, and F. Severino. 2015. House prices, collateral and self-employment. *Journal of Financial Economics* 117:288–306.

Ashcraft, A. B. 2005. Are banks really special? New evidence from the FDIC-Induced failure of healthy banks. *American Economic Review* 95:1712–30.

———. 2006. New evidence on the lending channel. *Journal of Money, Credit and Banking* 38:751–75.

Becker, B. 2007. Geographical segmentation of U.S. capital markets. *Journal of Financial Economics* 85:151–78.

Begley, T. A., and A. Purnanandam. 2017. Design of financial securities: Empirical evidence from private-label RMBS deals. *Review of Financial Studies* 30:120–61.

Bernanke, B. S. 1983. Nonmonetary effects of the financial crisis in the propagation of the great depression. *American Economic Review* 73:257–76.

Bernanke, B. S., and M. Gertler. 1989. Agency costs, net worth, and business fluctuations. *American Economic Review* 79:14–31.

Bleck, A., and X. Liu. 2013. Liquidity flooding, asset prices and the real economy. HKUST and UBC Working Paper.

Bolton, P., and X. Freixas. 2006. Corporate finance and the monetary transmission mechanism. *Review of Financial Studies* 19:829–70.

Brown, J. R., S. M. Fazzari, and B. C. Petersen. 2009. Financing innovation and growth: Cash flow, external equity, and the 1990s R&D boom. *Journal of Finance* 64:151–85.

Chaney, T., D. Sraer, and D. Thesmar. 2012. The collateral channel: How real estate shocks affect corporate investment. *American Economic Review* 102:2381–409.

Chava, S., and A. Purnanandam. 2011. The effect of banking crisis on bank-dependent borrowers. *Journal of Financial Economics* 99:116–35.

Chava, S., and M. R. Roberts. 2008. How does financing impact investment? The role of debt covenants. *Journal of Finance* 63:2085–121.

Cuñat, V., D. Cvijanović, and K. Yuan. 2013. How did US banks react to capital losses induced by real estate prices? UNC and LSE Working Paper.

Cvijanović, D. 2014. Real estate prices and firm capital structure. *Review of Financial Studies* 27:2690–735.

Davidoff, T. 2016. Supply constraints are not valid instrumental variables for home prices because they are correlated with many demand factors. *Critical Finance Review* 5:177–206.

Demyanyk, Y., C. Ostergaard, and B. E. Sørensen. 2007. U.S. banking deregulation, small businesses, and interstate insurance of personal income. *Journal of Finance* 62:2763–801.

Farhi, E., and J. Tirole. 2012. Bubbly liquidity. *Review of Economic Studies* 79:678–706.

Faulkender, M., and M. A. Petersen. 2006. Does the source of capital affect capital structure? *Review of Financial Studies* 19:45–79.

Gan, J. 2007a. Collateral, debt capacity, and corporate investment: Evidence from a natural experiment. *Journal of Financial Economics* 85:709–34.

———. 2007b. The real effects of asset market bubbles: Loan- and firm-level evidence of a lending channel. *Review of Financial Studies* 20:1941–73.

Gertler, M., and N. Kiyotaki. 2010. Financial intermediation and credit policy in business cycle analysis. In B. M. Friedman and M. Woodford, eds., *Handbook of Monetary Economics*, vol. 3 of *Handbook of Monetary Economics*, chap. 11, 547–99. Amsterdam: Elsevier.

Gorton, G. B., and G. G. Pennacchi. 1995. Banks and loan sales: Marketing non-marketable assets. *Journal of Monetary Economics* 35:389–411.

Hadlock, C. J., and J. R. Pierce. 2010. New evidence on measuring financial constraints: Moving beyond the KZ index. *Review of Financial Studies* 45:1909–40.

Hennessy, C. A., and T. M. Whited. 2007. How costly is external financing? Evidence from a structural estimation. *Journal of Finance* 62:1705–45.

Holmstrom, B., and J. Tirole. 1997. Financial intermediation, loanable funds, and the real sector. *Quarterly Journal of Economics* 112:663–91.

Ivashina, V., and D. Scharfstein. 2010. Bank lending during the financial crisis of 2008. *Journal of Financial Economics* 97:319–38.

Jayaratne, J., and P. E. Strahan. 1996. The finance-growth nexus: Evidence from bank branch deregulation. *Quarterly Journal of Economics* 111:639–70.

Kashyap, A. K., and J. C. Stein. 1995. The impact of monetary policy on bank balance sheets. *Carnegie-Rochester Conference Series on Public Policy* 42:151–95.

———. 2000. What do a million observations on banks say about the transmission of monetary policy? *American Economic Review* 90:407–28.

Keys, B. J., T. Mukherjee, A. Seru, and V. Vig. 2010. Did securitization lead to lax screening? Evidence from subprime loans. *Quarterly Journal of Economics* 125:307–62.

Khwaja, A. I., and A. Mian. 2008. Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review* 98:1413–42.

Kishan, R. P., and T. P. Opiela. 2000. Bank size, bank capital, and the bank lending channel. *Journal of Money, Credit and Banking* 32:121–41.

Kiyotaki, N., and J. Moore. 1997. Credit cycles. *Journal of Political Economy* 105:211–48.

Kleibergen, F., and R. Paap. 2006. Generalized reduced rank tests using the singular value decomposition. *Journal of Econometrics* 133:97 – 126.

Leary, M. T. 2009. Bank loan supply, lender choice, and corporate capital structure. *Journal of Finance* 64:1143–85.

Lemmon, M., and M. R. Roberts. 2010. The response of corporate financing and investment to changes in the supply of credit. *Journal of Financial and Quantitative Analysis* 45:555–87.

Lin, H., and D. Paravisini. 2013. The effect of financing constraints on risk. *Review of Finance* 17:229–59.

Loutskina, E., and P. E. Strahan. 2009. Securitization and the declining impact of bank finance on loan supply: Evidence from mortgage originations. *Journal of Finance* 64:861–89.

———. 2015. Financial integration, housing, and economic volatility. *Journal of Financial Economics* 115:25–41.

Mian, A., and A. Sufi. 2011. House prices, home equity-based borrowing, and the US household leverage crisis. *American Economic Review* 101:2132–56.

Morrison, A. D. 2005. Credit derivatives, disintermediation, and investment decisions. *Journal of Business* 78:621–48.

Paravisini, D. 2008. Local bank financial constraints and firm access to external finance. *Journal of Finance* 63:2161–93.

Parlour, C. A., and G. Plantin. 2008. Loan sales and relationship banking. *Journal of Finance* 63:1291–314.

Peek, J., and E. S. Rosengren. 2000. Collateral damage: Effects of the Japanese bank crisis on real activity in the united states. *American Economic Review* 90:30–45.

Pennacchi, G. G. 1988. Loan sales and the cost of bank capital. *Journal of Finance* 43:375–96.

Rampini, A. A., and S. Viswanathan. 2017. Financial intermediary capital. Working Paper 23302, National Bureau of Economic Research.

Saiz, A. 2010. The geographic determinants of housing supply. *Quarterly Journal of Economics* 125:1253–96.

Scharfstein, D. S., and J. C. Stein. 2000. The dark side of internal capital markets: Divisional rent-seeking and inefficient investment. *Journal of Finance* 55:2537–64.

Schnabl, P. 2012. The international transmission of bank liquidity shocks: Evidence from an emerging market. *Journal of Finance* 67:897–932.

Schwert, M. Forthcoming. Bank capital and lending relationships. *Journal of Finance.*

Stein, J. C. 1997. Internal capital markets and the competition for corporate resources. *Journal of Finance* 52:111–33.

———. 1998. An adverse-selection model of bank asset and liability management with implications for the transmission of monetary policy. *RAND Journal of Economics* 29:466–86.

Stiglitz, J. E., and A. Weiss. 1981. Credit rationing in markets with imperfect information. *American Economic Review* 71:393–410.

Sufi, A. 2009. The real effects of debt certification: Evidence from the introduction of bank loan ratings. *Review of Financial Studies* 22:1659–91.

Thakor, A. V. 1996. Capital requirements, monetary policy, and aggregate bank lending: Theory and empirical evidence. *Journal of Finance* 51:279–324.