Bank Risk-Taking and the Real Economy: Evidence from the Housing Boom and its Aftermath

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Abstract

If banks make lending decisions with a focus on short-term earnings and stock price performance, it amplifies boom-bust credit cycles, leading in turn to real cycles for the aggregate economy. We document that during the U.S. housing credit boom, publicly-traded banks increased mortgage lending activity and relaxed standards much more than privately-held banks, and more so if they were run by short-term oriented CEOs. In the ensuing bust, counties with greater exposure to short-term oriented public banks experienced more severe downturns across a variety of outcomes, including economically large drops in aggregate employment, durable consumption, and retail sales.

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1. Introduction

Economic recessions tend to be associated with credit busts, the seeds of which are often sown in the credit booms that precede them. The most recent instance of such a boom-bust cycle is the rapid expansion of household credit in the U.S. before 2007 followed by a sharp rise in mortgage defaults, financial market turmoil, and ultimately the Great Recession. There is growing macro time-series evidence that the strength of a credit expansion predicts the severity of the subsequent economic contraction (Jorda, Schularick and Taylor, 2013; Krishnamurthy and Muir, 2017; López-Salido, Stein, and Zakrajšek, 2015; Mian, Sufi and Verner, 2017). But systematic micro evidence on the factors that determine the strength of the credit cycle is limited in banking, and even less is known about whether these factors matter for the broader economy.[2]

Building on Falato and Scharfstein (2016), we put forth and empirically examine a specific explanation for the strength of a credit expansion – namely that banks that focus on short-term earnings and stock prices amplify a credit boom. One version of this explanation is based on the "short-termism" model of Stein (1989), which shows that when firms place weight on short-term stock prices they have incentives to take difficult-to-observe actions that boost current earnings at the expense of long-run profitability. Stock market investors rationally attribute higher current earnings in part to better long-run fundamentals and value, which in turn, creates incentives for firms to pump up short-term earnings. In banking, the easiest way to increase short-run profitability is to loosen lending standards -- to make more loans with higher yields but also higher default rates, potentially at the expense of long-run value. A related explanation is based on the idea that

[2] There is a related question of whether credit cycles matter for the real economy, which also remains open, with estimates of the real effects of credit contractions ranging widely between economically large (Mian and Sufi, 2014) to intermediate (Chodorow-Reich, 2014) to small (Greenstone, Mas, and Nguyen, 2014; see Mian and Sufi, 2018, and Gertler and Gilchrist, 2018 for recent surveys).

the stock market may over-extrapolate higher earnings into the future. In this view, the stock market does not fully incorporate the increase in risk that gave rise to the increase in short-term earnings and its potential for lowering earnings in the long run. This interpretation is consistent with La Porta (1996) who shows that stock analysts tend to extrapolate future earnings growth from past earnings growth even though earnings growth can be mean-reverting. In this more behavioral model, banks would also have an incentive to increase risk to pump up short-term earnings and the short-term stock price. Bolton, Scheinkman and Xiong (2006) present a model in which managers are optimally compensated to take advantage of market overvaluation of short-term performance, which in turn leads managers to focus on the short-term.

In this paper, we use rich micro data on bank lending decisions in the U.S. mortgage credit boom and its aftermath to explore the idea that short-termism could amplify a credit boom. We are not arguing here that it is the sole cause of a credit boom, only that it tends to be an amplification mechanism and may help to explain what types of lenders are more likely to ramp up lending in a boom. Using detailed geographic information on mortgage loan originations and a research design that controls for changes in local demand, we find support for the view that short-termism exacerbates credit cycles. In the boom, public banks increased mortgage lending activity and relaxed standards much more than their privately-held counterparts, who presumably care less about their short-term performance. And amongst public banks, increased risk-taking in mortgage origination was greater in banks run by CEOs with a short-term focus. In the aggregate, counties that had greater exposure to short-term oriented public banks experienced deeper economic downturns in the aftermath of the boom. In all, we offer a micro-founded channel through which bank short-termism leads to real economic cycles by amplifying credit cycles.

We start by documenting that, on average within a county, publicly-traded banks increased mortgage lending activity and relaxed lending standards much more than privately-held banks during the housing boom. The differences in mortgage origination activity between public and private banks are large. The marginal effect of moving from private to public ownership leads to a 9 percentage points increase in the growth rate of mortgage originations, the same order of magnitude as the sample mean growth rate of originations. Our estimates are identified from within-bank and within-county changes in lending behavior in the boom relative to the pre-boom years. Our research design controls for changes in local demand by including county-year effects and for unobserved heterogeneity across banks by including bank fixed effects. The identifying assumption is that the mortgage activity of public and private banks would have trended similarly in the absence of the boom, which we are able to corroborate.

Since public banks are larger on average than private banks, we use a battery of additional tests to establish that the differential response of public banks in the boom is not driven by bank size. In one of these tests, we repeat the difference-in-differences ("DD") analysis while under-weighting the largest public banks and over-weighting the smaller ones. This approach derives the DD estimates in a "re-weighted" sample where the weights are chosen to exactly offset differences in the size distribution between public and private banks, thus ensuring that these differences are not driving the result.

While greater risk-taking of public banks is consistent with a short-termism story, it could also be driven by other factors that increase risk-taking capacity of public banks relative to private banks. For example, public banks may optimally choose risker mortgages because they have more diversified public market shareholders, more diverse geographic locations, and easier access to equity capital. In an effort to tie our findings more directly to short-termism, we show that, in the

cross-section of publicly-traded banks, it is exactly those banks that are more likely to be focused on short-term performance that expand their mortgage originations and relax their standards more aggressively during the boom. We construct several proxies for banks' short-term focus using textual analysis of bank's earnings conference calls and of the MD&A section of their annual reports to the SEC. Our proxies measure how actively CEOs discuss short-term results, a text-based approach similar to Brochet, Loumioti and Serafeim (2015). The effects are more pronounced for public banks with greater short-term focus using a variety of text-based proxies for short-termism. The effects are also more pronounced for public banks who may be more short-term focused because their CEOs and institutional shareholders trade more actively.

As further evidence of the risk-taking interpretation, we show that during the boom more short-term focused public banks expanded their portfolio of originations more aggressively across a variety of risky mortgages – those with high loan-to-value ratios and interest only payments – and mortgages to risky borrowers – those with subprime credit quality and high debt-to-income. Mortgage performance in the ensuing bust also indicates that their loan originations were riskier. The probability of becoming seriously delinquent (being foreclosed) was about 1.5 (1.1) percentage points higher for mortgages originated by public banks, which is about 10% of the unconditional mean probability of delinquencies in the sample. These results hold even after controlling for observable mortgage risk characteristics at origination, such as FICO scores and loan-to-value ratios, and are again driven by the public banks that are more focused on the short-term.

In the second part of our analysis, we present evidence that short-term oriented public banks exacerbate the severity of economic cycles. A basic implication of our story is that lending by short-term oriented banks leads to a build-up of excessive risk, which, in turn, leads to a more

severe downturn once risks eventually materialize. In line with this reasoning, we find that counties with greater exposure to short-term focused public banks, which is measured based on the market share of these banks pre-boom, experienced more pronounced cycles across a variety of real outcomes, including house prices, employment, durable consumption, and retail sales. Exposure led to economically large drops in these outcomes during the bust, and to large long-run drops in house prices and employment during the overall boom-bust period. For example, an interquartile range increase in the pre-boom market share of public banks is associated with a 3 percentage point annual decline in house prices and half percentage point annual drop in employment between 2007 and 2010, which are about half and a quarter of a standard-deviation change in their respective unconditional distributions.

These results are robust to instrumenting the share of public banks in a county with an index of interstate branching restrictions developed by Rice and Strahan (2010). Finally, we address identification concerns using as a control group neighboring counties that faced similar local economic conditions pre-boom.

The remainder of this paper is organized as follows. Section 2 describes the data. Section 3 presents the first main finding that short-term oriented public banks amplify the boom and clarifies the risk-taking mechanism. Section 4 presents the second main result that bank short-termism has aggregate and real effects. Section 5 concludes.

2. Data

Our sample is drawn from the universe of U.S. mortgage originations in the "Home Mortgage Disclosure Act" (HMDA) dataset, to which we add detailed information on banks'

ownership status and several other governance characteristics. We also add ex-post mortgage performance from the Lender Processing Services (LPS) Applied Analytics dataset. The sample period for mortgage origination is an eight-year window from 1999 to 2006, which comprises the four years from 2003 to 2006, the "credit boom" period, and the four preceding years from 1999 to 2002, the "pre-boom" period. Mortgage performance is from LPS for 2007 to 2010, the "bust" period. This section details the construction and main features of the sample.

2.1. Information on Mortgage Credit Origination and Performance

We start by collecting information on the flow of new mortgages originated every year in the U.S. between 1998 and 2006 through the "Home Mortgage Disclosure Act" (HMDA) dataset, which is available at the mortgage application level.[3] For each mortgage application, HMDA provides information on final status (denied/originated), purpose (home purchase/refinancing), and amount. HMDA also reports detailed information on the identity of the institution that originates each mortgage, the "bank" which is the main focus of our study.

For each bank, we aggregate the HMDA data up to the county level based on the location of the purchased property. By doing so, we are able to track the number and dollar volume of mortgages originated for home purchase by each bank in each county. We also track the rejection rate, i.e., the fraction of mortgage applications that are denied by the bank. Originations and rejection rates are our primary outcomes of interest. Relative to previous papers that have examined the mortgage expansion and the ensuing bust (Demyanyk and Van Hemert, 2011; Mian

[3] HMDA is the largest source of primary U.S. mortgage originations (e.g., Avery et al., 2012). Any depository institution, such as commercial banks, thrifts, and credit unions, must report to HMDA if it has received a loan application, and if its assets are above an annually adjusted threshold. Asset thresholds are very mild and exempt only a very small number of institutions.

and Sufi, 2009; Adelino, Schoar, and Severino, 2016; Di Maggio and Kermani, 2016), we take a more disaggregated approach and define the outcomes of interest at the bank-county level rather than at the county level. Doing so helps to isolate the bank-specific behavior that drives the mortgage boom.

We complement these data with loan-level information on risk characteristics such as the borrower's FICO score the loan-to-value (LTV) ratio and the post-origination mortgage performance, including defaults and foreclosures. This information comes from the Lender Processing Services (LPS) Applied Analytics database (also known as McDash Analytics). LPS also provides information on whether mortgages are sold in the secondary market to a non-affiliated financial institution (private-label securitizations) or government-sponsored housing enterprise (GSE securitizations). Starting in 2004, LPS includes data from nine of the top-10 mortgage servicers and covers about two thirds of the mortgage market by value. We match mortgages originated from 2004 to 2006 in HMDA to mortgage-level information in LPS using a standard matching algorithm based on several mortgage characteristics at origination as in Agarwal et al. (2016).[4]

For each mortgage originated in the credit boom (from 2003 to 2006), the resulting merged HMDA-LPS dataset allows us to track subsequent performance of the mortgage in the bust period (from 2007 to 2010) while controlling for several observable risk characteristics of the borrower at origination. Specifically, we track two mortgage performance metrics: default and foreclosure.

[4]See also Favara and Giannetti (2016). Since servicers only provide information on loans that are active at the time they start reporting, the LPS database includes relatively few loans originated in the early 2000s, and prior to 2004 the coverage and the set of available loan characteristics is limited. Therefore, we restrict our analysis of ex-post loan performance to loans originated in the 2004–2006 period.

We measure default as delinquency for 90 or more days at least once between 2007 and 2010. Similarly, we classify a property as foreclosed if LPS records that a lender has started a foreclosure procedure on the mortgage at least once during the same period.

Finally, we add county–level data on a wide array of local household characteristics, such as average FICO score, income, share of subprime mortgages, as well as aggregate outcomes, including house prices, employment, durable consumption, and retail sales from various sources. Data on consumer debt outstanding, delinquencies, and credit scores are from the Federal Reserve Bank of New York's Consumer Credit Panel.[5] Gross income is from the IRS.[6] Foreclosures at the county level are from RealtyTrac.[7] House prices data are from CoreLogic. Employment data is from the Census Bureau County Business Patterns (CBP), durable consumption is measured as the number of auto sales from R.L. Polk,[8] and retail sales are from Moody's Analytics. The primary use of this county-level data is to examine whether public bank's incentives to originate riskier mortgages in the boom can help to explain geographic variation in house prices and aggregate real outcomes during the subsequent bust.

2.2. Information on Lender Ownership Status

[5] These data contain a wide range of consumer credit-related information for a random 5% of almost all individuals who have a Social Security number and a credit report in the U.S. (about 12 million consumers).

[6] As noted in Mian and Sufi (2009), measuring income from the IRS is important because it tracks the income of residents living inside a given area, as opposed to business statistics, which provide wage and employment statistics for individuals working, but not necessarily living, in that area.

[7] RealtyTrac.com is a leading online marketplace for foreclosure properties, covering over 92 percent of U.S. housing units.

[8] The R.L. Polk data are collected for the universe of new automobile registrations and provide information on the total number of new automobiles purchased in a given county and year. The address is derived from registrations, so the county corresponds to the address of the person who purchased the auto, not of the dealership where the car purchase was made.

The final step of our sample construction involves determining a banks' listing status. To that end, we use the confidential HMDA lender file compiled by the Board of Governors of the Federal Reserve System, which maps the lender identifier in HMDA to the unique RSSD ID assigned to the financial institution in the National Information Center (NIC) data of the Federal Reserve. From the NIC data, we retrieve the full history of top-tier holding companies of each depository institution, either commercial bank or thrift.

We determine whether a bank holding company (BHC) or thrift holding company (THC) are publicly traded using historical stock market listing information from the New York Fed CRSP-FRB link database, as well as data on all IPO filings of financial firms (SIC codes between 6000 and 6999) from Thomson Financial's SDC New Issues database, Capital IQ Key Developments database, and SNL Financial Capital Offerings database. The inclusion of banks that undergo a private-to-public transition during our sample period could raise an endogeneity concern to the extent that these transitions are correlated with actual or expected changes in mortgage demand. Thus, we consider only banks that for the whole sample period were either private or public.

This process leads to a final sample running from 1999-2006 of 375,406 county-bank-year observations for 3,693 unique banks whose historical stock listing status we are able to confirm. For this sample, we find matching information on subsequent performance for about 1.5 million distinct mortgages originated by approximately 2,500 banks in the boom.

2.3. Summary Statistics and Sample Coverage

Table 1 reports summary statistics and detailed definitions of the variables used in the main analysis (Merged Lender-HMDA Sample, Panel A), in the analysis of mortgage performance in the bust (Merged Lender-LPS Sample, Panel B), and in the county-level analysis of the aggregate

and real economic consequences (County-Level Sample, Panel C). By way of comparison and to gauge the representativeness of our sample of originations, we have calculated summary statistics for the same variables in the HMDA universe (for the same period and subject to the same filters). In our sample, a bank originates about 25 mortgage loans per county on average in a year, which corresponds to a dollar volume of originations of about $3.6 million. This figure is comparable to the HMDA universe, where banks originate an average of 27 mortgages per county-year and the value of originations is about $4 million. Mortgage rejection rates are similar across the two samples as well.

The geographic coverage of our HMDA sample is extensive and represents virtually the universe of U.S. counties. The sample includes a large swath of about 3,700 different depository institutions (commercial banks or thrifts), which corresponds to about three quarters of the overall number of commercial banks or thrifts in the HMDA universe. In fact, we cover the near universe of originations by commercial banks (97% of their corresponding unique banks or bank-county-year observations). Non-depository mortgage companies and credit unions are the only types of institutions that are not included in the sample. Finally, the sample covers roughly two thirds of the originations in the overall HMDA universe and about three quarters of the originations by all depository institutions (including credit unions) in the HMDA universe.[9]

3. Determinants of Bank Lending Behavior during the Housing Boom

[9] In the merged Lender-LPS sample (Panel B), average loan performance and characteristics at origination are in line with existing studies (Agarwal et al., 2016; and Demyanyk and Loutskina, 2016; Favara and Giannetti, 2017).

This section presents our findings that publicly-traded banks – particularly those with more short-term oriented CEOs – increased their mortgage origination activity and risk by more than privately-held banks. We follow our baseline results with a series of robustness tests that address issues of causality. We also present evidence on risky mortgage originations in the boom and mortgage performance in the crisis that buttress a risk-taking interpretation.

3.1. Empirical Framework and Graphical Analysis

We examine bank behavior in the boom using the following baseline regression specification, which is akin to difference-in-differences (DD):

$$Y_{ijt} = \alpha + \beta_1 Public\ Bank_i + \beta_2 Boom_t \times Public\ Bank_i + \gamma Z_{ijt} + \mu_{jt} + \mu_i + \varepsilon_{ijt} \qquad (1)$$

where i, j, and t index banks, counties, and years, respectively. Y is a measure of bank's county-level activity in the mortgage market, primarily the annual change in the logarithm of the number or dollar amount of mortgage loan originations. *Boom* is an indicator variable that takes a value of one for the housing boom years (2003-2006) and zero otherwise (1999-2002), and *Public Bank* is an indicator variable that takes a value of one for banks whose top-holder is publicly-traded and zero otherwise. Z_{ijt} is a (possibly empty) vector of time-varying bank- and county-level controls such as, for example, bank size, while μ_t, μ_j and μ_i are year, county, and bank fixed effects, respectively.

In order to address potential confounds related to local changes in demand, throughout the analysis, we control for county-specific demand shocks by including a full set of dummies for county interacted with year. County×year effects control for time-varying unobservable factors that are specific to each county and common across banks in a given markets, such as changes in

local demand. By including bank fixed effects, we also control for unobserved bank characteristics, which means that our estimates compare the (within-bank) change in lending activity over time for publicly-traded banks to that of privately-held banks in the same county. The inclusion of county×year fixed effects also addresses a potential concern that the results may be driven by differences in regulation across markets, such as, for example, anti-predatory lending laws (as in Di Maggio and Kermani, 2016) or foreclosure laws (as in Trebbi, Mian and Sufi, 2015).

Finally, the inclusion of county×year fixed effects in a regression in which the dependent variable is in first differences further ensures that we are controlling for potentially heterogeneous bank- or county-specific trends in the dependent variable. As such, estimates of our coefficient of interest, β_2, in equation (1) capture residual differences between public and private banks in the growth rate of mortgage credit during the boom. We evaluate statistical significance using robust clustered standard errors adjusted for non-independence of observations within county-year.[10]

The identifying assumption underlying our research design is not that there is random assignment of public vs. private ownership status. Rather, it is that public and public banks' mortgage activity would have trended similarly in the absence of the boom. To offer visual evidence, Figure 1 plots the time series of mean mortgage credit activity measured as the annual ($1,000) value of mortgage originations in a given county for public (the solid line) and private (the dotted line) banks. Mortgages originated by publicly-traded banks tracked the time series of those originated by privately-held banks closely in the years up to 2002, suggesting that the lending behavior of the two types of banks would have continued to track each other in the absence of the

[10] In robustness analysis, we show that the results are not sensitive to this particular choice of clustering (see Appendix Table A.6).

boom, which supports our 'common-trends' assumption. However, the two series stop tracking each other after 2002, with mortgage originations by public banks increasing sharply in the boom and those by private banks showing little to no movement. More formal regression analysis confirms that there are no differences in year-specific pre-trends between public and private banks, further corroborating the validity of the parallel-trend assumption (see Panel A of Appendix Table A.2).

3.2. Baseline DD Estimates

Table 2, Panel A reports estimates of our baseline DD regression (1) for two main measures of mortgage lending activity, the log change in the dollar volume and number of new mortgage originations (Columns 1-2), while Panel B reports results for the two main measures of mortgage lending standards, the dollar volume and number of mortgage rejection rates (Columns 1-2). For each of the two measures of mortgage loan origination activity in Panel A, the baseline estimates indicate that during the boom there was a much larger expansion of mortgage credit by public banks relative to private banks. The estimated effects in these regressions are statistically significant and quite large economically. For example, the estimate in column 1 implies that, on average in the boom, the annual growth rate of mortgages by public banks was about 9 percentage points higher than it was for private banks. This estimate is sizable but plausible. Specifically, it is about 10 percent of the (conditional) standard deviation of the annual growth rate of mortgages, about half a quartile movement in its distribution, and it is of the same order of magnitude as the unconditional sample mean growth rate of originations (0.076) as well as the average increase of originations in the boom (0.123).

One can also gauge the magnitude of the effect by examining how the estimate translates in the aggregate using an in-sample prediction.[11] In the counterfactual scenario where public banks lend at the same rate as the private ones, aggregate originations slightly decline in 2003 (-0.042), expand moderately in 2004 and 2005 (0.027 and 0.058, respectively) and start to contract sharply in 2006 (-0.142). In the actual data, the aggregate volume of originations grew at an average annual rate of about 0.074 between 2003 and 2006, reaching its peak in 2005 (0.110) and flattening out in 2006 (-0.009). Thus, the aggressive expansion by public banks has about as large an effect in the aggregate as the overall U.S. mortgage expansion.

Next, we examine mortgage lending standards. An implication of our bank risk-taking story is that the credit expansion by public banks should be accompanied by a deterioration in standards. Columns 1 and 2 of Panel B report results from estimating a version of our baseline DD regression (1) for measures of mortgage credit standards based on rejection rates. We later consider a more comprehensive set of mortgage risk measures from LPS (see Section 3.5). The estimates indicate that during the boom public banks were less likely to deny a mortgage application. The effect on rejections is also economically large. For example, the estimate in column 2 implies that, on average in the boom, the annual mortgage rejection rate by public banks was about 2.5 percentage points lower than it was for private banks, an economically sizable effect relative to both the sample mean rejection rate (0.230) as well as the average decrease of rejections in the boom relative to the pre-boom period (0.041).[12]

[11] Specifically, we construct a counterfactual growth rate for each bank-county-year in the boom by deflating the corresponding observation with the estimate in Column 1 of Table 2. We next multiply the counterfactual growth by previous-year mortgage loans outstanding to calculate a counterfactual level, and finally take sums across bank-county observations in each year to calculate a counterfactual aggregate annual level of originations.

[12] In appendix Table A.1, we show that the baseline estimates for originations and standards are little changed if we exclude rural counties (Panel A, Columns 1-2) or repeat the analysis at a finer level of aggregation (census tract

The results on rejection rates indicate that public banks increased originations in the boom not just in absolute terms but also relative to the applications they received. To the extent that applications capture an element of demand, the results on rejections help to distinguish our risk-taking interpretation from the alternative that public banks may tend to lend to households whose loan demand increased more during the boom.

3.3. Addressing Differences in Size between Public and Private Banks

One of the key differences between public and private banks is that public banks are considerably larger on average than private banks. Therefore, even though the inclusion of bank effects controls for time-invariant differences in behavior across banks, one may be concerned that the baseline results are driven by differential changes in the behavior of large vs. small banks over time rather than the risk-taking incentives associated with ownership status.

In this section, we examine whether size differences between public and private banks could explain our basic results. We first show that adding size as a control to the basic specification does not alter the main finding. In Column 3 of Table 2, we add controls for the interactions of pre-boom bank size (in 2002) and size squared with *Boom*. The additions to the regression do not alter the sign and statistical significance of our estimates. After controlling for size, the estimated coefficient of *Boom x Public Bank* is somewhat larger in the originations regression (Panel A) and somewhat smaller in the rejection rate regression (Panel B). The coefficient estimates on the interaction with size and size squared are not statistically significant.

instead of county) to better control for local demand shocks (Panel A, Columns 3-4). In Panel B of Appendix Table A.1, we also show that the results are robust to excluding observations involving mergers and acquisitions (Columns 1-2).

In our second analysis of the potential effect of size differentials between public and private banks, we repeat the basic analysis on a restricted sample of banks of very similar size. In particular, we construct this restrictive sample by excluding public banks that are larger than the largest (top decile) private banks and by excluding private banks that are smaller than the smallest (bottom decile) public bank. We exclude deciles around the largest private and smallest public bank to err on the side of caution and address the concern that there may be fewer private and public banks on either end of the size range. See Panel D of Appendix Table A.2 for a sample list of banks in the overlapping size sample. As the estimates in Column 4 of Table 2 show, the coefficient estimates are essentially unchanged in this overlapping size sample.

Finally, to address concerns about size differences between the samples, we repeat the DD analysis but under-weight the largest public banks and over-weight the smaller ones. This re-weighted DD approach ensures that differences in sizes between public and private banks are not driving the result because the weights are chosen to exactly offset differences in the size distribution between public and private banks (see Appendix A.1 for details). Column 5 of Table 2 shows that the sign, size, and statistical significance of the estimated effects are remarkably similar to our baseline estimates in Column 1 both for originations (Panel A) and rejections (Panel B).[13]

Another potential explanation of our findings is that public banks were not, in fact, taking more risk, but were more prone to securitize the mortgages they originated. However, Panel A of

[13] In Appendix Tables A.1-A.3, we provide additional robustness checks. In Panels B-C of Appendix Table A.1, we show results for combining the size-overlap and the size-reweighting approach (Panel B, Columns 3-4). In Appendix Table A.2 (Panel B) we show robustness to using a propensity score procedure to choose a match for each public lender. Standard diagnostics for this matched-sample analysis are summarized in Appendix Table A.3.

Appendix Table A.4 shows that during the boom public banks actually increased their rate of securitization by less than private banks. Moreover, in Panel B of Appendix Table A.4, we also show that our baseline estimates for originations and rejection rates remain stable and strongly significant for the subsample of bank-county-year observations in which the bank is not securitizing.

3.4. Cross-Sectional Evidence on Short-term Focus

One explanation for the more aggressive lending behavior of public banks in the boom is that they may want to pump up short-term earnings to influence market perceptions of their long-run value as would be implied by the short-termism model of Stein (1989). A behavioral story in which stock market investors over-extrapolate short-term earnings would lead to the same conclusion. While our results so far are consistent with this interpretation, they are also consistent with a number of other explanations. One simple alternative explanation is that the ownership shares of public banks are more widely held by more diversified investors who are arguably in a better position to bear risk. Another possibility is that publicly-traded banks can raise capital more easily and more cheaply than privately-owned banks after an adverse shock. In this view, the lower costs of external finance for publicly-traded banks makes them more willing to take risk. While we cannot rule out these explanations, we can explore whether public banks that are more short-term focused are more prone to increase mortgage origination activity and risk during the boom.

To probe our short-termism story more closely, we modify the baseline specification (1) to examine the relation between measures of the extent to which public banks and their CEOs care about the short-run and mortgage originations and standards in the boom. Note that we do not observe these variables for private banks, so we exclude them from this analysis. In this approach,

we are therefore comparing the behavior of public banks with different degrees of short-term focus. Table 3 reports estimates from this alternative specification for the dollar volume of mortgage originations and rejection rates, respectively. We consider several proxies for the extent to which managers have short horizons, which are constructed using textual analysis or information on the equity ownership structure of public banks.

3.4.1 Analysis of text-based proxies for short-term focus

In Panel A of Table 3, we report results for our primary proxy for CEO short-term focus, which is measured based on how frequently CEOs use the phrase "short-term" in their earnings calls and in the management discussion and analysis (MD&A) section of their annual reports to the SEC. Brochet, Loumioti and Serafeim (2015) show that the emphasis on short-term language in earnings calls is related to accounting choices such as discretionary accruals, which tend to increase short-term earnings. Our main proxy for short-term focus in an average over the pre-boom period (1999-2000) of the (net) frequency of short-term words in earnings calls and MD&As.[14] The estimates for this proxy are all statistically significant and the marginal effects are large. For example, the estimate in Column 1 of Panel A implies that, on average in the boom, a one standard deviation increase in the frequency of short-term words is associated with an about 11 percentage point increase in the growth rate of mortgage originations, which is similar in magnitude to our

[14] Specifically, our main proxy is defined as [Short-term horizon words- Long-term horizon words]/Total words. The list of words referring to time horizon is based on Brochet, Loumioti, and Serafeim (2015, Appendix A), and is as follows: Short-term horizon words = [day(-s or daily), short-run (or short run), short-term (or short term), week(-s or -ly), month(-s or -ly), quarter(-s or -ly)]; Long-term horizon words = [long-term (or long term), long-run (or long run), year(-s or annual(-ly)), look(ing) ahead, outlook].

baseline estimates for public ownership in Table 2 and is roughly half as large as the sample mean growth rate of originations for public banks in the boom (0.205).[15]

Panel B of Table 3 considers two additional text-based measures of CEO short-term disclosure, both also based on textual analysis of the management discussion and analysis (MD&A) section of the banks' annual reports to the SEC. The first additional measure requires relatively less a-priori judgment about the choice of keywords related to the short-term. It is constructed by recording each instance when dates of future performance are discussed in any given MD&A. For each of these instances, we measure how short the time-horizon of future performance is. Specifically, this proxy is defined as the inverse of the average difference (number of days) between dates of future performance discussed in a given filing and the date of the respective filing. The measure gauges short-term focus from the extent to which management emphasizes relatively shorter-term metrics in their discussion of performance. The second additional measure is more closely related to our main proxy, but uses a smaller sub-set of the main keywords that pertain more directly to the frequency of disclosed performance.[16] Again, the idea here is that the extent to which management relies on high-frequency performance metrics is indicative of a preference for short-term earnings. The estimated effects for originations and

[15] Appendix Table A.5 shows that the results are robust to two additional sensitivity checks. Namely, Panels A-B show that the results are robust to using an alternative proxy for short-term focus based on whether banks meet or miss their analysts' targets for earnings-per-share (EPS). Panels C-F show that the results are robust to a sensitivity check on the specification which is to add controls for time-invariant differences in lender short-term focus using bank fixed effects and the lagged proxies for short-term focus (rather than their pre-boom average).

[16] Namely, short-term horizon words for this measure include just the following: (daily, weekly, monthly, and quarterly). The measure is otherwise defined analogously to the main short-term proxy as follows: Short-term horizon words = [daily, weekly, monthly, quarterly], Long-term horizon words = [yearly], and the proxy is the frequency of (net) short term horizon words=[Short-term horizon words- Long-term horizon words]/Total words.

rejection rates remain statistically significant and economically large using either of these additional measures (Columns 1 and 3 and Columns 2 and 4, respectively).

The collection of evidence we present here suggests that the public banks that expanded more aggressively in the boom were those for which short-term performance was of greater concern to managers.

3.4.2 Analysis of short-term proxies based on ownership

Table 4 presents additional cross-sectional evidence on the short-term focus of public banks in the mortgage boom that does not rely on textual analysis. In Panel A, we show that the results on short-term focus are robust to using a measure of CEO share turnover (Columns 1 and 3) and a measure of institutional share turnover (Columns 2 and 4).[17] Bebchuk, Cohen, and Spaman (2010) show case-study evidence that some top bank executives "cashed out" by selling shares in the boom. These results lend additional support to a short-termism interpretation.

3.5. Evidence on Mortgage Risk

[17] CEO share turnover is defined as the frequency of the lender's CEO net-sales of stock using Thomson-Reuters Insider Filings database (Forms 3, 4, 5, and 144). The number of CEO sales of shares minus the number of CEO purchases of shares divided by the total number of CEO trades within a given quarter. Only cleansed, non-derivative transactions are included. Institutional share turnover is defined as average (using portfolio shares) institutional investors' portfolio turnover based on Cahart (1997). Specifically, if we denote the set of companies held by investor *i* by Q; the turnover rate of investor *i* at quarter *t* is defined as $TR_{it} = \frac{\sum_{j\in Q}|N_{jit}P_{jt} - N_{jit-1}P_{jt-1} - N_{jit-1}\Delta P_{jt}|}{\frac{1}{2}\sum_{j\in Q}N_{jit}P_{jt} + N_{jit-1}P_{jt-1}}$, where N_{jit} and P_{jt} are the number of shares and the price of company *j* held by institutional investor *i* at quarter *t*. The data source is Thomson-Reuters Institutional Holdings (13F) database. Gaspar, Massa and Matos (2005) show that firms with high institutional share turnover are more likely to receive a takeover bid, which may also lead to a greater concern for short-term stock prices.

A direct implication of our short-termism story is that the credit expansion by public banks should be accompanied by more risky mortgage originations, and especially so for those amongst them that have a short-term focus. Table 5 offers additional evidence on mortgage origination standards by repeating the analysis separately for several finer metrics of risk based on observable mortgage and borrower risk characteristics at origination, which are available in LPS for the boom years but not in HMDA. Panel A shows that, in the boom, public banks expanded more aggressively relative to private banks their originations of mortgages with higher loan-to-value (LTV) and interest-only payments (IO) and those to subprime borrowers (credit score or FICO below 660) and borrowers with high debt-to-income ratios. In line with our baseline results, Panel B confirms that the behavior of public banks was driven by those with a short-term focus.

Another direct test of risk taking is to examine subsequent performance of the cohort of mortgages that were originated in the boom. If public banks originated riskier mortgages during the boom, then these mortgages should have performed more poorly during the crisis. To examine this prediction, we use our loan-level sample of HMDA originations merged to LPS, and test whether mortgages originated by public banks in the boom period are more likely to default, which we measure by whether they become seriously (90+ days) delinquent, and more likely to be foreclosed in the ensuing bust. To that end, we estimate a linear probability model that, in addition to our main explanatory variable, includes controls for a vector of mortgage risk characteristics at origination, such as the borrower's credit score, the loan-to-value ratio, and whether the mortgage is jumbo, interest-only, or sub-prime,[18] or interest only.

[18] We classify a mortgage as subprime if it has a high default risk, as measured by the high-cost mortgage category in HMDA – i.e., if its interest rate at origination exceeds the prime rate by three percentage points or more. Because

The results are reported in Panels A and B of Table 6 for public ownership status and for short-term focus, respectively. The estimates indicate that mortgages originated by public banks during the boom were more likely to default or be foreclosed (Panel A), and especially so for public bank with a short-term focus (Panel B). The result holds even if we include the full set of controls for observable risk characteristics at the time of mortgage origination (Columns 2 and 4), suggesting that public banks were taking risk in ways that these ex ante measures do not capture. The estimate in Column 1 of Panel A imply that the likelihood that a mortgage originated by a public bank becomes seriously delinquent is 1.4 percentage points higher than it is for a mortgage originated by a private bank. This estimate is about 10% of the unconditional mean probability of delinquencies in the sample (13 percentage points). The magnitude of the effect for foreclosures is 1.1 percentage points, also about 10% of the unconditional probability of foreclosure in the sample (12 percentage points). The estimates remain strongly statistically significant and sizable for the short-term focus variable (Panel B), which is in line with our baseline results in Table 3.[19]

4. Aggregate Implications

In the second part of our analysis, we examine the consequences of bank short-termism for real economic activity.

of the limited coverage of LPS before 2004, we cannot include originations before the boom in the analysis of loan performance and, thus, cannot include controls for lender effects in this analysis.

[19] Panel C of Appendix Table A.4 addresses the concern that the risk for lenders may have been mitigated by the fact that they could securitize mortgages after origination. The results hold even just for mortgages that were not securitized and, thus, were kept on banks' balance sheets.

4.1 Aggregate and Real Effects

An important implication of our short-termism story is that, by exacerbating credit cycles, short-term oriented banks also lead to deeper business cycles for the real economy. If lax lending standards reflect excessive bank risk taking, short-termism should also ultimately harm the real economy in the long run. We explore these possibilities using a variety of aggregate and real outcomes at the county level, which include house prices, employment, durable consumption, and retail sales. We test whether counties with more exposure to short-term oriented public banks experienced more severe economic cycles, and whether their overall long-term economic performance throughout the boom-bust cycle was harmed.[20]

More formally, we examine the aggregate implications using the following cross-county regression specification:

$$Y_{jt} = \alpha + \beta \times Mkt.\ Share\ of\ Public\ Banks_{jt=2002} + \gamma \times Z_{jt=2002} + \varepsilon_j$$

where *j* and *t* index counties and time period, respectively. The dependent variable, Y_{jt}, is a measure of the change in house prices in the county, or the change in a measure of real economic activity. *Mkt. Share of Public Banks*$_{jt=2002}$ is our baseline measure of exposure to bank risk taking and is measured as the average of the annual ratio of the number of mortgages originated by public banks in county j in 2002 (”Pre-Boom”) to the total number of mortgages originated by all banks in county j in the same year. $Z_{jt=2002}$ is a vector of pre-boom county-level controls. We examine

[20] A growing literature highlights the link between credit conditions (Mian and Sufi, 2009, 2014; Mian, Rao and Sufi, 2013; Chodorow-Reich, 2014; Giroud and Mueller, 2015; López-Salido, Stein, and Zakrajšek, 2015) and economic performance. Figure 2 shows that the market share of public lenders displays considerable geographic dispersion across U.S. counties.

aggregate and real outcomes both in the boom period (2003 to 2006) and in the bust period (2007 to 2010), in turn.

Table 7 reports the main estimates of the cross-county analysis. The results in Panel A indicate that counties with higher exposure to public banks subsequently experienced greater appreciations of house prices in the boom (Column 1) and greater house price declines in the bust (Column 5). These counties also experienced bigger cyclical swings in employment (Columns 2 and 6), durable consumption (Columns 3 and 7), and retail sales (Columns 4 and 8). These results are for the specification that controls for a host of observable county characteristics and other variables that have been recognized as important drivers of the mortgage boom in the literature, such as the subprime share and the share of national banks.[21] The estimates of the aggregate effects are plausibly large. For example, the estimate of -0.139 in Column 5 of Panel A implies that an interquartile range (min-max) increase in the market share of public banks is associated with a 3 (10) percentage points annual decline in house prices, which is about half as large as the standard deviation of the annual change in house prices in the bust (6 percentage points) and of the same order of magnitude of its mean (-2 percentage points).[22] For employment, the estimate of -0.030 in Column 6 implies that an interquartile range (min-max) increase in the share of public banks is associated with over half (2) percentage point annual drop in employment, which is about a quarter of the standard deviation of the change in employment during the bust (2 percentage points).

[21] See Appendix Table A.7 (Panel A) for the coefficient estimates of the full list of controls. Panel B shows robustness to controlling for local exposure to other bank characteristics, including their size, diversification, and reliance on securitization.

[22] The interquartile range (IQR) of the market share of public lenders is about 0.2 (=0.92-0.70). The max-min range is about 0.7. Using the IQR, the marginal effect is -0.028 (=0.2*(-0.139)).

To corroborate the short-termism mechanism, Panel B of Table 7 repeats the analysis of aggregate outcomes using the market share of public banks whose CEO have a short-term focus in the county in 2002 ("Pre-Boom") as the main explanatory variable. The definition of CEO short-term focus is based on the top quartile of our main proxy for CEO short-term focus, CEO short-term disclosure (see the description of Panel A of Table 3 for details). Interestingly, while all coefficient estimates remain negative and highly statistically significant in the bust, there is weaker evidence of real effects in the boom, indicating that the amplification effect is asymmetric on the real side. As for economic significance, the estimates of the aggregate effects of short-term focused public banks are plausibly large. For example, the estimate of -0.067 in Column 5 of Panel B implies that an interquartile range increase in the market share of short-termist public banks is associated with a 1.5 percentage points average annual decline in house prices in the bust. The estimate in Column 2 of -0.014 implies that an interquartile range increase in the share of short-termist public banks is associated with an annual drop in employment of about one third of a percentage point. Overall, these results indicate that the short-termism of banks exacerbates business cycles.

Next, we address the identification concern that county exposure to public banks may be related to local economic conditions, if, for example, public banks target more cyclical areas, thus leading to selection bias in the OLS estimates. We refine identification using two approaches. First, we use an approach similar to Favara and Imbs (2015) and instrument for market share of public banks with the index of interstate branching laws restrictiveness of Rice and Strahan (2010). The index, which ranges from zero to four, is set to zero for states whose laws are most open to out-of-

state entry and adds one when a state adds any of four main barriers to entry from out-of-state.[23] We conjecture that the market share of public banks is likely to be higher in states that are more open to entry, which is confirmed by the strong negative relation between the share and the index in our first-stage regressions (Panel C of Table 8). Under the assumption that pre-boom state laws are uncorrelated with local county-level economic conditions, the predicted value of the first-stage regression should be purged of the component of the share that could be correlated with changes in local economic conditions.

We complement this strategy with a local identification approach that, for each county that straddles the state border,[24] uses its neighbor(s) across the border as a control group. In this approach, we restrict the sample to include only contiguous county pairs and add a full set of controls for county-pair fixed effects to the baseline specification. The resulting estimates are identified from within county-pair variation. Neighbor counties represent good control groups if there remain differences in exposure within cross-state county-pairs, say due to differences in state laws, and if a given county is more similar to its cross-state neighbor than to the average county. Under this assumption, within county-pair *differences* in exposure to public banks are plausibly unrelated to pre-boom local economic conditions. The results of diagnostic tests in Appendix Table A.8 support the validity of this assumption, as well as of the interstate branching laws instrument. While pre-boom exposure to public banks and to short-term oriented public banks are both

[23] As detailed in Table 1 of Rice and Strahan (2010), the index covers the following four provisions: the minimum age of the institution for acquisition, allowance of de novo interstate branching, allowance of interstate branching by acquisition of a single branch or portions of an institution, and statewide deposit cap on branch acquisitions. Specifically, we add one to the index: if a state imposes a minimum age of 3 or more years on target institutions of interstate acquirers; if a state does not permit de novo interstate branching; if a state does not permit the acquisition of individual branches by an out-of-state bank; and if a state imposes a deposit cap less than 30%.

[24] Local identification with contiguous counties has also been used in other contexts, for example, by Card and Krueger (1994) and Holmes (1998).

correlated with pre-trends and other county covariates (Columns 1 and 2), pre-trends are not significantly correlated either with the interstate branching laws instrument (Column 3) or with within-pair county exposures (Columns 4-5). After neighbor-matching, also county characteristics are not significantly correlated with the exposure variables. These results lend support to the validity of our two identification approaches.

The instrumental variable and neighbor-county results in Tables 8 and 9 confirm our baseline finding that exposure to short-term oriented public banks exacerbates real economic cycles. Robustly across the two identification strategies, the estimates indicate that counties with greater exposure to public banks (Panel A) and to public banks whose CEOs have a short-term focus (Panel B) experienced a more severe cyclical downturn robustly across the aggregate outcomes. Interestingly, while strongly statistically and economically significant in the bust, the estimates are not significant in the boom, again pointing to an asymmetry in the amplification effect on the real side.[25]

Finally, in Table 10 we examine the long-run economic consequences by repeating our baseline analysis (Panels A and C) and local identification (Panels B and D) for cumulative performance throughout the boom-bust period. In line with our main findings, the results indicate that exposure to short-term oriented public banks carries detrimental real effects in the long run, especially in the housing and labor markets. The economic significance of the estimates is confirmed by the analysis of the long-term outcomes. For example, Columns 1 and 2 in Panel C of Table 10 indicate that an interquartile range increase in the market share of short-termist public

[25] Appendix Table A.9 shows that our baseline estimates are little changed after adding to the baseline specification controls for pre-boom local economic conditions.

banks is associated with a drop of 5 percentage points in house prices and of 3.5 percentage points in employment from boom to bust.[26]

5. Conclusion

The fact that banks loosened lending standards during the U.S. housing boom is well understood. What is less clear is why they chose to do so and whether it matters for the real economy. In this paper, we argued that banks that are more focused on short-term earnings and stock prices have incentive to boost short-term earnings by relaxing lending standards, which increases short-term earnings through its increase in both loan volume and yield. We provided several pieces of evidence that are consistent with this reasoning. Our results indicate that there was significant heterogeneity across banks in the extent to which they relaxed lending standards in the mortgage boom, with banks' emphasis on the short-term leading to a stronger mortgage portfolio expansion and more lax standards in the boom.

One important question we have not addressed is whether the stock market actually rewards such risk-taking. As implied by Stein's (1989) model, as long as a component of risk-taking behavior is not observable there will be an incentive for banks to engage in this behavior even if the stock market understands that such incentives exist. Alternatively, it may be that the stock market underprices the risk inherent in the bank's loan portfolio and simply rewards banks for high earnings even if they are generated by making risky loans. Indeed, there is a very close statistical relationship between return on equity (ROE) and the market-to-book ratio. To the extent that the

[26] In Appendix Table A.10, we use "shift-share" analysis to offer additional reassurance about the interpretation of the real effects. This analysis confirms that our results on the real effects of exposure to short-term oriented public lenders continue to hold after incorporating more directly into the analysis the mortgage origination decisions of these lenders (see Appendix A.2 for more details).

market does not penalize banks for an increase in ROE that stems from increased risk-taking, it creates incentives to take such risk. Thus, a combination of short-termism and inefficient stock market pricing could be at the heart of the mortgage crisis that had such negative consequences for U.S. and international economies.

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Appendix A – Details of Additional Robustness Checks

A.1 – Details of the Reweighting Estimation

In the reweighting procedure used in Table 2 (Column 5), we assign each bank to one of 10 bins according to the size-decile distribution of private banks before the boom (in 2002). For each observation, we inflate or deflate each bin's weight so that the resulting distribution of public banks as of 2002 matches that of private banks in terms of size. For example, if public banks are more prevalent than private ones in the 90th size percentile, our procedure penalizes them in this size bin all the way up to the point where the (conditional) probability of observing a public bank in the 90th size percentile is roughly the same as the probability of observing a private bank. By applying a counterfactual distribution of outcomes to public banks *as if* they faced the private banks' outcome, this procedure ensures that, for example, differential changes in behavior of large banks do not influence the results. This is the case because large banks will contribute equally to our reweighted estimates for each of the two ownership types and year.

A.2 – Details of the Additional Analysis of Aggregate and Real Effects

In Appendix Table A.10, we summarize results of a "shift-share" analysis that uses variables constructed by multiplying to pre-boom exposure shares by two types of shift instruments for changes in county mortgage lending that are purged of local economic conditions. In Panels A-B of Appendix Table A.10, the instruments are constructed similarly to Greenstone, Mas, and Nguyen (2014) and Amiti and Weinstein (2018) as the (pre-boom) market-share weighted sum of bank–specific annual changes in the dollar volume of mortgage originations by banks that are active in the county. The bank–specific annual changes are estimated using a regression-based decomposition method as the bank-year effects in a regression of the annual changes in the dollar volume of mortgage originations that includes county-year effects to control for local demand shocks. In Panels C and D of Appendix Table A.10, the instruments are constructed as a geographic "shift" instrument whereby, for each county, the bank–specific annual changes are the bank-year specific average logarithmic annual changes in the dollar volume of mortgage originations in all other counties excluding own county. Since we focus only on mortgage originations outside a given county, these bank-specific shocks are plausibly unrelated to changes in local economic conditions.

The results of the additional shift-share analysis confirm our baseline finding of real effects. Robustly across the two instruments, the estimates in Panels A-D of Appendix Table A.10 indicate that mortgage originations by public banks and by short-term oriented public banks have real effects. The estimated effects are stable across the two instruments and are again strongly economically significant across outcomes. For example, Columns 2 and 3 in Panel A of Appendix Table A.10 indicate that a one standard-deviation contraction in mortgage originations by public banks leads to a drop of 1.3 percentage points in employment and to a drop of 3.3 percentage points in durable consumption, respectively. By contrast, the estimates are never statistically significant for private banks or relatively less short-term oriented public banks. The lack of statistical significance for these sub-groups provides a useful falsification, or placebo, test. If we failed to purge the instruments of local economic conditions, then we should see significant estimates also for private or less short-term oriented banks. As such, the results of the additional shift-share analysis offer additional reassurance that omitted common factors related to local economic conditions are unlikely to be driving our estimates of the real effects.

Table 1: Samples and Variables Description

This table reports variable definitions and summary statistics for the samples used in the analysis. Panel A refers to the merged *Lender-HMDA Sample*, which consists of 375,406 bank-county-year observations involving 3,693 unique banks between 1999 and 2006. This sample consists of data in *HMDA* on mortgages originated or denied between 1999 and 2006 by banks for which information on whether their top-holder is privately-held or publicly-traded is available. Panel B refers to the merged *Lender-LPS Sample*, which consists of 1,463,278 mortgage observations involving 2,467 unique banks between 2007 and 2010. This sample consists of mortgages in the merged Lender-HMDA sample that were originated between 2004 and 2006 and for which information on performance and additional mortgage and borrower risk characteristics at origination is available in LPS. Panel C refers to the *Country-Level Sample* used in the analysis of real and aggregate effects on local economic conditions. In this panel, $\text{Bust}_{2007-2010}$ denotes averages between 2007 and 2010 (the "Bust" period).

Variable Name	Description (Source)	Mean	St. Dev.
Panel A: Merged Lender-HMDA Sample			
Bank Listing Status:			
Unique Public Banks (%)	Dummy variable that takes the value of 1 if the bank is publicly-traded, and is 0 otherwise 9 (Hand-collected).	0.25	
Bank-County Mortgage Originations and Standards:			
Mortgages Originated (number)	Number of conventional loans originated for purchase of single family owner occupied houses. Bank-county level aggregation of loan level data (HMDA).	24.6	20.9
Mortgages Originated ($1,000)	Dollar amount of conventional loans originated for purchase of single family owner occupied houses. Bank-county level aggregation of loan level data (HMDA).	3,570	3,450
Rejection Rate	Number of loan applications denied for purchase of single family owner occupied houses divided by number of loan applications received. Bank-county level aggregation of loan level data (HMDA).	0.24	0.18
	Observations (bank-county-year)	375,406	
	Banks	3,693	
Panel B: Merged Lender-LPS Sample			
Bank Listing Status:			
Unique Public Banks (%)	Dummy variable that takes the value of 1 if the bank is publicly-traded, and is 0 otherwise (Hand-collected).	0.26	
Mortgage Loan Standards and Performance:			
90+ days delinquency	Dummy variable that takes the value of 1 if a mortgage is ever 90 plus days delinquent between 2007 and 2010, and is 0 otherwise (LPS).	0.13	0.13
Foreclosure	Dummy variable that takes the value of 1 if a mortgage is ever foreclosed between 2007 and 2010, and is 0 otherwise (LPS).	0.12	0.13
FICO	Borrower's FICO score at origination (LPS).	720	23
Interest Only (IO)	Dummy variable that takes the value of 1 if a mortgage is interest rate only, and is 0 otherwise (LPS).	0.13	0.12
Debt-to-Income Ratio	Borrower's debt to income ratio at origination (LPS).	32	6
LTV Ratio	Borrower's loan to value (LTV) ratio at origination (LPS).	0.78	0.06
	Observations (mortgage)	1,463,278	
	Banks	2,467	

Table 1: Samples and Variables Description (Continued)

This table reports variable definitions and summary statistics for the samples used in the analysis. Panel A refers to the merged *Lender-HMDA Sample*, which consists of 375,406 bank-county-year observations involving 3,693 unique banks between 1999 and 2006. This sample consists of data in *HMDA* on mortgages originated or denied between 1999 and 2006 by banks for which information on whether their top-holder is privately-held or publicly-traded is available. Panel B refers to the merged *Lender-LPS Sample*, which consists of 1,463,278 mortgage observations involving 2,467 unique banks between 2007 and 2010. This sample consists of mortgages in the merged Lender-HMDA sample that were originated between 2004 and 2006 and for which information on performance and additional mortgage and borrower risk characteristics at origination is available in LPS. Panel C refers to the *Country-Level Sample* used in the analysis of real and aggregate effects on local economic conditions. In this panel, $\text{Bust}_{2007-2010}$ denotes averages between 2007 and 2010 (the "Bust" period).

Panel C: County-Level Sample			
Variable Name	Description (Source)	Mean	St. Dev.
Bank Listing Status:			
Mkt. share of public banks$_{t=2002}$	Dollar amount of conventional loans originated for purchase of single family owner occupied houses. County level aggregation of loan level data, as the annual ratio of total originations by public banks to total originations (HMDA).	0.78	0.15
Mkt. share of short-term pub. banks$_{t=2002}$	Dollar amount of conventional loans originated for purchase of single family owner occupied houses. County level aggregation of loan level data, as the annual ratio of total originations by public banks whose CEO have a short-term focus to total originations (HMDA).	0.14	0.17
Outcomes:			
House price change, $\text{Bust}_{2007-2010}$	Logarithmic annual change in house prices (CoreLogic).	-0.02	0.06
Change in Employment, $\text{Bust}_{2007-2010}$	Logarithmic annual change in employment (Census Bureau, CBP).	-0.02	0.02
Durable cons. change, $\text{Bust}_{2007-2010}$	Logarithmic annual change in the total number of new automobile purchases (R.L. Polk).	-0.10	0.05
Change in Retail Sales, $\text{Bust}_{2007-2010}$	Logarithmic annual change in retail sales (Moody's Analytics)	-0.00	0.04
Controls:			
Subprime credit share$_{t=2002}$	Fraction of borrowers with FICO<660.	0.14	0.06
Share of National Banks$_{t=2002}$	Dollar amount of conventional loans originated for purchase of single family owner occupied houses. County level aggregation of loan level data, as the annual ratio of total originations by national banks to total originations (HMDA).	0.31	0.10
Median FICO$_{t=2002}$	Median FICO score (Equifax).	689	24
Mortgage credit to income$_{t=2002}$	Median mortgage balances relative to median income (Equifax-IRS).	0.09	0.06
Log Median Income$_{t=2002}$	(IRS)	14.46	1.24
Log Median Wages$_{t=2002}$	(BLS)	14.14	1.25
Log Population$_{t=2002}$	(US Census)	11.43	1.11
+65 Population Share$_{t=2002}$	(US Census)	0.13	0.04
	Counties (up to)	1,292	

Table 2: Analysis of Mortgage Originations and Standards in the Boom by Bank Ownership

This table summarizes our baseline estimates from regression analysis of mortgage originations and standards in the boom on banks' ownership structure. The sample is the merged Lender-HMDA sample, which consists of bank-county-year observations between 1999 and 2006 with data in HMDA on mortgages originated or denied by banks with available information on whether their top-holder is privately-held or publicly-traded. Panel A reports results of difference-in-differences (DD) analysis for the following specification:

$$Y_{ijt} = \alpha + \beta_1 PublicBank_i + \beta_2 Boom_t \times PublicBank_i + \gamma Z_{ijt} + \mu_{jt} + \mu_i + \varepsilon_{ijt},$$

where i denotes bank, j denotes county, and t denotes time. *Boom* is an indicator variable that takes a value of one for the housing boom years (2003-2006) and zero for the pre-boom years (1999-2002). *Public Bank* is an indicator variable that takes a value of one for banks whose top-holders is publicly-traded and zero otherwise. Year-county fixed effects, μ_{jt}, and bank fixed effects μ_i, are included in all regressions. The dependent variable, Y_{ijt}, is the annual change in log dollar value (Column 1) and in the log number (Column 2) of mortgages originated. Columns 1 and 2 of Panel B report baseline results for the rejection rates of mortgage volumes and numbers, respectively. Columns 3 to 5 report results of several specification checks to refine identification. Specifically, Colum 3 adds controls for the interaction of pre-boom bank size (in 2002) and size squared with *Boom*, to control for size-dependent changes in originations and standards. Column 4 repeats the analysis in the size-overlap sub-sample, which excludes public banks that are larger than the largest (top decile) private bank and private banks that are smaller than the smallest (bottom decile) public bank. Column 5 reports results of matched-sample analysis using the reweighting method of DiNardo, Fortin, and Lemieux (1996), which controls for time-varying bank-specific shocks by non-parametrically reweighting the public bank sample (within each year) to match the distribution of private banks across bins based on bank size. Specifically, we show results for binning each bank into 10 bins according to the size-decile distribution of private banks pre-boom (in 2002). Within each bank type (public or private) and year, we inflate or deflate each bin's weight so that each bin carries the same relative weight as the 2002 distribution of private banks. Bank and county fixed effects are included in all regressions. Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Baseline Analysis of Banks' Ownership Structure, Δ Log Originations					
	Baseline		Robustness to Size Differences		
	Volume ($)	Number	Add Size Controls	Size-Overlap	Size-Reweight
	(1)	(2)	(3)	(4)	(5)
Boom*Public Bank	0.088***	0.066***	0.109***	0.093**	0.084***
	(0.016)	(0.007)	(0.037)	(0.042)	(0.028)
Year-County FE	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Obs.	375,406	375,406	199,211	109,118	199,211
R^2	0.098	0.104	0.171	0.201	0.228
Economic Significance					
Sample Mean	0.076	0.036			
Sample SD	0.766	0.680			
Sample Mean, Boom	0.123	0.061			
Panel B: Baseline Analysis of Banks' Ownership Structure, Rejection Rates					
	Baseline		Robustness to Size Differences		
	Volume ($)	Number	Add Size Controls	Size-Overlap	Size-Reweight
	(1)	(2)	(3)	(4)	(5)
Boom*Public Bank	-0.025***	-0.025***	-0.016***	-0.013**	-0.021***
	(0.002)	(0.002)	(0.039)	(0.005)	(0.004)
Year-County FE	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Obs.	375,406	375,406	199,211	109,118	199,211
R^2	0.399	0.268	0.435	0.489	0.459
Economic Significance					
Sample Mean	0.230	0.244			
Sample SD	0.185	0.181			
Sample Mean, Boom	0.210	0.222			

Table 3: Analysis of Mortgage Originations and Standards in the Boom by Bank Short-term Focus

This table summarizes our baseline estimates from regression analysis of mortgage originations and standards in the boom on banks' short-term focus. The sample is the merged Lender-HMDA sample, which consists of bank-county-year observations between 1999 and 2006 with data in HMDA on mortgages originated or denied by banks with available information on whether their top-holder is privately-held or publicly-traded. We report results on cross-sectional heterogeneity among public banks in the housing boom years (2003-2006) based on the short-term focus of their CEOs using the following specification:

$$Y_{ijt} = \alpha + \beta_1 Bank\ ST_{i,Pre-Boom} + \gamma Z_{ijt} + \mu_{jt} + \varepsilon_{ijt},$$

where i denotes bank, j denotes county, and t denotes time. $Bank\ ST_{i,Pre-Boom}$ is a time-invariant average over the pre-boom period (1999-2002) of each of our proxies for the short-term focus of public banks' CEOs. Year-county fixed effects, μ_{jt}, are included in all regressions. Relative to the baseline (see description of Table 2 for details), we do not include bank fixed effects, μ_i, to allow for a broader cross-sectional comparison. In Panel A, bank CEO short-term focus is measured as the frequency of CEO words related to short-term horizon in the transcripts of the bank's earnings conference calls and in the MD&A section of the bank's annual reports to the SEC. In Panel B, we consider two additional proxies for bank CEO short-term disclosure that are also based on the MD&A section of the bank's annual reports to the SEC. The first additional proxy is measured as the inverse of the average difference (number of days) between future disclosed dates that appear in any given report and the filing date of the report (Columns 1 and 3). The second additional proxy is measured as the frequency of words related to high-frequency disclosure horizons (daily, weekly, monthly, and quarterly; Columns 2 and 4). Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Analysis of Heterogeneity of Public Banks in the Boom – By CEO Short-Term Focus				
	Δ Log Originations		Rejection Rates	
	Volume ($)	Number	Volume ($)	Number
	(1)	(2)	(3)	(4)
Bank $ST_{Pre-Boom}$	0.073***	0.059***	-0.013***	-0.012***
	(0.008)	(0.005)	(0.003)	(0.002)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No
Obs.	50,056	50,056	50,056	50,056
Economic Significance				
Sample Mean, Boom	0.205	0.117	0.194	0.200
Sample SD, Boom	0.715	0.606	0.150	0.154
Panel B: Additional Bank CEO Short-Term Disclosure Proxies				
	Δ Log Originations ($)		Rejection Rates	
	Short-Horizon Future Disclosure	High-Frequency Disclosure	Short-Horizon Future Disclosure	High-Frequency Disclosure
	(1)	(2)	(3)	(4)
Bank $ST_{Pre-Boom}$	0.075***	0.131***	-0.017***	-0.019***
	(0.016)	(0.034)	(0.002)	(0.001)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No
Obs.	39,392	39,839	39,392	39,839

Table 4: Mechanism – Additional Heterogeneous Effects by Public Bank in the Boom

This table summarizes additional sensitivity analysis of mortgage originations and standards in the housing boom years (2003-2006) to using alternative proxies for banks' short-term focus. The sample is the merged Lender-HMDA sample, which consists of bank-county-year observations between 1999 and 2006 with data in HMDA on mortgages originated or denied by banks with available information on whether their top-holder is privately-held or publicly-traded. The specification used is the same as in Table 3 (see description for details), where the main explanatory variable, $Bank\ ST_{i,Pre-Boom}$, is a time-invariant average of each of our proxies over the pre-boom period (1999-2002), and we do not include bank fixed effects, μ_i, to allow for a broader cross-sectional comparison. In Panel A, the two proxies for bank short-term focus are the frequency of the bank's CEO net-sales of stock (Columns 1 and 3) and the bank's average institutional investors' portfolio turnover based on Cahart (1997) (Columns 2 and 4). The dependent variables are either the annual change in log dollar value or the rejection rates of mortgage volumes. Year-county effects are included in all regressions. Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Heterogeneity in the Boom – By Bank CEO Compensation and Institutional Ownership				
	Δ Log Originations ($)		Rejection Rates	
	CEO Net-Sales	Inst. Own. Share Turnover	CEO Net-Sales	Inst. Own. Share Turnover
	(1)	(2)	(3)	(4)
Bank $ST_{Pre-Boom}$	0.142***	0.172**	-0.014***	-0.015***
	(0.019)	(0.084)	(0.002)	(0.001)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No
Obs.	21,732	57,475	21,732	57,475

Table 5: Additional Analysis of Standards in the Boom

This table extends the analysis of mortgage origination standards in the boom by considering several types of risky mortgage originations. The sample is the merged Lender-LPS sample, which consists of mortgages in the merged Lender-HMDA sample that were originated between 2004 and 2006 and for which information on performance and additional mortgage and borrower risk characteristics at origination is available in LPS. The dependent variable is measured as the annual change in log dollar value of risky mortgages originated by a given bank in a given year-county. Mortgage and borrower risk characteristics from LPS include a dummy variable for high (top quartile) borrowers' loan-to-value ratio (LTV, Column 1), a dummy variable for interest-only mortgages (IO, Column 2), a dummy variable for subprime borrowers (FICO score below 660, Column 3) and a dummy variable for high (top quartile) borrowers' debt-to-income ratio (Column 4). Panel A reports results for *Public Bank*, which is an indicator variable that takes a value of one for banks whose top-holders is publicly-traded and zero otherwise. Panel B focuses on the comparison within public banks based on the short-term focus of their CEOs. The proxy for public banks' CEO short-term focus, *Bank ST*, is measured as the frequency of CEO words related to short-term horizon in the transcripts of the bank's earnings conference calls and in the MD&A section of the bank's annual reports to the SEC. Year-county fixed effects are included in all regressions, where year stands for origination cohort year. Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Analysis of Standards by Banks' Ownership Structure – Δ Log X Originations, X=				
	High LTV	IO	Subprime	High DTI
	(1)	(2)	(3)	(4)
Public Bank	0.315***	0.186**	0.445**	0.135***
	(0.033)	(0.033)	(0.026)	(0.033)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No
Obs.	27,449	27,449	27,449	27,449
R^2	0.083	0.126	0.083	0.075
Economic Significance				
Sample Mean, Boom	0.148	0.231	0.188	0.143
Sample SD, Boom	0.507	0.547	0.744	0.479
Panel B: Analysis of Standards by Banks' CEO Short-Term Focus – Δ Log X Originations, X=				
	High LTV	IO	Subprime	High DTI
	(1)	(2)	(3)	(4)
Bank ST	0.236**	0.395***	0.253***	0.166***
	(0.100)	(0.091)	(0.072)	(0.099)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	7,935	7,935	7,935	7,935
R^2	0.287	0.294	0.335	0.237

Table 6: Analysis of Mortgage Performance after the Boom

This table summarizes the analysis of mortgage performance after the boom (2007-2010) as measured by 90+ day delinquencies and foreclosures. The sample is the merged Lender-LPS sample, which consists of mortgages in the merged Lender-HMDA sample that were originated between 2004 and 2006 and for which information on performance and additional mortgage and borrower risk characteristics at origination is available in LPS. Panel A reports results for *Public Bank*, which is an indicator variable that takes a value of one for banks whose top-holders is publicly-traded and zero otherwise. Panel B reports results for public banks' CEO short-term focus, which is measured as the frequency of CEO words related to short-term horizon in the transcripts of the bank's earnings conference calls and in the MD&A section of the bank's annual reports to the SEC. Additional regressors are: dummy variables that take the value of 1 if a mortgage is securitized (Securitized) or it is a jumbo mortgage (Jumbo) or it is an interest-only mortgage (IO) or it is a subprime mortgage (High Cost), and are 0 otherwise; the borrowers' loan-to-value ratio (LTV) and borrower's credit score (FICO) and a dummy variable that takes the value of 1 for Black or Hispanic borrowers (Minority) and is 0 otherwise. Year-county fixed effects are included in all regressions. Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Analysis of Banks' Ownership Structure				
	90+ Day Mortgage Delinquencies		Mortgage Foreclosures	
	(1)	(2)	(3)	(4)
Public Bank	0.014***	0.009***	0.011***	0.007***
	(0.002)	(0.001)	(0.002)	(0.001)
Securitized		-0.001		0.012***
		(0.002)		(0.002)
LTV		0.233***		0.221***
		(0.010)		(0.010)
FICO		-0.125***		-0.103***
		(0.001)		(0.001)
Jumbo		-0.019***		-0.022***
		(0.003)		(0.003)
IO		0.069***		0.078***
		(0.003)		(0.003)
High Cost		0.116***		0.135***
		(0.002)		(0.003)
Minority		0.083***		0.061***
		(0.004)		(0.003)
Year-County FE	Yes	Yes	Yes	Yes
Obs.	1,463,278	1,463,278	1,463,278	1,463,278
R^2	0.101	0.199	0.109	0.194
Panel B: Analysis of Public Banks' Short-Term Focus				
Bank ST	0.006***	0.005**	0.007***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
Securitized		-0.003		0.004
		(0.004)		(0.004)
LTV		0.256***		0.246***
		(0.011)		(0.012)
FICO		-0.131***		-0.108***
		(0.002)		(0.002)
Jumbo		-0.006		-0.008**
		(0.005)		(0.004)
IO		0.079***		0.091***
		(0.003)		(0.004)
High Cost		0.084***		0.089***
		(0.003)		(0.003)
Minority		0.084***		0.065***
		(0.005)		(0.004)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	486,393	486,393	486,393	486,393
R^2	0.141	0.222	0.149	0.219

Table 7: Aggregate and Real Effects

This table reports estimates from cross-sectional regression analysis of several county-level measures of economic activity during the 2003 to 2006 period ("Boom", Columns 1-4) and during the 2007 to 2010 period ("Bust", Columns 5-8): the average logarithmic annual change in house prices (Columns 1, 5), the average logarithmic annual change in employment (Columns 2, 6), logarithmic annual change in durable consumption (Columns 3, 7), and the average logarithmic annual change in retail sales (Columns 4, 8). In Panel A, we estimate the following specification:

$$Y_{jt} = \alpha + \beta MktSharePublicBank_{jt=2002} + \gamma Z_{jt=2002} + \varepsilon_{jt},$$

where j denotes county, and t denotes time. The main explanatory variable is the market share of public banks in the county measured in 2002 ("Pre-Boom"). In Panel B, the main explanatory variable is the market share of public banks whose CEO have a short-term focus in the county in 2002 ("Pre-Boom"). The definition of CEO short-term focus is based on the top quartile of our primary proxy (see the description of Panel A of Table 3 for details). All specifications include the following county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Aggregate and Real Effects by Local Exposure to Public Banks

	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of public banks$_{t=2002}$	0.124***	0.022***	0.085***	0.017*	-0.139***	-0.030***	-0.086***	-0.032***
	(0.018)	(0.007)	(0.017)	(0.009)	(0.023)	(0.005)	(0.016)	(0.007)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	785	791	796	781	785	791	796	781
R^2	0.274	0.189	0.313	0.073	0.282	0.170	0.336	0.101

Panel B: Aggregate and Real Effects by Local Exposure to Public Banks with CEO Short-term Focus

	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of short-term pub. banks$_{t=2002}$	0.046***	0.012***	0.050***	0.014***	-0.067***	-0.014***	-0.057***	-0.015***
	(0.016)	(0.004)	(0.011)	(0.004)	(0.015)	(0.003)	(0.015)	(0.006)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	785	791	796	781	785	791	796	781
R^2	0.320	0.154	0.316	0.091	0.200	0.125	0.225	0.079

Table 8: Refining Identification - Instrumental Variable Analysis of Aggregate and Real Effects

This table reports 2SLS-IV estimates from cross-sectional regression analysis of several county-level measures of economic activity during the 2003 to 2006 period ("Boom," Panel A) and during the 2007 to 2010 period ("Bust," Panel B): the average logarithmic annual change in house prices (Column 1), the average logarithmic annual change in employment (Column 2), logarithmic annual change in durable consumption (Column 3), and the average logarithmic annual change in retail sales (Column 4). We estimate the following 2SLS-IV specification:

$$Y_{jt} = \alpha_1 + \beta_1 \widehat{MktSharePublicBank}_{jt=2002} + \gamma_1 Z_{jt=2002} + \varepsilon_{jt},$$

where j denotes county, and t denotes time. The main explanatory variable, $\widehat{MktSharePublicBank}$, is the predicted market share of public banks in the county measured in 2002 ("Pre-Boom"). This predicted market share is estimated from the first–stage regression:

$$MktSharePublicBank_{jt=2002} = \alpha_2 + \beta_2 InterstateBranchingLawsIndex_{t=2002} + \gamma_2 Z_{jt=2002} + \varepsilon_{jt=2002},$$

where we are using the index of interstate branching laws restrictiveness of Rice and Strahan (2010) as the instrument. The index covers the following four provisions: the minimum age of the institution for acquisition, allowance of de novo interstate branching, allowance of interstate branching by acquisition of a single branch or portions of an institution, and statewide deposit cap on branch acquisitions. The index ranges from zero to four as it is set to zero for states that are most open to out-of-state entry and adds one when a state adds any of the four barriers just described. In Panels A and B, we report the estimates for the boom and bust periods, respectively. In Panel C, we report the first-stage estimates. All specifications include the following county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the state level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: $Boom_{2003-2006}$ by Exposure to Local Public Banks				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
Mkt. share of $\widehat{\text{public}}$ $\text{banks}_{t=2002}$	0.076	0.037	0.063	0.009
	(0.047)	(0.026)	(0.049)	(0.016)
County Controls	Yes	Yes	Yes	Yes
Obs.	785	791	796	781
Panel B: $Bust_{2007-2010}$ by Exposure to Local Public Banks				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
Mkt. share of $\widehat{\text{public}}$ $\text{banks}_{t=2002}$	-0.106**	-0.050***	-0.138**	-0.022***
	(0.047)	(0.013)	(0.046)	(0.006)
County Controls	Yes	Yes	Yes	Yes
Obs.	785	791	796	781
Panel C: First-stage estimates for Exposure to Local Public Banks, Mkt. share of public banks				
Interstate Branching Laws $\text{Index}_{t=2002}$	-0.029***	-0.027***	-0.027***	-0.027***
	(0.010)	(0.010)	(0.010)	(0.010)
County Controls	Yes	Yes	Yes	Yes
Obs.	785	791	796	781
R^2	0.145	0.130	0.133	0.128

Table 9: Refining Identification - Analysis of Aggregate and Real Effects Using Contiguous Border Counties as Controls

This table reports within county-pair estimates from cross-sectional regression analysis of several county-level measures of economic activity during the 2003 to 2006 period ("Boom", Columns 1-4) and during the 2007 to 2010 period ("Bust", Columns 5-8): the average logarithmic annual change in house prices (Columns 1, 5), the average logarithmic annual change in employment (Columns 2, 6), logarithmic annual change in durable consumption (Columns 3, 7), and the average logarithmic annual change in retail sales (Columns 4, 8). Specifically, we restrict the sample to include only county pairs that straddle state borders and estimate the following specification:

$$Y_{jpt} = \alpha + \beta MktSharePublicBank_{jpt=2002} + \gamma Z_{jpt=2002} + \mu_p + \varepsilon_{jpt},$$

where j denotes county, p denotes county-pair (as counties may be repeated for all pairs they are part of), and t denotes time. County pair-specific fixed effects, μ_p, for each pair of contiguous counties are added to the baseline specification of Table 7. In Panel A, the main explanatory variable is the market share of public banks in the county measured in 2002 ("Pre-Boom"). In Panel B, the main explanatory variable is the market share of public banks whose CEO have a short-term focus in the county in 2002 ("Pre-Boom"). The definition of CEO short-term focus is based on the top quartile of our primary proxy (see the description of Panel A of Table 3 for details). All specifications include the following county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

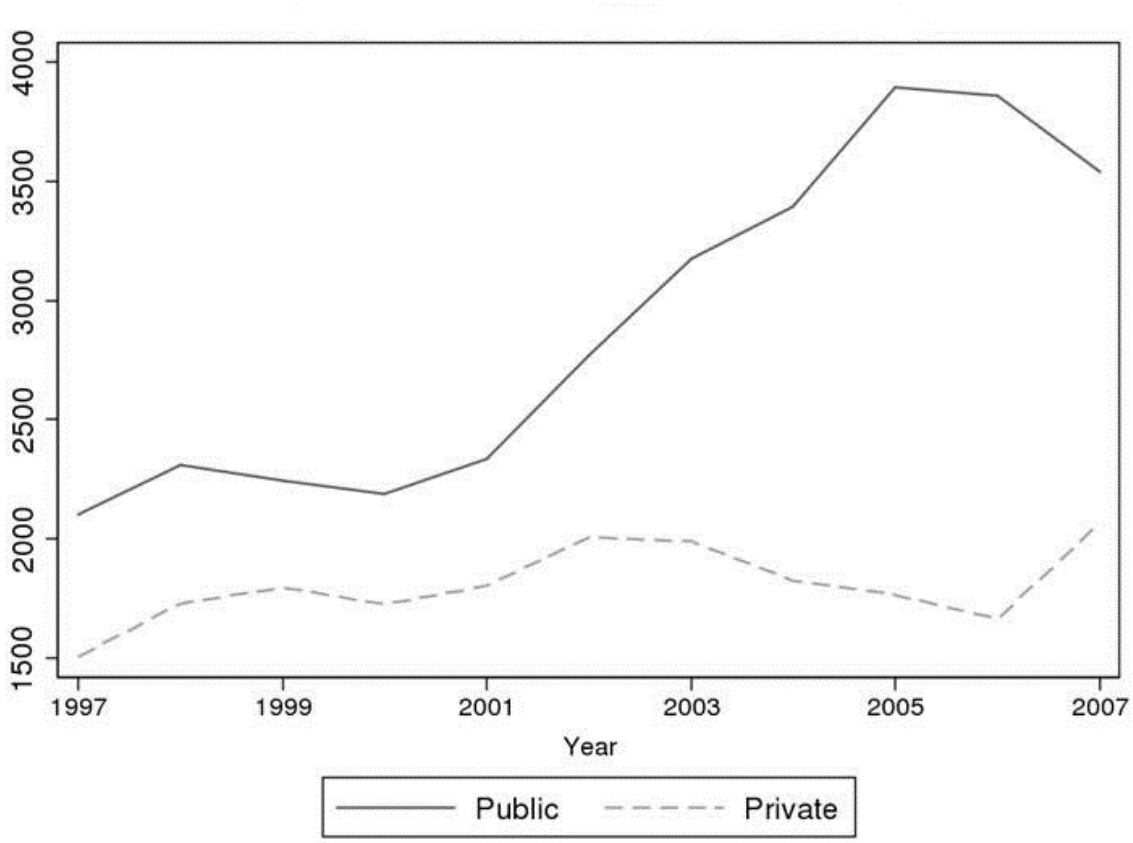
Panel A: Contiguous Border County-Pair Sample, by Local Exposure to Public Banks								
	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of public banks$_{t=2002}$	0.034	0.007	0.013	0.014	-0.055***	-0.053***	-0.035***	-0.021***
	(0.040)	(0.020)	(0.024)	(0.029)	(0.014)	(0.016)	(0.009)	(0.005)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	672	677	682	675	672	677	682	675
Unique Counties	295	298	299	297	295	298	299	297
R^2	0.843	0.903	0.840	0.837	0.875	0.868	0.796	0.864
Panel B: Contiguous Border County-Pair Sample, by Local Exposure to Public Banks with CEO Short-term Focus								
	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of short-term pub. banks$_{t=2002}$	0.005	0.010	0.002	0.005	-0.018***	-0.011***	-0.012***	-0.009**
	(0.013)	(0.024)	(0.027)	(0.017)	(0.006)	(0.003)	(0.004)	(0.004)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	672	677	682	675	672	677	682	675
Unique Counties	295	298	299	297	295	298	299	297
R^2	0.846	0.903	0.840	0.838	0.863	0.877	0.859	0.858

Table 10: Aggregate and Real Effects, Boom to Bust

This table repeats the cross-sectional regression analysis of several county-level measures of economic activity for the overall 2003 to 2010 period ("Boom to Bust"): the cumulative (logarithmic) change in house prices between 2003 and 2010 (Column 1), the cumulative (logarithmic) change in employment between 2003 and 2010 (Column 2), the cumulative (logarithmic) change in durable consumption between 2003 and 2010 (Column 3), and the cumulative (logarithmic) change in retail sales between 2003 and 2010 (Column 4). In Panels A and B, the main explanatory variable is the market share of public banks in the county measured in 2002 ("Pre-Boom"). In Panel B, we use contiguous border counties as control group (See Table 9 for details). In Panels C and D, the main explanatory variable is the market share of public banks whose CEO have a short-term focus in the county in 2002 ("Pre-Boom"). The definition of CEO short-term focus is based on the top quartile of our primary proxy (see the description of Panel A of Table 3 for details). In Panel D, we use contiguous border counties as control group (See Table 9 for details). All specifications include the following county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

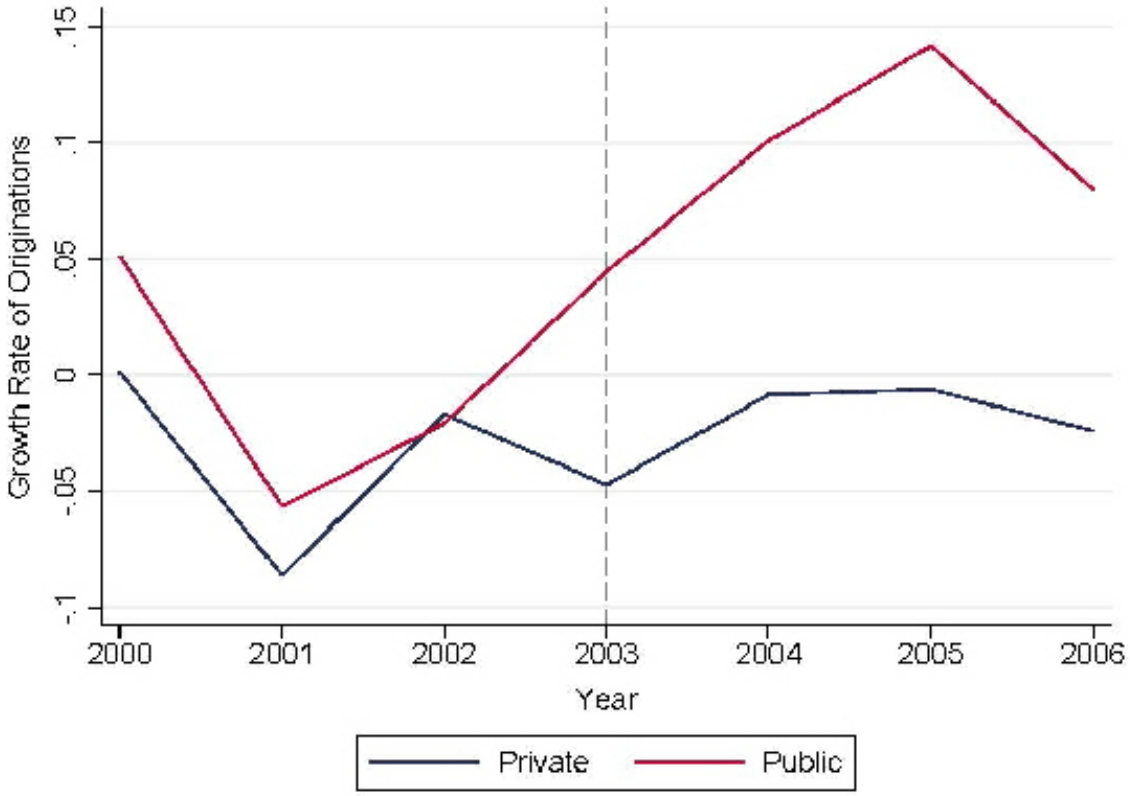
Panel A: (Boom to Bust)$_{2003-2010}$ by Exposure to Public Banks, OLS				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
Mkt. share of public banks$_{t=2002}$	-0.065	-0.108***	-0.108*	-0.117**
	(0.085)	(0.035)	(0.058)	(0.056)
County Controls	Yes	Yes	Yes	Yes
Obs.	785	791	796	781
Panel B: (Boom to Bust)$_{2003-2010}$ by Exposure to Public Banks, County-Pair				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
Mkt. share of public banks$_{t=2002}$	-0.196***	-0.289***	-0.103	-0.162***
	(0.043)	(0.089)	(0.127)	(0.059)
County Controls	Yes	Yes	Yes	Yes
Obs.	672	677	682	675
Panel C: (Boom to Bust)$_{2003-2010}$ by Exposure to Public Banks with CEO Short-term Focus, OLS				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
Mkt. share of short-term pub. banks$_{t=2002}$	-0.227**	-0.153***	-0.143*	-0.137***
	(0.096)	(0.032)	(0.074)	(0.048)
County Controls	Yes	Yes	Yes	Yes
Obs.	785	791	796	781
Panel D: (Boom to Bust)$_{2003-2010}$ by Exposure to Public Banks with CEO Short-term Focus, County-Pair				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
Mkt. share of short-term pub. banks$_{t=2002}$	-0.166*	-0.184***	-0.063	-0.085***
	(0.097)	(0.046)	(0.066)	(0.028)
County Controls	Yes	Yes	Yes	Yes
Obs.	672	677	682	675

Figure 1: Mortgage Originations by Public vs. Private Banks Before and in the Boom
Panel A: The Level of Mortgage Originations



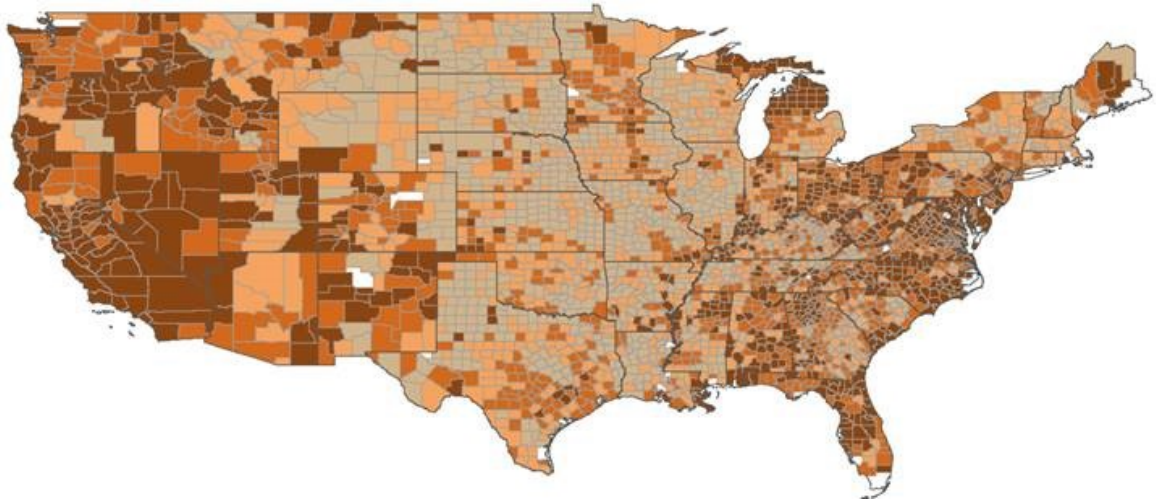
Panel A of this figure plots the average annual ($1,000) value of mortgage originations at the bank-county level over time. The solid line is for publicly-traded banks, while the dashed line is for privately-held banks. The sample is the merged Lender-HMDA sample, which is defined as those banks that over the sample period receive a mortgage application in a given year and for which information on whether their top-holder is privately-held or publicly-traded is available.

Figure 1: Mortgage Originations by Public vs. Private Banks Before and in the Boom
Panel B: The Growth Rate of Mortgage Originations



Panel B of this figure plots the logarithmic growth rate of mortgage originations – i.e., the annual change in log dollar value of mortgage originations at the bank-county level over time. The red line is for publicly-traded banks, while the blue line is for privately-held banks. The sample is the merged Lender-HMDA sample, which is defined as those banks that over the sample period receive a mortgage application in a given year and for which information on whether their top-holder is privately-held or publicly-traded is available. The figure is constructed by regressing the logarithmic growth rate of mortgage originations at the bank-county-year level on the interaction of a public-listing status indicator and year dummies in a specification that is otherwise the same as our baseline with county-year and bank effects, and requiring that the vertical distance between the two lines equals the regression coefficient on the public-listing indicator for each year and that the average of the lines equals the sample average in that year.

Figure 2: County Distribution of the Boom in Mortgage Originations by Public vs. Private Banks



This figure plots the market share of public banks in each U.S. county – i.e., fraction of mortgages originated by publicly-traded banks in each county – during the 2003-2006 period ("Boom"). The sample is the merged Lender-HMDA sample, which is defined as those banks that over the sample period receive a mortgage application in a given year and for which information on whether their top-holder is privately-held or publicly-traded is available.

Internet Appendix: Supplementary Materials for "Bank Risk-Taking and the Real Economy"

Appendix Table IA.1: Robustness Analysis of Mortgage Originations and Standards in the Boom by Bank Ownership

This table reports additional results of the difference-in-differences analysis of mortgage originations volumes and standards. Panel A shows robustness of the main results for bank ownership in Table 2 to excluding rural counties (Columns 1-2) and to defining markets at a finer level of aggregation (census tract) so that the outcomes are measured at the bank-census tract-year level. The specification is otherwise the same as in Table 2, to which we refer for details. Panel B shows robustness to excluding mergers and acquisitions (M&As) – i.e., observations involving banks that were acquirers in any given year (Columns 1-2) – and to combining size-reweighting and the size overlap-sample (Columns 3-4). Panel C shows additional robustness to an alternative quasi-regression discontinuity (RD) approach to addressing size differences, which is to limit the sample using a very narrow bandwidth of size within which there are no statistically significant differences in size between public and private banks (Columns 1-2). The size bandwidth is chosen to exclude public banks that are larger than the average private bank and private banks that are smaller than the average public bank. Columns 3-4 show results of a placebo test that repeats the baseline analysis within public banks using the interaction of *Boom* with a dummy for large (top quartile) banks. Standard errors (in parentheses) are clustered at the county-year level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Additional Analysis of Mortgage Originations and Standards				
	Exclude Rural Counties		Census Tract Level Analysis	
	Δ Log Originations $ Originations	Rejection Rate	Δ Log Originations $ Originations	Rejection Rate
	(1)	(2)	(3)	(4)
Boom*Public Bank	0.098***	-0.019***	0.084***	-0.021***
	(0.020)	(0.003)	(0.010)	(0.002)
Year-County FE	Yes	Yes	Year-Tract FE	Year-Tract FE
Bank FE	Yes	Yes	Yes	Yes
Obs.	199,722	199,722	1,283,490	1,283,490
R^2	0.070	0.383	0.010	0.311
Panel B: Additional Analysis of Mortgage Originations and Standards				
	Exclude M&As		Size Overalp+Reweight	
	Δ Log Originations $ Originations	Rejection Rate	Δ Log Originations $ Originations	Rejection Rate
	(1)	(2)	(3)	(4)
Boom*Public Bank	0.127***	-0.026***	0.193***	-0.008**
	(0.019)	(0.003)	(0.026)	(0.004)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	257,761	257,761	109,118	109,118
R^2	0.129	0.444	0.036	0.413
Panel C: Additional Analysis of Mortgage Originations and Standards				
	Size "Quasi-RD", X=Public Bank		Size Placebo, X=Large Bank	
	Δ Log Originations $ Originations	Rejection Rate	Δ Log Originations $ Originations	Rejection Rate
	(1)	(2)	(3)	(4)
Boom*X	0.148***	-0.079***	-0.195	0.074**
	(0.044)	(0.014)	(0.315)	(0.034)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	Yes
Obs.	17,612	17,612	105,071	105,071
R^2	0.346	0.410	0.210	0.407

Appendix Table IA.2: Validation and Additional Matched-Sample Analysis of Originations and Standards in the Boom by Bank Ownership

This table reports additional diagnostics and identification tests of mortgage originations and standards in the boom by bank ownership. Panel A shows results of diagnostic tests of the parallel trend assumption. Columns 1 and 2 of Panel B report results of matched-sample analysis using propensity-score matching. The matched-sample specification that is estimated is: $Y_{ijt}^{Public} - Y_{ijt}{}^{Match} = \alpha + \beta_1 Boom_t + \gamma Z_{ijt} + \mu_j + \mu_i + \varepsilon_{ijt}$, where μ_j are county fixed effects and $Y_{ijt}{}^{Match}$ is the value of the outcome variable for the match of public bank i in county j in year t in the control group of private banks. We use a propensity score procedure to choose a match for each public bank, which is given by the private banks in the same quintile of the propensity score. The propensity score is estimated based on pre-boom bank size (measured by the natural logarithm of total assets in 2002), diversification (measured by the HHI index of bank' originations across counties in 2002), securitization (measured by the ratio of private-label mortgage securitizations relative to originations in 2002), and national bank status. Columns 3 and 4 of Panel B repeat the analysis for the size overlap sub-sample. Columns 1 and of Panel C further refine the overlap-sample by also excluding banks in the top and bottom deciles of the distributions of diversification and securitization, as well as those that are not national banks (all measured in 2002). Columns 3 and 4 of Panel C add deciles of diversification and securitization as well as national status to the size-reweighting analysis. Standard errors (in parentheses) are clustered at the county-year level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Testing for Pre-Boom Trends				
	Unweighted, with Bank Size Controls		Pop. Weighted, with Bank Size Controls	
	Δ Log Originations Volume ($)	Rejection Rate	Δ Log Originations Volume ($)	Rejection Rate
	(1)	(2)	(3)	(4)
Boom*Public Bank	0.173***	-0.016**	0.175***	-0.016**
	(0.059)	(0.008)	(0.059)	(0.008)
I_{2002}*Public Bank	-0.000	-0.002	-0.003	0.000
	(0.068)	(0.009)	(0.069)	(0.009)
I_{2001}*Public Bank	-0.115	0.003	-0.111	0.001
	(0.091)	(0.009)	(0.073)	(0.009)
I_{2000}*Public Bank	0.093	0.004	0.111	0.002
	(0.069)	(0.009)	(0.079)	(0.009)
Year-County	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	199,199	199,199	199,199	199,199

Panel B: Matched-Sample Analysis of Banks' Ownership Structure by Pre-Boom Bank Size, Diversification, Securitization, and National Bank Status				
	All		Size Overlap Sub-Sample	
	Δ Log Originations Volume ($)	Rejection Rate	Δ Log Originations Volume ($)	Rejection Rate
	(1)	(2)	(3)	(4)
$Boom^{Public-Match}$	0.075***	-0.021***	0.059***	-0.022***
	(0.010)	(0.001)	(0.006)	(0.002)
County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	145,436	145,436	109,118	109,118
R^2	0.041	0.282	0.052	0.326

Panel C: Additional Analysis of Banks' Ownership Structure by Pre-Boom Bank Size, Diversification, Securitization, and National Bank Status				
	All-Overlap		All-Reweight	
	Δ Log Originations Volume ($)	Rejection Rate	Δ Log Originations Volume ($)	Rejection Rate
	(1)	(2)	(3)	(4)
Boom*Public Bank	0.136***	-0.017***	0.079***	-0.019***
	(0.036)	(0.005)	(0.031)	(0.004)
County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	65,451	65,451	199,199	199,199
R^2	0.262	0.235	0.262	0.235

Appendix Table A.2: Validation and Additional Matched-Sample Analysis of Originations and Standards in the Boom by Bank Ownership

This table reports additional diagnostics and identification tests of mortgage originations and standards in the boom by bank ownership. Panel D lists names and size (total assets $B as of 2002) information for public and private banks in the size-overlap sample of Table 2 (Column 4), which correspond to banks with total assets between $3B and $0.5B (size-overlap sample banks are listed inside the box). For reference, we also list examples of relatively larger and smaller banks.

Panel D: Additional Details on Overlap Sample

Public		Private	
Bank Name	Bank Size Total Assets ($B, 2002)	Bank Name	Bank Size Total Assets ($B, 2002)
Citibank (NY)	22.2		
Comerica (CA)	19.8		
...	...		
TCF	12.3	USAA Federal	12.3
Riggs	6.6	Emigrant	9.7
UCB	4.9	FirstBank (CO)	6.1
First Commonwealth	4.5	Safra NY	5.1
SVB	3.9	Ocean	4.1
...	...	...	...
Cathay	2.7	Cole Taylor	2.5
United	2.4	City Natl. (FL)	2.1
Sandy Spring	2.3	Johnson	2.1
Harleysville Natl.	1.8	First American (IL)	2.0
Midwest Bank & Trust	1.7	Amboy	1.9
Republic	1.7	Riverside Natl.	1.9
Century	1.6	Amarillo Natl.	1.6
Hanmi	1.5	American State	1.5
Lake City	1.2	Broadway Natl.	1.2
AmeriServ	1.2	Woodforest Natl.	1.2
...	...	...	...
First United	0.9	Hillcrest	1.0
Union Bank & Trust	0.8	F&M Bank & Trust	0.9
...	...	...	...
United Security	0.5	Citywide	0.5
South State	0.5	Putnam County	0.5
...	...	...	...
Saratoga Natl.	0.2	Peoples State (IN)	0.2
Security Bank	0.2	First Natl. of Broken Arrow	0.2

Appendix Table IA.3: Diagnostics of Matched-Sample Analysis of Originations and Standards in the Boom by Bank Ownership

This table reports diagnostic tests of the validity of the control group construction for the matched sample analysis of mortgage originations and standards in the boom by bank ownership. Panels A and B show univariate t-tests and the coefficient estimates for the propensity score covariates. Panel A reports t-tests of the null hypothesis that treated (Public) and control (Private) banks are similar along pre-boom bank characteristics used in the matching procedure, which include pre-boom bank size (measured by the natural logarithm of total assets), diversification (measured by the HHI index of bank' originations across counties), securitization (measured by the ratio of private-label mortgage securitizations relative to originations), and national bank status. Specifically, Column 6 reports results of t-test before matching, while Columns 1-5 reports results after matching for each quintile bin of the propensity score. Panel B reports OLS estimates from a linear probability model relating the likelihood of a bank being public to the pre-boom bank characteristics.

Panel A: Difference of Pre-Boom Bank Characteristics for Treated (Public) vs. Controls (Private)						
	After Matching					Before
(t-stat)	Pscore1	Pscore2	Pscore3	Pscore4	Pscore5	Matching
	(1)	(2)	(3)	(4)	(5)	(6)
Total Assets$_{1999-2002}$, log ($1,000s)	-0.004	0.089**	0.070**	0.025*	1.234***	0.807***
	(-0.10)	(1.76)	(2.07)	(1.32)	(7.14)	(19.95)
Diversification$_{1999-2002}$	0.002	0.011	0.011	0.003	0.009	-0.004**
	(0.68)	(0.92)	(1.13)	(0.45)	(1.12)	(-2.33)
Securitization$_{1999-2002}$	0.017	0.018	-0.054	0.055	-0.012	0.014*
	(1.25)	(1.03)	(-0.98)	(0.69)	(0.53)	(1.53)
National Bank Status$_{1999-2002}$	0.053	0.052	-0.081	0.066	0.153	0.096***
	(1.13)	(0.80)	(-0.32)	(0.72)	(0.76)	(2.75)
Obs.	640	640	640	640	640	3,200

Panel B: Propensity Score is Probability of Treatment (Public)			
	(1)	(2)	(3)
Total Assets$_{1999-2002}$	0.195***	0.195***	0.151***
	(0.009)	(0.009)	(0.018)
Diversification$_{1999-2002}$		-0.353***	-0.158***
		(0.025)	(0.021)
Securitization$_{1999-2002}$			0.531
			(0.352)
National Bank Status$_{1999-2002}$			0.065*
			(0.038)
Obs.	3,200	3,200	3,200
Adj-R^2	0.110	0.110	0.100

Appendix Table IA.4: Additional Analysis of Securitization

This table summarizes additional analysis of mortgage securitization in the boom and of the performance of non-securitized mortgages after the boom. Panel A reports results for mortgage securitization in the boom. We use the same specification as in the baseline analysis of mortgage originations and standards (see the descriptions of Table 2 for details) with two dependent variables, Y_{ijt}, the GSE securitization rates of mortgage volumes and numbers (measured by the ratio of GSE mortgage securitizations relative to originations, Columns 1-2) and the private-label securitization rates of mortgage volumes and numbers (measured by the ratio of private-label mortgage securitizations relative to originations, Columns 3-4). Panel B reports results of sub-sample analysis of mortgage originations and standards in the boom. We repeat the baseline analysis of Table 2 for two sub-samples, one comprised of observations for which the likelihood of securitization is zero (i.e., for bank-county-years when all mortgages are retained, Columns 1-2) and one comprised of observations that are below the median likelihood of securitization (either GSE or private label, Columns 3-4). Panel C reports results on mortgage performance in the bust for the sub-sample of mortgages that are retained on banks' balance sheets. The specification is the same as the baseline one used in Table 6 (see description for details). Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Analysis of Securitization in the Boom by Banks' Ownership Structure				
	GSE Securitization Rate		Private-Label Securitization Rate	
	Volume ($)	Number	Volume ($)	Number
	(1)	(2)	(3)	(4)
Boom*Public Bank	-0.028***	-0.030***	-0.010***	-0.009***
	(0.002)	(0.002)	(0.002)	(0.003)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	375,406	375,406	375,406	375,406
Panel B: Analysis of Originations and Standards in the Boom by Securitization				
	No Securitization		Low Securitization	
	Δ Log Originations $ Originations	Rejection Rate	Δ Log Originations $ Originations	Rejection Rate
	(1)	(2)	(3)	(4)
Boom*Public Bank	0.068***	-0.028***	0.087***	-0.026***
	(0.022)	(0.004)	(0.021)	(0.002)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	92,383	92,383	184,596	184,596
Panel C: Analysis of Performance in the Bust for the Sub-sample of Retained Mortgages				
	90+ Day Mortgage Delinquencies		Mortgage Foreclosures	
	(1)	(2)	(3)	(4)
Public Bank	0.029***	0.016***	0.022***	0.014***
	(0.004)	(0.003)	(0.004)	(0.003)
Loan Controls	No	Yes	No	Yes
Year-County FE	Yes	Yes	Yes	Yes
Obs.	253,029	253,029	253,029	253,029

Appendix Table IA.5: Robustness Analysis of Mortgage Originations and Standards in the Boom by Bank Short-term Focus

This table summarizes estimates from robustness analysis of mortgage originations and standards in the boom on banks' short-term focus. In Panels A and B, we report results of additional cross-sectional heterogeneity among public banks in the housing boom years (2003-2006) based on the short-term focus of their CEOs using a specification that is otherwise the same as in the baseline (see description of Table 3 for details) but with the following two modifications: the main explanatory variable, $Bank\ ST_{it.,}$ is a proxy for the short-term focus of public banks' CEO based on whether they meets analysts' earnings-per-share (EPS) estimates. Specifically, bank CEO short-term focus is measured as a dummy that takes value of one for banks that (just) meet EPS forecasts in Panel A (Panel B). To control for differences in performance and isolate the local estimate at the forecast threshold, we include controls for forecast errors, which are EPS minus consensus EPS analyst forecasts from a 1-quarter horizon from IBES. Close meet vs. close miss are defined as 1/3 of a standard deviation range around zero forecast error. The dependent variables are either the annual change in log dollar value or the rejection rates of mortgage volumes. Year-county effects are included in all regressions. Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Additional Bank CEO Short-Term Focus Proxies – Meet vs. Miss EPS Forecasts				
	Δ Log Originations		Rejection Rate	
	Volume ($)	Number	Volume ($)	Number
	(1)	(2)	(3)	(4)
Bank ST	0.256***	0.230***	-0.019***	-0.019***
	(0.040)	(0.018)	(0.005)	(0.005)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	27,931	27,931	27,931	27,931
R^2	0.276	0.253	0.311	0.315
Panel B: Additional Bank CEO Short-Term Focus Proxies – Close Meet vs. Close Miss EPS Forecasts				
	Δ Log Originations ($)		Rejection Rates	
	Volume ($)	Number	Volume ($)	Number
	(1)	(2)	(3)	(4)
Bank ST	0.211***	0.088***	-0.034***	-0.040***
	(0.068)	(0.029)	(0.011)	(0.011)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	21,970	21,970	21,970	21,970
R^2	0.302	0.283	0.365	0.374

Appendix Table IA.5: Robustness Analysis of Mortgage Originations and Standards in the Boom by Bank Short-term Focus

This table summarizes estimates from robustness analysis of mortgage originations and standards in the boom on banks' short-term focus. In Panels C-E, we report results on cross-sectional heterogeneity among public banks in the housing boom years (2003-2006) based on the short-term focus of their CEOs using a specification that is otherwise the same as in the baseline (see description of Table 3 for details) but with the following two modifications: the main explanatory variable, $Bank\ ST_{it-1.}$, is a (lagged) proxy for the short-term focus of public banks' CEO, and the specification now includes bank fixed effects, μ_i, to control for time-invariant differences across banks. In Panel C, bank CEO short-term focus is measured as the frequency of CEO words related to short-term horizon in the transcripts of the bank's earnings conference calls and in the MD&A section of the bank's annual reports to the SEC. In Panel D, we consider two additional proxies for bank CEO short-term disclosure that are also based on the MD&A section of the bank's annual reports to the SEC. The first additional proxy is measured as the inverse of the average difference (number of days) between future disclosed dates that appear in any given report and the filing date of the report (Columns 1 and 3). The second additional proxy is measured as the frequency of words related to high-frequency disclosure horizons (daily, weekly, monthly, and quarterly; Columns 2 and 4). The dependent variables are either the annual change in log dollar value or the rejection rates of mortgage volumes. Year-county effects are included in all regressions. Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel C: Analysis of Heterogeneity of Public Banks in the Boom – By CEO Short-Term Focus				
	Δ Log Originations		Rejection Rate	
	Volume ($)	Number	Volume ($)	Number
	(1)	(2)	(3)	(4)
Bank ST	0.110***	0.086***	-0.019***	-0.021***
	(0.015)	(0.012)	(0.004)	(0.004)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	50,056	50,056	50,056	50,056
R^2	0.253	0.269	0.420	0.424
Panel D: Additional Bank CEO Short-Term Disclosure Proxies				
	Δ Log Originations ($)		Rejection Rates	
	Short-Horizon Future Disclosure	High-Frequency Disclosure	Short-Horizon Future Disclosure	High-Frequency Disclosure
	(1)	(2)	(3)	(4)
Bank ST	0.111***	0.133***	-0.012***	-0.020***
	(0.028)	(0.030)	(0.004)	(0.004)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	39,392	39,839	39,392	39,839
R^2	0.259	0.257	0.403	0.400

Appendix Table IA.5: Robustness Analysis of Mortgage Originations and Standards in the Boom by Bank Short-term Focus (Continued)

This table summarizes estimates from robustness analysis of mortgage originations and standards in the boom on banks' short-term focus. In Panels C-E, we report results on cross-sectional heterogeneity among public banks in the housing boom years (2003-2006) based on the short-term focus of their CEOs using a specification that is otherwise the same as in the baseline (see description of Table 3 for details) but with the following two modifications: the main explanatory variable, $Bank\ ST_{it-1.}$ is a (lagged) proxy for the short-term focus of public banks' CEO, and the specification now includes bank fixed effects, μ_i, to control for time-invariant differences across banks. In Panel C, bank CEO short-term focus is measured as the frequency of CEO words related to short-term horizon in the transcripts of the bank's earnings conference calls and in the MD&A section of the bank's annual reports to the SEC. In Panel D, we consider two additional proxies for bank CEO short-term disclosure that are also based on the MD&A section of the bank's annual reports to the SEC. The first additional proxy is measured as the inverse of the average difference (number of days) between future disclosed dates that appear in any given report and the filing date of the report (Columns 1 and 3). The second additional proxy is measured as the frequency of words related to high-frequency disclosure horizons (daily, weekly, monthly, and quarterly; Columns 2 and 4). In Panel E, the two proxies for bank short-term focus are the frequency of the bank's CEO net-sales of stock (Columns 1 and 3) and the bank's average institutional investors' portfolio turnover based on Cahart (1997) (Columns 2 and 4). The dependent variables are either the annual change in log dollar value or the rejection rates of mortgage volumes. Year-county effects are included in all regressions. Standard errors (in parentheses) are clustered at the year-county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel E: Heterogeneity in the Boom – By Bank CEO Compensation and Institutional Ownership				
	Δ Log Originations ($)		Rejection Rates	
	CEO Net-Sales	Inst. Own. Share Turnover	CEO Net-Sales	Inst. Own. Share Turnover
	(1)	(2)	(3)	(4)
Bank ST	0.193***	0.244***	-0.019***	-0.024***
	(0.025)	(0.023)	(0.003)	(0.003)
Year-County FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Obs.	21,732	57,475	21,732	57,475
R^2	0.332	0.216	0.501	0.436

Appendix Table IA.6: Additional Analysis of Mortgage Originations and Standards in the Boom

This table summarizes a robustness check of our baseline estimates of mortgage originations and standards in the boom on banks' ownership structure (Panels A-B) and short-term focus (Panels C-D) to alternative clustering. The sample and specifications are otherwise the same as those in Panel A of Table 2 and Table 3, respectively, to which we refer for details. Standard errors (in parentheses) are clustered at progressively higher levels of aggregation, starting with bank-county level (Column 1), followed by bank-MSA (Column 2), bank-census division (Column 3), and bank (Column 4), with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Analysis of Originations by Banks' Ownership Structure				
	Δ Log Originations			
	(1)	(2)	(3)	(4)
Boom*Public Bank	0.088***	0.088***	0.088***	0.088***
	(0.014)	(0.021)	(0.027)	(0.031)
Clustering	Bank-County	Bank-MSA	Bank-Division	Bank
Panel B: Analysis of Standards by Banks' Ownership Structure				
	Rejection Rate			
	(1)	(2)	(3)	(4)
Boom*Public Bank	-0.025***	-0.025***	-0.025***	-0.025***
	(0.002)	(0.004)	(0.007)	(0.008)
Clustering	Bank-County	Bank-MSA	Bank-Division	Bank
Panel C: Analysis of Heterogeneity of Originations by Public Banks in the Boom – CEO Short-Term Focus				
	Δ Log Originations			
	(1)	(2)	(3)	(4)
Bank ST	0.073***	0.073***	0.073***	0.073***
	(0.009)	(0.011)	(0.015)	(0.019)
Clustering	Bank-County	Bank-MSA	Bank-Division	Bank
Panel D: Analysis of Heterogeneity of Standards by Public Banks in the Boom – CEO Short-Term Focus				
	Rejection Rate			
	(1)	(2)	(3)	(4)
Bank ST	-0.013***	-0.013***	-0.013**	-0.013**
	(0.004)	(0.005)	(0.006)	(0.007)
Clustering	Bank-County	Bank-MSA	Bank-Division	Bank

Appendix Table IA.7: Analysis of Aggregate and Real Effects – Estimates for County Controls

This table reports additional details for the cross-sectional county-level regressions of aggregate and real outcomes (Panel A) as well as results of an additional robustness check (Panel B). Panel A reports coefficient estimates from Table 7 (Panel A) for the full set of standard county-level covariates that are all measured in 2002 and include median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Aggregate and Real Effects by Local Exposure to Public Banks								
	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of public banks$_{t=2002}$	0.124***	0.022***	0.085***	0.017*	-0.139***	-0.030***	-0.086***	-0.032***
	(0.018)	(0.007)	(0.017)	(0.009)	(0.023)	(0.005)	(0.016)	(0.007)
Subprime credit share, Pre-Boom	-0.296***	0.041	-0.332***	-0.090*	-0.062	-0.041	0.279	-0.055
	(0.091)	(0.053)	(0.103)	(0.050)	(0.135)	(0.037)	(0.246)	(0.054)
Share of National Banks, Pre-Boom	-0.180*	-0.061	-0.263**	-0.150**	-0.221	0.024	0.085	-0.056
	(0.108)	(0.043)	(0.117)	(0.059)	(0.168)	(0.033)	(0.108)	(0.049)
Median FICO, Pre-Boom	-0.002	0.011	-0.080***	-0.023	-0.071**	-0.018*	0.001	-0.027**
	(0.024)	(0.015)	(0.024)	(0.014)	(0.031)	(0.009)	(0.020)	(0.013)
Log Median Income, Pre-Boom	0.084*	0.093***	0.245***	0.027	-0.168**	-0.064***	-0.187***	-0.071***
	(0.050)	(0.024)	(0.044)	(0.023)	(0.070)	(0.018)	(0.036)	(0.026)
Log Median Wages, Pre-Boom	-0.171***	-0.102***	-0.300***	-0.037	0.251***	0.068***	0.260***	0.082***
	(0.050)	(0.024)	(0.043)	(0.024)	(0.068)	(0.017)	(0.037)	(0.024)
Log Population, Pre-Boom	0.100***	0.004	0.053***	0.007	-0.101***	-0.005	-0.079***	-0.012*
	(0.013)	(0.006)	(0.014)	(0.008)	(0.018)	(0.005)	(0.011)	(0.007)
+65 Population Share, Pre-Boom	0.047	-0.282***	-0.603***	-0.118*	0.136	0.019	0.493***	-0.013
	(0.108)	(0.055)	(0.098)	(0.060)	(0.141)	(0.036)	(0.082)	(0.054)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	785	791	796	781	785	791	796	781
R^2	0.274	0.189	0.313	0.073	0.282	0.170	0.336	0.101

Appendix Table A7: Analysis of Aggregate and Real Effects – Robustness to Adding More County Controls

This table reports additional details for the cross-sectional county-level regressions of aggregate and real outcomes (Panel A) as well as results of an additional robustness check (Panel B). In Panel B, we report estimates from a robustness check on the baseline cross-sectional regression analysis of several county-level measures of economic activity during the 2003 to 2006 period ("Boom", Columns 1-4) and during the 2007 to 2010 period ("Bust", Columns 5-8): the average logarithmic annual change in house prices (Columns 1, 5), the average logarithmic annual change in employment (Columns 2, 6), logarithmic annual change in durable consumption (Columns 3, 7), and the average logarithmic annual change in retail sales (Columns 4, 8). The specification used is the same as in Table 7 (to which we refer for detail), to which we add three more controls for the local share of large banks, diversified banks, and banks with high reliance on private-label securitizations all measured in 2002 and bases on the top quartile of their respective variable. The main explanatory variable is the market share of public banks in the county measured in 2002 ("Pre-Boom"). All specifications also include the baseline county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel B: Aggregate and Real Effects by Local Exposure to Public Banks								
	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of public banks$_{t=2002}$	0.121***	0.021***	0.093***	0.016*	-0.137***	-0.030***	-0.086***	-0.032***
	(0.018)	(0.006)	(0.016)	(0.009)	(0.023)	(0.005)	(0.016)	(0.007)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	785	791	796	781	785	791	796	781

Appendix Table IA.8: Local Determinants of Exposure – Diagnostic Tests of Pre-Boom Trends and Covariate Balancing for the Analysis of Aggregate and Real Effects

This table reports results of diagnostic tests for the analysis of aggregate and real effects. We test for pre-trends and covariate balancing by running cross-sectional county-level regressions of our main exposure variables on pre-boom trends in aggregate and real economic variables as well as on county characteristics. All regressions include our four measures of economic activity during the 1999 to 2002 period ("Pre-Boom"), as well as all the county-level covariates. The four measures of economic activity are the average logarithmic annual change in house prices, the average logarithmic annual change in employment, logarithmic annual change in durable consumption, and the average logarithmic annual change in retail sales. The full set of county-level covariates (all measured in 2002) include median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. Columns 1-2 report diagnostics for the two exposure variables used in the main analysis of Table 7, the market share of public banks in the county measured in 2002 and the market share of public banks whose CEO have a short-term focus in the county in 2002. The definition of CEO short-term focus is based on the top quartile of our primary proxy (see the description of Panel A of Table 3 for details). Column 3 reports diagnostics for the instrument used in the 2SLS-IV analysis of Table 8, the index of interstate branching laws restrictiveness of Rice and Strahan (2010) (see the description of Table 8 for details). Columns 4-5 report diagnostics for the analysis of Table 9 using contiguous border counties as controls, where the two exposure variables used are the same as in the main analysis of Table 7, but the sample is restricted to include only county-pairs that straddle state borders and fixed effects for each pair of contiguous counties are added to the specification (see the description of Table 9 for details). All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Pre-Trends and Covariate Balancing				
	OLS		2SLS-IV	County-Pair Sample	
	Mkt. share of public banks$_{t=2002}$	Mkt. share of ST pub. banks$_{t=2002}$	Interstate Branching Laws Index$_{t=2002}$	Mkt. share of public banks$_{t=2002}$	Mkt. share of ST pub. banks$_{t=2002}$
	(1)	(2)	(3)	(4)	(5)
Pre-Trends					
Change in House Prices, Pre-Boom	0.233**	0.389***	0.275	-0.011	-0.193
	(0.103)	(0.129)	(0.231)	(0.403)	(0.417)
Change in Employment, Pre-Boom	-0.263**	-0.007	0.508	0.018	-0.247
	(0.128)	(0.261)	(0.349)	(0.302)	(0.393)
Change in Durable Consumption, Pre-Boom	0.217**	0.043	-0.232	0.299	-0.185
	(0.090)	(0.112)	(0.187)	(0.299)	(0.312)
Change in Retail Sales, Pre-Boom	0.101***	-0.083**	0.056	-0.169	-0.012
	(0.038)	(0.038)	(0.072)	(0.665)	(0.394)
County Characteristics					
Subprime credit share, Pre-Boom	-0.699***	-0.559	-1.245***	0.174	0.068
	(0.219)	(0.365)	(0.476)	(0.671)	(0.654)
Share of National Banks, Pre-Boom	-0.013	0.131	0.302*	0.202	0.733
	(0.091)	(0.094)	(0.172)	(1.238)	(1.097)
Median FICO, Pre-Boom	-0.310***	-0.270***	-0.163	-0.146	-0.184
	(0.062)	(0.092)	(0.131)	(0.207)	(0.199)
Log Median Income, Pre-Boom	0.299***	0.518***	1.834***	0.385	0.434
	(0.107)	(0.174)	(0.232)	(0.475)	(0.785)
Log Median Wages, Pre-Boom	-0.214**	-0.581***	-2.070***	-0.300	-0.335
	(0.111)	(0.175)	(0.239)	(0.519)	(0.808)
Log Population, Pre-Boom	-0.085***	0.067	0.263***	-0.080	-0.106
	(0.029)	(0.050)	(0.067)	(0.093)	(0.130)
+65 Population Share, Pre-Boom	-0.082	-0.877**	-3.230***	-0.169	-0.865
	(0.255)	(0.382)	(0.559)	(0.665)	(1.354)
Obs.	781	781	781	672	672

Appendix Table IA.9: Robustness Analysis of Aggregate and Real Effects

This table reports estimates from additional robustness analysis of the county-level regressions of county-level measures of economic activity. In Panels A and B, the sensitivity check on the baseline specification of Table 7 is to add controls for pre-boom trends by including for each measure of economic activity its respective pre-boom value, which is measured in 2002. The measures of economic activity during the 2003 to 2006 period ("Boom", Columns 1-4) and during the 2007 to 2010 period ("Bust", Columns 5-8) are as in the baseline (see descriptions of Table 7 for details): the average logarithmic annual change in house prices (Columns 1, 5), the average logarithmic annual change in employment (Columns 2, 6), logarithmic annual change in durable consumption (Columns 3, 7), and the average logarithmic annual change in retail sales (Columns 4, 8). In Panel A, the main explanatory variable is the market share of public banks in the county measured in 2002 ("Pre-Boom"). In Panel B, the main explanatory variable is the market share of public banks whose CEO have a short-term focus in the county in 2002 ("Pre-Boom"). The definition of CEO short-term focus is based on the top quartile of our primary proxy (see the description of Panel A of Table 3 for details). All specifications include the following county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: Aggregate and Real Effects by Local Exposure to Public Banks

	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of public banks$_{t=2002}$	0.118***	0.022***	0.072***	0.017*	-0.117***	-0.030***	-0.081***	-0.032***
	(0.020)	(0.006)	(0.015)	(0.009)	(0.022)	(0.005)	(0.016)	(0.007)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls for Pre-Boom	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	785	791	796	781	785	791	796	781

Panel B: Aggregate and Real Effects by Local Exposure to Public Banks with CEO Short-term Focus

	$Boom_{2003-2006}$				$Bust_{2007-2010}$			
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mkt. share of short-term pub. banks$_{t=2002}$	0.025**	0.011**	0.047***	0.011*	-0.067***	-0.013***	-0.052***	-0.015***
	(0.013)	(0.005)	(0.010)	(0.007)	(0.015)	(0.003)	(0.014)	(0.006)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls for Pre-Boom	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	785	791	796	781	785	791	796	781

Appendix Table IA.9: Robustness Analysis of Aggregate and Real Effects (Continued)

This table reports estimates from additional robustness analysis of the county-level regressions of county-level measures of economic activity. In Panels C and D, the sensitivity check on the 2SLS-IV specification of Table 8 is to use an indicator for the top quartile of the market share of public banks in the county measured in 2002 ("Pre-Boom") as the main explanatory variable. All specifications are otherwise as in Table 8 (see the description of the table for details) and include measures of economic activity during the 2003 to 2006 period ("Boom," Panel C) and during the 2007 to 2010 period ("Bust," Panel D): the average logarithmic annual change in house prices (Column 1), the average logarithmic annual change in employment (Column 2), logarithmic annual change in durable consumption (Column 3), and the average logarithmic annual change in retail sales (Column 4). The following county-level controls (not reported) all measured in 2002 are included: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel C: $Boom_{2003-2006}$ by Exposure to Local Public Banks				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
$\widehat{\text{Mkt. share of public banks}}_{t=2002}^{Top\ Quartile}$	0.046**	0.081***	0.041***	0.011***
	(0.019)	(0.031)	(0.016)	(0.004)
County Controls	Yes	Yes	Yes	Yes
Obs.	785	791	796	781
Panel D: $Bust_{2007-2010}$ by Exposure to Local Public Banks				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
$\widehat{\text{Mkt. share of public banks}}_{t=2002}^{Top\ Quartile}$	-0.055***	-0.059***	-0.054***	-0.031***
	(0.021)	(0.020)	(0.018)	(0.012)
County Controls	Yes	Yes	Yes	Yes
Obs.	785	791	796	781

Appendix Table IA.10: Additional Analysis of Aggregate and Real Effects – A Shift-Share Approach

This table reports results for an alternative approach to the cross-sectional county-level regression analysis of aggregate and real outcomes. Panel A reports results of the second-stage shift-share analysis where we instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public vs. private banks using the (pre-boom) market-share weighted sum of bank–specific annual changes in the dollar volume of mortgage originations by banks that are active in the county. These bank-specific shocks are estimated as the bank-year effects in a regression that controls for local demand shocks by including county-year effects. Panel B reports results for the same approach to instrument for originations in the county by public banks whose CEO have a short-term focus. The definition of CEO short-term focus is based on the above median of our primary proxy (see the description of Panel A of Table 3 for details). All specifications include the following county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. To ease exposition, the outcome variables for the boom and bust period are collapsed into one by taking the difference between their respective values during the 2007 to 2010 period ("Bust") and during the 2003 to 2006 period ("Boom"). Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel A: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Banks, Bartik Analysis				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
$\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$	0.457***	0.120***	0.216***	0.112***
	(0.063)	(0.023)	(0.049)	(0.026)
$\Delta \widehat{Log\ Orig}\ (\$)_{\text{Pr}ivate,t=2007-2010}$	0.099	0.037	0.093	0.041
	(0.072)	(0.028)	(0.072)	(0.037)
County Controls	Yes	Yes	Yes	Yes
Obs.	769	779	769	781
R^2	0.461	0.210	0.370	0.124
Economic Significance, 1 St.Dev.Change in RHS (pct.pt.)				
$\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$	4.1	1.0	2.1	1.0
$\Delta \widehat{Log\ Orig}\ (\$)_{\text{Pr}ivate,t=2007-2010}$	1.0	0.3	1.0	0.4
Panel B: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Banks with CEO Short-term Focus, Bartik Analysis				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
$\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$	1.617***	0.252***	0.847***	0.174**
	(0.294)	(0.065)	(0.201)	(0.074)
$\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$	0.138	0.119	0.152	0.110
	(0.257)	(0.092)	(0.267)	(0.094)
County Controls	Yes	Yes	Yes	Yes
Obs.	769	779	769	781
R^2	0.393	0.145	0.344	0.097
Economic Significance, 1 St.Dev.Change in RHS (pct.pt.)				
$\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$	2.3	0.6	1.9	0.4
$\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$	0.3	0.3	0.4	0.2

Appendix Table IA.10: Additional Analysis of Aggregate and Real Effects – A Shift-Share Approach (Continued)

This table reports results for an alternative approach to the cross-sectional county-level regression analysis of aggregate and real outcomes. Panel C reports results of the second-stage shift-share analysis where we instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public vs. private banks using the (pre-boom) market-share weighted sum of bank–specific annual changes in the dollar volume of mortgage originations by banks that are active in the county. These bank-specific shocks are estimated as the bank-year effects in a regression that controls for local demand shocks by including county-year effects, as an alternative strategy that only includes lending outside any given county to control for local demand shocks. Panel D reports results for the same approach to instrument for originations in the county by public banks whose CEO have a short-term focus. The definition of CEO short-term focus is based on the above median of our primary proxy (see the description of Panel A of Table 3 for details). All specifications include the following county-level controls (not reported) all measured in 2002: median FICO score, subprime share, delinquency rates, median income, wage income, population, share of population older than 65 years. All regressions are weighted by the total population of each county. To ease exposition, the outcome variables for the boom and bust period are collapsed into one by taking the difference between their respective values during the 2007 to 2010 period ("Bust") and during the 2003 to 2006 period ("Boom"). Standard errors (in parentheses) are clustered at the county level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

Panel C: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Banks, Geographic Instrument				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
$\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$	0.325***	0.085***	0.210***	0.090***
	(0.042)	(0.012)	(0.036)	(0.014)
$\Delta \widehat{Log\ Orig}\ (\$)_{Private,t=2007-2010}$	-0.086	-0.016	0.142	-0.014
	(0.069)	(0.023)	(0.160)	(0.027)
County Controls	Yes	Yes	Yes	Yes
Obs.	769	779	769	781
R^2	0.474	0.229	0.384	0.149
Economic Significance, 1 St.Dev.Change in RHS (pct.pt.)				
$\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$	4.8	1.3	3.3	1.4
$\Delta \widehat{Log\ Orig}\ (\$)_{Private,t=2007-2010}$	-0.9	-0.1	1.3	-0.1
Panel D: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Banks with CEO Short-term Focus, Geo. Instr.				
	Change in House Prices	Change in Employment	Change in Durable Consumption	Change in Retail Sales
	(1)	(2)	(3)	(4)
$\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$	1.580***	0.175***	0.594***	0.218***
	(0.246)	(0.051)	(0.189)	(0.074)
$\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$	0.068	0.139	0.299	-0.068
	(0.268)	(0.101)	(0.263)	(0.094)
County Controls	Yes	Yes	Yes	Yes
Obs.	769	779	769	781
R^2	0.407	0.148	0.339	0.080
Economic Significance, 1 St.Dev.Change in RHS (pct.pt.)				
$\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$	2.8	0.6	1.8	0.5
$\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$	0.1	0.3	0.6	-0.1