UNIL | Université de Lausanne
Département de finance

ÉCOLE POLYTECHNIQUE FÉDÉRALE DE LAUSANNE

# FINANCE RESEARCH SEMINAR SUPPORTED BY UNIGESTION

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


## Prof. Antonio FALATO

*The Federal Reserve Bank*



**Abstract**

The short-termism of lenders amplifies boom-bust credit cycles, leading in turn to real costs for the aggregate economy. 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. The findings hold for text-based measures of short-term focus and are robust to using an identification strategy that instruments for county mortgage lending with shocks that are plausibly unrelated to local economic conditions. In all, we provide micro-founded evidence that the ownership structure and short-term focus of depository institutions matter for the real economy.


**Friday, November 2, 2018, 10:30-12:00**

**Room 126**, Extranef building at the University of Lausanne

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

Antonio Falato
Federal Reserve Board

Giovanni Favara
Federal Reserve Board

David Scharfstein[1]
Harvard University

September 2018

**Abstract**

The short-termism of lenders amplifies boom-bust credit cycles, leading in turn to real costs for the aggregate economy. 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. The findings hold for text-based measures of short-term focus and are robust to using an identification strategy that instruments for county mortgage lending with shocks that are plausibly unrelated to local economic conditions. In all, we provide micro-founded evidence that the ownership structure and short-term focus of depository institutions matter for the real economy.

[1] Views expressed are those of the authors and do not represent the views of the Board or its staff. Contacts: antonio.falato@frb.gov, giovanni.favara@frb.gov, dscharfstein@hbs.edu. We thank Marco di Maggio, Dan Greenwald (discussant), Marcin Kacperczyk (discussant), Amir Kermani (discussant), seminar participants at UC Berkeley, Stanford, Cambridge, LSE, Imperial, Dartmouth, and the New York Fed, and conference participants at the AFA, SFS Cavalcade, and LBS Summer Finance Symposium for helpful comments and discussions. Jane Brittingham, Mihir Gandhi, Gerardo Sanz-Maldonado and Becky Zhang provided excellent research assistance. Scharfstein thanks the Division of Research at Harvard Business School for financial support. All remaining errors are ours.

## 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 micro evidence on the factors that amplify boom-bust credit cycles is limited, as it is formal testing of theories of cyclical credit volatility in banking. And there is an ongoing debate on the real effects of credit contractions, with estimates ranging widely between economically large (Mian and Sufi, 2014) to intermediate (Chodorow-Reich, 2014) to small (Greenstone, Mas, and Nguyen, 2014).

In an attempt to make progress on these questions, we use rich micro data on bank lending decisions in the U.S. mortgage credit boom and its aftermath as a laboratory. Building on Falato and Scharfstein (2016), we argue that publicly-traded banks should have an incentive to originate more and riskier mortgages in the boom because of their focus on short-term performance. Using detailed geographic information on mortgage loan originations and a research design that controls for changes in local demand, we find strong evidence that public banks run by CEOs with a short-term focus drove the boom. In the aggregate, counties with greater exposure to short-term oriented mortgage originators experienced more severe economic downturns across several important real outcomes in the aftermath of the boom and the Great Recession that ensued. In all, we offer a micro-founded channel through which bank ownership structure and short-termism affect the real economy 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 lenders 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, as our research design is to control for changes in local demand with the inclusion of county-year effects and for unobserved heterogeneity across lenders with the inclusion of lender 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. As such, the granularity of household credit data allows us to isolate a causal link between listing status and the mortgage lending expansion. Since public banks are larger on average than private banks, we also verify that the estimates are robust to matching public and private banks based on their pre-boom size distribution as well as other bank characteristics, such as their reliance on securitization and their national charter.

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 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 lenders' short-term focus using textual analysis of lender's earnings conference calls and of the MD&A section of their annual reports to the SEC. Our proxies include a measure of how actively CEOs discuss short-term results similar to Brochet, Loumioti and Serafeim (2015), and a measure of how short-sighted they are in the discussion of their performance. The effects are more pronounced for public banks with greater short-term focus based on these proxies, as well as those whose CEOs and institutional shareholders trade more actively and those that face short-term pressure because they have relatively low equity valuations both in absolute terms and with respect to their peer banks. In all, robustly across proxies, short-term oriented public lenders amplified the boom.

To buttress a risk-taking interpretation of the mortgage expansion by short-term focused public lenders, we next examine heterogeneity of the effect by loan type. In line with lax lending standards, short-term focused public lenders 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 – during the boom. 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 lenders that are more focused on the short-term.

In the second part of our analysis, we present comprehensive evidence that the risky lending of short-term oriented public banks carries real economic consequences in the aftermath

of the boom. A basic implication of our story is that lending by short-term oriented public banks leads to a build-up of excessive risk, which, in turn, should exacerbate the severity of the subsequent crisis once risks eventually materialize. In line with this reasoning, counties with greater exposure to short-term focused public banks, which is measured based on the market share of these banks pre-boom, experienced a larger decline in house prices, a larger employment drop, and a larger drop in durable consumption and retail sales during the bust. These results are robust to controlling for a host of observable county characteristics and for other drivers of the housing boom that have been recognized in the literature, such as the share of subprime borrowers (Mian and Sufi, 2009) and the share of national banks (Di Maggio and Kermani, 2016). Importantly, the results are also robust to using an identification strategy that directly addresses omitted variable concerns by instrumenting for county mortgage lending with Bartik-style shocks that are plausibly unrelated to local economic conditions. Our estimates of the aggregate effects are economically large. For example, an interquartile range increase in the pre-boom market share of public lenders is associated with an approximate 5 percentage point greater annual decline in house prices and a 1 percentage point greater annual drop in employment between 2007 and 2010, which are about half and a third of a standard-deviation change in their respective unconditional distributions. In all, by amplifying credit cycles, public banks that are focused on the short-term lead to real economic contractions.

In summary, we make two main contributions. First, we highlight a novel microeconomic source of credit volatility based on the ownership structure and short-term focus of some depository institutions. Our evidence that the short-term focus of lenders is an amplification mechanism of credit cycles supports incentive-based theories of aggregate volatility such as, for example, Scharfstein and Stein (1990), and has potentially important policy implications. Namely,

the evidence suggests that financial stability regulation may benefit from taking into account the ownership structure and governance of banks. Second, we provide comprehensive new evidence that the micro source of credit volatility matters for the aggregate effects of credit contractions. While the macro literature has exclusively focused on *whether* credit shocks matter for the real economy, our analysis highlights that it is also important to understand *which* shocks matter. Our finding that there is systematic heterogeneity in the effect of credit contractions depending on the ownership structure and short-term focus of lenders indicates that some but clearly not all credit shocks matter, which can help to reconcile the mixed results in the literature.

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. Section 4 clarifies the risk-taking mechanism and presents the second main result that short-termism driven credit busts have 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 lenders' 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 the four subsequent years

from 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.[2] 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 "lender," which is the main focus of our study.

For each lender, 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 lender in each county. We also track the rejection rate, i.e., the fraction of mortgage applications that are denied. 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 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.

[2] 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.

We complement these data with loan-level information on risk characteristics of the borrower, such as the FICO score, and of the loan, such as LTVs, and post-origination mortgage performance, including defaults and foreclosures, 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).[3]

For each mortgage originated in the credit boom (from 2003 to 2006), the resulting merged HMDA-LPS dataset allows us to track its subsequent performance 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: borrowers' default and mortgage foreclosure. We measure borrowers' default as mortgages that have been delinquent for 90 and more days at least once between 2007 and 2010. Similarly, we classify a mortgage as foreclosed if LPS records that a lender has started a foreclosure procedure on the mortgage at least once during the same period.

[3] These characteristics include, for example, date, zip code, amount, type, purpose, occupancy type, and lien (see, also, Favara and Giannetti, 2016; and Demyanyk and Loutskina (2016)). In performing the HMDA-LPS merge, we replace HMDA lender identifying information with anonymized identifiers in order to adhere to the contract terms of the data providers. 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.

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.[4] Gross income is from the IRS.[5] Foreclosures at the county level are from RealtyTrac.[6] 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,[7] 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*

The final step of our sample construction involves adding comprehensive historical information on lenders' listing status to the HMDA data. To that end, we use the confidential HMDA lender file compiled by the Board of Governors of the Federal Reserve System, which

[4] 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).

[5] 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.

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

[7] 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.

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 demand. Thus, we consider only banks that for the whole sample period were either private or public.

This process leads to a final merged lender-HMDA sample running from 1999-2006 of 375,406 county-lender-year observations for 3,693 unique lenders 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 lenders 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) and in the analysis of mortgage performance in the bust (Merged Lender-LPS Sample, Panel B), as well as in the county-level analysis of the aggregate and real economic consequences in the bust (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 lender 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 the number of annual lenders' originations per county is about 27 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 lenders or lender-county-year observations). Non-depository mortgage companies, which do not have an ID RSSD, 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.[8]

## 3. Determinants of Bank Lending Behavior during the Housing Boom

[8] In the merged Lender-LPS sample in 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 establishes our baseline results on the lending behavior of short-term oriented public banks during the mortgage boom, followed by a battery of robustness tests.

*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):

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

where $i$, $j$, and $t$ index banks, counties, and years, respectively. *Y* is a measure of lender'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 Lender* 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 $\mu_t$, $\mu_j$ and $\mu_i$ 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 lender 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 lender- or county-specific trends in the dependent variable. As such, estimates of our coefficient of interest, $\beta_2$, 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 country-year.[9]

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 lenders' 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) lenders. 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 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.[10]

[9] In robustness analysis, we ensure that the results are not sensitive to this particular choice of clustering (see Appendix Table A.4).

[10] A formal test of the parallel trend assumption is in Appendix Table A.2, Panel A.

Next, we investigate this differential increase in mortgage lending by public banks during the boom in a regression setting that controls for factors related to local demand and for unobserved heterogeneity across lenders.

*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), and two main measures of mortgage lending standards, the dollar volume and number of mortgage rejection rates (Columns 3-4). 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 lenders 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 lenders was about 9 percentage points higher than it was for private lenders. 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

[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

lenders 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 lenders 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 3 and 4 of Panel A 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 4). The estimates indicate that during the boom public lenders were less likely to deny a mortgage application. The effect on rejections is also economically large. For example, the estimate in column 4 implies that, on average in the boom, the annual mortgage rejection rate by public lenders was about 2.5 percentage points lower than it was for private lenders, 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]

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 instead of county) to better control for local demand shocks (Panel A, Columns 3-4). In Panel B of Appendix Table A.1, we replicate the analysis at the bank level using data from Call Reports to show that the differential growth was specific to mortgages. The estimates indicate that public lenders expanded their mortgages as a share relative to total loans in the boom, and interestingly not in the previous credit expansion episode of the late 1980s.

To the extent that the number of applications received captures an element of demand that is bank-specific rather than just county-specific, the results on rejection rates also 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. Matching on Size and Other Lender Covariates*

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 lenders, one may be concerned that the baseline results are driven by differential changes in the behavior of large vs. small lenders over time rather than the risk-taking incentives associated with ownership status. Other potential differences involve the degree of geographic diversification of mortgage risk across markets,[13] reliance on securitization (Keys et al., 2010), and differences in regulation and supervision between national and state-chartered banks (Di Maggio and Kermani, 2016). In this section we use a size-matching procedure that is similar to matched-sample difference-in-differences (Heckman, Ichimura, and Todd, 1997) to ensure that time-varying shocks that are correlated with lender size and these other covariates are not driving the results. Specifically, in Panel B of Table 2 we repeat our DD analysis of mortgage originations and standards with the reweighting method of DiNardo, Fortin, and Lemieux (1996), which flexibly controls for lender-specific shocks by non-

[13] Such geographic diversification, which we measure by the Herfindahl Index (HHI) of mortgage originations across counties, might make public banks more inclined to take risk within any given local market.

parametrically reweighting the public lender sample within every year to match the pre-boom distribution of private lenders based on lender-specific covariates.[14]

In the matching procedure we assign each lender to one of 10 bins according to the size-decile distribution of private lenders before the boom (in 2002), as well as their geographic diversification and securitization, and 2 bins for their national banks status. Within each ownership type 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 lenders in terms of these covariates. For example, if public lenders are more prevalent than private ones in the 90$^{th}$ 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 lender is roughly the same as the probability of observing a private lender. 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 lenders will 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.

Columns 1 and 2 in Panel B of Table 2 report the main results of the regression with this weighting procedure. The estimated effects for originations (Column 1) are similar to our baseline estimate in Column 1 of Panel A, and the results for standards (Column 2) are similar those in Column 4 of Panel A. In Columns 3 and 4 we report the results of an alternative implementation of "matching," which reweights by county population, and for a refined sample that includes only banks that are relatively comparable in terms of covariates by excluding those in the top and bottom

[14] Busso, DiNardo, and McCrary (2014) show that the finite sample properties of reweighting estimators are superior to propensity score matching techniques (where each treated firm is matched to one or several controls).

deciles of the distributions of size, geographic diversification and securitization, as well as those that are not national banks. The specifications that are estimated are otherwise as in Panel A, with lender and county-year fixed effects included in all regressions. The estimates remain strongly significant throughout and are stable across the two samples.

Finally, to assess the validity of the parallel-trend assumption, in Panel A of Appendix Table A.2 we allow for year- specific trends, which are insignificant pre-boom both for originations and standards.[16] In all, these results support the identifying assumption of parallel pre-boom trends and corroborate the internal validity of our DD design.

### *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 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

[16] Panel B of Appendix Table A.2 shows that the results are also robust to an alternative implementation of the overlap sub-sample that excludes lenders that are larger than the largest private lender and those that are smaller than the smallest public lender. Appendix Table A.3 shows that the baseline results on originations are robust to alternative implementations of the matching estimators, which include propensity score matching based on lender size as well as a variety of other observable lender characteristics.

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 exhibit a more aggressive behavior 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. This analysis, therefore, compares the differential behavior of public banks with different degrees of short-term focus in the boom years. 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 additional 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 measure of 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.[20] 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

[20] 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].

increase short-term earnings. The estimates 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 baseline estimates for public ownership in Table 2 and is roughly half as large as the sample mean growth rate of originations for public lenders in the boom (0.205).

Panel B of Table 3 considers two additional text-based measures of CEO short-term disclosure, both based on textual analysis of the management discussion and analysis (MD&A) section of the lenders' annual reports to the SEC. Columns 1 and 3 report results for a measure of short-term disclosure which is defined as the inverse of the average distance (number of days) between dates of future performance discussed in the MD&As and the filing date of their respective annual report. The intuition is that one can gauge short-term focus from the extent to which management emphasizes relatively shorter-term metrics in their discussion of performance. As yet another related alternative, Columns 2 and 4 show results for a measure based on the frequency of words related to short-term disclosure horizons (daily, weekly, monthly, and quarterly) relative to long-term horizons (yearly). This measure is premised on the idea that the extent to which management relies on high-frequency performance metrics should be indicative of a preference for short-term earnings. The estimated effects for originations and rejection rates are statistically significant and economically large for both measures.

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 and stock performance*

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).[23] In Panel B, we ask whether lagged lenders' equity valuation multiples have predictive power for loan originations and standards in the boom. The estimates indicate that relatively low market to book equity ratios tend to be followed by a more aggressive expansion in loan originations and looser standards (Columns 1 and 3, respectively). The results also hold for equity valuation ratios relative to their mean across other lenders that operate in the same county (Columns 2 and 4). There are several reasons why relatively undervalued lenders are likely to face greater pressure to boost short-term prices, including a higher likelihood to receive a takeover bid, as highlighted in Stein (1988), and a higher likelihood of CEO dismissal, as per the evidence in Jenter and Kanaan (2015). As such, these results further support a short-termism interpretation.

[23] 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.

## 4. Evidence on Risk Mechanism and Aggregate Implications

In the second part of our analysis, we present evidence on risky mortgage originations in the boom and mortgage performance in the crisis that buttress a risk-taking interpretation. We then examine the consequences of bank risk taking for real economic activity.

### *4.1. Evidence on Mortgage Risk*

A direct implication of our bank risk-taking 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 lenders expanded more aggressively relative to private lenders 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 lenders 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,[24] or interest only.

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 lenders during the boom were more likely to default or be foreclosed (Panel A), and especially so for public lender 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 lenders 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.[25]

[24] 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 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.

[25] Appendix Table A.5 addresses the concern that the risk for lenders may have been mitigated by the fact that they could securitize mortgages after origination, as the results hold also for mortgages that were not securitized and, thus, remained on banks' balance sheets. We address this concern also in the analysis of origination and standards by including the propensity to securitize as a covariate in the reweighting estimator.

*4.2 Aggregate and Real Effects*

An important implication of our excessive bank risk taking story is that counties with more exposure to short-term oriented public lenders' mortgage expansion should experience a more severe economic downturn. Figure 2 shows that the market share of public lenders displays considerable geographic dispersion across U.S. counties. To examine this important implication, we consider a variety of aggregate and real outcomes at the county level, including house prices, employment, durable consumption, and retail sales. For each of these outcomes, there is considerable geographic heterogeneity in the severity of the cyclical downturn during the crisis. We test whether county-level measures of real economic activity during the bust are explained by exposure to short-term oriented public lenders.[27]

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

$$\Delta Y_{jt=07-10} = \alpha + \beta \times Mkt.\ Share\ of\ Public\ Lenders_{jt=2002} + \gamma \times Z_{jt=2002} + \mu_j + \varepsilon_{jt=07-10}$$

where $j$ and $t$ index counties and time period, respectively. The dependent variable, $\Delta Y$, is a measure of county-level change in house prices, or of the severity of the drop in overall real economic activity, which are all measured in the bust period (2007 to 2010) relative to the boom period (2003 to 2006). *Mkt. Share of Public Lenders* 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

[27] 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.

originated by public lenders in county *j* in 2002 (”Pre-Boom”) to the total number of mortgages originated by all lenders in county *j* in the same year. $Z_{jt}$ is a vector of time-varying county-level controls.

Tables 7 reports the first set of estimates of the cross-county analysis. The results in Panel A indicate that counties with higher exposure to public banks subsequently experienced greater declines in house prices (Column 1). Counties with higher exposure to public banks also experienced larger employment drops (Column 2) and a larger decline in durable consumption (Column 3) and in retail sales (Column 4) in the bust. 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.[29] Finally, all the estimates of the aggregate effects are plausibly large. For example, the estimate of -0.264 in Column 1 of Panel A implies that an interquartile range increase in the market share of public lenders is associated with a 5.3 percentage points average annual decline in house prices, which is a bit over half as large as the standard deviation of the unconditional sample distribution of the annual change in house prices during the crisis (8 percentage points). [31] The estimate of -0.052 in Column 2 implies that an interquartile range increase in the share of public lenders is associated with a 1 percentage points annual drop in employment, which is about a third of a standard deviation of the unconditional sample mean of the change in employment during the bust (3 percentage points).

[29] See Appendix Table A.6, Panel A for the coefficient estimates on the full list of controls.

[31] 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 -0.053 (=0.2*(-0.264)).

To corroborate the short-termism mechanism, Panel B of Table 7 repeats the analysis of aggregate outcomes in the bust using the market share of public lenders whose CEO have a short-term focus in the county in 2002 ("Pre-Boom") as the key 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). All coefficient estimates remain negative and highly statistically significant. As for economic significance, the estimates of the aggregate effects of exposure to short-term focused public lenders are also plausibly large. For example, the estimate of -0.170 in Column 1 of Panel B implies that an interquartile range increase in the market share of short-termist public lenders is associated with a 3 percentage points average annual decline in house prices. The estimate in Column 2 of -0.021 implies that an interquartile range increase in the share of short-termist public lenders is associated with about half percentage point annual drop in employment. Overall, these results indicate that the economic costs of exposure to public ownership can be plausibly attributed to short-termism.

Tables 8 and 9 address the important omitted variable concern that the baseline estimates may spuriously reflect the response of bank lending to changes in local economic conditions, rather than the real effect of bank lending decisions. To address the concern, we refine identification using a Bartik-style strategy that instruments for county mortgage lending with two sets of shocks that are plausibly unrelated to local economic conditions. The shocks in Table 8 are constructed similarly to Greenstone, Mas, and Nguyen (2014) and Amiti and Weinstein (2018) as the (lagged) market-share weighted sum of bank–specific annual changes in the dollar volume of mortgage originations by lenders 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. The shocks in Table 9 are constructed as a geographic “shift” instrument whereby, for each county, the bank–specific

annual changes are the lender-year specific average logarithmic annual changes in the dollar volume of mortgage originations in all other counties excluding the own county. Since we focus only on mortgage originations outside any given county, these lender-specific shocks are by construction unrelated to local economic conditions.

The instrumental variable results confirm our baseline finding that exposure to short-term oriented public lenders has economically significant real effects. Robustly across the two instruments, the estimates in Tables 8 and 9 indicate that credit shocks have real effect only for originations by public lenders (Panel A) and by public lenders whose CEOS have a short-term focus (Panel B). The estimate of the real effects are quite stable across the two instruments and are again strongly economically significant across outcomes. For example, Columns 2 and 3 in Panel A of Table 9 indicate that a one standard-deviation contraction in mortgage originations by public lenders leads to 1.3 percentage points and 3.3 percentage points drop in employment and durable consumption, respectively, which correspond to roughly half and one third of their respective unconditional sample means in the bust. By contrast, the estimates are never statistically significant for private lenders or public lenders that are not short-term oriented, indicating that contractions in mortgage originations by these lenders do not carry detrimental real effects. The lack of statistical significance for some sub-groups provides a useful falsification or placebo test for our Bartik identification strategy because, if we failed to purge the instruments of shocks to local economic conditions, then we should see significant estimates for all sub-groups. As such, omitted common factors related to local economic conditions that are unrelated to mortgage lending are unlikely to be driving our estimates of the aggregate effects.[33]

[33] Appendix Table A.6 addresses the residual concern of geographic spillovers, which may induce spurious correlation if lending in a given county is affected by changes in local economic conditions in close contiguous counties. Specifically, we show that the main estimates in Table 9 are little changes when we repeat the Bartik analysis using the same approach to construct the geographic instrument but with two modifications, in turn: first, we construct the lender-specific shocks as the lender-year specific average logarithmic excess annual change in the dollar volume of mortgage originations in all other counties excluding the own county, which is obtained after de-meaning originations by each lender in any given county-year of their county-year specific mean to address local

Finally, in Table 10 we corroborate the relevance of our results by repeating the Bartik analysis for a broader set of aggregate housing and labor market outcomes, which include the change in housing permits, and the change in the unemployment rate as well as in wages and median incomes. The results confirm our earlier findings and indicate that the real effects extend to these other outcomes. The economic significance of the estimates is also confirmed by the analysis of these additional outcomes. For example, Columns 2 and 3 in Panel A of Table 10 indicate that a one standard-deviation contraction in mortgage originations by public lenders leads to 0.5 percentage points increase in the unemployment rate and 0.6 percentage points drop in wage income, which correspond to about half and the full unconditional sample mean changes in the unemployment rate and wages in the bust, respectively.

## 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 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 lenders in the extent to which they relaxed lending standards in the mortgage

shocks in other counties that may be correlated with own county shocks (Panel B); second, we construct the lender-specific shocks as the lender-year specific average logarithmic annual change in the dollar volume of mortgage originations in all other counties excluding all counties in own state, to control for correlated local shocks between counties within state (Panel C).

boom, with lenders' emphasis on the short-term being systematically related in the cross-section to their mortgage portfolio expansion and 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 market appreciates that the ROE can be increased simply by taking more risk, then risk-adjusted measures of ROE should better explain valuation multiples. However, the fact that the stock market actually appears to reward banks that have high ROE because of their high leverage, as shown by Begenau and Stafford (2016), suggests that the market may also reward – or at least not penalize – banks that increase earnings through an increase in the risk of their mortgage loan portfolio. 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. Exploring this possibility and whether there is a tension between bank equity investors' short-term gains from risk-taking and the real costs to the economy we have documented in this paper constitutes an interesting venue for future research.

**References**

Adelino, M., A. Schoar, and F. Severino, 2016, "Loan Originations and Defaults in the Mortgage Crisis: The Role of the Middle Class," *Review of Financial Studies*, forthcoming.

Agarwal, S., G. Amromin, I. Ben-David, and D. Evanof, 2016, "Loan Product Steering in Mortgage Markets," Working Paper, University of Chicago.

Amiti, M. and D. E. Weinstein, 2018, "How Much Do Idiosyncratic Bank Shocks Affect Investment? Evidence from Matched Bank-Firm Loan Data," *Journal of Political Economy*, 126(2), 525-587.

Angrist, J. and J.-S., Pischke, 2009, *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press.

Avery, R.B., N. Bhutta, K.P. Brevoort, and G.B. Canner, 2012, "The Mortgage Market in 2011: Highlights from the Data Reported under the Home Mortgage Disclosure Act," Federal Reserve Bulletin, Vol. 98, No. 6, Board of Governors of the Federal Reserve System.

Begenau, J. and E. Stafford, "Inefficient Banking," 2016, Harvard Business School Working Paper.

Brochet, F., M. Loumioti, and G. Serafeim, 2015, "Speaking of the Short-Term: Disclosure Horizon and Managerial Myopia," Harvard Business School Working Paper.

Burnside C., M. Eichenbaum and S. Rebelo (2011), "Understanding Booms and Busts in Housing Markets," NBER Working Paper 16734.

Busso, M., J. DiNardo, and J. McCrary (2009) "New Evidence on the Finite Sample Properties of Propensity Score Matching and Reweighting Estimators," *Review of Economics and Statistics*, 96(5), 885-897.

Chodorow-Reich, G., 2014, "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-09 Financial Crisis," *Quarterly Journal of Economics*, 129(1), 1-59.

Demyanyk, Y. and E. Loutskina, 2016, "Mortgage Companies and Regulatory Arbitrage," *Journal of Financial Economics*, 122(2), 328-351.

Demyanyk, Y. and O. Van Hemert, 2011, "Understanding the Subprime Mortgage Crisis," *Review of Financial Studies*, 24 (6): 1848-1880.

Di Maggio, M. and A. Kermani, 2016, "Credit-Induced Boom and Bust," *Review of Financial Studies*, forthcoming.

DiNardo, J., N.M. Fortin, and T. Lemieux, 1996, "Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach," Econometrica, 64 (5), 1001-1044.

Falato, A. and D. Scharfstein, 2016, "The Stock Market and Bank Risk-Taking," NBER Working Paper No. 22689.

Favara, G. and M. Giannetti, 2016, Forced Asset Sales and the Concentration of Outstanding Debt: Evidence from the Mortgage Market, *Journal of Finance*, forthcoming.

Favara, G. and J. Imbs, 2015. Credit Supply and the Price of Housing, *American Economic Review*, 105, 958-992.

Gaspar, J., M. Massa and P. Matos, 2005, "Shareholder Investment Horizon and the Market for Corporate Control," *Journal of Financial Economics*, 76, 135-165.

Garmaise, M., and T. Moskowitz, 2006, "Bank Mergers and Crime: The Real and Social Effects of Credit Market Competition," *Journal of Finance*, 61, 495—538.

Giroud, X. and H. M. Mueller, 2015, “Firm Leverage and Unemployment during the Great Recession,” *Quarterly Journal of Economics*, forthcoming.

Greenstone, M., A. Mas, and H.-L. Nguyen, 2014, “Do Credit Market Shocks Affect the Real Economy? Quasi-Experimental Evidence from the Great Recession and "Normal" Economic Times," NBER Working Paper No. 20704.

Heckman, J. J., H. Ichimura and P. E. Todd, 1997, "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme," *The Review of Economic Studies*, 64, 605-654.

Jenter D. and F. Kanaan, 2015, “CEO Turnover and Relative Performance Evaluation, *Journal of Finance*, 70(5), 2155-2183.

Keys, B.J., T. Mukherjee, A. Seru, V. Vig, 2010, “Did Securitization Lead to Lax Screening? Evidence from Subprime Loans,” The Quarterly Journal of Economics, Volume 125, Issue 1, 307–362.

Krishnamurthy, A. and T. Muir, 2017, “How Credit Cycles across a Financial Crisis,” Working paper, Stanford University.

La Porta, R., 1996, “Expectations and the Cross-Section of Expected Returns,” *Journal of Finance*, 51, 1715-1742.

Lamont, O. and J. Stein, 1999, “Leverage and House-Price Dynamics in U.S. Cities,” *RAND Journal of Economics*, 30, 498-514.

López-Salido, D., J. C. Stein, and E. Zakrajšek, 2015, “Credit-Market Sentiment and the Business Cycle,” Working paper, Federal Reserve Board and Harvard University.

Loughran T. and B. McDonald, 2011, ″When is a Liability not a Liability? Textual Analysis, Dictionaries, and 10-Ks,″ Journal of Finance, 66:1, 35-65.

Mian, A., K. Rao, and A. Sufi, 2013, "Household Balance Sheets, Consumption, and the Economic Slump," *Quarterly Journal of Economics*, 128, 1687—1726.

Mian, A. R. and A. Sufi, 2009, "The Consequences of Mortgage Credit Expansion: Evidence from the U.S. Mortgage Default Crisis," *Quarterly Journal of Economics*, 124, 1449-1496.

Mian, A. R. and A. Sufi, 2014, "What Explains the 2007–2009 Drop in Employment?" *Econometrica*, 82(6), 2197–2223.

Mian, A. R., A. Sufi and E. Verner, 2017, "Household Debt and Business Cycles Worldwide," *Quarterly Journal of Economics*, 132, 1755–1817.

Paravisini, D., 2008, "Local Bank Financial Constraints and Firm Access to External Finance," *Journal of Finance*, 63, 2161-2193.

Scharfstein, D. S., and J. C Stein, 1990, "Herd Behavior and Investment," *The American Economic Review*, 80 (3), pp. 465-479

Scharfstein, D. S., and A. Sunderam, 2015, "Market Power in Mortgage Lending and the Transmission of Monetary Policy," HBS Working Paper.

Stein, J. C., 1988, "Takeover Threats and Managerial Myopia," *Journal of Political Economy*, 96(1), 61-80.

Stein, J. C., 1989, "Efficient Capital Markets, Inefficient Firms: A Model of Myopic Corporate Behavior," *Quarterly Journal of Economics*, 104:655-669.

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 lender-county-year observations involving 3,693 unique lenders between 1999 and 2006. This sample consists of data in *HMDA* on mortgages originated or denied between 1999 and 2006 by lenders 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 lenders 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 $\Delta \text{Bust}_{2007-2010}$ denotes averages between 2007 and 2010 (the "Bust" period) relative to between 2003 and 2006 (the "Boom" period).

| Variable Name | Description (Source) | Mean | St. Dev. |
|---|---|---|---|
| **Panel A: Merged Lender-HMDA Sample** | | | |
| *Lender Listing Status:* | | | |
| Unique Public Banks (%) | Dummy variable that takes the value of 1 if the lender is pubicly-traded, and is 0 otherwise 9 (Hand-collected). | 0.25 | |
| *Lender-County Mortgage Originations and Standards:* | | | |
| Mortgages Originated (number) | Number of conventional loans originated for purchase of single family owner occupied houses. Lender-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. Lender-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. Lender-county level aggregation of loan level data (HMDA). | 0.24 | 0.18 |
| | Observations (county-entity-year) | 375,406 | |
| | Banks | 3,693 | |
| **Panel B: Merged Lender-LPS Sample** | | | |
| *Lender Listing Status:* | | | |
| Unique Public Banks (%) | Dummy variable that takes the value of 1 if the lender is pubicly-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 lender-county-year observations involving 3,693 unique lenders between 1999 and 2006. This sample consists of data in *HMDA* on mortgages originated or denied between 1999 and 2006 by lenders 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 lenders 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 $\Delta\text{Bust}_{2007-2010}$ denotes averages between 2007 and 2010 (the "Bust" period) relative to between 2003 and 2006 (the "Boom" period).

| Panel C: County-Level Sample | | | |
|---|---|---|---|
| Variable Name | Description (Source) | Mean | St. Dev. |
| *Lender Listing Status:* | | | |
| Mkt. share of public lenders$_{t=2002}$ | 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 lenders to total originations (HMDA). | 0.78 | 0.15 |
| Mkt. share of short-term pub. lenders$_{t=2002}$ | 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 lenders whose CEO have a short-term focus to total originations (HMDA). | 0.14 | 0.17 |
| *Outcomes:* | | | |
| House price change, $\Delta\text{Bust}_{2007-2010}$ | Logarithmic annual change in house prices (CoreLogic). | -0.09 | 0.08 |
| Change in Retail Sales, $\Delta\text{Bust}_{2007-2010}$ | Logarithmic annual change in retail sales (Moody's Analytics) | -0.06 | 0.04 |
| Change in Employment, $\Delta\text{Bust}_{2007-2010}$ | Logarithmic annual change in employment (Census Bureau, CBP). | -0.03 | 0.03 |
| Durable cons. change, $\Delta\text{Bust}_{2007-2010}$ | Logarithmic annual change in the total number of new automobile purchases (R.L. Polk). | -0.10 | 0.07 |
| *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 Lender Ownership

This table summarizes our baseline estimates from regression analysis of mortgage originations and standards in the boom on lenders' ownership structure. The sample is the merged lender-HMDA sample, which consists of lender-county-year observations between 1999 and 2006 with data in HMDA on mortgages originated or denied by lenders 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:

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

where $i$ denotes lender, $j$ 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 Lender* is an indicator variable that takes a value of one for lenders whose top-holders is publicly-traded and zero otherwise. Year-county fixed effects, $\mu_{jt}$, and lender fixed effects $\mu_i$, 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 3 and 4 report results for the rejection rates of mortgage volumes and numbers, respectively. Panel B refines identification. Columns 1 and 2 report results of matched-sample analysis using the reweighting method of DiNardo, Fortin, and Lemieux (1996), which flexibly controls for time-varying lender-specific shocks by non-parametrically reweighting the public lender sample (within each year) to match the distribution of private lenders across bins based on lender-specifc covariates. Specifically, we show results for binning each lender into 10 bins according to the size-decile distribution of private lenders pre-boom (in 2002), 10 bins for the geographic diversification-decile distribution of private lenders pre-boom (measured by the HHI index of lender' originations across counties), 10 bins for the securitization-decile distribution of private lenders pre-boom (measured by the ratio of private-label mortgage securitizations relative to originations), and 2 bins for national bank status-distribution of private lenders pre-boom . Within each lender 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 lenders. Columns 3 and 4 show results for the "overlap sub-sample," which refines the sample by excluding lenders in the top and bottom deciles of the distributions of size, geographic diversification and securitization, as well as those that are not national banks. 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 Lenders' Ownership Structure | | | | |
|---|---|---|---|---|
| | Δ Log Originations | | Rejection Rate | |
| | Volume ($) | Number | Volume ($) | Number |
| | (1) | (2) | (3) | (4) |
| Boom*Public Lender | 0.088*** | 0.066*** | -0.025*** | -0.025*** |
| | (0.016) | (0.007) | (0.002) | (0.002) |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 375,406 | 375,406 | 375,406 | 375,406 |
| $R^2$ | 0.098 | 0.104 | 0.399 | 0.268 |
| Economic Significance | | | | |
| Sample Mean | 0.076 | 0.036 | 0.230 | 0.244 |
| Sample SD | 0.766 | 0.680 | 0.185 | 0.181 |
| Sample Mean, Before | 0.030 | 0.011 | 0.251 | 0.266 |
| Sample Mean, Boom | 0.123 | 0.061 | 0.210 | 0.222 |
| Panel B: DFL (1996)-Reweighted Analysis of Lenders' Ownership Structure by Pre-Boom Lender Size, Diversification, Securitization, and National Bank Status | | | | |
| | All, DFL(1996)-Reweighted | | Overlap Sub-Sample, Pop. Weighted | |
| | Δ Log Originations Volume ($) | Rejection Rate | Δ Log Originations Volume ($) | Rejection Rate |
| | (1) | (2) | (3) | (4) |
| Boom*Public Lender | 0.079*** | -0.019*** | 0.132*** | -0.054*** |
| | (0.031) | (0.004) | (0.038) | (0.010) |
| Year-County | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 199,199 | 199,199 | 103,262 | 103,262 |
| $R^2$ | 0.235 | 0.467 | 0.287 | 0.504 |

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

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

$$Y_{ijt} = \alpha + \beta_1 Lender\ ST_{it-1} + \gamma Z_{ijt} + \mu_{jt} + \mu_i + \varepsilon_{ijt},$$

where $i$ denotes lender, $j$ denotes county, and $t$ denotes time. *Lender ST* is a proxy for the short-term focus of public lenders' CEOs. Year-county fixed effects, $\mu_{jt}$, and lender fixed effects $\mu_i$, are included in all regressions. In Panel A, lender CEO short-term focus is measured as the frequency of CEO words related to short-term horizon in the transcripts of the lender's earnings conference calls and in the MD&A section of the lender's annual reports to the SEC. In Panel B, we consider two additional proxies for lnder CEO short-term disclosure that are also based on the MD&A section of the lender's annual reports to the SEC and are measured as the inverse of the average distance (number of days) between future disclosed dates that appear in any given report report relative to the report filing date (Columns 1 and 3) and as the frequency of words related to high-frequency disclosure horizons (daily, monthly, and quarterly) relative to those related to a yearly frequency horizon (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 Lenders in the Boom – CEO Short-Term Focus | | | | |
|---|---|---|---|---|
| | Δ Log Originations | | Rejection Rate | |
| | Volume ($) | Number | Volume ($) | Number |
| | (1) | (2) | (3) | (4) |
| Lender ST | 0.110*** | 0.086*** | -0.019*** | -0.021*** |
| | (0.015) | (0.012) | (0.004) | (0.004) |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 50,056 | 50,056 | 50,056 | 50,056 |
| $R^2$ | 0.253 | 0.269 | 0.420 | 0.424 |
| 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 Lender CEO Short-Term Disclosure | | | | |
| | Δ Log Originations ($) | | Rejection Rates | |
| | Short-Horizon Future Disclosure | High-Frequency Disclosure | Short-Horizon Future Disclosure | High-Frequency Disclosure |
| | (1) | (2) | (3) | (4) |
| Lender ST | 0.111*** | 0.133*** | -0.012*** | -0.020*** |
| | (0.028) | (0.030) | (0.004) | (0.004) |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 39,392 | 39,839 | 39,392 | 39,839 |
| $R^2$ | 0.259 | 0.257 | 0.403 | 0.400 |

Table 4: Mechanism – Additional Heterogeneous Effects By Public Lender in the Boom

This table summarizes sensitivity analysis of mortage originations and standards in the housing boom years (2003-2006) to using alternative proxies for lenders' short-term focus. The sample is the merged lender-HMDA sample, which consists of lender-county-year observations between 1999 and 2006 with data in HMDA on mortgages originated or denied by lenders with available information on whether their top-holder is privately-held or publicly-traded. In Panel A, the two proxies for lender short-term focus are the frequency of the lender's CEO net-sales of stock (Columns 1 and 3) and the lender's average institutional investors' portfolio turnover based on Cahart (1997) (Columns 2 and 4). Panel B examines lenders' (lagged) equity valuation multiples. Columns 1 and 3 are for the lender market-to-book ratio, while Columns 2 and 4 use the residual of the market-to-book ratio over the average market-to-book ratio in the county-year to proxy for relative valuation. The dependent variabales are either the annual change in log dollar value or the rejection rates of mortgage volumes. Year-county and lender 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: Lender 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) |
| Lender ST | 0.193*** | 0.244*** | -0.019*** | -0.024*** |
| | (0.025) | (0.023) | (0.003) | (0.003) |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 21,732 | 57,475 | 21,732 | 57,475 |
| $R^2$ | 0.332 | 0.216 | 0.501 | 0.4 |
| Panel B: Lender Equity Valuations (Market-to-Book Ratio) | | | | |
| | Δ Log Originations ($) | | Rejection Rates | |
| | MB | MB-rival MB | MB | MB-rival MB |
| | (1) | (2) | (3) | (4) |
| Lag Lender MB | -0.115*** | -0.073*** | 0.028*** | 0.014*** |
| | (0.031) | (0.025) | (0.004) | (0.003) |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 57,316 | 57,113 | 57,316 | 57,113 |
| $R^2$ | 0.214 | 0.215 | 0.404 | 0.404 |

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 lender 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 varibale 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 Lender*, which is an indicator variable that takes a value of one for lenders whose top-holders is publicly-traded and zero otherwise. Panel B focuses on the comparison within public lenders based on the short-term focus of their CEOs. The proxy for public lenders' CEO short-term focus, *Lender ST*, is measured as the frequency of CEO words related to short-term horizon in the transcripts of the lender's earnings conference calls and in the MD&A section of the lender'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 Lenders' Ownership Structure – Δ Log X Originations, X= | | | | |
|---|---|---|---|---|
| | High LTV | IO | Subprime | High DTI |
| | (1) | (2) | (3) | (4) |
| Public Lender | 0.315*** | 0.186** | 0.445** | 0.135*** |
| | (0.033) | (0.033) | (0.026) | (0.033) |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender 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 Lenders' CEO Short-Term Focus – Δ Log X Originations, X= | | | | |
| | High LTV | IO | Subprime | High DTI |
| | (1) | (2) | (3) | (4) |
| Lender ST | 0.236** | 0.395*** | 0.253*** | 0.166*** |
| | (0.100) | (0.091) | (0.072) | (0.099) |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender 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 Lender*, which is an indicator variable that takes a value of one for lenders whose top-holders is publicly-traded and zero otherwise. Panel B reports results for public lenders' CEO short-term focus, which is measured as the frequency of CEO words related to short-term horizon in the transcripts of the lender's earnings conference calls and in the MD&A section of the lender'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 Lenders' Ownership Structure | | | | |
|---|---|---|---|---|
| | 90+ Day Mortgage Delinquencies | | Mortagage Foreclosures | |
| | (1) | (2) | (3) | (4) |
| Public Lender | 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 Lenders' Short-Term Focus | | | | |
| Lender 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 |
| Lender 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 2007 to 2010 period ("Bust") relative to the 2003 to 2006 period ("Boom"): 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). In Panel A, the main explanatory variable is the market share of public lenders in the county measured in 2002 ("Pre-Boom"). In Panel B, the main explanatory variable is the market share of public lenders 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 pupulation 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: $\Delta Bust_{2007-2010}$ by Exposure to Local Public Lenders | | | | |
|---|---|---|---|---|
| | Change in House Prices | Change in Employment | Change in Durable Consumption | Change in Retail Sales |
| | (1) | (2) | (3) | (4) |
| Mkt. share of public lenders$_{t=2002}$ | -0.264*** | -0.052*** | -0.208*** | -0.050*** |
| | (0.035) | (0.008) | (0.033) | (0.010) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 769 | 779 | 769 | 781 |
| $R^2$ | 0.372 | 0.172 | 0.330 | 0.101 |
| Panel B: $\Delta Bust_{2007-2010}$ by Exposure to Local Public Lenders with CEO Short-term Focus | | | | |
| | 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. lenders$_{t=2002}$ | -0.170*** | -0.021** | -0.109*** | -0.028*** |
| | (0.056) | (0.010) | (0.028) | (0.010) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 769 | 779 | 769 | 781 |
| $R^2$ | 0.300 | 0.129 | 0.277 | 0.079 |

Table 8: Bartik Analysis of Aggregate and Real Effects

This table reports results for the cross-sectional county-level Bartik regression analysis of aggregate and real outcomes during the 2007 to 2010 period ("Bust") relative to the 2003 to 2006 period ("Boom"). Panel A reports results of the second-stage Bartik analysis where we instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public vs. private lenders using the (lagged) market-share weighted sum of bank–specific annual changes in the dollar volume of mortgage originations by lenders that are active in the county. These lender-specific shocks are estimated using a regression that controls for local demand shocks by including county-year effects. Panel B reports results of the second-stage Bartik analysis where we follow the same approach to instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public lenders 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 pupulation 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: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders, 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) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Private,t=2007-2010}$ | 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 |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Private,t=2007-2010}$ | 1.0 | 0.3 | 1.0 | 0.4 |
| Panel B: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders 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 |

Table 9: Bartik Analysis of Aggregate and Real Effects: Geographic Instrument

This table reports results for the cross-sectional county-level Bartik regression analysis of aggregate and real outcomes during the 2007 to 2010 period ("Bust") relative to the 2003 to 2006 period ("Boom") using a geographic "shift" instrument. Panel A reports results of the second-stage Bartik analysis where we instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public vs. private lenders using the (lagged) market-share weighted sum of bank–specific annual changes in the dollar volume of mortgage originations by lenders that are active in the county. These lender-specific shocks are constructed for any given country-year as the (lender-year specific) average logarithmic annual change in the dollar volume of mortgages originated by a given lender-year in other counties excluding the own county, as an alternative strategy that only includes lending outside any given county to control for local demand shocks. Panel B reports results of the second-stage Bartik analysis using the geographic "shift" instrument where we follow the same approach to instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public lenders 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 pupulation 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.

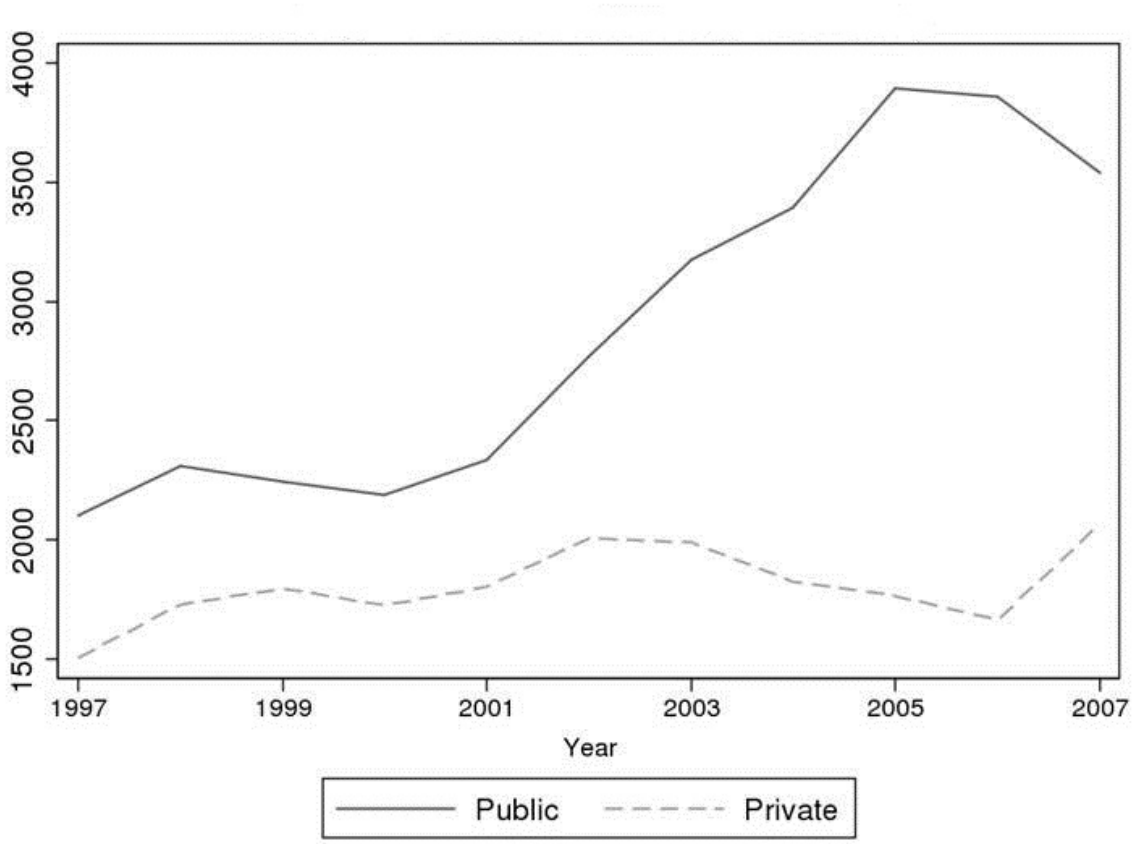
| Panel A: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders, Geographic Instrument | | | | |
|---|---|---|---|---|
| | Change in House Prices | Change in Employment | Change in Durable Consumption | Change in Retail Sales |
| | (1) | (2) | (3) | (4) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$ | 0.325*** | 0.085*** | 0.210*** | 0.090*** |
| | (0.042) | (0.012) | (0.036) | (0.014) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{\text{Pr}ivate,t=2007-2010}$ | -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 |
| $\Delta \widehat{Log\ Orig}\ (\$)_{\text{Pr}ivate,t=2007-2010}$ | -0.9 | -0.1 | 1.3 | -0.1 |
| Panel B: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders 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 |

Table 10: Bartik Analysis of Aggregate and Real Effects: Additional Housing and Labor Market Outcomes

This table reports results for the cross-sectional county-level Bartik regression analysis of additional aggregate and real outcomes during the 2007 to 2010 period ("Bust") relative to the 2003 to 2006 period ("Boom"). Panel A reports results of the second-stage Bartik analysis where we instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public vs. private lenders using the (lagged) market-share weighted sum of bank–specific annual changes in the dollar volume of mortgage originations by lenders that are active in the county. These lender-specific shocks are estimated using a regression that controls for local demand shocks by including county-year effects. Panel B reports results of the second-stage Bartik analysis where we follow the same approach to instrument for the average logarithmic annual change in the dollar volume of mortgage originations in the county by public lenders 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 pupulation 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.

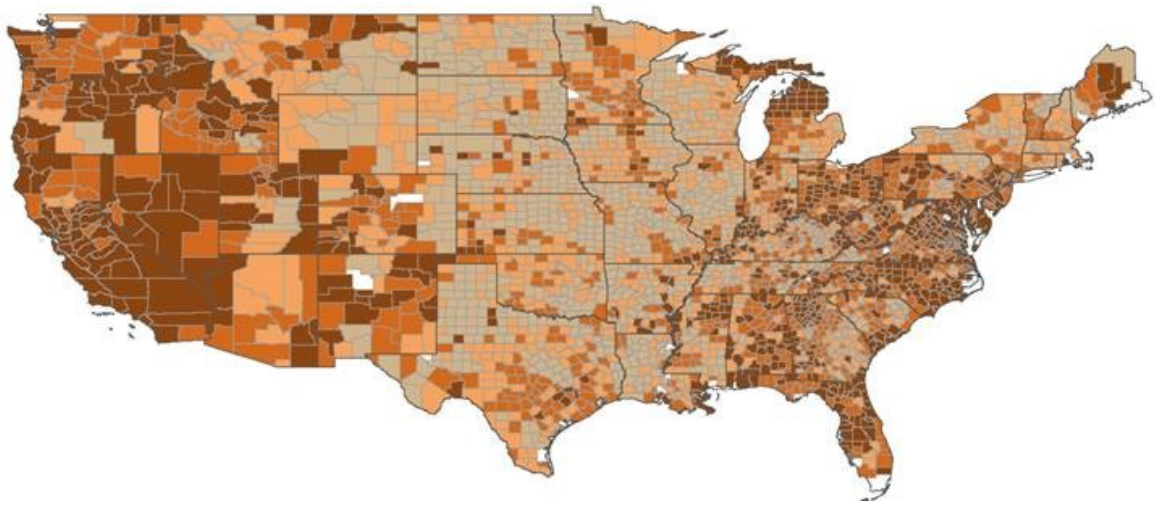
| Panel A: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders, Bartik Analysis | | | | |
|---|---|---|---|---|
| | Change in Housing Permits | Change in Unemployment Rate | Change in Wage Income | Change in Median Income |
| | (1) | (2) | (3) | (4) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$ | 0.449*** | -0.062*** | 0.072*** | 0.093*** |
| | (0.152) | (0.006) | (0.024) | (0.027) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{\text{Pr}ivate,t=2007-2010}$ | 0.055 | -0.007 | 0.004 | 0.022 |
| | (0.130) | (0.008) | (0.020) | (0.022) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 777 | 768 | 788 | 788 |
| $R^2$ | 0.186 | 0.142 | 0.120 | 0.191 |
| Economic Significance, 1 St.Dev.Change in RHS (pct.pt.) | | | | |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$ | 4.4 | -0.5 | 0.6 | 0.7 |
| $\Delta \widehat{Log\ Orig}\ (\$)_{\text{Pr}ivate,t=2007-2010}$ | 0.6 | -0.1 | 0.0 | 0.2 |
| Panel B: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders with CEO Short-term Focus, Bartik Analysis | | | | |
| | Change in Housing Permits | Change in Unemployment Rate | Change in Wage Income | Change in Median Income |
| | (1) | (2) | (3) | (4) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$ | 1.562** | -0.073*** | 0.266** | 0.564*** |
| | (0.630) | (0.009) | (0.125) | (0.162) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$ | -0.576 | -0.005 | 0.122 | 0.130 |
| | (0.599) | (0.014) | (0.104) | (0.106) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 777 | 768 | 788 | 788 |
| $R^2$ | 0.173 | 0.115 | 0.123 | 0.211 |
| Economic Significance, 1 St.Dev.Change in RHS (pct.pt.) | | | | |
| $\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$ | 3.5 | -0.2 | 0.6 | 1.1 |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$ | -1.3 | -0.0 | 0.2 | 0.3 |

Figure 1: Mortgage Originations by Public vs. Private Banks Before and in the Boom




This figure plots the average annual ($1,000) value of mortgage originations at the lender-county level over time. The solid line is for puiblicly-traded banks, while the dashed line is for privately-held banks. The sample is the merged lender-HMDA sample, which is defined as those lenders 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 2: County Distribution of the Boom in Mortgage Originations by Public vs. Private Banks



This figure plots the market share of public lenders in each U.S. county – i.e., fraction of mortgages originated by publicly-traded lenders in each county – during the 2003-2006 period ("Boom"). The sample is the merged lender-HMDA sample, which is defined as those lenders 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.

# Appendix: Additional Results For "Bank Risk-Taking and the Real Economy"

Table A.1: Additional Analysis of Mortgage Originations and Standards in the Boom by Lender Ownership

This table reports addditional results of the difference-in-differences analysis of mortgage originations volumes and standards. Specifically, Panel A shows robustness of the main results for lender ownership in Table 2, Panel A 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 lender-census tract-year level The specification is otherwise the same as detailed in Table 2, to which we refer for details. Panel B reports additional results of difference-in-differences analysis of the growth of residential mortgages relative to total loans. 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 Lender | 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 |
| Lender 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: Call Reports – Growth of Residential Mortages/Total Loans | | | | |
| | 1999-2006 | | Placebo Test, 1987-1994 | |
| | (1) | (2) | (3) | (4) |
| | Year Effects | Bank Effects | Year Effects | Bank Effects |
| Boom*Public Lender | 0.106*** | 0.039** | -0.000 | 0.000 |
| | (0.016) | (0.018) | (0.000) | (0.000) |
| Year FE | Yes | Yes | Yes | Yes |
| Lender FE | No | Yes | No | Yes |
| Lender Controls | Yes | Yes | Yes | Yes |
| Obs. | 24,109 | 23,910 | 47,894 | 47,682 |
| $R^2$ | 0.192 | 0.353 | 0.014 | 0.159 |

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

This table reports additional identification tests of mortgage originations and standards in the boom by lender ownership. Panel A shows results of diagnostic tests of the parallel trend assumption. Panel B show results for an alternative implementation of the overalp sub-sample that excludes lenders that are larger than the largest private lender and smaller than the smallest public lender. The respective specifications that are estimated are otherwise as in Table 2, to which we refer for details. 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 Lender Size Controls | | Pop. Weighted, with Lender Size Controls | |
| | Δ Log Originations Volume ($) | Rejection Rate | Δ Log Originations Volume ($) | Rejection Rate |
| | (1) | (2) | (3) | (4) |
| Boom*Public Lender | 0.173*** | -0.016** | 0.175*** | -0.016** |
| | (0.059) | (0.008) | (0.059) | (0.008) |
| $I_{2002}$*Public Lender | -0.000 | -0.002 | -0.003 | 0.000 |
| | (0.068) | (0.009) | (0.069) | (0.009) |
| $I_{2001}$*Public Lender | -0.115 | 0.003 | -0.111 | 0.001 |
| | (0.091) | (0.009) | (0.073) | (0.009) |
| $I_{2000}$*Public Lender | 0.093 | 0.004 | 0.111 | 0.002 |
| | (0.069) | (0.009) | (0.079) | (0.009) |
| Year-County | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 199,199 | 199,199 | 199,199 | 199,199 |
| Panel B: Alternative Overlap Sub-sample based on Lender Size | | | | |
| | Δ Log Originations ($) | | Rejection Rate | |
| | Baseline | DFL (1996) Rew. | Baseline | DFL (1996) Rew. |
| | (1) | (2) | (3) | (4) |
| Boom*Public Lender | 0.188*** | 0.193*** | -0.015*** | -0.008* |
| | (0.026) | (0.026) | (0.003) | (0.005) |
| Year FE | Yes | No | Yes | No |
| County FE | Yes | No | Yes | No |
| Year-County FE | No | Yes | No | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 151,605 | 151,605 | 151,605 | 151,605 |
| $R^2$ | 0.035 | 0.036 | 0.380 | 0.413 |

Table A.3: Additional Matched-Sample Analysis of Originations and Standards in the Boom by Lender Ownership

This table reports additional identification tests of mortgage originations and standards in the boom by lender ownership. Panels A-C report results of alternative implementations of the matched-sample analysis. Specifically, the matched-sample specification that is estimated is $(Y_{ijt} - Y_{ijt-1}) - (Y_{ijt} - Y_{ijt-1})^{Match} = \alpha + \beta_1 Boom_t + \gamma Z_{ijt} + \mu_j + \mu_i + \varepsilon_{ijt}$, where $i$ denotes lender, $j$ denotes county, and $t$ denotes time. $Boom$ is an indicator variable that takes a value of one for all the housing boom years (2003-2006) and zero otherwise, and $(Y_{ijt} - Y_{ijt-1})^{Match}$ is the value of mortage originations volumes for the match of lender $i$ in county $j$ in year $t$ in the control group of matched private lenders. We use two different procedures to choose a match: size matching (Panel A), and propensity score matching (Panel B). Lender size is measured based on total assets pre-boom. Propensity score matching adds top size the following covariates, which are also all measured as of pre-boom: geographic diversification, the ratio of deposits to total assets, tier 1 capital, the ratio of total loans to total assets, return on equity (ROE), and pre-boom growth of originations. Panels C and D show univariate t-tests and the coefficeint estimates for the propensity score covariates. Lender and county fixed effects are included in all regressions. 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: Size Matching Estimators – Excluding Top & Bottom Quintiles, Using Different Perccentiles | | | | |
|---|---|---|---|---|
| | Quintile w/o Top & Bottom | Tercile Matching | Decile Matching | Rejection Rate |
| | (1) | (2) | (3) | (4) |
| $ATE^{Public-Private\ Lender}$ | 0.274*** | 0.095*** | 0.084*** | -0.037*** |
| | (0.015) | (0.011) | (0.011) | (0.002) |
| Year FE | No | No | No | No |
| County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 84,820 | 144,650 | 144,389 | 84,820 |
| Panel B: Propensity Score Matching – Excluding Top & Bottom Quintiles, Adding Covariates | | | | |
| | w/o Top & Bottom Quintiles of Size | Add Covariates | w/o Top & Bottom Quintiles of PS | Deciles |
| | (1) | (2) | (3) | (4) |
| $ATE^{Public-Private\ Lender}$ | 0.141*** | 0.155*** | 0.203*** | 0.122*** |
| | (0.015) | (0.010) | (0.013) | (0.012) |
| Year FE | No | No | No | No |
| County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 84,932 | 144,688 | 79,486 | 119,471 |

Table A.3: Additional Matched-Sample Analysis of Originations and Standards in the Boom by Lender Ownership (Continued)

This table reports additional identification tests of mortgage originations and standards in the boom by lender ownership. Panels A-C report results of alternative implementations of the matched-sample analysis. Specifically, the matched-sample specification that is estimated is $(Y_{ijt} - Y_{ijt-1}) - (Y_{ijt} - Y_{ijt-1})^{Match} = \alpha + \beta_1 Boom_t + \gamma Z_{ijt} + \mu_j + \mu_i + \varepsilon_{ijt}$, where $i$ denotes lender, $j$ denotes county, and $t$ denotes time. $Boom$ is an indicator variable that takes a value of one for all the housing boom years (2003-2006) and zero otherwise, and $(Y_{ijt} - Y_{ijt-1})^{Match}$ is the value of mortage originations volumes for the match of lender $i$ in county $j$ in year $t$ in the control group of matched private lenders. We use two different procedures to choose a match: size matching (Panel A), and propensity score matching (Panel B). Lender size is measured based on total assets pre-boom. Propensity score matching adds top size the following covariates, which are also all measured as of pre-boom: geographic diversification, the ratio of deposits to total assets, tier 1 capital, the ratio of total loans to total assets, return on equity (ROE), and pre-boom growth of originations. Panels C and D show univariate t-tests and the coefficeint estimates for the propensity score covariates. Lender and county fixed effects are included in all regressions. Standard errors (in parentheses) are clustered at the county-year level, with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

| Panel C: Pre-Boom Bank Characteristics for Treated (Public) and Controls (Private) | | |
|---|---|---|
| Difference of Means (t-stat) | Quintile Matching | All |
| | (1) | (2) |
| Total Assets$_{1999-2002}$, log ($1,000s) | 0.036** | 0.872*** |
| | (2.380) | (20.667) |
| Diversification$_{1999-2002}$ | 0.000 | 0.005*** |
| | (0.044) | (2.582) |
| Deposits to Assets$_{1999-2002}$ | 0.019*** | -0.017*** |
| | (5.301) | (-3.642) |
| Tier 1 Capital$_{1999-2002}$ | -0.002*** | -0.003*** |
| | (-3.072) | (-4.735) |
| Loans to Assets$_{1999-2002}$ | 0.012*** | 0.027*** |
| | (2.925) | (5.016) |
| ROE$_{1999-2002}$ | 0.000 | 0.003*** |
| | (0.120) | (4.102) |
| $\Delta$ Originations$_{1999-2002}$ | -0.035 | -0.049* |
| | (-1.428) | (-1.669) |
| Number of Obs. | 1,043 | 1,043 |
| Panel D: Probability of Treatment (Public) | | |
| | pre-event size & HHI | pre-event all |
| | (1) | (2) |
| Total Assets$_{1999-2002}$ | 0.167*** | 0.172*** |
| | (0.009) | (0.009) |
| Diversification$_{1999-2002}$ | 0.015 | -0.020 |
| | (0.201) | (0.201) |
| Deposits to Assets$_{1999-2002}$ | | 0.230*** |
| | | (0.082) |
| Tier 1 Capital$_{1999-2002}$ | | 0.188 |
| | | (0.466) |
| Loans to Assets$_{1999-2002}$ | | 0.112* |
| | | (0.067) |
| ROE$_{1999-2002}$ | | 0.556 |
| | | (0.502) |
| Year & County Effects | No | No |
| Number of Obs. | 3,202 | 3,202 |
| Adj-$R^2$ | 0.097 | 0.100 |

Table A.4: 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 lenders' 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 lender-county level (Column 1), followed by lender-MSA (Column 2) lender-state (Column 3), and lender-census division (Column 4), with ***, **, and * denoting significance at the 1%, 5%, and 10% level, respectively.

| Panel A: Analysis of Originations by Lenders' Ownership Structure | | | | |
|---|---|---|---|---|
| | Δ Log Originations | | | |
| | (1) | (2) | (3) | (4) |
| Boom*Public Lender | 0.088*** | 0.088*** | 0.088*** | 0.088*** |
| | (0.014) | (0.021) | (0.023) | (0.027) |
| Clustering | Lender-County | Lender-MSA | Lender-State | Lender-Division |
| Panel B: Analysis of Standards by Lenders' Ownership Structure | | | | |
| | Rejection Rate | | | |
| | (1) | (2) | (3) | (4) |
| Boom*Public Lender | -0.025*** | -0.025*** | -0.025*** | -0.025*** |
| | (0.002) | (0.004) | (0.006) | (0.007) |
| Clustering | Lender-County | Lender-MSA | Lender-State | Lender-Division |
| Panel C: Analysis of Heterogeneity of Originations by Public Lenders in the Boom – CEO Short-Term Focus | | | | |
| | Δ Log Originations | | | |
| | (1) | (2) | (3) | (4) |
| Lender ST | 0.110*** | 0.110*** | 0.110*** | 0.110*** |
| | (0.014) | (0.023) | (0.031) | (0.039) |
| Clustering | Lender-County | Lender-MSA | Lender-State | Lender-Division |
| Panel D: Analysis of Heterogeneity of Standards by Public Lenders in the Boom – CEO Short-Term Focus | | | | |
| | Rejection Rate | | | |
| | (1) | (2) | (3) | (4) |
| Lender ST | -0.021*** | -0.021*** | -0.021** | -0.021** |
| | (0.004) | (0.005) | (0.009) | (0.010) |
| Clustering | Lender-County | Lender-MSA | Lender-State | Lender-Division |

Table A.5: Additional Analysis of Mortgage Performance After the Boom

This table summarizes additional analysis of mortgage performance after the boom (2007-2010) as measured by 90+ day delinquencies and foreclosures for the subsample of retained mortgages. 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 Lender*, which is an indicator variable that takes a value of one for lenders whose top-holders is publicly-traded and zero otherwise. Panel B reports results for public lenders' CEO short-term focus, which is measured as the frequency of CEO words related to short-term horizon in the transcripts of the lender's earnings conference calls and in the MD&A section of the lender'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 Lenders' Ownership Structure in the Sub-sample of Retained Mortages | | | | |
|---|---|---|---|---|
| | 90+ Day Mortgage Delinquencies | | Mortagage Foreclosures | |
| | (1) | (2) | (3) | (4) |
| Public Lender | 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 |
| Panel B: Analysis of Public Lenders' Short-Term Focus in the Sub-sample of Retained Mortages | | | | |
| | 90+ Day Mortgage Delinquencies | | Mortagage Foreclosures | |
| | (1) | (2) | (3) | (4) |
| Lender ST | 0.029** | 0.031** | 0.051*** | 0.051*** |
| | (0.012) | (0.013) | (0.019) | (0.019) |
| Loan Controls | No | Yes | No | Yes |
| Year-County FE | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes |
| Obs. | 53,120 | 53,120 | 53,120 | 53,120 |

Table A.6: Additional Analysis of Aggregate and Real Effects

This table reports additional results for the cross-sectional county-level regressions of aggregate and real outcomes. Panel A reports coefficient estimates from Table 7 (Panel A) for the full set of standard county-level covariates that include median FICO score, subprime share, delinquencies, median income, wage income, population, demographic characteristics such as age (%65+) and race (%black), homeownership. Panels B-C report results of robustness analysis to address geographic spillovers. Specifically, we repeat the Bartik analysis using the geographic instrument of Table 9 with two modifications: first, we construct the lender-specific shocks as the lender-year specific average logarithmic excess annual change in the dollar volume of mortgage originations in all other counties excluding the own county, which is obtained after de-meaning originations by each lender in any given county-year of their county-year specific mean to address local shocks in other counties that may be correlated with own county shocks (Panel B); second, we construct the lender-specific shocks as the lender-year specific average logarithmic annual change in the dollar volume of mortgage originations in all other counties excluding all counties in own state, to control for correlated local shocks between counties within state (Panel C). 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 pupulation 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: Coefficient Estimates for Additional Covariates | | | | |
|---|---|---|---|---|
| | House Prices | Employment | Durable Consumption | Retail Sales |
| Mkt. share of public lenders, Pre-Boom | -0.264*** | -0.052*** | -0.208*** | -0.050*** |
| | (0.035) | (0.008) | (0.033) | (0.010) |
| Subprime credit share, Pre-Boom | 0.222 | -0.087 | 0.291 | 0.044 |
| | (0.178) | (0.063) | (0.240) | (0.066) |
| Share of National Banks, Pre-Boom | -0.051 | 0.033 | -0.217 | 0.093 |
| | (0.227) | (0.051) | (0.291) | (0.066) |
| Median FICO, Pre-Boom | -0.073 | -0.029 | -0.058*** | -0.007 |
| | (0.047) | (0.018) | (0.021) | (0.009) |
| Log Median Income, Pre-Boom | -0.249** | -0.157*** | -0.482*** | -0.091*** |
| | (0.106) | (0.031) | (0.072) | (0.028) |
| Log Median Wages, Pre-Boom | 0.419*** | 0.171*** | 0.627*** | 0.106*** |
| | (0.105) | (0.030) | (0.076) | (0.028) |
| Log Population, Pre-Boom | -0.202*** | -0.011 | -0.151*** | -0.015* |
| | (0.028) | (0.008) | (0.021) | (0.008) |
| +65 Population Share, Pre-Boom | 0.080 | 0.305*** | 0.132*** | 0.053 |
| | (0.221) | (0.065) | (0.017) | (0.060) |
| Full County Controls | Yes | Yes | Yes | Yes |
| Obs. | 769 | 779 | 769 | 781 |
| $R^2$ | 0.372 | 0.172 | 0.330 | 0.101 |

Table A.6: Additional Analysis of Aggregate and Real Effects (Continued)

This table reports additional results for the cross-sectional county-level regressions of aggregate and real outcomes. Panel A reports coefficient estimates from Table 7 (Panel A) for the full set of standard county-level covariates that include median FICO score, subprime share, delinquencies, median income, wage income, population, demographic characteristics such as age (%65+) and race (%black), homeownership. Panels B-C report results of robustness analysis to address geographic spillovers. Specifically, we repeat the Bartik analysis using the geographic instrument of Table 9 with two modifications: first, we construct the lender-specific shocks as the lender-year specific average logarithmic excess annual change in the dollar volume of mortgage originations in all other counties excluding the own county, which is obtained after de-meaning originations by each lender in any given county-year of their county-year specific mean to address local shocks in other counties that may be correlated with own county shocks (Panel B); second, we construct the lender-specific shocks as the lender-year specific average logarithmic annual change in the dollar volume of mortgage originations in all other counties excluding all counties in own state, to control for correlated local shocks between counties within state (Panel C). 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 pupulation 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.1: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders, Geographic Instrument | | | | |
|---|---|---|---|---|
| | Change in House Prices | Change in Employment | Change in Durable Consumption | Change in Retail Sales |
| | (1) | (2) | (3) | (4) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$ | 0.442*** | 0.115*** | 0.217*** | 0.132*** |
| | (0.065) | (0.018) | (0.056) | (0.022) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{\text{Private},t=2007-2010}$ | 0.038 | -0.030 | 0.137 | -0.016 |
| | (0.075) | (0.027) | (0.165) | (0.029) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 769 | 779 | 769 | 781 |
| Panel B.2: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders 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) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$ | 1.884*** | 0.163*** | 0.640*** | 0.252*** |
| | (0.337) | (0.056) | (0.220) | (0.087) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$ | 0.230 | 0.046 | -0.068 | -0.049 |
| | (0.299) | (0.104) | (0.276) | (0.087) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 769 | 779 | 769 | 781 |

Table A.6: Additional Analysis of Aggregate and Real Effects (Continued)

This table reports additional results for the cross-sectional county-level regressions of aggregate and real outcomes. Panel A reports coefficient estimates from Table 7 (Panel A) for the full set of standard county-level covariates that include median FICO score, subprime share, delinquencies, median income, wage income, population, demographic characteristics such as age (%65+) and race (%black), homeownership. Panels B-C report results of robustness analysis to address geographic spillovers. Specifically, we repeat the Bartik analysis using the geographic instrument of Table 9 with two modifications: first, we construct the lender-specific shocks as the lender-year specific average logarithmic excess annual change in the dollar volume of mortgage originations in all other counties excluding the own county, which is obtained after de-meaning originations by each lender in any given county-year of their county-year specific mean to address local shocks in other counties that may be correlated with own county shocks (Panel B); second, we construct the lender-specific shocks as the lender-year specific average logarithmic annual change in the dollar volume of mortgage originations in all other counties excluding all counties in own state, to control for correlated local shocks between counties within state (Panel C). 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 pupulation 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.1: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders, Geographic Instrument | | | | |
|---|---|---|---|---|
| | Change in House Prices | Change in Employment | Change in Durable Consumption | Change in Retail Sales |
| | (1) | (2) | (3) | (4) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Public,t=2007-2010}$ | 0.338*** | 0.084*** | 0.175*** | 0.090*** |
| | (0.040) | (0.010) | (0.031) | (0.013) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{\text{Pr}\,ivate,t=2007-2010}$ | -0.010 | -0.028 | 0.102 | -0.012 |
| | (0.062) | (0.021) | (0.152) | (0.023) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 769 | 779 | 769 | 781 |
| Panel C.2: $\Delta \text{Bust}_{2007-2010}$ by Exposure to Local Public Lenders 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) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{ST\ Public,t=2007-2010}$ | 1.611*** | 0.169*** | 0.560*** | 0.213*** |
| | (0.246) | (0.048) | (0.180) | (0.067) |
| $\Delta \widehat{Log\ Orig}\ (\$)_{Other\ Public,t=2007-2010}$ | 0.176 | 0.104 | 0.323 | 0.013 |
| | (0.249) | (0.102) | (0.280) | (0.083) |
| County Controls | Yes | Yes | Yes | Yes |
| Obs. | 769 | 779 | 769 | 781 |