How Does the Equity Market Stabilize the Economy via Bank Lending? A Randomized Regulatory Experiment

Abstract

This paper provides microlevel evidence on how the enhanced monitoring of the equity market dampens economic cycles by suppressing banks' supply of household credit. Household credit expansion has been documented as the main driver of economic cycles. I exploit a regulatory experiment that randomly selected a group of listed pilot firms and removed their short-selling restrictions from 2004 to 2007. Some of these pilot firms are banks involved in residential mortgage origination. Using a difference-in-differences specification at the bank-county-year level, I find that the growth rate of portfolio mortgage origination is 25 to 54 percentage points lower for the treated banks. Textual analyses show declines in treated banks' short-termism and increases in their attention to mortgage risks. The effect on mortgage origination was stronger for banks with higher short-termism and lower attention to mortgage risks before the treatment period and was not driven by changes in bank financial characteristics or market attention. I do not find a similar pattern in business credit or securitized mortgages. Counties with higher exposures to the experiment experienced less deterioration in housing markets, unemployment rates, and per capita income during the Great Recession. The findings shed light on the role of the equity market in the stability of the real economy.

Keywords: Banking, Equity Market, Economic Cycles, Mortgages, Short Selling, Housing Markets

JEL Classifications: G20, G21.

1 Introduction

Economic cycles worldwide over the past 40 years have been characterized by expansions of household credit followed by recessions (Mian and Sufi 2018). The extant literature has documented the association between household credit supply and fluctuations in real economic activities.[1] Specifically, the real economic expansions were generated by household credit supply shocks rather than technology or permanent income shocks, and such excess expansions resulted in the contraction of real economic activities (Mian and Sufi 2018). While policy discussions regarding household credit expansions have involved macroprudential regulations and countercyclical monetary policies, the role of the financial market *per se* has rarely been investigated.

This paper attempts to shed light on this issue using a randomized regulatory experiment and microlevel data on residential mortgage lending. The regulation SHO (Reg SHO) program lifted the short-selling restriction on 1000 randomly selected stocks (including listed banks). I argue that the enhanced monitoring due to the lifting of short-selling restrictions discouraged these pilot banks from aggressively expanding their household credit supply and hence alleviated the boom and bust cycle. Eliminating the effects of local demand changes, I find that pilot banks increased the origination of residential mortgages more slowly than the control group during the boom period; this effect is only observed for retained mortgages and is absent for securitized mortgages. The effect was more pronounced for banks with more CEO short-termism. More importantly, counties with higher market shares of the pilot banks experienced less severe economic downturns during the Great Recession. Overall, improvement in the equity market monitoring helped mitigate the economic cycle by curbing aggressive household credit supply.

Price has conventionally been considered a source of information (Hayek 1945), and its information production is particularly important in financial markets. In an ideal market without trading frictions, investors with heterogeneous information and beliefs trade shares and hence price their information into the stock market. Therefore, share prices reflect firms'

[1] See for instance, Borio (2014), Drehmann, Juselius, and Korinek (2017), Jordà, Schularick, and Taylor (2015), Krishnamurthy and Muir (2017), López-Salido, Stein, and Zakrajšek (2017), Mian, Sufi, and Verner (2017a). Particularly, Bahadir and Gumus (2016) and Mian, Sufi, and Verner (2017b) point out that the credit expansion associated with economic cycles is in the household credit market rather than the business credit market.

fundamental information and have monitoring effects on firms: Investors sell shares in response to negative information about firms, and the resulting decline in share prices may reduce firms' market value; this threat discourages firms from making decisions that may generate negative information and reduce firms' market value (Bond, Edmans, and Goldstein 2012). In reality, however, market frictions inhibit these monitoring effects. For example, short-selling restrictions prevent investors from shorting stocks at low prices. Thus, their negative information is less likely to be reflected in share prices, and the monitoring effects are decreased.

The financial sector has been criticized for providing excessive credit supply to the residential mortgage market during the housing boom and thus contributing to the subprime crisis. In particular, banks *that originated residential mortgages* have been considered among the culprits of that crisis, whose aggressive risk-taking in mortgage markets caused substantial losses to themselves as well as to other financial institutions.[2] If the stock market could efficiently price the banks' aggressive risk-taking before the crisis, namely, the banks' stock prices could decline in response to the aggressive risk-taking, bank executives would refrain from such risk-taking since their compensation which is tied to share prices would decrease (Bond, Edmans, and Goldstein 2012).[3] However, the stock market frictions, particularly the short-selling restriction which retards pessimistic investors' short selling, inhibit the stock market's negative response to the banks' risk-taking and hence reduce the monitoring effects. Therefore, I hypothesize that when stock market frictions are reduced, the consequent enhanced monitoring effects would impose a stronger restriction on banks' aggressive risk-taking in the mortgage market during a boom period, which would attenuate the boom and bust cycle.

I exploit the Reg SHO Pilot program to test this hypothesis. The program removed the short-selling restriction for a group of randomly selected firms from the Russell 3000 Index and has been shown by the extant literature to reduce trading friction and increase market quality (Alexander and Peterson 2008; Diether, Lee, and Werner 2009) and monitoring

[2] Falato, Favara, and Scharfstein (2018) demonstrate that the short-termism of bank management made banks orgniate more mortgages and drove the credit boom. Baron and Xiong (2017) and Fahlenbrach, Prilmeier, and Stulz (2017) show that banks were overoptimistic about the risk of the loans and extended too much credit during the boom period.

[3] For example, I obtain information on stock options from Execucomp and find that all firm-year observations in my sample granted opttions to executives, and 99.5% of them had unexercised exercisable options.

(Fang, Huang, and Karpoff 2016).[4] I consider this pilot program as a plausibly exogenous decrease in trading friction for banks in pilot firms.[5]

Using a difference-in-difference specification, I find that the treated (pilot) banks increased the origination of portfolio mortgages (which remain on the banks' balance sheets) more slowly during the treatment period compared to the control group. The magnitude of the effect is substantial. The growth rate of origination was lower by 24.7~53.8 percentage points for treated banks. However, securitization seemed to mitigate this effect. Some mortgages were securitized (sold) by the banks, and the credit risk was transferred to the buyers. There is little difference in the growth rates of these securitized mortgages between the treated and the control banks. The research design accounts for changes in local demand by county-year fixed effects and for lender heterogeneities by lender fixed effects. The assumption of parallel pretreatment trends of the treated and the control banks is also verified by a dynamic test. The effects of the pilot program are robust when lender financials are controlled, eliminating the concern that the decline in credit supply is due to changes in financial characteristics such as bank size and capital ratio.

I provide further supporting evidence for the channel. First, short interest increased more during the treatement period for the pilot banks than the control banks, suggesting increases in short sellers' monitoring. Second, textual analyses shows that there were more declines in short-termism and more increases in the attention to mortgage risks during the treatment period for the pilot banks. Moreover, the effects on mortgage origination are more pronounced for lenders with higher CEO short-termism and less attention to mortgage risks before the treatment period, and this pattern is robust after I control for the interaction term involving capital ratio. Lastly, the market attention did not change for both the pilot and the control banks, which rules out the explanation of market attention.

Besides, I do not observe similar patterns in commercial loans, industrial loans, and agricultural loans. The pilot banks experienced fewer losses on mortgage loans during the

[4] Diether, Lee, and Werner (2009) focus on NYSE and Nasdaq markets and show that the Reg SHO Pilot program increased the market quality in both markets. Alexander and Peterson (2008) demonstrate that price discovery associated with short sell orders was improved during the program. Fang, Huang, and Karpoff (2016) show that during the pilot gram, short interest increased in months in which firms were later revealed to have engaged in financial misrepresentation, and pilot firms were more likely to get caught for financial fraud than control firms.

[5] It is possible that the majority of the investors were also overoptimistic. My theory relies on the argument that the removal of short sale restrictions enabled more short sellers who were more rational and expected the cycles to participate the market and incorporate in their information into the stock prices.

subprime crisis. The lenders did not manage their exposure to mortgage risk using loan-to-income ratio which is more difficult to measure than origination volume.

More importantly, I document the effects of the treatment on the economic cycle. The Reg SHO program limited banks' risk-taking in the boom period and thus is expected to have alleviated the economic downturn when the crisis broke out. I find that counties with higher market shares of the treated banks prior to the treatment period experienced smaller decreases in the growth rates of housing prices and per capita income and smaller increases in unemployment rates during the Great Recession.

This paper contributes to the literature in several aspects. First, it contributes to a broad literature on the role of financial markets in the real economy, a critical fundamental question in economics. For example, researchers have discussed how finance promotes economic growth (Levine and Zervos 1998), entrepreneurship (Guiso, Sapienza, and Zingales 2004), innovation (Cornaggia et al. 2015), better education (Flug, Spilimbergo, and Wachtenheim 1998), and even inhibits crime (Garmaise and Moskowitz 2006) and inequality (Beck, Demirgüç-Kunt, and Levine 2007). This paper contributes to the literature by showing empirical evidence that the price informativeness of the equity market helps to maintain the stability of the economy.

Second, this paper contributes to the literature on household credit and economic cycles. Numerous studies have documented international evidence that increases in household debt predict economic recession following an expansion (Borio 2014; Drehmann, Juselius, and Korinek 2017; Krishnamurthy and Muir 2017; López-Salido, Stein, and Zakrajšek 2017; Mian, Sufi, and Verner 2017a). In particular, Bahadir and Gumus (2016) and Mian, Sufi, and Verner (2017b) highlight that economic cycles are not due to business credit expansion, which does not boost the real exchange rate, prices for nontradable goods, or employment in the nontradable sector. Di Maggio and Kermani (2017) and Mian, Sufi, and Verner (2017b) provide evidence at a more granular level. As the most recent instance of recession, the subprime crisis has also been studied extensively (Adelino, Gerardi, and Willen 2013; Agarwal et al. 2011; Demyanyk and Loutskina 2016; Griffin and Maturana 2016; Keys et al. 2010; Kruger 2018; Piskorski, Seru, and Vig 2010). The literature discusses potential solutions such as macroprudential policy, risk retention, and more flexible contracts (e.g., Akinci and Olmstead-Rumsey 2018; Cerutti, Claessens, and Laeven 2017; DeFusco, Johnson, and Mondragon 2017; Mian and Sufi 2017, 2018; Shiller et al. 2013) but rarely discusses how the financial market itself can help to mitigate credit and economic cycles. This paper

shows that an equity market with less trading friction helps to restrict aggressive household credit expansion and alleviate the severity of the recession.

Besides, within the literature on household credit mentioned above, several studies have discussed the role of securitization in the run-up to the subprime crisis (Adelino, Gerardi, and Willen 2013; Agarwal et al. 2011; Demyanyk and Loutskina 2016; Griffin and Maturana 2016; Keys et al. 2010; Kruger 2018; Piskorski, Seru, and Vig 2010). This paper also contributes to this literature, as it implies that the disciplinary effects of the equity market are weakened by the risk-transfer of securitization.

This paper also makes an incremental contribution to the literature on the effects of short-selling. Researchers have explored how short-selling pressure affects stock price and liquidity, investors' behavior, M&A, managerial compensation, financial misconduct, disclosure, credit rating, cost of debt, and earnings management (Bai et al. 2018; Beber and Pagano 2013; Billett, Liu, and Tian 2018; Billingsley and Kovacs 2011; Boehmer, Jones, and Zhang 2013; Boehmer and Wu 2012; Chang, Cheng, and Yu 2007; Chang, Lin, and Ma 2018; De Angelis, Grullon, and Michenaud 2017; Fang, Huang, and Karpoff 2016; Frino, Lecce, and Lepone 2011; Grullon, Michenaud, and Weston 2015; Karpoff and Lou 2010; Li and Zhang 2015; Lin, Liu, and Sun 2019; Massa, Zhang, and Zhang 2015). My paper contributes to this literature by identifying household credit supply as an alternative channel through which short-selling pressure in the stock market affects the real economy. More importantly, I explicitly test the effects of short selling on economic cycles, a topic scarcely addressed in the literature.

Last but not least, this paper is associated with a few parallel studies on the relationship between access to public capital and mortgage origination (Chu and Zhao 2018; Falato, Favara, and Scharfstein 2018). These studies show that access to public capital *increases* mortgage origination due to either the short-termism of the managers or a lowered cost of raising external capital. In contrast, my study shows that the monitoring of the capital market decreases mortgage origination conditional on publicly traded banks.

The remainder of this paper is organized as follows. Section 2 describes the data and methodology. The empirical results are discussed in Section 3. I conclude in Section 4.

2 Methodology, Data, and Sample Construction

2.1 Methodology

To provide a causality explanation, I exploit an experiment implemented by the U.S. Securities and Exchange Commission (SEC), the Reg SHO Pilot Program, from 2004 to 2007. The SEC randomly selected one-third of the firms in the member list of 2004 Russell 3000 Index and removed the short-sale restrictions (called the "uptick rule") for the pilot firms.[6,7] The first pilot order was issued in July 2004.[8] The experiment has been shown to be effective in reducing trading friction and improving stock market quality (Alexander and Peterson 2008; Diether, Lee, and Werner 2009). Therefore, I consider this pilot program a plausibly exogenous decrease in market frictions for the stocks of Post × banks and conjecture that the pilot program prevented Post × banks from aggressively taking risks in the mortgage markets during the housing boom period. Consequently, the communities served by those banks should have suffered less during the subsequent bust of the Great Recession. Details on regression models are introduced in corresponding subsections of Section 3.

2.2 Data Sources

I obtain information on mortgage origination from the loans reported to regulators under the Home Mortgage Disclosure Act (HMDA), which covers approximately 90% of residential mortgages in the U.S. This data source provides information regarding each mortgage application, including the calendar year of the loan application, the originator of the mortgage, the county where the loan was originated, the purpose of the loans (home purchase or refinancing), loan amount, property type, owner-occupancy status, application outcome (accepted or not), whether the loan was sold in the year of origination, and who (e.g., Fannie, Freddie, or private securitizers) purchased the loan if the loan was sold.[9] Consistent with

[6] The SEC sorted the 2004 Russell 3000 Index firms by average daily dollar volume from June 2003 through May 2004 for each exchange separately, and it selected every third company starting with the second within each exchange.

[7] The uptick rule is a trading restriction that short selling is only allowed on an uptick. The short must be either at a price above the last traded price of the security or at the last traded price when the most recent movement between traded prices was upward.

[8] See Securities Exchange Act Release No. 50104. In an order issued on 28 July 2004, the program was scheduled to commence in January 2005. On 29 November 2004, the SEC reset the Pilot to commence on 2 May 2005 and end on 28 April 2006. On 20 April 2006, the SEC extended the program to 6 August 2007.

[9] Most of the loans were sold to securitizers for securitization. Some loans may not be securitized. The point here is that the risk is transferred along with the sale. Therefore, I use the concepts of "selling loans" and "securitizing loans" interchangeably hereafter.

the literature, I focus on conventional owner-occupied home purchase loans in urban counties (Chu and Zhao 2018; Favara and Imbs 2015).[10]

I collect several additional datasets to complement the mortgage data. I obtain the annual housing price index at the county level from the Federal Housing Finance Authority (FHFA). Information on county-year unemployment rates and per capita income come from the Bureau of Labor Statistics and Bureau of Economic Analysis, respectively. The financial data of listed firms and banks come from Compustat and Call Reports, respectively. Small business lending data at the bank-county-year level come from the Federal Financial Institutions Examination Council (FFIEC) under the Community Reinvestment Act (CRA).

2.3 Sample Construction

Starting with the HMDA data described in Section 2.1, I consider only lenders originating mortgages throughout the period from 2001 to 2007 to obtain a balanced sample. I then map HMDA lenders to the constituents of the Russell 3000 Index and the pilot firms in the Regulation SHO program.[11][12] Following the literature, I remove stocks that were not listed in NYSE, Amex, or NASDAQ-NM and stocks that went public or had spin-offs after April 30, 2004, because these firms were not eligible for the pilot program (Fang, Huang, and Karpoff 2016).

The final sample includes 47 treated banks and 109 control banks, which were the subsidiaries of 35 treated firms and 76 control firms.[13] I compare the treatment and control groups to verify that the assignment in Reg SHO is random. Table 1 reports the t-test results of the differences in the means of several key financial characteristics of the parent firms (i.e., the listed firms) and the banks. The financial data are for 2003, one year before Reg SHO was announced. The p-values indicate that the treatment and control groups are

[10] In some cases an originator may also be the sponsor of a securitization. At the time of mortgage-backed security (MBS) issuance, the sponsor may hold the equity tranche to signal the quality of the MBS. However, before the Dodd-Frank Act of 2010, the sponsor could easily offload the risks by selling or hedging the equity tranche (Begley and Purnanandam 2016; Fender and Mitchell 2009).

[11] I restrict the sample to firms with size in 2003 and number of subsidiaries originating mortgages in 2003 within the common support of the distributions of the treated and control firms, accounting for 91% of the original sample. The results are robust using the full sample.

[12] Following the literature, I remove stocks that were not listed in NYSE, Amex, or NASDAQ-NM and stocks that went public or had spin-offs after April 30, 2004, because these firms were not eligible for the pilot program (Fang, Huang, and Karpoff 2016).

[13] No bank changed its treatment status during the sample period.

statistically indistinguishable. I then aggregate the mortgage origination in HMDA to construct bank-county-year level information regarding the volume and number of mortgage originations of these banks.

In addition, I use a loan-level HMDA sample to analyze the risk of each loan in Section 3.4. To examine the real economy, I construct a county-level sample on housing price, unemployment rates, and per capita income, which is introduced in detail in Section 3.5.

Table 2 presents the summary statistics of the bank-county-year level HMDA sample. Each lender on average originates approximately eighteen mortgages, with a total value of 2,871,020 dollars in each county every year. The summary statistics of the other three samples, the loan-level HMDA sample, the loan performance sample, and the county-level sample are shown in the following sections along with the regression results.

[Insert Table 1 about here]

[Insert Table 2 about here]

3 Empirical Analysis

This section discusses four sets of empirical results. First, I investigate the changes in banks' mortgage origination in the pilot program. Second, I provide supporting evidence regarding the channel of monitoring. I then investigate how the bank losses, loan-to-income ratio of originated mortgage, and corporate loans were affected by the pilot program. Finally, I demonstrate the program's effects on the recent economic cycle.

3.1 Loan Originations

3.1.1. The Baseline Model

First, I investigate how Reg SHO affects mortgage origination. The monitoring effects of the stock market are expected to reduce aggressive risk-taking, as discussed in Section 1. Therefore, I expect Reg SHO to reduce banks' origination in the mortgage markets in which banks are criticized for originating too many risky mortgages.

I exploit the following difference-in-difference (DID) regression. The treated banks and control banks are defined in Section 2.3. The baseline regression model is specified as follows:

$$\Delta Y_{i,c,t} = \alpha_0 + \alpha_1 \times Treated_{i,c,t} \times Post_{i,c,t} + FEs + \varepsilon_{i,c,t} \quad (1)$$

The subscripts i, c, and t refer to banks, counties, and years, respectively. The dependent variable $\Delta Y_{i,c,t}$ is the difference in the natural logarithm of the bank-county-level aggregate mortgage origination between year t and *t-1* (Favara and Imbs 2015). I consider both the number and volume of origination. *Treated* is a dummy variable that assumes the value of one for treated lenders and zero otherwise. *Post* is a dummy variable that assumes the value of one if year t is equal to or later than 2004.[14]

FEs include the county-year fixed effects and lender fixed effects. County-year fixed effects control for county-specific time trends. The fixed effects eliminate the effects of time-varying county characteristics on mortgage lending and allow me to consider within-county variation. Particularly, they isolate the impacts of mortgage *demand* on my results, and the identification focuses on the changes in mortgage *supply*. *FEs* act as the treatment group dummy and treatment period dummy in a standard difference-in-differences regression, allowing heterogeneities in county characteristics across the years. Therefore, *Treated* × *Post* and *FEs* constitute a difference-in-differences specification, and the coefficient of *Treated* × *Post* is the main coefficient of interest. Standard errors are clustered at the county level.

I focus on the origination of portfolio loans. Portfolio loans refer to the mortgages remaining on the balance sheets of lenders, and lenders bear the risks of portfolio loans. Securitized loans are mortgages originated and *sold* by the lenders. The risks of securitized loans are transferred to purchasers and have little effect on the originators' risk (Duchin and Sosyura 2014). Therefore, I expect that Reg SHO affected mainly the origination of portfolio loans. I also test securitized loans in the next section.

[14] The program and the list of pilot firms were announced in 2004, and the program was schedueld to start in January 2005. (The program was postponed to May 2005 in an order issued in December 2004.) The majority of the mortgages did not become delinquent immediately after origination. Therefore, even after the year 2004, banks still bore the risk of the mortgages originated in 2004. This risk-taking could be more efficiently reflected by the stock price in 2005 due to the program, and the treated banks have an incentive to reduce mortgage origination ahead of time (in 2004) to avoid a potential negative share price response in January 2005 when the program was expected to started. The dynamic test described in Section 3.1.2 confirms this argument. I also conduct a robustness test excluding the observations in 2004 in Section 3.1.2.

3.1.2. Estimates

Table 3 reports the results. Column 1 of Panel A displays the results for portfolio loans. The estimates indicate that the treated banks expand mortgage origination more slowly than the control banks. The coefficient is not economically significant. The treated banks increase mortgage originations more slowly than the control banks by 53.8%. In contrast, the origination of securitized loans (Column 2 of Panel A) changes little, which is in line with the risk-taking and risk-transfer explanation.

In Panel B, I focus on portfolio loans and conduct several robustness tests. In Column 1, I replace the dependent variable $\Delta Y_{i,c,t}$ with the difference in the natural logarithm of the number of originated mortgages and obtain consistent results. In Column 2, I change the dependent variable back to the growth rate of origination volume and drop observations in 2004 (the announcement year of the pilot program). The estimates are similar to those in Column 1 of Panel A. I then consider the time-varying shocks correlated with lender size. I reweight the sample to make the size distributions of treated and control banks within every year as close as possible to the size distribution of the control banks before the program (in 2003) using the method of DiNardo, Fortin, and Lemieux (1996).[15] Then, I repeat the baseline DID using the reweighted sample, and the results indicate that the treated banks increase originations less than the control banks (Column 2 of Panel B), similar to the results in Panel A. I also replace the *Post* dummy by a year categorical variable to conduct a dynamic test (Column 3). The coefficients of the interaction terms involving pretreatment years are all insignificant, confirming the parallel pretreatment assumption. Besides, the statistically significant coefficient of the interaction of the year 2004 also supports the definition of the treatment period in Section 3.1.1. The Reg SHO starts to affect origination in 2004 rather than 2005.

[Insert Table 3 about here]

[15] The reweighting process is similar to Falato, Favara, and Scharfstein (2018). I assign each lender in the restricted sample to one of the 10 bins based on the decile of lender assets of the control banks in 2003 and local market concentration, measured by the Herfindahl-Hirschman Index (HHI), and two bins for the national banks status. I use the algorithm by DiNardo, Fortin, and Lemieux (1996) to adjust the weight of each bin within the subsamples of the treated or control banks for every year of our sample except the control banks in 2003 to make the distributions of the bins of the thirteen subsamples as close as possible to that of the control banks in 2003. In other words, the reweighting makes the size distributions of the treatment and control groups as close as possible to each other in every year.

3.1.3. Lender Financials

This section considers lender financial characteristics. I add all of the time-varying characteristics of firms and banks to the baseline specification (1). These financial characteristics are listed in Table 2. Panel A of Table 4 displays the summary statistics, and Panel B displays the estimates. Conditional on financial characteristics, the treated banks increased mortgage origination more slowly, consistent with the finding in the baseline specification in Column 1 of Table 3. The results suggest that the finding in the baseline analysis is not driven by changes in banks' financial characteristics.

[Insert Table 4 about here]

3.2 The Channel

In this section, I provide further evidence to validate the monitoring channel. I document the increases in short sellers' monitoring as well as the subsequent declines in short-termism and increases in banks' attention to mortgage risks. More importantly, I show that banks with higher short-termism and lower attention to mortgage risk before the treatment period were affected more by the pilot program. I also rule out the alternative explanation of market attention.

3.2.1. Increases in Monitoring: Short Interest

One assumption in my hypothesis is that the pilot program enabled investors to short sell more freely than before. In this section, I provide evidence that short-selling activities increased during the pilot program. I apply the following regression to a bank holding company (BHC) – year sample from 2001 to 2007:

$$SI_{i,t} = \alpha_0 + \alpha_1 \times Treated_{i,t} \times Pilot_t + \Gamma X_{i,t} + FEs + \varepsilon_{i,c,t} \quad (2)$$

The BHCs are the parent firms of the banks in my sample. The subscripts i and t refer to BHCs and years, respectively. The dependent variable $SI_{i,j}$ is average scaled monthly short

interest for the stock of BHC i in year t. I scale the monthly short interest by the outstanding shares of the BHC at the end of the month. Then I average the scaled short interest through the year. $Pilot_t$ equals one if year t is between 2005 and 2007 and zero otherwise.[16] *Treated* is a dummy variable that assumes the value of one for the BHCs of the pilot banks and zero otherwise. X includes all the parent firm characteristics introduced in Section 3.1.3. for BHC i in year t. *FEs* include the BHC and year fixed effects. Table 5 displays the estimates. The results in Column 1 suggest that the scaled short interest of the pilot BHCs increased by 21.4% (0.006/0.028) compared with the control BHCs. In Column 2, I drop observations in 2004 and the results change little. Therefore, the pilot banks were under more intense monitoring from short sellers.

[Insert Table 5 about here]

3.2.2. Declines in Short-Termism

One explanation for banks' aggressive mortgage lending is short-termism (Kolasinski and Yang 2018). CEOs pay more attention to firm short-term earnings to prop up current share prices and get more compensation, but they ignore the consequences in the long run. Short selling pushes current share prices downward and affects executives' compensation, and hence and mitigates the myopic risk-taking. Therefore, if the channel of short-termism exists, Reg SHO would have reduced banks' short-termism.

I construct two measures of CEO short-termism. The first one is *CEO share turnover* which equals 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 year (Falato, Favara, and Scharfstein 2018). Second, I construct a direct measure of short-termism based on textual analyses of 10-K filings following Brochet, Loumioti, and Serafeim (2015). Specifically, I count the number of short-term oriented and long-term oriented keywords respectively in "Item 7. Management's Discussion and Analysis of Financial Condition and Results of Operations" and "Item 7A. Quantitative and Qualitative Disclosure about Market Risk" from the 10-K filings. The short-term and long-term oriented keywords are defined by Brochet,

[16] In Section 3.1.1, I aruge that banks might respond before the program started. However, short sellers could only increased their short selling after the restriction was actually lift.

Loumioti, and Serafeim (2015).[17] I calculate the variable *short ratio* as the number of short-term oriented keywords divided by the sum of the number of long-term oriented keywords and one.[18] *Short ratio* is defined as the firm-year level.

I exploit a similar regression to the one in Section 3.1.3 and replace the dependent variable with the two measures of short-termism respectively. Panel A of Table 6 shows the summary statistics, and Columns (1) and (2) of Panel B in Table 6 displays the results. The negative coefficients of *Treated* × *Post* (-0.226, t = -6.94 in Column (1); -0.033, t = -4.98 in Column(2)) indicate that the short-termism of the pilot banks declined during the treatment period.

[Insert Table 6 about here]

3.2.3. Increase in the Attention to Mortage Risk

My argument on the channel indicates that the channel of monitoring reduces banks' CEO short-termism, and hence banks care more about the long-term risk of mortgages. To test this conjecture, I exploit a similar textual analysis to measure banks' attention to mortgage risks. I count the number of sentences that include *both* the strings "mortgage" and "risk" in "Item 7" and "Item 7A".[19] Then, I define a measure *mortgage risk attention* as the aforementioned sentence count divided by the aggregate length of "Item 7" and "Item 7A". The value of *mortgage risk attention* is between 0 and 0.00009, and I time it by ten thousand to make the magnitude of their coefficients interpretable.

I repeat the difference-in-difference regression in Section 3.1.3 with the dependent variable being replaced by *mortgage risk attention.* Column (3) of Panel B in Table 6 displays the results. The coefficients of *Treated* × *Post* (0.048, t = 7.94) indicates that the treatment group increased their attention to mortgage risk relative to the control group during the treatment period, consistent with my hypothesis.

---

[17] Short-term oriented keywords include "day(-s or daily)," "week(-s or -ly)," "month(-s or -ly)," "quarter(-s or -ly)," "latter half (of the year)," "short-term," and "short-run." Long-term oriented keywords include "year(-s or annual(-ly))," "long-term," "long-run," "look(ing) forward," "go(ing) forward," "looking ahead," "trend," "expect," "anticipate," "outlook," and "intend."

[18] One is included here to ensure the denominoator of *short_ratio* is non-zero.

[19] In all textual analyses in this paper, I convert the strings in 10-K fillings and in the keywords to upper case, so the measures are case insensitive.

3.2.4. Heterogeneities Based on Short-termism and Mortgage Risk Attention

I conduct two additional tests to link the monitoring effects with the changes in lending. First, if the stock market's monitoring takes effect through declines in short-termism, the effects would be greater on the mortgage origination of banks with higher short-termism before the treatment period.

I restrict the sample to the treatment group and replace *Treated* × *Post* in the baseline specification (1) with the interaction of short-termism and *Post.* Specifically, I replace *Treated* × *Post* with *short ratio - before 2004* × *Post,* where *short ratio - before 2004* is the average *short ratio* of the bank during the pre-treatment period. In an alternative regression, I replace *Treated* × *Post* with *CEO share turnover - Before 2004* × *Post,* where *CEO share turnover - Before 2004* is average *CEO share turnover* of the bank during the pre-treatment period. The specification can be considered a continuous difference-in-difference comparing the changes of treated banks with high and low pre-treatment CEO short-termism before and after the treatment. Panel A of Table 7 displays the summary statistics of the short-termism measures. Columns (1) and (2) of Panel B in Table 7 displays the results. The estimates of the interaction term are significantly negative (-0.464, t = -3.29 in Column (1); -1.974, t = -3.63 in Column(2)), which suggests that the decrease in mortgage origination of the treated banks is greater for those with higher short-termism, consistent with the channel of short-termism.[20]

Second, if the channel works through the increasing attention to mortgage risk due to declines in short-termism, the effects should be smaller for banks with more attention to mortgage risk before the treatment period. Similarly to the previous tests, I restrict the sample to the treatment group and replace *Treated* × *Post* in the baseline specification (1) with *mortgage risk attention - before 2004* × *Post,* where *mortgage risk attention - before 2004* is average *mortgage risk attention* of the bank during the pre-treatment period. The

[20] The coefficient of the *short rato-before 2004* × *Post*, -197.4% (Column(2)), looks too huge. However, note the value of *short ratio-beofre 2004* is between 0.28 to 0.97, and the coefficient represent the difference between 0 and 1. In other words, the average effect indicated by the coefficient is -13.6% (-197.4% × (0.97-0.28) × 10%) for every ten-percentiles increase in *short ratio-before 2004.* A similar intepreation can be applied to the coefficient of *mortgage risk attention-before 2004* × *Post* in Column(3), where the value of *mortgage risk attention-before 2004* is between 0 and 0.57. The average effect indicated by the coefficient is -21.2% (-415.6% × (0.51-0) × 10%) for every ten-percentiles increase in *mortgage risk attention-before 2004.*

specification can be considered a continuous difference-in-difference comparing the changes of treated banks with more and less pre-treatment attention to mortgage risk before and after the treatment. Columns (3) of Panel B in Table 7 displays the results. The positive coefficient of the interaction term (4.156, $t = 5.76$) suggests that the decrease in mortgage origination of the treated banks is smaller for those with more attention to mortgage risk, consistent with the hypothesis.

One may posit that banks responded to the pilot program by increasing their regulatory capital ratios, and the easiest way for them to do this is by cutting their portfolio lending. In Panel B, I control for *Risk-adjusted capital ratio* $\times$ *Post* and repeat all regressions in Panel A. I consider the capital ratios of both the parent firms and that of the banks. The coefficients of *short ratio - before 2004* $\times$ *Post, CEO share turnover - before 2004* $\times$ *Post, and mortgage risk attention - before 2004* $\times$ *Post* change little. The results suggest that the short-termism channel continues to hold after conditioning on the capital ratio.

[Insert Table 7 about here]

3.2.5. Market Attention

An alternative explanation is that the pilot program caught the market attention, and the pilot banks reduced their mortgage credit supply due to the increase in market attention rather than attention from short sellers. The literature has excluded this explanation (Fang, Huang, and Karpoff 2016). Nevertheless, I conduct a similar analysis to confirm that market attention does not play a role in my sample. I exploit two variables to measure market attention: the number of earnings forecasts from Thomson Reuters and trading volume from CRSP.

The list of pilot banks was announced on July 28, 2004. I first restrict the sample to 2004 and investigate whether the market attention to these banks changed before and after July 28, 2004. I construct a BHC-month level sample. In the top panel of Panel A in Table 8, I conduct t-tests to compare the market attention of the pilot BHCs and the control BHCs for both the pre-announcement period and the post-announcement period. The differences in market attention to the pilot BHCs and the control BHCs are statistically indistinguishable for both periods. This pattern is confirmed in the bottom panel of Panel A in Table 8.

I regress the market attention measure on *Post* which equals one if the observation is after July 28, 2004, and 0 otherwise. This regression is conducted separately for the pilot BHCs and the control BHCs and generates four time series estimators, with two for the number of earnings forecasts and two for the trading volume. All of them are statistically insignificant. I also exploit a univariate difference-in-difference in which I regress the market attention measure on *t,* the pilot BHCs dummy, and their interaction. The coefficient for the interaction term is also insignificant. Overall, the market attention changed little before and after the announcement.

Then, I extend the sample to the period between 2001 and 2007. I average the monthly attention measure within each year for each BHC to get a BHC - year sample of market attention. Then I consider 2004 to 2007 as the treatment period and replicate the tests from Panel A in Panel B in Table 8. The results are similar. Market attention to both groups of BHCs did not change. Besides, I include market attention in equation (1) and repeat the regression. The results displayed in Table 9 indicate that the decreases in mortgage origination were not driven by market attention.

[Insert Table 8 about here]

[Insert Table 9 about here]

3.3 Further Evidence

3.3.1. Bank Losses

I also explore how the pilot program affects the banks' ex post losses. Intuitively, less risk-taking during the boom period (due to SHO) should be followed by fewer losses during the subprime crisis. I apply the following regression to a bank-year sample from 2008 to 2012:

$$LOSS_{i,j} = \alpha_0 + \alpha_1 \times Treated_i + \Gamma X_i + FE + \varepsilon_{i,j} \quad (3)$$

The subscripts i and j refer to banks and years, respectively. The dependent variable $LOSS_{i,j}$ is the net loss (change-offs less recoveries) on single-family mortgage loans for bank i in year j. The measure of the net loss is defined by Call Reports. *Treated* is a dummy variable that assumes the value of one for loans originated by treated lenders and zero otherwise. X includes all the firm and bank characteristics introduced in Section 3.1.3, and all of them are measured in 2003. *FE* is the year fixed effects. Panel A of Table **10** displays the summary statistics, and Panel B displays the estimates. Column 1 of Panel B displays the results. Treated banks suffered fewer losses in mortgage credit during the subprime crisis. In Column 2, I replace the dependent variable with the net loss rate (*LOSS* divided by average single-family mortgages) defined by Call Reports and obtain similar results.

[Insert Table 10 about here]

3.3.2. Commercial, Industrial, and Agricultural Loans

Another interesting question is whether Reg SHO affected banks' risk-taking in only residential mortgages or both residential mortgages and other loans. In this section, I shed light on this issue using a cleansed Call Reports data by Drechsler, Savov, and Schnabl (2017) which complies loans unrelated to household credit into two types: agricultural loans and (the sum of) commercial and industrial loans. I construct a bank-quarter level sample of the outstanding balance of these two loans and conduct the following regression:

$$\Delta Y_{i,t} = \alpha_0 + \alpha_1 \times Treated_i \times Post_t + FEs + \varepsilon_{i,c,t} \tag{4}$$

The subscripts i and t refer to banks and quarters respectively. The dependent variable $\Delta Y_{i,t}$ is the growth rate of the bank-level loan balance between year t and *t-1*. The loan could be commercial and industrial loans or agricultural loans. *Treated* is a dummy variable that assumes the value of one for treated lenders and zero otherwise. *Post* is a dummy variable that assumes the value of one if quarter t is equal to or later than the third quarter of 2004. Table 11 displays the results. Unlike mortgage loans, the growth rate changed little for either commercial and industrial loans or agricultural loans. That is consistent with the argument that banks took too much risk in the residential housing sector rather than other sectors during the boom period, and the housing bubble triggered the subprime crisis.

[Insert Table 11 about here]

3.4 Loan Risks

Banks' risk-taking is reflected not only in the quantity but also in the quality of originated mortgages. In this section, I conduct a test on the risk of originated mortgages and explore how the Reg SHO Pilot program affects it.

Information about loan risk in HMDA is scarce for the sample period from 2001 to 2007. It does not include FICO, loan-to-value (LTV), or default information. Therefore, I exploit the loan-to-income ratio, which equals the loan amount divided by the borrower's annual income. This variable is shown to be a good predictor of mortgage default and has been used to measure the riskiness of mortgages (Campbell and Cocco 2015; Duchin and Sosyura 2014).

Conditional on origination volume, banks with riskier mortgages have a higher exposure to the housing crisis. Therefore, the treated banks could reduce the loan-to-income ratio after Reg SHO. However, the loan risk is determined by loan officers to a large extent. The bias of income measures (Ambrose, Conklin, and Yoshida 2016) makes it less cost-benefit effective for the management to monitor the origination of loan officers using the loan-to-income ratio. Therefore, banks might simply impose restrictions on the dollar amount of mortgages that their branches can originate, and Reg SHO might not affect the loan-to-income ratio.

I apply the following difference-in-differences specification, similar to model (1), to a loan-level sample:

$$LI_j = \alpha_0 + \alpha_1 \times Treated_j \times Post_j + \Gamma X_j + FEs + \varepsilon_j \tag{5}$$

The subscript j refers to loans. The dependent variable LI_j is the loan-to-income ratio. *Treated* is a dummy variable that assumes the value of one for loans originated by treated lenders and zero otherwise. *Post* is a dummy variable that assumes the value of one if the loan was originated between 2004 and 2007. X are loan and borrower characteristics from

HMDA, including the loan amount, applicant's gender, and applicant's race.[21] *FEs* include the county-year fixed effects and lender fixed effects. Standard errors are clustered at the county level. The sample for this regression is the loan-level performance sample introduced in Section 2, and the summary statistics are provided in Panel A of Table 12.

Panel B of Table **12** shows the results. The coefficient of the interaction terms implies that the loan-to-income ratios of loans originated by the treated banks did not change relative to that of the loans originated by the control banks in the treatment period. Overall, the results indicate that the treated banks did not rely on the loan-to-income ratio to restrict their exposure to mortgage risk.[22] [23] [24]

[Insert Table **12** about here]

3.5 Economic Stability

Household credit has a substantial influence on the real economy. Mian and Sufi (2018) survey the literature and summarize that an expansion of household debt could lead to a severe recession and depress economic activity through the drop in household demand, foreclosure sales, and credit crunch. Therefore, the Reg SHO program is expected to mitigate such an economic downturn by reining in banks' risk-taking in household credit.

---

[21] Note that the sample is restricted to conventional owner-occupied home purchase mortgages in urban counties, as mentioned in Section 2.2.

[22] The test only shows some dynamics of loan-to-income ratio and does not aim to rule out the possibility that other loan characteristics were affected by Reg SHO.

[23] Note that I control for mortgage demand by county-year fixed-effects as introduced in Section 3.1.1.

[24] In addition to all of the aforementioned tests, one might consider measuring the credit supply by comparing the approval rates of mortgage applications between the treated and control banks in difference-in-difference specification. However, such a test does fit my setting. When the treated banks decrease the approval rate and reduce the mortgage supply, applicants would respond by leaving the treated banks and turning to other banks. Therefore, the mortgage application (the denominator of the approval rate) to the treated banks would decrease, and the approval rate would rebound. My HMDA data are at an annual frequency and can hardly capture these dynamics. Therefore, the decrease in the approval rate could seriously underestimate the credit supply change. Worse, there might be "overshoots" in the response of the applicants: Too many applicants leave the treated banks such that the approval rate increases. Overshoots are possible when there is a herding behavior among applicants. I conduct an unreported approval rate analysis using baseline specification (1) with the dependent variable being replaced by the approval rate (the dollar amount of originated mortgages divided by the dollar amount of both the originated and rejected mortgages) at the bank-county-year level. The results indicate that the increase in the approval rate from the pre-treatment period to the treatment period is 1 percentage point less for the treatment group than that of the control group, which is qualitatively similar to but quantitatively different from the baseline results in Section 3.1. Due to the aforementioned concerns, I do not use the approval rate to measure the change in the credit supply.

Figure 1 plots the time series of the mean county-level housing price index in my sample.[25] In the early 2000s, the housing price index kept soaring and peaked in 2007. Then, the financial crisis erupted, and the housing price continued to decline until 2012. I focus on the period of 2004-2012, which includes the treatment period and the following recession period, and I analyze changes in activities of the real economy through the boom and bust cycle.

Table 13 lists the annual means of the county-level housing price index, employment, and number of establishments. Column 1 is the housing price index and corresponds to Figure 1. Columns 2 and 3 are for unemployment rates and per capita income, respectively. According to the hypothesis, I expect that counties that were more exposed to the pilot program (and hence to a more prudential household credit extension) would experience a less severe economic downturn when the economy shifted from an upward trend to a downward trend.

[Insert Figure 1 about here]

[Insert Table 13 about here]

I apply the following regression model to test the effects on the real economy:

$$\Delta R_c = \alpha_0 + \alpha_1 \times Share\_treated_c + \Gamma X_c + \varepsilon_c \tag{6}$$

The subscript c refers to counties. The dependent variable ΔR_c is the difference in the average logarithmic annual change of the real economy measure R for county c between the treatment period and the recession period of R, where R denotes housing price, unemployment rate, and per capita income, respectively. $Share\_treated_c$ is the market share of treated banks for portfolio mortgages in county c before the treatment period (in 2003). I use it to measure the exposure of county c to the pilot program. Because the measure is calculated based on the market structure before the announcement of the pilot banks, and the pilot banks were randomly chosen, the variation in this measure is exogenous conditional on the

[25] I keep all the counties with non-missing information on mortgage origination from the HMDA sample described in Section 2.3 and the housing price index throughout the period from 2003 to 2012.

market share of Russell 3000 Index constituents.[26] Figure 2 shows the distribution of $Share\_treated_c$, which varies across different counties, ranging from 0% to 42%. The mean and standard deviation of $Share\_treated_c$ are both 9%.

[Insert Figure 2 about here]

X is a vector including county-level controls. First, I control for the market share of all lenders in the sample (i.e., all mortgage lenders in the member list of Russell 3000 Index) for portfolio mortgages in county c. Because the treated banks are randomly selected from Russell 3000 Index constituents, $Share\_treated_c$ is random conditional on the market share of Russell 3000 Index constituents in county c. Second, I control for the market shares of portfolio loans (for all HMDA lenders) to address the concern that banks may increase originations of securitized loans in response to the securitization boom. I also control for the market shares of loans originated by lenders on the FHFA subprime lender list, the market shares of the treated banks in the small business lending market, the share of men, the share of black citizens, and the share of population older than 65 years old. All controls are measured at the county level in 2003, similar to $Share\_treated_c$. Panel A in Table 14 shows the summary statistics. Standard errors are clustered at the county level.

The results are reported in Table 14. The coefficient of $Share\_treated_c$ for housing price (Column 1) is positive, which implies that the housing growth rate increases more (decreases less) in counties with larger market shares of the treated banks before the pilot program, consistent with my hypothesis. The ratio of the coefficient and the mean dependent variable is approximately 110.4% (0.106/0.096), which indicates that if the market share of the treated banks prior to the pilot program increases by one standard deviation (9%), the decrease in housing price growth rates would be *smaller* by 9.9% (110.4% times 9%) during the subprime crisis. Columns 2 and 3 report the results for unemployment rates and per capita income, respectively. The results show that larger market shares of the treated banks are associated with larger decreases in the growth rate of unemployment rates and smaller decreases in the growth rate of per capita income during the financial crisis, and the magnitude is nonnegligible (12.2% (0.081/0.163×9%) for unemployment rates and 12.4% (0.033/0.024×9%) for per capita income for a one-standard-deviation change in

[26] Note the market share of treated banks in a county in the treatment period could be endogeneous since treated banks may select counties with better economic fundamentals to extend household credit.

Share_treated$_c$). Overall, higher market shares of the treated banks are associated with better real economy outcomes.

[Insert Table 14 about here]

In the above analysis, I use the market share of the treated banks to measure the exposure of the county to the pilot program. It is a reasonable measure given the compelling evidence in Section 3.1. Nevertheless, I conduct the following test to verify this conjecture:

$$\Delta Y_{c,t} = \alpha_0 + \alpha_1 \times Share\_treated_c + \Gamma X_c + \varepsilon_c \quad (7)$$

The subscripts c and t refer to counties and years respectively. The dependent variable $\Delta Y_{c,t}$ is the difference in the natural logarithm of the dollar volume of the county-level aggregate mortgage origination between years t and *t-1*, where t is restricted to the treatment period (from 2004 to 2007). The independent variables of regression (5) are identical to those in regression (4). Standard errors are clustered at the county level. I apply this regression to a county-level sample to investigate whether the market share of the treated banks in a county is associated with the growth rate of mortgage origination during the treatment period.

The results are reported in Table 15. The estimates justify the negative correlation between the market share of the treated banks and mortgage origination growth. The coefficient of *Share_treated*$_c$ indicates that if the market share of the treated banks prior to the pilot program increases by one standard deviation (9%), the mortgage origination growth rates would be *smaller* by 11.6% (0.347/0.27×9%) during the treatment period. The results validate the market share measure.

[Insert Table 15 about here]

4 Conclusion

This paper examines how the equity market's monitoring affects economic cycles via household credit. Using the pilot program of Reg SHO, which randomly selected a group of pilot firms and relaxed their short-selling restrictions, I find that pilot banks decreased the

origination of portfolio mortgages relative to control banks in the treatment period, while there was little change in the origination of securitized mortgages and business credit. This decrease was not driven by changes in bank financial characteristics or market attention.

Further analyses show that the pilot banks were under more intense monitoring, reduced their short-termism, and increased their attention to mortgage risks during the treatment period. The effects on mortgage origination were more pronounced for banks with higher pre-treatment short-termism and lower pre-treatment attention to mortgage risks. The pilot banks also suffered fewer losses in the subprime crisis. More importantly, the effects are transmitted to the real economy: counties with larger market shares of the treated banks exhibit smaller decreases in the growth rate of housing price, employment, and per capita income.

These findings suggest that short selling in the equity market imposes non-negligible impacts on the real economy; it mitigates the boom and bust cycle triggered by the expansion of household credit mentioned in Mian and Sufi (2018). These findings generate important policy implications. The enormous loss in the recession calls for restrictions of household credit expansion, and this paper demonstrates that the market itself can self-correct the expansion to some extent. Therefore, when regulators exploit some macroprudential restrictions that might incur costs of implementation and distortion in resource allocation, they could also consider reducing the frictions on the equity market and taking advantage of its monitoring effects.

References

Adelino, Manuel, Kristopher Gerardi, and Paul S. Willen. 2013. "Why don't Lenders renegotiate more home mortgages? Redefaults, self-cures and securitization." *Journal of Monetary Economics* no. 60 (7):835-853. doi: 10.1016/j.jmoneco.2013.08.002.

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, and Douglas D Evanoff. 2011. "The role of securitization in mortgage renegotiation." *Journal of Financial Economics* no. 102 (3):559-578.

Akinci, Ozge, and Jane Olmstead-Rumsey. 2018. "How effective are macroprudential policies? An empirical investigation." *Journal of Financial Intermediation* no. 33:33-57.

Alexander, Gordon J, and Mark A Peterson. 2008. "The effect of price tests on trader behavior and market quality: An analysis of Reg SHO." *Journal of Financial Markets* no. 11 (1):84-111.

Ambrose, Brent W, James Conklin, and Jiro Yoshida. 2016. "Credit rationing, income exaggeration, and adverse selection in the mortgage market." *The Journal of Finance* no. 71 (6):2637-2686.

Bahadir, Berrak, and Inci Gumus. 2016. "Credit decomposition and business cycles in emerging market economies." *Journal of International Economics* no. 103:250-262.

Bai, Jennie, Sterling Huang, Massimo Massa, and Hong Zhang. 2018. "Can equity market efficiency help improve the quality of credit ratings? Evidence from a controlled experiment."

Baron, Matthew, and Wei Xiong. 2017. "Credit expansion and neglected crash risk." *The Quarterly Journal of Economics* no. 132 (2):713-764.

Beber, Alessandro, and Marco Pagano. 2013. "Short-selling bans around the world: Evidence from the 2007–09 crisis." *The Journal of Finance* no. 68 (1):343-381.

Beck, Thorsten, Asli Demirgüç-Kunt, and Ross Levine. 2007. "Finance, inequality and the poor." *Journal of economic growth* no. 12 (1):27-49.

Begley, Taylor A, and Amiyatosh Purnanandam. 2016. "Design of financial securities: Empirical evidence from private-label RMBS deals." *The Review of Financial Studies* no. 30 (1):120-161.

Billett, Matthew T, Fangzhou Liu, and Xuan Tian. 2018. "Information Spillovers and Cross Monitoring between the Stock Market and Loan Market." *Kelley School of Business Research Paper* (16-20).

Billingsley, Randall S, and Tunde Kovacs. 2011. "The 2008 short sale ban: Liquidity, dispersion of opinion, and the cross-section of returns of US financial stocks." *Journal of Banking & Finance* no. 35 (9):2252-2266.

Boehmer, Ekkehart, Charles M Jones, and Xiaoyan Zhang. 2013. "Shackling short sellers: The 2008 shorting ban." *The Review of Financial Studies* no. 26 (6):1363-1400.

Boehmer, Ekkehart, and Juan Wu. 2012. "Short Selling and the Price Discovery Process." *The Review of Financial Studies* no. 26 (2):287-322. doi: 10.1093/rfs/hhs097.

Bond, Philip, Alex Edmans, and Itay Goldstein. 2012. "The real effects of financial markets." *Annu. Rev. Financ. Econ.* no. 4 (1):339-360.

Borio, Claudio. 2014. "The financial cycle and macroeconomics: What have we learnt?" *Journal of Banking & Finance* no. 45:182-198.

Brochet, Francois, Maria Loumioti, and George Serafeim. 2015. "Speaking of the short-term: Disclosure horizon and managerial myopia." *Review of Accounting Studies* no. 20 (3):1122-1163.

Campbell, John Y, and Joao F Cocco. 2015. "A model of mortgage default." *The Journal of Finance* no. 70 (4):1495-1554.

Cerutti, Eugenio, Stijn Claessens, and Luc Laeven. 2017. "The use and effectiveness of macroprudential policies: New evidence." *Journal of Financial Stability* no. 28:203-224.

Chang, Eric C, Joseph W Cheng, and Yinghui Yu. 2007. "Short-sales constraints and price discovery: Evidence from the Hong Kong market." *The Journal of Finance* no. 62 (5):2097-2121.

Chang, Eric C, Tse-Chun Lin, and Xiaorong Ma. 2018. "Does short-selling threat discipline managers in mergers and acquisitions decisions?" *Journal of Accounting and Economics*.

Chu, Yongqiang, and Daxuan Zhao. 2018. "Access to Public Capital Markets and Bank Lending." *Available at SSRN 3123848*.

Cornaggia, Jess, Yifei Mao, Xuan Tian, and Brian Wolfe. 2015. "Does banking competition affect innovation?" *Journal of financial economics* no. 115 (1):189-209.

De Angelis, David, Gustavo Grullon, and Sébastien Michenaud. 2017. "The effects of short-selling threats on incentive contracts: Evidence from an experiment." *The Review of Financial Studies* no. 30 (5):1627-1659.

DeFusco, Anthony, Stephanie Johnson, and John Mondragon. 2017. "Regulating household leverage." *Available at SSRN 3046564*.

Demyanyk, Yuliya, and Elena Loutskina. 2016. "Mortgage companies and regulatory arbitrage." *Journal of Financial Economics* no. 122 (2):328-351. doi: 10.1016/j.jfineco.2016.07.003.

Di Maggio, Marco, and Amir Kermani. 2017. "Credit-induced boom and bust." *The Review of Financial Studies* no. 30 (11):3711-3758.

Diether, Karl B, Kuan-Hui Lee, and Ingrid M Werner. 2009. "It's SHO time! Short-sale price tests and market quality." *The Journal of Finance* no. 64 (1):37-73.

DiNardo, John, Nicole M Fortin, and Thomas Lemieux. 1996. "Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach." *Econometrica* no. 64 (5):1001-1044.

Drechsler, Itamar, Alexi Savov, and Philipp Schnabl. 2017. "The deposits channel of monetary policy." *The Quarterly Journal of Economics* no. 132 (4):1819-1876.

Drehmann, Mathias, Mikael Juselius, and Anton Korinek. 2017. "Accounting for debt service: the painful legacy of credit booms." *Bank of Finland Research Discussion Paper* (12).

Duchin, Ran, and Denis Sosyura. 2014. "Safer ratios, riskier portfolios: Banks′ response to government aid." *Journal of Financial Economics* no. 113 (1):1-28.

Fahlenbrach, Rüdiger, Robert Prilmeier, and René M Stulz. 2017. "Why does fast loan growth predict poor performance for banks?" *The Review of Financial Studies* no. 31 (3):1014-1063.

Falato, Antonio, Giovanni Favara, and David Scharfstein. 2018. "Bank Risk-Taking and the Real Economy: Evidence from the Housing Boom and its Aftermath."

Fang, Vivian W, Allen H Huang, and Jonathan M Karpoff. 2016. "Short selling and earnings management: A controlled experiment." *The Journal of Finance* no. 71 (3):1251-1294.

Favara, Giovanni, and Jean Imbs. 2015. "Credit supply and the price of housing." *American Economic Review* no. 105 (3):958-92.

Fender, Ingo, and Janet Mitchell. 2009. "International banking and financial market developments." *BIS Quarterly Review* no. 3.

Flug, Karnit, Antonio Spilimbergo, and Erik Wachtenheim. 1998. "Investment in education: do economic volatility and credit constraints matter?" *Journal of Development Economics* no. 55 (2):465-481.

Frino, Alex, Steven Lecce, and Andrew Lepone. 2011. "Short-sales constraints and market quality: Evidence from the 2008 short-sales bans." *International Review of Financial Analysis* no. 20 (4):225-236.

Garmaise, Mark J, and Tobias J Moskowitz. 2006. "Bank mergers and crime: The real and social effects of credit market competition." *the Journal of Finance* no. 61 (2):495-538.

Griffin, John M, and Gonzalo Maturana. 2016. "Who facilitated misreporting in securitized loans?" *The Review of Financial Studies* no. 29 (2):384-419.

Grullon, Gustavo, Sébastien Michenaud, and James P Weston. 2015. "The real effects of short-selling constraints." *The Review of Financial Studies* no. 28 (6):1737-1767.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2004. "Does local financial development matter?" *The Quarterly Journal of Economics* no. 119 (3):929-969.

Hayek, Friedrich August. 1945. "The use of knowledge in society." *The American economic review* no. 35 (4):519-530.

Jordà, Òscar, Moritz Schularick, and Alan M Taylor. 2015. "Betting the house." *Journal of International Economics* no. 96:S2-S18.

Karpoff, Jonathan M, and Xiaoxia Lou. 2010. "Short sellers and financial misconduct." *The Journal of Finance* no. 65 (5):1879-1913.

Keys, Benjamin J, Tanmoy Mukherjee, Amit Seru, and Vikrant Vig. 2010. "Did securitization lead to lax screening? Evidence from subprime loans." *The Quarterly journal of economics* no. 125 (1):307-362.

Kolasinski, Adam C, and Nan Yang. 2018. "Managerial myopia and the mortgage meltdown." *Journal of financial economics* no. 128 (3):466-485.

Krishnamurthy, Arvind, and Tyler Muir. 2017. How credit cycles across a financial crisis. National Bureau of Economic Research.

Kruger, Samuel. 2018. "The effect of mortgage securitization on foreclosure and modification." *Journal of Financial Economics* no. 129 (3):586-607. doi: 10.1016/j.jfineco.2018.01.008.

Levine, Ross, and Sara Zervos. 1998. "Stock markets, banks, and economic growth." *American economic review*:537-558.

Li, Yinghua, and Liandong Zhang. 2015. "Short selling pressure, stock price behavior, and management forecast precision: Evidence from a natural experiment." *Journal of Accounting Research* no. 53 (1):79-117.

Lin, Tse-Chun, Qi Liu, and Bo Sun. 2019. "Contractual Managerial Incentives with Stock Price Feedback." *American Economic Review* (Forthcoming).

López-Salido, David, Jeremy C Stein, and Egon Zakrajšek. 2017. "Credit-market sentiment and the business cycle." *The Quarterly Journal of Economics* no. 132 (3):1373-1426.

Massa, Massimo, Bohui Zhang, and Hong Zhang. 2015. "The invisible hand of short selling: does short selling discipline earnings management?" *The Review of Financial Studies* no. 28 (6):1701-1736.

Mian, Atif, and Amir Sufi. 2017. "The Macroeconomic Advantages of Softening Debt Contracts." *Harv. L. & Pol'y Rev.* no. 11:11.

———. 2018. "Finance and business cycles: the credit-driven household demand channel." *Journal of Economic Perspectives* no. 32 (3):31-58.

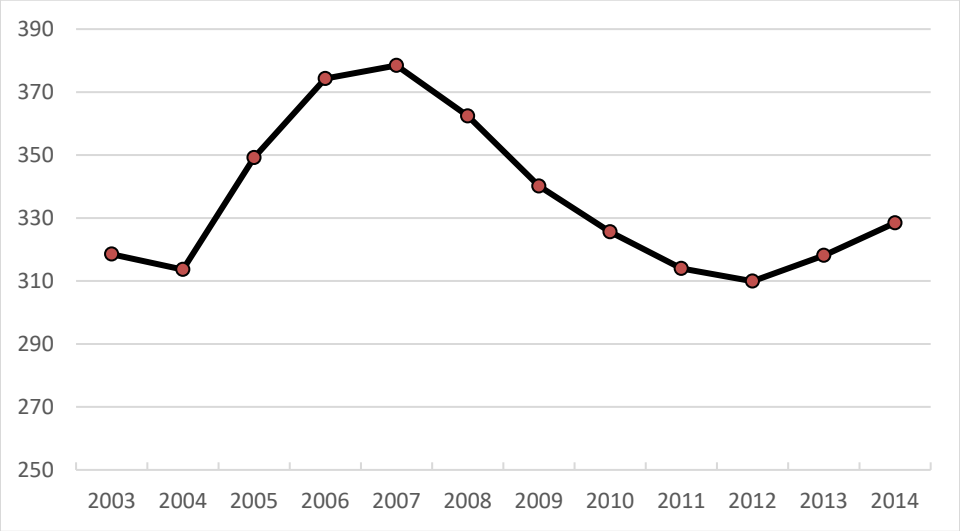
Mian, Atif, Amir Sufi, and Emil Verner. 2017a. "Household debt and business cycles worldwide." *The Quarterly Journal of Economics* no. 132 (4):1755-1817.

———. 2017b. How do credit supply shocks affect the real economy? Evidence from the United States in the 1980s. National Bureau of Economic Research.

Piskorski, Tomasz, Amit Seru, and Vikrant Vig. 2010. "Securitization and distressed loan renegotiation: Evidence from the subprime mortgage crisis." *Journal of Financial Economics* no. 97 (3):369-397.

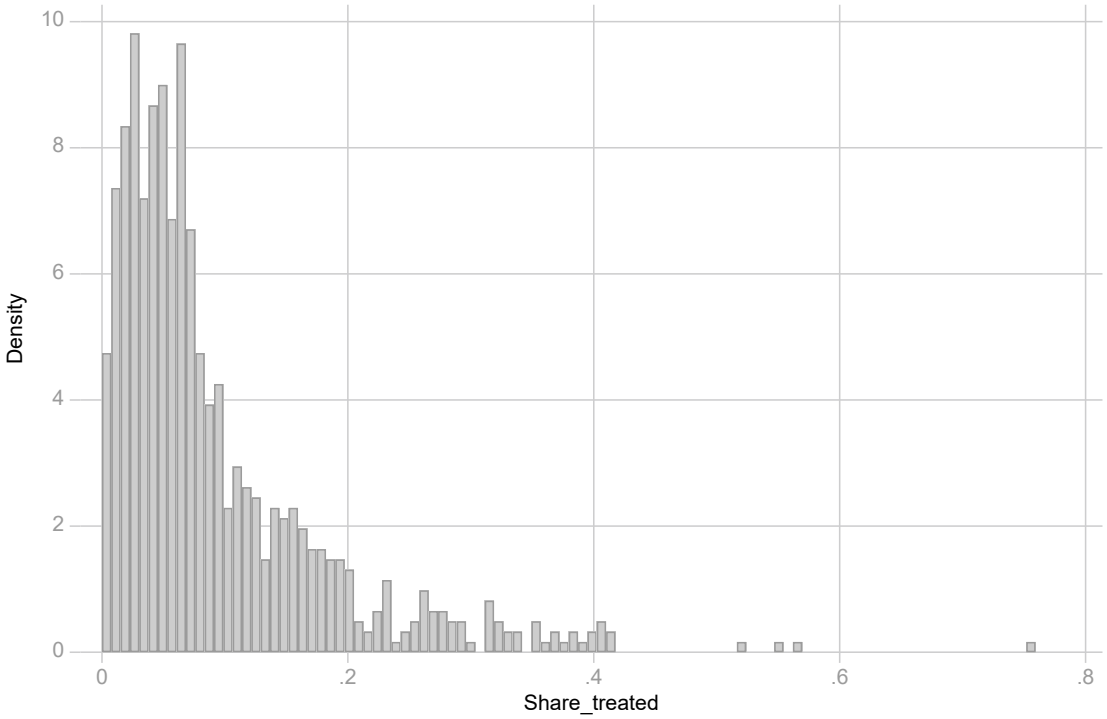
Shiller, Robert J, Rafał M Wojakowski, M Shahid Ebrahim, and Mark B Shackleton. 2013. "Mitigating financial fragility with continuous workout mortgages." *Journal of Economic Behavior & Organization* no. 85:269-285.

Figure 1: Annual Mean County-Level Housing Price Indices



This figure plots the mean county-level FHFA housing price index from 2003 to 2014.

Figure 2: Distribution of Treated Banks' Market Share at the County Level



This figure shows the distribution of the market share of the treated banks in a county in 2003.

Table 1 Comparison of the Treatment Group and Control Group

	Treated		Control		Diff	P-value
Parent Firm	N	Mean	N	Mean		
Assets ($million)	109	8998.935	47	11558.82	2559.8871	0.3795
		[1629.945]		[2295.735]	[2904.6819]	
Risk-Adjusted Capital Ratio (%)	109	13.794	47	13.778	-0.0156	0.9747
		[0.287]		[0.339]	[0.4908]	
ROA (%)	109	1.217	47	1.22	0.0023	0.9686
		[0.035]		[0.040]	[0.0595]	
Market-to-Book Ratio	109	2.397	47	2.302	-0.0954	0.3571
		[0.054]		[0.095]	[0.1033]	
Bank						
Assets ($million)	109	5192.041	47	6367.857	1175.8161	0.5749
		[1190.860]		[1586.829]	[2092.2470]	
Risk-Adjusted Capital Ratio (%)	106	12.834	46	12.796	-0.0381	0.9302
		[0.246]		[0.335]	[0.4341]	
Total Deposits ($million)	104	1800.523	43	2372.207	571.6833	0.2730
		[289.018]		[404.950]	[519.6002]	

This table reports the t-test results regarding the differences in the means of the firm and bank characteristics. The data are obtained from the financial reports of 2003.

Table 2 Descriptive Statistics of the Main Sample

Variable	N	Mean	Std. Dev.
Total volume of originated loans ($000)	24,827	2871.02	7436.59
Number of originated loans	24,827	17.90	43.22
Treated	24,827	0.44	0.50
Post	24,827	0.71	0.45

This table displays the descriptive statistics of the main sample (unit: bank-county-year). The sampling period is from 2001 to 2007.

Table 3: Mortgage Origination

Panel A: Baseline Results

	(1) Portfolio Loans	(2) Securitized Loans
Post × Treated	-0.538***	-0.092
	(-10.41)	(-1.31)
County × Year FE	Yes	Yes
Lender FE	Yes	Yes
Observations	14940	9887
R^2	0.306	0.381
Mean DV	0.106	0.140

Panel B: Robustness Tests

	(1) Number of Loans	(2) Drop 2004	(3) Re-Weighted	(4) Dynamic
Post × Treated	-0.321***	-0.570***	-0.247***	
	(-9.24)	(-10.61)	(-4.59)	
year=2001 × Treated				-0.162
				(-1.16)
year=2002 × Treated				-0.081
				(-0.75)
year=2003 × Treated				Base level
				(.)
year=2004 × Treated				-0.483***
				(-4.22)
year=2005 × Treated				-0.084
				(-0.64)
year=2006 × Treated				-0.389***
				(-3.45)
year=2007 × Treated				-0.281**
				(-2.32)
County × Year FE	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes
Observations	14940	12803	14940	14940
R^2	0.311	0.310	0.475	0.477
Mean DV	0.008	0.088	0.088	0.088

Bank-county-year regressions estimating the effect of Reg SHO on mortgage origination. The sampling period is from 2001 to 2007. The dependent variable is the annual change in the natural logarithm of the dollar volume of mortgage origination except for Column 1 (of Panel B) which uses the annual change in the natural logarithm of the number of mortgage origination. The sample in Column 2 excludes the observations in 2004. The samples in Columns 3 and 4 in Panel B are reweighted by the method described in Section 3.1.2. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 4: Lender Financials

Panel A: Summary Statistics

Variable	N	Mean	Std. Dev.
ΔLn(origination volume)	13,748	0.11	1.23
Treated	13,748	0.42	0.49
Post	13,748	0.68	0.47
Ln(firm assets)	13,748	23.48	1.42
Risk-adjusted capital ratio-firm (%)	13,748	12.97	2.64
ROA (%)	13,748	1.14	0.44
Market-to-book ratio	13,748	2.04	0.67
Ln(bank assets)	13,748	23.17	1.55
Risk-adjusted capital ratio-bank (%)	13,748	11.83	1.84
Ln(bank deposits)	13,748	21.51	1.44

Panel B: Regression Results

	(1) Baseline	(2) Firm Capital Ratio	(3) Bank Capital Ratio
Post × Treated	-0.563***		
	(-9.97)		
Post × Risk-adjusted capital ratio-firm (%)		0.224***	
		(4.80)	
Post × Risk-adjusted capital ratio-bank (%)			0.238***
			(5.55)
Ln(firm assets)	-0.196**	0.073	0.031
	(-2.55)	(0.23)	(0.10)
Risk-adjusted capital ratio-firm (%)	-0.002	-0.192***	0.012
	(-0.16)	(-3.61)	(0.26)
Market-to-book ratio	0.004	-0.048	-0.110
	(0.12)	(-0.44)	(-1.05)
ROA (%)	-0.178***	0.491***	0.496***
	(-3.71)	(4.41)	(4.61)
Ln(bank assets)	0.198	1.358***	1.392***
	(1.62)	(4.90)	(5.06)
Risk-adjusted capital ratio-bank (%)	0.001	0.045	-0.178***
	(0.06)	(0.98)	(-3.00)
Ln(bank deposit)	-0.002	-0.193***	-0.171***
	(-0.08)	(-4.36)	(-3.95)

County × Year FE	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes
Observations	13748	4262	4262
R^2	0.324	0.457	0.458
Mean DV	0.110	0.106	0.106

Bank-county-year regressions estimating the effect of Reg SHO on mortgage origination considering lenders' financial information. The sampling period is from 2001 to 2007. The sample for Column 1 is the full sample, and the samples for Columns 2 and 3 include only the treated banks. The dependent variable is the annual change in the natural logarithm of the dollar volume of mortgage origination. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 5: Short Interest

	(1)	(2)
Dep. var.: short interest	Full Sample	Drop 2004
Post × Treated	0.006***	0.006**
	(2.77)	(1.98)
Ln(firm assets)	0.014***	0.016***
	(3.02)	(2.85)
Risk-adjusted capital ratio-firm (%)	-0.000	-0.000
	(-0.20)	(-0.07)
Market-to-book ratio	-0.001	-0.000
	(-0.44)	(-0.02)
ROA (%)	0.004**	0.004*
	(2.09)	(1.66)
BHC FE	Yes	Yes
County × Year FE	Yes	Yes
Observations	600	490
R^2	0.830	0.826
Mean DV	0.028	0.031

BHC-year regressions estimating the effect of Reg SHO on short interest. The sampling period is from 2001 to 2007. The sample for Column 1 is the full sample, and the samples for Column 2 exclude observations in 2004. The dependent variable is the average monthly scaled short interest. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 6 Changes in Short-Termism and Mortgage Risk Attention

Panel A: Summary Statistics

Varibale	N	Mean	Std. Dev.
CEO turnover ratio	11,181	0.35	0.88
Short ratio	16,996	0.53	0.20
Mortgage risk attention	16,996	0.19	0.19

Panel B: Regression Results

Dep.var.	CEO share turnover	Short ratio	Mortgage risk attention
	(1)	(2)	(3)
Treated × Post	-0.226***	-0.033***	0.048***
	(-6.94)	(-4.98)	(7.94)
Ln(firm assets)	0.591***	0.034***	-0.019*
	(4.12)	(3.42)	(-1.83)
Risk-adjusted capital ratio-firm (%)	-0.071***	-0.007***	0.006***
	(-6.76)	(-2.78)	(2.90)
Market-to-book ratio	0.215***	0.077***	-0.063***
	(8.37)	(16.97)	(-15.29)
ROA (%)	0.345***	0.018***	0.028***
	(17.53)	(4.81)	(6.51)
Ln(bank assets)	-0.827***	-0.119***	0.022**
	(-7.16)	(-8.40)	(2.05)
Risk-adjusted capital ratio-bank (%)	0.018	-0.010***	0.005**
	(1.25)	(-4.22)	(2.23)
Ln(bank deposit)	0.102***	-0.007***	0.029***
	(6.48)	(-2.66)	(12.19)
Lender FE	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes
Observations	11181	16996	16996
R^2	0.750	0.778	0.781
Mean DV	0.355	0.526	0.191

Bank-county-year regressions estimating the effect of Reg SHO on short-termism and mortgage risk attention. The sampling period is from 2001 to 2007. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 7 Heterogeneities Based on Short-Termism and Mortgage Risk Attention

Panel A Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Max
CEO turnover ratio-before 2004	1,354	0.22	0.84	-1.00	1.00
Short ratio-before 2004	4,017	0.52	0.14	0.28	0.97
Mortgage risk attention-before 2004	4,017	0.10	0.11	0.00	0.51

Panel B Regression Results - Heterogeneities

Dep.var. ΔLn(origination volume)	(1)	(2)	(3)
CEO share turnover-before 2004 × Post	-0.464***		
	(-3.29)		
Short ratio-before 2004 × Post		-1.974***	
		(-3.63)	
Mortgage risk attention-before 2004 × Post			4.156***
			(5.76)
Ln(firm assets)	-1.473*	-0.343	0.059
	(-1.68)	(-0.93)	(0.16)
Risk-adjusted capital ratio-firm (%)	0.200*	-0.127**	-0.023
	(1.86)	(-2.52)	(-0.48)
Market-to-book ratio	-0.201	0.097	0.070
	(-0.77)	(0.83)	(0.62)
ROA (%)	0.294*	0.313**	0.469***
	(1.69)	(2.23)	(3.45)
Ln(bank assets)	1.489**	1.474***	1.500***
	(2.52)	(4.79)	(4.92)
Risk-adjusted capital ratio-bank (%)	-0.136	0.070	0.058
	(-1.28)	(1.32)	(1.12)
Ln(bank deposit)	0.134	-0.183***	-0.090**
	(0.55)	(-4.00)	(-2.05)
Lender FE	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes
Observations	1354	4017	4017
R^2	0.566	0.461	0.465
Mean DV	0.237	0.112	0.112

Panel C Regression Results - Control for Capital Ratio

Dep.var.	(1)	(2)	(3)	(4)	(5)	(6)
CEO share turnover-Before 2004 × Post	-0.554***	-0.587***				
	(-3.86)	(-3.10)				
Short ratio-before 2004 × Post			-1.572***	-1.792***		
			(-2.96)	(-3.51)		
Mortgage risk attention-before 2004 × Post					2.777***	2.528***
					(3.85)	(3.29)
Risk-adjusted capital ratio-firm (%) × Post	-0.040		0.265***		0.204***	
	(-0.33)		(5.52)		(4.10)	
Risk-adjusted capital ratio-bank (%) × Post		-0.048		0.287***		0.215***
		(-0.40)		(5.88)		(4.32)
Ln(firm assets)	-0.482	-0.473	0.126	0.201	0.264	0.267
	(-0.53)	(-0.52)	(0.33)	(0.53)	(0.68)	(0.70)
Risk-adjusted capital ratio-firm (%)	0.202*	0.169	-0.268***	-0.009	-0.165***	0.026
	(1.66)	(1.47)	(-4.65)	(-0.18)	(-2.89)	(0.53)
Market-to-book ratio	-0.267	-0.257	0.027	-0.026	0.017	-0.025
	(-0.96)	(-0.90)	(0.23)	(-0.23)	(0.15)	(-0.22)
ROA (%)	0.440**	0.442**	0.395***	0.456***	0.492***	0.535***
	(2.40)	(2.41)	(2.79)	(3.29)	(3.54)	(3.89)
Ln(bank assets)	0.255	0.253	1.269***	1.306***	1.369***	1.428***
	(0.35)	(0.35)	(4.13)	(4.31)	(4.52)	(4.76)
Risk-adjusted capital ratio-bank (%)	-0.047	0.001	0.059	-0.212***	0.048	-0.160**
	(-0.43)	(0.01)	(1.12)	(-3.11)	(0.92)	(-2.26)
Ln(bank deposit)	0.314	0.318	-0.188***	-0.175***	-0.118***	-0.109**
	(1.24)	(1.23)	(-4.11)	(-3.92)	(-2.64)	(-2.47)
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1354	1354	4017	4017	4017	4017
R^2	0.573	0.573	0.469	0.471	0.469	0.469
Mean DV	0.237	0.237	0.112	0.112	0.112	0.112

Bank-county-year regressions estimating the effect of Reg SHO on mortgage origination considering heterogeneities based on pre-treatment short-termism and mortgage risk attention. The sampling period is from 2001 to 2007. The dependent variable is the annual change in the natural logarithm of the dollar volume of mortgage origination. The sample includes only the treated banks. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 8 Market Attention

Panel A Within 2004

	Treatment Group (*PILOT = 1)*		Control Group (*PILOT = 0)*		Cross-sectional estimator:	
	N	Mean	N	Mean	Difference in Mean	
No. of earnings forecasts						
1st Half (01/01/2004-07/28/2004)	210	6.216	456	5.964		-0.252
2nd Half (07/29/2004-12/31/2004)	210	6.479	456	5.917		-0.562
Scaled trading volume						
1st Half (01/01/2004-07/28/2004)	210	0.049	456	0.046		-0.003
2nd Half (07/29/2004-12/31/2004)	210	0.044	456	0.043		-0.001
Univariate DiD test	N	Time-series estimator	N	Time-series estimator	DiD estimator	t-statistic
Δ No. of earnings forecasts						
2nd Half – 1st Half	210	-0.262	456	-0.048	-0.310 (0.611)	0.51
Δ Scaled trading volume						
2nd Half – 1st Half	210	-0.004	456	-0.003	0.002 (0.004)	-0.43

Panel B Full Sample

	Treatment Group (*PILOT = 1)*		Control Group (*PILOT = 0)*		Cross-sectional estimator:	
	N	Mean	N	Mean	Difference in Mean	
No. of Earnings Forecasts						
Pre-treament (2001-2003)	129	5.248	274	5.016		-0.231
Treament (2004-2007)	135	7.118	288	6.419		-0.699
Scaled Trading Volume						
Pre-treatment (2001-2003)	105	0.495	228	0.483		-0.013
Treatment(2004-2007)	140	0.802	303	0.743		-0.059
Univariate DiD test	N	Time-series estimator	N	Time-series estimator	DiD estimator	t-statistic
Δ No. of Earnings Forecasts						
Treatment – Pre-treatment	264	1.870	562	1.402	0.468 (0.752)	0.62
ΔScaled Trading Volume						
Treatment – Pre-treament	245	0.307	531	0.260	0.047 (0.073)	0.64

In Panel A, the sample is restricted to the year of 2004 and is divided into two sub-periods by July 28, 2004. In Panel B, the sample covers the period from 2001 to 2007 and is divided into two sub-periods by 2004. The top half of each panel reports summary statistics of the two market attention measures for the balanced panel sample of the treatment and control groups in the two sub-periods and the differences in means. The bottom half of each panel reports the time-series estimators as well as the univariate results of DID tests, with standard errors reported in parentheses below the DID estimators. Variable definitions are provided in the Appendix. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

Table 9: Mortgage Origination Controlling for Market Attention Measures

	(1) Trading Volume	(2) No. of Earnings Forecasts
Post × Treated	-0.556***	-0.593***
	(-9.76)	(-10.28)
Scaled trading volume	0.336***	
	(5.19)	
No. of earnings forecasts		0.023***
		(2.62)
Ln(firm assets)	-0.224***	-0.226**
	(-2.76)	(-2.49)
Risk-adjusted capital ratio-firm (%)	-0.022*	-0.010
	(-1.68)	(-0.79)
Market-to-book ratio	0.052	0.002
	(1.36)	(0.05)
ROA (%)	-0.089*	-0.244***
	(-1.87)	(-4.95)
Ln(bank assets)	0.212*	0.206
	(1.73)	(1.61)
Risk-adjusted capital ratio-bank (%)	0.002	0.024
	(0.12)	(1.06)
Ln(bank deposit)	0.011	0.006
	(0.45)	(0.26)
BHC FE	Yes	Yes
County × Year FE	Yes	Yes
Observations	13748	13110
R^2	0.326	0.329
Mean DV	0.110	0.108

BHC-year regressions estimating the effect of Reg SHO on mortgage origination controlling for market attention measures. The sampling period is from 2001 to 2007. The dependent variable is the annual change in the natural logarithm of the dollar volume of mortgage origination. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 10: Bank Losses

Panel A: Summary Statistics

Varibale	N	Mean	Std. Dev.
Net loss ($000)	586	25365.08	90099.45
Net loss (%)	571	0.01	0.01
Treated	586	0.35	0.48
Ln(firm assets)	586	22.19	1.12
Risk-adjusted capital ratio-Firm (%)	586	13.97	2.94
ROA (%)	586	1.23	0.32
Market-to-book ratio	586	2.37	0.59
Ln(lender assets)	586	21.51	1.37
Risk-adjusted capital ratio-Lender (%)	586	12.95	2.60
Ln(lender deposits)	586	20.89	1.13

Panel B: Regression Results

	(1)	(2)
	Net loss ($000)	Net loss(%)
Treated	-18454.77***	-0.002**
	(-2.96)	(-1.97)
Ln(firm assets)	16227.982***	0.000
	(4.17)	(0.54)
Risk-adjusted capital ratio-firm (%)	-2420.716	-0.001***
	(-1.32)	(-3.33)
ROA (%)	49090.326***	0.001
	(3.99)	(0.54)
Market-to-book ratio	445.801	-0.001
	(0.07)	(-1.29)
Ln(bank assets)	37918.539***	0.001
	(9.03)	(1.12)
Lender risk-adjusted capital ratio (%)	667.217	0.000
	(0.31)	(0.71)
Ln(bank deposits)	-22732.7***	-0.000
	(-5.90)	(-0.80)
Year FE	Yes	Yes
Observations	586	571
R^2	0.401	0.111
Mean DV	25365.078	0.009

Bank-county-year regressions estimating the effect of Reg SHO on bank losses. The sampling period is from 2008 to 2012. The dependent variable is the annual net losses (change-offs less recoveries) on the single-family mortgage loans of banks. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 11 Commercial, Industrial, and Agricultural Loans

	(1)	(2)
Dep. var.:	Δ Commercial and Industrial loans	Δ Agricultural Loans
Post × Treated	0.014	-0.084
	(0.92)	(-1.61)
Year-quarter FE	YES	YES
Bank FE	YES	YES
Observations	4200	3205
R^2	0.982	0.921
Mean DV	12.215	8.379

Bank-quarter regressions estimating the effect of Reg SHO on non-mortgage loan origination. The sampling period is from 2001 to 2007. The dependent variable is the annual change in the balance of commercial and industrial loans or agricultural loans. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 12 Mortgage Loan-to-Income Ratio

Panel A: Summary Statistics

Variable	N	Mean	Std. Dev.
Loan-to-income ratio	2,620,631	1.98	4.82
Treated	2,620,631	0.55	0.50
Post	2,620,631	0.10	0.30
Male	2,620,631	0.66	0.47
Minority	2,620,631	0.28	0.45
Loan amount ($000)	2,620,631	191.56	242.94
Ln(firm assets)	2,620,631	12.34	1.60
Risk-adjusted capital ratio-Firm (%)	2,620,631	11.92	1.32
ROA(%)	2,620,631	1.28	0.43
Market-to-book ratio	2,620,631	2.13	0.60
Ln(lender assets)	2,620,631	18.78	1.78
Risk-adjusted capital ratio-Lender (%)	2,620,631	0.12	0.01
Ln(lender deposits)	2,620,631	16.36	1.88

Panel B: Regression Results

Dep.var.	loan-to-income rtio
Post × Treated	-0.008
	(-0.25)
Male	-0.202***
	(-12.63)
Minority	0.093***
	(7.04)
Loan amount	0.003***
	(10.68)
Firm characteristics	Yes
Bank characteristics	Yes
County × Year FE	Yes
Lender FE	Yes
Observations	2620631
R^2	0.032
Mean DV	1.976

Loan-level regressions estimating the effect of Reg SHO on the mortgage loan-to-income ratio. The sampling period is from 2001 to 2007. The dependent variable is the loan-to-income ratio. Panel A presents the summary

statistics, and Panel B reports the regression results. The firm characteristics and bank characteristics are identical to those in Table 4. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 13: Dynamics of Real Economic Activities in the Boom-Bust Cycle

Year	HPI	Unemployment rate (%)	Per capita income
2004	354.47	5.34	30983.39
2005	396.43	5.11	32183.22
2006	424.81	4.64	33961.72
2007	427.66	4.57	35526.96
2008	407.67	5.65	37010.56
2009	381.93	9.02	35878.94
2010	365.84	9.33	36576.02
2011	352.54	8.67	38507.09
2012	348.03	7.79	39913.48

This table displays the average county-level housing price index, unemployment rates, and per capita income per year.

Table 14: The Effects of Reg SHO on Economic Cycles

Panel A: Summary Statistics

Variable	N	Mean	Std. Dev.
Dependent variables (Δgrowth)			
HPI	805	-0.10	0.07
Unemployment rate	805	0.16	0.06
Per capita income	776	-0.02	0.03
Independent variables (in 2003)			
Share_treated	805	0.09	0.09
Index (%)	805	0.47	0.15
Portfolio (%)	805	0.26	0.08
Subprime (%)	805	0.05	0.05
Small business lending (%)	805	0.17	0.15
Male (%)	805	0.49	0.01
Minority (%)	805	0.11	0.13
Age (%65+)	805	0.12	0.03

Panel B: Regression Results

	(1)	(2)	(3)
	HPI	Unemployment rate	Per capita income
Share_treated	0.106***	-0.081**	0.033**
	(3.29)	(-2.16)	(2.21)
Index (%)	-0.087***	0.008	-0.037***
	(-6.10)	(0.44)	(-5.14)
Portfolio loans (%)	0.076	-0.131***	0.007
	(1.55)	(-3.28)	(0.38)
Subprime (%)	-0.358***	0.165***	-0.015
	(-5.78)	(2.68)	(-0.92)
Small business lending (%)	0.012	0.082***	-0.017**
	(0.69)	(4.10)	(-2.17)
Male (%)	-1.269***	-0.063	-0.067
	(-4.83)	(-0.24)	(-0.67)
Minority (%)	-0.061***	0.020	-0.012
	(-3.52)	(0.98)	(-0.97)
Age (%65+)	-0.731***	0.138*	-0.077*
	(-5.41)	(1.65)	(-1.67)
Observations	805	805	776
R^2	0.186	0.083	0.057
Mean DV	-0.096	0.163	-0.024

County-level regressions estimating the effects of the market share of the treated banks on the changes in the growth rates of the housing price index, employment, and the number of establishments. The sample is cross-sectional and is constructed based on real economic activity from 2004 to 2012. The dependent variable is the difference in the average annual change in the natural logarithm of the house price index, employment, or the number of establishments between the treatment period of 2004-2007 and the recession period of 2008~2012. Panel A presents the summary statistics, and Panel B reports the regression results. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 15: Mortgage Origination Growth

Panel A: Summary Statistics

Variable	N	Mean	Std. Dev.
ΔLn(county-level origination volume)	3,226	0.03	0.31
Share_treated	3,226	0.09	0.09
Index (%)	3,226	0.47	0.16
Portfolio (%)	3,226	0.26	0.08
Subprime (%)	3,226	0.06	0.05
Small business lending (%)	3,226	0.17	0.15
Male (%)	3,226	0.49	0.01
Minority (%)	3,226	0.11	0.13
Age (%65+)	3,226	0.12	0.03

Panel B: Regression Results

Dep. var.	ΔLn(county-level origination volume)
Share_treated	-0.347***
	(-3.19)
Index (%)	-0.037
	(-1.13)
Portfolio loans (%)	0.242
	(0.60)
Subprime (%)	0.280
	(1.32)
Small business lending (%)	0.147***
	(2.88)
Male (%)	-0.381
	(-0.61)
Minority (%)	0.119***
	(2.68)
Age (%65+)	-0.527*
	(-1.71)
Observations	3226
R^2	0.136
Mean DV	0.027

The table shows the county-year level regression estimating the effect of the market share of the treated banks on the growth rates of mortgage origination. The sample period is the treatment period. The dependent variable is the annual change in the natural logarithm of the dollar volume of the county-level mortgage origination. Panel A presents the summary statistics, and Panel B reports the regression results. Robust standard errors are clustered at the county level. The t-statistics are reported in parentheses. The mean

dependent variable is reported at the bottom to assess the marginal effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

Appendix: Variable Definitions

Variable	Definition	Level
Age (%65+)	The share of the population older than 65 years old in 2003	County
CEO share turnover	CEO share turnover of the bank's parent firm	Firm-Year
CEO share turnover-Before 2004	Average CEO share turnover of the bank's parent firm during the period from 2001 to 2003	Firm
Unemployment rate	The difference in the average logarithmic annual change in the unemployment rate between the treatment period and the recession period defined in Section 3.3	County
Per capita income	The difference in the average logarithmic annual change in the per capita income between the boom period and the recession period defined in Section 3.3	County
HPI	The difference in the average logarithmic annual change in the housing price index between the boom period and the recession period defined in Section 3.3	County
Index (%)	The market share of banks on the member list of the Russell 3000 Index 2004 in 2003 for portfolio mortgages	County
Ln(county-level origination volume)	The natural logarithm of the dollar value of mortgage origination in the county	County-Year
Ln(bank assets)	The natural logarithm of the bank's total assets	Bank-Year
Ln(bank deposits)	The natural logarithm of the bank's total deposits	Bank
Ln(firm assets)	The natural logarithm of the total assets of the bank's parent firm	Firm-Year
Loan amount	The original balance of a loan	Loan
Loan-to-income ratio	The loan amount divided by the applicant's annual income	Loan
Market-to-book ratio	The ratio of the market value and the book value of the bank's parent firm	Firm
Male	A dummy that equals 1 if the borrower is male	Loan
Male (%)	The share of the male population in 2003	County
Minority	A dummy that equals 1 if the borrower is a minority	Loan
Minority (%)	The share of the minority in 2003	County
Net loss	Charge-offs less recoveries for single-family mortgage loans	Bank-Year
Net loss (%)	Net loss divided by the average balance of single-family residential mortgages	Bank-Year
No. of earnings forecasts	The number of earnigns forecasts	Firm-Year/Firm-Month
Post	A dummy that equals 1 if the year is between 2004 and 2007	Year
Risk-adjusted capital ratio-Bank (%)	The risk-adjusted capital ratio of the bank (tier 1 plus tier 2)	Bank
Risk-adjusted capital ratio-Firm (%)	The risk-adjusted capital ratio of the bank's parent firm	Firm
ROA (%)	The return on assets	Bank

Portfolio loans (%)	The mortgage market share of portfolio mortgages in 2003. It equals the dollar value of portfolio mortgages divided by that of all of the mortgages in the county	County
Share_treated	The market share of the treated banks in 2003 for portfolio mortgages. It equals the dollar value of the portfolio mortgage originated by the treated banks divided by that of all of the portfolio mortgage in the county	County
Scaled trading volume	The trading volume scaled by the number of outstanding shares	Firm-Year/Firm-Month
SI	The average monthly short interest scaled by the number of outstanding shares	Firm-Year
Small business lending (%)	The market share of the treated banks in the small business lending market in 2003	County
Subprime (%)	The mortgage market share of the subprime lenders identified by the U.S. Department of Housing and Urban Development (HUD) in 2003	County
The number of originated loans	The total number of originated mortgages	Bank-County-Year
Total volume of originated loans	The total dollar value of originated mortgages	Bank-County-Year
Treated	A dummy that equals 1 if the bank is in the treatment group	Bank
ΔLn(county-level origination volume)	The difference in the natural logarithm of the dollar value of the county-level aggregate mortgage origination between year t and t-1	County-Year
ΔLn(origination volume)	The difference in the natural logarithm of the dollar value of the bank-county-level aggregate mortgage origination between year t and t-1	Bank-County-Year