Bank leverage limits and risk-taking in the mortgage market: evidence from post-crisis reforms

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Abstract

As part of the Basel III framework, U.S. regulators introduced a minimum leverage ratio requirement on their largest banks, denominated Supplementary Leverage Ratio (SLR). Theoretical work on portfolio choice indicates that raising minimum bank leverage ratios can potentially induce increased risk-taking behavior. I test the hypothesis of an adjustment in risk and interest rate of mortgages originated by banks affected by the new SLR requirement, and evaluate consequences to local house prices. I find that (i) banks affected by the leverage limit increase overall risk-taking on mortgages; (ii) for home loans classified as higher priced, the effect is substantially amplified, interest rates are raised in order to adjust the return for risk, and riskier loans are kept longer in the balance sheet of originating banks; (iii) the aggregate increase in credit supply resulting from the adjustment is correlated with higher future home prices. Overall, there is evidence of heterogeneous effects of policy, in which borrowers of higher risk are more affected. The findings carry implications for the revision of post-crisis bank regulation. They indicate that a raise in bank leverage limits can coexist with the expansion of credit conditions, contradicting common claims of the banking industry against this form of capital requirement. At the same time, as leverage shifts from bankers' to borrowers' balance sheet, households become more exposed to risk once negative income shocks materialize.

JEL codes: G20, G21, G28.
Keywords: Basel III regulation, bank capital requirements, leverage ratio, credit supply, bank lending.

1 Introduction

In the aftermath of the 2007-2009 financial crisis, governments in the United States and abroad engaged in the most ample banking regulatory reform since the Great Depression. As these changes have been implemented, a rich empirical debate has emerged in order to assess their efficacy and outcomes (Crump & Santos, 2018; Duffie, 2018). For some authors, the post-crisis regulatory reform was insufficient to limit borrowing and to control risk-taking incentives of large bank holding companies (Admati, 2014). Others have argued against excessive complexity and high compliance costs of regulation, pointing out that the reforms induced reductions in credit supply and failed to achieve their original objectives (Calomiris, 2018). In any case, proposals to enhance the current framework benefit critically when supported by the empirical assessment of its effectiveness.

Among recent changes in prudential regulation is the introduction of the Basel III Leverage Ratio (LR) requirement, a leverage limit advocated by the Basel Committee on Bank Supervision. Leverage limits are capital requirements that do not vary with banks' asset risk. The Basel III LR is defined as the ratio of *tier 1 capital* to *total leverage exposures*. The denominator of the ratio is composed of *total assets* plus some *off-balance sheet exposures*, such as, for example, the notional amount of credit derivatives. All exposures are treated the same way, independent of risk, which differs from typical risk-based capital requirements which are part of the Basel I and II Accords. The aim of the new Basel III LR is to decrease solvency risk of financial institutions, avoiding the inherent difficulties of assessing risks of banks' assets (Miller, 2016). The simpler, unweighted capital requirement should work as a backstop in case the risk-weighted requirement fails to capture true asset risk (Basel Committee on Banking Supervision, 2014). In the U.S., the Basel III LR was denominated Supplementary Leverage Ratio (SLR). It was first announced by regulators in January 2012 and became effective only six years later, in January 2018. When the SLR rule was finalized in 2014, many financial institutions reported that the new leverage limit became their main binding capital constraint, meaning it was more binding than their risk-based capital requirement (Choi, Holcomb, & Morgan, 2018).

The hypotheses I analyze in this paper are derived from theoretical models of optimal bank portfolio choice, subject to minimum leverage ratio requirements. In Acosta-Smith, Grill, and Lang (2018), in line with the Basel III framework, banks face two constraints on capital, the risk-based capital requirement and the leverage ratio. Banks choose their asset portfolio between a risky and a safer asset, and their liability composition between capital obtained from investors and deposits from the public. The authors show that if banks are subject only to risk-based capital requirements, they will choose to hold as little capital as possible, making the requirement a binding constraint. In other words, risk-based capital requirements force banks to hold more capital if they wish to take more risk, a well-know motivation of regulators for this type of rule. Key to the previous conclusions are the assumptions of limited liability of the bank and full deposit insurance. The former means a bank only repay depositors and investors if it survives negative shocks. The latter makes depositor behavior insensitive to bank risk, and so the marginal cost of obtaining debt becomes constant to the banker. However, if banks are also subject to a minimum leverage ratio, and this requirement becomes binding, then the optimal portfolio choice is to hold a larger share of the risky asset than before. Once the LR binds, it forces banks to put more capital in, and additional risk taking comes for free, with higher expected returns to the banker and eventual costs burden by depositors and taxpayers. Thus, in this type of model, imposing a binding leverage ratio requirement will always incentivize banks to take more risk, when equity is sufficiently costly[1]. At the same time, there is the mechanical effect of holding more equity: banks experience lower probabilities of failure due to the increased loss absorbing capacity. This leads to lower expected loss of depositors' and taxpayers' funds in adverse scenarios. Similar conclusions are obtained in the earlier work of Koehn and Santomero (1980). Here, bankers choose the amount of capital and deposits, and the allocation across assets of different risk and return, but they face only a leverage ratio constraint. After demonstrating ambiguous effects that the introduction of the minimum LR has on probabilities of bank failure, the authors argue that regulation should be complemented by constraining the asset composition of banks, or adopting some type of risk-based requirement. This recommendation was further extended in Kim and Santomero (1988). In summary, theoretical models demonstrate two apparently contradictory consequences of the introduction of a minimum leverage ratio. A better capital position automatically reduces the risk of bank insolvency, but a binding LR creates an incentive to reach for yield, and to increase risk in asset composition.

This paper investigates whether the imposition of leverage limits on the very large U.S. banks by the Supplementary Leverage Ratio (SLR) rule have impacted risk-taking and interest rates in the mortgage market. Specifically, I analyze changes in the risk of originated new home purchase loans extended by banks covered by SLR regulation, after the final rule announcement, when compared with similar loans originated by comparable banks non-covered by the rule. For a subset of loans where price data is available, I also assess changes in the price of credit originated by SLR covered banks. The use of detailed loan level data on mortgages allows me to control for observed risk factors, and general demand conditions of the geographic location. In order to identify causal effects, I adopt the changes-in-changes treatment effects framework of Athey and Imbens (2006). The method assumes different average benefits between treatment and control groups, and heterogeneity of the treatment effect on the treated. Therefore, it accommodates the possibility that treatment was assigned to banks which would benefit mostly from the intervention, as judged by regulators.

I choose to analyze the mortgage market because of its size and economic importance. Residential real estate loans and mortgage backed securities represent about 32% of total credit, and 25% of total assets held by commercial banks in the U.S.[2] The group of banks directly affected by the SLR rule originated on average $129 billion yearly in new home purchase loans between 2011 to 2017[3]. Even adjustments of small magnitude in risk and in the amount of credit supplied by these banks at loan level can add to sizable impacts in the aggregate. Besides, mortgages represent by far the largest form of household debt, reaching 69% of total debt, on average, between 2011 to 2017 (Federal Reserve Bank of

[1]The exact conditions for this proposition are described in Acosta-Smith et al. (2018).

[2]This figures are from the H.8 report from Federal Reserve Board (2020), and refer to August 2019. Residential real estate loans added up $2,271 billion compared to $2,011 for mortgage backed securities.

[3]This represents a reasonable share of total origination. According to the Consumer Financial Protection Bureau (2019), the average volume of mortgages originated for purchase or refinancing was $1,834 billion per year during 2011 to 2017. Note that the value of $129 billion per year originated by SLR covered banks excludes refinancing.

New York, 2020). From the macroeconomic perspective, household leverage is considered a determinant factor for business cycle fluctuations (Jordà, Schularick, & Taylor, 2016; Mian, Sufi, & Verner, 2017). Adjustments in risk-taking by banks in mortgage origination will eventually impact household balance sheets, and can interact with the macro dynamics.

Consistent with theoretical models of portfolio choice, I find that banks subject to the new leverage limit increase risk-taking on home mortgage origination after the announcement of the final SLR rule by an average of 7.8 to 8.9 percentage points (p.p.) in loan-to-income ratios, even when controlling for observed loan level risk factors. There is evidence of heterogeneous effects, in which loans on the upper quantiles of the distribution of risk are considerably more affected. Besides, the adjustment towards increased risk is specially strong on mortgages classified as "higher-priced". In this subsample, I find that the average treatment effect on loan-to-income ratios for SLR covered banks is remarkably high, ranging from 39.70 to 45.64 p.p., and that treatment implied a raise of 0.53 to 0.61 p.p. in loan annual spread. Interestingly, for loans which are kept longer in the balance-sheet of affected banks, the adjustment in risk and spread is even larger. This result strongly suggests that banks shifted their behavior to a combination of higher risk and return in mortgage origination, as a consequence of the leverage limit constraint.

In a second stage of the analysis, I explore how the adjustment in risk of loan origination implied by the SLR is correlated with future house prices at the local level. Although banks subject to the SLR are large and operate across the U.S., I explore the variability in concentration to define a measure of treatment intensity at the county level. In a difference-in-difference setup, I find that an increase in credit relative to county income by affected banks after the introduction of the SLR rule leads to higher future house prices. The magnitude of the treatment effect is economically significant. For each percentage point raise in credit relative to income I observe an increase of 0.21 percent in home prices. This finding is consistent with a positive credit supply shock resulting from the introduction of the SLR, and indicates a possible channel between bank capital regulation and house prices.

My paper relates to research about general and distortionary effects caused by the adoption of the Basel III Leverage Ratio requirement. Previous authors have established significant effects of the leverage rule on several dimensions of risk-taking and liquidity provision, but, to the best of my knowledge, no previous study has analyzed consequences to credit supply. Duffie (2018) argues that leverage ratio rules reduce the incentives for banks to intermediate markets for safe assets. Since the SLR rule was announced in 2012, the largest U.S. domestic bank holding companies cut back significantly on some types of intermediation and raised their ratio of risk-weighted assets to total assets, according to the author. Acosta-Smith et al. (2018) found that U.K. banks bounded by the Basel III LR increased overall risk by changing their composition of assets, after the rule announcement, when compared with similar higher capitalized banks not bounded by the LR. Choi et al. (2018) analyze U.S. banks and find evidence consistent with risk-shifting on the asset composition due to the SLR rule. Banks subject to the new rule rebalanced their portfolio toward riskier assets overall, when looking at shares of securities, trading and lending assets. Detailed analysis was carried out on the securities portfolio, at an individual level, and the authors confirm a reaching-for-yield behaviour. Allahrakha, Cetina, and Munyan (2018) investigate effects of the adoption of the SLR on the U.S. repurchase agreement (repo) market. They find an economically significant reduction of repo lending by institutions subject to the new limits, as well as evidence that some activities were shifted to non-bank dealers. Finally, Du, Tepper, and Verdelhan (2018) argue that deviations from the covered interest rate parity observed in foreign exchange and swap markets may be have been caused by the higher cost of capital in arbitrage operations implied by the Basel III Leverage Ratio.

More generally, my research contributes to the literature on capital requirements and bank behavior. Most studies focus on changes in risk-based capital requirements, as these are the cornerstone of prudential regulation since the Basel I and II Accords. Regarding this topic, there is ample evidence that capital requirements proportional to asset risk are an important determinant of bank investment choices, as banks act to conserve regulatory capital by modifying the cost and supply of credit (Gambacorta & Mistrulli, 2004; Behn, Haselmann, & Wachtel, 2016; de Ramon, Francis, & Harris, 2016; Jiménez, Ongena, Peydró, & Saurina, 2017; Plosser & Santos, 2018; Gropp, Mosk, Ongena, & Wix, 2019; Juelsrud & Wold, 2020). Studies typically find that increases in risk-based capital requirements incentivize banks to reduce credit supply, as in Gropp et al. (2019) or Juelsrud and Wold (2020). A complementary strand of this literature is dedicated to understanding the behavior and efficacy of countercyclical capital buffers (Koch, Richardson, & Van Horn, 2020; Basten, 2020). This policy tool, which is also part of the Basel Committee on Banking Supervision post-crisis agenda, requires systemically-important banks to accumulate capital when the economy expands so that they could survive crises that occur occasionally when the economy contracts. On the other hand, simple leverage limits have received much less attention from the empirical literature. In practice, with the exception of the U.S., leverage limits were not widely adopted by regulators previously to Basel III implementation. My paper contributes to our understanding of the effects of leverage limits on bank credit supply decisions by analyzing an event where the requirements were a relevant constraint for a reasonable number of large U.S. banks.

The findings of my paper carry implications for the revision of post-crisis regulation and, more broadly, for the design of financial stability policy. First, they indicate that a raise in bank leverage limits can coexist with the expansion of credit conditions. When banks choose to raise capital as a response to the binding leverage limit, the slack on their risk-based capital requirement widens[4]. It becomes, therefore, profitable for banks to increase risk-taking, which they can achieve by shifting credit origination. In this paper, I verified the existence of this channel. Next, the findings show that the risk adjustment of originated credit as a response to regulation leads to higher leverage for borrowers. For the case of mortgages, as households borrow more as a fraction of their income, they become more exposed to default risk, specially if negative income shocks materialize. Finally, the results suggest that risk-shifting, and the aggregate credit supply effect it entails, may act as an impulse to house prices. In conclusion, the overall findings are useful to inform policy makers in charge of assessing changes in the regulation of leverage ratios, and for those evaluating enhancements in the post-crisis regulatory framework.

This paper is organized as follows. Section 2 details the regulatory framework of the Supplementary Leverage Ratio and the data sources used in the study. The empirical strategy

[4]There is also the possibility that banks choose to decrease asset size and the share of debt in response to a binding leverage limit (Furfine, 2001). This was not verified empirically in the case under study, and it is further discussed in Section 5.

and results of the main analysis, which is focused on the effects of the regulatory change on loan origination in the mortgage market, are presented in Section 3. Section 4 describes the method and results for the second stage of the analysis, which focuses on how the adjustment implied by the SLR is correlated with future home prices at the county level. At last, Section 5 concludes by discussing policy implications and contributions of the paper to the current debate on financial regulation.

2 Regulatory framework and datasources

Leverage limits have a previous history in U.S. financial regulation, dating back to at least 1981 when the Federal Deposit Insurance Corporation (FDIC) introduced the first numerical capital standards applicable to all banks (Kling, 2016). The minimum leverage ratio (LR) was initially set at 6% of total capital relative to total assets, but it suffered adjustments over time (Choi et al., 2018). As of 2019, for example, the FDIC requires that all depository institutions must hold a minimum LR of *tier 1 capital* to *average total assets* of 4 percent. With the Basel I Accord in 1990, the focus of regulation changed to risk-based capital requirements. Standard risk weights were defined for broad asset classes, and minimum capital ratios were set relative to *total risk-weighted assets*. The following Basel II Accord in 2004 further elaborated risk-sensitive capital requirements. It also allowed very large "advanced approach" bank holding companies to use internal models to estimate asset risk, instead of using the standard weights by asset class.

Leverage ratio requirements made an important comeback when the Basel Committee on Banking Supervision introduced a leverage ratio in the 2010 Basel III package of reforms[5]. According to the Comittee, an underlying cause of the 2007-2009 financial crisis was the build-up of excessive on- and off-balance sheet leverage in the banking system. In most cases, banks were able to built up leverage while maintaining strong risk-based capital ratios. The proposed Basel III Leverage Ratio was thus intended to reinforce the risk-based capital requirements with a simple, non risk-based backstop, at the same time addressing concerns about model risk. A simple leverage limit aims to reduce the risk of periods of deleveraging in the future, and the damage they inflict on the broader economy. The Basel III LR is defined as the ratio of *tier 1 capital* to a combination of on- and off-balance sheet exposures[6]. Off-balance sheet exposures include, for example, notional principal amount of credit derivatives, credit and liquidity commitments, guarantees and standby letters of credit.

In the U.S., regulators[7] adopted the Supplementary Leverage Ratio (SLR) requirement as the equivalent to the Basel III Leverage Ratio, and also created an additional version of the same requirement, named "enhanced" SLR (eSLR), applicable only to the largest banks. Both rules were designated to "advanced approach" banking organizations only, which use internally generated risk estimates for setting risk-based capital requirements. Regulators

[5]For details about the Basel III Leverage Ratio recommendations, see Basel Committee on Banking Supervision (2017).

[6]As published by Office of the Comptroller of the Currency and Federal Reserve System (2013).

[7]The regulators are the Office of the Comptroller of the Currency (OCC), Board of Governors of the Federal Reserve System (Fed) and Federal Deposit Insurance Corporation (FDIC). The September 2014 final rule was published in Office of the Comptroller of the Currency, Federal Reserve System and Federal Deposit Insurance Corporation (2014).

recognize that the SLR was proposed only for advanced approach banks because these organizations tend to have more significant amounts of off-balance sheet exposures that are not captured by the previously existent leverage ratio. The SLR rule requires bank holding firms to maintain a minimum ratio of *tier 1 capital* per *total leverage exposures*, including off-balance sheet assets, of 3 percent. All advanced approach banking organizations, which are those having consolidated assets of at least \$250 billion or foreign exposures of at least \$ 10 billion, are subject to the SLR rule[8]. Furthermore, the largest advanced approach bank organizations, defined as Global Systemically Important Banks (G-SIB) must comply with the eSLR, which initially added an extra 2% buffer on top of the 3% minimum ratio[9], summing up 5% of total exposures. A key difference between the earlier LR and the new SLR rule is that the latest includes a wider set of off-balance sheet exposures in the calculation of the denominator of the ratio. In practice, if an institution holds a large amount of off-balance sheet exposures relative to total assets, a minimum SLR can become binding even though the traditional LR is not.

Table 1 presents a summary of the SLR implementation timeline. Six years separate the first announcement of the rule, in January 2012, and the compliance date of January 2018. Key events happened during 2014, when details about which off-balance sheet exposures would be included in the ratio's calculation were being discussed, with much public comment (Choi et al., 2018). In September 2014, the final SLR rule is published. Covered banks began public disclosure of their measured ratios beginning January 2015, and the rule became effective in January 2018.

2.1 Datasources

I obtain loan level data from the Home Mortgage Disclosure Act (HMDA) public dataset, provided by the Consumer Financial Protection Bureau. HMDA, enacted by Congress in 1975, requires most mortgage lenders located in metropolitan areas to collect data about their housing-related lending activity, report the data annually to the government, and make the data publicly available. HMDA reports the geographic location of originated and purchased home loans, information about denied home loan applications, characteristics of the loans (amount, insurance), borrower attributes (race, sex, income), and price data for a limited subsample of loans. Price data take the form of a rate spread between the annual percentage rate on a loan and the rate on Treasury securities of comparable maturity. The price is reported for "higher-priced" loans only, which carry rates that exceed certain thresholds set by the Federal Reserve Board[10]. For the purposes of this research, I filter yearly loan level

[8]The advanced approach characterization extends to all subsidiaries of a bank holding company which is already in this category.

[9]The G-SIB subject to the eSLR are bank holding companies with more than \$700 billion in consolidated total assets or more than \$10 trillion in assets under custody. Depository institutions subsidiaries of the G-SIB holding company must, in their turn, comply with a 3% additional capital on top of the 3% minimum as part of the eSLR requirement, summing up to 6% of total exposures.

[10]For example, for first-lien loans, the threshold is three percentage points above the Treasury security of comparable maturity. Banks are not required to report spread information for loans of this type with annual percentage rates below this threshold. According to the Federal Reserve Board, the thresholds are chosen to exclude the majority of prime-rate loans and to include the majority of subprime-rate loans (Federal Reserve Board, 2005).

data on originated home purchases by the bank holding companies and its subsidiaries in the sample.

I gather balance sheet information about the bank holding companies (BHCs) from the Reports of Condition and Income (Call Reports), FR Y-9C, FR Y-15 and FFIEC 101, published by the Federal Reserve and the Federal Financial Institutions Examination Council (FFIEC). Economic data at geographical level is obtained from three other sources. The Federal Housing Finance Agency (FHFA) provides yearly data on house prices by state, metropolitan statistical area (MSA) and county. The Financial Accounts of the United States, published by the Federal Reserve Board, provide yearly measures of household debt-to-income ratio by state, MSA and county. Additional county level data measuring economic outcomes, such as employment and annual payroll, is obtained from the County Business Patterns (CBP) series published by the U.S. Census Bureau.

The following process was used to link the loan level data with the corresponding bank holding companies. First, the list of BHCs in the sample was defined by the criteria described in Section 3 (see also Table 2). Then, for each BHC, I built a list of subsidiaries, at each year, using organizational structure data from FFIEC National Information Center (NIC). The list of subsidiaries was complemented manually, to add mortgage originators which are not part of the NIC register, but are part of the BHCs in the sample and were active in reporting mortgages to HMDA. When the full list of subsidiaries is completed for each year, the HMDA dataset is searched and the loans selected. The main bank mergers occurring in the sample period are listed in the Appendix. In terms of data, the mergers were simply treated as incorporating the subsidiaries in the BHCs when they start reporting as part of the conglomerate.

3 Loan level analysis: risk taking and spread

The main objective of this paper is to evaluate how the introduction of the Supplementary Leverage Ratio (SLR) rule has affected risk-taking and interest rates in the mortgage market in loans originated by banks covered by the rule. In order to conduct a rigorous empirical testing, I make use of a treatment effects framework assuming that (i) regulation has potentially different average effects for covered and non-covered banks; (ii) regulation has potentially affected covered banks with different intensity.

The first assumption accounts for the fact that policy change could have been imposed on banks that would derive unusual benefits from that same policy change. Regulators selected the criteria for SLR coverage by setting a size threshold, that is, they explicitly assigned treatment. Realistically, they could have done so according to some criteria correlated with expected outcomes. Policy evaluation studies in the banking literature usually disregard this possibility, and use standard difference-in-differences methods (Choi et al., 2018; Acharya, Berger, & Roman, 2018; Pierret & Steri, 2019). The common claim is that, given observed bank characteristics, for example size, selection into treatment is independent of outcomes, or exogenous. In this paper, I take a more cautious approach by not assuming exogeneity of treatment assignment.

The second assumption is aligned with theoretical models such as Acosta-Smith et al. (2018), where the optimal banker's choice when subject to a leverage limit depends on which capital requirement is binding. Intuitively, banks held different levels of capital before the new leverage requirement was announced. Conservative, more risk-averse banks were likely holding higher levels of capital than more aggressive, risk-seeking banks. Thus, the SLR rule was likely binding for a subset of the treated banks. For well capitalized banks, where the rule was not binding, there is no expected reaction in terms of changes in risk-taking. The opposite is true for poorly capitalized banks. In summary, I expect to observe effects of increased risk-taking in loans proportional to initial risk preferences of covered banks. Banks in the upper tail of the distribution of risk are expected to be more affected by the leverage rule.

As a way to address the mentioned issues, I adopt the changes-in-changes (CIC) model of Athey and Imbens (2006). The method is a heterogeneous treatment effects framework which generalizes the standard difference-in-differences (DID) model. Under CIC assumptions, the control and treatment groups are allowed to have different average benefits from the treatment. At the same time, the CIC model provides estimates of the treatment effect on the treated over the entire distribution of outcomes. My empirical analysis in this section is concerned with the estimation of treatment effects of SLR regulation in risk taking and in the price of credit by using a CIC model on loan level data.

In the next subsection, I describe the sample of banks and assumptions about the timing of treatment. Then, I analyze the comparability of banks in the sample, and how they adjusted overall balance-sheet variables during the announcement and implementation of the SLR rule. Next, I detail the baseline changes-in-changes model, as well as the econometric specification for the loan level analysis. The findings are presented in the following subsections. Finally, I test the results for robustness and alternative explanations.

3.1 Sample of banks and timing of treatment

A total of twenty-two bank holding companies (BHCs) form the sample under analysis, divided in two groups. The treated group is composed of all nine BHCs which are both subject to the SLR rule[11] and active in the home mortgage market. The control group contains the next thirteen BHCs in terms of size, which are not covered by the SLR but are also active in the home mortgage market. I define that BHCs must report at least 1,000 originated home purchase loans in each year during the period 2008 to 2017 to be considered active in the home mortgage market. Given that the assignment rule for the SLR requirement is based on size criteria, the BHCs in the treated group are substantially larger than those in the control group. The loan level analysis is carried out using originated mortgages from all the subsidiaries of each BHC, while any aggregate analysis will refer to the financial reports of the bank holding company.

The list of all bank holding companies in the sample is shown in Table 2 with their respective size (*total assets*) as of December 2014. All institutions in the sample hold more than $50 billion in total assets. This cut-off matches the Dodd Frank Act of 2010 qualification for designating "systemically important financial institutions" (SIFIs). The objective is to make the treatment and control group as comparable as possible. All SIFIs are subject to

[11]The list of BHCs covered by the SLR is based on Choi et al. (2018).

the same capital requirements, with the exception of the Supplementary Leverage Ratio[12], face heightened regulatory scrutiny, including Comprehensive Capital and Analysis Review (CCAR) stress tests, and must comply with similar liquidity regulation. According to Choi et al. (2018), SLR covered banks face a stricter version of the new liquidity coverage rule than banks in the control group. The treated group of banks is required to hold more liquid assets in comparison to the control group, which tends to limit the risk shifting effect I am investigating. If the liquidity requirement was binding at any point in time, it would result in a conservative, downwards bias in my estimates.

I choose the year of 2014 when the SLR rule was finalized as the treatment start date. Given that the final rule publication was in September, and there was a relevant announcement in April of that same year, I choose to drop 2014 out of the sample. Recall that the HMDA dataset only provides the year of origination of loans, and not the specific origination date. Including the year 2014, either as pre or post treatment, would add unnecessary noise to the estimation. I consider three years before and after the start date as the observation period, thus the pre-treatment period covers 2011 to 2013, while the post-treatment covers 2015 to 2017. It is possible that banks have started to adjust mortgage origination earlier, in 2012 when the SLR rule was first announced, so I also test for effects around this year when presenting the baseline results.

3.1.1 Comparability of banks and aggregate adjustment

Average capitalization and other bank characteristics for the treatment and control groups are provided in Table 3 for the periods before and after treatment. Data is obtained from quarterly regulatory financial reports. The institutions are comparable in terms of relative capitalization. SLR covered banks show higher average levels of risk-based capital ratios (RBCR) and a lower average level of tier 1 leverage ratio (LR) than their non-covered peers[13]. The data makes it clear that the implementation of the Supplementary Leverage Ratio motivated covered banks to increase their LR considerably. Average tier 1 leverage ratio increases by 1.37 percentage points (p.p.) between periods for SLR covered banks (equivalent to 17.7% of the initial level), while by only 0.18 p.p. (or 1.9%) for the non-covered group. This is also evident in Figure 1, which shows the time series evolution of the average LR for both groups. The adjustment in the LR for SLR covered banks appears to begin in the end of 2012 and goes roughly until 2016. This period includes the critical phase between the first announcement of the SLR rule, in January 2012 until its finalization in September 2014. There is an apparent rise in the LR for non-covered banks as well from 2012 to 2013, but much smaller in magnitude.

Banks in the sample are also comparable regarding measures of profitability. Return on equity (ROE), net income and interest income are in the same range both groups, although SLR banks exhibit somewhat higher levels of ROE. In the post-treatment period, for example,

[12] The risk-sensitive capital requirements are based on minimum ratios of: (i) common equity tier 1 capital over risk weighted assets (RWA); (ii) tier 1 capital over RWA; (iii) total regulatory capital over RWA (Pierret & Steri, 2019). All depository institutions are still subject to the standard minimum leverage ratio, defined as tier 1 capital over total assests.

[13] I discuss observed tier 1 leverage ratio (LR) instead of the SLR because the latter is only reported after January 2015. I assume both measures are sufficiently correlated for the purposes of this aggregate analysis.

average ROE is 4.89% for SLR banks and 4.11% for non-SLR peers. Note that banks subject to SLR are larger and usually more complex financial organizations, with some of them engaging in trading, brokerage and activities typical of investment banks. This also translates in greater non-interest income. Nonetheless, non-covered banks have increased their ROE by a faster rate in the full period.

With respect to asset composition, some features are worth noticing. SLR covered banks have a more diversified portfolio, holding less loans as a share of assets, but more trading and liquid assets. The ratio of risk-weighted assets (RWA) to total assets is higher for non-covered banks. This could suggest greater relative level of risk taking for the control group, but it might be also a consequence of different methods, with varying degrees of flexibility, for calculating RWA. The treated group, SLR covered banks, are classified as "advanced approach" organizations, which are allowed by regulation to use internal models for calculating their risk-weights, instead of the standardized methods. More important are the changes in the ratio of RWA to total assets observed over time for the two groups: increments of 4.05 and 0.86 percentage points respectively for the treated and control group. This difference in trend indicates that treated banks increased overall risk taking by a larger magnitude when compared to the control group after treatment. The shares in the loans portfolio confirm greater diversification in holdings of SLR covered banks. Loans secured by real estate represent around 40% of loans in this group compared to 50% for non-covered institutions. The changes over time in the loan shares are of similar size between groups. In terms of loan quality, aggregate measures point to higher charge-offs ratio for SLR covered banks, which signal a riskier portfolio of loans.

The aggregate volume of credit originated yearly by all banks in the sample is shown in Table 4, from 2008 to 2017, and also in Figure 3a. There is an overall decrease in credit originated from the beginning of the sample until 2011, both by SLR covered banks and non-covered, as the economy experienced the Great Recession. The total amount originated starts at $217 billion and reaches $145 billion in 2011 (see Panel A). From there on, there is a steady increase in credit originated, which stabilizes around $187 in the last two years of the sample. The share of the amount originated by SLR banks is reasonably stable, fluctuating around 76 to 80%. Importantly, there is no sign of reductions in credit supply around the treatment start date in 2014, for any of the groups. The same is true if we consider 2012, when the SLR rule was first announced. The second part of Panel A documents the remarkable increase in the share of loans unsold in the year of origination. This happens for both groups of banks, but appears more intense for the case of institutions subject to the SLR, where the share of loans unsold raises from around 20% in the first few years to two thirds by the end of the sample. The trend is also verified in Figures 3b and 3c which plot time series of total credit originated. Independently of the causes for this rearrangement, it implies that originated mortgages are remaining for longer in the balance sheet of banks. Thus, adjustments in risk-taking and interest rate at origination became more relevant to the profitability of this group of financial institutions during this period of time. The last part of Panel A shows the steady decline over the years in the number of loans originated by both groups of banks. As the volume of credit, in dollars, expanded after 2011, the number of loans kept decreasing. The share of the quantity of loans originated by treated banks is very stable over time, around 73% after 2011, which confirms that the declining trend is roughly parallel for both groups. This fact suggests no correlation between the decline in quantity and the adoption of the SLR rule. Panel B presents similar statistics for loans classified as "higher priced". I highlight the sharp drop in the volume of mortgage originations of this kind during the Great Recession, from \$10.3 billion to around \$1 billion in total. The decrease was more intense for SLR banks, but this same group also shows consistent growth in volume originated after 2012. During the last four years of the sample period, the share of higher priced loans originated by SLR banks seems to have stabilized around 70 to 74% of the total.

Average characteristics of mortgages originated by banks in the sample are presented in Table 5. Banks in the treatment and control group are fairly comparable in most measures. The average loan-to-income ratio is higher, in levels, for loans originated by SLR covered banks, and it also grows at a higher rate during the period. It raises 16.90 p.p for SLR covered banks compared to 10.60 p.p. for non-covered institutions. This is an initial indicative of increased risk-taking behavior. The treated banks extend on average larger loan amounts, and the raise in their mean loan size is noticeable: from \$260.8 to \$377.0 thousand, a 44.5% rate in just a few years. Comparatively, the control group raises the average loan size by 28.9%, from \$226.2 to \$291.7 thousand. At the same time, SLR covered banks lend to borrowers of higher income, and the average income raises over time. The demographic characteristics of borrowers are very similar. There is an overall decrease in the share of government insured loans, which is more intense for SLR covered banks. The share of loans unsold in the same year of origination presents an upward trend in both groups, that is stronger for SLR covered banks. It appears that banks were incentivized to retain the originated mortgages in their portfolio for longer. The share of higher priced loans increases on average for treated banks while it decreases for the control group. Again, this could signal the intention of assuming higher risk by SLR covered banks. In turn, the economic characteristics of the loan location reveal that SLR banks tend to lend in slightly wealthier and more indebted neighborhoods, and increased their participation in regions which experienced stronger house price growth. The general evolution in average loan-to-income ratio (LIR) for both groups of banks is shown in Figure 2, from 2008 to 2017. It confirms that SLR covered banks typically originated loans of higher LIR through the whole period. The gap in LIR between the two groups appears to widen from 2014 to 2016, which corroborate data from Table 5.

In summary, aggregate ratios demonstrate reasonable comparability in the sample and suggests the occurrence of an adjustment in the balance-sheet of SLR covered institutions which matches the expected behavior of banks constrained by a leverage limit. Treated banks raise the relative level of capital to assets, decrease holdings of liquid assets, and increase overall asset risk. This findings were already explored by previous literature, such as Duffie (2018) and Choi et al. (2018). The analysis of aggregate volume of originated mortgages shows no sign of credit restrictions by banks subject to the SLR around the treatment time. My next step is to test the hypothesis of increased risk taking on the portfolio of originated home mortgages by treated banks. The aggregate evolution of loan-to-income ratios provides suggestive evidence for this claim. For robust inference, I turn to the use of detailed micro level data, which allows me to control for observable characteristics of loan risk, and more precisely estimate the magnitude of the regulatory effect. The next session presents the formal method used to accomplish this task.

3.2 Changes-in-changes model

Athey and Imbens (2006) propose a generalization of the standard difference-in-differences (DID) model, denominated changes-in-changes (CIC). The CIC approach allows for heterogeneous treatment effects, in which the effects of both time and treatment can differ systematically across individuals. In this section, I will follow closely their description, as well as the summary in Imbens and Wooldridge (2009).

The CIC model is formally described as follows. Assume the setting with two groups, treatment and control, and two time periods, pre and post treatement, where repeated cross-sections are observed. Individual i belongs to group $G_i \in \{0, 1\}$, where group 1 is the treatment group, and is observed in time period $T_i \in \{0, 1\}$, where time 0 is the pre treatment. Let the outcome be Y_i, so the observed data are (Y_i, G_i, T_i, X_i), where X_i is a set of covariates representing observable characteristics of individuals. Let Y_i^N denote the outcome for individual i in the absence of treatment and let Y_i^I be the outcome for the same individual in case it receives the treatment. For simplicity of exposition, the covariates X_i are ignored at first. All the results from Athey and Imbens (2006) hold conditional on X_i. Later, I will show particular functional forms that can be assumed for the relationship between X_i and observed outcomes.

Athey and Imbens (2006) relax the additive linear DID model by assuming, in the absence of intervention, that the outcomes satisfy

$$Y_i^N = h(U_i, T_i) \tag{1}$$

with $h(u, t)$ an increasing function in u. The random variable U_i represents the unobservable characteristics of individual i. Equation (1) incorporates the idea that the outcome of individuals with the same unobservable characteristics, i.e. $U_i = u$, will be the same in a given time period, irrespective of group membership. The outcome is a function of unobserved characteristics and the time period. The distribution of U_i is allowed to vary across groups, but not over time within groups, so that $U_i \perp T_i | G_i$.

Thus, in CIC the treatment group's distribution of unobservables may be different from that of the control group in arbitary ways. In the absence of treatment, all differences between groups are modeled as differences in the conditional distribution of U given G. Changes over time in the distribution of a group's outcome are due to $h(u, 0) \neq h(u, 1)$. This feature makes the model sufficiently flexible to cover realistic scenarios of policy adoption, while at the same time enables identification.

It can be shown that the standard difference-in-differences model can be nested as a special case of CIC, by adopting three additional assumptions

$$\begin{aligned} &U_i = \alpha + \gamma \cdot G_i + \epsilon_i \quad \text{with} \quad \epsilon \perp (G_i, T_i) && \text{(additivity)} \\ &h(u, t) = \phi(u + \delta \cdot t) && \text{(single index model)} \end{aligned}$$

for a strictly increasing function $\phi(\cdot)$, and

$$\phi(\cdot) \text{is the identity function} \qquad\qquad \text{(identity transformation)}$$

Note that in contrast to the standard DID model, the assumptions for CIC do not depend on the scaling of the outcome, for example, whether outcomes are measured in levels of logarithms. Besides, CIC does not assume a particular form for the $h(u,t)$ function, which is linear in time for the case of DID.

To analyze the counterfactual effect of the intervention on the control group, the authors assume that in the presence of the intervention

$$Y_i^I = h^I(U_i, T_i) \tag{2}$$

for some function $h^I(u,t)$ increasing in u. That is, the effect of the treatment at a given time is the same for individuals with the same $U_i = u$, irrespective of group membership. There is no need for further assumptions on the functional form of $h^I(.)$. The treatment effect for individuals with unobserved component u is equal to $h^I(u,1) - h(u,1)$, and can differ across individuals. Because the distribution of unobserved characteristics U can vary across groups, the average return to the policy intervention can vary across groups as well. Therefore, in the changes-in-changes framework heterogeneous treatment effects are modeled as a consequence of different realizations u (across individuals) or different distributions U (across groups) of unobserved characteristics.

Next, I summarize the identification and estimation of the CIC model in the continuous case. To simplify notation, let us assume the shorthand $Y_{gt}^N \sim Y^N|G=g, T=t$, $Y_{gt}^I \sim Y^I|G=g, T=t$, $Y_{gt} \sim Y|G=g, T=t$, $U_g \sim U|G=g$. The corresponding conditional cumulative distribution functions (CDF) are $F_{Y^N,gt}$, $F_{Y^I,gt}$, $F_{Y,gt}$, $F_{U,g}$, with supports $\mathbb{Y}_{gt}^N$, $\mathbb{Y}_{gt}^I$, $\mathbb{Y}_{gt}$ and $\mathbb{U}_g$ respectively. The following model assumptions were already mentioned, and are formalized here[14]:

1. Model: the outcome of an individual in the absence of intervention satisfies the relationship $Y^N = h(U,T)$.

2. Strict monotonicity: the production function $h(u,t)$, where $h : \mathbb{U} \times 0, 1 \mapsto \mathbb{R}$, is strictly increasing in u for $t \in \{0,1\}$.

3. Time invariance within groups: we have $U \perp T|G$

4. Support: we have $\mathbb{U}_1 \in \mathbb{U}_0$

Athey and Imbens (2006) show that the counterfactual distribution of Y_{11}^N is identified through the equality

$$F_{Y^N,11}(y) = F_{Y,10}(F_{Y,00}^{-1}(F_{Y,01}(y))) \tag{3}$$

In intuitive terms, we can use directly estimable distributions $F_{Y,10}$, $F_{Y,00}$ and $F_{Y,01}$ to determine $F_{Y^N,11}$, the counterfactual distribution of the outcome of the treatment group in period $t=1$ in the absence of intervention. Using the representation from (3), the average treatment effect on the treated can be written as

$$\begin{aligned} \tau^{CIC} &= E[Y_{11}^I - Y_{11}^N] = E[Y_{11}^I] - E[Y_{11}^N] \\ &= E[Y_{11}^I] - E[F_{Y,01}^{-1}(F_{Y,00}(Y_{10}))] \end{aligned} \tag{4}$$

[14] Assumption (4) was not mentioned previously, but Athey and Imbens (2006) prove it can be relaxed for practical purposes.

and an estimator for this effect can be constructed using empirical distributions. Besides, the authors show that the continuous CIC treatment effect can be calculated at each specific quantile of the distribution of outcomes for the treated group, using the same cumulative distribution functions.

3.2.1 CIC estimator and adjusting for covariates

The average treatment effect for the continuous changes-in-changes model can be estimated non-parametrically. The needed assumptions on the data generating process are the following. Let the observations from group g and time period t be denoted by $Y_{gt,i}$, where Y_i is a random draw from the subpopulation conditional on $G_i = g$, $T_i = t$. For all $t, g \in \{0, 1\}$, $\alpha_{gt} \equiv Pr(T_i = t, G_i = g) > 0$. The four random variables Y_{gt} are continuous with densities $f_{Y,gt}(y)$ that are continuously differentiable, bounded from above by $\bar{f}_{gt}$ and from below by $\underline{f}_{gt} > 0$ with support $\mathbb{Y}_{gt} = [\underline{y}_{gt}, \bar{y}_{gt}]$.

The empirical distribution is used as an estimator for the cumulative distribution function

$$\hat{F}_{Y,gt}(y) = \frac{1}{N_{gt}} \sum_{i=1}^{N_{gt}} \mathbb{I}\{Y_{gt,i} \leq y\} \tag{5}$$

where $\mathbb{I}$ is an indicator function. In turn, an estimator for the inverse of the distribution function is

$$\hat{F}^{-1}_{Y,gt}(q) = \inf\{y \in \mathbb{Y}_{gt} : \hat{F}_{Y,gt}(y) \geq q\} \tag{6}$$

so that $\hat{F}^{-1}_{Y,gt}(0) = \underline{y}_{gt}$. Finally, an estimator of $\tau^{CIC} = E[Y^I_{11}] - E[F^{-1}_{Y,01}(F_{Y,00}(Y_{10}))]$ is

$$\tau^{\hat{CIC}} = \frac{1}{N_{11}} \sum_{i=1}^{N_{11}} Y_{11,i} - \frac{1}{N_{10}} \sum_{i=1}^{N_{10}} \hat{F}^{-1}_{Y,01}(\hat{F}_{Y,00}(Y_{10,i})) \tag{7}$$

In this paper, I consider a parametric approach to adjust for covariates in line with suggested by Athey and Imbens (2006). I assume

$$h(u,t,x) = h(u,t) + x'\beta \quad \text{and} \quad h^I(u,t,x) = h^I(u,t) + x'\beta$$

with U independent of (T, X) given G. In this specification the effect of the intervention does not vary with X, although it still varies by unobserved differences between individuals. The average treatment effect when I adjust for covariates is given by

$$\tau^{CIC} = E[\tilde{Y}^I_{11}] - E[F^{-1}_{Y,01}(F_{Y,00}(\tilde{Y}_{10}))]$$
$$\text{where} \qquad \tilde{Y}_{gt,i} = Y_{gt,i} - X'_{gt,i}\beta$$

The estimator for τ^{CIC} is obtained as follows. First, I estimate β as a linear regression of outcomes Y on X and four group-time dummies (no need for intercept). The regression is estimated by ordinary least squares. Then, I apply the CIC estimator to the residuals from the previous linear regression, adding the effects of the dummy variables back in. Formally, define $D = ((1-T)(1-G), T(1-G), (1-T)G, TG)'$. The first stage regression is

$$Y_i = D_i'\delta + X_i'\beta + \varepsilon_i \tag{8}$$

I calculate the residuals with the group and time effects back in by

$$\hat{Y}_i = Y_i - X_i'\hat{\beta} = D_i'\hat{\delta} + \hat{\varepsilon}_i \tag{9}$$

Finally, I apply the CIC estimator to the empirical distribution of the augmented residuals $\hat{Y}_i$. Athey and Imbens (2002) show the consistency of this covariance-adjusted estimator.

For the purposes of this paper, I will base inference on confidence intervals obtained from bootstrap procedures, as suggested by Athey and Imbens (2006). A bootstrap sample of size N_{gt} is taken from each group and time, for $g \in \{0,1\}$, $t \in \{0,1\}$. The CIC model is estimated, adjusting for covariates, using the bootstrap sample. The process is typically repeated for $B = 1,000$ times. The standard deviation of each estimate is then calculated using the percentile method. I take the difference between the 0.975 and 0.025 quantiles and divide that by 2×1.96 to get standard errors estimates.

3.3 Empirical strategy of the loan level analysis

The loan level analysis tests the main hypotheses of the paper. I assess changes in risk-taking and in the price of credit for new home purchase mortgages originated by banks subject to the SLR rule, after the regulatory intervention, when compared to peer non covered banks. Using the changes-in-changes model with detailed micro level data allows me to control for observable characteristics of loan risk, as well as to capture demand factors, in order to precisely estimate the magnitude of the regulatory effect.

3.3.1 Risk-taking

For the analysis of changes in risk-taking, the hypotheses can be stated as: (i) SLR covered banks have increased risk-taking given treatment, so the average treatment effect on the treated τ^{CIC} is positive; (ii) the treatment effect on the treated is heterogeneous, it is stronger on the upper tail of the distribution of mortgage risk.

The outcome variable $y_{i,g,t}$ is the loan-to-income ratio (LIR) on mortgage i, originated by a bank from group g at time t, where $g \in \{0,1\}$, $t \in \{0,1\}$. The LIR represents the borrower's ability to repay the loan amount considering his gross annual income. Riskier loans have increasing loan-to-income ratios, given other risk factors. According to Ignatowski and Korte (2014) this measure is commonly used in the mortgage business to assess borrower risk, and as a criterion for eligibility for loans to be insured by the Federal Housing Administration. Besides, Rosen (2011) finds that LIR usually correlates strongly with other measures of individual loan risk such as credit scores. To lessen the influence of outliers, I winsorize the loan-to-income ratio at the 0.1 and 99.9 percentiles. The groups $(0,1)$ represent, respectively, banks non-covered by the SLR rule (control), and covered banks (treatment). The time periods $(0,1)$ define pre and post treatment, as previously explained.

I control for covariates adopting the following parametric form

$$y_{i,g,t} = h(u,t) + x'_{i,g,t}\beta \qquad \text{, and} \tag{10}$$

$$y^I_{i,g,t} = h^I(u,t) + x'_{i,g,t}\beta \tag{11}$$

The covariates in $x_{i,g,t}$ can be classified in four groups: bank characteristics, loan characteristics, economic factors and demographics of loan location, geographical fixed effects. The functional forms in Equations (10) and (11) assume a linear relationship between covariates and the outcomes. I evaluate three choices of geographical fixed effects: state, county and metropolitan statistical area (MSA). For comparison reasons, I also estimate a simple model specification with no controls. Bank specific control variables are measured at the bank holding level and lagged by one quarter. The controls are intended to account for the notable size differences and for the different business models of BHCs. I include the log and log squared of *total assets*, and the following variables: *trading assets ratio*, *liquid assets ratio* and *net income to assets ratio*. Loan characteristics control for factors directly correlated with loan risk, which are dummies for *government insured loan*, *female borrower*, *non-white race borrower*. Economic factors, demographics of the mortgage location and geographical fixed effects are correlated with loan risk and at the same time are intended to capture the dynamics of the demand side. The controls are *population* and *median family income*, both in logs and measured at census tract level; *house price index* in level and in log difference, measured at either county, or MSA level; *debt-to-income ratio* of households, measured at state, county, or MSA level. The choice of level for the measures *house price index* and *debt-to-income ratio* depend on the model specification, that is, the type of geographical fixed effects.

I interpret the assumptions required for the CIC model by first defining $h(u,0) = u_0$. In my case, u_0 measures the mortgage loan amount, as a ratio of borrowers annual income, a bank lent to an individual in period 0 regulatory environment, taking into account bank and loan characteristics, individuals' attributes, and the economic state and demographics of the home location. Intuitively, u_0 represents the amount of risk the bank took in the loan which is not explained by the covariates. The observed loan amount u_0 is a function of an unobserved factor u, which I assume captures risk preferences of the bank. The transformation function $h(u,0)$ maps the unobserved factor to an observed loan amount, and it is naturally assumed to be monotonic. The distribution of $U|G = g$ can differ across the different groups of banks. This means banks covered by regulation can have different risk preferences than non-covered ones, which would imply different distributions of U. The CIC model requires two other assumptions. First, the distribution of U should stay constant over time within a group. This fits my hypothesis, as I am exploring whether banks adjusted their portfolio to an optimal risk-return combination, as a response to a regulatory intervention, given their risk preferences. In the short time period under investigation, I rule out changes in risk preferences of financial institutions, and thus in the distribution of U. Second, the untreated outcome function $h(u,t)$, which maps unobserved factors u to loan amounts $h(u,t)$, is monotone in u and is the same for both groups. This is a methodological *a priori* assumption. I allow control and treated banks to have different risk preferences, as long as the mapping from unobserved factors u to loan amounts is the same between the groups.

3.3.2 Spread

Supposing that the risk-taking adjustment of affected banks is verified as expected (towards loans of higher relative risk), the spread analysis will verify two competing hypothesis regarding price adjustment. I call them, respectively, a *pure credit loosening* versus a *higher return* hypothesis. In the *pure credit loosening* case there is either a decrease or no adjustment in spread, meaning affected banks are taking more risk in loan origination without requiring higher interest payments from borrowers. In the alternative *higher return* case, I expect to observe a positive adjustment in spread, in which lending becomes more expensive on average. This implies that banks are choosing a combination of higher risk and return, and is the hypothesis most consistent with the expected theoretical effects of a binding leverage limit.

The outcome variable $y_{i,g,t}$ is spread in loan i, originated by a bank from group g at time t, measured in percentage points. The spread represents the cost of credit to the borrower and expected return on gross interest income to the bank. As before, I winsorize the outcome variable at the 0.1 and 99.9 percentiles. The spread analysis adopts the same parametric form of Equation (10), and control for observable characteristics of loan risk by including the same set of covariates as in the risk-taking analysis. Note that, given restrictions of data availability, I am only able to perform the spread analysis on a subset of loans classified as "higher priced".

The CIC model assumptions are interpreted as follows. Define $h(u, 0) = u_0$, where u_0 is the mortgage spread charged by the originating bank to an individual borrower in period 0 regulatory environment, given bank and loan characteristics, borrowers' attributes and economic state and demographics variables of home location. Here, u_0 represents the loan price not explained by the covariates, and it is a function of the unobserved factor u, which I interpret as the unobserved value of the loan to the borrower. In principle, loans of higher value to the borrower, which offer for example a longer maturity or a larger amount relative to borrower's income, should be also more expensive as they are more costly for the bank[15]. Just as before, the transformation function $h(u, 0)$ maps the unobserved factor to an observed loan spread, and it is naturally assumed to be monotonic. The distribution of $U|G = g$ can differ across groups of banks, meaning that affected banks can originate different types of loans than non affected ones. The next assumption is that the distribution of U should stay constant over time within a group, which means that there are no changes in the short time period under investigation. Finally, I must assume that the untreated outcome function $h(u, t)$, which maps unobserved factors u to loan spreads $h(u, t)$, is monotone in u and is the same for both groups.

3.4 Results

This section presents and discusses the paper main findings. I begin with the loan level analysis of changes in risk-taking considering all originated loans. Then, I use the subset of

[15]For the time being, I am ignoring how pricing may depend on the degree of local competition, market power, and strategic choices by the banks. I assume that banks simply adjust price according to its marginal cost or simply refuse to originate a certain type of loan, for example of longer maturity. Issues of market power and competition are left to be explored in further work.

higher priced loans to investigate changes on both risk and spread. In all cases, the baseline assumption is that the adjustment started in 2014, when the SLR rule was finalized. I also investigate the alternative hypothesis that banks started to increase risk-taking earlier, in 2012, when the SLR rule was first announced. Besides, I explore whether loans kept longer in the balance sheet of SLR covered banks were affected differently than loans sold in the same year of origination. I conclude by showing robustness tests, such as a placebo event and testing the model in a reduced sample of more similar sized banks.

3.4.1 Increased risk-taking in loan origination

The baseline case evaluates treatment effects of the SLR regulatory intervention on risk-taking considering all originated loans. The full sample is composed of 3,302,002 observations from 2011 to 2017, already excluding the year of 2014. Such a large size hinders the estimation task due to the computationally intensive nature of the changes-in-changes estimator. To circumvent this problem, I extract a random sample of 200,000 observations from the full dataset, which is then used in estimation[16].

In Table 6, I present the results from the estimation of the effect of the SLR rule on loan-to-income ratios (LIR) of covered banks for the baseline case. There are four different model specifications, one in each column, depending on how I control for covariates. Column (1) is a simple CIC model with no covariates, columns (2) to (4) include all bank, loan level and economic controls as well as geographic fixed effects for state, county and MSA, respectively. The model with no covariates, column (1), is included for comparative reasons only. As it does not control for individual loan risk factors, I assume the estimates from this model do not allow reliable inference about changes in risk-taking. For all models, I present estimates for the average effect on the treated, followed by treatment effects estimated by quantiles of the distribution of outcomes. Standard errors are shown in parenthesis and are calculated based on 1,000 bootstrap replications. The sample size is smaller for the MSA model due to some missing data on the house price index covariate. At the bottom of the table, I show some general measures of model fit for the covariates linear regression estimated in the first stage.

I find that SLR covered banks increased loan-to-income ratios by an average of 7.76 to 8.88 percentage points (p.p), depending on the model specification, and this effect is precisely estimated. There is clear evidence of heterogeneous effects. Loans in the lower quantiles of the loan-to-income ratio were less affected by treatment, and the estimated effect is increasing on the level of the outcome. For example, the estimated effect for loans in the 20th quantile are positive around 4.6 to 6.7 p.p., while the same estimate for loans in the 70th quantile is in the range of 9.4 to 11.2 p.p. Overall, this finding confirms the research hypothesis, revealing that the SLR rule led to increased risk-taking on mortgages originated by affected banks, and that the effect is also increasing with the level of individual mortgage risk. The estimated treatment effect is economically significant. As shown in the descriptive statistics of Table 5, the average observed LIR of loans originated by affected banks raised by 17.12 p.p., from 244.5 to 261.6 p.p. between the periods before and after treatment. An average

[16] As an example of the performance of the estimation procedure, even when using the subsample of 200 thousand observations, each full run of the CIC model with 1,000 bootstrap replications takes from 4 to 5 days to finish execution in a 4 virtual cores CPU Intel Xeon 2.5 GHz with 32 Gb of memory.

treatment effect of 7.76 to 8.88 p .p. represents between 45% to 52% of the total observed unconditional raise in LIR, which is a fairly significant share[17]. The simple model with no controls provides results comparable to those obtained with more complex specifications. The model with county fixed effects dominates in terms of fit, while measures of information criteria do not offer concluding evidence in favor of either the state or county level models[18].

As an alternative exercise, I test the hypothesis of an earlier adjustment starting in January/2012, where the pre-treatment period includes 2010 and 2011 and the post-treatment covers 2012 and 2013. The objective is not to overlap this alternative post period with events which occurred in the year 2014 when the rule was finalized. I extract a random sample of 200,000 observations from the full dataset of loans, covering the years 2008 to 2013, and then select only the years of interest, which results in 116,635 observations ready to estimation. The results are presented in Table 9, and they show no evidence of change in risk-taking on mortgages originated by affected banks, considering a treatment start date of January 2012. For the models with controls, the point estimates for the average treatment effect are on the positive side, but with very small magnitude and relatively high standard deviation. They are not statistically different from zero at any reasonable level of confidence, and this holds for basically any of the estimated quantiles. The model with no controls provides point estimates in a different direction, of negative average treatment effects, but, as previously explained, given its simplicity this model does not allow conclusions regarding changes in risk-taking. At this point, one would tend to reject the hypothesis of a treatment effect that started when the SLR rule was first announced in 2012, but a closer look at different subsamples of loans uncovers an interesting subtlety.

I further detail the analysis considering separately two subsamples of loans. The first group is composed of loans which were not sold by the originating bank during the calendar year of origination, denominated "unsold". The second group are loans sold to government agencies (Fannie/Ginnie Mae, Freddie/Farmer Mac), private securitization, commercial banks and other financial institutions during the year of origination. Note that the share of unsold loans represents about a third of the full sample, and there is no information on what happens to each loan after the year of origination. For both groups, I test the baseline hypothesis of treatment starting in 2014 against the alternative of January/2012.

Table 8 presents the results from the estimation of the CIC models with state, MSA and county fixed effects and all controls, for unsold and sold loans, using the baseline timing assumption for treatment starting in 2014 and loan-to-income ratio as the outcome variable[19]. I find that the average treatment effect on loans originated by affected banks is positive and precisely estimated for the subsample of *loans sold* only. The point estimates are precisely estimated at 10.9 to 11.9 p.p., depending on the model specification. The heterogeneous treatment effects are increasing in LIR, and they go up to 17.8 to 18.7 p.p. on the 90th quantile. This finding basically confirms the results obtained previously in the whole sample estimation, with effects of higher magnitude. In contrast, for *unsold loans* I find no evidence

[17]As the descriptive statistics refer to an average unconditional change in LIR, there are naturally other reasons than treatment effects which could explain the raise, such as changes in the composition of originated loans.

[18]Note that given the differences in sample size, information criteria measures are not comparable between the MSA model and the remaining ones, state or county.

[19]For the sake of simplicity, I do not report the estimates for the model with no covariates in this case.

of adjustment in loan-to-income ratios given treatment. The point estimates are small and positive in the range of 1.1 to 3.5 p.p. but with standard deviations around 2.2 to 3.2 p.p. Basically, over the full distribution of the outcome, the estimated treatment effects are statistically zero in this case. In conclusion, the adjustment in risk-taking by affected banks is relatively large, statistically and economically significant for the subsample of sold loans but not verified for unsold loans.

A different picture emerges when I consider the alternative assumption that treatment started in January/2012 by the first announcement of the SLR rule. Table 10 provides the estimates. This time, in the state and county fixed effects models, the average treatment effect for *unsold loans* is positive, of 7.1 to 7.4 p.p. respectively, with standard deviations of 3.1 and 3.6 p.p., while it is statistically zero for *sold loans*. This is suggestive evidence that the adjustment in risk-taking for unsold loans might have occurred as well, but starting earlier than for sold loans. The magnitude of the adjustment in unsold loans in 2012 is similar to the average treatment effect estimated in the baseline case, for the whole sample, and taking 2014 as the treatment date (see Table 6). Contrarily, in the model with MSA fixed effects, the estimates are very small in magnitude, and statistically not different from zero, for both cases of unsold and sold loans. I take this last finding with care, given that many loans do not have an MSA identifier, causing the sample to be biased and reduced. At last, Figures 4a and 4b provide an illustration of the results under discussion. The plots represent the average LIR on loans originated by SLR covered and non-covered banks from 2008 to 2017. For unsold loans (Figure 4a) SLR covered banks seem to have adjusted their average unconditional LIR by about 20 p.p. consistently after 2012 when compared to the control group. On the other hand, for sold loans (Figure 4b), the gap appears to widen only after 2014 and its magnitude is less distinguishable.

To conclude, I interpret the findings of this section as confirming the research hypothesis. Banks affected by the SLR rule increased overall risk-taking on mortgages originated after the regulation was finalized in 2014, when compared to non-affected banks. The treatment effect is higher for loans in the upper quantiles of the distribution of risk. There is some weaker evidence that suggests the adjustment might have started earlier, when the rule was first announced in 2012, but only for loans which were not sold during the year of origination.

3.4.2 Adjustment in higher priced loans: risk and spread

In this part of the analysis, I explore a particular subsample of loans, classified as "higher priced". As previously explained (see Section 2.1), whenever the rate spread of a loan exceed certain thresholds fixed by regulators, lenders are required to report the spread and classify this loan as higher priced. This classification aims to include the majority of subprime-rate loans (Federal Reserve Board, 2005), and is thus expected to cover loans of higher relative risk. I test the hypothesis of increased risk-taking in higher priced loans originated by affected banks by estimating the CIC model using loan-to-income ratio as the outcome variable. Then, I investigate the hypothesis of price adjustment by estimating treatment effects on loan spread. In both cases, I repeat the exercise of splitting the sample in loans unsold versus sold in the same year of origination. The baseline assumption is that treatment started in 2014, when the SLR was finalized. The estimation is conducted using the full sample of 72,096 loans reported in the higher price category.

The results for the risk-taking analysis are presented in Table 11. The model specifications and the reported statistics are equivalent to what was presented in the previous section. Column (1) contains the statistics for a simple model with no controls while columns (2) to (4) contain statistics for models with all controls and different types of geographic fixed effects. I find that the average treatment effect on loan-to-income ratios for SLR covered banks is remarkably high for the subsample of higher priced loans. The point estimates vary from 39.70 to 45.64 p.p, depending on the model specification, and are precisely estimated. This represents a treatment effect about five times larger than what was verified in the full sample. Higher priced loans, which are assumed to carry higher risk, were substantially more affected than average loans in the portfolio of treated banks. This finding provides additional evidence for the presence of heterogeneous effects proportional to risk-taking. Besides, heterogeneous effects are verified inside the group of higher priced loans. Loans in the lower quantiles of LIR were relatively less affected by treatment, than those in the upper quantiles. For example, considering the model with county fixed effects, the estimated treatment effect starts at 26.54 p.p for loans in the 10th quantile and raise to more than 51 p.p. after the 80th quantile.

Even though the number of higher priced loans originated by SLR covered banks is relatively small when compared to the full mortgage market, the treatment effect on risk-taking is of large economic magnitude, at least in terms of increased liability to individual borrowers. Consider the observed statistics from higher priced originated loans reported in Table 5. Average borrowers' yearly income remains roughly constant at \$74 to \$73 thousand between the pre and post treatment periods. At the same time, average loan amount increased from \$94 to \$ 127.7 thousand, implying a raise in LIR of 50.1 p.p. The estimated treatment effects between 39.7 and 43.3 p.p., obtained in the models with controls, represent 79% to 86% of the adjustment, which translates to an additional debt of \$26 to \$28 thousand for each borrower.

A more detailed investigation of the risk adjustment is attained when I estimate the same models splitting the sample between loans *unsold* in the same year of origination and loans *sold*. The results are provided by Table 15 for models with full controls. I find that the treatment effect on LIR for affected banks is positive for both groups, precisely estimated, but substantially higher for loans *unsold* in the same year, over the full distribution of quantiles. The average treatment effect for unsold loans is between 57.0 to 60.8 p.p., while for sold loans it is in the range of 21.1 to 26.0 p.p., less than half the magnitude. This reveals that the introduction of the SLR rule led affected banks to intentionally hold riskier loans on their portfolio for more time, while they increased risk-taking overall on the class of higher priced loans. Heterogeneous effects are again verified, and increasing with the level of risk. For example, for unsold loans, the treatment effect for the 20th quantile is between 23.7 to 37.3 p.p. and between 75.6 to 86.3 p.p. for the 80th quantile. The findings reinforces the initial hypothesis that binding minimum leverage ratios incentivized banks to increase risk-taking.

Next, I explore how the SLR rule adoption affected loan spread on higher priced originated loans by covered banks, and the results are provided in Table 13. The average treatment effect is positive, in the range of 0.5260 to 0.6095 p.p., and precisely estimated, in the models with all controls. Affected banks raised the price of lending, in the category of higher priced loans, as a result of the regulatory intervention when I control for risk factors. This finding strengthen the hypothesis that banks were requiring *higher return* on their loans as they increased risk-taking. Again, there is evidence of heterogeneous treatment effects, but this time it is not increasing in the outcome variable. Loans in the lower and middle part of the distribution of spread (cheaper and median price) are more affected by treatment than loans in the upper part. For example, for loans in the 20th quantile, the treatment effect is around 0.61 to 0.72 p.p., while loans at the median were affected by 0.72 to 0.81 p.p. In contrast, loans in the 90th quantile were affected by increases of 0.22 to 0.46 p.p. In my interpretation, this heterogeneity may be related to different price elasticities, or to the amount of increased risk that was taken at each range. Note that the distribution of spread is not necessarily the same as the distribution of loan-to-income ratios[20]. Still, the effect is economically sizable in terms of the average spread. A treatment effect of 0.52 p.p. represents 28% of the average rate spread observed for loans originated by treated banks after treatment.

The estimates obtained from the spread model with no controls are in disagreement to those provided by the other, more complete, specifications. The average treatment effect is negative in the order of -0.3784 p.p., and it becomes stronger in magnitude for the upper tail of the distribution of spread. This would mean that affected banks originated cheaper credit due to treatment. However, assuming that controlling for risk factors on loan origination is critical to the analysis of spread, I interpret this finding as evidence against the model with no controls. An analysis of some aggregate statistics in Table 5 helps to elucidate this point. Government insurance is considered a key factor in loan pricing, with insured loans expected to be cheaper[21]. The share of higher priced government insured loans originated by SLR covered banks rises from 35.4 to 42.6 p.p. between the pre and post treatment periods, while it decreases slightly for non-covered banks. The change in composition by affected banks towards more insured loans results in a drop in the average rate spread from 2.59 to 1.85 percent. However, this does not automatically imply that loans of comparable risk became cheaper. On contrary, it demonstrates that pricing analysis should be conducted adjusting the spread for loan risk, which in my case is obtained in the models with full controls.

Similarly as before, I look at how the SLR rule differentially affected spread in loans *unsold* and *sold* in the year of origination. As shown in Table 16, average treatment effects are positive in both cases, precisely estimated, but substantially higher for loans unsold. Depending on the model, they vary between 0.278 to 0.319 p.p. for unsold loans, and between 0.077 to 0.090 p.p. for loans sold. Once more, this finding reaffirms the hypothesis that, at least for this category of loans, affected banks were willing to hold loans of higher return (and risk) for longer time. Heterogeneity in treatment effects follows different patterns depending on the subsample. For loans unsold in the same year, the lower and middle part of the distribution of spread (cheaper and median price) are more affected by treatment. The opposite is true for loans sold, the higher part of the distribution is more affected. It is remarkable to observe how the covariates model fails to explain the variability of spread for

[20]In principle, it is possible to design a multivariate analysis of treatment effects on LIR and spread, over the distribution surface of the outcome variables. The exercise is left to future work.

[21]Government insurance for housing loans can be provided to some borrowers by the Federal Housing Administration, the Veterans Administration, the Farm Service Agency or the Rural Housing Service. Historically, these programs have allowed lower income borrowers to obtain mortgage loans that would otherwise not be affordable. This is an attribute observed in the HMDA dataset, and I represent it as a dummy for *government insured* at loan level.

loans sold, with an *R-squared* between 0.048 to 0.121, while at the same time it fits well for the subsample of unsold loans, reaching an *R-squared* of 0.424 to 0.509. I speculate this may indicate differences in pricing criteria depending on the destination of the loan, but further investigation is left to future work.

At last, I test the alternative hypothesis that the adjustment in risk started in 2012, instead of after 2014, for higher priced loans. This is equivalent to the test carried out in the previous section for the whole sample, where the pre-treatment period is defined as 2010-2011 and the post period covers 2012-2013. Table 17 presents the results for the CIC model with loan-to-income ratio as the outcome variable. Indeed, I find positive average treatment effects for loans originated by affected banks, between 0.061 and 0.095 p.p. in the models with controls. These estimates are four to six times lower in magnitude than the effects estimated when assuming the baseline treatment date. Still, they suggest that banks already started to adjust the risk characteristics of higher priced loans originated early in the period, just after the first announcement of the SLR rule. Logically, it follows that one should interpret the estimates from the baseline assumption, which considers 2011-2013 as the pre-treatment period, as a conservative lower bound of the average treatment effects.

In summary, findings from the risk and spread adjustment analysis on the subsample of higher priced loans offer strong support for the *higher return* hypothesis. Banks affected by the SLR rule increased risk taking given treatment, specially in the upper tail of the distribution of risk, and raised the average spread. Loans hold for longer time in the portfolio of affected banks, that is unsold in the same year of origination, were more affected in terms of increased risk-taking and return. The findings are economically significant and robust to different specifications of the covariates model.

3.4.3 Placebo and robustness tests

Supplementary analysis of some forms can improve the credibility of results obtained in policy evaluation studies (Athey & Imbens, 2017). In this regard, I conduct a placebo test on the changes-in-changes loan level model where I shift the treatment date to a placebo period where no effect is expected. Besides, I also test whether the largest banks in the sample are excessively influential in the results, by re-estimating the baseline CIC model with a restricted sample.

The placebo test repeats the risk-taking analysis but considers two years previously to the first announcement of the SLR rule, from 2010 to 2011, as the observation window and assume that the placebo treatment started in 2011. The same treatment and control groups of banks are assumed, and the sample of loans used for estimation is draw randomly from the full dataset. The results for the placebo test on loan-to-income ratio are displayed in Table 18. As expected, the average placebo effect is statistically not different from zero. The point estimates are all of small magnitude, on the negative side, and the standard deviations are fairly large. The zero placebo effect holds for all model specifications and practically at any quantile. This insignificant placebo effect is consistent with the assumptions for the CIC model, specifically that the distribution of U is constant over time within groups, and that the untreated outcome function $h(u, 0)$ does not change in the pre-treatment period.

In addition, I test the baseline results from the risk-taking analysis for the influence of the largest banks in the sample. As previously noted, due to the nature of the SLR regulation which applies only to the largest banks in the U.S., control and treatment groups differ significantly in terms of average size[22]. Even if I control non-linearly for size in the covariates model used in the changes-in-changes analysis, one may wonder if the results are being driven by the specific reaction of the largest banks in the sample. To confront this concern, I re-estimate the baseline risk-taking CIC model, but ignore all loans originated by the two largest banks in the sample[23]. The results of this exercise are shown in Table 19. The average treatment effects are estimated at positive values, with a very similar magnitude as obtained for the full sample, but with larger standard deviations. This result holds for all models. The treatment effects are statistically different from zero only in part of the quantiles. I conclude that the reaction of the largest treated banks in the sample is an important determinant of the precision of the baseline results. At the same time, the test does not contradict the hypothesis of adjustment in risk-taking due to treatment, and the lack of precision in estimation could be caused by the smaller sample size. In any case, it is clear that the behavior of the largest banks is not the only factor determining the verified change in the risk profile of originated mortgages.

4 County level analysis: house price changes

The previous section found that banks affected by the SLR rule increased risk-taking in mortgage origination after the introduction of the regulation relative to non-affected banks. In the second stage of the analysis, I explore how the adjustment in risk of loan origination implied by the SLR is correlated with future house prices at the local level. The objective is to test for potential effects of regulation on aggregate credit supply and market prices for homes. The rationale is that a positive credit supply shock resulting from the regulatory intervention would be consistent with higher future rates of growth in house prices for geographic areas previously more exposed to lending activity by SLR banks.

For this purpose, I propose a difference-in-differences model with changes in home prices at the county level as the dependent variable, controlling for local economic conditions and price dynamics. A measure of treatment intensity is defined at county level as the ratio of *all mortgage credit* originated by banks subject to SLR normalized to county *annual payroll*. The period of observations is the same as before, from 2011 to 2017, with 2014 out of the sample, and treatment starting in 2015.

Note that I am assuming that causal identification is addressed by the changes-in-changes model estimated at loan level. In this sense, for the difference-in-differences model of house price changes, the increase in credit undertaken by SLR banks due to the introduction of the new regulation is exogenous to the path of home prices. The next subsections detail the econometric specification, describe the findings, and provide some robustness tests.

[22]This is frequently true for macroprudential financial regulation. In general, there is a size cut-off defining the group of institutions which must comply.

[23]The two largest bank holding companies in the sample are JPMorgan Chase & Co and Bank of America Corporation, both of which individually hold more than \$2 trillion in assets as of December 2014. Combined they originate approximately 20.3% of the mortgages in the sample.

4.1 Empirical strategy

The difference-in-differences model is defined for county c at yearly frequency t as follows:

$$\begin{aligned}\Delta y_{c,t} =& \alpha_c + \alpha_t + \gamma_1 \sum_{j=1}^{J} \Delta y_{c,t-j} + \gamma_2 \sum_{j=1}^{J} X_{c,t-j} \\ &+\beta_1 Credit_{c,t-1} + \beta_2 Credit^{SLRbanks}_{c,t-1} + \beta_3 (Credit^{SLRbanks}_{c,t-1} * Post) + \varepsilon_{c,t}\end{aligned} \tag{12}$$

where $\Delta y_{c,t}$ is the change in the *house price index* in county c time t, in log differences; α_c and α_t are county and time fixed effects, respectively; the vector $X_{c,t}$ contains the economic variables changes in *employment* and in *annual payroll*, both in log differences, and household *debt-to-income ratio* in levels.

The measure $Credit_{c,t}$ is the ratio of *all mortgage credit* originated by banks in the sample over county *annual payroll*. The variable is normalized in order to account for county relative income. Likewise, $Credit^{SLR}_{c,t}$ is the same ratio but only considering credit originated by SLR covered banks. The dummy $Post$ is set to one in the periods after treatment starts, and zero before that. The error term $\varepsilon_{c,t}$ is assumed to be normally distributed.

The main interest lies in the estimated coefficient β_3, in the interaction between credit originated by SLR covered banks and post treatment period. The hypothesis of a positive β_3 implies that the intensity of aggregate change in credit originated by treated banks, after treatment, is positively correlated with future increases in local house prices. This finding, if confirmed, would suggest a channel from capital regulation to house prices via an aggregate credit supply shock. This dynamic panel model can be estimated consistently by ordinary least squares if we explicitly estimate the dummies α_c, or by using the Arellano-Bond Generalized Method of Moments (GMM) estimator (Arellano & Bond, 1991).

4.2 Results: from loan level adjustment to house prices

I estimate four versions of the model in Equation (12), and the results are shown in Table 20. The first two models (columns) ignore lags of the dependent variable, in contrast to the remaining models which include the dynamic component. Column (1) is a simple ordinary least squares regression, which also ignores county and time fixed effects. Column (2) represents a panel fixed effects (FE), estimated with the standard "within differences" estimator. Columns (3) is a dynamic panel with two lags of the dependent variable, saturated with dummies for each county, and estimated by ordinary least squares. Lastly, column (4) is a dynamic panel of one lag estimated by the GMM approach of Arellano and Bond (1991). All specifications include the same set of time-varying economic controls at county level. The sample period is 2012 to 2017, and the frequency of observation is yearly. Columns (1) to (3) ignore the year 2014 in the same spirit of the loan level analysis as treatment started in September of that year. The GMM estimation in column (4) includes 2014 as non treatment period but drops 2012, as it uses previous lags the dependent variable as instruments for $t-1$. In this sense, considering that banks could have reacted during 2014, the findings of column (4) can be interpreted as a lower bound of the treatment effect.

I find a positive treatment effect across all specifications. Treatment intensity at county level, that is, an increase in credit relative to county income by banks affected by the SLR rule, leads to higher future house prices. The positive effect is precisely estimated and statistically significant for all models, except for column (4)[24]. Using either Akaike or Bayesian information criteria as measures of model comparison across the first three specifications, I find that the preferred model is column (3)[25]. This highlights the importance of the dynamic component of price changes.

The magnitude of the treatment effect is economically significant as well. Considering the preferred specification, a one percentage point raise in credit relative to income corresponds to an increase of 0.26 percent in home prices in the following year, and a long run increase of 0.21 percent[26]. In the last section, I have estimated the average treatment effect of policy change, that is the introduction of the SLR rule, to be between 7.77 to 8.88 percentage points in loan to income, at the loan level. Loosely speaking, and considering this effect as the average across counties, this would imply that policy change on aggregate had an average effect of lifting home prices by 1.64 to 1.88% over the period. For the other model specifications, the treatment effect is also positive however lower in magnitude. Overall, the findings of this section suggest that the adjustment in risk-taking verified at loan level is consistent with a positive credit supply shock, which translated in higher future house price growth at local level.

Regarding the other coefficient estimates, I find a positive correlation between annual payroll and future changes in home prices, as expected. Household debt-to-income is negatively correlated with changes in home prices. This means that counties with lower levels of initial debt have experienced higher home price increases, which reinforces a possible role for credit. Changes in employment is not found to be statistically correlated with home price changes.

4.3 Robustness tests

As typical in the difference-in-differences literature, I test for parallel trends in the house prices model. I consider the period 2011 to 2013, previously to treatment introduction. The null hypothesis is of no trend in the correlation between credit originated by SLR banks relative to county income and changes in home prices over time. The findings are in Table (21), for two specifications of the panel fixed effects model. Column (2) considers the dynamic component while the first column does not[27]. Again the dynamic specification is the preferred one by measures of information criteria. The test do not reject the null, as the interaction between $Credit^{SLR}_{c,t}$ and the time trend is estimated very close to zero and it is statistically insignificant.

[24]Note that the specification in the column (4) also estimates coefficients for other covariates with less precision. I speculate this could be due to the shorter sample span or to the inclusion of the year 2014 as a pre-treatment. In any case, the point estimate for the treatment effect is on the positive side, while not statistically significant.

[25]GMM estimation of model (4) is not based on model likelihood, and thus do not provide an information criteria.

[26]The long run correlation considers the dynamics estimated on the autocorrelation coefficients.

[27]The models in Table (21) are equivalent to columns (2) and (3) on the last subsection.

5 Concluding remarks and policy implications

I have investigated how the adoption of the Supplementary Leverage Ratio (SLR) rule in the U.S. have impacted risk-taking and loan spread in the mortgage market. I show that banks affected by the new requirement adjusted origination towards mortgages of higher risk after the final SLR rule was announced, when compared to similar banks not subject to the rule, even after controlling for observed risk factors. The increased risk-taking effect is substantially stronger for a subsample of mortgages classified as higher priced, where banks also adjusted origination for higher loan spread. The findings are consistent with theoretical models of banks' portfolio choice under leverage ratio constraints. Banks shift their asset holdings to a combination of higher risk-return when leverage ratios are binding. Further, I show that the aggregate credit supply shock implied by the raise in loan level risk-taking is correlated with future house price increases at county level. In this last section, I discuss the contributions of the findings to the current debate on financial regulation and suggest avenues for future research.

Among proposals for enhancing financial regulation, some authors advocate shifting the focus from controlling banks' asset risk to implementing simpler, higher and non risk-based capital requirements (Haldane, 2012; Miller, 2016). This change aims to increase the "skin in the game" of bankers and to alter their risk-taking incentives, while reducing regulation complexity and the opportunities for regulatory arbitrage. Admati (2014) and Admati and Hellwig (2013) suggest that minimum equity ratios for banks should be set in the range of 20 to 30% of total assets. These values are draw from pre-FDIC historical evidence, when the lack of governmental safety net and the double liability faced by some banks created sufficient market discipline for banks to hold substantially more equity than in the modern era. Admati and Hellwig (2013) stress that a common defense of bankers against higher equity requirements is that they would restrict bank lending and reduce economic growth. According to the authors, these claims are invalid, as many others made in the debate about capital regulation. The findings of my paper offer empirical support for Admati and Hellwig's (2013) argument and contradict common claims of the banking industry against higher leverage limits. They show that raising the minimum leverage ratio would not necessarily induce a reduction in credit supply. On the contrary, for credit originated in the mortgage market I have observed increased risk-taking at loan level, higher aggregate volume of originated credit and higher future house prices as effects of the adoption of a tighter leverage ratio.

A necessary note of caution regards the conditions under which the observed results should hold. Recall that in Acosta-Smith et al. (2018) banks react to the binding leverage ratio by raising equity levels, and the adjustment in asset risk comes as a result of the slack in the risk-based capital requirement. Furfine (2001), on the other hand, indicates that an alternative reaction of banks constrained by a leverage ratio could be to deleverage by decreasing total asset size and the amount of debt. The expected reaction of banks between these two different predicted outcomes should be related to the marginal cost of raising equity, to state of the economy (e.g. credit demand and future expectations), and to issues of corporate strategy. The Supplementary Leverage Ratio was adopted in a relatively favorable economic environment, between 2012 and 2018, which probably incentivized banks to raise equity instead of shrinking size. Thus, the results observed in this paper may not hold for policy changes which raise leverage ratios during recessions, or under worst states of the economy.

Finally, the results obtained so far open various opportunities for future research. I have investigated effects of leverage regulation in credit supply to the mortgage market, but other forms of credit could have been differently impacted. In particular, lending to the corporate sector involves more complex frictions and information asymmetries. It would be interesting to study whether the binding leverage ratio led banks to adjust the origination of corporate credit in similar ways as it was verified in mortgages, and if relationship lending played any role. Still on this topic, recent literature has recognized that bank capital is a determinant factor in the matching between banks and credit dependent firms (Schwert, 2018). One wonders if the raise in equity levels resulting from the leverage ratio constraint induced any changes in previous matching arrangements. Furthermore, the cost of borrowing is known to be related with the degree of competition in the banking sector (Rice & Strahan, 2010). Until now, my analysis has abstracted from these issues. It would be valuable to investigate how the degree of local competition interacted with the adjustments in risk-taking and pricing of credit verified in my research. At last, to the extend that raising risk-taking in mortgages induced higher borrowers' leverage, it would be fruitful to investigate how this effect translates to future default rates experienced by affected banks, once a negative shock to household income, such as a recession, materializes.

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Figure 1: Leverage Ratios from 2010 to 2017.
This figure plots the average tier 1 leverage ratios (*tier 1 capital / total assets*) over time for banks in the treatment (red line) and control groups (blue dotted line). The treatment group is composed of banks subject to Supplementary Leverage Ratio (SLR) rule active in the home mortgage market, while comparable banks form the control group. Dotted vertical line in 2014/q3 marks the publication of the final SLR rule. Source: FRY-9C.

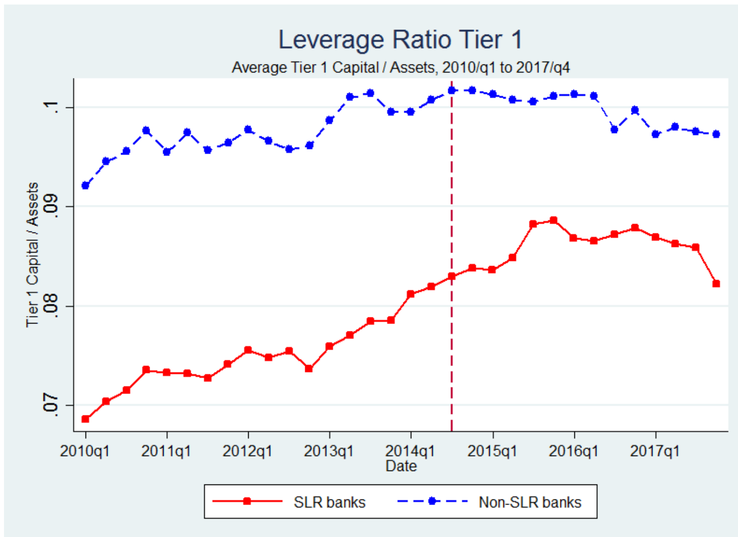


Figure 2: Loan-to-income ratios on home mortgages from 2008 to 2017.
This figure plots the average loan-to-income ratios (LIR) of originated home purchase loans over time for banks in the treatment (red line) and control groups (blue dotted line). The treatment group is composed of banks subject to Supplementary Leverage Ratio (SLR) rule active in the home mortgage market, while comparable banks form the control group. Dotted vertical line in 2014 marks the publication of the final SLR rule. Source: HMDA.

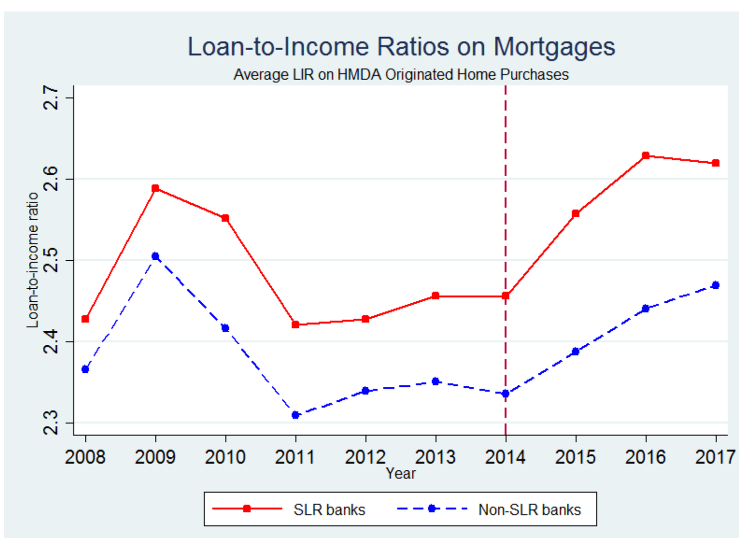
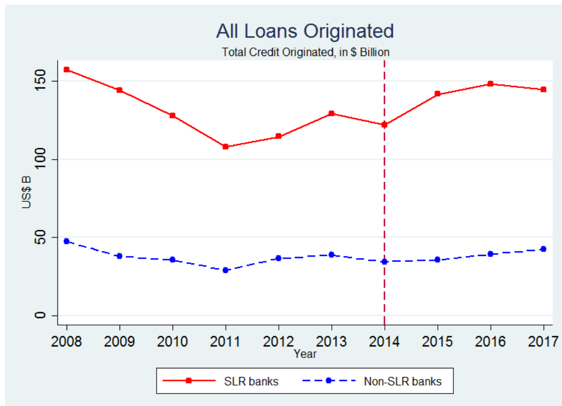
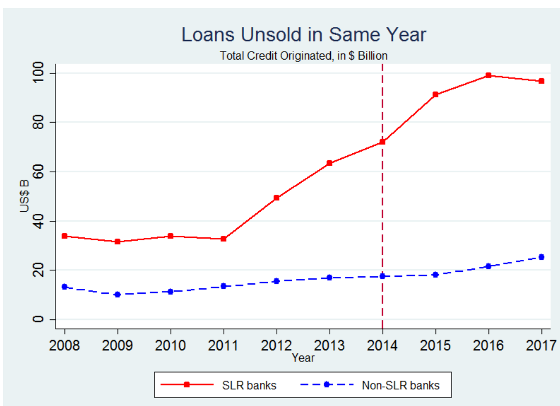


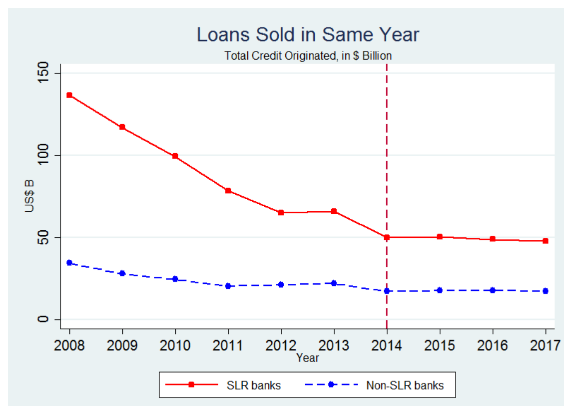
Figure 3: Aggregate amount of home mortgages originated from 2008 to 2017.
This figure plots the aggregate amount, in US$ Billion, of originated home purchase loans over time for banks in the treatment (red line) and control groups (blue dotted line). Panel (a) includes all originated loans; Panel (b) represents only loans unsold in the same year of origination; and Panel (c) represents only loans sold. The treatment group is composed of banks subject to Supplementary Leverage Ratio (SLR) rule active in the home mortgage market, while comparable banks form the control group. Dotted vertical line in 2014 marks the publication of the final SLR rule. Source: HMDA.



(a) All loans originated

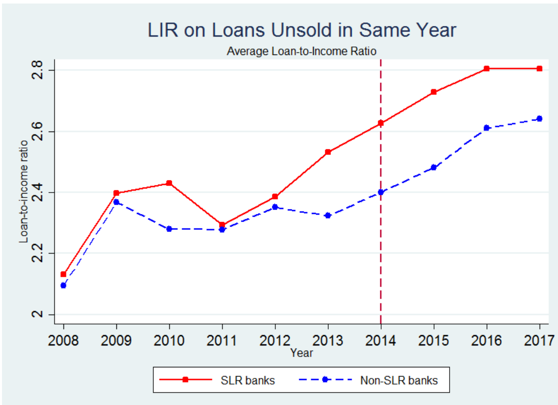


(b) Loans unsold in same year

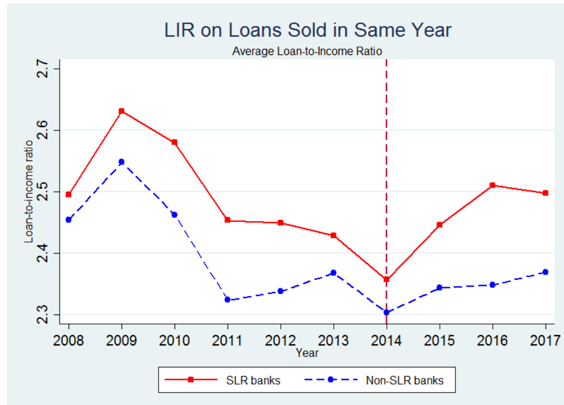


(c) Loans sold in same year

Figure 4: Loan-to-income ratios on unsold and sold home mortgages from 2008 to 2017.
This figure plots the average loan-to-income ratios (LIR) of originated home purchase loans over time for banks in the treatment (red line) and control groups (blue dotted line). Panel (a), left side, represents only loans unsold in the same year of origination, while Panel (b), right side, represents only loans sold. The treatment group is composed of banks subject to Supplementary Leverage Ratio (SLR) rule active in the home mortgage market, while comparable banks form the control group. Dotted vertical line in 2014 marks the publication of the final SLR rule. Source: HMDA.



(a) LIR on loans unsold in same year



(b) LIR on loans sold in same year

Table 1: Six year timeline of SLR implementation.
Source: Federal Reserve Board publications; Choi, Holcomb and Morgan (2018).

Date	Event
January 2012	U.S. regulators propose SLR
July 2013	SLR finalized, enhanced SLR (eSLR) proposed
April 2014	eSLR finalized, revisions to denominator of SLR proposed
September 2014	SLR final rule published
January 2015	Mandatory disclosures of SLR
January 2018	SLR and eSLR compliance
April - May 2018	Changes proposed to eSLR requirements

Table 2: Sample of Bank Holding Companies.
Bank holding companies subject to the Supplmentary Leverage Ratio (SLR) active in the home mortgage market (left panel) define the treatment group. Comparable institutions not subject to SLR (right panel) form the control group. Total Assets in USD Billion as of Dec/2014. Source: FRY-9C.

	SLR group			**Non-SLR group**	
	Bank Holding Company	Total Assets		Bank Holding Company	Total Assets
1	JPMorgan Chase & Co	2,573	1	Suntrust Bk	190
2	Bank Of Amer Corp	2,107	2	BB&T Corp	187
3	Citigroup	1,842	3	Fifth Third Bc	139
4	Wells Fargo & Co	1,687	4	Citizens Fncl Grp	133
5	U S BC	403	5	Regions FC	120
6	PNC Fncl Svc Group	345	6	BMO Fncl Corp	116
7	Capital One FC	309	7	MUFG Amers Holds Corp	114
8	Hsbc N Amer Holds	290	8	M&T Bk Corp	97
9	TD Bk US HC	248	9	Keycorp	94
			10	BNP Paribas USA	90
			11	BBVA Compass Bshrs	83
			12	Huntington Bshrs	66
			13	Zions BC	57

Table 3: Capitalization and bank characteristics before and after treatment.
Average bank capitalization and characteristics before and after the release of final rule for Supplementary Leverage Ratio (SLR), for banks in the treatment (SLR banks) and control groups (Non-SLR). The treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. Averages are taken from quarterly reported data. Period before treatment is 2011/q1 to 2013/q4, period after is 2015/q1 to 2017/q4. Year of 2014 is taken out of sample due to the timing of SLR announcements. Source: FRY-9C and FRY-15.

	SLR Banks			Non-SLR Banks		
	Before	After	Change	Before	After	Change
Capital Ratios (%)						
Risk-Based Capital Ratio	15.58	16.37	0.78	14.61	14.29	-0.32
Risk-Based Capital Ratio Tier 1	12.03	13.51	1.48	11.80	11.97	0.17
Leverage Ratio Tier 1	7.74	9.11	1.37	9.38	9.57	0.18
RWA / Total Assets	64.75	68.80	4.05	79.38	80.24	0.86
Asset Composition, Liability and Profitability Ratios (%)						
Loans-to-Assets Ratio	48.75	47.98	-0.77	66.61	65.80	-0.81
Liquid Assets Ratio	26.03	21.62	-4.41	14.90	15.92	1.01
Trading Assets Ratio	7.57	7.15	-0.42	1.18	1.78	0.60
Securities-to-Assets Ratio	18.22	13.64	-4.58	13.71	14.04	0.34
Loans-to-Deposits Ratio	102.16	93.80	-8.36	91.40	90.54	-0.86
ROE	4.72	4.89	0.17	3.65	4.11	0.46
Net Income-to-Assets Ratio (ROA)	0.52	0.57	0.04	0.42	0.51	0.08
Interest Income-to-Assets Ratio	2.19	1.93	-0.26	2.11	1.83	-0.29
Non Interest Income-to-Assets Ratio	1.14	1.08	-0.06	0.95	0.90	-0.05
Loan Portfolio Ratios (%)						
Share of Loans Secured by Real Estate	46.46	38.80	-7.66	55.14	48.30	-6.84
Share of Commercial & Industrial Loans	15.10	19.07	3.97	24.01	27.83	3.82
Loans Past Due Ratio	0.18	0.26	0.08	0.28	0.35	0.07
Charge-offs Ratio	1.30	0.65	-0.64	0.68	0.24	-0.43
Number of observations	108	98		156	155	

Table 4: Aggregate home mortgage credit and number of loans originated from 2008 to 2017.

Aggregate credit originated for home purchase loans by SLR covered and non-covered banks in the sample. Panel (A) shows statistics for all loans, while Panel (B) considers only loans classified as "higher priced". Source: HMDA.

Year	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Panel A: All loans										
Amount originated (USD B)	217.59	186.23	168.57	144.96	150.95	167.72	156.41	177.10	187.33	186.89
- by SLR banks	170.25	148.41	133.03	111.33	114.43	128.99	121.99	141.56	148.03	144.48
- by non-SLR banks	47.33	37.82	35.55	33.63	36.52	38.73	34.42	35.54	39.29	42.42
Share of amount by SLR banks (%)	78.2	79.7	78.9	76.8	75.8	76.9	78.0	79.9	79.0	77.3
Amount unsold in first year (USD B)	46.91	41.50	44.99	46.15	64.84	80.17	89.30	109.38	120.77	122.02
- by SLR banks	33.80	31.47	33.78	32.81	49.40	63.32	71.94	91.34	99.18	96.80
- by non-SLR banks	13.12	10.04	11.21	13.34	15.44	16.85	17.36	18.04	21.58	25.22
Unsold as share of total amount (%)	21.6	22.3	26.7	31.8	43.0	47.8	57.1	61.8	64.5	65.3
- for SLR banks	19.9	21.2	25.4	29.5	43.2	49.1	59.0	64.5	67.0	67.0
- for non-SLR banks	27.7	26.5	31.5	39.7	42.3	43.5	50.4	50.8	54.9	59.5
Number of loans originated (1,000s)	912	854	729	630	593	589	507	525	515	495
- by SLR banks	689	667	560	474	433	429	371	392	381	360
- by non-SLR banks	223	186	168	156	160	160	136	133	134	135
Share of quantity by SLR banks (%)	75.5	78.2	76.9	75.2	73.0	72.8	73.1	74.7	73.9	72.8
Panel B: Higher priced (HP) loans										
Amount of HP originated (USD B)	10.30	4.36	1.14	1.35	1.07	1.98	2.28	1.36	1.64	1.62
- by SLR banks	7.46	3.20	0.71	0.67	0.50	1.01	1.61	0.93	1.22	1.21
- by non-SLR banks	2.83	1.16	0.43	0.68	0.57	0.97	0.67	0.43	0.41	0.41
Share of HP amount by SLR banks (%)	72.5	73.4	62.3	49.6	46.6	51.1	70.7	68.3	74.9	74.7
Amount of HP unsold in first year (USD B)	3.01	1.45	0.88	0.91	0.78	0.92	0.72	0.43	0.46	0.36
- by SLR banks	1.91	0.98	0.52	0.47	0.31	0.38	0.39	0.15	0.24	0.16
- by non-SLR banks	1.10	0.46	0.36	0.43	0.47	0.54	0.32	0.28	0.22	0.19
Unsold as share of total HP amount (%)	29.2	33.2	76.8	67.1	72.7	46.6	31.5	31.5	28.2	22.1
Number of HP loans originated (1,000s)	67	30	11	12	10	15	18	11	12	12
- by SLR banks	48	23	8	8	7	9	13	8	9	9
- by non-SLR banks	20	7	3	5	4	6	5	3	3	3
Share of HP quantity by SLR banks (%)	70.6	75.7	74.5	60.9	65.2	57.7	72.0	73.0	76.8	76.2

Table 5: Originated loans characteristics before and after treatment.
Average characteristics of originated home purchase loans, before and after the release of final rule for the Supplementary Leverage Ratio (SLR), for banks in the treatment (SLR banks) and control groups (Non-SLR). Panel (A) shows statistics for all loans, while Panel (B) considers only loans classified as "higher priced". Averages are taken from yearly reported data. Period before treatment is 2011 to 2013, period after is 2015 to 2017. Year of 2014 is taken out of sample due to the timing of SLR announcements. Source: HMDA.

	SLR Banks			Non-SLR Banks		
	Before	After	Change	Before	After	Change
Panel A: All loans						
Loan characteristics						
Loan-to-Income ratio (%)	244.47	261.59	17.12	233.51	243.93	10.42
Loan-to-Income ratio (%) wins.	243.37	260.28	16.90	232.76	243.36	10.60
Loan amount (USD 1,000s)	260.8	377.0	116.3	226.2	291.7	65.5
Borrower income (USD 1,000s)	131.2	172.1	40.9	125.9	146.2	20.3
Share of government insured (%)	33.15	11.57	-21.58	28.91	16.25	-12.66
Share of female borrowers (%)	26.39	25.85	-0.54	27.09	27.26	0.17
Share of non-white borrowers (%)	23.99	27.57	3.58	21.71	23.95	2.24
Share of loans unsold in same year (%)	26.28	41.64	15.36	32.50	36.67	4.17
Share of "higher priced" loans (%)	1.72	2.35	0.63	3.19	2.13	-1.07
Economic characteristics and demographics of loan location						
Population	5,552	5,496	-56	5,543	5,375	-167
Median family income (USD)	70,636	73,439	2,803	65,812	67,932	2,120
House price index	180.4	236.7	56.3	169.5	209.0	39.6
House price index y-o-y change (%)	1.11	6.17	5.05	0.59	5.80	5.21
HH debt-to-income ratio in county (%)	188.9	167.2	-21.8	180.9	161.1	-19.8
HH debt-to-income ratio change in county (%)	-3.2	-1.4	1.9	-3.3	-1.1	2.2
Number of observations (1,000s)	1,337	1,118		449	399	
Panel B: Higher priced loans						
Loan characteristics						
Loan-to-Income ratio (%)	159.33	209.47	50.14	194.15	196.93	2.78
Loan-to-Income ratio (%) wins.	159.32	209.46	50.14	194.05	196.87	2.81
Loan amount (USD 1,000s)	94.0	127.7	33.7	150.4	146.1	-4.3
Borrower income (USD 1,000s)	74.1	72.7	-1.4	106.9	94.7	-12.2
Share of government insured (%)	35.44	42.63	7.19	39.66	37.87	-1.79
Share of female borrowers (%)	32.36	33.51	1.16	29.62	32.97	3.35
Share of non-white borrowers (%)	17.80	24.20	6.40	21.95	20.82	-1.13
Share of loans unsold in same year (%)	55.86	15.08	-40.78	53.64	44.75	-8.89
Rate spread (%)	2.59	1.85	-0.74	2.00	1.87	-0.13
Economic characteristics and demographics of loan location						
Population	5,333	5,175	-158	5,294	4,985	-309
Median family income (USD)	61,468	64,933	3,465	62,233	62,474	241
House price index	170.2	204.4	34.3	162.8	198.4	35.6
House price index y-o-y change (%)	1.49	5.80	4.32	0.60	6.10	5.50
HH debt-to-income ratio in county (%)	175.8	160.1	-15.7	174.2	153.2	-21.0
HH debt-to-income ratio change in county (%)	-2.7	-0.3	2.4	-3.5	-1.0	2.5
Number of observations (1,000s)	23	26		14	8	

Table 6: Effect of the SRL rule on risk-taking in originated home purchase loans: baseline changes-in-changes estimation results.

This table presents changes-in-changes estimates of the effect of the Supplementary Leverage Ratio (SLR) rule on loan-to-income ratio of mortgages originated by treated banks, across the distribution of outcomes. The treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. Period before treatment is 2011 to 2013, period after is 2015 to 2017. Year of 2014 is taken out of sample due to the timing of SLR final rule announcements. This estimation uses 200,000 observations randomly sampled from the full dataset of loans.

	Loan-to-income ratio			
	(1)	(2)	(3)	(4)
CIC estimate (quantile)	No Controls	State FE	MSA FE	County FE
Mean	0.0857	0.0812	0.0888	0.0776
	(0.0123)	(0.0123)	(0.0159)	(0.0119)
q10	0.0193	0.0058	0.0198	0.0059
	(0.0144)	(0.0184)	(0.0234)	(0.0182)
q20	0.0478	0.0462	0.0665	0.0511
	(0.0147)	(0.0163)	(0.0213)	(0.015)
q30	0.0535	0.0594	0.0717	0.0590
	(0.0142)	(0.0157)	(0.0199)	(0.0153)
q40	0.0650	0.0773	0.0936	0.0744
	(0.0151)	(0.0147)	(0.019)	(0.0143)
q50	0.0777	0.0855	0.0853	0.0795
	(0.0143)	(0.0144)	(0.0194)	(0.0151)
q60	0.0734	0.0907	0.0839	0.0877
	(0.0164)	(0.0154)	(0.0194)	(0.015)
q70	0.1118	0.1003	0.0945	0.0973
	(0.0183)	(0.0163)	(0.0211)	(0.017)
q80	0.1364	0.1136	0.1196	0.1215
	(0.0194)	(0.0196)	(0.0218)	(0.019)
q90	0.2168	0.1436	0.1616	0.1418
	(0.0321)	(0.0225)	(0.0255)	(0.0211)
Bank controls	N	Y	Y	Y
Loan level controls	N	Y	Y	Y
Economic controls	N	Y	Y	Y
Observations	200,000	200,000	119,832	199,999
Bootstrap size	1,000	1,000	1,000	1,000
Covariates regression:				
R-squared	NA	0.1597	0.1542	0.1891
AIC	NA	603,710	355,212	596,567
BIC	NA	603,873	355,367	596,731

Table 7: Covariates regression from baseline changes-in-changes risk-taking model.
This table presents estimates of the covariates regression for the loan-to-income ratio changes-in-changes model from Table 6. Dummies *T=1* and *G=1* indicate, respectively, the post-treatment period and the treatment group. This estimation uses the same sample of loans from Table 6.

	Loan-to-income ratio		
	State FE	MSA FE	County FE
Total assets (log)	0.2043	-0.0824	-0.1206
	(0.4494)	(0.2782)	(0.2198)
Total assets (log) sq	-0.0056	0.0012	0.0023
	(0.0111)	(0.0069)	(0.0055)
Trading assets ratio	0.9804	0.7922	0.8131
	(0.1125)	(0.1739)	(0.1449)
Liquid assets ratio	-0.1726	-0.1603	-0.2238
	(0.142)	(0.1435)	(0.0968)
Net income-to-assets	2.6359	3.0305	3.2234
	(1.4463)	(1.3606)	(0.8987)
Government insured loan	0.6012	0.6102	0.6239
	(0.0358)	(0.0213)	(0.0123)
Female borrower	0.1528	0.1733	0.1541
	(0.0102)	(0.011)	(0.0088)
Non-White borrower	0.0494	0.0111	0.0262
	(0.0249)	(0.0118)	(0.0112)
Population (log)	0.1121	0.1133	0.0877
	(0.0154)	(0.0159)	(0.0134)
Median family income (log)	0.3055	-0.3838	-0.0889
	(0.1078)	(0.14)	(0.0931)
HPI	0.0009	0.0042	0.0006
	(0.0001)	(0.0003)	(0.0001)
HPI change y-o-y	1.4765	0.4122	1.2058
	(0.238)	(0.1274)	(0.0928)
HH debt-to-income ratio	0.1160	-0.0889	0.0002
	(0.0953)	(0.0707)	(0.0257)
HH debt-to-income ratio change	0.1568	0.1616	0.0235
	(0.0847)	(0.0692)	(0.0273)
T = 1	0.0644	0.0478	0.1093
	(0.0158)	(0.0133)	(0.0118)
G = 1	-0.0431	-0.0245	-0.0353
	(0.0397)	(0.0335)	(0.0222)
Observations	200,000	119,832	199,999
R-squared	0.1597	0.1542	0.1891
AIC	603,710	355,212	596,567
BIC	603,873	355,367	596,731

Table 8: Comparing the effect of the SRL rule on risk-taking between unsold and sold loans. This table presents changes-in-changes estimates of the effect of the Supplementary Leverage Ratio (SLR) rule on mortgages originated by treated banks, across the distribution of loan-to-income ratio. Columns (1) to (3) consider only loans not sold in the same year of origination, while columns (4) to (6) consider loans sold in the same year of origination. The treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. Period before treatment is 2011 to 2013, period after is 2015 to 2017. Year of 2014 is taken out of sample due to the timing of SLR final rule announcements. This estimation uses 200,000 observations randomly sampled from the full dataset of loans.

	Loan-to-income ratio					
	Unsold in same year			Sold in same year		
	(1)	(2)	(3)	(4)	(5)	(6)
CIC estimate (quantile)	State FE	MSA FE	County FE	State FE	MSA FE	County FE
Mean	0.0118	0.0358	0.0134	0.1198	0.1169	0.1089
	(0.0229)	(0.0316)	(0.0216)	(0.0152)	(0.0184)	(0.0145)
q10	-0.0500	-0.0044	-0.0394	0.0672	0.0375	0.0419
	(0.0318)	(0.0411)	(0.036)	(0.0236)	(0.027)	(0.0237)
q20	-0.0105	0.0321	-0.0014	0.0822	0.0994	0.0713
	(0.0289)	(0.0369)	(0.0315)	(0.02)	(0.0255)	(0.02)
q30	-0.0036	0.0424	0.0069	0.0994	0.0917	0.0827
	(0.028)	(0.0356)	(0.0285)	(0.0188)	(0.0205)	(0.018)
q40	0.0203	0.0628	-0.0081	0.1098	0.1187	0.1064
	(0.0275)	(0.0348)	(0.0267)	(0.0179)	(0.0218)	(0.0171)
q50	0.0037	0.0379	0.0023	0.1247	0.1167	0.1118
	(0.0283)	(0.0363)	(0.0267)	(0.0186)	(0.0214)	(0.0181)
q60	0.0370	0.0540	0.0467	0.1194	0.1166	0.0998
	(0.0301)	(0.0394)	(0.028)	(0.0191)	(0.0228)	(0.0188)
q70	0.0507	0.0381	0.0521	0.1112	0.1071	0.0973
	(0.0296)	(0.0438)	(0.0295)	(0.0224)	(0.026)	(0.0201)
q80	0.0535	0.0222	0.0449	0.1286	0.1378	0.1377
	(0.0358)	(0.0452)	(0.0326)	(0.0254)	(0.0277)	(0.0229)
q90	0.0186	0.0339	0.0241	0.1784	0.2135	0.1876
	(0.0385)	(0.0514)	(0.0358)	(0.0305)	(0.0378)	(0.0283)
Bank controls	Y	Y	Y	Y	Y	Y
Loan level controls	Y	Y	Y	Y	Y	Y
Economic controls	Y	Y	Y	Y	Y	Y
Observations	66,666	36,009	66,666	133,334	83,823	133,333
Bootstrap size	1,000	1,000	1,000	1,000	1,000	1,000
Covariates regression:						
R-squared	0.1678	0.1730	0.2133	0.1553	0.1541	0.1898
AIC	21,284	9,824	21,739	13,590	5,429	13,335
BIC	21,912	13,076	41,529	14,266	9,033	39,992

Table 9: Test for early treatment hypothesis in risk-taking.

This table presents changes-in-changes estimates of the effect of an early treatment on loan-to-income ratio of mortgages originated by SLR covered banks, across the distribution of outcomes. The early treatment timing is defined as January/2012, when the first proposal of the Supplementary Leverage Ratio (SLR) rule was published. Period before treatment is 2010 to 2011, period after is 2012 to 2013. As before, the treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. This estimation uses 116,635 observations randomly sampled from the full dataset of loans.

	Loan-to-income ratio			
	(1)	(2)	(3)	(4)
CIC estimate (quantile)	No Controls	State FE	MSA FE	County FE
Mean	-0.0350	0.0101	0.0016	0.0145
	(0.0158)	(0.0183)	(0.0209)	(0.0181)
q10	0.0015	0.0213	0.0268	0.0201
	(0.0239)	(0.0257)	(0.0273)	(0.0258)
q20	-0.0330	0.0163	-0.0021	-0.0026
	(0.0214)	(0.0245)	(0.0288)	(0.0237)
q30	-0.0543	-0.0396	-0.0362	-0.0192
	(0.0189)	(0.02)	(0.0251)	(0.022)
q40	-0.0591	-0.0186	-0.0210	-0.0089
	(0.0195)	(0.0201)	(0.025)	(0.0217)
q50	-0.0652	-0.0025	-0.0220	-0.0071
	(0.0206)	(0.0227)	(0.0244)	(0.0214)
q60	-0.0626	-0.0033	-0.0098	-0.0080
	(0.0214)	(0.0228)	(0.0243)	(0.0212)
q70	-0.0618	0.0013	0.0151	0.0083
	(0.0213)	(0.0237)	(0.0283)	(0.0232)
q80	-0.0571	-0.0023	-0.0002	0.0182
	(0.024)	(0.0261)	(0.0293)	(0.0234)
q90	0.0231	0.0610	0.0177	0.0656
	(0.0344)	(0.0311)	(0.0348)	(0.0278)
Bank controls	N	Y	Y	Y
Loan level controls	N	Y	Y	Y
Economic controls	N	Y	Y	Y
Observations	116,635	116,635	69,980	116,631
Bootstrap size	1,000	1,000	1,000	1,000
Covariates regression:				
R-squared	NA	0.1718	0.1707	0.2071
AIC	NA	13,846	4,865	13,894
BIC	NA	14,513	8,142	39,298

Table 10: Test for early treatment hypothesis in risk-taking: unsold and sold loans.
This table presents changes-in-changes estimates of the effect of an early treatment on loan-to-income ratio of mortgages originated by SLR covered banks, across the distribution of outcomes. Columns (1) to (3) consider only loans not sold in the same year of origination, while columns (4) to (6) consider loans sold in the same year of origination. The early treatment timing is defined as January/2012, when the first proposal of the Supplementary Leverage Ratio (SLR) rule was published. Period before treatment is 2010 to 2011, period after is 2012 to 2013. As before, the treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. This estimation uses 116,635 observations randomly sampled from the full dataset of loans.

	Loan-to-income ratio					
	Unsold in same year			Sold in same year		
	(1)	(2)	(3)	(4)	(5)	(6)
CIC estimate (quantile)	State FE	MSA FE	County FE	State FE	MSA FE	County FE
Mean	0.0709	0.0074	0.0738	-0.0051	0.0098	-0.0024
	(0.0315)	(0.0466)	(0.0362)	(0.0206)	(0.0227)	(0.0208)
q10	0.0984	0.0481	0.0777	0.0103	0.0250	-0.0050
	(0.044)	(0.0632)	(0.0546)	(0.0262)	(0.0306)	(0.0311)
q20	0.0703	-0.0202	0.0741	0.0034	0.0136	-0.0124
	(0.0471)	(0.0656)	(0.0509)	(0.0263)	(0.0301)	(0.0273)
q30	0.0222	-0.0433	0.0384	-0.0474	-0.0084	-0.0347
	(0.0385)	(0.0549)	(0.0439)	(0.0231)	(0.0286)	(0.0252)
q40	0.0502	-0.0551	0.0330	-0.0281	0.0060	-0.0194
	(0.0418)	(0.0509)	(0.0412)	(0.0233)	(0.0282)	(0.0239)
q50	0.0636	0.0196	0.0545	-0.0181	-0.0009	-0.0294
	(0.0383)	(0.0532)	(0.0428)	(0.0253)	(0.0274)	(0.0247)
q60	0.0715	0.0058	0.0679	-0.0282	-0.0071	-0.0247
	(0.0482)	(0.0578)	(0.045)	(0.026)	(0.0283)	(0.0264)
q70	0.0556	0.0319	0.0370	-0.0156	0.0141	-0.0053
	(0.0454)	(0.066)	(0.0457)	(0.0266)	(0.0294)	(0.0272)
q80	0.0491	0.0147	0.0632	-0.0045	0.0187	-0.0012
	(0.043)	(0.0654)	(0.0464)	(0.0302)	(0.0343)	(0.0287)
q90	0.1048	0.0410	0.1293	0.0198	0.0232	0.0304
	(0.0613)	(0.0807)	(0.063)	(0.0367)	(0.0442)	(0.0341)
Bank controls	Y	Y	Y	Y	Y	Y
Loan level controls	Y	Y	Y	Y	Y	Y
Economic controls	Y	Y	Y	Y	Y	Y
Observations	30,199	16,958	30,197	86,436	53,022	86,434
Bootstrap size	1,000	1,000	1,000	1,000	1,000	1,000
Covariates regression:						
R-squared	0.1540	0.1633	0.2152	0.1821	0.1819	0.2244
AIC	9,182	4,842	10,573	3,997	-167	4,266
BIC	9,756	7,582	26,348	4,644	3,003	27,674

Table 11: Effect of the SRL rule on risk-taking, higher priced loans.
This table presents changes-in-changes estimates of the effect of the Supplementary Leverage Ratio (SLR) rule on loan-to-income ratio of mortgages originated by treated banks, across the distribution of outcomes. Sample is restricted to all loans classified as "higher priced". The treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. Period before treatment is 2011 to 2013, period after is 2015 to 2017. Year of 2014 is taken out of sample due to the timing of SLR final rule announcements.

	Loan-to-income ratio			
	(1)	(2)	(3)	(4)
CIC estimate (quantile)	No Controls	State FE	MSA FE	County FE
Mean	0.4564	0.4331	0.4327	0.3970
	(0.0163)	(0.0208)	(0.024)	(0.0187)
q10	0.1046	0.2070	0.2504	0.2653
	(0.0164)	(0.0212)	(0.0382)	(0.0294)
q20	0.2518	0.2405	0.2790	0.2555
	(0.0219)	(0.0238)	(0.0329)	(0.0274)
q30	0.3912	0.3024	0.2940	0.2854
	(0.0218)	(0.0268)	(0.0327)	(0.0244)
q40	0.5088	0.3603	0.3605	0.3274
	(0.0204)	(0.0238)	(0.0268)	(0.0238)
q50	0.5692	0.4324	0.4414	0.3839
	(0.0238)	(0.0254)	(0.0285)	(0.0231)
q60	0.6028	0.4762	0.5033	0.4277
	(0.0233)	(0.0255)	(0.0296)	(0.0246)
q70	0.6146	0.5396	0.5483	0.4729
	(0.0251)	(0.0276)	(0.029)	(0.0237)
q80	0.6492	0.5845	0.5969	0.5169
	(0.0271)	(0.0297)	(0.0314)	(0.0247)
q90	0.6316	0.6364	0.6143	0.5434
	(0.0383)	(0.032)	(0.0377)	(0.0293)
Bank controls	N	Y	Y	Y
Loan level controls	N	Y	Y	Y
Economic controls	N	Y	Y	Y
Observations	72,096	72,096	47,470	72,095
Bootstrap size	1,000	1,000	1,000	1,000
Covariates regression:				
R-squared	NA	0.2841	0.3172	0.3420
AIC	NA	193,005	125,972	186,917
BIC	NA	193,152	126,113	187,064

Table 12: Covariates regression from changes-in-changes risk-taking model, higher priced loans.

This table presents estimates of the covariates regression for the loan-to-income ratio changes-in-changes model from Table 11. Sample is restricted to all loans classified as "higher priced". Dummies *T=1* and *G=1* indicate, respectively, the post-treatment period and the treatment group. This estimation uses the same sample of loans from Table 11.

	Loan-to-income ratio		
	State FE	MSA FE	County FE
Total assets (log)	0.4130	0.5336	0.3094
	(1.1819)	(0.7356)	(0.4867)
Total assets (log) sq	-0.0095	-0.0122	-0.0067
	(0.0289)	(0.0186)	(0.0123)
Trading assets ratio	-1.7018	-2.2771	-1.8471
	(0.6215)	(0.5268)	(0.3648)
Liquid assets ratio	1.1938	1.6146	1.4591
	(0.5892)	(0.4694)	(0.2991)
Net income-to-assets	7.9721	7.3034	9.2787
	(3.7122)	(1.9247)	(1.6749)
Government insured loan	0.9024	0.9191	0.8992
	(0.0462)	(0.0235)	(0.0179)
Female borrower	0.1694	0.1779	0.1740
	(0.0123)	(0.0104)	(0.0089)
Non-White borrower	-0.0274	-0.0052	-0.0125
	(0.0299)	(0.0222)	(0.0201)
Population (log)	0.1827	0.1537	0.1269
	(0.022)	(0.0138)	(0.0124)
Median family income (log)	-0.1261	-1.0984	-0.3873
	(0.1505)	(0.2836)	(0.1713)
HPI	0.0010	0.0100	0.0028
	(0.0001)	(0.001)	(0.0003)
HPI change y-o-y	2.0706	-0.3447	0.9193
	(0.3172)	(0.3187)	(0.2179)
HH debt-to-income ratio	0.1534	-0.0777	-0.0089
	(0.1631)	(0.1096)	(0.0382)
HH debt-to-income ratio change	0.2152	0.2549	0.0333
	(0.2022)	(0.1325)	(0.0409)
$T = 1$	0.1443	-0.0066	0.0570
	(0.0535)	(0.0367)	(0.0279)
$G = 1$	-0.2728	-0.3729	-0.3305
	(0.1415)	(0.0845)	(0.0557)
Observations	72,096	47,470	72,095
R-squared	0.2841	0.3172	0.3420
AIC	193,005	125,972	186,917
BIC	193,152	126,113	187,064

Table 13: Effect of the SRL rule on loan spread, higher priced loans.
This table presents changes-in-changes estimates of the effect of the Supplementary Leverage Ratio (SLR) rule on spread of mortgages originated by treated banks, across the distribution of outcomes. Sample is restricted to all loans classified as "higher priced". The treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. Period before treatment is 2011 to 2013, period after is 2015 to 2017. Year of 2014 is taken out of sample due to the timing of SLR final rule announcements.

	Loan Spread			
CIC estimate (quantile)	(1) No Controls	(2) State FE	(3) MSA FE	(4) County FE
Mean	-0.3784	0.6095	0.5260	0.5906
	(0.0219)	(0.0207)	(0.0229)	(0.0166)
q10	0.0100	0.7238	0.5819	0.5981
	(0.0051)	(0.0139)	(0.018)	(0.0155)
q20	0.0100	0.7242	0.6083	0.6556
	(0.0051)	(0.008)	(0.0112)	(0.0091)
q30	0.0000	0.7674	0.6531	0.6971
	(0.0077)	(0.0085)	(0.0108)	(0.0081)
q40	-0.0400	0.7982	0.7067	0.7408
	(0.0077)	(0.0092)	(0.0116)	(0.0084)
q50	-0.0900	0.8143	0.7220	0.7558
	(0.0077)	(0.0097)	(0.0112)	(0.0101)
q60	-0.2200	0.7600	0.7265	0.7711
	(0.0153)	(0.0126)	(0.0146)	(0.0116)
q70	-0.5100	0.6944	0.7169	0.7349
	(0.0281)	(0.0239)	(0.0194)	(0.0145)
q80	-0.6700	0.4310	0.5202	0.6263
	(0.0663)	(0.0541)	(0.0535)	(0.0343)
q90	-1.4700	0.4568	0.2196	0.4058
	(0.0536)	(0.1263)	(0.1397)	(0.0723)
Bank controls	N	Y	Y	Y
Loan level controls	N	Y	Y	Y
Economic controls	N	Y	Y	Y
Observations	72,096	72,096	47,470	72,095
Bootstrap size	1,000	1,000	1,000	1,000
Covariates regression:				
R-squared	NA	0.3963	0.3842	0.4562
AIC	NA	153,142	93,062	145,597
BIC	NA	153,289	93,211	145,744

Table 14: Covariates regression from changes-in-changes loan spread model, higher priced loans.

This table presents estimates of the covariates regression for the loan spread changes-in-changes model from Table 13. Sample is restricted to all loans classified as "higher priced". Dummies *T=1* and *G=1* indicate, respectively, the post-treatment period and the treatment group. This estimation uses the same sample of loans from Table 13.

	Loan Spread		
	State FE	MSA FE	County FE
Total assets (log)	1.6518	2.2128	1.3274
	(1.3841)	(0.7699)	(0.5727)
Total assets (log) sq	-0.0583	-0.0708	-0.0492
	(0.0345)	(0.0198)	(0.0145)
Trading assets ratio	0.7347	0.6464	0.8616
	(0.3098)	(0.2878)	(0.192)
Liquid assets ratio	0.6973	0.6526	0.4497
	(0.3025)	(0.2676)	(0.1834)
Net income-to-assets	5.3871	2.3006	1.2098
	(3.6464)	(2.0861)	(1.9526)
Government insured loan	-0.2440	-0.2212	-0.2246
	(0.0238)	(0.0172)	(0.011)
Female borrower	0.0028	0.0115	0.0074
	(0.0062)	(0.0065)	(0.0057)
Non-White borrower	-0.0106	-0.0217	-0.0060
	(0.0119)	(0.0106)	(0.0082)
Population (log)	-0.0138	0.0026	0.0012
	(0.0118)	(0.0103)	(0.009)
Median family income (log)	-0.0146	-0.0939	-0.1836
	(0.1158)	(0.1912)	(0.1092)
HPI	-0.0001	-0.0003	-0.0002
	(0.0001)	(0.0006)	(0.0001)
HPI change y-o-y	-0.2656	-0.0909	0.0049
	(0.3193)	(0.2195)	(0.1646)
HH debt-to-income ratio	-0.1452	-0.1939	-0.0785
	(0.1302)	(0.0914)	(0.0324)
HH debt-to-income ratio change	-0.2776	0.0269	0.0259
	(0.119)	(0.0801)	(0.0296)
T = 1	-0.1016	-0.1080	-0.0871
	(0.0298)	(0.0314)	(0.0201)
G = 1	1.7334	1.5963	1.6285
	(0.1403)	(0.081)	(0.0527)
Observations	72,096	47,470	72,095
R-squared	0.3963	0.3842	0.4562
AIC	153,142	93,062	145,597
BIC	153,289	93,211	145,744

Table 15: Comparing the effect of the SRL rule on risk-taking between unsold and sold loans, higher priced loans.

This table presents changes-in-changes estimates of the effect of the Supplementary Leverage Ratio (SLR) rule on loan-to-income ratio of mortgages originated by treated banks, across the distribution of outcomes. Sample is restricted to all loans classified as "higher priced". Columns (1) to (3) consider only loans not sold in the same year of origination, while columns (4) to (6) consider loans sold in the same year of origination. The treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. Period before treatment is 2011 to 2013, period after is 2015 to 2017. Year of 2014 is taken out of sample due to the timing of SLR final rule announcements.

	Loan-to-income ratio					
	Unsold in same year			Sold in same year		
CIC estimate (quantile)	(1) State FE	(2) MSA FE	(3) County FE	(4) State FE	(5) MSA FE	(6) County FE
Mean	0.5699	0.6080	0.5848	0.2599	0.2121	0.2108
	(0.0346)	(0.039)	(0.0332)	(0.0271)	(0.0334)	(0.0281)
q10	0.1694	0.3018	0.3282	0.1515	0.0387	0.1129
	(0.0376)	(0.0619)	(0.0511)	(0.0302)	(0.0558)	(0.0449)
q20	0.2372	0.3737	0.3444	0.1599	0.0746	0.1593
	(0.0421)	(0.0521)	(0.0451)	(0.0323)	(0.0444)	(0.0387)
q30	0.3342	0.4798	0.4793	0.2143	0.1623	0.1974
	(0.0434)	(0.049)	(0.0405)	(0.0373)	(0.0456)	(0.0374)
q40	0.4319	0.5444	0.5392	0.2523	0.2234	0.2151
	(0.0421)	(0.0478)	(0.0396)	(0.0329)	(0.0402)	(0.0352)
q50	0.5734	0.6298	0.5882	0.2793	0.3030	0.2398
	(0.0447)	(0.045)	(0.039)	(0.0373)	(0.0398)	(0.0313)
q60	0.6574	0.7001	0.6196	0.3081	0.3295	0.2736
	(0.0449)	(0.0492)	(0.0422)	(0.0328)	(0.0383)	(0.0332)
q70	0.7776	0.7516	0.7175	0.3353	0.3123	0.2901
	(0.0474)	(0.0511)	(0.0427)	(0.0358)	(0.0403)	(0.0338)
q80	0.8633	0.8195	0.7561	0.3382	0.3019	0.2682
	(0.0479)	(0.057)	(0.0485)	(0.0427)	(0.0434)	(0.0352)
q90	1.0058	0.9347	0.8584	0.3053	0.2759	0.2595
	(0.0514)	(0.0626)	(0.0516)	(0.0473)	(0.0491)	(0.0436)
Bank controls	Y	Y	Y	Y	Y	Y
Loan level controls	Y	Y	Y	Y	Y	Y
Economic controls	Y	Y	Y	Y	Y	Y
Observations	28,301	18,374	28,300	43,795	29,096	43,795
Bootstrap size	1,000	1,000	1,000	1,000	1,000	1,000
Covariates regression:						
R-squared	0.2523	0.3083	0.3438	0.3221	0.3502	0.3822
AIC	-4,972	-3,399	-4,015	-9,273	-5,966	-8,517
BIC	-4,403	-443	15,754	-8,674	-2,771	13,010

Table 16: Comparing the effect of the SRL rule on spread between unsold and sold loans, higher priced loans.

This table presents changes-in-changes estimates of the effect of the Supplementary Leverage Ratio (SLR) rule on spread of mortgages originated by treated banks, across the distribution of outcomes. Sample is restricted to all loans classified as "higher priced". Columns (1) to (3) consider only loans not sold in the same year of origination, while columns (4) to (6) consider loans sold in the same year of origination. The treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. Period before treatment is 2011 to 2013, period after is 2015 to 2017. Year of 2014 is taken out of sample due to the timing of SLR final rule announcements.

	Loan Spread					
	Unsold in same year			Sold in same year		
CIC estimate (quantile)	(1) State FE	(2) MSA FE	(3) County FE	(4) State FE	(5) MSA FE	(6) County FE
Mean	0.3193	0.2896	0.2780	0.0866	0.0773	0.0900
	(0.0393)	(0.0416)	(0.0345)	(0.0065)	(0.0079)	(0.0063)
q10	0.4709	0.4030	0.3080	0.0060	0.0054	0.0139
	(0.0286)	(0.0331)	(0.0347)	(0.0047)	(0.0065)	(0.0058)
q20	0.4912	0.4752	0.4103	0.0238	0.0211	0.0323
	(0.0252)	(0.0276)	(0.0269)	(0.005)	(0.007)	(0.0057)
q30	0.4421	0.4625	0.4548	0.0399	0.0368	0.0484
	(0.025)	(0.0263)	(0.0277)	(0.0058)	(0.0074)	(0.0063)
q40	0.4526	0.5185	0.4808	0.0556	0.0528	0.0612
	(0.0265)	(0.0289)	(0.0259)	(0.0062)	(0.0077)	(0.0069)
q50	0.3390	0.4528	0.3865	0.0749	0.0810	0.0823
	(0.0383)	(0.0351)	(0.0311)	(0.0072)	(0.0092)	(0.0075)
q60	0.2949	0.2936	0.3512	0.0964	0.0920	0.0918
	(0.0668)	(0.0584)	(0.0389)	(0.0074)	(0.0097)	(0.0077)
q70	0.3394	0.2318	0.3502	0.1222	0.1173	0.1194
	(0.13)	(0.0885)	(0.0555)	(0.0083)	(0.0111)	(0.0085)
q80	0.4030	0.1444	0.2908	0.1548	0.1391	0.1491
	(0.0659)	(0.1146)	(0.0745)	(0.0096)	(0.0118)	(0.0096)
q90	0.4066	0.0461	0.0448	0.1972	0.1545	0.1850
	(0.058)	(0.1126)	(0.095)	(0.0142)	(0.0169)	(0.0136)
Bank controls	Y	Y	Y	Y	Y	Y
Loan level controls	Y	Y	Y	Y	Y	Y
Economic controls	Y	Y	Y	Y	Y	Y
Observations	28,301	18,374	28,300	43,795	29,096	43,795
Bootstrap size	1,000	1,000	1,000	1,000	1,000	1,000
Covariates regression:						
R-squared	0.4246	0.4566	0.5095	0.0482	0.0589	0.1210
AIC	-2,808	-4,705	-2,671	-111,628	-74,706	-110,294
BIC	-2,239	-1,749	17,098	-111,029	-71,511	-88,767

Table 17: Test for early treatment hypothesis in risk-taking, higher priced loans.
This table presents changes-in-changes estimates of the effect of an early treatment on loan-to-income ratio of mortgages originated by SLR covered banks, across the distribution of outcomes. Sample is restricted to all loans classified as "higher priced". The early treatment timing is defined as January/2012, when the first proposal of the Supplementary Leverage Ratio (SLR) rule was published. Period before treatment is 2010 to 2011, period after is 2012 to 2013. As before, the treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group.

	Loan-to-income ratio			
	(1)	(2)	(3)	(4)
CIC estimate (quantile)	No Controls	State FE	MSA FE	County FE
Mean	0.1235	0.0616	0.0951	0.0666
	(0.0217)	(0.0225)	(0.0244)	(0.0213)
q10	0.0158	0.0200	0.0297	0.0311
	(0.017)	(0.0212)	(0.03)	(0.0278)
q20	-0.0041	0.0273	0.0494	0.0355
	(0.026)	(0.026)	(0.031)	(0.0293)
q30	-0.0092	0.0467	0.0889	0.0570
	(0.0285)	(0.0294)	(0.0314)	(0.0288)
q40	0.0211	0.0666	0.0993	0.0683
	(0.0277)	(0.0294)	(0.0336)	(0.0291)
q50	0.0607	0.0781	0.1207	0.0772
	(0.0323)	(0.032)	(0.0346)	(0.0277)
q60	0.1354	0.0853	0.1085	0.0701
	(0.0302)	(0.0294)	(0.0356)	(0.0278)
q70	0.2120	0.0693	0.1066	0.0913
	(0.0271)	(0.0315)	(0.0367)	(0.0291)
q80	0.2667	0.0867	0.1189	0.0992
	(0.0317)	(0.0315)	(0.037)	(0.0294)
q90	0.3333	0.1533	0.1606	0.1501
	(0.0372)	(0.0379)	(0.0448)	(0.038)
Bank controls	N	Y	Y	Y
Loan level controls	N	Y	Y	Y
Economic controls	N	Y	Y	Y
Observations	47,971	47,971	29,949	47,970
Bootstrap size	1,000	1,000	1,000	1,000
Covariates regression:				
R-squared	NA	0.3010	0.3348	0.3645
AIC	NA	-13,454	-8,618	-12,935
BIC	NA	-12,848	-5,644	9,985

Table 18: Placebo test: risk-taking in originated home purchase loans.
This table presents changes-in-changes estimates of the effect of a placebo treatment on loan-to-income ratio of mortgages originated by SLR covered banks, across the distribution of outcomes. Period before treatment is 2010, period after is 2011. As before, the treatment group is composed of banks subject to SLR rule active in the home mortgage market, while comparable banks form the control group. This estimation uses 62,462 observations randomly sampled from the full dataset of loans.

	Loan-to-income ratio			
	(1)	(2)	(3)	(4)
CIC estimate (quantile)	No Controls	State FE	MSA FE	County FE
Mean	-0.0060	-0.0105	-0.0105	-0.0179
	(0.0218)	(0.0238)	(0.0283)	(0.0254)
q10	0.0007	0.0045	0.0039	-0.0145
	(0.0347)	(0.0368)	(0.0437)	(0.0386)
q20	-0.0151	-0.0427	-0.0397	-0.0510
	(0.0304)	(0.0313)	(0.0382)	(0.031)
q30	-0.0016	-0.0264	-0.0385	-0.0596
	(0.0291)	(0.0327)	(0.0368)	(0.0301)
q40	-0.0318	-0.0354	-0.0610	-0.0576
	(0.0281)	(0.0278)	(0.0334)	(0.0279)
q50	-0.0247	-0.0432	-0.0325	-0.0387
	(0.0284)	(0.0312)	(0.0322)	(0.0256)
q60	-0.0341	-0.0313	-0.0240	-0.0269
	(0.0329)	(0.0278)	(0.0359)	(0.0296)
q70	-0.0106	-0.0238	-0.0252	-0.0339
	(0.0355)	(0.0294)	(0.0375)	(0.0354)
q80	-0.0158	-0.0144	0.0031	-0.0340
	(0.0406)	(0.0345)	(0.0452)	(0.0374)
q90	0.0168	0.0047	0.0195	0.0286
	(0.0455)	(0.0441)	(0.0519)	(0.045)
Bank controls	N	Y	Y	Y
Loan level controls	N	Y	Y	Y
Economic controls	N	Y	Y	Y
Observations	62,462	62,462	37,752	62,460
Bootstrap size	500	500	500	500
Covariates regression:				
R-squared	NA	0.1925	0.1959	0.2386
AIC	NA	5,570	1,831	6,541
BIC	NA	6,194	4,888	28,170

Table 19: Robustness test ignoring largest banks on the treated group.

This table presents changes-in-changes estimates of the effect of the SLR rule on loan-to-income ratio of mortgages originated by a subgroup of treated banks. The treated subgroup is composed of all banks, except the two largest, subject to SLR rule active in the home mortgage market, while comparable banks non covered by the rule form the control group. Period before treatment is 2011 to 2013, period after is 2015 to 2017. This estimation uses 23,888 observations randomly sampled from the full dataset of loans.

	Loan-to-income ratio			
	(1)	(2)	(3)	(4)
CIC estimate (quantile)	No Controls	State FE	MSA FE	County FE
Mean	0.0475	0.0672	0.0508	0.0693
	(0.0341)	(0.037)	(0.0408)	(0.0342)
q10	-0.0808	-0.0197	-0.0049	0.0142
	(0.0503)	(0.0536)	(0.0671)	(0.0531)
q20	0.0082	-0.0155	-0.0329	0.0053
	(0.0402)	(0.0471)	(0.057)	(0.0466)
q30	-0.0050	0.0068	-0.0155	0.0333
	(0.0378)	(0.0451)	(0.0519)	(0.0427)
q40	0.0269	0.0468	0.0478	0.0858
	(0.0403)	(0.0427)	(0.0499)	(0.0421)
q50	0.0678	0.0887	0.0455	0.1147
	(0.0418)	(0.0426)	(0.0525)	(0.044)
q60	0.0580	0.1523	0.1076	0.0971
	(0.042)	(0.049)	(0.057)	(0.0437)
q70	0.0959	0.1731	0.1172	0.1661
	(0.053)	(0.0446)	(0.0571)	(0.0437)
q80	0.0929	0.1213	0.1365	0.1397
	(0.0659)	(0.0597)	(0.0681)	(0.0541)
q90	0.1170	0.0941	0.1570	0.0795
	(0.0732)	(0.067)	(0.0827)	(0.067)
Bank controls	N	Y	Y	Y
Loan level controls	N	Y	Y	Y
Economic controls	N	Y	Y	Y
Observations	23,888	23,888	14,628	23,887
Bootstrap size	500	500	500	500
Covariates regression:				
R-squared	NA	0.1555	0.1668	0.2304
AIC	NA	4,023	2,287	5,431
BIC	NA	4,581	5,164	20,640

Table 20: Credit supply and changes in house prices: difference-in-differences estimation.
This table presents estimation results from the difference-in-differences model for changes in house prices at county level. Columns (1) to (4) show different model specifications and estimation methods. Estimation period is 2012 to 2017, and the frequency of observation is yearly. Models (1) to (3) drop the year 2014 from sample. Robust standard errors are reported in parenthesis.

Dependent variable: House price index (log difference)				
	OLS	Panel FE	Panel FE	Panel FE
	(1)	(2)	(3)	(4)
House price index (t-1)			-0.3065	-0.1402
			(0.0162)	(0.0209)
House price index (t-2)			0.0723	
			(0.0156)	
Employment (t-1)	0.0421	0.0086	0.0062	-0.0017
	(0.0152)	(0.0165)	(0.0147)	(0.0172)
Employment (t-2)	0.0582	0.0117	0.0199	-0.0059
	(0.0164)	(0.0177)	(0.0153)	(0.0155)
Annual payroll (t-1)	0.0267	0.0288	0.0455	0.0135
	(0.0112)	(0.013)	(0.0108)	(0.0138)
Annual payroll (t-2)	0.0373	0.0328	0.0427	0.0268
	(0.0107)	(0.0116)	(0.0107)	(0.0116)
HH debt to income (t-1)	-0.0012	-0.0168	-0.0187	-0.0125
	(0.0006)	(0.0021)	(0.0021)	(0.0028)
Credit (t-1)	-0.0824	0.1334	0.1599	-0.1294
	(0.0305)	(0.062)	(0.071)	(0.1083)
Credit SLR banks (t-1)	0.1582	-0.1636	-0.2049	0.3136
	(0.0465)	(0.0979)	(0.0999)	(0.1513)
Credit SLR banks (t-1) * post	0.1717	0.1612	0.2613	0.0914
	(0.0384)	(0.0354)	(0.0332)	(0.0598)
Time FE	N	Y	Y	Y
County FE	N	within diffs	intercepts	within diffs
Drop year 2014 from sample	Y	Y	Y	N
Estimation	OLS	OLS	OLS	GMM
Observations	13,385	13,357	13,357	13,155
R-squared	0.1312	0.1805	0.4109	0.2562
AIC	-45,885	-46,707	-45,679	N/A
BIC	-45,809	-46,617	-25,197	N/A

Table 21: Difference-in-differences parallel trend test for house prices model.
This table presents estimation results for the parallel trend test in house prices model. Columns (1) to (2) show different model specifications and estimation methods. Estimation period is 2011 to 2013, and the frequency of observation is yearly. Robust standard errors are reported in parenthesis.

Dependent variable: House price index (log difference)		
	Panel FE	Panel FE
	(1)	(2)
House price index (t-1)		-0.5213
		(0.0344)
House price index (t-2)		-0.0988
		(0.0417)
Employment (t-1)	-0.0095	0.0028
	(0.0299)	(0.0255)
Employment (t-2)	-0.0066	0.0112
	(0.0293)	(0.0233)
Annual payroll (t-1)	0.0076	0.0153
	(0.0233)	(0.0193)
Annual payroll (t-2)	-0.0006	0.0132
	(0.024)	(0.0205)
HH debt to income (t-1)	-0.0224	-0.0244
	(0.0047)	(0.0039)
Credit (t-1)	0.0677	0.0711
	(0.2249)	(0.1743)
Credit SLR banks (t-1) * year	0.0000	0.0000
	(0.0002)	(0.0001)
Time FE	Y	Y
County FE	witin diffs	intercepts
Estimation	OLS	OLS
Observations	5,385	5,380
R-squared	0.1709	0.6285
AIC	-23,306	-19,248
BIC	-23,253	-1,586