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Essays on Banking, Credit and the Macroeconomy

DISSERTATION

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for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Fabrizio Almeida Marodin

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2021

DEDICATION

To Kisie, for her unconditional love and support during this endeavour.

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ABSTRACT OF THE DISSERTATION

Essays on Banking, Credit and the Macroeconomy

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Doctor of Philosophy in Economics

University of California, Irvine, 2021

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This dissertation is composed of three empirical studies on banking, credit and the macroeconomy. The first chapter revisits the Roaring Twenties (1920s) to investigate how shocks to credit supply originating from the financial sector interacted with stock prices and macroeconomic fluctuations, and how effective was monetary policy aimed at credit stabilization. I find that financial factors were an important determinant of real output, and monetary policy contraction implied a relevant output/price level loss while not sufficient to stabilize credit and stock prices growth. Besides, the existence of a channel between credit supply in the form of brokers' loans and the level of stock prices is confirmed. The second chapter studies how bank capital relates to credit growth during periods of financial distress. I propose an econometric approach which considers portfolio adjustment strategies as discrete choices made by the bankers. Using this framework, I analyze the 1990s "credit crunch" and find evidence that the contraction in lending was probably not driven by the adoption of risk-based capital requirements, as part of the Basel Accord. Banks were more likely recovering from negative shocks to capital, constrained by leverage ratio requirements, and reacting to the negative economic environment. The third chapter studies the effects of bank capital regulation in credit origination by investigating the introduction of the Basel III Leverage Ratio. I find that banks affected by the Supplementary Leverage Ratio (SLR) requirement, finalized in 2014, reacted by increasing risk-taking and interest rates on mortgages. There

is evidence of heterogeneous effects of policy, in which borrowers of higher risk are more affected. In addition, the aggregate increase in credit supply resulting from the adjustment is correlated with higher future home prices at the local level. 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.

Chapter 1

The 1920s Credit Boom, Stock Prices and Macroeconomic Fluctuations

This paper investigates how the credit boom in brokers' loans interacted with fluctuations in stock prices and macroeconomic variables during the 1920s. I estimate demand, supply, monetary and financial shocks in a Bayesian VAR by using a combination of sign and variance decomposition restrictions. The results indicate that monetary policy contraction was not effective to stabilize credit growth and stock prices while implying a relevant output and price level trade-off. Besides, I find that financial factors played an important role in output growth. Further, I confirm the existence of an effect of credit supply shocks in the level of stock prices.

1.1 Introduction

The dramatic expansion in stock market credit in the 1920s, in the form of brokers' loans, is considered one of the key factors that contributed to the boom in stock prices prior to the crash of October 1929 (Smiley and Keehn (1988); White (1990); Rappoport and White (1993)). Brokers' loans were important money market instruments at that time, and a reasonable share of banks' asset position, specially in New York City. The loans were used to buy stocks, and the acquired securities would enter as collateral in the transaction. Variable margin was required by the lender. Brokers' loans were very short-term, usually daily and renewable, but could be called by the bank at any time.

Figure 1.1[1] shows the amount of brokers' loans in New York City banks and the S&P index of stock prices from January 1920 to December 1932. The level of brokers' loans grew from roughly from $1.5 Billion in the middle of the decade to $3 Billion in late 1929. The rise in stock prices is obviously very correlated with the growth in brokers' loans, and they peak just at the crash of October 24th 1929. One year prior, on October 3rd 1928, the amount of brokers' loans extended by New York City member banks was $2,416 million. In October 3rd 1929, just three weeks before the crash, this figure has grown to $3,040 million, a 25.8% rise from previous year (Board of Governors of the Federal Reserve System (1943)).

From a wider perspective, the 1920s decade is characterized by overall economic growth and credit expansion. Total loans for the weekly reporting member banks accounted $11,349 million in January 1922, raising 50% in nominal terms to $17,041 in January 1930. Brokers' loans experimented an even more aggressive 109% growth rate going from $3,791 to $7,906 in the same time interval. As a consequence, we observe a growing share of brokers' loans as a percentage of the total portfolio of loans. For the weekly reporting banks, the share rises from 33% to 46% between January 1922 and 1930, and for New York City Banks the share is already more than 55% by the end of 1928.

Excessive credit in loans to brokers was a relevant concern to policy makers at that time period (Wright (1929); Warburg (1930)). Particularly influenced by the real bills doctrine,

[1] In some statistical sources loans to brokers are described as loans on securities, secured by stock. We use the names loans to brokers and loans on securities interchangeably.

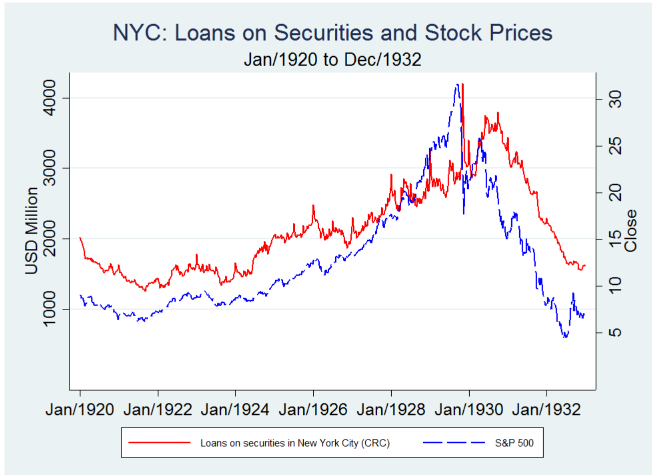


Figure 1.1: Loans on securities in New York City banks and S&P 500 Stock Prices, from Jan/1920 to Dec/1932.

the founders of the Federal Reserve had hoped that the new central bank structure and activities would channel credit away from "speculative" uses towards what was considered "productive" activities (White (1990)). Paul Warburg, one of the founders of the Fed, writing in 1927 stated that one of the System's most serious shortcomings was its inability to create important discount markets outside of New York City, and consequently its failure to "lessen the congestion of the country's unemployed funds on the New York Stock Exchange (NYSE)" (Warburg (1930)). The author recognizes that the concentration of money in the stock exchange was more pronounced than ever before, an such condition carried dangers for the banking system, a well as for the NYSE itself.

The desire to end the "orgy of speculation" in stocks and to halt the "undue absortion" of the country's credit supply to speculative uses guided the Federal Reserve System's decision in January 1928 when it started contracting monetary policy (Warburg (1930)). The discount rate was raised from 3.5% to 5% in six months, as a consequence of fears about excessive flow of credit to the stock market. In the following months, even though there was still general agreement on the risks involved, the continuation of monetary contraction was subject to debate. Directors at the New York Fed argued that speculation could only be reduced by further raising the discount rate. However, members of the Board pushed for a "direct pressure" procedure, which would deny access to the discount window to member banks making loans on securities (White (1990)). The Board view prevailed and the discount rate stood still until August 1929, when it was raised only in New York.

Although the important role that brokers' loans played in the expansion of the stock price bubble is widely recognized, some critical research questions remain to be addressed. Fundamentally, it is not clear how much of the stock price rise can be attributed to the growth in the supply of brokers' loan credit. In other words, was the credit boom really affecting stock prices, or did the boom in credit just followed from higher demand to buy stocks? The question follows not only from historical interest, but it can also be framed as a fundamental question in macroeconomics and finance, as it relates to how credit supply shocks can impact asset prices and propagate to the rest of the economy. From the policy perspective, it is crucial to understand the link between credit supply and asset price fluctuations. Different propagation mechanisms imply different policy prescriptions for financial stability.

This paper, thus, aims to investigate the relationship between the credit boom in brokers' loans and fluctuations in stock prices and other macroeconomic aggregates during the 1920s cycle. My interest lies in three basic questions. First, I explore how effective was the policy tool, that is adjusting the discount rate, in stabilizing credit growth and stock prices. This assumption was at the heart of the motivation for the contractionary monetary cycle started in January 1928, but even if it is valid, it is interesting to understand what was the trade-off between output loss and credit stabilization at that time. Second, I assess how financial factors, like the easing of credit conditions and the valuation of stock prices guided by either fundamentals or optimism, contributed to aggregate macroeconomic growth, to fluctuations in prices and to the reaction of the policy rate. Third, and most importantly, I want to measure how credit supply in brokers' loans impacted stock prices. The answer should reveal important facts about the transmission of credit supply shocks to asset prices, as well as help our understanding of possible policy choices designed to address financial stability issues.

I address the research questions by estimating a Bayesian Vector Autoregressive (BVAR) model on monthly U.S. data, and applying different econometric procedures to identify structural shocks. I start with a simple recursive ordering identification approach. Although useful, this method requires strong assumptions about how the structural (unobserved) shocks impact observed macro variables on the first period. To circumvent this limitation, I adopt a sign restrictions identification strategy, which is a widely used method in contemporary empirical macroeconomic research (Canova and De Nicolo (2002); Furlanetto et al. (2017)), in combination with additional variance decomposition restrictions, such as in Weale and Wieladek (2016).

Previous research have analyzed the US economy by the use similar methods, but either focused in different time periods or were interested in alternative reseach questions not related to credit. Calomiris and Hubbard (1989) estimate a structural VAR model using monthly data from the pre-World War I era and find that credit availability contribute substantially to explain output fluctuations. Canova (1991) investigates how the macroeconomic dynamics of the US changed by the creation of the Fed. He estimates a structural VAR model using monthly data from two separate samples: from 1891-1913 and 1924-1937. The focus is on how financial crisis were generated by a combination of internal seasonal movements and unexpected external shocks, but there is no specific role for credit in his model. Nason and Tallman (2015) also apply a VAR method, but cover a longer period at a lower, annual frequency from 1890 to 2010. They are interested in how the propagation of shocks differ in periods of financial crisis, and do not specifically address the 1920s credit cycle nor the role of asset price fluctuations. The work by Furlanetto et al. (2017) is methodologically the most similar to the research presented here. The authors specifically address the role of financial factors in macroeconomic fluctuations, but they cover the modern period of 1985 until 2013.

The research is related to a wide literature on macroeconomics and finance which investigates the relationship between credit expansions, fluctuations in asset prices and its consequences to the business cycle. Housing finance has been recognized as the primary form of debt to influence economic activity in modern economies, and the subject naturally attracted a great deal of attention after the Financial Crisis of 2007/08 (Mian et al. (2017b)). Jordà et al. (2016) demonstrates that mortgage credit became an increasingly important factor for business cycle dynamics during the twentieth century for most advanced countries, as well as an important source of financial fragility. The financial stability concern is also brought by Schularick and Taylor (2012) who argue that credit booms concurrent with asset price booms are strong predictors for banking crisis. We claim that mechanisms similar to the housing finance channel may operate for other asset classes as well. So something can be learned by investigating how credit extended to buy stocks on margin might drive fluctuations in stock prices. The magnitude of the credit and asset price boom observed in the 1920s make this a great *locus* for research.

An additional contribution of the research is related to whether the central bank should react to asset price fluctuations, and what what are the consequences of each policy instrument. Even though the subject has been discussed for decades it remains an open question for policy making nowadays (Bernanke and Gertler (2001); Schularick and Taylor (2012)). This paper provides measures useful to understand how interventions in the financial system could be beneficial, if ever, in order to prevent extreme fluctuations in asset prices, and its negative effects in the real economy.

The paper is organized as follows. In section 1.2, I present the model along with the estimation method and data sources. Section 1.3 presents the results of several estimation and identification exercises. Finally, in the last session I discuss the relevance of the findings.

1.2 Model estimation

I adopt a Vector Autoregressive (VAR) model represented in reduced form as

$$Y_t = B_0 + B(l)Y_{t-1} + u_t$$

where vector Y_t of size $N \times 1$ holds all endogenous variables observed at t, (l) is the lag operator, p is the number of lags, and u_t is the vector of reduced form disturbances. The matrix B_0 of size $N \times 1$ contains the intercepts of each equation, to be estimated. The coefficients to be estimated for each lagged endogenous variable are held in matrix $B(l)$, of size $N \times N$.

The distributional assumption characterizes reduced form disturbances by a multivariate normal process, $u_t|(l)Y_{t-1} \sim N_N(0, \Sigma_{N\times N})$. This implies that reduced form shocks can be correlated between N variables for same period, but are independent over time.

1.2.1 Bayesian estimation

The reduced form model estimation will follow methods presented by Greenberg (2012) and Koop et al. (2007). We first represent the VAR system in SUR form as in Zellner (1962). The vector X_{it} is composed of lagged values for all endogenous variables for a chosen p number of lags. We add a constant 1 to the last position in order to estimate an intercept. To save on notation, we call each variable on Y_t as y_{it}, where $i = 1, ..., N$.

$$X_{it} = \begin{bmatrix} y_{1,t-1} & \dots & y_{N,t-1} & \dots & y_{1,t-p} & \dots & y_{N,t-p} & 1 \end{bmatrix}$$

We then build the matrix of regressors X_t, of size $N \times (pN + 1)$, with X_{it} on the main diagonal and zeros on the rest. For ease of notation we will define $K = pN + 1$, which is the width of each line of regressors.

$$X_t = \begin{bmatrix} X_{it} & 0 & ... & 0 \\ 0 & X_{it} & ... & 0 \\ ... & 0 & ... & 0 \\ 0 & ... & ... & X_{it} \end{bmatrix}$$

The system in matrix form is

$$Y_t = X_t\beta + \varepsilon_t \tag{1.1}$$

The defining assumption of the SUR model is that $\varepsilon_t|X \sim N_N(0, \Omega)$, where $X = (X_1, ..., X_T)$ and $\Omega_{N\times N} = \omega_{ij}, i = 1, .., N, j = 1, ..., N$ Greenberg (2012). Thus, in our formulation this implies that the disturbances are allowed to be correlated between N variables for the same time period, but not correlated across time. This is the usual assumption of a reduced form VAR system.

Next, we specify a conditionally conjugate prior for the model, in order to proceed estimation with a standard Gibbs sampler (Greenberg (2012); Koop et al. (2007)). The regression coefficients are assumed to have a Gaussian prior, $\beta_{KN\times 1} \sim N_{KN}(\beta_0, B_0)$, while the covariance matrix of disturbances is assumed to follow an inverse Wishart distribution $\Omega \sim IW_N(\nu_0, R_0^{-1})$.

The parameters β_0 and B_0 are chosen by using a Minnesota Prior as suggested by Kilian and Lütkepohl (2017). I take the hyper parameters $\lambda = 0.2$ and $\theta = 0.6$. The authors originaly suggest $\theta = 0.3$ for analysis of quarterly macroeconomic time series, but I choose less shrinkage as our model is estimated on a monthly frequency. Note that the use of this procedure requires that we normalize all the variables before estimation so the coefficient magnitudes are comparable across all variables. Finally, regarding the covariance matrix of disturbances hyperparameters, I choose a low value for the degrees of freedom $\nu_0 = K + 1$, following Kilian and Lütkepohl (2017) , and a standard R_0^{-1} that equals to the frequentist estimate of the covariance matrix $\hat{\Omega}$.

1.2.2 Data sources

The observed variables to be used in the VARs are six: output, prices, the discount rate of the Federal Reserve Bank of New York, a measure of leverage of the financial system in New York, the spread on brokers' loans and stock price index for the New York Stock Exchange (NYSE). In this paper, I am primarily concerned with banks in New York City as the most representative of the financial system and where most of the market for brokers' loans operated. The observed variables will be represented in levels as widely adopted in empirical macroeconomic studies in order to avoid the loss of information (Furlanetto et al. (2017)).

Data for output and prices are broad US measures. I use the Consumer Price Index and Industrial Production available from the FRED database at a monthly frequency. Data about discount rate come from *Banking and Monetary Statistics* (Board of Governors of the Federal Reserve System (1943)). The publication provides discount rates for the whole period and the specific day they were adjusted. I take the prevailing rate at the last day of the month set by the New York Fed.

The measure of leverage must be designed to consider only brokers' loans, as this is the specific type of credit we are interested in. The datasource is the *Weekly Reporting Member Banks in Leading Cities* (Federal Reserve Board (1915-1935)), which provides aggregated data on the main assets and liabilities of banks, classified by each of the twelve Federal

Reserve Districts plus the Central Reserve City of New York. This dataset allow us to measure brokers' loans for the full 1920s decade, at a relatively high frequency [2], which can be then converted to monthly as needed. The variable *leverage* is defined as the ratio of brokers' loans to public deposits. Public deposits are calculated as the sum of net demand deposits and time deposits.

Data for the price index for NYSE is taken from *Global Financial Data.* I use the S&P 500 series as representative of the market. Data for the spread on brokers' loans is calculated as the difference between the average rate on call loans in NYC and the discount rate in the same district. I collect the monthly average rate on Stock Exchange new call loans in New York City from *Banking and Monetary Statistics* (Board of Governors of the Federal Reserve System (1943)).

My sample of interest starts in December 1919 and goes until the end of the credit boom in September 1929. The sample is restricted to end just before the stock market crash of October 1929 on purpose. The reason is clear, as the research interest is to investigate the dynamics of the credit cycle during the boom period, and how monetary policy shocks were potentially affecting credit growth and stock prices. Besides that, the magnitude of the shock resulted in a huge revision of expectations and possibly altered the relationship of macro variables. The semi-structural model adopted here would not be able to address this issues.

1.3 Results

This section describes the results of estimating different specifications of the structural VAR model. I begin with a simple baseline model with three equations and no financial shocks.

[2] In comparison to the Weekly Reporting, the quarterly Call reports dataset only collects specific statistics for broker's loans after October 1928.

Then, I proceed to analyze the main model with two financial factors, namely a credit supply and a stock preference shock. Finally, in the last part I run a series of robustness checks in a simpler model with only one financial factor, credit supply.

All variables are normalized to mean zero and standard deviation before estimation. The Markov-Chain Monte Carlo simulation was run for 10,000 draws in each estimation. I considered the first 1,000 draws as burn-in time.

1.3.1 Preliminary exercise: three equation monetary VAR

As a baseline exercise, I estimate a standard monetary VAR with only three variables, output, prices and interest rate. The objective is to enable comparison of the baseline model with richer models including financial variables. I will analyze the model using two different identification strategies, first a standard recursive ordering strategy, and later a sign-restriction identification method similar to Weale and Wieladek (2016).

The observed data series at this moment are, respectively, US industrial production in logs, US Consumer Price Index in levels, and the nominal discount rate in New York Fed in levels. The vector of endogenous variables is thus $Y_t = [y_t, p_t, i_t]'$, where y represents output, p stands for prices, and i for the discount rate. I will use a sample of monthly observations, from Dec/1919 to Sept/1929, totaling 117 observations. The number of lags is arbitrarily set to $p = 2$. I have previously run frequentist estimations of the model, and the specification with 1 lag was suggested by Schwarz's Bayesian information criterion. I decided for one additional lag in order to better capture the dynamics of the system. Figure 1.2 shows a plot of all observed series, including financial variables which are going to be addressed in later sections.

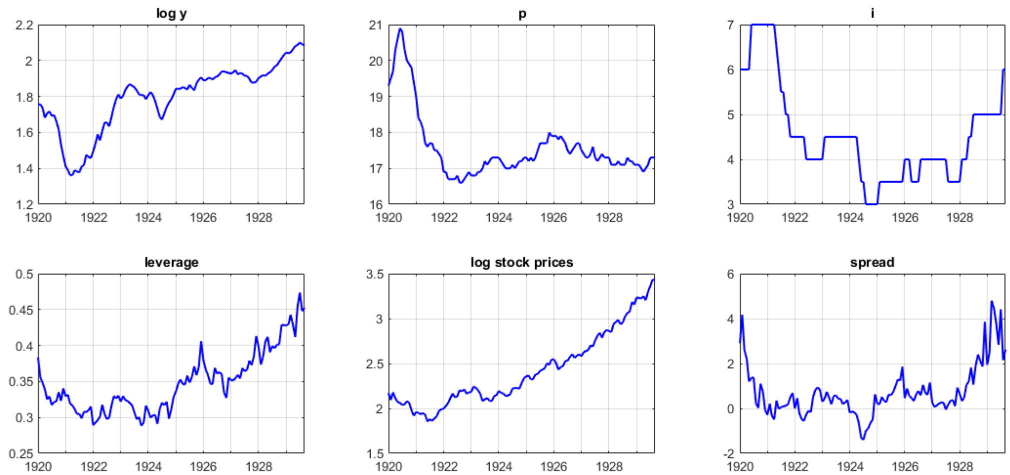


Figure 1.2: Observed variables from Dec/1919 to Sept/1929: log output, prices, discount rate, leverage, log stock prices, spread.

Recursive ordering identification

The identification of structural shocks in the first exercise will be done by standard recursive ordering. This strategy is widely adopted in this type of small VAR (Kilian and Lütkepohl (2017)). The underlying assumption is that output and inflation respond with lags to changes in discount rate, while the central bank use information about the current month to set the rate. Moreover, we assume that output has a slower adjustment than prices.

The impulse response functions are plotted in Figure 1.3 in the Appendix. First, we observe the usual effect of a restrictive monetary policy shock, that is an increase in the discount rate, in reducing the price level. The effect in prices is relatively strong and persistent. On the other hand, the effect of monetetary shocks in output is less distinguishable. The wide uncertainty bands do not allow us to identify a negative output response. Second, shocks to the price level lead to output reduction, as expected, combined with a positive response of the discount rate for at least two years. Finally, shocks to output lead to an increase in price levels for approximately two years, while inducting a positive response of the discount rate. In general, the responses are well in line with expected results.

Identification by sign-restrictions

In the second exercise, I follow a sign-restriction scheme to identify demand, supply and monetary shocks. The assumptions are as follows. A positive demand shock should affect output, prices and the discount rate positively. A supply shock must drive output and prices in opposite directions. A monetary contraction shock must increase the discount rate, and drive down both output and prices. The restrictions are summarized in Table 1.1. Note that the system is fully identified even though I leave the reaction of the discount rate to supply shocks unrestricted. This scheme is equivalent to a subset of restrictions in Weale and Wieladek (2016) or Furlanetto et al. (2017).

Observed Variable	Demand	Supply	Monetary
Output	$+$	$+$	$-$
Prices	$+$	$-$	$-$
Discount rate	$+$	NA	$+$

Table 1.1: Restrictions in the baseline three equation model.

The estimation of impulse response functions (IRFs) for the model identified with sign restrictions involves drawing M parameters from the posterior distribution, and then calculating a set of rotation matrices Q for each draw, which must not violate the sign-restrictions (Kilian and Lütkepohl (2017)). We obtain a distribution of IRFs, which we summarize by taking the median at each point in time. The procedure accounts for uncertainty about both parameters and identification, and is described below.

First, I draw β^i and Ω^i from the posterior distribution and compute an initial Cholesky decomposition P^i. Next, I consider a random rotation matrix Q, and compute the implied impulse response function $\Theta^i|\beta^i, \Omega^i, P, Q$. Third, if Θ^i satisfies the sign restrictions, I store the value in a sequence $\{\Theta^i\}_{i=1,..,M_\Theta}$. Otherwise, Θ^i is discarded. The steps are repeated in order to sample a fixed number M_Q of IRFs for each value of the posterior. In this exercise, I have fixed $M_Q = 100$, resulting in $M_\Theta = 90,000$ given the posteriors are of size 9,000.

Figure 1.4 shows the median impulse response functions and credibility intervals for all three shocks. The general findings are well in line with expected, and similar to those obtained in the last section. I highlight the fact that this identification scheme allow us to observe a clearer effect of monetary policy on output. An increase in the rate drops output for about 14 months, and the credibility interval is now negative for the first four months.

Table 1.2 reports the contribution of each of the three identified structural shocks to the forecast error variance of each observed variable. The variance decompositions are calculated based on the median impulse response function for each shock, as described by Kilian and Lütkepohl (2017). Some results are worth noticing. First, supply shocks account for the largest share of output fluctuation, as well as considerable share of price variability. Second, demand shocks explain the largest share of fluctuations in the discount rate, which may reflect the fact that policy was responding aggressively in an intent to stabilize output. This was specially the case during the recession 1921-1922, when the discount rate fell from 7% to 4%. Finally, monetary shocks appear to have important effects on price fluctuations as was already observed in the IRFs.

	Horizon	Demand	Supply	Monetary policy
Output	1	0.24	0.54	0.22
	12	0.12	0.74	0.14
	24	0.17	0.72	0.11
Prices	1	0.24	0.53	0.23
	12	0.16	0.39	0.45
	24	0.12	0.32	0.56
Discount rate	1	0.58	0.00	0.41
	12	0.84	0.00	0.16
	24	0.84	0.02	0.15

Table 1.2: Median forecast error variance decompositions for the three equation model with sign identification.

1.3.2 Financial factors

In this section, I adopt a larger model with financial factors to capture the dynamics of credit and stock markets. The three observed variables added to the VAR model are *leverage*, *stock* and *spread*. The first variable represents the relative leverage of the financial system in the New York City district considering loans to brokers. It is calculated as the ratio of brokers' loans to public deposits, in levels. The second variable, *stock*, is the stock price index from NYSE in logs. The last variable, *spread*, is the difference between the observed rate on loans to brokers in the New York City district and the NY Fed discount rate.

The VAR model with financial factors holds 6 endogenous variables:

$$Y_t = [y_t, \pi_t, i_t, leverage_t, stock_t, spread_t]' \tag{1.2}$$

I am going to use a combination of sign restrictions and variance decomposition restrictions to identify two different financial shocks, a stock preference and a credit supply shock, in combination with the previous demand, supply and monetary policy shocks.

The stock preference shock is simply defined as a structural shock which increases the demand for stocks. So, given this shock, we should expect stock prices to increase along with the spread for brokers' loans. Note that the stock preference shock can be motivated by economic fundamentals, such as an increase in payed dividends as well as a revision of expectations about future dividends or cash flows. It also can be motivated by market sentiment, or factors not related to fundamentals, such as exogenous changes in expectations of future price growth, behavioral factors such as herd behaviour, etc. At this time, I adopt an ample definition of the stock preference shock.

On the other hand, the credit supply shock in brokers' loans is a structural shock which increases leverage and stock prices while decreasing the spread for brokers' loans. This shock captures banker's and other investors' willingness to lend in the form of brokers' loans. Note that leverage can also increase due to a stock preference shock, so the observed variable which allow us to differentiate between the two structural shocks is the spread. Table 1.3 provides a list of the sign restrictions adopted for identification.

Observable	Demand	Supply	Monetary	Financial (Credit)	Financial (Stock pref.)
Output	+	+	−	+	+
Prices	+	−	−	NA	NA
Discount rate	+	NA	+	+	+
Leverage	NA	NA	NA	+	+
Stock prices	NA	NA	NA	+	+
Spread	NA	NA	NA	−	+

Table 1.3: Sign restrictions in the model with two financial factors.

So far, the use of sign restrictions is not sufficient to uniquely identify financial shocks from each of the other structural innovations. I am avoiding any assumptions about the effect of financial shocks on prices, so they are not distinguishable from supply shocks. Besides, demand shocks are assumed to have positive effect on output and the discount rate so they are not distinct, at this point, to any financial shock. The strategy I adopt is to use additional variance decomposition restrictions as in Weale and Wieladek (2016) to fully identify the model. The technique rests on the assumption is that a shock that is variable-specific should explain the largest fraction of the variance of that variable, at least upon impact and for the first few time periods. In our particular model, this gives rise to the following assumptions: (i) financial shocks, either credit or stock preference, must explain a larger fraction of the variation in leverage and stock prices than supply or demand shocks; (ii) supply shocks must explain a larger fraction of the variation in prices than financial shocks. This set of assumptions are summarized in Table 1.4, and they now allow me to fully identify the five shocks of interest in the model. As the VAR has six observed variables, there is still one structural residual shock which will not be of our interest.

The procedure to identify the model calculating the set of admissible IRFs is similar to the one used previously, when we generated sequences of decomposition matrices Q, and rejected the ones which violated the conditions (Kilian and Lütkepohl (2017)). Specifically, I assumed the variance restrictions should hold upon impact and for the first three time periods, following Weale and Wieladek (2016). Given the size of the model, the simulation is now repeated by sampling $M = 200$ random draws of parameters (β^i, Ω^i) from the posterior distribution and accepting $M_Q = 25$ rotation matrices for each draw, resulting in $M_\Theta = 5,000$ impulse response functions.

Shock	Restriction
Demand	$Var_{leverage}(shockDemand) < Var_{leverage}(shockCredit)$
	$Var_{stock}(shockDemand) < Var_{stock}(shockStock)$
Supply	$Var_{leverage}(shockSupply) < Var_{leverage}(shockCredit)$
	$Var_{stock}(shockSupply) < Var_{stock}(shockStock)$
Financial (Credit)	$Var_{price}(shockCredit) < Var_{price}(shockSupply)$
Financial (Stock preference)	$Var_{price}(shockStock) < Var_{price}(shockSupply)$

Table 1.4: Variance decomposition restrictions in the model with two financial factors.

Figure 1.5 shows all the impulse response functions identified in the model with financial factors. The first important finding is that a monetary policy shock is apparently not effective to contain either stock price growth or leverage during the period. In principle, we would expect that a higher discount rate would discourage lending to brokers by the banking system, and lead to lower leverage. Avoiding further expansion of the amount of brokers' loans was, in fact, one important reason behind the Federal Reserve decision when pursuing monetary policy tightening between January and September of 1929, as previously discussed. At the same time, the rise in the discount rate should reprice stocks downwards in a standard forward-looking asset pricing model. Once the risk free rate is adjusted, the present value of future dividends must fall.

Contrary to this logic, the results obtained in estimation for this period do not imply a negative reaction of stock prices to an unexpected increase in the discount rate. The IRFs show a response very close to zero and not conclusive credibility intervals. The point estimate is even positive, which would imply the opposite result. Regarding the effect on leverage, we observe very small negative response and also non conclusive credibility intervals. Additionally, the forecast error variance decomposition estimated at the median IRF, see Table 1.5, confirms very little contribution of monetary policy shocks to the variability of financial variables. One possible interpretation is that the stock price bubble was an autonomous stochastic process, that was independent of the interest rate.

We should note, however, that this finding is observed for the period previously to the stock market Crash of October 1929. A common view in the literature is that monetary policy contraction conducted in the later part of the decade contributed to initiating an economic downturn. Some authors point that the evidence of an oncoming recession, beginning in July 1929, consequently caused an aggressive reversal of expectations about economic growth, leading to the Crash (White (1990)). From this interpretation, and considering a larger time frame, then raising the discount rate was, ironically, an extremely successful policy to lower the level of brokers' loans and stock prices, but which carried disastrous side effects. The estimation results show that policy was not effective to attain the original objective of policy makers, that is to moderately contain the credit boom or stock prices, but it was effective in causing real output loss.

The second important finding is evidence that financial factors have relevant real effects during the economic cycle of the 1920s. A positive stock preference shock of one standard deviation, resulting in an increase of 2.48% in stock prices, leads industrial production to increase 0.76% after four months and carries positive effect in the long run. Credibility intervals for this impulse response include positive effect for ten months. Likewise, a one standard deviation credit supply shock, which increases leverage in the banking sector by 0.73 percentage point, leads to output increasing by 0.36% in the short run. In this case, the effect is positive in the long run, 0.51% after two years, but uncertainty is higher and credibility interval includes negative responses. The variance decomposition in Table 1.5 confirms that financial factors have a contribution of about 15% the variability of output.

Finally, the model identification allow us to disentangle the relationship between the two financial factors, stock preferences and credit to brokers. Theoretically there should be a positive feedback between both factors. The first channel can be understood as the "asset prices to credit" effect. An exogenous increase in demand for stocks should raise the demand for credit as well, raising the spread charged for brokers' loans, provided the supply of credit remains constant. The complementary channel is the "credit to asset prices" effect. An exogenous increase in credit supply should lower the spread for brokers' loans, decreasing the cost of investing in stocks and encouraging investors to buy more of the asset, thus raising stock prices.

In our model, we estimate that a typical stock preference shock, of 2.48% raise in stock prices, leads to persistent higher leverage in brokers' loans by 0.31 percentage points in the same month, and 0.22 after one year. The credibility interval remains positive for 24 months. The relevance the "asset price to credit" channel is confirmed by the variance decomposition, which suggests that stock preference shocks are the second most important factor driving the variability of leverage.

More importantly, the estimation confirms the existence of a "credit to asset prices" channel. The typical credit supply shock raises leverage in the banking sector by 0.73 percentage point, and leads to an increase in stock prices of 0.98% on impact. The credibility interval is positive for the first two months. After that, the point estimate is persistently positive and implies an increase in stock price level of 1.25% after two years, but this estimate is subject to higher uncertainty. The variance decomposition confirms that credit supply shocks can explain a reasonable variability of stock prices in the short run.

1.3.3 Robustness checks

In this section, I present different identification exercises in a simplified model with only one financial factor. My interest is to check the findings for robustness, regarding the effect of

	Horizon	Demand	Supply	Monetary policy	Credit	Stock preference
Output	1	0.30	0.27	0.29	0.06	0.09
	12	0.19	0.27	0.31	0.04	0.19
	24	0.34	0.26	0.25	0.05	0.10
Prices	1	0.19	0.66	0.14	0.00	0.01
	12	0.09	0.31	0.40	0.20	0.00
	24	0.07	0.23	0.43	0.25	0.02
Discount rate	1	0.38	0.00	0.44	0.10	0.08
	12	0.39	0.09	0.14	0.06	0.33
	24	0.29	0.11	0.18	0.04	0.38
Leverage	1	0.12	0.03	0.01	0.71	0.13
	12	0.16	0.02	0.02	0.58	0.22
	24	0.24	0.02	0.02	0.46	0.25
Stock prices	1	0.12	0.01	0.01	0.12	0.75
	12	0.27	0.00	0.01	0.05	0.67
	24	0.35	0.01	0.06	0.06	0.52

Table 1.5: Median forecast error variance decompositions for model with two financial factors.

monetary policy on credit to brokers, and also how financial shocks interacted with economic activity. The VAR model adopted includes the basic three variables, output, prices and the interest rate, plus leverage.

Did monetary policy affect leverage prior to the Crash of 1929?

In order to further investigate this question, I first revisit the recursive ordering strategy in a model with one financial factor. The ordering for the observables is $(y_t, p_t, leverage_t, i_t)$. The additional assumption, from the baseline three equation model, is that banks respond to changes in the discount rate by adjusting their leverage with a delay of one month. I keep the initial assumption that the Fed sets its discount rate using all available information. I also assume that changes in output and prices are slower than changes in leverage.

The resulting impulse response functions can be seen in Figure 1.6, and we find that leverage does not appear to react substantially to changes in the discount rate, at least in the period estimated which starts in December 1919 and goes until September 1929. In quantitative terms, the point estimate for the response of leverage to an increase in the nominal discount rate is negative for the first 30 months. A one-standard deviation increase in the rate, representing 0.19 percentage points, implies a negative effect of approximately -0.001 percentage points in leverage, after 17 months when the response is on its lower bound. Not only is this effect very small, but also the calculated one standard-deviation credibility interval includes positive effects. Besides, the negative effect is transitory, as leverage returns to the original level after less than three years.

Next, I re-estimate the model by changing the ordering of the variables. The objective is to explore whether the recursive approach chosen initially may be driving the findings. By placing *leverage* after i we are assuming that policy reacts only with a one month delay to changes in leverage, and that banks react within the month to changes in the policy rate[3]. The ordering, thus, become $(y_t, p_t, i_t, leverage_t)$. As can be seen in Figure 1.7, the effect in leverage given a shock in interest rates is slightly higher (in absolute value) than before, but still relatively small. A one-standard deviation non-expected increase in the discount rate results in -0.0013 percentage points in leverage, after 12 months. Importantly, the effects continue to be transitory and the one standard-deviation credibility interval is all over the positive and negative ranges.

My second robustness exercise to check the effect of monetary policy on leverage is to apply an "agnostic" identification procedure similar to Uhlig (2005) . The method consists of imposing restrictions on the response of some variables, while leaving the response of the main variable of interest unrestricted. The only assumption I make is that a contractionary monetary policy shock leads to a raise on the discount rate, as well as a decrease in output and prices. The objective is to be as concise as possible with the set of restrictions, so I

[3]Note that assumptions for output and prices, as well as its impulse response functions remain the same as their order has not changed from the previous specification. The block y, p is always before the block $i, leverage$.

purposefully do not identify any other structural shock.

Figure 1.8 displays the responses of each variable to an unexpected monetary policy contraction. Output drops during the first 14 months, while the negative effect on prices is much more persistent and spans 40 months. These results are very much in line with the previous three equation model exercise with sign restriction identification. The relevant finding of the "agnostic" procedure is that the response of leverage appears not distinguishable from zero. Even if the median of the IRF is negative for the first 18 months, the absolute value of the response is relatively very small and the credibility intervals are wide, including both positive and negative responses. Again, monetary policy contraction appears to be ineffective as a tool to restrain credit to brokers.

My last robustness test in this section is to apply a full identification scheme on the model with one financial factor. I take the sign restrictions adopted in the three equation model and further assume that a positive financial shock should impact leverage, output and the interest rate positively. In this case, the financial shock can be interpreted either as increase in credit supply to brokers' loans or a stock preference shock, which would cause leverage to increase too. The set of assumptions is similar to Furlanetto et al. (2017), except that I am avoiding any restrictions about the effect of financial factors on prices.

Table 1.6 summarizes the sign-restrictions. I will have to adopt additional variance decomposition restrictions (Weale and Wieladek (2016)) to distinguish the financial shock from innovations in demand or supply. At this time, financial shocks are assumed to explain the largest fraction of the variation in leverage upon impact and with a three period delay.

The impulse response functions in Figure 1.9 show the response of all variables to the four identified shocks in this model. Again we confirm the findings that an unexpected monetary policy shock does not seem to affect credit supply. The response is very small and our confidence includes both positive and negative values. The variance decomposition shown

Observed Variable	Demand	Supply	Monetary	Credit
Output	+	+	−	+
Prices	+	−	−	NA
Discount rate	+	NA	+	+
Leverage	NA	NA	NA	+
Variance decomp. restrictions	$Var_l(shock_i) < MAX(Var_l)$	$Var_l(shock_i) < MAX(Var_l)$	NA	$Var_l(shock_i) = MAX(Var_l)$

Table 1.6: Sign and variance decomposition restrictions in the model with leverage.

in Table 1.7 confirms very little contribution of monetary policy shocks to the variability of leverage.

Moreover, we also observe the usual effect of tightening monetary policy on prices, that is a decrease for long period, and temporary output contraction. A 0.12 percentage points increase in discount rate impacts industrial production by 1% after 4 months, and effects are neutralized after 17 months. I highlight the fact that we find relevant variability of the discount rate due to demand and financial shocks in the variance decomposition. This finding may be related to the fact that Fed was reacting to stabilize demand during the recession period of the early 1920s, as well as acting in an attempt to limit credit expansion, as it was clearly stated by policy makers.

Do financial factors drive output fluctuations?

This robustness test verifies how financial factors impact output under different identification schemes. As the model now has only four observables, it is not possible to distinguish the source of financial shock as it is done in the full VAR model with six variables. The assumption taken is that a positive financial shock increases leverage on impact, and that this increase is orthogonal to other structural demand, supply and monetary shocks. The source of the financial shock could either be coming from innovations in stock preferences, from the supply of credit or from something else.

The results from the initial recursive ordering identification $(y, p, leverage, i)$, seen in Figure

	Horizon	Demand	Supply	Monetary policy	Financial (Credit)
Output	1	0.22	0.43	0.23	0.12
	12	0.17	0.28	0.18	0.36
	24	0.36	0.25	0.10	0.29
Prices	1	0.26	0.49	0.24	0.01
	12	0.26	0.28	0.41	0.05
	24	0.21	0.21	0.50	0.08
Discount rate	1	0.48	0.00	0.43	0.09
	12	0.41	0.06	0.16	0.38
	24	0.26	0.06	0.15	0.53
Leverage	1	0.15	0.05	0.03	0.77
	12	0.16	0.02	0.03	0.78
	24	0.20	0.02	0.03	0.76

Table 1.7: Median forecast error variance decompositions for model with leverage and identification by sign and variance decomposition restrictions.

1.6, confirms that positive financial shocks drive permanent output gains. The estimate is that industrial production increases by 0.5% in the long run due to a structural financial shock that increases 1.26 percentage points in leverage. This is a rather sizable effect quantitatively. In the short run, industrial production increases by 1.54% at the peak, after one year. We still find that prices respond negatively to financial shocks, while the discount rate reacts positively after a few months.

Under the full identification method, using sign and variance decomposition restrictions and shown in Figure 1.9, we again confirm that financial factors have real effects. A typical financial shock, that results in higher leverage by 1 percentage point on impact, increases industrial production by 1.3% after 9 months and by 0.6% in long run. The credibility intervals are on the positive sign for almost twenty months. The structural innovation also leads to a reaction of interest rate, which increases 0.12 percentage points after one year. In this case, there is nothing we can say about the effect on prices. The variance decomposition presented in Table 1.7 confirms that financial shocks explain a relatively large share of output variability, of about 30% after one and two years. The share is comparable to demand and supply shocks.

1.4 Conclusion

I propose a Bayesian VAR model to analyze the dynamics of the 1920s credit cycle, stock prices and macroeconomic fluctuations. The model was estimated with monthly data from December 1919 until September 1929, and I applied a combination of sign and variance decomposition restrictions to identify several structural shocks of interest. The contributions of this research can be summarized in three parts as follows.

First, I find that monetary policy was a blunt instrument to respond to the boom in credit and stock prices during the 1920s expansion. The Federal Reserve policy makers were concerned about excessive credit for speculation, but the financial system did not react to their tight money policy as expected until the Crash of 1929. The estimation demonstrated that the effect of unexpected changes in the discount rate on credit to brokers is either very small or not different from zero, and transitory in any case. The use of different model identification methods, like recursive ordering, "agnostic procedure" or a full sign-restrictions identification scheme does not alter the results qualitatively, and very little in quantitative terms. At the same time, contractionary monetary policy implied short-term output loss and persistent decline in the price level. We stress that this finding is observed for the period from December 1919 until September 1929, that is, prior to the Crash of 1929 which substantially depressed stock prices and, as a consequence, reduced lending to brokers.

Second, financial factors played an important role for output fluctuations during the 1920s expansion. The estimation demonstrated that output reacted positively and permanently to financial shocks, and suggested that financial factors may have accounted for between

15% to 30% of the variability in output, as measured by industrial activity. This result was also tested for robustness by the use of different model identification strategies. The finding is related to modern research on financial factors as drivers of fluctuations in output and investment, such as Furlanetto et al. (2017). The authors use a similar model, a Bayesian VAR with sign restrictions identification, and find that financial factors explain around 24-30% of fluctuations in GDP for the period 1985 to 2013. Our contribution is to test and confirm the hypothesis for an earlier business cycle era. Furthermore, a particular result stressed by the Furlanetto et al. (2017) is the limited response of prices to financial shocks, which they sustain "has not been discussed in the previous literature". In our calculated IRFs, financial shocks generally imply a negative response of prices, so we are able to confirm this fact too.

Third, the main contribution of this research is to confirm and quantify the relevance of the "credit to asset prices" channel for the case of stocks. The estimation have found that stock prices are expected to increase by almost 1% on impact, due to a structural credit supply shock of typical size, in the form of brokers' loans. To the best of my knowledge, this is the first research to demonstrate this relationship empirically for the 1920s credit boom. While the credit to asset prices channel has been intensively debated by previous historical research on the subject (Kindleberger (1978); White (1990)), it was never quantified econometrically. Moreover, this research finding in a way revisits the hypothesis of Eichengreen and Mitchener (2004) who characterize the Great Depression as a credit boom gone wrong. Although the authors take into account the general credit expansion, I focus on the particular form of credit that experienced the largest relative boom in the period, which is brokers' loans. Brokers' loans were directly related to the asset price bubble. The method proposed in this paper is able to quantify the impact of credit expansion on further inflating asset prices.

This research finding is related to recent work on credit supply shocks, economic activity and asset prices (Mian et al. (2017a); Gilchrist et al. (2018)). Most of this line of investigation focus on the housing market and the mortgage channel, specially during the 2000s expansion. In turn, I was able to demonstrate that stocks are also assets that can be subject to the dynamics of the credit cycle, by measuring how much the price of stocks were influenced by credit conditions during the 1920s expansion.

Finally, the fact that credit to buy stocks on leverage can be potentially fueling an asset price bubble, and playing a role in economic booms and busts has policy implications. Eichengreen and Mitchener (2004) suggest that policy makers should act to prevent the development of unsustainable credit booms, that may have serious negative macroeconomic and financial consequences when they turn to bust. After the Financial Crisis of 2007/08, the need for specific policy regarding credit expansion became widely recognized. Policy makers have adopted a set of macroprudential financial regulation tools, such as the Basel III recommendations, but the interactions of monetary policy and financial stability measures are still actively debated. My contribution is to empirically demonstrate the relevance of an additional channel that occurs between the intensity of credit supplied on margin and stock price fluctuations. Hopefully, the findings can provide useful elements to the analysis of macroprudential policies designed to smooth the credit cycle and consequently prevent negative macroeconomic outcomes of credit busts.

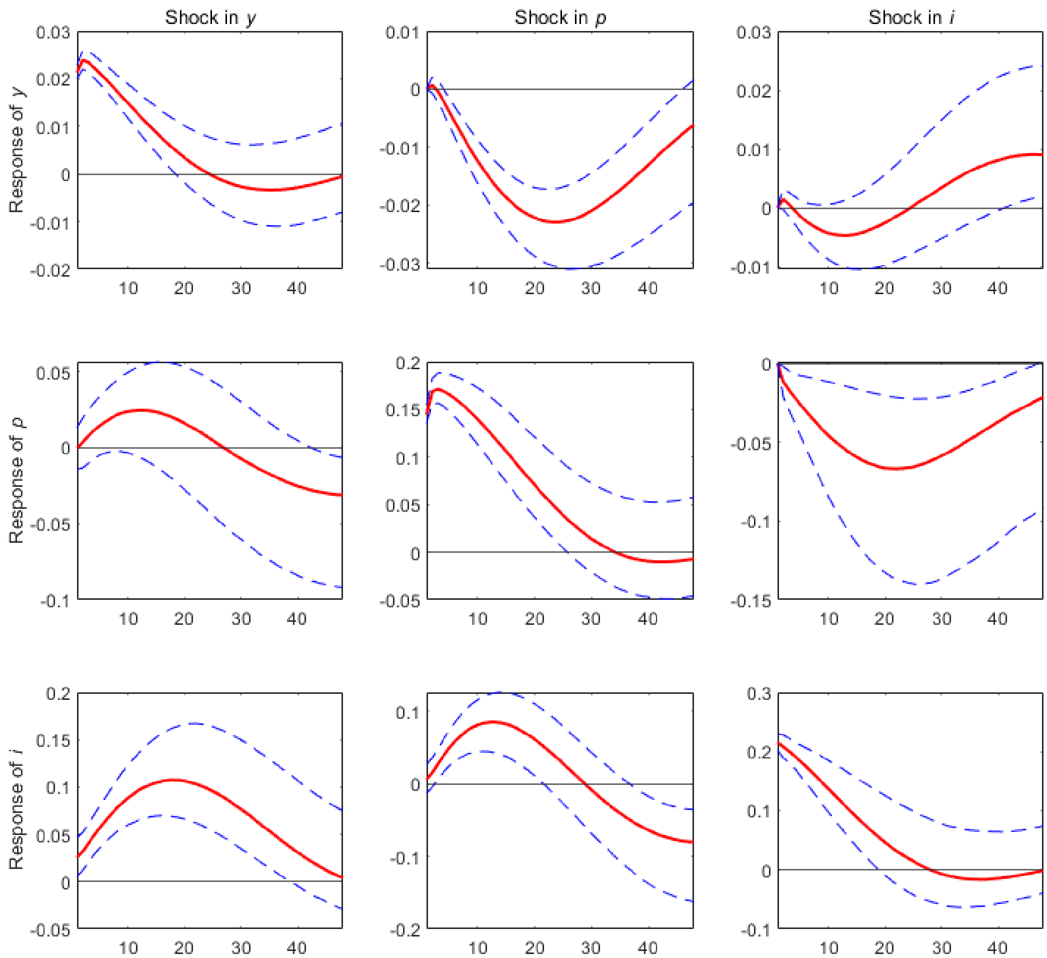


Figure 1.3: Impulse Response Functions in three equation model with recursive identification.

Note: Recursive ordering is (y, p, i). Red line is median IRF, blue dotted lines are one standard deviation credibility intervals. Sample is Dec/1919 to Sept/1929. Size of shock is one standard deviation.

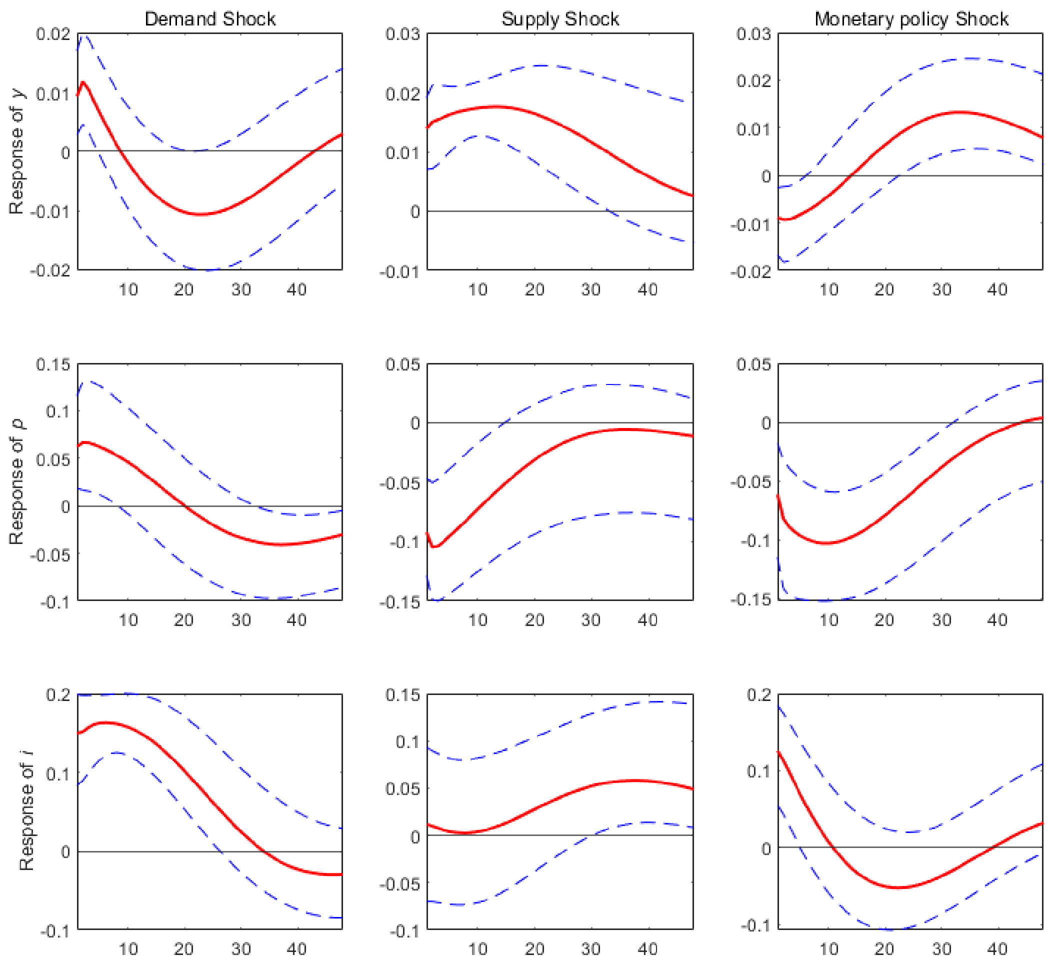


Figure 1.4: Impulse Response Functions in three equation model with identification by sign restrictions.

Note: Red line is median IRF, blue dotted lines are one standard deviation credibility intervals. Sample is Dec/1919 to Sept/1929. Size of shock is one standard deviation.

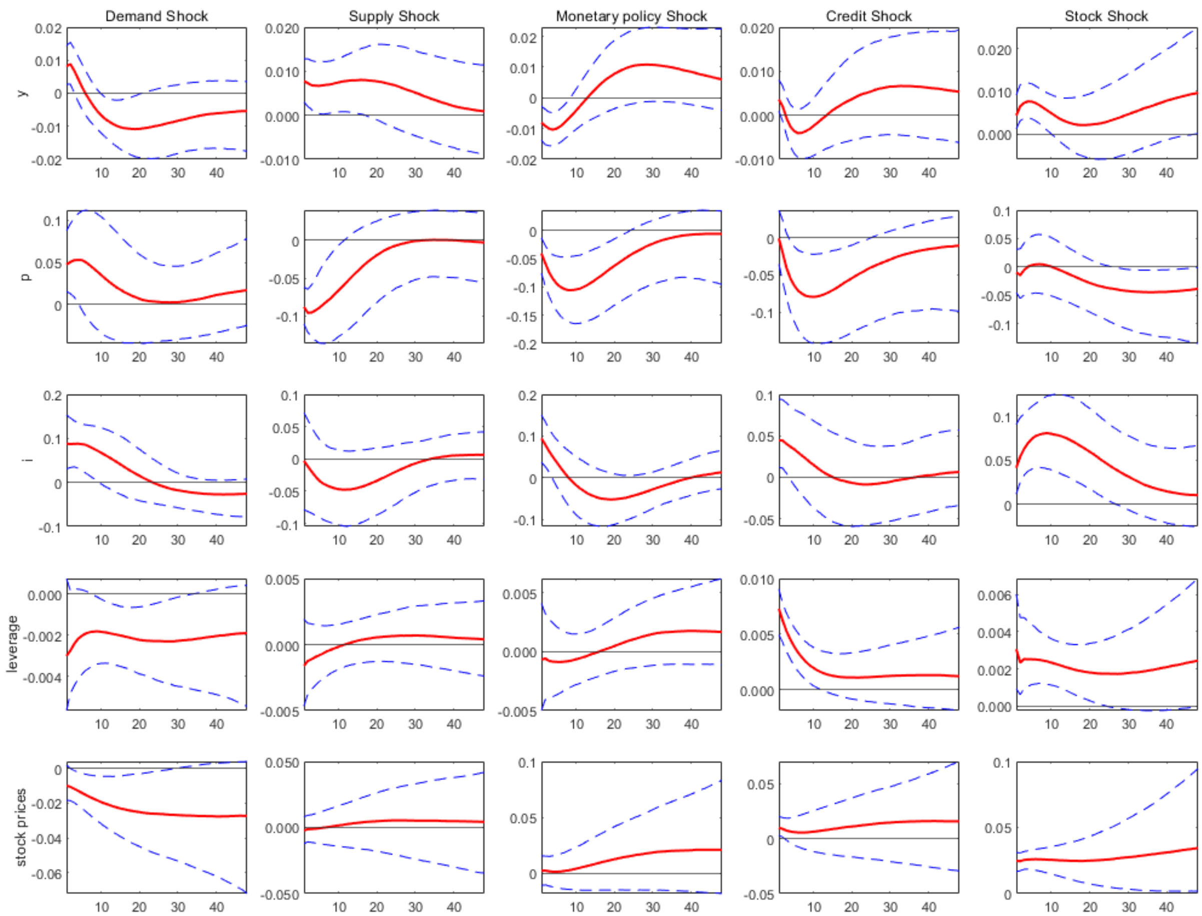


Figure 1.5: Impulse Response Functions in financial factors model with six variables.

Note: Shocks in columns are, respectively: demand, supply, monetary policy contraction, credit supply and stock preference. Red line is median IRF, blue dotted lines are one standard deviation credibility intervals. Sample is Dec/1919 to Sept/1929. Size of shock is one standard deviation.

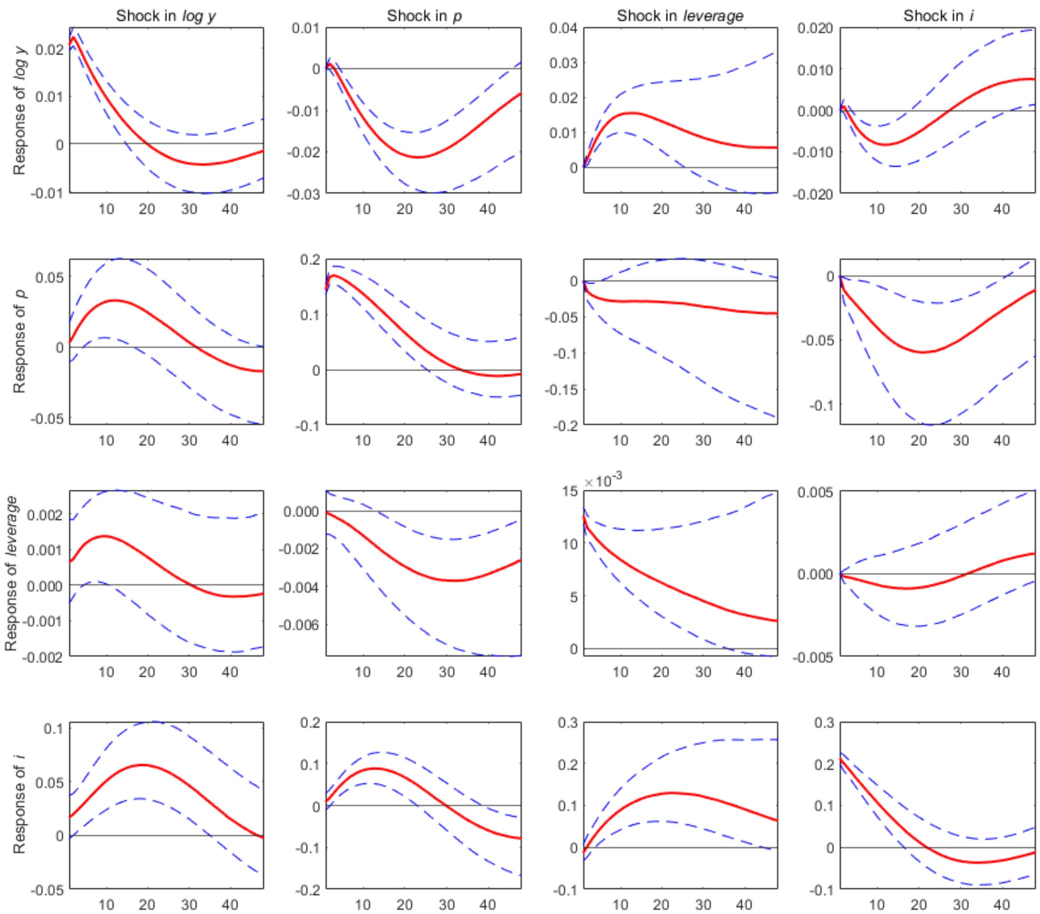


Figure 1.6: Impulse Response Functions in four variable model, with recursive identification.

Note: Recursive ordering is (y, p, $leverage$, i). Red line is median IRF, blue dotted lines are one standard deviation credibility intervals. Sample is Dec/1919 to Sept/1929. Size of shock is one standard deviation.

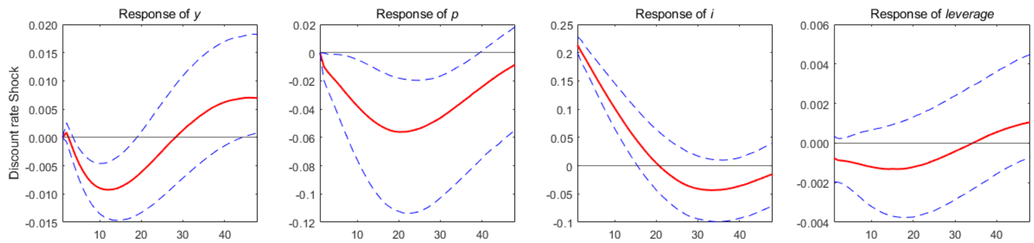


Figure 1.7: Impulse Response Functions in four variable model with recursive identification, alternative ordering.

Note: Recursive ordering is (y, p, i, $leverage$). Red line is median IRF, blue dotted lines are one standard deviation credibility intervals. Sample is Dec/1919 to Sept/1929. Size of shock is one standard deviation.

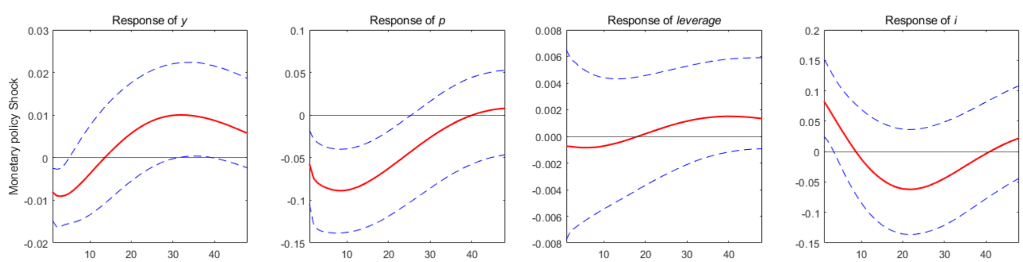


Figure 1.8: Impulse Response Functions to monetary policy tightening shock in four variable model, agnostic identification procedure.

Note: Columns are respectively responses of $(y, p, leverage, i)$. Red line is median IRF, blue dotted lines are one standard deviation credibility intervals. Sample is Dec/1919 to Sept/1929. Size of shock is one standard deviation.

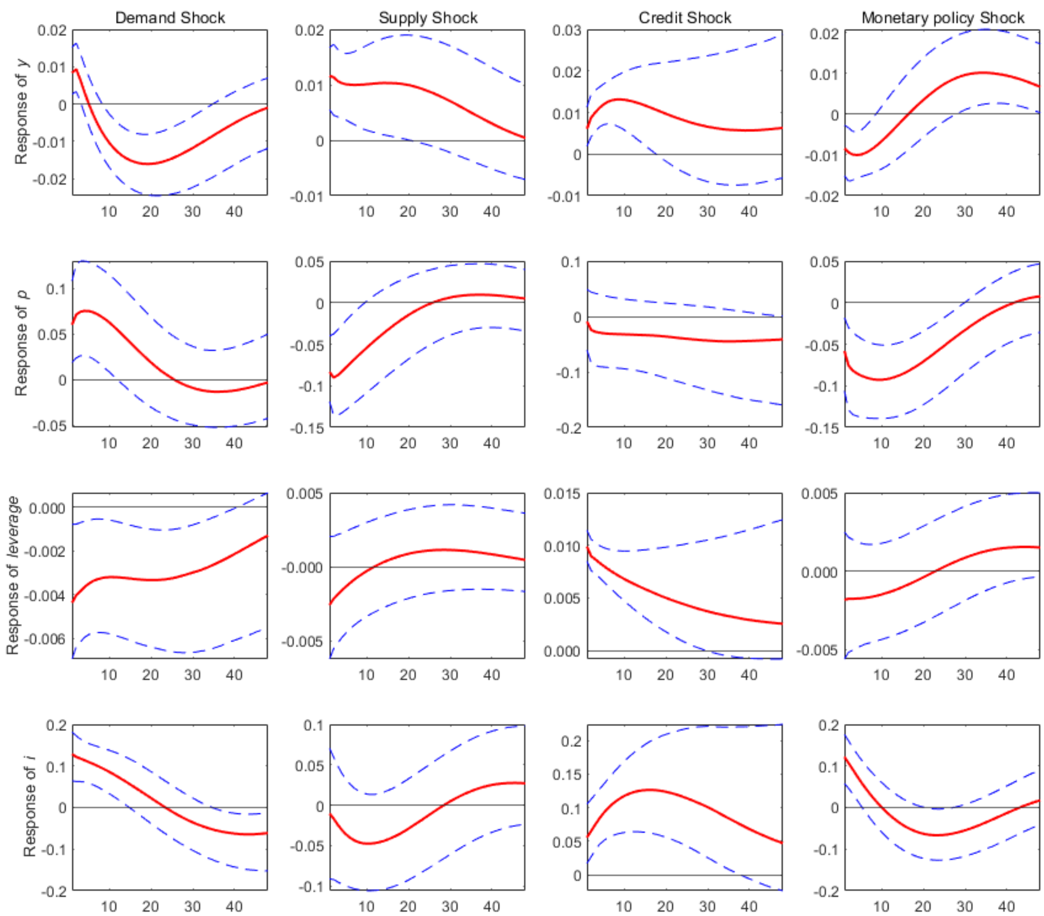


Figure 1.9: Impulse Response Functions in four variable model, using sign and variance decomposition restrictions identification.

Note: Red line is median IRF, blue dotted lines are one standard deviation credibility intervals. Sample is Dec/1919 to Sept/1929. Size of shock is one standard deviation.

Chapter 2

Capital Ratios and Bank Portfolio Allocation: a discrete choice approach

This chapter investigates how capital ratios are related to bank portfolio allocation during periods of financial distress. Optimal portfolio adjustment strategies are treated as discrete choices by the bankers. The expected correlation between capital and each strategy, when compared to estimates from a Bayesian discrete choice model, allows me to assess the most plausible shocks driving the response of banks. I analyze the behavior of US commercial banks during the 1990s "credit crunch" , in terms of adjustments in the loans and securities portfolio. The findings suggests that the adoption of risk-based capital requirements contained in the Basel Accord was not the most important driver of the "credit crunch", but banks were more likely recovering from negative shocks to capital, constrained by the leverage ratio requirements, and reacting to the economic environment.

2.1 Introduction

This paper revisits a classical empirical question in banking: how capital affects bank portfolio allocation, in terms of changes in the holdings of loan and securities, during periods of financial distress. The topic has been subject to intense debate during the early 1990s "credit crunch" (Bernanke et al. (1991); Berger and Udell (1994); Lown et al. (1994); Sharpe et al. (1995); Shrieves and Dahl (1995)). It has also regained attention after the 2007/2009 financial crisis (Berger and Bouwman (2013); Carlson et al. (2013)), as it basically frames any analysis of capital regulation and post-crisis reforms. In this section, I will briefly discuss the main motivation of the paper, before introducing the research strategy and contributions.

2.1.1 Motivation: the 1990s "credit-crunch"

The decision of banks on how to adjust their portfolio of assets is part of the Asset and Liability Management (ALM) process (Rosen and Zenios (2006)). At each time period, banks choose to increase, decrease or maintain the same level of a particular asset class, with the objective of maximizing its profits given the estimated risk and return of the asset (Furfine (2001)). This decision depends on economic and market conditions which influence the demand for assets, and is constrained by the liability structure of the bank and by current regulation standards. For example, regulatory changes which require financial institutions to increase their levels of capital may induce restrictions in the supply of credit during a certain period of time, until the banks reach the new targeted capital position. This is important because a negative shock to the supply of credit could potentially cause a reduction in macroeconomic activity, given that many borrowers cannot easily substitute their sources of funding.

Starting in 1990, the US implemented for its banking sector the first round of the Basel

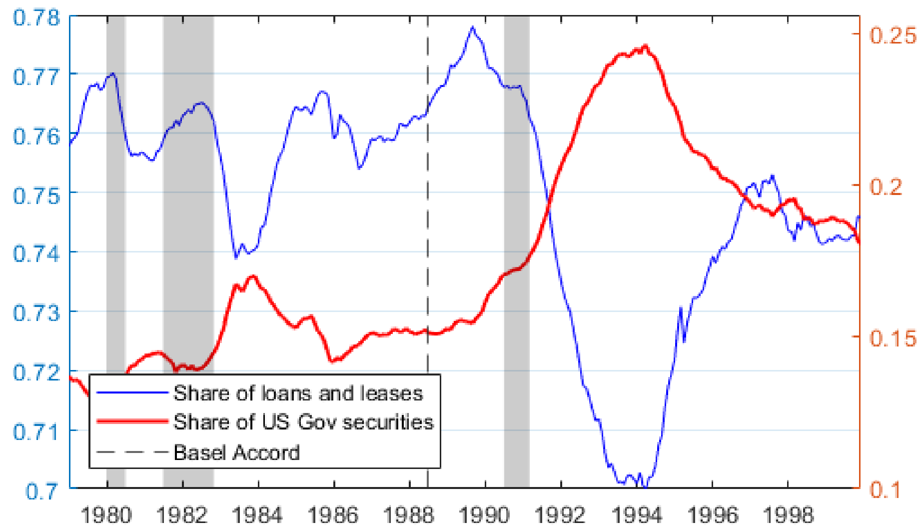


Figure 2.1: U.S. banks portfolio share from Jan/1979 to Dec/1999: loans vs. US Government securities.

Note: Share of total bank credit invested in loans and leases (blue line, left axis) and US Government securities (thick red line, right axis). Data is for all commercial banks, monthly, seasonally adjusted. Grey bars are NBER recession dates. Source: Federal Reserve H8 and NBER.

Accord, signed by its members in July 1988. The Accord mandates, for the first time, *risk-based capital requirements*, which establish different capital requirements depending on the perceived riskiness of assets (Furfine (2001)). At the same time, US regulators added a *leverage requirement*, mandating that banks hold a minimum capital ratio, which is calculated by considering all assets with the same weight (Berger and Udell (1994)). For reasons that shall become clear, most of the attention was draw to the newly introduced risk-based capital requirements. It states, in particular, that banks should hold a higher ratio of capital per loan than per government securities, as loans are considered to carry much higher risk. Because capital is more expensive to raise than insured deposits, risk based requirements made lending relatively more expensive than investing in securities, giving banks an incentive to shift their portfolios away from loans and into less risky instruments (Berger and Udell (1994)).

This was exactly what was observed in the US just after the introduction of the Basel Accord, in the beginning of the 1990s: a relevant phenomenon of portfolio shifting at banks, away from loans and into government securities, commonly referred to as the "credit crunch" (Bernanke et al. (1991)), as it can be seen in Figure 2.1. The share of total bank credit invested in total loans and leases fell from around 77% in 1989 to less than 70% in 1994. At the same time, the share of total bank credit invested in US Government securities increased from about 15% to nearly 25% over the same period. Given that the timing of the "credit crunch" coincided with the introduction of risk-based capital requirements and the 1990 recession[1], a great deal of research was devoted to assess causality (Berger and Udell (1994); Shrieves and Dahl (1995); Sharpe et al. (1995); Furfine (2001)). As usual, researchers face the inherent identification problem of how to distinguish whether the observed contraction in lending was caused by demand factors, e.g. recession, or coming from a negative shock to credit supply, which in turn may have been caused by more stringent capital requirements.

While previous literature is not unanimous regarding the causes of the decrease in lending, most papers indicate at least some role for capital shortage in the "credit-crunch". Bernanke et al. (1991) argue that demand factors caused much of the slowdown in lending activity through the US, although the shortage of equity capital limited banks' ability to extend loans in some regions. Shrieves and Dahl (1995) point to a additional of factors that include changes in the supervisory climate and in bank capital regulation, conjugated with changes in bankers' risk perception. The latter factor, together with inadequate capital is also pointed by Lown et al. (1994). Berger and Udell (1994) argue that explanations based on leverage, loan examination or voluntary risk-retrenchment by financial institutions are more consistent with the data than hypothesis that sustain risk-based regulation was binding. Hancock et al. (1995) provide evidence that capital shocks were larger, and portfolio responses to these shocks tended to be more rapid in the early 1990s when compared to the late 1980s. Peek and Rosengren (1995) find little independent role for capital ratios causing shrinkage of banks loans in the absence of regulatory enforcement actions. Contrary to previously cited studies, these authors argue that the observed portfolio reallocation reflect a response

[1]The recession lasted eight months, from July 1990 to March 1991, according to NBER dating.

forced by bank regulators rather than a voluntary behavior of banks' management, but their study is restricted to New England banks and they do not consider how enforcement actions may be an endogenous response of regulators towards weekly capitalized banks. Finally, Furfine (2001) suggests some role for regulatory involvement, either due to increased capital requirements or more intensive regulatory monitoring.

2.1.2 Bank's optimal response as a discrete choice

Structural models of optimal bank portfolio choice (Furfine (2001)) indicate that the response of banks is qualitatively different whether shocks are originated from changes in the regulatory minimum level of risk-based capital (RBC) or coming from an economic slowdown which reduces demand for credit. This can be summarized in Table 2.1, which is adapted from Furfine (2001). For each type of shock considered, the model predicts an optimal change in the rate of growth for each asset type - loans and securities. For example, an unexpected shock that increases risk-based capital requirements imply a substitution strategy between assets: a fall in the loan growth rate and a rise in the securities growth rate. On the other hand, a deepening economic recession imply lower growth for both loans and securities, in what I denominate a deleveraging strategy. The shocks are not distinguishable, though, by only looking at the aggregate behavior of the two main variables - changes in loan and securities growth. For instance, the deleveraging strategy can also be an optimal response to a negative shock to capital, which causes the bank to be below its targeted capital ratio, and even possibly non-compliant with the minimum regulatory leverage ratio (LR). This is the reason why Furfine (2001) estimates the full model and analyses changes in capital ratio, RBC ratio and equity issuing, in order to shed light on the prominent reasons for the "credit-crunch".

In this paper, I propose a novel approach. Given that the optimal responses of banks are

Shock	Predicted direction of change Loan growth	Securities growth	Strategy
Increase in risk-based capital (RBC) requirements	Fall	Rise	Asset substitution
Increase in regulatory monitoring	Fall	Rise	Asset substitution
Economic recession	Fall	Fall	Deleveraging
Negative shock to capital or binding LR requirements	Fall	Fall	Deleveraging

Source: Adapted from Furfine (2001).

Table 2.1: Predicted direction of change in bank portfolio due to shocks.

qualitatively different for the shocks we are interested in, portfolio allocation can be modeled as a discrete choice problem. The method can be described as follows. First, I consider observed changes in the holdings of two main assets types, loans and US securities, for each bank in the sample. The outcomes observed during the period of interested are then sorted, or discretized, between three classes: contraction, moderate growth, high growth. The model is estimated based on these discrete outcomes as dependent variables, conditional on a series of bank-specific factors, such as the capital ratio and risk-based capital ratio, which may be correlated with the adjustment choice. The result is an estimate of the probability of each discrete outcome, for each asset class, and for each bank. Additionally, I can obtain the marginal effect of a change in the covariate of interest, e.g. capital ratio, in the probability of each outcome, e.g. contraction in lending. As the model is multivariate, it considers the adjustment of different asset classes concurrently and their correlation. For instance, the model can evaluate the probability of contraction in lending and expansion of US securities, i.e. the choice of an asset substitution strategy, for a bank of a given size and level of capital. I will compare the expected correlation between capital and each portfolio allocation strategy to the estimates from the model in order to assess the most plausible shocks driving the response of banks.

The use of a discrete model can be justified from the following reasons. First, not all banks have experienced a contraction in loans during the "credit crunch" episode. Indeed, a relatively large number of institutions were able to increase the absolute level of loans, gaining market share even though the aggregate change was negative. From Mar/1990 to Dec/1992, 73.3% of the US commercial banks expanded their total loans and leases, while the aggregate decrease in volume was of 0.62%[2]. On the US Treasuries portfolio we observe an aggregate increase of 61.3% in dollars volume, even though 40.4% of the institutions decreased their holdings. A simple linear correlation statistics between the rates of growth in the two portfolios, loans and US Treasuries, is of very small magnitude, only -0.0079. This is all to say that it is far from clear whether on average banks were reacting with a substitution strategy, for example due to an increase in RBC requirements, during the "credit crunch" even though the aggregate data may suggest that fact. It raises the question of which types of banks may be choosing each strategy and how that might be correlated with initial levels of capital. Second, the adopted econometric approach directly addresses the non-linearity issue of the elasticity of bank lending with respect to capital ratios. During a typical financial crisis, higher capital ratios are usually related to less contraction in bank lending activity. However, it is plausible to assume that banks near the regulatory minimum capital might respond differently to changes in capital than banks far from that threshold. In other words, the marginal effect of one additional unit of capital may depend on the current level. Indeed, this point was already mentioned by the early study of Bernanke et al. (1991) but it was very rarely addressed by existing empirical studies, which most of the time adopt linear specifications. Third, inference based on the full estimation of a structural non-linear model may be more sensitive to model uncertainty as it depends on assuming particular functional forms, and hindered by higher parameter variance. By taking a discrete choice approach, the portfolio allocation problem is considered from a very general perspective, which also requires less restrictive assumptions.

The objective of this paper is, thus, to investigate how inital capital ratios are correlated with

[2]This period is typically considered to be the crisis (Berger and Bouwman (2013)). Data is from a sample of 10,971 commercial banks, and it is treated for banks merging during the observation interval. Procedure is detailed in the following section of the text.

the probability of banks making different portfolio allocation choices in their asset side during periods of financial distress, often characterized by contraction of lending. I analyze the behavior of US commercial banks during the 1990s "credit crunch" period, from Mar/1990 to Dec/1992, focusing in two main asset types: total loans and leases, and US securities. I compare the findings with a benchmark non-crisis period from Mar/1995 to Dec/1996, in order to assess whether capital played a special role during the crisis. The research explores how portofolio choices are related to different measures of capital, the *Capital Ratio* and the *Risk-Based Capital Ratio*, which are both subject to specific regulatory limits.

I find that the initial *Capital Ratio* is negatively correlated with the probability of contraction for both loans and securities during the "credit crunch" period. Less capitalized banks are more likely to choose to contract both asset classes at the same time, characterizing a typical deleveraging strategy. On the other hand, the correlation between *Risk-Based Capital Ratio* and portfolio adjustment is less clear. In the preferred specification, banks with lower levels of RBC ratios are more likely to increase the holdings of securities, while also increasing the amount of loans, although the latter result is subject to considerable uncertainty. In other words, I find no evidence that the asset substitution strategy is correlated with lower levels of risk-based capital. Overall, the findings are consistent with the hypothesis that either leverage ratio requirements where a binding constraint for banks, or the economic recession affected the demand for loans from less capitalized banks more heavily during the "credit-crunch". The findings offer less support for explanations based on the introduction of risk-based capital requirements by the Basel Accord regulation as a main cause for the crunch.

During the non-crisis period, instead, I find a different relationship between capital and asset allocation choice. Both *Capital Ratio* and *Risk-Based Capital Ratio* are negatively correlated with large expansions in loans. This characterize a leverage cycle, in which less capitalized banks, and banks holding more loans-to-securities in their portfolios, are more likely to expand loans heavily. Additional findings indicate that liquidity, profitability and loan quality are positively correlated with loan expansion, either during crisis or non-crisis periods. I also confirm the non-linear effect of the capital ratio, implying that the marginal effect of capital is greater (in magnitude) for less capitalized banks. Besides, the holding of US Treasuries is subject to mean reversion: less liquid banks *ex-ante* are more likely to increase the holdings of securities. All the main results in the paper are robust to a different treatment of bank mergers occurring during the observation period.

The main contribution of this research is to propose and test an econometric methodology based on discrete choice in order to study how bank portfolio allocation is conditional on capital levels and other banks' characteristics. The rationale for the method is that optimal responses to shocks are qualitatively different, and thus can be considered as distinct discrete choices, at from a strategic viewpoint. The method's advantages are to consider asset choices concurrently for both risky and riskless assets, which better identifies adjustment strategies, to assume non-linearity of marginal effects of covariates, and to require less restrictive assumptions than structural models. I have argued that the discrete choice method provides useful insights for the understanding of the 1990s "credit crunch", which complements previous research.

The paper is related to a wide literature that addresses the impact of regulatory capital requirements on bank lending, bank performance during financial crisis, and more generally the behavior of the financial system along the credit cycle. Recent work include Beltratti and Stulz (2012) and Berger and Bouwman (2013) who investigate bank performance in crisis events using conventional linear or survival models, and Carlson et al. (2013) who innovates in terms of a matched approach to control for local demand. Naturally, my research is also related to literature specific about the 1990s "credit-crunch", where the main question of interest has been to what degree the introduction of Basel RBC regulation was responsible for the aggregate contraction of credit and for the surge in holdings of government bonds by

US banks (Bernanke et al. (1991); Berger and Udell (1994); Lown et al. (1994); Sharpe et al. (1995); Shrieves and Dahl (1995); Furfine (2001)).

The remainder of the paper is organized as follows. Section 2.2 describes the data sources and Bayesian econometric estimation procedures. Section 2.3 presents and analyses the findings obtained from the estimation of the model on the crisis and non-crisis periods. Section 2.4 concludes and provides directions for future research.

2.2 Data and methodology

This section describes the data sources, the discretization procedure and the discrete choice econometric model applied to estimation.

2.2.1 Data sources and discretization

The dataset is at bank-level, composed by income and balance sheet data from the Reports of Income and Condition (Call Reports). In terms of timing, I consider the 1990s "credit crunch" as a banking crisis covering the period from March/1990 until Dec/1992. Capital ratios and other covariates are observed just before the crisis, at Dec/1989, while the response variables, i.e. growth rates of each asset, are observed during the crisis period. This is the typical procedure used in the literature (Bernanke et al. (1991); Berger and Bouwman (2013)). The model is also estimated for a non-crisis period, from Mar/1995 to Dec/1996, in order to better explore the research hypothesis.

The main sample contains all U.S. commercial banks which were active during the full period and hold at least \$100,000 in *Total loans and leases* in the baseline date. Institutions which fail within the period are currently being ignored. Although this procedure is standard

(Bernanke et al. (1991); Berger and Bouwman (2013); Carlson et al. (2013)), it may introduce survivorship bias in the estimation. I leave this problem to be addressed by future work. Adjustments for bank mergers can be done in two ways. The first and simpler procedure ignores the acquired banks, and use only the reported balance sheets of the merge survivors (Carlson et al. (2013)), which may change substantially during the observation window when the merge occurs. This procedure implies higher rates of growth for merge survivors, but it is adequate if we are interested in assessing how capital and other bank conditions may be enablers of a merger-driven growth strategy. An alternative procedure treats the mergers as if they occurred previously to the observation window. So, the balance sheets of the acquired banks are summed up with the merge survivor for the baseline date and all other future dates (Bernanke et al. (1991)). The objective here is to obtain conservative estimates of factors driving credit growth. I choose the first and simpler procedure as my baseline and reported case, but most findings are robust for both procedures of dealing with bank mergers.

The outcome variable of interest is the growth rate of each asset class. Let us call it $\tilde{y}_{i,j}$ for bank $i = 1, ..., I$ and asset class $j = 1, ..., J$. I am interested in two assets: *Total loans and leases* and *US Government securities*. The methodological approach starts by classifying the outcome $\tilde{y}_{i,j}$ in one of three possible discrete classes, or categories, named $y_{i,j}$, according to the relative level of observed change. Recall that the objective of the discrete model is to assess the probability of a relatively "bad" outcome. So, the first class ($y_{i,j} = 1$) is defined as negative growth, or any value $\tilde{y}_{i,j} < 0$. The second class ($y_{i,j} = 2$) will represent positive but relatively small growth, which I define by growing less than the median change for asset j. Formally, I let $y_{i,j} = 2$ in case $0 \leq \tilde{y}_{i,j} \leq \gamma_{2j}$. Finally, the third class ($y_{i,j} = 3$) represents the best relative outcome in terms of growth, which I define as growing more than the median change, or $\tilde{y}_{i,j} \geq \gamma_{2j}$.

The discretization procedure will allow me to apply a multivariate discrete choice model, to be estimated by standard Bayesian methods (Jeliazkov et al. (2008)). The model provides a

	Mean	Median	Std. Dev.	Min.	Max.
Growth rate in Total Loans	0.237	0.163	0.391	-0.330	1.574
Growth rate in US Treasuries	1.603	0.241	3.315	-1.000	10.521
Capital ratio	0.092	0.083	0.049	-0.030	0.993
Risk-Based Capital ratio	0.101	0.090	0.040	0.050	0.230
Assets (in millions)	259	44	2,490	1	162,000
Liquidity ratio	0.36	0.34	0.15	0.00	0.98
Core deposits ratio	0.77	0.80	0.11	0.00	0.98
Brokered deposits ratio	0.00	0.00	0.02	0.00	0.71
Trading assets ratio	0.00	0.00	0.01	0.00	0.58
Loan concentration ratio	0.34	0.31	0.11	0.00	1.00
ROE	0.09	0.11	0.09	-0.21	0.23
Provision for loan losses ratio	0.01	0.00	0.01	0.00	0.04
Share C&I loans	0.23	0.20	0.15	0.00	1.00
Share Real Estate C&I loans	0.13	0.10	0.11	0.00	1.00
Share Real Estate Residential loans	0.28	0.27	0.16	0.00	1.00
Share Consumer loans	0.22	0.19	0.14	0.00	1.00
Charge off ratio C&I loans	0.03	0.01	0.05	0.00	0.20
Number of observations	$N = 10,971$				

Notes: Growth rates calculated from Mar/1990 to Dec/1992. Remaining balance-sheet variables as of Dec/1989. Variables winsorized at 3% are *Growth rate in Total Loans, Growth rate in US Treasuries, Risk-Based Capital Ratio, ROE, Provision for Loan Losses Ratio, Charge Off Ratio C&I loans*. Bank mergers are not treated on sample.

Table 2.2: Summary statistics for observed variables in the "credit-crunch" period.

	Num. obs. in each category			Total
	$y_i = 1$	$y_i = 2$	$y_i = 3$	
Growth rate in Total Loans	2,718	2,767	5,486	10,971
Growth rate in US Treasuries	4,329	1,156	5,486	10,971
	Share in each category			Total
	$y_i = 1$	$y_i = 2$	$y_i = 3$	
Growth rate in Total Loans	0.248	0.252	0.500	1.000
Growth rate in US Treasuries	0.395	0.105	0.500	1.000

Table 2.3: Summary statistics for discretized response variables, between 1990-1992 (crisis).

probability distribution of each bank experimenting each outcome conditional on the state of its Capital Ratio and other balance-sheet variables. In this type of model, the marginal effect of any covariate is already non-linear by construction (Jeliazkov et al. (2008)).

Most variable calculations are defined following previous literature (Berger and Bouwman (2013)), and can be described as follows. *Capital Ratio* is my main variable of interest calculated as equity capital divided by total assets. *Risk-Based Capital Ratio* is a proxy for the risk-based capital ratio defined by the Basel Accord. In the original regulatory definition, all assets are assigned risk weights between 0 and 100 percent according to their perceived credit risk, and added to form a measure of risk weighted assets (RWA). The risk-based capital is then the ratio between equity capital to RWA. Due to data limitations, my proxy is calculated by assigning 100 percent weight to every asset, except US treasuries which enter with zero weight. Even though not precise, I expect the proxy to be well correlated with risk-based capital in order for the results to be useful.

Assets is a measure of size and it is the only variable that enters the model in logs. Banks of different size may adopt distinct business models, and face distinct demand conditions. A more mechanical aspect is that larger banks may find it harder to adjust their portfolio in percentage terms, simply due to their size. *Liquidity Ratio* captures the relative amount of liquid assets available to the bank, and it is calculated as total securities plus Fed funds sold divided by total assets. Banks in a more liquid condition are expected to face less constraints

	Mean	Median	Std. Dev.	Min.	Max.
Growth rate in Total Loans	0.244	0.186	0.258	-0.122	1.109
Growth rate in US Treasuries	0.122	0.037	0.387	-0.425	1.424
Capital ratio	0.10	0.09	0.04	0.00	0.94
Risk-Based Capital ratio	0.16	0.13	0.12	0.00	5.75
Assets (in millions)	371	56	3,840	1	210,500
Liquidity ratio	0.36	0.34	0.15	0.00	0.99
Core deposits ratio	0.78	0.80	0.11	0.00	0.96
Brokered deposits ratio	0.00	0.00	0.03	0.00	0.91
Trading assets ratio	0.14	0.11	0.13	0.00	0.84
Loan concentration ratio	0.35	0.31	0.12	0.00	1.00
ROE	0.12	0.12	0.05	-0.01	0.24
Provision for loan losses ratio	0.003	0.002	0.004	-0.004	0.016
Share C&I loans	0.18	0.16	0.13	0.00	1.00
Share Real Estate C&I loans	0.16	0.13	0.13	0.00	1.00
Share Real Estate Residential loans	0.33	0.32	0.18	0.00	1.00
Share Consumer loans	0.19	0.16	0.14	0.00	1.00
Charge off ratio C&I loans	0.01	0.00	0.02	0.00	0.10
Number of observations	$N = 9,163$				

Notes: Growth rates calculated from Mar/1995 to Dec/1996. Remaining balance-sheet variables as of Dec/1994. Variables winsorized at 3% are *Growth rate in Total Loans, Growth rate in US Treasuries, Risk-Based Capital Ratio, ROE, Provision for Loan Losses Ratio, Charge Off Ratio C&I loans*. Bank mergers are not treated on sample.

Table 2.4: Summary statistics for observed variables in non-crisis period.

on adjusting their balance sheet during crisis. Core deposits are considered to be a more stable source of funding to financial institutions, and less sensitive to small variations on the interest payed on deposits. The *Core Deposits Ratio* is defined as the sum of demand, savings and small time deposits (less than $100,000) divided by total assets. Again, banks with higher core deposits ratios are expected to face less withdrawals during periods of distress, giving them more ability to adjust assets. At the same time, deposit insurance associated with this type of liabilities may induce additional risk taking by the bankers (Berger and Bouwman (2013)). Brokered deposits are large and relatively expensive types of deposits, and are an important factor to increase the risk of failure during crisis (Berger and Bouwman (2013)). I include the *Brokered Deposits Ratio* as the ratio of brokered deposits over total assets. Their expected effect on asset allocation is not clear *a priori*, specially as they were less widely used during the 1990s in comparison to the recent period post 2000s. Trading assets are

	Num. obs. in each category			Total
	$y_i = 1$	$y_i = 2$	$y_i = 3$	
Growth rate in Total Loans	976	3,605	4,582	9,163
Growth rate in US Treasuries	3,964	617	4,582	9,163
	Share in each category			Total
	$y_i = 1$	$y_i = 2$	$y_i = 3$	
Growth rate in Total Loans	0.107	0.393	0.500	1.000
Growth rate in US Treasuries	0.433	0.067	0.500	1.000

Table 2.5: Summary statistics for discretized response variables, between Mar/1995 to Dec/1996 (non-crisis).

held for resale, and their position is subject to frequent reevaluation, making them harder to monitor. Given their complexity, I include *Trading Assets Ratio* as a control, calculated as assets held in trading accounts divided by total assets. The *Loan Concentration Ratio* captures the degree of concentration on the loan portfolio, by calculating a Herfindahl-Hirschman Index (HHI) of the five main loan categories: agricultural, individuals, family real estate, commercial real estate and commercial and industrial loans (C&I). The index varies from zero to one, and the higher values represent more concentrated portfolios, which are supposed to increase risk. *ROE* is a measure of profitability, as more profitable banks are expected to face less constraints to increase their portfolio of assets if they chose to do so. I calculate *ROE* as net income divided by stockholders equity.

The quality of the loan portfolio held by banks is captured by the variables *Provision for Loan Losses Ratio* and *Charge-off Ratio C&I loans*. As argued by Lown et al. (1994), the slowdown in bank lending during the early 1990s was, at least in part, originated from large debt burdens and losses incurred by the banking system during the 1980s. According to the authors, by the year 1990, banks were under pressure to increase their loan loss provisions as the rate of nonperforming loans surged. Thus, I include *Provision for Loan Losses Ratio* measured as provisions for loan losses divided by total loans and leases, and expect this variable to be highly correlated with contractions in loans. An alternative measure of portfolio quality, even though more restricted, is the *Charge-off Ratio C&I loans* calculated as charge offs less recoveries in C&I loans divided by total C&I loans. This variable is used by Carlson et al. (2013) who find it to be negatively correlated with loan growth. Additionally, I control for differences in the loan portfolio by calculating the shares of the main four classes of loans: C&I, real estate C&I, real estate residential, and consumer loans.

Survivor from Merge is an indicator variable assigned to one if the bank is a buyer in a merge which takes place during the observation period and zero otherwise. Note that this indicator variable is only present when we treat the sample for bank mergers, which means we are summing up the balance-sheets of merged banks through the whole period. Intuitively, merge survivors are expected to increase their total assets, although nothing can be assumed *a priori* about composition. Some variables are winsorized at 3% in order to avoid the results being driven by outliers, as it is usual in the literature (Berger and Bouwman (2013)). These are the response variables *Growth rate in Total Loans, Growth rate in US Treasuries* and the covariates *Risk-Based Capital Ratio, ROE, Provision for Loan Losses Ratio, Charge Off ratio C&I.*

Table 2.2 displays the summary statistics for the covariates and response variables used in the baseline model for the "credit-crunch" period, when ignoring bank mergers. The baseline model included 14 covariates, plus controls for State[3] and an intercept. The total number of covariates is 70, for a sample size of 10,971. Some characteristics of the banks in the sample are worth noticing. First, there exist substantial variation in the capital ratios. While the mean bank reaches a 0.092 ratio, standard deviation is 0.049 and the range goes from the negative side to almost one. The risk-based measure of capital has a higher mean and median, smaller standard deviation and range, as expected. Second, the distribution of the bank size is very asymmetric. While mean asset value is the relatively small amount of \$ 259 Million, there are a few very large institutions. Third, there is a fair deal of variation

[3]For the State controls, I use the variable *State Code*, which provides 55 different classifications. A dummy variable is created for each case.

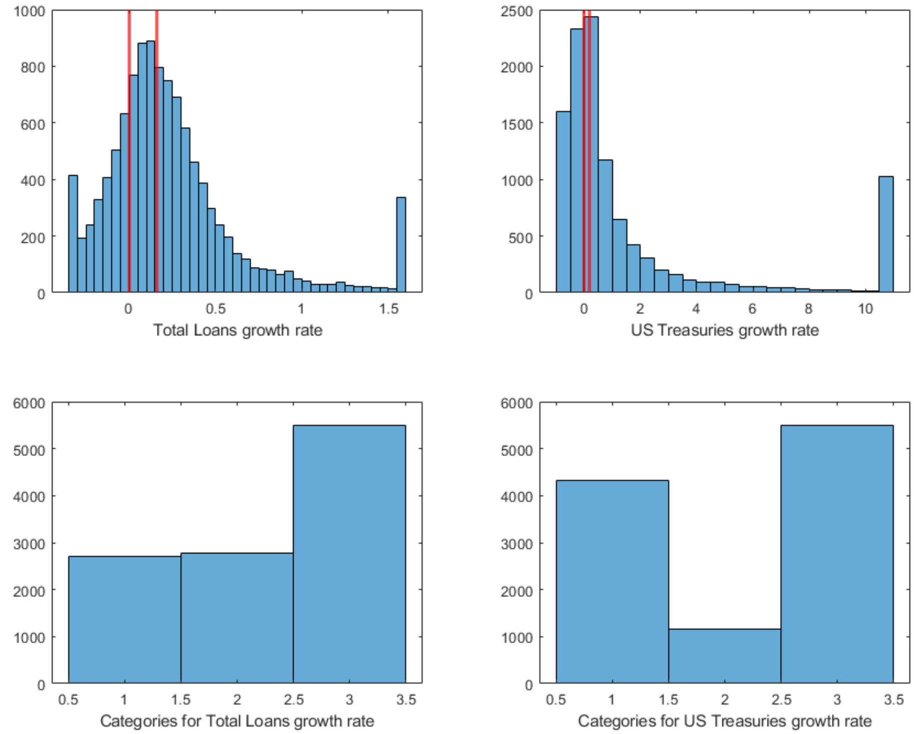


Figure 2.2: Frequency distribution of growth rates for *Total loans and leases* and *US Treasury securities* during the "credit-crunch".

Notes: Period is from Mar/1990 to Dec/1992. Variable *Total loans and leases* is in left columns and *US Treasury securities* is in right columns. First row shows winsorized distribution of original observed variables. Vertical red lines are set at zero and median value. Second row shows distribution of categories ($y_{ij} = 1, 2, 3$), in discretized variables. Categories are defined as: $y_{ij} = 1$ is negative growth, $y_{ij} = 2$ is positive growth below median, $y_{ij} = 3$ is positive growth above median. Number of observations N=10,971.

in liquidity and profitability of banks, as well as in the share of core deposits. On the other hand, most banks do not carry or do not report brokered deposits nor trading assets. Finally, regarding loan quality variables, provisions for loan losses vary from zero to 4%, and charge-off ratios of C&I loans from zero up to 20%, which shows that at least some banks were suffered significant losses on their loan portfolios on the previous quarters.

The respective summary statistics for the non-crisis period, from Mar/1995 to Dec/1996, are presented in Table 2.5. In comparison to Table 2.2, we observe that, after recovering from the "credit-crunch", surviving banks held more capital, by both measures, were more profitable and carried less problematic loans. The institutions were equally liquid and had similar shares of core deposits as before. Regarding the composition of the loans portfolio, the share of residential real estate loans increased by most, while C&I loans suffered the highest reduction. Moreover, it became more common for banks to report a larger amount of trading assets.

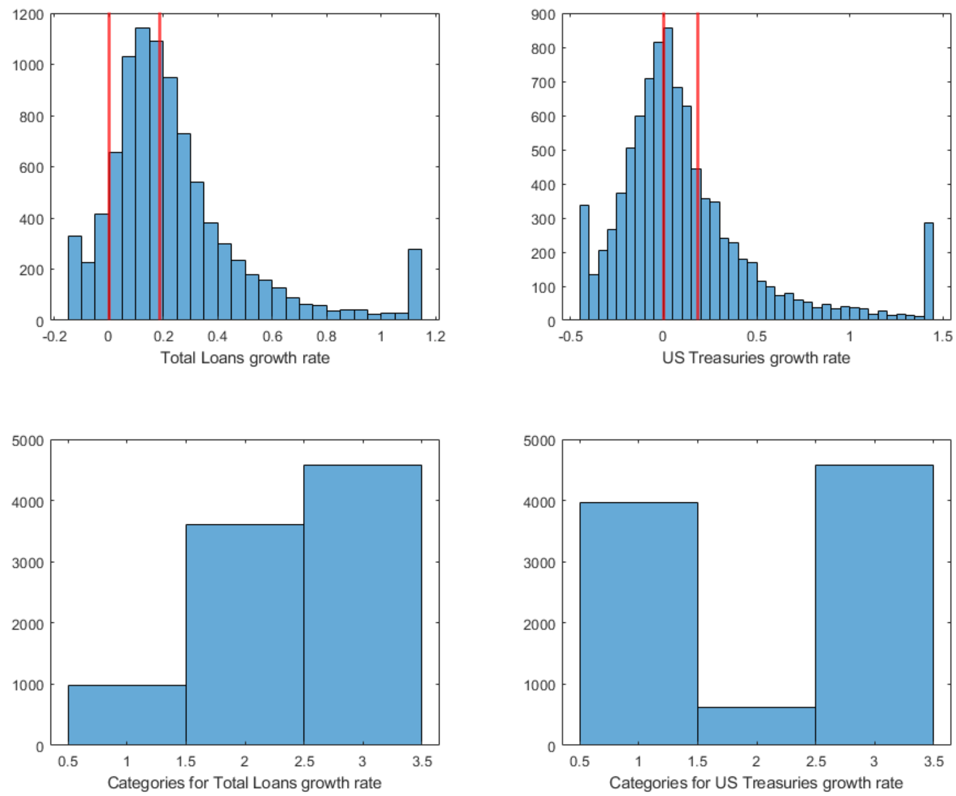


Figure 2.3: Frequency distribution of growth rates for *Total loans and leases* and *US Treasury securities* during non-crisis period.

Notes: Period is from Mar/1995 to Dec/1996. Variable *Total loans and leases* is on left columns while *US Treasury securities* is on right columns. First row shows distribution of original winsorized observed variables. Vertical red lines are set at zero and median value. Second row shows distribution of categories ($y_{ij} = 1, 2, 3$), in discretized variables. Number of observations N=9,163.

Table 2.3 and Figure 2.2 provide an overview of the discretized response variables for the crisis period. As mentioned in Section 2.1, 24.8% of banks exhibited contraction in lending, while 39.5% contracted US Treasury holdings. The equivalent for the non-crisis period are Table 2.5 and Figure 2.3, which show that contraction in loans was much less frequent than before, 10.7% of cases, while contractions in US Treasuries were slightly more frequent and happened for 43.3% of banks. Note that the sample is large enough in each category and time period to allow for estimation.

2.2.2 Model and Bayesian estimation

As previously explained, observed $\tilde{y}_{i,j}$ represents the growth rate in the level of asset class j for bank i, from the beginning until the end of a specific period of financial distress. I classify the outcome $\tilde{y}_{i,j}$ for each bank into three discrete classes, $y_{i,j} = \{1, 2, 3\}$, according to the relative level of change. For estimation purposes, the discrete variable $y_{i,j}$ is considered observed from now on. Given the structure adopted, the J choices are mapped in an ordinal scale. The set of covariates X_i include measures of capital ratio, controls for bank size and business model, profitability, as well as proxies for loan quality and market conditions. Assuming Gaussian disturbances, the model is characterized as multivariate ordinal Probit and estimated by standard Bayesian econometric techniques.

Following Jeliazkov et al. (2008), I augment the model with a latent variable z_i:

$$z_i = X_i\beta + \varepsilon_i \tag{2.1}$$

$$y_{i,j} = \begin{cases} 1 & , \text{if } -\infty < z_{i,j} < \gamma_{1,j} \\ 2 & , \text{if } \gamma_{1,j} < z_{i,j} < \gamma_{2,j} \\ 3 & , \text{if } \gamma_{2,j} < z_{i,j} < \infty \end{cases} \tag{2.2}$$

where $\varepsilon_i \sim N(0, \Omega)$. Observables are $y_{i,j}$, the discrete choices j for individual i, and the covariates X_i which we assume are either features of the individual i or the environment. There are K possible covariates per individual, including a constant. So, the vector z_i is size $J \times 1$, matrix X_i is size $J \times K$, the vector of coefficients of interest is β size $K \times 1$. The covariance matrix Ω is size $J \times J$, and it is normalized to have ones in the main diagonal.

Bayesian estimation is done using a Gibbs-sampler for the augmented model as in Jeliazkov et al. (2008). The procedure can be summarized as follows.

- Sample z_i from a truncated Normal, given (y, β, Ω). The region of truncation $\mathcal{B}_{ij}$ depends on y_i. For each individual z_i, we sample $j = 1, ..., J$ componentes, one at a time, using the full conditional distributions.
  $[z_{i,j}|z_{i,not(j)}, \beta, \Omega, y_i] \sim TN_{B_{ij}}(\mu_{i,j|i,not(j)}, \Omega_{j,j|not(j)})$
- Sample β from Normal, given (z, Ω).
  $[\beta|z, \Omega] \sim N(\hat{\beta}, \hat{B})$, where
  $\hat{B} = (B_0^{-1} + \sum_{i=1}^{N} X_i'\Omega^{-1}X_i)^{-1}$ and
  $\hat{\beta} = \hat{B}(B_0^{-1}\beta_0 + \sum_{i=1}^{N} X_i'\Omega^{-1}z_i)^{-1}$
- Sample Ω from Inverse Wishart, given (z, β).
  $[\Omega^{-1}|z, \beta] \sim Wish(r_0 + N, (R_0^{-1} + \sum_{i=1}^{N}(z_i - X_i\beta)(z_i - X_i\beta)')^{-1})$

The prior hyperparameters to be chosen are: (β_0, B_0^{-1}), the location and precision for the β parameter; and (r_0, R_0^{-1}), the tightness and location for the covariance parameter Ω. I assume little previous knowledge of the model parameters, and choose the priors $\beta_0 = 0_{KJ\times 1}$, $B_0^{-1} = \frac{1}{3}I_{KJ\times KJ}$, $r_0 = 4$ and $R_0^{-1} = 5I_{J\times J}$.

Model selection is done using Bayesian methods of model comparison (Greenberg (2012); Jeliazkov et al. (2008)). In general, given observed data y, we are interested on the collection of models $\{\mathcal{M}_1, ..., \mathcal{M}_L\}$ representing competing hypothesis about y. Each model $\mathcal{M}_l$ is characterized by a model-specific parameter vector θ_l and a sampling density $f(y|\mathcal{M}_l, \theta_l)$. Bayesian model selection compares the models $\mathcal{M}_l$ through their posterior odds ratio, which for any two models $\mathcal{M}_i$ and $\mathcal{M}_j$ is written as

$$\frac{P(\mathcal{M}_i|y)}{P(\mathcal{M}_j|y)} = \frac{Pr(\mathcal{M}_i)}{Pr(\mathcal{M}_j)} \times \frac{m(y|\mathcal{M}_i)}{m(y|\mathcal{M}_j)} \tag{2.3}$$

where $m(y|\mathcal{M}_l) = \int f(y|\mathcal{M}_l, \theta_l)\pi_l(\theta_l|\mathcal{M}_l)d\theta_l$ is the marginal likelihood of $\mathcal{M}_l$, and $Pr(\mathcal{M}_l)$ is the prior probability of model $\mathcal{M}_l$. In equation (2.3), the first fraction on the right hand side is known as the prior odds and the second as the Bayes factor. Greenberg (2012) discuss some guidelines, namely *Jeffreys Guidelines*, for evaluating the result of the posterior odds ratio. A relatively high value of $R_{i,j} = \frac{P(\mathcal{M}_i|y)}{P(\mathcal{M}_j|y)}$ would mean decisive support for $\mathcal{M}_i$ in contrast to $\mathcal{M}_j$.

Jeliazkov et al. (2008) and Jeliazkov and Hee Lee (2010) present efficient methods to calculate the marginal likelihood for ordinal data models. In particular, I adopt the ARK method in this paper given it offers continuous, differentiable and simulation consistent estimates of the marginal likelihood while at the same time being relatively straightforward to implement. During the model comparison exercises, I will usually assume the same prior probability $Pr(\mathcal{M}_l)$ between competing models.

2.3 Analysis and results

This section first provides baseline estimation results for the "credit-crunch" period and a comparison with the non-crisis period. Next, I describe the model comparison exercise and discuss some limitations of the current approach.

2.3.1 Baseline results

The estimation results from three baseline specifications are presented on Tables 2.6, 2.7 and 2.8, which differ on the measures of capital being considered. Changes in the asset side are conditional, respectively, on the *Capital Ratio* (Table 2.6), on the *Risk-Adjusted Capital Ratio* (Table 2.7) and on both measures (Table 2.8). The specifications include controls for State and ignore bank mergers[4]. State controls are expected to capture, at least in part, variations in credit demand, given that the severity of the recession was different across states. The model includes the two main asset classes in banks' portfolio as dependent variables: *Total loans and leases* and *US Treasuries.* Some control variables are not listed in the tables for simplicity, whenever they did not show economically meaningful results.

The first two columns in any table present the point estimate and the credibility interval for the $\hat{\beta}$ coefficients in Equation 2.1. The last three columns show the marginal effect of increasing each listed covariate by one standard deviation in the probability of each outcome, keeping all other covariates fixed. For example, in the first model (Table 2.6), if the capital ratio is increased by 4.9 percentage points, which is the sample standard deviation for this variable, the probability of loan contraction is lowered by 1.7 percentage points (see third column, $Pr(y = 1)$). Marginal effects are calculated by integrating over the posterior and averaging for all observations in sample, as in Jeliazkov and Vossmeyer (2018). Because of the nonlinearity of the model, the effect of a change in a covariate depends on all other covariates and model parameters. The marginal effects presented in the tables are the average effects on the sample, already accounting for parameter estimation uncertainty.

The model allow us to analyze the effect of capital on the portfolio choice strategy of banks. Starting with Table 2.6, I find that banks with lower levels of *Capital Ratio* are more likely to contract both *Total Loans and Leases* and *US Treasuries* at the same time. In other words,

[4]Results for the case with no State controls, as well as adjustments for bank mergers are qualitatively similar to the baseline model.

			Marginal Effect of Covariate		
	Posterior Mean	Credibility Interval	Pr(y=1)	Pr(y=2)	Pr(y=3)
Total Loans and Leases					
Capital Ratio	1.939	[1.05,2.83]	-0.017	-0.003	0.019
	(0.445)				
Size (log of Assets)	-0.160	[-0.19,-0.13]	0.036	0.004	-0.040
	(0.017)				
Liquidity Ratio	0.745	[0.5,0.99]	-0.020	-0.003	0.023
	(0.124)				
Core Deposits Ratio	-0.399	[-0.8,0]	0.008	0.001	-0.009
	(0.198)				
ROE	1.204	[0.77,1.64]	-0.019	-0.003	0.021
	(0.219)				
Provision for loan losses	-6.212	[-9.15,-3.28]	0.010	0.001	-0.011
	(1.467)				
Charge off ratio C&I	-2.964	[-3.78,-2.15]	0.024	0.003	-0.027
	(0.407)				
US Treasuries					
Capital Ratio	3.465	[1.63,5.3]	-0.005	0.000	0.005
	(0.915)				
Size (log of Assets)	-0.010	[-0.1,0.08]	0.000	0.000	0.000
	(0.043)				
Liquidity Ratio	-3.130	[-3.79,-2.47]	0.014	0.000	-0.014
	(0.33)				
Core Deposits Ratio	1.417	[0.43,2.4]	-0.005	0.000	0.005
	(0.493)				
ROE	1.596	[0.51,2.68]	-0.004	0.000	0.004
	(0.541)				
Provision for loan losses	0.751	[-2.58,4.08]	0.000	0.000	0.000
	(1.666)				
Charge off ratio C&I	1.340	[-0.5,3.18]	-0.002	0.000	0.002
	(0.922)				
Num. of observations	10,971				
Log Marginal Likelihood	-21,207				

Notes: Response variables are the discrete class of growth rate in *Total loans and leases* in upper panel and *US Treasuries* in lower panel. Control variables are: *Size, Liquidity Ratio, Core deposits ratio, Brokered deposits ratio, Trading assets ratio, Loan concentration ratio, ROE, Provision for loan losses, Share C&I loans, Share Real Estate C&I loans, Share Real Estate Residential loans, Share Consumer loans, Charge off ratio C&I loans, dummies for every State.* Credibility intervals are shown for two standard deviations. Discrete classes are defined as: $Pr(y_{ij} = 1)$ is contraction; $Pr(y_{ij} = 2)$ is positive growth below median; $Pr(y_{ij} = 3)$ is positive growth above median. Marginal effects are calculated by integrating over the posterior and averaging for all observations in sample. Marginal likelihood is calculated by ARK method. Bank non-survivors from mergers are ignored from sample.

Table 2.6: Effect of *Capital Ratio* on asset growth for 1990-1992 (crisis period), posterior estimates in baseline specification with State controls.

			Marginal Effect of Covariate		
	Posterior Mean	Credibility Interval	Pr(y=1)	Pr(y=2)	Pr(y=3)
Total Loans and Leases					
Risk-Based Capital Ratio	1.038	[0.05,2.03]	-0.007	-0.001	0.008
	(0.494)				
Size (log of Assets)	-0.176	[-0.21,-0.14]	0.040	0.004	-0.044
	(0.016)				
Liquidity Ratio	0.786	[0.52,1.05]	-0.021	-0.003	0.024
	(0.131)				
Core Deposits Ratio	-0.652	[-1.02,-0.28]	0.013	0.002	-0.015
	(0.186)				
ROE	1.211	[0.78,1.64]	-0.019	-0.003	0.022
	(0.216)				
Provision for loan losses	-6.206	[-9.1,-3.31]	0.010	0.001	-0.011
	(1.449)				
Charge off ratio C&I	-3.026	[-3.84,-2.22]	0.025	0.003	-0.028
	(0.405)				
US Treasuries					
Risk-Based Capital Ratio	-0.411	[-2.57,1.75]	0.001	0.000	-0.001
	(1.079)				
Size (log of Assets)	-0.056	[-0.14,0.03]	0.002	0.000	-0.002
	(0.043)				
Liquidity Ratio	-2.758	[-3.46,-2.06]	0.012	0.000	-0.012
	(0.351)				
Core Deposits Ratio	0.791	[-0.14,1.72]	-0.003	0.000	0.003
	(0.466)				
ROE	1.556	[0.48,2.63]	-0.004	0.000	0.004
	(0.538)				
Provision for loan losses	0.651	[-2.69,3.99]	0.000	0.000	0.000
	(1.672)				
Charge off ratio C&I	1.142	[-0.74,3.03]	-0.002	0.000	0.002
	(0.942)				
Num. of observations	10,971				
Log Marginal Likelihood	-21,225				

Notes: Same as Table 2.6.

Table 2.7: Effect of *Risk-Based Capital Ratio* on asset growth for 1990-1992 (crisis period), posterior estimates in baseline specification with State controls.

	Posterior Mean	Credibility Interval	Marginal Effect of Covariate Pr(y=1)	Pr(y=2)	Pr(y=3)
Total Loans and Leases					
Capital Ratio	2.398 (0.595)	[1.21,3.59]	-0.021	-0.003	0.024
Risk-Based Capital Ratio	-0.769 (0.657)	[-2.08,0.55]	0.006	0.001	-0.006
Size (log of Assets)	-0.161 (0.017)	[-0.19,-0.13]	0.036	0.004	-0.040
Liquidity Ratio	0.808 (0.132)	[0.54,1.07]	-0.021	-0.003	0.025
Core Deposits Ratio	-0.403 (0.192)	[-0.79,-0.02]	0.008	0.001	-0.009
ROE	1.185 (0.218)	[0.75,1.62]	-0.018	-0.003	0.021
Provision for loan losses	-6.250 (1.449)	[-9.15,-3.35]	0.010	0.001	-0.011
Charge off ratio C&I	-2.998 (0.404)	[-3.81,-2.19]	0.025	0.003	-0.027
US Treasuries					
Capital Ratio	4.528 (1.019)	[2.49,6.57]	-0.007	0.000	0.007
Risk-Based Capital Ratio	-2.839 (1.181)	[-5.2,-0.48]	0.003	0.000	-0.003
Size (log of Assets)	-0.018 (0.043)	[-0.1,0.07]	0.001	0.000	-0.001
Liquidity Ratio	-2.875 (0.35)	[-3.58,-2.17]	0.013	0.000	-0.013
Core Deposits Ratio	1.360 (0.487)	[0.39,2.33]	-0.004	0.000	0.004
ROE	1.575 (0.531)	[0.51,2.64]	-0.004	0.000	0.004
Provision for loan losses	0.710 (1.675)	[-2.64,4.06]	0.000	0.000	0.000
Charge off ratio C&I	1.242 (0.951)	[-0.66,3.14]	-0.002	0.000	0.002
Num. of observations	10,971				
Log Marginal Likelihood	-21,206				

Notes: Same as Table 2.6.

Table 2.8: Effect of *Capital Ratio* and *Risk-Based Capital Ratio* on asset growth for 1990-1992 (crisis period), posterior estimates in baseline specification with State controls.

more leveraged banks are more likely to choose a deleveraging strategy, and to shrink their balance sheet both on the risky and on the riskless asset. This finding can be inferred from the positive coefficients on the *Capital Ratio*, and on the negative sign of the marginal effect of capital in the probability of contraction, $Pr(y = 1)$, for both asset classes. Note that the marginal effect is stronger for the adjustment of loans. More importantly, this finding holds and it is even stronger in the full model presented in Table 2.8, which controls for the level of *Risk-Based Capital Ratio*.

Two possible interpretations emerge for the evidence that more leveraged banks were more likely to deleverage during the "credit-crunch". In Furfine (2001), in order for the optimal response to be a reduction in the portfolio for both loans and securities, banks should be either experiencing a decline in loan demand resulting from an economic downturn, or recovering from negative shocks to capital. For the first case, which I call demand shock hypothesis, to be consistent with my findings, less capitalized banks should have been hit harder by the fall in loan demand. This may be the true if these banks were engaged in more intense relationships with customers who also suffered more during the recession. For the time being, I cannot rule out this hypothesis, given that my only control for local demand is at the state level[5].

In the second case, negative shocks to capital cause banks to be below their targeted capital ratio, and possibly even below the regulatory leverage ratio (LR). I call this case the binding LR hypothesis. The banks respond by contracting both sides of the portfolio - risky and riskless asset - at the same rate, which is observationally equivalent to the reaction to the demand shock hypothesis. A plausible explanation to the binding LR hypothesis is that banks which held worst quality portfolios during the late 1980s suffered more capital losses, and thus became more leveraged and further from their targeted capital ratio. It may be the case that the regulatory leverage ratio (LR) was a binding constraint for a significant

[5]More comments about this caveat and possible approaches are discussed in the last section of the paper.

number of banks. So, naturally, we observe institutions with lower levels of capital being more likely to deleverage in order to remain compliant with regulation[6].

Turning to the analysis of the effect of *Risk-Based Capital Ratio* on portfolio allocation, in Table 2.7, I find that banks with a lower level of risk-based capital were more likely to choose an asset substitution strategy, in which they contract the level of *Total Loans and Leases* and expand the amount of *US Treasuries*. This can be read from the positive estimated coefficient of *Risk-Based Capital Ratio* for the risky asset choice, and the negative point estimate coefficient for the safe asset. However, evidence for this behavior is not conclusive, given relatively small marginal effects in absolute value, and high uncertainty around the estimated coefficient for *Risk-Based Capital Ratio* in the response of *US Treasuries*.

Again, there are two observationally equivalent explanations for the asset substitution strategy consistent with optimal banks' portfolio choice Furfine (2001). The first one is that the introduction of risk-based capital (RBC) requirements, a regulatory shock caused by the Basel Accord, caused the reallocation of bank portfolios from loans to securities. I call this case the binding RBC requirements hypothesis. In my findings, the substitution strategy is more likely to be observed for banks with lower RBC, which naturally were closer to the binding constraint. Note that the RBC requirements could be binding while the bank is still compliant with minimum LR if the bank is carrying a portfolio that is risky enough, with a high ratio of loans to securities. Besides, it is not necessarily related with previous capital losses and the quality of loans. So, the binding RBC requirements hypothesis, in principle, can be independent of the binding LR and demand shock hypothesis. The second explanation for the asset substitution strategy is that an increase in regulatory scrutiny could have led to a porfolio reallocation of this kind. To be consistent with my findings, the increase in regulatory scrutiny should have affected more intensively banks holding riskier assets, and thus with lower risk based capital ratios, which is a plausible assumption from the point of

[6]This hypothesis can be further investigated by reestimating the model in subsamples of banks, which are below or above the regulatory LR, or by controlling for non-compliance.

view of the supervisory agency.

Even though I cannot rule out the RBC requirements nor the regulatory scrutiny hypothesis based solely on the findings from Table 2.7, the findings for the full model in Table 2.8 are revealing. When controlling for the level of *Capital Ratio*, I no longer find that banks with lower RBC are more likely to engage in asset substitution. In contrary, they are (slightly) more likely to choose the expansion of both asset classes, which is demonstrated by the negative coefficients of *Risk-Based Capital Ratio* and negative marginal effects of this covariate in the probability of expansion, $Pr(y = 3)$. The expected effect is relatively small on both cases - loans and treasuries. At the same time, this result must be taken with some care, as the uncertainty interval is wide around the estimate in the *Total Loans and Leases* equation and does not rule out a positive coefficient. In any case, asset substitution is not correlated with banks having lower RBC levels, which contradicts the hypothesis of binding RBC requirements and increased regulatory scrutiny, at least in the way they were previously described. The evidence favors either the binding LR or the demand shock hypothesis.

Apart from the correlation between capital ratios and asset choice, some additional results are worth noticing. I find that liquidity, profitability and the quality of the loan portfolio are all positively correlated with the likelihood of growth in *Total Loans and Leases*. This finding holds in all three specifications of the model. More liquid or profitable banks are more likely to increase the portfolio of loans. At the same time, banks with problem loans, measured as a higher *Charge off ratio C&I*, are more prone to experience contractions. For example, the marginal effect of this variable is to increase the probability of contraction by 2.4 to 2.5 percentage points, depending on the specification. This is the higher marginal effect of a covariate, in absolute value, with the exception of size. *Provision for loan losses* tries to measure how much the banks expect to lose on loans, and thus serves as an indirect proxy for signaling portfolio quality. I find that banks holding more provisions are more likely to contract loans.

In turn, *Size* has the larger absolute marginal effect on loan contraction, though this finding must be taken with careful consideration as the standard deviation of this variable is very high ($2.49 Billion in total assets)[7]. Still, larger banks are more likely to contract loans in the sample. Covariates related to the liability side of banks, core deposits and brokered deposits, showed only small correlation with the probability of loan contraction. This was also the case for trading assets, which were quite rarely reported.

Regarding the allocation of *US Treasuries*, I find a mean reversion effect of liquidity: more liquid banks *ex-ante* are more likely to decrease their holdings of liquid assets. Besides, there is a small effect of profitability (*ROE*) in the likelihood of increasing holdings of securities. Profitability is apparently enabling banks to growth their total assets and gain market share. Again, this findings hold for all model specifications.

2.3.2 Non-crisis period

For the sake of comparison I estimate the model for a non-crisis period, from Mar/1995 until Dec/1996, using Dec/1994 as the baseline date to measure the capital ratios and other covariates. The results are presented on Tables 2.9, 2.10 and 2.11.

Interestingly, I find that the expected effect of capital on the portfolio allocation decision of banks is different than before. First, banks holding lower *Capital Ratio*, thus more leveraged, are more likely to expand their *Total Loans and Leases* by more than the median change, and slightly more likely to decrease their holdings of *US Treasuries*. This behavior clearly identifies a leverage cycle, where highly leveraged banks are likely to continue on this strategy[8]. The conclusion can be inferred from the negative marginal effect of *Capital Ratio* on the probability of expansion in loans ($Pr(y = 3)$), and the negative marginal effect of the

[7] Recall that the marginal effect is always calculated as a change of one standard deviation in the covariate of interest.

[8] For simplicity, in this analysis I am assuming that equity issuance is independent of initial capital ratios.

	Posterior Mean	Credibility Interval	Marginal Effect of Covariate Pr(y=1)	Pr(y=2)	Pr(y=3)
Total Loans and Leases					
Capital Ratio	-1.393	[-1.96,-0.83]	0.011	0.023	-0.035
	(0.283)				
Size (log of Assets)	-0.068	[-0.09,-0.05]	0.018	0.034	-0.052
	(0.01)				
Liquidity Ratio	0.335	[0.18,0.49]	-0.009	-0.022	0.031
	(0.077)				
Core Deposits Ratio	-0.394	[-0.64,-0.15]	0.008	0.017	-0.025
	(0.123)				
Loan Concentration Ratio	-0.254	[-0.45,-0.06]	0.006	0.013	-0.019
	(0.095)				
ROE	0.522	[0.11,0.94]	-0.005	-0.012	0.017
	(0.207)				
Provision for loan losses	-1.918	[-4.95,1.11]	0.001	0.003	-0.005
	(1.514)				
Charge off ratio C&I	-2.818	[-3.71,-1.92]	0.012	0.025	-0.037
	(0.448)				
US Treasuries					
Capital Ratio	3.095	[0.46,5.73]	-0.002	0.000	0.002
	(1.318)				
Size (log of Assets)	0.022	[-0.11,0.15]	0.000	0.000	0.000
	(0.065)				
Liquidity Ratio	-9.026	[-10.22,-7.83]	0.018	0.000	-0.018
	(0.599)				
Core Deposits Ratio	0.231	[-1.16,1.62]	0.000	0.000	0.000
	(0.693)				
Loan Concentration Ratio	-0.125	[-1.38,1.13]	0.000	0.000	0.000
	(0.625)				
ROE	3.532	[1.23,5.83]	-0.002	0.000	0.002
	(1.151)				
Provision for loan losses	0.278	[-3.13,3.69]	0.000	0.000	0.000
	(1.706)				
Charge off ratio C&I	-0.504	[-3.64,2.64]	0.000	0.000	0.000
	(1.571)				
Num. of observations	9,163				
Log Marginal Likelihood	-16,510				

Notes: Same as Table 2.6.

Table 2.9: Effect of *Capital Ratio* on asset growth for 1995-1996 (non-crisis period), posterior estimates in specification with State controls.

	Posterior Mean	Credibility Interval	Marginal Effect of Covariate Pr(y=1)	Pr(y=2)	Pr(y=3)
Total Loans and Leases					
Risk-Based Capital Ratio	-1.198	[-1.64,-0.76]	0.017	0.034	-0.051
	(0.22)				
Size (log of Assets)	-0.064	[-0.08,-0.04]	0.016	0.032	-0.048
	(0.01)				
Liquidity Ratio	0.641	[0.43,0.85]	-0.016	-0.042	0.059
	(0.106)				
Core Deposits Ratio	-0.363	[-0.6,-0.13]	0.008	0.016	-0.023
	(0.117)				
Loan concentration Ratio	-0.273	[-0.46,-0.08]	0.006	0.014	-0.020
	(0.095)				
ROE	0.467	[0.05,0.88]	-0.005	-0.011	0.015
	(0.208)				
Provision for loan losses	-2.081	[-5.11,0.95]	0.001	0.003	-0.005
	(1.517)				
Charge off ratio C&I	-2.823	[-3.71,-1.93]	0.012	0.025	-0.038
	(0.446)				
US Treasuries					
Risk-Based Capital Ratio	0.454	[-1.91,2.82]	0.000	0.000	0.000
	(1.181)				
Size (log of Assets)	0.009	[-0.12,0.14]	0.000	0.000	0.000
	(0.066)				
Liquidity Ratio	-8.992	[-10.36,-7.62]	0.017	0.000	-0.017
	(0.684)				
Core Deposits Ratio	-0.014	[-1.43,1.41]	0.000	0.000	0.000
	(0.71)				
Loan concentration Ratio	0.002	[-1.28,1.29]	0.000	0.000	0.000
	(0.643)				
ROE	3.372	[1.1,5.65]	-0.002	0.000	0.002
	(1.138)				
Provision for loan losses	0.330	[-3.08,3.74]	0.000	0.000	0.000
	(1.706)				
Charge off ratio C&I	-0.538	[-3.71,2.63]	0.000	0.000	0.000
	(1.584)				
Num. of observations	9,163				
Log Marginal Likelihood	-16,509				

Notes: Same as Table 2.6.

Table 2.10: Effect of *Risk-Based Capital Ratio* on asset growth for 1995-1996 (non-crisis period), posterior estimates in specification with State controls.

	Posterior Mean	Credibility Interval	Marginal Effect of Covariate Pr(y=1)	Pr(y=2)	Pr(y=3)
Total Loans and Leases					
Capital Ratio	-0.519	[-1.34,0.3]	0.004	0.009	-0.013
	(0.41)				
Risk-Based Capital Ratio	-0.922	[-1.56,-0.29]	0.013	0.026	-0.039
	(0.318)				
Size (log of Assets)	-0.066	[-0.09,-0.05]	0.017	0.033	-0.050
	(0.01)				
Liquidity Ratio	0.584	[0.35,0.82]	-0.015	-0.039	0.054
	(0.118)				
Core Deposits Ratio	-0.405	[-0.65,-0.16]	0.008	0.017	-0.026
	(0.123)				
Loan concentration Ratio	-0.266	[-0.46,-0.07]	0.006	0.014	-0.020
	(0.096)				
ROE	0.459	[0.04,0.88]	-0.005	-0.011	0.015
	(0.21)				
Provision for loan losses	-2.056	[-5.08,0.97]	0.001	0.003	-0.005
	(1.512)				
Charge off ratio C&I	-2.835	[-3.74,-1.94]	0.013	0.025	-0.038
	(0.45)				
US Treasuries					
Capital Ratio	3.363	[0.48,6.24]	-0.002	0.000	0.002
	(1.441)				
Risk-Based Capital Ratio	-0.628	[-3.06,1.81]	0.000	0.000	0.000
	(1.218)				
Size (log of Assets)	0.020	[-0.11,0.15]	0.000	0.000	0.000
	(0.066)				
Liquidity Ratio	-8.911	[-10.28,-7.55]	0.017	0.000	-0.017
	(0.683)				
Core Deposits Ratio	0.190	[-1.2,1.58]	0.000	0.000	0.000
	(0.693)				
Loan concentration Ratio	-0.116	[-1.4,1.17]	0.000	0.000	0.000
	(0.641)				
ROE	3.452	[1.21,5.69]	-0.002	0.000	0.002
	(1.121)				
Provision for loan losses	0.285	[-3.14,3.71]	0.000	0.000	0.000
	(1.714)				
Charge off ratio C&I	-0.493	[-3.56,2.57]	0.000	0.000	0.000
	(1.531)				
Num. of observations	9,163				
Log Marginal Likelihood	-16,507.749				

Notes: Same as Table 2.6.

Table 2.11: Effect of *Capital Ratio* and *Risk-Based Capital Ratio* on asset growth for 1995-1996 (non-crisis period), posterior estimates in specification with State controls.

same variable on the contraction of securities in Table 2.9. The effect holds when controlling for the level of *Risk-Based Capital Ratio*, in Table 2.11. It is lower in magnitude and more uncertain for the case of loans growth, but robust for the case of treasuries contraction.

Second, regarding initial *Risk-Based Capital Ratio*, the findings again point to a leverage cycle. Banks with lower risk-based capital are significantly more likely to increase their loans by more than the median. This can be read in Table 2.10 from the negative marginal effect of *Risk-Based Capital Ratio* in the probability of higher growth in loans. The results hold for the full model in Table 2.11, although they are lower in magnitude. We cannot make any conclusions about how the adjustment on the side of securities is correlated with risk-based capital, given the high uncertainty around credibility intervals. In any case, the findings obtained for the non-crisis period are the opposite when compared to the "credit-crunch", and they indicate that neither LR requirements nor RBC regulatory limits were binding for most financial institutions.

Other bank characteristics continue to show similar correlations with loan and securities growth as before. More liquid and profitable banks are more likely to increase loans by more than the median, and the marginal effect of liquidity is higher than during the previous period. Profitability appears to be slightly less important to changes in loans. Banks with a worst quality portfolio of loans, which are holding higher *Charge off ratio C&I*, are less likely to experience large expansions in loans. *Size* still have negative expected effect on the likelihood of loan growth, as big banks were probably growing less in percentage terms. The mean reversion effect of liquidity in the likelihood of expansion of securities is again observed, with a similar magnitude.

2.3.3 Model comparison

The results for the Bayesian model comparison exercise are presented in Table 2.12. I am comparing the three model specifications presented in the previous section, which differ in their measures of capital. Models $\mathcal{M}_1$, $\mathcal{M}_2$ and $\mathcal{M}_3$ consider respectively *Capital Ratio*, *Risk-Based Capital Ratio*, and both measures as explanatory variables. All models are estimated for two different time periods, the "credit-crunch" (columns 3 and 4) and the post-crisis sample (columns 5 and 6). For models 1 and 2, I run two estimations, with and without State controls.

The first part of the table (top) shows the log marginal likelihood of each model specification, calculated by the ARK method (Jeliazkov and Hee Lee (2010)). I assume that competing models have the same prior probability, such that the prior odds ratio is equal to one. In this case, the burden to discriminate between models falls on the ratio of marginal likelihoods, called Bayes factor, as stressed by Greenberg (2012). This is shown in the second half of the table, the log of the Bayes factor, which is calculated as $log_{10}(\frac{m(y|\mathcal{M}_i)}{m(y|\mathcal{M}_j)}))$, where $m(y|\mathcal{M}_i)$ is the marginal likelihood of model i. According to *Jeffreys Guidelines*, a value of the log of Bayes factor greater than one can be interpreted as decisive support for model $\mathcal{M}_i$ when compared with model $\mathcal{M}_j$ (Greenberg (2012)), while values smaller than one-half offer only weak evidence for $\mathcal{M}_i$.

The first clear conclusion is that data prefers specifications with State controls. The estimated marginal likelihood of any specifications with State controls is always significantly higher when compared to the same $\mathcal{M}_i$ model without these controls. When comparing the same model with and without State controls, the log of the Bayes factor calculated is always greater than 50 for the crisis period and greater than 3 for the non-crisis, showing strong support for specifications which include the controls[9].

[9] For simplicity, the values for the log of Bayes factor when comparing models with and without State controls are not shown in the Table.

		Credit-Crunch (1990-1992)		Non-crisis (1995-1996)	
Log marginal likelihood		State	No State	State	No State
Model	Measure of Capital				
1	*Capital Ratio*	-21,207.69	-21,343.14	-16,510.04	-16,517.34
2	*Risk-Based Capital Ratio*	-21,225.05	-21,364.11	-16,509.40	-16,517.80
3	*Capital Ratio* and *RBCR*	-21,205.61	-21,336.47	-16,507.74	-16,515.21
Model comparison: $log_{10}(Bayesfactor)$					
Model 1 over Model 2		7.538	9.106	-0.279	-0.236
Model 3 over Model 1		0.904	2.899	0.997	0.926
Model 3 over Model 2		8.442	12.005	0.718	0.689

Table 2.12: Bayesian model comparison.

The most interesting comparison, though, is between competing models $\mathcal{M}_1$, $\mathcal{M}_2$ and $\mathcal{M}_3$. During the "credit-crunch", and considering the preferred specifications with State controls, data shows decisive support for model $\mathcal{M}_1$, when compared to $\mathcal{M}_2$, given that the log of Bayes factor is 7.538. At the same time, I find some evidence for $\mathcal{M}_3$, which includes both measures of capital, against $\mathcal{M}_1$. In this case, though, the log of Bayes factor is less than one (0.904) meaning I cannot rule out the competing model. In other words, the evidence points out that during the crisis a model where portfolio allocation is conditional on the *Capital Ratio* is definitely a better description of the observed data than a competing model where the same choice is conditional on *Risk-Based Capital Ratio*. There is also a reasonable probability that a model which includes both measures of capital is an even better description. On the other hand, outside of the crisis period evidence is not too clear in favor of any competing model. While I find some evidence in favor of the full model, which includes both *Capital Ratio* and *Risk-Based Capital Ratio*, there is only weak evidence for the *Risk-Based Capital Ratio* model when compared against the *Capital Ratio* specification.

If taken seriously, the results of model comparison support two possible conclusions. First, explanations for the "credit-crunch" based on the role of *Capital Ratio* as a factor conditioning portfolio choice appear more relevant than the explanations based on the *Risk-Based*

Capital Ratio. As discussed before, these include the hypothesis of binding LR requirements and of a negative economic shock affecting less capitalized banks more intensively. The competing explanations that are related to *Risk-Based Capital Ratio* and the asset substitution strategy, namely the hypothesis of binding RBC requirements or increased regulatory monitoring towards riskier institutions, seem weaker in comparison. The model comparison exercise offers additional support to the findings already discussed in the previous section. Second, for the non-crisis period 1995-1996, evidence suggests that both capital measures are equally important to condition bank choices with a probably greater role for the *Risk-Based Capital Ratio*. This may indicate that, after a few years of implementation, RBC requirements became internalized in the strategic decision making framework of financial institutions, and thus became more relevant in portfolio choice than during the initial adoption period, which began in 1990. Or it may simply indicate that banks were back to a normal expansionary credit cycle, which is inherently different than a crisis period[10].

2.3.4 Nonlinear effect of capital ratio

In this section, I assess the nonlinear effect of the capital ratios on the probability of contraction in loans. The analysis is carried out for the preferred specification, that is Model $\mathcal{M}_3$ with State controls.

Figure 2.4 shows, for each bank in the sample, the marginal effects (ME) of the covariates *Capital Ratio* (left plot) and *Risk-Based Capital Ratio* (right plot) in the probability of contraction in *Total Loans and Leases*. The two graphs in the first row are for the "credit-crunch" period and the second row refers tothe non-crisis years. The marginal effects (y-axis) are calculated for each observation by integrating over the posterior distribution. The respective covariates are represented in the horizontal axis[11].

[10] An additional research question that arises is whether both measures of capital were already relevant in previous expansionary credit cycles, even before the introduction of the 1988 Basel Accord.

[11] A few banks in the sample have a *Risk-based Capital Ratio* greater than one. They are not shown in

During the early 1990s crisis, there is attenuation of the marginal effects as the capital ratios increase, as expected. This is true for both measures. For instance, the marginal effect of increasing the *Capital Ratio* is to decrease the probability of contraction in loans. This effect is expected to be higher (in absolute values) for banks with low levels of capital. Conversely, for the risk-based measure of capital, the ME is positive but decreasing in magnitude. The average marginal effect (ME) by percentile of the covariate is calculated and showed in Table 2.13. For example, for banks in the 0-10th percentile holding capital ratios between -0.03 and 0.06, the ME of this covariate is about -0.0231 percentage points in the probability of contraction in loans. Comparing with the 90-100th percentile, the effect drops, in magnitude, to -0.0182. The difference between the two groups is only 0.0049 percentage points. Overall, the results confirm a nonlinear effect of both measurs of capital, but find it is not economically significant during the crisis. On the other hand, for the non-crisis period, the lower plots in Figure 2.4 does not suggest any non-linear effect. The distribution of ME seems independent of the level of capital.

Percentile group	*Capital Ratio* lower limit	Marginal effect of *Capital Ratio*	*RBCB* lower limit	Marginal effect of *RBCB*
0-10	-0.0303	-0.0231	-0.0310	0.0061
10-20	0.0604	-0.0219	0.0633	0.0059
20-30	0.0675	-0.0214	0.0714	0.0058
30-40	0.0728	-0.0209	0.0779	0.0056
40-50	0.0778	-0.0205	0.0838	0.0056
50-60	0.0828	-0.0204	0.0904	0.0055
60-70	0.0888	-0.0203	0.0984	0.0055
70-80	0.0964	-0.0201	0.1088	0.0054
80-90	0.1079	-0.0196	0.1243	0.0053
90-100	0.1291	-0.0182	0.1565	0.0050

Notes: Period is "credit-crunch" from Mar/1990 to Dec/1992, model is $\mathcal{M}_3$ with State controls.

Table 2.13: Average marginal effect of *Capital Ratio* and *Risk-Based Capital Ratio (RBCR)* on the probability of contraction in *Total loans and leases*, by percentile group of each covariate.

the figure (right plot) for the sake of clarity and comparison between both graphs. The left plot includes all banks in the sample.

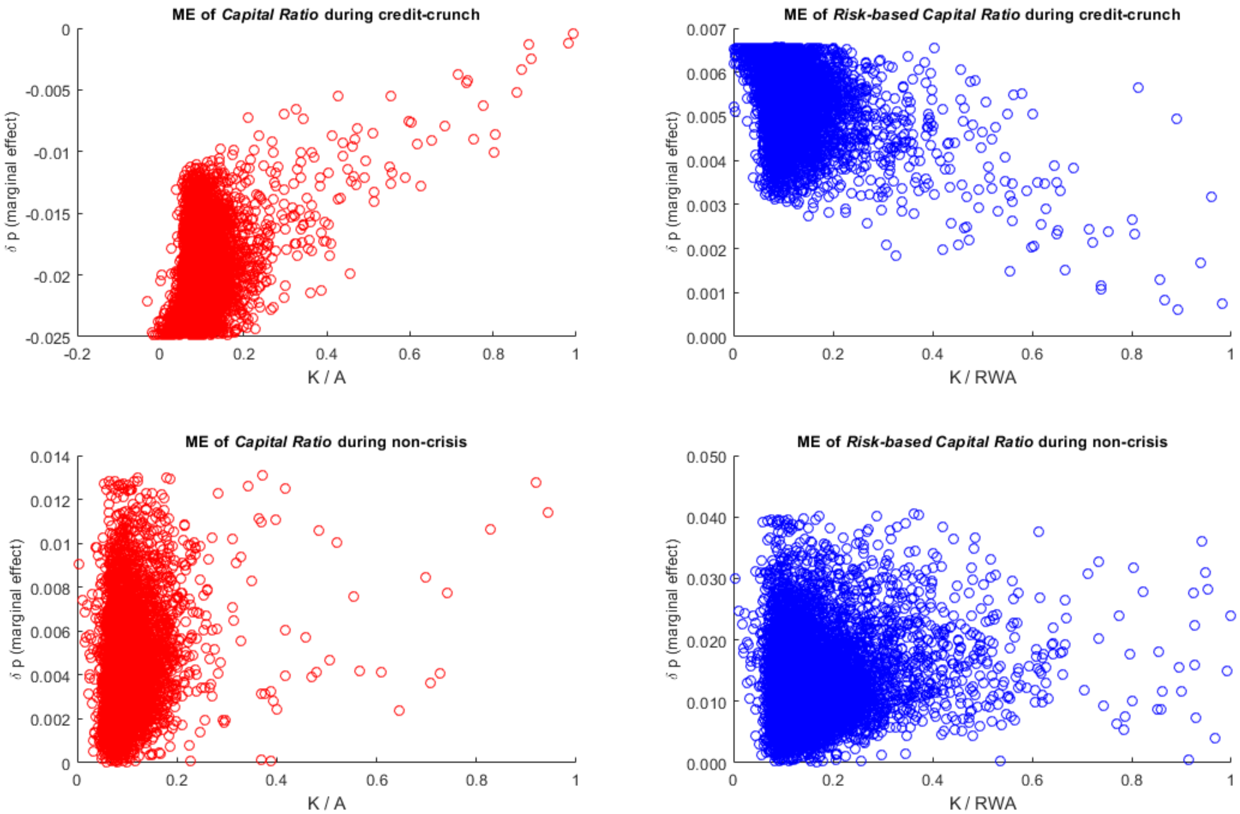


Figure 2.4: Marginal effect of *Capital Ratio* (left plot) and *Risk-based Capital Ratio* (right plot) on the probability of contraction in *Total Loans and Leases*.

Note: Periods are in rows: first row is "credit-crunch" from Mar/1990 to Dec/1992, second row is non-crisis from Mar/1995 to Dec/1996. Each circle represent one bank in the sample. Model is $\mathcal{M}_3$ with State controls. Marginal effect is calculated for each bank by integrating over the posterior distribution.

2.3.5 Caveats

One limitation of my research is that, at the moment, my only control for local demand conditions is at the State level. Differences in local demand for credit within a State will interfere with loan growth and may be correlated with the initial capital level. The literature has approached this issue in several ways.

Papers focusing only on small banks typically use the geographic location of the branch, at county or metropolitan area, in order to obtain data correlated to local demand, given the widely recognized fact that lending activity of smaller institutions is mostly driven by local borrowers. For example, Kiser et al. (2016) control for economic conditions, adding four variables measured at the local market level to the regressions: annual percentage changes in unemployment rate, income, population, and the number of new small firms established. The local market is defined as the metropolitan statistical areas (MSAs) or non-MSA counties where each bank (or branch) has its deposits, and additional data is obtained from the FDIC's annual Summary of Deposits. An alternative approach, still based on the assumption that banks in the same location face the same economic environment, is adopted by Carlson et al. (2013). They compare each bank to a matched set of neighbors and test whether differences in the covariate of interest, for example the capital ratio, is correlated with differences in the response variable.

When dealing with datasets that include larger banks, the issue of separating the effects of loan demand become more cumbersome. Berger and Udell (1994) use a panel setup covering a large time period, from 1979 until 1992, and control for macro and regional economic variables measured at national or state level, which show enough variation across time and space. They include controls for real GNP growth, national unemployment rate, state income growth, state unemployment rate, interest rate and term structure. Shrieves and Dahl (1995), in turn, propose a simultaneous equation approach which controls for cyclical economic fluctuations related to demand for credit. Their sample includes only banks with more than $100 million in assets and cover two periods, both prior and during the "credit-crunch", pooling the data from 1985 to 1991.

2.4 Conclusion

In this paper, I propose a discrete choice model to study how bank portfolio allocation decisions are correlated with capital ratios and other banks' characteristics. I use the model to empirically assess the response of US banks during the 1990s "credit-crunch", and I compare the findings with a subsequent non-crisis period 1995-1996.

My main results are as follows. During the crunch period, less capitalized banks were more likely to choose a deleveraging strategy, shrinking both loans and securities at the same time. Moreover, there is no evidence that lower risk-based capital ratio is correlated with the likelihood of choosing an asset substitution strategy, in which loans contract while securities increase. According to a model of optimal portfolio adjustment, this result suggests that the adoption of risk-based capital requirements contained in the Basel Accord was not the most important driver of the "credit crunch", but banks were either recovering from negative shocks to capital, binded by the leverage ratio requirements, or reacting to the economic environment. Regarding the non-crisis period, the findings are compatible to an overall leverage cycle of the banking system, where capital is not a constraint to most banks. Besides, the risk-based capital ratio appears to have become more important to the asset allocation decision of bankers in this last period.

Some limitations of the current approach are left to future work. They include controlling for local credit demand and adjusting the estimation method to account for survivorship bias. Additionally, the analysis can be expanded to assess how large and small banks may be differently constrained by capital, how banks near the regulatory minimum capital levels were responding during the crunch, and whether the leverage cycle identified between 1995-1996 is similar to previous ones, where the risk-based requirements were absent.

Chapter 3

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

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. In this chapter, 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.

3.1 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 and 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))[1]. 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 et al. (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 et al. (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,

[1]The complementary between the two types of regulatory capital - leverage ratio and risk-based requirements - is further discussed in Blum (2008).

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. 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 New York (2020)). From the macroeconomic perspective, household leverage is considered a determinant factor for business cycle fluctuations (Jordà et al. (2016); Mian et al. (2017b)). 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

[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.

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 et al. (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 et al. (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 and Mistrulli (2004); Behn et al. (2016); de Ramon et al. (2016); Jiménez et al. (2017); Plosser and Santos (2018); Gropp et al. (2019); Juelsrud and 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 et al. (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 3.2 details the regulatory framework of the Supplementary Leverage Ratio and the data sources used in the study. The empirical strategy 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 3.3. Section 3.4 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 3.5 concludes by discussing policy implications and contributions of the paper

[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 3.5.

to the current debate on financial regulation.

3.2 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 Committee, 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

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

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 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

[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).

[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.

the traditional LR is not.

Table 3.1 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.

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

[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)).

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 3.3 (see also Table 3.2). 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.

3.3 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 et al. (2018); Pierret and 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.

3.3.1 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.

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

The list of all bank holding companies in the sample is shown in Table 3.2 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 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.

[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 and Steri (2019)). All depository institutions are still subject to the standard minimum leverage ratio, defined as tier 1 capital over total assests.

Comparability of banks and aggregate adjustment

Average capitalization and other bank characteristics for the treatment and control groups are provided in Table 3.3 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 3.1, 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, 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.

[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.

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 3.4, from 2008 to 2017, and also in Figure 3.3a. 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 3.3b and 3.3c 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 3.5. 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 3.2, 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 3.5.

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.

3.3.2 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

$$Y_i^N = h(U_i, T_i) \tag{3.1}$$

with $h(u,t)$ an increasing function in u. The random variable U_i represents the unobservable characteristics of individual i. Equation (3.1) 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

$$U_i = \alpha + \gamma \cdot G_i + \epsilon_i \quad \text{with} \quad \epsilon \perp (G_i, T_i) \qquad \text{(additivity)}$$

$$h(u,t) = \phi(u + \delta \cdot t) \qquad \text{(single index model)}$$

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

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

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

$$Y_i^I = h^I(U_i, T_i) \tag{3.2}$$

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 $t \in \{0,1\}$.

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

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

$$F_{Y^N,11}(y) = F_{Y,10}(F_{Y,00}^{-1}(F_{Y,01}(y))) \tag{3.3}$$

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 (3.3), the average treatment effect on the treated can be written as

$$\begin{aligned} \tau^{CIC} &= E[Y_{11}^I - Y_{11}^N] = E[Y_{11}^I] - E[Y_{11}^N] \\ &= E[Y_{11}^I] - E[F_{Y,01}^{-1}(F_{Y,00}(Y_{10}))] \end{aligned} \tag{3.4}$$

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.

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

$$\hat{F}_{Y,gt}(y) = \frac{1}{N_{gt}} \sum_{i=1}^{N_{gt}} \mathbb{I}\{Y_{gt,i} \leq y\} \tag{3.5}$$

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

$$\hat{F}^{-1}_{Y,gt}(q) = \inf\{y \in \mathbb{Y}_{gt} : \hat{F}_{Y,gt}(y) \geq q\} \tag{3.6}$$

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

$$\tau^{\hat{CIC}} = \frac{1}{N_{11}} \sum_{i=1}^{N_{11}} Y_{11,i} - \frac{1}{N_{10}} \sum_{i=1}^{N_{10}} \hat{F}^{-1}_{Y,01}(\hat{F}_{Y,00}(Y_{10,i})) \tag{3.7}$$

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

$$Y_i = D_i'\delta + X_i'\beta + \varepsilon_i \tag{3.8}$$

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

$$\hat{Y}_i = Y_i - X_i'\hat{\beta} = D_i'\hat{\delta} + \hat{\varepsilon}_i \tag{3.9}$$

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.

3.3.3 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.

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

$$y_{i,g,t} = h(u,t) + x'_{i,g,t}\beta \qquad \text{, and} \tag{3.10}$$

$$y^I_{i,g,t} = h^I(u,t) + x'_{i,g,t}\beta \tag{3.11}$$

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 (3.10) and (3.11) 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.

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 (3.10), 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.

[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.

3.3.4 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 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.

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 3.6, 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.

[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.

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 3.5, 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 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

[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.

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 3.9, 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

[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.

treatment starting in 2014 against the alternative of January/2012.

Table 3.8 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 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 3.10 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 3.6). Contrarily, in the model with MSA fixed effects,

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

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 3.4a and 3.4b 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 3.4a) 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 3.4b), 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.

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 3.2.1), 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 3.11. 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 3.5. 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 3.15 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 3.13. 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 3.5 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.

[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.

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 3.16, 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 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 3.17 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.

Placebo and robustness tests

Supplementary analysis of some forms can improve the credibility of results obtained in policy evaluation studies (Athey and 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 3.18. 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 3.19. 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.

3.4 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

[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.

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.

3.4.1 Empirical strategy

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

$$\begin{aligned}\Delta y_{c,t} =& \alpha_c + \alpha_t + \gamma_1 \sum_{j=1}^{J} \Delta y_{c,t-j} + \gamma_2 \sum_{j=1}^{J} X_{c,t-j} \\ &+ \beta_1 Credit_{c,t-1} + \beta_2 Credit_{c,t-1}^{SLRbanks} + \beta_3 (Credit_{c,t-1}^{SLRbanks} * Post) + \varepsilon_{c,t}\end{aligned} \tag{3.12}$$

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, $Credit_{c,t}^{SLR}$ 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 and Bond (1991)).

3.4.2 Results: from loan level adjustment to house prices

I estimate four versions of the model in Equation (3.12), and the results are shown in Table 3.20. 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

[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.

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.

3.4.3 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 (3.21), 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 $Credit_{c,t}^{SLR}$ and the time trend is estimated very close to zero and it is statistically insignificant.

3.5 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

[27]The models in Table (3.21) are equivalent to columns (2) and (3) on the last subsection.

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 and 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.

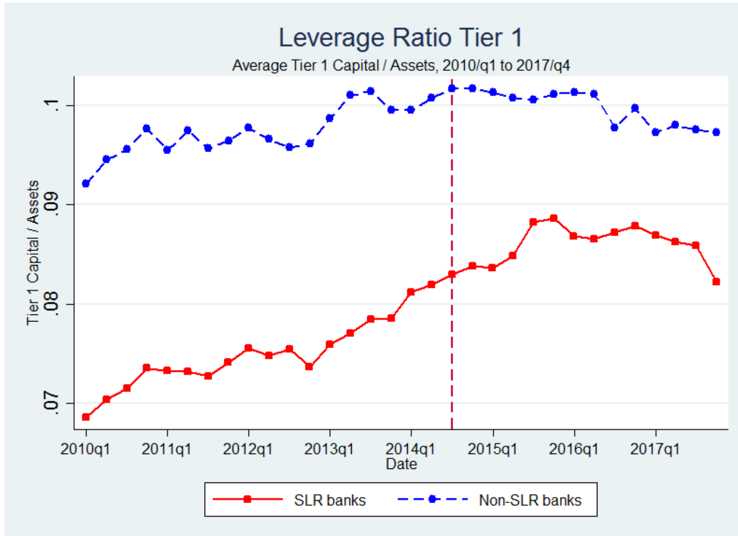


Figure 3.1: Leverage Ratios from 2010 to 2017.

Note: 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.

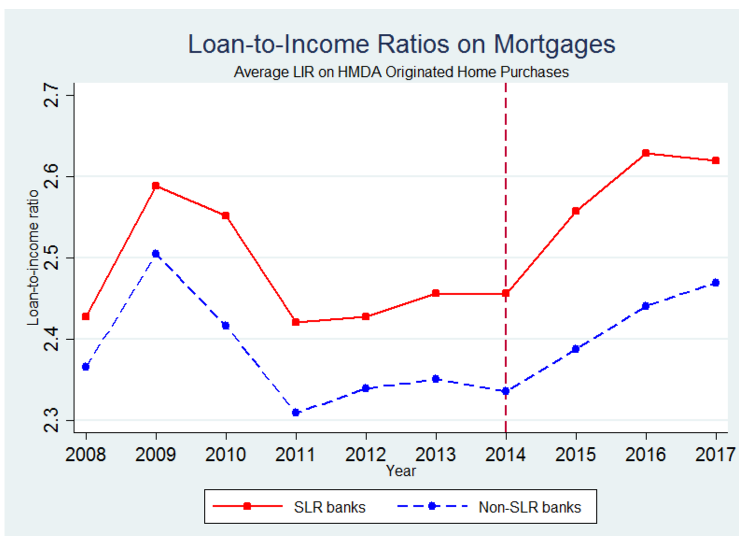
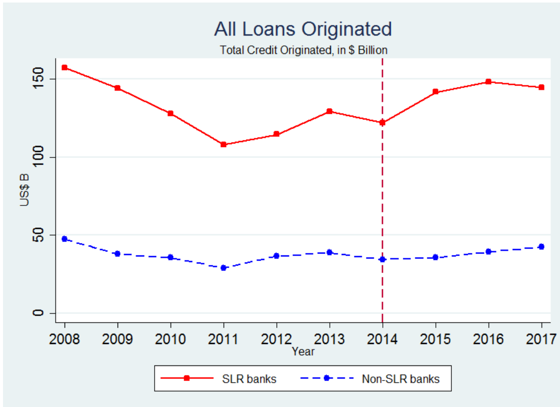
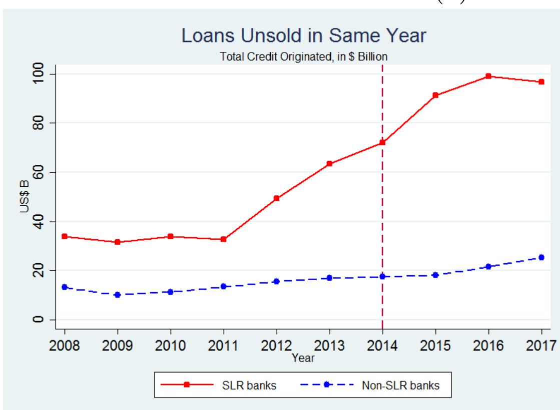


Figure 3.2: Loan-to-income ratios on home mortgages from 2008 to 2017.

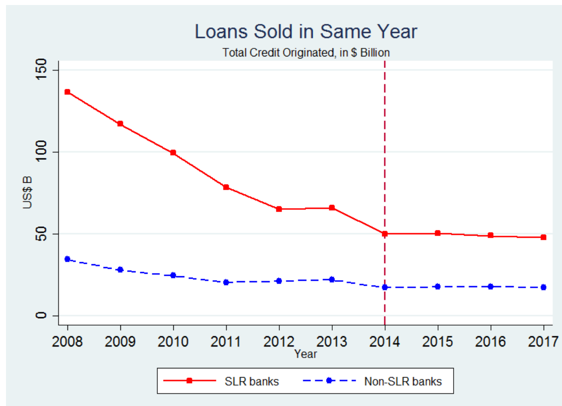
Note: This figure plots the average loan-to-income ratios (LIR) of originated home purchase loans over time for banks in the treatment (red line) and control groups (blue dotted line). The treatment group is composed of banks subject to Supplementary Leverage Ratio (SLR) rule active in the home mortgage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



(a) All loans originated



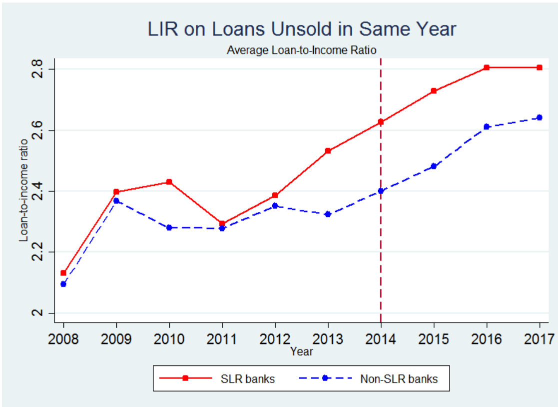
(b) Loans unsold in same year



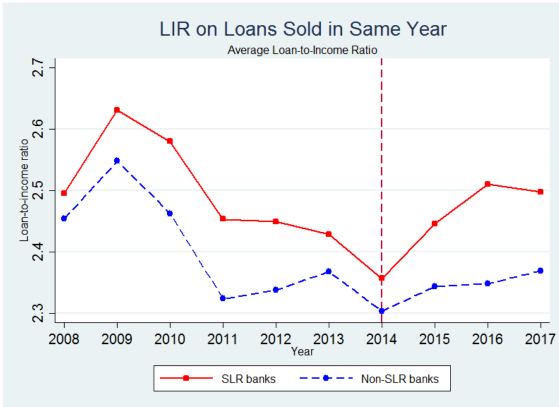
(c) Loans sold in same year

Figure 3.3: Aggregate amount of home mortgages originated from 2008 to 2017.

Note: 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) LIR on loans unsold in same year



(b) LIR on loans sold in same year

Figure 3.4: 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.

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

Source: Federal Reserve Board publications; Choi, Holcomb and Morgan (2018).

Table 3.1: Six year timeline of SLR implementation.

	SLR group Bank Holding Company	Total Assets		Non-SLR group 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

Note: 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.

Table 3.2: Sample of Bank Holding Companies.

	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	

Note: 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.

Table 3.3: Capitalization and bank characteristics before and after treatment.

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

Note: 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.

Table 3.4: Aggregate home mortgage credit and number of loans originated from 2008 to 2017.

	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	

Note: 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.

Table 3.5: Originated loans characteristics before and after treatment.

	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

Note: 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.

Table 3.6: Effect of the SRL rule on risk-taking in originated home purchase loans: baseline changes-in-changes estimation results.

	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

Note: This table presents estimates of the covariates regression for the loan-to-income ratio changes-in-changes model from Table 3.6. Dummies *T=1* and *G=1* indicate, respectively, the post-treatment period and the treatment group. Sample of loans is the same as Table 3.6.

Table 3.7: Covariates regression from baseline changes-in-changes risk-taking model.

	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

Note: 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. This estimation uses 200,000 observations randomly sampled from the full dataset of loans.

Table 3.8: Comparing the effect of the SRL rule on risk-taking between unsold and sold loans.

	Loan-to-income ratio			
CIC estimate (quantile)	(1) No Controls	(2) State FE	(3) MSA FE	(4) 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

Note: 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. This estimation uses 116,635 observations randomly sampled from the full dataset of loans.

Table 3.9: Test for early treatment hypothesis in risk-taking.

	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	0.0709	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

Note: 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. This estimation uses 116,635 observations randomly sampled from the full dataset of loans.

Table 3.10: Test for early treatment hypothesis in risk-taking: unsold and sold loans.

	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

Note: 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".

Table 3.11: Effect of the SRL rule on risk-taking, higher priced loans.

	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

Note: This table presents estimates of the covariates regression for the loan-to-income ratio changes-in-changes model from Table 3.11. 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 3.11.

Table 3.12: Covariates regression from changes-in-changes risk-taking model, higher priced loans.

	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

Note: 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".

Table 3.13: Effect of the SRL rule on loan spread, higher priced loans.

	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

Note: This table presents estimates of the covariates regression for the loan spread changes-in-changes model from Table 3.13. 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 3.13.

Table 3.14: Covariates regression from changes-in-changes loan spread model, higher priced loans.

	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

Note: 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.

Table 3.15: Comparing the effect of the SRL rule on risk-taking between unsold and sold loans, higher priced loans.

	Loan Spread					
	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	0.3193	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

Note: 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.

Table 3.16: Comparing the effect of the SRL rule on spread between unsold and sold loans, higher priced loans.

	Loan-to-income ratio			
CIC estimate (quantile)	(1) No Controls	(2) State FE	(3) MSA FE	(4) 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

Note: 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.

Table 3.17: Test for early treatment hypothesis in risk-taking, higher priced 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

Note: 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.

Table 3.18: Placebo test: risk-taking in originated home purchase 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

Note: 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.

Table 3.19: Robustness test ignoring largest banks on the treated group.

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

Note: 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.

Table 3.20: Credit supply and changes in house prices: difference-in-differences estimation.

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

Note: 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.

Table 3.21: Difference-in-differences parallel trend test for house prices model.

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Appendix A

Data source details

This Appendix presents some details on data sources and procedures used in the chapter "Bank leverage limits and risk-taking in the mortgage market: evidence from post-crisis reforms". The first section presents the main bank mergers occurring in the sample period while the second section describes variable definitions and original sources.

A.1 Bank mergers

	Surviving Bank	Acquired Bank	Merger Date
1	BMO Fncl Corp	Harris FC	2011/q3
2	Capital One FC	ING Direct USA	2012/q1
3	MUFG Amers Holds Corp	Pacific Capital	2012/q4
4	M&T Bk Corp	Hudson City	2015/q4
5	BNP Paribas USA	First Hawaiian	2016/q3
6	Huntington Bshrs	FirstMerit Corp	2016/q3
7	Keycorp	First Niagara FG	2016/q3
8	MUFG Amers Holds Corp	*change to holding company*	2016/q3

Table A.1: Main bank mergers during sample period (2011-2017). Source: FFIEC/NIC and FRY-9C.

A.2 Variable definitions and sources

Variable name	Definition
Risk-Based Capital Ratio	capitalRatio_riskBased
Risk-Based Capital Ratio Tier 1	capitalRatio_riskBased_tier1
Leverage Ratio Tier 1	capital_tier1 / assets_total
RWA / Total Assets	assets_RWA / assets_total
Loans-to-Assets	loans_leases_total / assets_total
Liquid Assets	(fed_funds_sold_avg + securities_availSale) / assets_total
Trading Assets	assets_trading_total / assets_total
Securities-to-Assets	securities_availSale / assets_total
Loans-to-Deposits	loans_leases_total / (deposits_interestBearingTotal + deposits_nonInterestBearing)
ROE	income_net / capital_equity_total
Net Income-to-Assets (ROA)	income_net / assets_total
Interest Income-to-Assets (ROA)	income_interest_total / assets_total
Non Interest Income-to-Assets	income_nonInterest_total / assets_total
Share of Loans Secured by Real Estate	loans_secured_restate / loans_leases_total
Share of Commercial & Industrial Loans	loans_comm_ind_us / loans_leases_total
Loans Past Due Ratio	(loans_pastDue_90_acc + loans_pastDue_nonAccrual) / loans_leases_total
Charge-offs Ratio	chargeOffs_allowLoanLeasesLoss / loans_leases_total

Primary variable name	Original source code
assets_total	bhck2170
capitalRatio_riskBased	bhca7205 or bhck7205 (before 2014)
capitalRatio_riskBased_tier1	bhca7206 or bhck7206 (before 2014)
capital_tier1	bhca8274
capital_equity_total	bhck3210
assets_RWA	bhcaa223 or bhcka223 (before 2014)
loans_leases_total	bhck2122
fed_funds_sold_avg	bhck3365
securities_availSale	bhck1773
assets_trading_total	bhck3545
deposits_interestBearingTotal	bhdm6636
deposits_nonInterestBearing	bhdm6631
income_net	bhck4340
income_nonInterest_total	bhck4079
income_interest_total	bhck4107
loans_secured_restate	bhck1410
loans_comm_ind_us	bhck1763
loans_pastDue_90_acc	bhck1607
loans_pastDue_nonAccrual	bhck1608
chargeOffs_allowLoanLeasesLoss	bhck4635

Table A.2: Bank variable definitions and original data sources.