Management Compensation and Risk Transfer: Evidence from Mortgage Lending and Securitization

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Abstract

We examine how management compensation policies affect bank mortgage lending and securitization decisions using exogenous variation in stock option grants generated by FAS 123R, which requires all firms to expense options. Embedding a difference-in-differences specification within a multinomial logit model, we find that reduced option compensation causes the treated banks to be more likely to sell risky mortgages for securitization, that is, to transfer risk off their own balance sheets. Interestingly, however, they do become more likely to reject risky mortgage applications, that is, option compensation does not change the overall riskiness of approved mortgages. Furthermore, the effect is concentrated in large banks, suggesting that managers' risk-taking incentives at large banks are more sensitive to option compensation.

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Key Words: Management Compensation, Stock Options, Mortgage, Bank Risk Taking, FAS 123R, HMDA, Securitization

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1. Introduction

The meltdown of the mortgage market triggered the 2008-2009 financial crisis. The Financial Crisis Inquiry Commission cites the explosion in risky subprime lending and securitization as the main reason for the financial crisis (The Financial Crisis Inquiry Commission 2011) in their final report. Many recent studies also suggest that the relaxation of mortgage lending standards was the main contributing factor of the subsequent large-scale mortgage default (Demyanyk and Van Hemert 2011 and Dell'Ariccia, Igan, and Laeven 2012).

On the other hand, management compensation policies are also often blamed as one of the underlying causes of the 2008-2009 financial crisis. Many argue that compensation policies, especially stock options, often encourage managers of financial institutions to take excessive risks, leading to the near-collapse of the U.S. financial system (e.g., Bebchuck, Cohen, and Spamann 2010). In his testimony to Congress, former Treasury Secretary Timothy Geithner argues, "I think, although many things caused this crisis, what happened to compensation and the incentives that created risk taking did contribute in some institutions to the kind of vulnerability we saw in this financial crisis."[2] The incentive compensation guidance issued by Federal Reserve, OCC, OTS, and FDIC in July 2010 called for "incentive compensation arrangements at banking organizations appropriately tie rewards to longer-term performance and do not undermine the safety and soundness of the firm or create undue risks to the financial system." [3] Management stock options,

[2] Statement of Treasury Secretary Geithner to the Senate Subcommittee of the Committee on Appropriations, 111th Congress. "Financial Services and General Government Appropriations for Fiscal Year 2010," 16-17. U.S. Government Printing Office, Washington, DC.

[3] http://www.federalreserve.gov/newsevents/press/bcreg/20100621a.htm

which are believed to be extremely powerful in inducing risk-taking behavior, are at the center of the debate because holders of options would benefit from high volatility of underlying asset prices.

Despite the widely-held belief that management compensation contributes to excessive bank risk-taking, however, little empirical evidence exists linking mortgage lending and management compensations in financial institutions. This paper contributes to the literature by investigating the role of management option compensation on mortgage lending and securitization policies during the years surrounding the financial crisis. This paper is the first to examine how risk-taking incentives affect lending and securitization policies simultaneously, which turns out to be critical in fully understanding bank risk-taking behavior.

One major challenge in identifying the effect of management compensation is that compensation policies are likely endogenous and are potentially correlated with many unobservable firm characteristics. Hence, a correlation between compensation policies and mortgage lending may tell us little about the causal effect of compensation on mortgage origination. In this paper, we overcome this difficulty by exploiting plausibly exogenous variation in management stock options generated by the accounting rule change, FAS 123R, which increases the cost of granting stock options to corporate employees.

FAS 123R was passed in 2004 and mandated by U.S. Securities and Exchange Commission (SEC) in 2005. Firms are required to adopt FAS 123R starting from the first fiscal quarter after June 2005.[4] FAS 123R requires all firms to expense employee stock option compensation, which makes granting stock options more costly and less attractive. In response, following the passage of this accounting change, firms significantly reduced their stock option grants and replaced

[4] For example, if a firm's fiscal year ends in July, then the firm will start adopting FAS 123R in August of 2005; likewise, if a firm's fiscal year ends in December, then January 2006 is the first month that FAS123R becomes mandatory for the firm.

options by restricted stocks, resulting in lower risk-shifting incentives (Carter, Lynch, and Tuna 2007; Chava and Purnandum 2010; Hayes, Lemmon and Qiu 2012; Bakke, Mahmudi, Fernando, and Salas 2015). It is worth noting that FAS 123R not only can affect stock option granted to executives but also can potentially affect stock options granted to rank and file employees, who make daily operating decisions affecting the overall risk-taking behavior of the bank. In this study, we exploit the exogenous decrease in management stock option grants and examine how compensation policies affect risky mortgage lending after the implementation of FAS 123R.

We use loan level information from Home Mortgage Disclosure Act (HMDA) to identify risky mortgage lending and securitization. Specifically, to examine the *causal* effect of management compensation policies on mortgage lending and securitization, we nest a difference-in-differences specification within a multinomial logit model. In this setting, the treated group consists of banks that had stock options but did not expense stock options before the implementation of FAS 123R. Banks in the treated group, therefore, will find stock options less attractive after FAS 123R. We have two control groups. The first control group consists of banks that had stock options but also voluntarily expensed stock options and the second control group consists of banks that had no stock options before the implementation of FAS 123R. Because banks in both control groups do not have to make any changes, they are unlikely to be affected by FAS 123R. Consistent with the intended effect of FAS 123R, we indeed find that the number of option grants to CEO declines by almost 10 percent and the proportion of the value of options grants relative to total compensation decreases from 45 percent to 19 percent after FAS 123R, a reduction of 26 percentage points for treated banks. In sharp contrast, control banks increase rather than decrease option awards to their CEOs and experience a decrease of only 4 percentage points in the proportion after FAS 123R.

Our multinomial logit results show that treated banks are more likely to sell mortgage with high loan-to-income ratios, i.e., the risky mortgage application, for securitization after FAS 123R relative to control banks, suggesting that treated banks become more risk averse and shift risk off their own balance sheets after FAS 123R. However, we do not find evidence that the treated banks become less likely to originate risky mortgages. The results suggest that, while option compensation is powerful in affecting bank risk-taking, it does not affect the overall riskiness of mortgages and rather it only affects whether or not banks retain risky mortgages on their own balance sheets. Our results may also partly explain the melt down of the subprime mortgage market, as FAS 123R causes the banks to sell more risky subprime mortgages for securitization.

One challenge to the identification strategy is that the assignment of treated and control banks is not random. A bank's decision to grant stock options or to voluntarily expense the options prior to FAS 123R may be endogenously determined, which may result in systematic pre-treatment differences between treated and control banks and may bias our baseline results. To ensure that the results are not driven by these potential pre-treatment differences, we conduct several robustness and placebo tests. We first conduct two diagnostic checks on the parallel trend condition as suggested by Roberts and Whited (2013). Specifically, in the first test, we compare the moving trends of the percentage of risky loans rejected or securitized by treated banks to those by control banks. We find that the trends are similar prior to FAS 123R. However, after FAS 123R, the two trends suddenly split with control banks continuing on the pre-event trend while treated banks exhibiting a sharp change in direction (see Figure 1).

Next, we conduct a placebo test by creating a placebo event before the actual event while maintaining the assignment of treated and control banks. We then examine whether the placebo event has similar effects on bank risk-taking as FAS 123R. We indeed find no effects of the placebo

event. Therefore, results from both tests suggest that the key identifying assumption of the difference-in-differences estimation, the parallel trend condition, is likely to be satisfied, suggesting that FAS 123R has a causal *causes* impact on bank risk-taking.

Next, we switch the focus to our two control groups and conduct additional tests to further alleviate the concerns of potential biases. We show that our difference-in-differences results remain robust if we compare the treated banks with banks in the two control groups separately. In particular, as our two control groups differ substantially across many dimensions and the very fact that one group had no stock options and the other group had and expensed stock options before FAS 123R makes it unlikely that they share the same bias arising from pre-treatment differences. In other words, the biases, if they exist, are likely to be orthogonal to each other, and are therefore unlikely to lead to the same observed effects. The fact that we obtain the same results from using these two control groups separately therefore suggests that the baseline results are unlikely driven by either bias.

We also conduct a propensity score matching exercise to ensure treated and control banks are similar, at least along the dimension of those observable characteristics. Comparing matched treated and control banks, we find qualitatively similar results.

Another concern is that our observed outcome may be driven by changes in demand for mortgage loans rather than by changes in the supply of mortgage due to option compensation surrounding FAS 123R. For instance, treated and control banks may attract different applicants after FAS 123R, and the baseline results are driven by the heterogeneity in applicant characteristics rather than by managers' differential incentives of risky loan originations. To disentangle the supply side determinants of bank risk-taking from the demand side, we match all loan applications with exactly the same applicant characteristics and treat the matched loan applications as from the

same applicant. After removing loans without any matched pairs, we re-estimate the impact of FAS 123R on bank risk-taking, which allows us to examine treated and control banks decisions on loan applications made by the same applicant. Using the same difference-in-differences method, we continue to find similar results that treated banks become more likely to shift high loan-to-income mortgages off their balance sheets after FAS 123R relative to control banks.

Additional analyses show that the effect of FAS 123R is mostly concentrated in large banks, which is consistent with the idea that implicit government guarantee for too-big-to-fail banks makes managers' risk incentives more sensitive to option compensation and encourages managerial risk taking for large banks.

The rest of the paper is organized as follows. Section 2 reviews related literature; section 3 describes the implementation of FAS 123R; section 4 describes the data and sample construction; section 5 presents the identification strategy; section 6 discusses the empirical results; and section 7 concludes.

2. Literature Review

Many recent papers suggest that mortgage lending standards deterioration before the 2008-2009 financial crisis was one of the main driving forces of the financial crisis. Demyanyk and Van Hemert (2009) find that mortgage quality deteriorates before the financial crisis. Mian and Sufi (2009) find that credit expansion in subprime lending leads to sharp increase in mortgage default. Later literature tries to explain why lenders relax lending standards and expand credit before the financial crisis. Dell'Ariccia, Igan, and Laven (2011) find that lenders expand credit supply in areas with higher house price growth rates. Maddaloni and Peydro (2011) find that low short-term interest rates soften lending standards. Purnanandm (2011) find that securitization causes banks to

originate poor quality mortgages. Mian, Sufi, and Trebbi (2013) find that political economy plays an important role in mortgage credit expansion. This paper contributes to this strand of literature by examining how incentives within the lending firms may have contributed to risky mortgage lending and securitization. Specifically, this is the first paper to model how risk-taking incentives affect the lending and securitization decisions simultaneously.

This paper contributes to the general debate on whether management compensation induces excessive risk-taking of financial institutions. On one side of the debate, Bebchuk, Cohen, and Spamann (2010), DeYoung, Peng, and Yan (2009, Mehran and Rosenberg (2009), Balachandran, Kogut, and Harnal (2010), Chesney, Stromberg, and Wagner (2012) all find that higher equity-based pay (restricted stocks and/or options) are associated with a higher default probability. In contrast, the other side of the debate argues that management compensation plays little role in bank risk-taking or at least it does not *cause* excessive risk-taking. Fahlenbrach and Stulz (2011) find that higher options grants to bank CEOs do not lead to worse performance of the banks during the financial crisis. They conclude that incentive compensation plans do not affect bank risk-taking. Using federal loan assistance to measure risk-taking, Gande and Kalpathy (2014) find no evidence that pre-crisis CEO risk-taking incentives are related to bank performance during the financial crisis. Cheng, Hong, and Scheinkman (2015) provide an alternative perspective on the relationship between management compensation and risk-taking by showing, in a classical principal-agent setting, that riskier firms may offer higher total pay as compensation for extra risk managers bear. They show that the positive relationship between management compensation and risk taking can be driven by reverse causality. Their findings suggest that it is critically important to use exogenous variation in management compensation for any study trying to identify the *causal* effect of management compensation on risk-taking.

All these papers, while providing some insights on the relationship between management compensation and bank risk-taking, suffer from the lack of identification in their empirical models. Management compensation is endogenously determined and is correlated with bank characteristics that are potentially unobservable but yet affect risk-taking of the bank. Therefore, most existing studies can only claim correlation but not causality. Even authors that use the simultaneous equation or the instrumental variable approach to address this problem (for example, DeYoung, Peng and Yan 2009) acknowledge the threat of imposing arbitrary exclusion conditions on the validity of identification. This identification issue is also pointed out by Cheng, Hong, and Scheinkman (2015), who show that the positive relationship between management compensation and risk-taking can be driven by reverse causality. This paper, by using FAS 123R as a quasi-natural experiment, is the first to identify the *causal* effect of management compensation on risky mortgage lending and securitization.

This paper is also the first to examine the effect of management compensation on risk-taking at the loan level. Most existing studies rely on bank-level data to measure risk-taking. Focusing on the loan-level data of mortgage applications enables us to effectively disentangle the effect of management compensation from demand side factors. In addition, examining the loan approval decisions at the origination enables us to more accurately capture *ex ante* risk-taking incentives. Most existing studies only focus on *ex post* measures of bank risk-taking, which may not accurately reflect the *ex ante* risk-taking incentives (Houston and James 1995). Furthermore, focusing on individual mortgages allows us to study lending and securitization decisions simultaneously, which is critically in fully understanding how banks shift risk when facing different incentives.

This paper also contributes to the literature on how bank corporate governance in general affects bank risk-taking (for example, Saunders, Strock, and Travlos 1990; Gorton and Rosen 1995; Laeven and Levine 2009; Thakor 2014 ; Berger, Imbierowicz, and Rauch 2015.

Several recent papers also use FAS 123R as a natural experiment to study the relation between management stock option grants and corporate risk-taking. Chava and Purnanandam (2010) find that FAS 123R results in reduced leverage, higher cash balances, and greater earnings management, which is consistent with the argument that executive stock options increase CEO risk-taking incentives. In contrast, however, Hayes, Lemmon and Qiu (2012) find little effect of FAS 123R on five measures of corporate risk-taking, namely, R&D expense, capital expenditure, leverage, cash holdings, and stock volatility. Bakke, Mahmudi, Fernando, and Salas (2015) instead examine the effect of FAS 123R on corporate risk management and find that FAS 123R leads to higher hedging intensities of treated firms, which is consistent with the argument that more stock option grants increase managerial risk-taking incentives. Chu and Ma (2015) examine how FAS 123R affects the cost of debt via its effect on managerial risk-shifting incentives. We are the first to use FAS 123R to examine the *causal* effect of management compensation on risky mortgage lending and securitization.

3. The Accounting Treatment of Stock Options and FAS 123R

In October 1972, the Accounting Principle Board (APB) issued Opinion No. 25, *Accounting for Stock Issued to Employees*. Under APB Opinion 25, the amount of option expense recognized for issuing stock options to employees is based on the intrinsic value of the stock options, which is the difference between the exercise price of the stock options and the grant date price of the underlying stock. Hence, under the intrinsic value method, firms can avoid option expense by granting a fixed number of options with exercise prices equal to or above the

underlying stock prices at the grant date. As stock options became increasingly popular in the 90's, the Financial Accounting Reporting Board (FASB) revised APB Opinion NO. 25 and issued FAS 123 in October 1995, promoting the use of the fair value method of accounting for stock option compensation. Under the fair value method, firms are required to expense the fair value of options granted based on an option valuation model (such as the Black & Scholes model). However, due to heavy political pressure, the fair value method was not mandated by FAS 123 and the intrinsic value method was still allowed as an alternative method. However, if a firm elected to use the intrinsic value method, it had to disclose in the footnote what costs of stock options would be on a pro forma basis if the fair value method had been used. Not surprisingly, after the passage of FAS 123, most firms chose to use the intrinsic value method and avoided expensing options by granting options with exercise prices equal to grant date underlying stock prices. Due to the favorable accounting treatment, the usage of stock options increased significantly in the late 1990s and early 2000s (Murphy 2002; Hall and Murphy 2003). The prevalence of stock option awards had a significant impact on managers' risk preferences and increased managers' incentives to engage in risky investments. As a consequence, managers' excessive risk taking was linked to aggressive accounting practices and the subsequent collapsing of many corporations. The best-known cases are the bankruptcies of Enron and WorldCom (Cassidy 2002; Madrick 2003).

In light of these accounting scandals, FASB started focusing on problems with the option accounting practices and issued an exposure draft, proposing to eliminate the intrinsic value method and requiring the fair value method for stock options. In December 2004, FASB issued FAS 123R, *Share-Based Payment*, which supersedes FAS 123 and makes the fair value method the only accounting method allowed for stock option compensation. FAS 123R became effective

starting from the first fiscal year after June 15, 2005 for large public firms.[5] Under FAS 123R, firms have to measure the fair value of the stock options granted to employees and recognize option expenses through the income statement over the vesting period. Hence, FAS 123R eliminates the favorable accounting treatment of stock options and alters firms' use of stock options in compensation plans.

4. Data and Sample Construction

Our sample construction begins with all financial firms (SIC code between 6000 and 6999) in the annual ExecuComp database from 2002 through 2008, from which we also obtain CEOs' option compensation data. We merge the list of financial firms with CRSP to get the PERMCO of these financial firms and then use the New York Fed link file to get the RSSDID, which is the unique identifier for banks or bank holding companies. The RSSDID is then used to match with the Call Report data.

We obtain mortgage application data from Home Mortgage Disclosure Act (HMDA) Loan Application Registry, which covers almost all residential mortgage applications in the U.S.. The HMDA data has recently been used in the finance literature to examine bank lending behavior (e.g. Loutskina and Strahan 2009, 2011; Duchin and Sousyura 2014; and Gilje, Loutskina and Strahan 2016). The observation unit is an individual mortgage application, which contains information on borrower characteristics (income, gender, and race), location of originations (state, metropolitan statistical area, county, and census tract), requested loan information (loan amount and loan type), banks' decision (approved, denied, or withdrawn), and whether or not the loan is sold during the year of origination. We apply the following procedures to filter the loan applications. First, we keep only loan applications that are either approved or denied. Incomplete or withdrawn

[5] The effective date for small public firms and private firms is the first reporting period after December 15, 2005.

applications are removed from the sample. Second, we only keep conventional loans, that is, we exclude government insured FHA, VA, FAS, or RHS loans because these loans receive government insurance and therefore are different from conventional loans in lenders' risk exposure. Third, we require loan applications to be new loan applications and exclude refinancing. Lastly, we only keep loans that are taken to finance owner-occupied 1-4 family residential units because loans for multiple family homes are very different.

We then merge the HMDA mortgage application data with the Call Report data at the fourth quarter of the year prior to the mortgage application. Specifically, for banks reporting to Office of the Comptroller of the Currency (OCC, agency code equal to 1), we match the respondent ID in HMDA to RSSD9055 in the Call Report; for banks reporting to the Federal Reserve (agency code equal to 2), we match the respondent ID in HMDA to RSSD9001 in the Call Report; and for banks reporting to the Federal Deposit Insurance Corporation (FDIC, agency code equal to 3), we match the respondent ID in HMDA to RSSD9050 in the Call Report.

The final sample consists of 3,499,591 mortgage applications from 2003 to 2008, among which 773,440 applications are rejected, 987,844 are approved and not sold during the year, and 1,738,307 loan applications are approved with loans sold during the year. These mortgage applications are made to 91 unique banks, among which 67 banks are in the treated group, i.e., banks that had stock option compensation but did not voluntarily expensed stock options before FAS 123R. The treated banks account for 1,972,384 mortgage application, and the remaining applications are made to control banks. Among the remaining 24 banks in the control group, 14 banks are in the first control group, i.e., banks that had and voluntarily expensed stock options before FAS 123R; and 10 banks are in the second control group, i.e., banks that paid no stock options before FAS 123R.

The summary statistics of the data are presented in Table 1. While most variables are self-explanatory, the definition of *Risk* needs some explanation. We define a mortgage application to be risky, or the variable *Risk* equals one if the loan-to-income ratio of the mortgage application is greater than three. We choose three as the threshold for risky loans based on the following calculation. The mortgage underwriting standards in the United States often require the debt payment-to-income ratio be lower than 28%. For a 30-year fixed-rate mortgage at a 6% interest rate and 1.5% other payments (property tax, insurance, etc.), the 28% payment-to-income ratio implies a loan-to-income ratio around three. While the choice of the threshold may appear arbitrary, the results are robust to other thresholds. Specifically, we tried the following alternative thresholds: (1) the annual top quartile of the loan-to-income ratio of the sample; (2) the annual threshold calculated based on the average prevailing mortgage interest rates on 30 year fixed rate conventional mortgages;[6] (3) the annual threshold calculated based on the average prevailing mortgage interest rates on 15 year fixed rate conventional mortgages.

The average loan amount is $256,371 and the average applicant income is $110,714. Among all the applicants, about 65.9% are male; 75% are white; and 12.1% are Hispanic/Latino.

[Insert Table 1 Here]

5. Empirical Strategy

To empirically identify the causal effect of management compensation on mortgage lending and securitization, we explore plausibly exogenous variation in management compensation due to the implementation of FAS 123R. Specifically, we compare the effect of FAS

[6] We use the mortgage interest rate on30-year fixed rate conventional mortgages (obtained from the Federal Reserve Website) to calculate mortgage payments, and define an application as risky if the total payment to income ratio is higher than 28%.

123R on a group of banks that are affected by FAS 123R (the treated group) and on the other group of banks that are not affected by FAS 123R (the control group) before and after the implementation of FAS 123R. Following Bakke et al (2015), we identify two control groups that are unlikely to be affected by the implementation of FAS 123R. The first control group consists of banks that started to use fair value method in 2003 or earlier.[7] Because these banks have already implemented the requirements of FAS 123R before the mandatory implementation, they are less likely to be affected. The second control group consists of banks that did not pay options to their executives in the pre-treatment period, and the requirement of expensing stock options therefore does not apply to them. The treated banks are all remaining banks that are covered by the ExecuComp database and paid options to executives but did not expense the options before the implementation of FAS 123R. In our main specification, we combine the two control groups. However, we also present results using the two control groups separately.

Formally, we embed a difference-in-difference specification in a multinomial logit model of loan application outcome for treated and control banks surrounding the adoption of FAS 123R.[8] By pooling both approved and denied loan applications, we are able to isolate banks' lending policy from changes in demand for loan credit. Empirically, we estimate the following difference-in-differences model:

$$Prob(Outcome = l) = \frac{Exp(Y_{ijktl})}{\sum_m Exp(Y_{ijktm})}, \qquad (1)$$

Where the *Outcome l* can be rejected, approved and retained, and approved and sold, and Y_{ijktl} for mortgage application *i* at bank *j* in metropolitan statistical area (MSA) *k* and year *t* is a linear difference-in-difference specification as follows:

[7] We obtain the list of firms that adopted the expensing of stock options prior to the effective date of FAS 123R from Bear Stearns Equity Research dated December 16, 2014 (Bear Stern & Company, Inc. 2004).
[8] The difference-in-difference specification follows Duchin and Sosyura (2013).

$$Y_{ijktl} = \alpha_{kl} + \alpha_{tl} + \beta_{1l}Risk_i \times Post_t \times Treat_j + \beta_{2l}Post_t \times Treat_j + \beta_{3l}Treat_j \times Risk_i + \beta_{4l}Post_t \times Risk_i + \beta_{5l}LTI_i + \gamma_{1l}X_i + \gamma_{2l}Z_j + \varepsilon_{ijktl} \quad (2)$$

where α_{kl} is the local market (MSA) fixed effects; $\alpha_{tl}$is the year fixed effect; $Risk_i$ is an indicator for risky loans, which equals one if the loan-to-income ratio is greater than three.[9] $LTI_i$is the loan-to-income ratio. $Treat_j$ equals one if bank j is in the treated group, and equals zero otherwise. $Post_t$ equals one if the mortgage application is made after the implementation of FAS 123R, and equals zero otherwise. Our main variable of interest is the three-way interaction term$Risk_i \times Post_t \times Treat_j$, which captures the effect of FAS 123R on risky mortgage lending at treated banks relative to control banks. Because the treatment status is defined at the bank level, we also cluster the standard errors at the bank level.

X_i is a vector of applicant characteristics, including applicant sex, race, and ethnicity; $Z_j$is a vector of bank characteristics, including bank size, liquidity, capital, charge-off, ROA, deposits, and also bank CEO compensation sensitivity to stock price, CEO age and, CEO tenure.

One concern for the difference-in-differences method in this context is that the treated and control banks may be fundamentally different before FAS 123R, which may explain why some banks choose not to grant stock options or choose to voluntarily expense stock options when options are granted while other banks do not. In particular, the control banks' decision not to issue stock options or to expense options voluntarily prior to FAS 123R is not random, and the endogenous decisions may bias our baseline results. To mitigate this concern, we first examine bank characteristics between the treated and control groups in the pre-treatment period. To do so, we conduct univariate t-tests comparing bank characteristics measured at the fourth quarter of

[9] We use loan-to-income ratio to measure customer riskiness as it is a commonly used in prior studies (e.g., Rosen 2010; Duchin and Sosyura 2014) and as well as in practice. For example, regulators use this ratio to determine the eligibility for FHA loans and later the federal loan modification programs.

2004, that is, right before the implementation of FAS 123R, and present the results in Table 2. An important observation from Table 2 is that the treated and control banks are similar in almost all measurements and the difference-in-means tests are statistically insignificant except for bank deposits, which is positive and statistically significant. This result is reassuring because the similarity between the treated and control groups makes it unlikely that our results are driven by unobservable differences between the two groups.

[Insert Table 2 Here]

In addition, the use of the two different control groups also helps mitigate this potential concern. As argued in Rosenbaum (1987), Heckman and Hotz (1989), and Roberts and Whited (2012), because the differences between the treated group and the different control groups are likely to be different, the two different control groups would result in different biases if they exist. Therefore, finding the same difference-in-differences effects of FAS 123R for both control groups will lend confidence that our results are driven by the treatment effect rather than by any unobservable bias.

6. Empirical Results

6.1 Effect of FAS 123R on CEO Option Compensation

In this section, we first verify that FAS 123R indeed has differential impacts on executive option compensation for the treated and control banks. To do this, we obtain CEO compensation data from ExecuComp and compare the CEOs' option compensation and the sensitivity of their option compensation to changes in stock returns volatility (vega) and equity compensation

sensitivity to changes in stock price (delta). We follow Guay (1999) and Hayes et al. (2012) to measure compensation vega and delta.[10] The Results are tabulated in Table 3.

As shown in the table, for the treated banks, the average fair values of options granted decreases 2.5 million dollars prior to FAS 123R to 1.18 million dollars after FAS 123R. In contrast, for control banks, the average fair values of options granted only decreases from 3.3 million dollars to 3 million dollars, the univariate difference-in-differences estimate is -0.963 million dollars, and is statistically significant. The proportion of fair values of option grants relative to total compensation decreases from 45% to 19% for treated banks after FAS 123R, a reduction of 26%. For the same ratio, control banks only experience a 4% reduction from 31% to 27% after FAS 123R. The univariate difference-in-differences estimate is again negative and statistically significant. The sensitivity of compensation to return volatility (option vega) is reduced from 210 to 132 for treated banks whereas the sensitivity decreases slightly from 492 to 488 for control banks. Overall, the results confirm our arguments that FAS 123R affects treated and control banks differently on their option awards to CEOs with treated banks experiencing a significant reduction in their stock option grants but not for control banks. As a consequence, CEO risk incentives arising from option compensation decrease significantly only for treated banks.

[Insert Table 3 Here]

As noted in the table, control banks and treated banks are quite different in the number and fair value of options grants as well as in option compensation vega and equity compensation delta. For example, the average option vega is 492 prior to FAS 123R for control banks and the same

[10] Securities and Exchange Commission imposed new disclosure requirements for compensation starting from 2007. As a consequence, individual compensations data items and their presentation for the period prior to 2006 are different from those after 2006 in ExecuComp. Hayes et al. (2012) provide detailed discussion on the differences. We strictly follow Hayes et al. (2012) in order to ensure that our vega and delta measures are consistent throughout our sample period.

value for treated banks is 210.[11] However, the significant difference by itself should not be a concern because our focus here is the change in option compensation surrounding FAS 123R, which effectively controls for the difference in the level of option compensation between treated and control banks. For instance, the average option vega for treated banks decreases by 68, which is a reduction of more than 37%; whereas for control banks, the average option vega decreases only by 4, which is less than a 1% decrease.

6.2 Baseline Results

We present the baseline multinomial logit regression results in Panel A of Table 4, in which we treat approved and retained as the base outcome. The results for denial is presented in column (1). The coefficient of interest, that is, the coefficient on the three-way interaction term, *Treat*×*Post*×*Risk*, is positive and statistically significant, suggesting that the treated banks become more likely to deny risky loan applications after FAS 123R relative to control banks. In Column (2), we present the results for approved and securitized. The coefficient on the three-way interaction term is again positive and statistically significant, suggesting that the treated banks are also more likely to securitize risky loans.

To assess the economic magnitude of the effects, we compute the marginal effects of the coefficients and present the results in Panel B of Table 4. The marginal effects results show that treated banks are 10.02% more likely to approve and securitize, and 8.51% less likely to approve and retain a risky loan application, and both effects are statistically significant. However, the marginal effect of rejecting a risky loan application is not statistically significant and the magnitude is also small. Overall, the results suggest that treated banks are not becoming more

[11] Although one group of control banks did not issue options between 2002 and 2005, some of them did issue options prior to 2002, which is included in the calculation of option vega and delta.

likely to reject risky loan applications, but rather they become more likely to transfer risky loans off their balance sheets by selling them for securitization.

Next, we include bank CEO characteristics and bank characteristics in the regressions. Most notably, we include *Delta*, the sensitivity of CEO compensation to stock prices, as an additional control variable. As documented in the literature, reducing stock options due to FAS 123R is often accompanied by an increase in restricted stocks, and therefore the effect in Columns (1) and (2) can instead be driven by the increase in restricted stocks. Controlling *Delta* mitigates this concern because any increase in restricted stocks should be reflected in the changes in *Delta.* The results are similar to those in Columns (1) and (2) of Panel A, and the coefficient estimates on *Treat*×*Post*×*Risk* in Columns (3) and (4) of Panel A remain positive and statistically significant. The marginal effects are also similar. The fact that adding more bank-level controls does not significantly change the magnitudes of the coefficient estimates is reassuring that the assignment of treated and control banks are not highly correlated with bank characteristics.

[Insert Table 4 Here]

In all specifications, the coefficient estimates on the loan-to-income ratio for the rejection equation (Columns (1) and (3)) are positive and statistically significant, suggesting that the likelihood of loan rejection (relative to approve and retain) increases with the loan-to-income ratio increases. However, the coefficients on *Treat*×*Post* have mixed signs and are statistically insignificant across all three model specifications, which indicates that FAS 123R does not cause significant changes in overall lending or securitization for treated banks.

Because the coefficient estimates of the multinomial logit regressions are difficult to interpret, we only present the marginal effects estimates of the three-way interaction term in results below.

6.3 Threats to Identification

The key identification assumption of the difference-in-differences method is the parallel trend assumption, that is, in the absence of the treatment, the outcome variables should have parallel trends both before and after the FAS 123R. Although the parallel trend condition is untestable, we follow Roberts and Whited (2012) to conduct several diagnostic tests. First, we conduct a graphical examination of the pre-treatment trend in risky mortgage lending. Specifically, we plot the annual average of *Risk,* i.e., the percentage of risky loans among approved and retained loans and securitized loans, and the results are presented in Figure 1, with Panel A for the percentage of risky loans approved and retained and Panel B for the percentage of risky loans securitized.

Focusing on the percentage of risky loans approved and retained in Panel A, before FAS 123R, i.e., years 2003-2005, the percentages of risky loans approved by treated and control banks are almost identical in that both are trending upward at the same rate, which suggests that, in the absence of FAS 123R, banks in the two groups would have similar appetite for risk. However, after the implementation of FAS 123R, while the control group continues the pre-treatment upward trend, the treatment group abruptly starts trending downward, resulting in a significant divergence in risky mortgage approvals between treated and control banks. Although the two trends start moving in the same direction again in 2007 and 2008, the gap between the two trends remains significantly large with the percentage of risky loans approved and retained by treated banks being much lower than that by control banks, suggesting that treated banks become more risk-averse after FAS 123R. In Panel B, the percentage of risky loans securitized show an opposite pattern. While the percentage of risky loans of the treated and control banks remain almost identical before FAS 123R, the percentage of risky loans for the treated banks become significantly higher than

those for the control banks, suggesting that the treated banks are transferring risky mortgages off their balance sheets by securitization.

[Insert Figure 1 Here]

Next, we follow Roberts and Whited (2012) and conduct a placebo test to further show that pre-existing trend differences between treated and control banks are unlikely to drive the baseline results. Specifically, we create an out-of-sample placebo experiment and use data 1997-1999 as the pre-event period and 2000-2002 as the post-event period. We maintain the same assignment of banks into the treated and control groups as in our baseline tests. We then use the same multinomial logit model to examine whether the placebo experiment has any effect on bank lending and securitization. If the parallel trend assumption holds, we should observe no difference in the outcome variables between treated and control banks before and after the event. The results are presented in Table 5. In all specifications, the marginal effects are all much smaller in magnitude than those in Table 4 and statistically insignificant. The results further suggest that the baseline results are unlikely to be driven by pre-existing trend differences between treated and control banks.

[Insert Table 5 Here]

We next turn our attention to our two control groups. The use of two different control groups should help mitigate the concern that the results may be driven by unobservable differences (bias) between the treated and control groups prior to FAS 123R (Rosenbaum 1987; Heckman and Hotz 1989; Roberts and Whited 2012). We first verify that the results are similar when we separate the two control groups. In Panel A of Table 6, we first present the results with treated banks and banks in the first control group only, i.e., bank issued stock options and chose to voluntarily expense stock options before FAS 123R. The results are qualitatively similar to those in Table 4. In Columns (2) and (4) for approve and retain decision, the marginal effects estimates of the three-

way interaction term *Treat*×*Post*×*Risk* are all negative and statistically significant, suggesting that, relative to banks in the first control group, treated banks are less likely to approve and retain risky loan application on their balance sheets . In Columns (3) and (6) for securitization, the marginal effects of the three-way interaction term remain positive and statistically significant, suggesting that, relative to banks in the first control group, treated banks are also more likely to securitize risky loans, and thereby shift risky loans off their balance sheets.

In Panel B of Table 6, we instead focus on the second control group, i.e., firms without stock option before FAS 123R. The results are again similar to those in Table 4. Because these two control groups differ substantially along many dimensions, it is unlikely that they have similar pre-existing differences with the treated banks that can result in similar effects. For example, banks in the treated group (with stock options and choosing not to expense the options before FAS 123R) may simply be riskier than firms in the first control group before, and the riskiness of the treated banks is decreasing over time. However, if the baseline results are driven by this difference, we are unlikely to observe this same effect between treated banks and banks in the second control group. Similarly, if the results are driven by differences between banks with stock options and banks without stock options, such as those in the treated group and those in the second control group, we are also unlikely to observe the same effect between treated banks and banks in the first control group. Obtaining similar estimation results with the two different control groups there reassures that the results documented in Table 4 are unlikely driven by pre-existing differences between treated and control banks.

[Insert Table 6 Here]

6.4 Matched Banks

To further alleviate the concern that the baseline results are driven by systematic differences between treated and control banks, we conduct a matching exercise. Because there are more treated banks than control banks, we start with the control banks for our matching exercise. Specifically, we first run a probit model of the treatment status (treat=1 and control=0) on bank characteristics for all banks before the implementation of FAS 123, and we choose one matched treated bank for each control bank with the closest propensity score to the control bank. We end up with 24 treated banks and 24 control banks, and then re-run the multinomial logit model on mortgage applications received by these 48 banks. The marginal effects of the results are presented in Table 7. The results are similar to those presented in Table 4, further suggesting that the baseline results are unlikely driven by systematic differences between treated and control banks.

[Insert Table 7 Here]

6.5 Matched Mortgage Applications

Another potential concern of the above results is that the treated and control banks may attract different types of borrowers, and the baseline results are driven by the heterogeneity in unobservable borrower characteristics that are correlated with the assignment of treated and control banks. For example, if treated banks attract high income (low risk) clientele than control banks, treated banks' mortgage lending will exhibit lower risk-taking than control banks. Hence, differences in customers' demand for mortgage loans across treated and control banks may explain the results documented in previous tables.

To mitigate this concern, we conduct a matching exercise and directly control for customer's demand effects. Specifically, we pool all loan applications, both approved and denied, from treated and control banks and match them based on observable applicant characteristics,

including location (state, MSA, county, and census tract), age, income, applicant and co-applicant gender, applicant and co-applicant race, applicant and co-applicant ethnicity. We require matched loans to have exactly the same characteristics and to be in the same year; however, we do allow the requested loan amount to be slightly different, but not by more than 10%. Because it is very unlikely that different applicants in the same census tract to have exactly the same set of characteristics, including income, it is reasonable to treat the matched loan applications as from the same applicant. We then delete all loan applications that we fail to find matched pairs. This newly constructed sample allows us to observe loan applications from the same applicant made to both treated and control banks.

We then re-estimate Equation (1) and examine how treated banks and control banks' lending decisions differ for the same applicant. To the extent that we correctly identify the applications from the same applicant, we can effectively control for all demand-side factors and reduce the bias due to the correlation between unobservable applicant characteristics and the assignment of treated and control bank. After the matching procedure, we are left with 587,426 loan applications. The average marginal effects estimates are presented in Table 8, and the results are similar to those presented in Table 4, suggesting that the baseline results are unlikely driven by unobservable demand side factors.

[Insert Table 8 Here]

6.6 Too-Big-To-Fail and Bank Risk-Taking

One particular concern of bank risk-taking behavior arises from the implicit government guarantee to bailout financial institutions when they are in trouble. The implicit government guarantee especially matters for banks that are deemed too big to fail (TBTF) because large banks

pose a systemic risk to the economy (Mishkin, Stern, and Feldman 2006 and Strahan 2013). Due to the implicit guarantee on banks' assets, managers from large banks have incentives to engage in risky business beyond what is efficient because they as well as shareholders do not internalize the adverse effects but reap all the upward benefits (Bebchuck and Spamann 2010). Therefore, managers' risk-taking incentives at large banks will be more sensitive to option compensation. As such, we expect the effects of FAS 123R on risky mortgage lending and securitization would be more pronounced for large banks than for small banks.

As TBTF is based on market perceptions rather than by any explicit government policy, no strong consensus on the determination of TBTF candidate banks among researchers. Nevertheless, using asset size is a simple yet intuitively appealing way to identify these groups. We partition the sample according to whether the size of a bank is larger than the annual median of the sample, and we then re-estimate the multinomial logit model on the two subsamples. The marginal effects estimates are presented in Table 9, with Columns (1)-(3) for larger banks and Columns (4)-(6) for smaller banks. We also use several alternative size cutoffs, such as $50 billion or $100 billion cutoffs, and find similar results.

Focusing on the results for larger banks in Columns (1)-(3), The result is consistent with our conjecture that the effect of option grants on risk taking is more pronounced for too-big-to-fail banks, which are more likely to be backed by implicit government guarantee. On the other hand, the marginal effects from Columns (4)-(6), for small banks, are all much smaller in magnitude and are statistically insignificant, suggesting that option compensation has little impact on risk taking for small banks.

[Insert Table 9 Here]

6.7 Additional Robustness Checks

The baseline results show that while option compensation affects how banks shift risky mortgages, it does not affect the overall decision of risky mortgage lending. To further show the overall effect, we then estimate a binary logit model, in which the outcome is either Approved (both retained and securitized) or rejected. The marginal effect of the effect of three-way interaction term *Treat*×*Post*×*Risk,* as presented in Column (1) of Table 10, is small and statistically insignificant, suggesting that FAS 123R does not change the overall approval rates of risky mortgage applications.

In Column (2), we then exclude the securitized loans from the sample and re-estimate the binary logit model. Consistent with the multinomial logit results, treated banks become less likely to approve and retain risky mortgages after FAS 123R. Finally, in Column (3), we focus only on approved loans, both retained and securitized, and the use the binary logit model to examine the banks' decision to securitize these approved loans. The results show that treated banks are more likely to securitized risky mortgages and thereby transferring risk off their balance sheets.

[Insert Table 10 Here]

We can reinterpret the results in Columns (1) and (3) as the result of a two-step decision making process. First, the banks make a decision whether or to approve the application; and in the second step, the banks decide whether or not to sell these mortgages for securitization. He results therefore suggest that option compensation does not affect the overall lending decision (whether or not to approve a mortgage application), however, it does affect how the bank transfer risk.

To show the robustness of the results, we also conduct additional tests to show the robustness of our results. First, in previous sections, we classify loan applications with a loan-to-

income ratio greater than 3 as risky loans. The threshold of 3 is derived based on parameters for a 30-year prime mortgage with a fixed interest rate of 6%. To ensure that our results are not sensitive to this threshold choice, we also use annual top quartile of the loan-to-income ratio as the cutoff and classify loan applications with the ratio greater than the median as risky loans. Alternatively, we avoid choosing a risky loan classification threshold by replacing the variable *Risk* in Equation (1) with the continuous variable loan-to-income ratio (*LTI*) as in Duchin and Sosyura (2014). Specifically, our difference-in-differences estimate is then the coefficient on $LTI_{it} \times Post_t \times Treat_j$. By doing this, we examine how the marginal effect of FAS 123R on loan approval rate varies with borrowers' risk for treated banks relative to control banks. Untabulated results show we obtain similar outcomes as the baseline results.

7. Conclusions

Bank managers' excessive risk-taking incentives, in particular, banks' aggressive mortgage lending practices, arising from their equity compensation, have been blamed for the onset of the financial crises in 2007. Consequently, financial institutions' incentive compensation practices have been under special scrutiny by regulators to ensure that compensation plans do not encourage such excessive risk-taking in the future. Despite the widely held belief that management option compensations contribute to mortgage market risk, little empirical evidence exists linking mortgage lending and management option compensations in financial institutions. To the best of our knowledge, our paper is the first to do so.

In this paper, we use the implementation of FAS 123R, which mandates firms to expense option grants, as a natural experiment to identify the causal effect of management compensation on risk-taking behavior in mortgage originations. We identify two groups of banks that either did not grant options to their executives or granted options but elected to voluntarily expense option

compensation as control groups. As such, those two groups of banks are not affected by FAS 123R. We use those banks as control banks and the remaining banks as treated banks. We find that banks that are affected by FAS 123R, i.e., banks that had options but did not expense the options before FAS 123R, became more likely to transfer risky mortgage off their balance sheets by selling those mortgages for securitization, however, they are not necessarily more likely to reject risky mortgage applications. We also find that the effect is more pronounced for large banks, which is consistent with the argument that government implicit guarantee for too-big-to-fail banks encourages banks to take excessive risk in the mortgage market.

Appendix: Variable definitions

Variable	Definition
Approval	A dummy variable, equal to 1 if the loan is approved, and 0 otherwise
Loan Amount	The amount requested by the application
Income	The annual gross income of the applicant
LTI	The loan-to-income ratio of the mortgage application
Risk	A dummy variable, equal to 1 if *LTI* is greater than 3, and 0 otherwise
Securitized	A dummy variable, equal to 1 if the loan is sold to third parties by the end of the year
Male	A dummy variable, equal to 1 if the applicant is a male, and 0 otherwise
White	A dummy variable, equal to 1 if the applicant is white, and 0 otherwise
Black	A dummy variable, equal to 1 if the applicant is black, and 0 otherwise
Asian	A dummy variable, equal to 1 if the applicant is Asian, and 0 otherwise
Hispanic	A dummy variable, equal to 1 if the applicant is Hispanic, and 0 otherwise
Bank Size	The natural logarithm of bank total assets
Capital	Bank total capital divided by risk-weighted assets
Liquidity	Bank cash and investment securities divided by total assets
ROA	Bank operating income divided by total assets
Deposits	Bank total deposits divided by total assets
Chargeoff	Bank total chargeoff divided by total assets
RWA	Bank risk-weighted assets divided by total assets
Option Numbers	Total number of stock options granted to CEOs, in thousands
Option Values	Fair value of options granted to CEOs as reported by a firm, in thousand dollars
Option Vega	Changes in the value of total stock options to a one percent change in stock return volatility as calculated in Hayes et al. (2012)
Equity Delta	Change in the value of total equity compensation to a one percentage change in stock price as calculated in Hayes et al. (2012)

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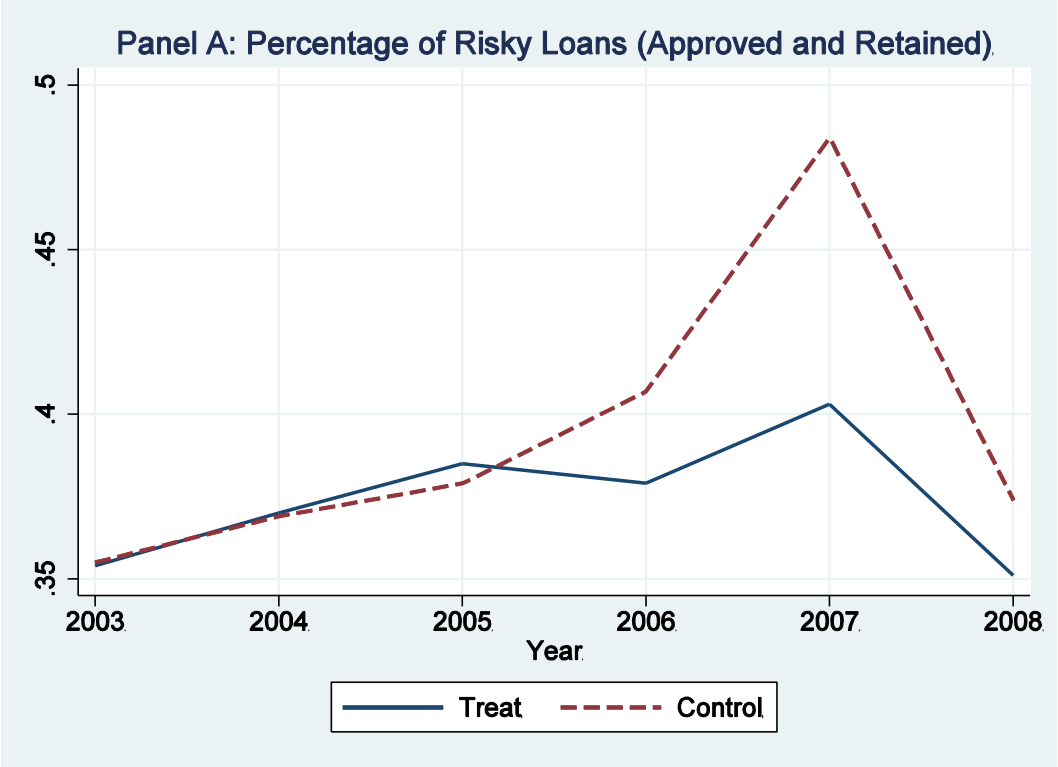
Figure 1: Bank mortgage originations around FAS 123R



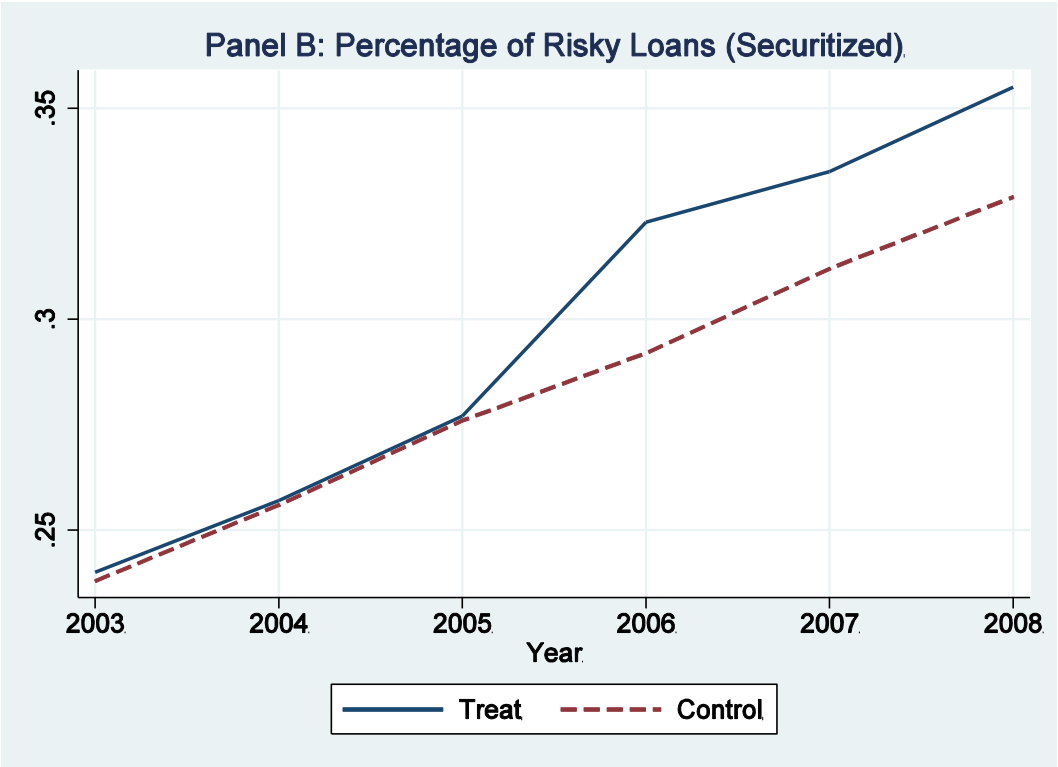


Table 1: Summary statistics

This table presents summary statistics for variables used in the analyses. The unit of analysis is the mortgage loan application obtained from HMDA Loan Application Registry from 2003 to 2008 that are made to 91 unique banks. Data on bank characteristics is obtained from the Call Report.

	N	Mean	S.D.	P25	Median	P75
Loan-to-Income	3,499,591	2.719	1.189	1.885	2.593	3.407
Loan Amount	3,499,591	256.371	241.538	125.000	196.000	315.000
Income	3,499,591	110.714	177.410	50.000	78.000	122.000
Male	3,499,591	0.659	0.474	0.000	1.000	1.000
Hispanic	3,499,591	0.121	0.326	0.000	0.000	0.000
Asian	3,499,591	0.072	0.258	0.000	0.000	0.000
White	3,499,591	0.750	0.433	1.000	1.000	1.000
Black	3,499,591	0.071	0.256	0.000	0.000	0.000
Bank Size	3,499,591	19.560	1.341	18.750	19.873	20.814
Capital	3,499,591	0.132	0.028	0.115	0.122	0.155
Liquidity	3,499,591	0.061	0.027	0.051	0.057	0.072
ROA	3,499,591	0.014	0.003	0.013	0.015	0.016
Deposits	3,499,591	0.640	0.100	0.558	0.647	0.735
RWA	3,499,591	0.748	0.084	0.707	0.758	0.811
Charge-Off	3,499,591	0.004	0.002	0.003	0.004	0.005

Table 2: Bank characteristics: treatment banks versus control banks

This table presents the mean values in bank characteristics for control and treated banks. Variable definitions are provided in Appendix. The differences in the mean values between treated and control banks are reported and their statistical significance at 10%, 5%, and 1% level are denoted by *, **, and ***, respectively.

	Treated		Control		Treated-Control	
	Mean	S.D.	Mean	S.D.	Difference	t-stats
Bank Size	16.922	1.224	17.382	1.726	-0.460	-1.411
Capital	0.128	0.031	0.132	0.038	-0.003	-0.372
Liquidity	0.049	0.031	0.054	0.039	-0.006	-0.739
ROA	0.012	0.004	0.012	0.004	0.000	-0.254
Chargeoff	0.003	0.002	0.004	0.003	-0.001	-1.540
Deposits	0.704	0.103	0.647	0.088	0.056**	2.387
RWA	0.753	0.128	0.774	0.127	-0.021	-0.687

Table 3: FAS 123R and CEO compensation

This table presents the mean values of CEO compensation in the period prior to and after FAS 123R for control and treated banks. Variable definitions are provided in the Appendix.

	Treated			Control			Treated - Control
	Pre	Post	Post - Pre	Pre	Post	Post - Pre	Post - Pre
Option Values	2,510	1,180	-1,330***	3,381	3,014	-367	-963**
	(327.1)	(220.6)	(407.1)	(586.3)	(625.5)	(856.4)	483.57
Option Values / Total Comp	0.451	0.19	-0.261**	0.313	0.274	-0.039	-0.222***
	(0.031)	(0.017)	(0.036)	(0.049)	(0.036	0.062	0.072
Option Vega	210	132	-78**	492	488	-4.00	-74**
	(25.57)	(25.62)	(36.41)	(76.51)	(187.7)	(117.3)	(37.284)

Table 4: FAS 123R and bank mortgage lending and securitization

This table presents the baseline results estimating the effects of FAS 123R on banks mortgage lending decisions using the multinomial logit model, with Panel A for the coefficient estimates, and Panel B for the average marginal effects. The outcomes are Reject, Approve and Retain, and Securitize. The key independent variable, i.e., the difference-in-difference term is *Treat* × *Post*× *Risk*, in which *Treat* equals one for treated banks, and equals zero otherwise, *Post* equals one if the loan is made after the implementation of FAS 123R and zero otherwise, and *Risk* equals one if the mortgage's loan-to-income ratio is greater than three and zero otherwise. Other control variables are as defined in the Appendix. Standard errors clustered at the bank level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Panel A: Coefficient Estimates

	(1)	(2)	(3)	(4)
	Reject	Securitize	Reject	Securitize
Treat × After	-0.204	0.242	-0.208	0.358
	(0.294)	(0.344)	(0.303)	(0.319)
Treat	-0.138	0.404	-0.152	0.288
	(0.591)	(0.465)	(0.610)	(0.427)
After	1.073***	0.892***	1.264***	1.314**
	(0.366)	(0.243)	(0.455)	(0.601)
Treat × Risk	-0.053	-0.333**	-0.059	-0.243***
	(0.045)	(0.136)	(0.061)	(0.076)
After × Risk	-0.294***	-0.330***	-0.301***	-0.378***
	(0.064)	(0.055)	(0.075)	(0.062)
Treat × After× Risk	0.222***	0.532***	0.232***	0.453***
	(0.072)	(0.172)	(0.060)	(0.134)
LTI	0.121***	-0.123**	0.129***	-0.092**
	(0.023)	(0.050)	(0.033)	(0.045)
Constant	-0.334	0.027	-0.736**	-0.248
	(0.345)	(0.320)	(0.346)	(0.760)
Applicant Characteristics	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes
Bank Characteristics			Yes	Yes
Observations	3,499,591	3,499,591	3,499,591	3,499,591
Pseudo R-squared	0.055	0.055	0.075	0.075

Panel B: Average Marginal Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Approve and			Approve and	
	Reject	Retain	Securitize	Reject	Retain	Securitize
Treat × After	-0.058	-0.019	0.075	-0.062	-0.024	0.086
	(0.059)	(0.050)	(0.081)	(0.064)	(0.053)	(0.075)
Treat	-0.061	-0.045	0.106	-0.063	-0.015	0.088
	(0.119)	(0.063)	(0.137)	(0.120)	(0.062)	(0.137)
After	0.087	-0.188***	0.101	0.105	-0.229***	0.124
	(0.066)	(0.045)	(0.071)	(0.072)	(0.056)	(0.126)
Treat × Risk	0.023	0.048***	-0.071**	0.019	0.039***	-0.058**
	(0.016)	(0.016)	(0.029)	(0.018)	(0.014)	(0.024)
After × Risk	-0.016**	0.063***	-0.047***	-0.017**	0.066***	-0.049***
	(0.007)	(0.011)	(0.012)	(0.007)	(0.011)	(0.010)
Treat × After× Risk	-0.015	-0.085***	0.100**	-0.010	-0.083***	0.093***
	(0.021)	(0.023)	(0.040)	(0.015)	(0.016)	(0.032)
LTI	0.031***	0.009	-0.040***	0.032***	0.002	-0.034***
	(0.004)	(0.008)	(0.011)	(0.004)	(0.008)	(0.010)
Applicant Characteristics		Yes			Yes	
Year Fixed Effects		Yes			Yes	
MSA Fixed Effects		Yes			Yes	
Bank Characteristics					Yes	
Observations		3,499,591			3,499,591	
Pseudo R-squared		0.055			0.075	

Table 5: Placebo tests: The effect of a fiction event

This table presents the average marginal effects with the placebo event in 1999. This test uses the same treated and control firms as in Table 4. The outcomes are Reject, Approve and Retain, and Securitize. The key independent variable, i.e., the difference-in-difference term is *Treat* × *Post*× *Risk*, in which *Treat* equals one for treated banks, and equals zero otherwise, *Post* equals one if the loan is made after the implementation of FAS 123R and zero otherwise, and *Risk* equals one if the mortgage's loan-to-income ratio is greater than three and zero otherwise. Other control variables are as defined in the Appendix. Standard errors clustered at the bank level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		Approve and			Approve and	
	Reject	Retain	Securitize	Reject	Retain	Securitize
Treat × After× Risk	0.003	-0.005	0.002	0.005	0.001	-0.006
	(0.033)	(0.034)	(0.056)	(0.035)	(0.032)	(0.067)
Applicant Characteristics		Yes			Yes	
Year Fixed Effects		Yes			Yes	
MSA Fixed Effects		Yes			Yes	
Bank Characteristics					Yes	
Observations		2,548,975			2,548,975	
Pseudo R-squared		0.035			0.028	

Table 6: FAS 123R and bank mortgage lending and securitization: Comparing the treated with the two control groups separately

This table presents the average marginal effects of FAS 123R on banks mortgage lending and securitization decisions by comparing the treated banks with the two control groups separately. The outcomes are Reject, Approve and Retain, and Securitize. The key independent variable, i.e., the difference-in-difference term is *Treat* × *Post*× *Risk*, in which *Treat* equals one for treated banks, and equals zero otherwise, *Post* equals one if the loan is made after the implementation of FAS 123R and zero otherwise, and *Risk* equals one if the mortgage's loan-to-income ratio is greater than three and zero otherwise. Other control variables are as defined in the Appendix. Standard errors clustered at the bank level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Panel A: First Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Reject	Approve and Retain	Securitize	Reject	Approve and Retain	Securitize
Treat × After× Risk	-0.014	-0.085***	0.099**	-0.011	-0.081***	0.092*
	(0.025)	(0.027)	(0.045)	(0.026)	(0.029)	(0.047)
Applicant Characteristics		Yes			Yes	
Year Fixed Effects		Yes			Yes	
MSA Fixed Effects		Yes			Yes	
Bank Characteristics					Yes	
Observations		3,493,160			3,493,160	
Pseudo R-squared		0.055			0.075	

Panel B: Second Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Reject	Approve and Retain	Securitize	Reject	Approve and Retain	Securitize
Treat × After× Risk	-0.016	-0.090**	0.106**	-0.011	-0.077**	0.096*
	(0.034)	(0.037)	(0.052)	(0.026)	(0.037)	(0.057)
Applicant Characteristics		Yes			Yes	
Year Fixed Effects		Yes			Yes	
MSA Fixed Effects		Yes			Yes	
Bank Characteristics					Yes	
Observations		1,978,815			1,978,815	
Pseudo R-squared		0.0329			0.0531	

Table 7: Matching banks

This table presents the average marginal effects estimates of the effects of FAS 123R on banks mortgage lending and securitization with matched banks. The outcomes are Reject, Approve and Retain, and Securitize. The key independent variable, i.e., the difference-in-difference term is *Treat* × *Post*× *Risk*, in which *Treat* equals one for treated banks, and equals zero otherwise, *Post* equals one if the loan is made after the implementation of FAS 123R and zero otherwise, and *Risk* equals one if the mortgage's loan-to-income ratio is greater than three and zero otherwise. Other control variables are as defined in the Appendix. Standard errors clustered at the bank level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		Approve and			Approve and	
	Reject	Retain	Securitize	Reject	Retain	Securitize
Treat × After× Risk	-0.014	-0.087**	0.101**	-0.009	-0.086**	0.095**
	(0.024)	(0.039)	(0.045)	(0.025)	(0.042)	(0.047)
Applicant Characteristics		Yes			Yes	
Year Fixed Effects		Yes			Yes	
MSA Fixed Effects		Yes			Yes	
Bank Characteristics					Yes	
Observations		2,801,526			2,801,526	
Pseudo R-squared		0.057			0.069	

Table 8: Controlling for heterogeneity in customer characteristics

This table presents the marginal effects estimates of the effects of FAS 123R on banks mortgage lending and securitization with matched applicants. We treat loan applications that share exactly the same observable applicant characteristics as from the same applicant and loan applications that do not have matched pairs are not included in the analysis. The outcomes are Reject, Approve and Retain, and Securitize. The key independent variable, i.e., the difference-in-difference term is *Treat* × *Post*× *Risk*, in which *Treat* equals one for treated banks, and equals zero otherwise, *Post* equals one if the loan is made after the implementation of FAS 123R and zero otherwise, and *Risk* equals one if the mortgage's loan-to-income ratio is greater than three and zero otherwise. Other control variables are as defined in the Appendix. Standard errors clustered at the bank level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		Approve and			Approve and	
	Reject	Retain	Securitize	Reject	Retain	Securitize
Treat × After× Risk	-0.005	-0.099**	0.104**	-0.004	-0.102***	0.106***
	(0.033)	(0.045)	(0.050)	(0.022)	(0.032)	(0.045)
Applicant Characteristics		Yes			Yes	
Year Fixed Effects		Yes			Yes	
MSA Fixed Effects		Yes			Yes	
Bank Characteristics					Yes	
Observations		587,426			587,426	
Pseudo R-squared		0.068			0.079	

Table 9: The effect of bank size

This table presents the results estimating the effects of FAS 123R on banks mortgage lending and securitization decisions on subsamples partitioned on bank size. The outcomes are Reject, Approve and Retain, and Securitize. The key independent variable, i.e., the difference-in-difference term is *Treat* × *Post*× *Risk*, in which *Treat* equals one for treated banks, and equals zero otherwise, *Post* equals one if the loan is made after the implementation of FAS 123R and zero otherwise, and *Risk* equals one if the mortgage's loan-to-income ratio is greater than three and zero otherwise. Other control variables are as defined in the Appendix. Standard errors clustered at the bank level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	Large			Small		
	(1)	(2)	(3)	(4)	(5)	(6)
	Reject	Approve and Retain	Securitize	Reject	Approve and Retain	Securitize
Treat × After× Risk	0.056**	-0.194***	0.0138**	-0.048	-0.018	0.067
	(0.024)	(0.034)	(0.024)	(0.030)	(0.035)	(0.056)
Applicant Characteristics		Yes			Yes	
Year Fixed Effects		Yes			Yes	
MSA Fixed Effects		Yes			Yes	
Bank Characteristics		Yes			Yes	
Observations		1,524,720			1,974,871	
Pseudo R-squared		0.0854			0.051	

Table 10-Binary logit regression results

This table presents the binary logit regression results, with column (1) for all mortgage applications, column 2 for non-securitized mortgages only, and column (3) for approved mortgages only. For columns (1) and (2) the dependent variable is whether a mortgage application is approved, and for column (3) the dependent variable is whether a mortgage is securitized. The key independent variable, i.e., the difference-in-difference term is *Treat* × *Post*× *Risk*, in which *Treat* equals one for treated banks, and equals zero otherwise, *Post* equals one if the loan is made after the implementation of FAS 123R and zero otherwise, and *Risk* equals one if the mortgage's loan-to-income ratio is greater than three and zero otherwise. Other control variables are as defined in the Appendix. Standard errors clustered at the bank level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	(1)	(2)	(3)
	All	Excluding Securitized	Excluding Rejected
Treat × After× Risk	0.012	-0.053***	0.119***
	(0.101)	(0.014)	(0.032)
Applicant Characteristics	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes
Bank Characteristics	Yes	Yes	Yes
Observations	3,499,591	1,761,284	2,726,151
Pseudo R-squared	0.0861	0.055	0.086