

## ADEMU WORKING PAPER SERIES

# Deposit Insurance and Bank Risk-Taking

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### Abstract

This paper aims to assess the effect of deposit insurance on the risk-taking behaviour of banks. As shown in the theoretical literature, deposit insurance may induce moral hazard and incentivize banks to take on more risk. In this paper we provide an experimental setup in which we exploit an increase in the coverage limit of deposit insurance in the U.S. in order to identify the difference in risk taking by banks that were affected and banks that were not. This difference comes from the fact that state chartered savings banks in Massachusetts had unlimited deposit insurance coverage at the time when it was increased for all other banks in the US. Given that all banks in the sample are subject to the same regulatory and supervisory requirements, and that they are similar in other characteristics, we can isolate the effect of such increase in deposit insurance. We find, contrary to the literature, that this increase in deposit insurance did not increase bank risk-taking.

**JEL Codes:** G11, G20, G21, G28

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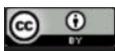
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# 1 Introduction

Deposit insurance has been discussed extensively from a theoretical point of view. Most notably, [Diamond and Dybvig, 1983] showed that deposit insurance can eliminate the risk of bank runs by ruling out the equilibrium where all depositors withdraw their deposits early. However, as shown by [Cooper and Ross, 2002], deposit insurance may bring about some social costs in the presence of moral hazard. If deposits are insured, depositors do not have incentives to monitor banks, which may induce them to choose riskier portfolios in search for a higher return.

The empirical literature is somewhat heterogeneous. Most of the evidence on this topic comes from cross-country studies. For instance, [Demirguc-Kunt, 2002] provide some evidence that deposit insurance increases the likelihood of banking crises. However, as pointed out by [Anginer et al., 2014], this effect was different during the crisis, as they found that deposit insurance decreased bank risk-taking during this period. In another paper, [Ioannidou and Penas, 2010] analyze the effect of deposit insurance introduction in Bolivia by comparing banks before and after its implementation. They use internal loan ratings to assess risk taking and conclude that after the introduction of deposit insurance, banks were more likely to initiate riskier loans.

In essence, there are two major caveats when estimating the effect of deposit insurance on risk-taking. First, using cross-country studies can prove to be challenging. Most developed economies have had deposit insurance for a long time. By regressing bank risk on a dummy variable that indicates countries with and without deposit insurance, these analysis are effectively comparing banks in developed economies with those in less developed ones. One could argue that these groups of countries have very different financial systems and the comparison is not very informative.

Furthermore, even if one restricts the analysis to only developed economies, the direction of causality is unclear. While deposit insurance may induce banks to take on more risk, it is also true that countries with higher risk in the banking sector are more likely to adopt a deposit insurance scheme.

In addition to this, the empirical literature lacks an appropriate control group to compare to the banks affected by deposit insurance. Examining the behavior of the same group of banks before and after the implementation of deposit insurance raises the issue of whether a change in behavior was due to the treatment or to other factors.

Second, there is the issue of how to measure risk taking. Most of the literature makes use of the bank Z-score, a distance-to-default-type measure which is based on balance sheet data. This measure has several shortcomings. The main one is that by using pre existing portfolios, the Z-score is more a measure of realized risk than it is of real time risk-taking choices.

The contribution of this paper is twofold. On the one hand, we provide an experimental setup in which we use a well suited treatment and control group for identification, from which we can draw causal inference. On the other hand, we construct a measure of risk-taking based on new loan issuances, which improves upon the Z-score insofar as it captures the intention of banks to take on more or less risk in real time.

We exploit an increase in the coverage limit of deposit insurance in the US for identification. In the US, all banks have deposit insurance coverage provided by the FDIC (Federal Deposit Insurance Corporation). In addition, state-chartered savings banks in the state of Massachusetts

are provided with *unlimited* coverage by a private insurer. Membership to this unlimited insurance scheme is mandatory for all savings banks chartered in Massachusetts since 1934. The unlimited nature of this coverage is key to our identification.

In order to measure risk-taking, we use mortgage application data from the Home Mortgage Disclosure Act. We estimate the propensity to lend for a given level of borrower risk, which we proxy using loan to income ratios of each loan application.

Contrary to the literature, we find no significant effect of an increase in deposit insurance coverage on bank risk taking. Banks are not more likely to grant a loan for any given loan to income ratio of the borrower after such an increase.

## 2 Data

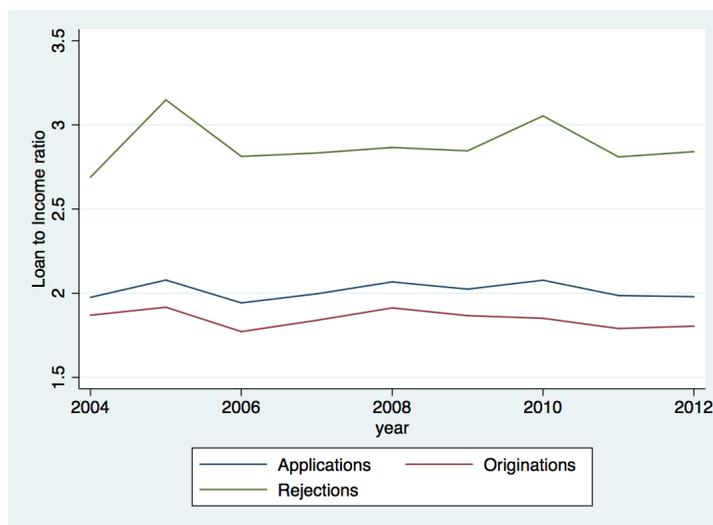
The main source of data for the empirical analysis are the Loan Application Registries. The Home Mortgage Disclosure Act (HMDA) obliges banks above a set threshold of assets to report on mortgage applications. The yearly Loan Application Registries of banks which meet the criteria to report all mortgage loan applications and the properties of the applicant and potential co-applicant (ethnicity, race, gender, income), the loan properties (amount, type, purpose, rate spread for some, occupancy), the properties of the property (type, census tract, etc.), the properties of the census tract (income relative to the relevant Metropolitan Statistical Area, minority population, number of housing units, etc.), and the action taken (origination, denial and its reason, purchase, etc.)<sup>1</sup>.

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<sup>1</sup>The data for 2013-2015 is available on <https://www.ffiec.gov/hmda/hmdaflat.htm>. The data for 1981 to 2012 is available at national archives.

For the purpose of estimating the elasticity of mortgage origination to the loan to income ratio of the applicant we define origination as an application which has been accepted and then either originated or refused by the applicant, a purchase of a loan, or a pre approved request. We define a non-origination as an application denied by the bank or a denied pre request. We ignore all applications withdrawn by the applicants or applications closed for incompleteness. The loan to income ratio is computed as the total loan amount in the application over the total gross annual income an institution relied upon in making the credit decision<sup>2</sup>.

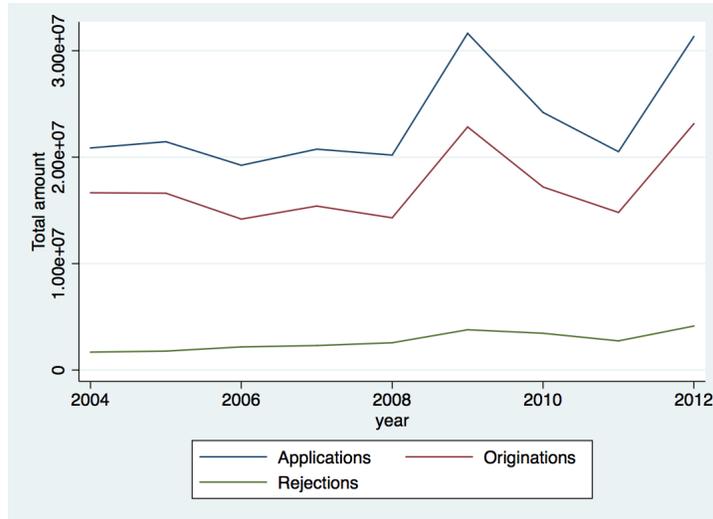
Figure 1: Loan to income ratio for the banks in the sample



Banks in the sample have received approximately 220 billion USD worth of applications in between 2004 and 2012 and have originated approximately 160 billion USD worth of mortgages. LtI seems to discriminate well between the rejected mortgages and the originated mortgages. The average LtI ratio of the accepted applications has been below 2 for the duration of the sample period while the average LtI ratio for the rejected has hovered around 3. Figure 1

<sup>2</sup>Gross annual income is not registered in HMDA due to four possible reasons: (i.) multifamily dwellings, (ii.) income was not registered in the loan purchase documentation, (iii.) loans to bank employees, (iv.) loans to non natural persons. These cases are excluded from the estimation as described in the methodology section

Figure 2: Total amount of loans for the banks in the sample



plots the average LtI for accepted, the rejected and all applications to the banks in the sample within the sample period and the total amounts by the same categories.

In addition, balance sheet data for each bank is taken from the FDIC, and it covers from 2002 to 2016 on a quarterly basis. We use this data to do matching on observables for the treatment and control group, and to compute the distance-to-default risk measure used in the literature, the Z-score.

### 3 Methodology

#### 3.1 Identification

In this paper we exploit an experimental set up from the deposit insurance system in the US. In order to understand how, a few preliminary statements are needed.

In the US, deposit insurance is carried out by the FDIC (Federal Deposit Insurance Corporation), which was set up in 1933. In addition to this Federal Deposit Insurance, since 1934

state-chartered savings banks in the State of Massachusetts are covered by a private deposit insurance company, the DIF<sup>3</sup> (Depositors Insurance Fund), which offers *unlimited* insurance on deposits of member banks. Membership to this insurance scheme is mandatory for all state chartered banks in Massachusetts.

On October 3rd, 2008, the FDIC increased the statutory coverage from \$100,000 per account to \$250,000. This was supposed to be a temporary measure, but the decision became permanent on July 21st, 2010. Since DIF members<sup>4</sup> in Massachusetts always had unlimited coverage, they are unaffected by this change.

From now on, we will refer to savings banks in Massachusetts, which were unaffected by the policy change, as the control group, and savings banks in other states, which were affected, as the treatment group. The treatment is the increase in deposit insurance coverage limit.

Using this framework, we use a difference in differences approach to examine how bank risk increased in the treated banks as a response to higher insurance, in comparison to that of control banks.

## 3.2 Matching

In order to properly assess the effect of the increase in the deposit insurance coverage limit, it is key that banks in the treatment and control group are as similar as possible before treatment. First, note that since only state-chartered savings banks are members of Massachusetts DIF, this

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<sup>3</sup>Not to be confused with the Deposit Insurance Fund, (whose initials are also DIF), which is one of the funds through which the FDIC carries out insurance.

<sup>4</sup>We track DIF membership yearly since 2000

analysis will be limited to savings banks and not commercial banks or other depository institutions. Furthermore, we only use banks that are state-chartered in order to ensure that all banks in the analysis are subject to the same regulation and the same supervision authority.

With this in mind, we do matching on observables before treatment. For each bank in the control group, we find three matching banks in the pool of treated<sup>5</sup>. The matching methodology is nearest neighbor exact matching, and we match on the pre treatment averages of balance sheet size, leverage ratio, capital to asset ratio and deposit to loan ratio.

Table 1 shows the mean of some key variables for the treatment and the control group before the treatment date, together with the p-values for the two sample t-test for the means<sup>6</sup>.

It can be seen that both groups are similar in observables. It is unsurprising that they have similar balance sheet size, capital to asset ratios and leverage ratios, given that the matching is done on those variables. In addition to that, their balance sheet composition is very similar. Most of the loan activity of both groups is allocated to residential loans (i.e. mortgage loans), and most of their deposit base comes from retail deposits.

The total amount of deposits held by both groups is not statistically different from one another, and neither is the share of those deposits which are held in transaction accounts (with higher liquidity) or non transaction accounts (with lower liquidity). This indicates that both groups face a similar liquidity structure in their funding sources, which is important towards their ability for maturity transformation.

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<sup>5</sup>The choice of number of matches is based on the fact that there are 50 banks in the control group. We would like to have more than one match for each in order to have a larger number of observations. Results are robust to other numbers of matches.

<sup>6</sup>The null hypothesis is that the means are equal, hence a p-value higher than 0.05 indicates that the means for both groups are not statistically different at the 95% confidence level

Table 1: Bank characteristics before treatment

<b>Variable</b>	<b>Control</b>	<b>Treated</b>	<b>p-value</b>
Financial Variables			
assets	621685.1	655719.34	.11
car	.1049	.1065	.12
leverage	8.86	8.74	.14
loantodep	.7925	.8459	.00
net interest margin	3.22	3.34	.00
Loan Composition			
residential	.7081	.6703	.00
commercial	.0486	.0499	.44
individual	.0250	.0503	.00
Deposit Composition			
total deposits	485449.57	488909.67	.82
transaction	.1502	.1466	.21
non transaction	.8497	.8533	.21
retail	.8524	.8736	.00
deposits below \$100k	.7958	.8606	.00
deposits by banks	.0017	.0027	.00

This table contains the means of some descriptive variables for the control and the treatment group before the treatment date. Assets are total assets in thousands of dollars. car denotes the capital to asset ratio. Leverage is defined as total debt over total equity. loantodep is total loans over total deposits. Loan composition variables consist of the ratios of residential loans, commercial loans and individual loans to total loans. Deposit composition variables consist of total deposits in thousands of dollars, and the ratios of transaction accounts (demand deposits, etc) and non transaction accounts (savings deposits, time deposits, etc), deposits below the pre-treatment coverage limit of \$100k, and deposits by other banks to total deposits.

Other variables, such as the loan to deposit ratio, the share of residential loans to total loans, net interest margin and share of retail deposits to total deposits are statistically different, but qualitatively similar. One variable that is especially important is the share of total deposits below the pre-treatment limit of \$100,000. We could expect that depositors with larger balances prefer to deposit them in MA banks, due to higher coverage. This could be an issue for the experiment if after the treatment, these depositors decided to move their funds to another bank,

because in this case the treatment would indirectly affect the control group through deposit supply. We address this issue in the Robustness section and show that it is not the case.

One could therefore conclude from this exercise that the balance sheet structure of both groups is similar enough to assume that facing a shock such as the financial crisis, they would be affected in a similar manner.

### **3.3 A Measure of Risk-Taking**

As discussed earlier, a measure of risk taking based on new issuances of loans does not have a deficiency of measuring risk of the portfolio based on past choices. Variation in such a measure results from the choices of banks with regards to the expected performance of loans and borrowers at the point of issuance and not on the ex post performance of loans issued in the past. To this end, we construct a measure of risk-taking on the issuances of new mortgage loans based on the Home Mortgage Disclosure Act (HMDA) dataset, administered by the Federal Financial Institutions Examination Council (FFIEC). It is a yearly dataset on the population of mortgage applications to banks and other mortgage lenders with detailed information on the borrower and loan characteristics. Given the high share of residential loans in the portfolios of state chartered banks, which are at the focus of our analysis, we take the risk of new mortgage lending as representative of risk-taking on the entire portfolio.

To construct a measure of banks inclination to take risk, we estimate a propensity to originate the loan given the loan risk characteristics. Some papers, such as [DellAriccia et al., 2012] and [Ignatowski and Korte, 2014] have shown that loan to income ratios are a good proxy for riskiness of loans. Following this idea, we measure the risk of the loan and the borrower using the loan to income ratio (LTI) measure computed from the HMDA dataset for every loan

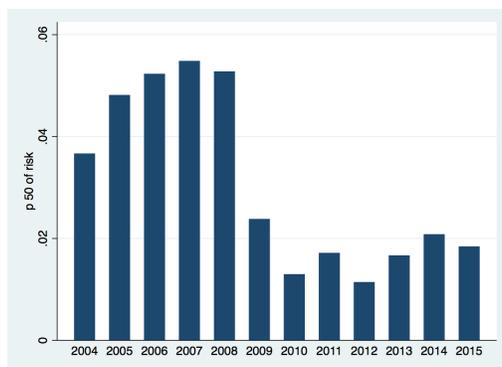
application. We construct a new measure of risk-taking through the following model<sup>7,8</sup>

$$Origin_{t,i,j} = \gamma_t^0 + \gamma_{t,i}^1 LtI_{t,i,j} + \epsilon_{t,i,j} \quad (1)$$

where  $Origin_{t,i,j}$  denotes a binary loan origination variable which takes the value  $Origin_{t,i,j} = 1$  if application in period  $t$  to a bank  $i$  by a borrower  $j$  is accepted and loan is originated, and takes the value  $Origin_{t,i,j} = 0$  if the application is rejected and the loan is not originated.  $\gamma_t^0$  captures the effect of the macroeconomic situation in period  $t$  for all banks, such as market liquidity and regulation. Finally, for every bank  $i$  in every period  $t$  we also obtain an estimate of the risk elasticity  $\gamma_{t,i}^1$  based on loan to income ratio of all applicants  $j$  from 1 to  $J$ , which serves as a measure of risk-taking by banks.

Figure 3 plots the risk elasticities for the banks included in the analysis over the years.

Figure 3: Median propensity to lend by year



### 3.4 Difference in Differences

Using the risk measure estimated above, We use a regression of the form

<sup>7</sup>In order for  $\gamma_t^0$  to capture the macroeconomic conditions affecting the origination choices, we estimate the model for all banks reporting to the HMDA dataset but of course only use the  $\gamma_{t,i}^1$  for banks included in the final regressions. This implies including all the loan applications in the HMDA reporting in the estimations. The number varies between 17 million applications and 40 million application which constrains us to estimating the model as a linear probability model

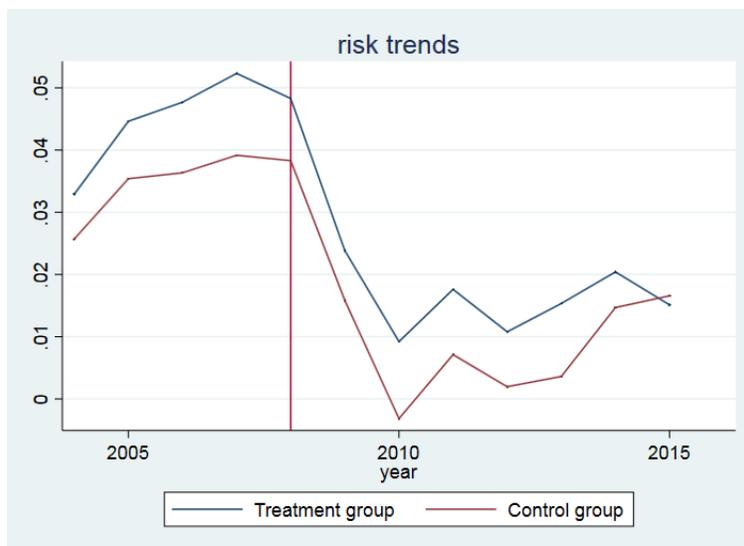
<sup>8</sup>This is joint work with [Ferrari et al., 2018].

$$\gamma_{t,i}^1 = \beta_0 + \beta_1 D_T + \beta_2 D_{after} + \beta_3 D_T D_{after} + \epsilon_{it} \quad (2)$$

where  $D_T$  is a dummy variable that takes value 1 for the treated and 0 otherwise, and  $D_{after}$  is a dummy variable that takes value 1 for the treatment period and 0 otherwise. Hence  $\beta_3$ , the coefficient for the interaction term between the treated dummy and the treatment period dummy, is the treatment effect.

The key assumption in the difference in differences estimator is that, in the absence of treatment, both groups would have followed similar time trends. The following graph shows the risk measure trends for the control and treatment group over the sample period:

Figure 4: Risk measure trends for treatment and control group



### 3.4.1 The Crisis

As pointed out by [Anginer et al., 2014], deposit insurance can induce moral hazard during normal times, but its stabilizing effect may outweigh the moral hazard effect during times of

financial turmoil. In order to test whether the treatment effect is different during the crisis, we add a dummy variable for the crisis period and an interaction term between the crisis period and the treated group to regression 2.

$$\gamma_{t,i}^1 = \beta_0 + \beta_1 D_T + \beta_2 D_{after} + \beta_3 D_T D_{after} \quad (3)$$

$$+ \beta_4 D_{crisis} + \beta_5 D_T D_{crisis} + \epsilon_{it} \quad (4)$$

Here,  $\beta_4$  reflects the effect of the crisis on bank risk, and  $\beta_5$  is the coefficient for the treatment effect of deposit insurance during the crisis.

It is worth pointing out that the timing of the crisis and the that of the policy announcements poses some difficulties. Recall that it was in October 2008 when the FDIC announced a temporary increase in the deposit insurance coverage, which was supposed to last until December 2010. In July 2010, the FDIC announced that this measure would become permanent. Arguably, October 2008 was the peak of the financial crisis, with Lehman Brothers filing for bankruptcy in September of that year. Furthermore, by July 2010 the crisis in the US was coming to an end. The coincidence in time of the policy announcements and the crisis period makes it hard to disentangle whether the different treatment effect during these years is due to the financial crisis, or to whether the measure was implemented temporarily or permanently. We address this issue later on in the robustness section.

## 4 Results

As a first approach to the literature, we run the analysis using the bank Z-score as a measure of risk taking. We will then point out at its limitations and compare the results with the ones obtained using the risk measure based on new loan issuances.

The Z-score is computed as the sum of the expected value of ROA plus the capital to asset ratio, divided by the standard deviation of ROA. As such, it is inversely related to an upper bound of the probability of default for a certain bank. Hence, the higher the z-score, the safer the bank <sup>9</sup>.

Table 2 shows regression results using the log of the Z-score as a dependent variable. Column (1) corresponds to the standard difference in differences regression, while Column (2) includes the crisis specification. The predictions of these regressions are in line with the existing literature. On the first column, the coefficient of interest ( $D_T D_{after}$ ) indicates that there is a significant negative effect of deposit insurance on the Z-score, which implies higher risk-taking since the Z-score is inversely related to the probability of default.

Furthermore, on the second column, the coefficients of interest ( $D_T D_{after}$  and  $D_T D_{crisis}$  respectively) indicate that, while this negative effect of deposit insurance on risk-taking exists during normal times, it appears that during the crisis period the treatment effect is positive, which implies a reduction in risk-taking. This corresponds to the findings of [Anginer et al., 2014].

However, as pointed out before, the Z-score has several shortcomings. As a distance-to-default measure based on balance sheet information, it does not exactly measure risk taking, but realized risk from past decisions. As such, it is hard to assess the treatment effect using this measure, even under an experimental setup like ours.

Observing the data on ex post performance of mortgages in different states in the US from the beginning of the crisis provides an explanation of why a measure of risk taking based on

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<sup>9</sup>See Appendix A for a more detailed description of the Z-score.

Table 2: Results for Z-score

VARIABLES	(1)	(2)
	logzscore	logzscore
$D_T$	-0.0397* (0.0208)	-0.0397* (0.0208)
$D_{after}$	0.0119 (0.0281)	0.213*** (0.0283)
$D_T D_{after}$	-0.173*** (0.0375)	-0.220*** (0.0389)
$D_{crisis}$		-0.844*** (0.0541)
$D_T D_{crisis}$		0.179** (0.0724)
Constant	4.348*** (0.0157)	4.348*** (0.0157)
Observations	8,156	8,156
R-squared	0.011	0.076
Robust standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

existing portfolios provides a source of potential bias. As is evident from figure 5 the average monthly delinquency rate of mortgages in the state of Massachusetts was below the nationwide average monthly delinquency rate. The origination of these mortgages was prior to the beginning of the crisis. Assuming that state chartered banks predominantly issue mortgages within their home state, the loan portfolios of banks chartered outside Massachusetts would underperform compared to those chartered in Massachusetts. A methodology relying on existing portfolios would therefore treat this as an indicator of increased risk-taking.

We now proceed to compare these results to the ones obtained using new loan issuances to measure risk-taking. Table 3 shows the regression results for the difference in differences estimation using the new risk measure as a dependent variable. Again, Column (1) corresponds

Figure 5: Average delinquency rates



to the standard difference in differences regression, while Column (2) includes the crisis specification.

In both cases, the coefficients of interest ( $D_T D_{after}$  and  $D_T D_{crisis}$  respectively) are not statistically significant. It is an important result that we find no significance both for the entire treatment period and also for each sub period (crisis and normal times). This is because one way argue that, according to the findings of [Anginer et al., 2014], the reason of this non significance may be that the treatment effect has opposite sign in each sub period, so that the overall effect is zero. We show by splitting into two sub periods that this is not the case, and that in fact the treatment effect is insignificant all around.

Summing up, the results of our empirical analysis do not confirm the results from the previous studies. Using an experimental setup and a measure of risk-taking that is based on new loans instead of balance sheet data, we find no significant effect of an increase of deposit insurance coverage limit on risk taking by banks. This result is important for policy making insofar as,

Table 3: Results for risk measure based on loan applications

VARIABLES	(1) $\gamma_{t,i}^1$	(2) $\gamma_{t,i}^1$
$D_T$	0.0104*** (0.00304)	0.0104*** (0.00304)
$D_{after}$	-0.0223*** (0.00334)	-0.0256*** (0.00350)
$D_T D_{after}$	-0.00172 (0.00441)	-0.00309 (0.00467)
$D_{crisis}$		0.00813 (0.00574)
$D_T D_{crisis}$		0.00343 (0.00687)
Constant	0.0342*** (0.00205)	0.0342*** (0.00205)
Observations	1,451	1,451
R-squared	0.073	0.082

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

when faced with the trade off between the risk of bank runs or the higher systemic risk induced by moral hazard, the regulator should take into consideration that the moral hazard channel may not be an issue.

It is worth pointing out that the treatment in our experiment is an increase in deposit insurance coverage, hence these results apply to the intensive margin. While it may be true that implementation of deposit insurance may increase moral hazard, it appears that moral hazard is not a concern when we consider increases in coverage.

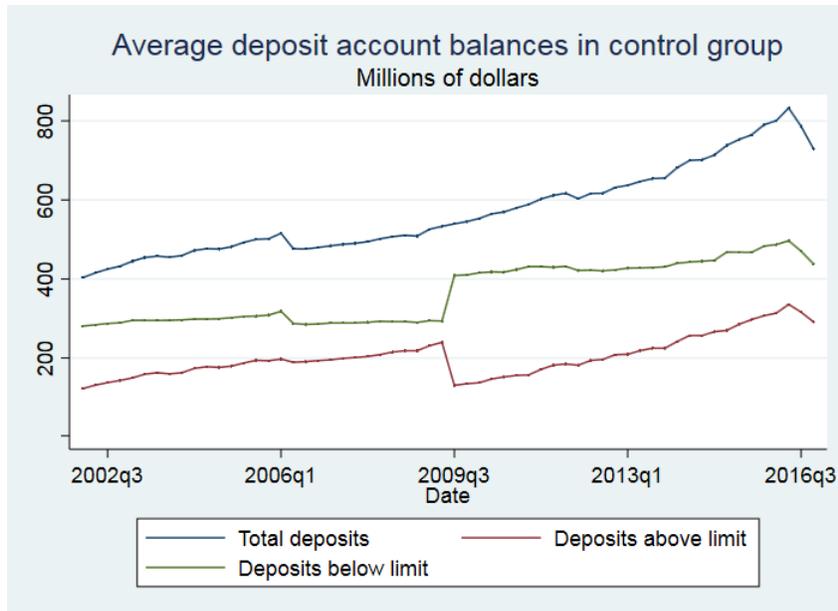
## 5 Robustness

### 5.1 Deposit Supply

We have established a treatment and a control group of banks that are similar in observables. However, we need to make sure that the control group was actually unaffected by the treatment. The control group in this case are banks whose deposit insurance is unlimited, so that they are unaffected by the country-wide increase in the national coverage limit, which went from \$100,000 to \$250,000. One could argue that although they are not directly affected by the treatment, they could be indirectly affected. For instance, imagine a depositor from New York with a deposit balance of \$150,000. Before the coverage limit was raised, she may have chosen to deposit her funds in one of the banks in Massachusetts with unlimited coverage, so as to get insurance for the extra \$50,000. When the coverage limit was increased, she may then choose to transfer her deposit to a bank in New York, which is more convenient for her. In this case, the bank in Massachusetts would be affected by the treatment through the deposit supply.

Figure 6 shows the average total balance of deposits for each bank, and how they are split between those which lie above the insurance limit and those below. The two kinks in the amount of deposits above and below the limit correspond to the change in policy. Note that with the increase in coverage, deposits between \$100,000 and \$250,000 were above the limit before the change, and are below the limit after, so the amount of deposits below the limit increases merely by accounting. The same logic can be applied to deposits above the limit. However, the total amount of deposits does not deviate from its trend, and thus it can be concluded that the banks in the control group did not experience any deposit flight due to the policy change.

Figure 6: Deposits of control banks



## 5.2 Results for different treatment dates

As stated before, the increase in deposit insurance that happened in 2008 was supposed to be temporary. However, it was made permanent in July 2010. In order to make sure that the absence of a treatment effect is not due to the temporary nature of this measure, we estimate the same model with 2010 as a treatment date.

Furthermore, in order to ensure that the lack of effect is also not due to anticipation effects, we use 2007 as a treatment year, in case some news circulated that the government may increase deposit insurance before it happened.

Table 4 shows that the treatment effect is still insignificant for these treatment dates. Again, we find no evidence of a moral hazard problem that would result in higher risk-taking by banks after an increase in deposit insurance coverage.

Table 4: Different treatment dates

VARIABLES	(1) Treatment in 2007	(2) Treatment in 2010
$D_T$	0.00936*** (0.00334)	0.00989*** (0.00270)
$D_{after}$	-0.0175*** (0.00332)	-0.0254*** (0.00375)
$D_T D_{after}$	-4.93e-05 (0.00448)	-0.00160 (0.00466)
Constant	0.0325*** (0.00227)	0.0318*** (0.00187)
Observations	1,451	1,451
R-squared	0.040	0.099

Robust standard errors in parentheses

\*\*\* p&lt;0.01, \*\* p&lt;0.05, \* p&lt;0.1

## 6 Conclusion

This paper addresses the question of whether deposit insurance increases bank risk-taking. This question has been previously discussed in the literature. However, there exist limitations in the identification strategies and measures of risk-taking.

Our contribution is two-fold. First, we exploit an experimental setup through which we can identify this effect. Second, we provide a measure of risk-taking based on new loan issuances, which improves upon the measure of risk that is predominantly used in the literature, the Z-score. The Z-score is based on balance sheet data, which implies that the risk captured by this measure is due to past decisions. By using loan application data we can pin down the risk choices of the bank at the time of origination.

Through a difference in differences approach we find no effect of an increase of deposit

insurance on bank risk-taking. For a given level of risk of loan applications, banks in the treatment group were not more likely to grant a mortgage loan compared to the control group than they were before treatment.

In order to assure that the lack of significance of the treatment effect is robust, we perform several tests. First, we note that the financial crisis is included in the treatment period. [Anginer et al., 2014] find that the effect of deposit insurance on bank risk-taking is different during times of crisis than in normal times. One could argue that the lack of significance is due to the fact that the treatment effect has significant treatment effects of opposite signs during the crisis and otherwise. In order to address this, we split the treatment period in two sub periods, from 2008 to 2010 (the crisis period) and from 2011 to 2015 (normal times). We find no significant treatment effect for either period.

Second, in order to check whether the lack of significance stems from anticipation to the treatment, we perform the analysis with 2007 as a treatment date. Results remain insignificant.

Third, as noted before, the increase in deposit insurance that was implemented in 2008 was supposed to be temporary, and it only became permanent in 2010. In order to test whether the lack of significance is due to the temporary nature of the initial treatment, we also perform the analysis with 2010 as a treatment date. Also in this case the treatment effect is not significant.

We can conclude therefore that given the experimental nature of our setup and the ability of the risk measure to capture risk taking in real time, in addition to the robustness tests, the insignificance of the treatment effect is a robust result. This result is especially important for policy making. In the case of deposit insurance, a regulator is faced with the trade off between

eliminating the risk of bank runs and increasing systemic risk due to a moral hazard problem.  
This paper points at the absence of the latter on the intensive margin of the policy.

## Appendix A: The Z-score

The z-score is a measure that reflects a bank's probability of insolvency. As originally argued by [Hannan and Hanweck, 1988], a bank is considered to be insolvent when its losses exceed its equity. Using common balance sheet items, one could say that a bank is insolvent when the sum of its capital to asset ratio plus its return on assets is negative, i.e.  $car + roa \leq 0$ . Formally,

$$\text{Probability of insolvency} = P(roa \leq -car) \quad (5)$$

If we consider  $roa$  as a random variable with mean-variance  $(\mu_{roa}, \sigma_{roa}^2)$ , we can apply Chebyshev's inequality to assess an upper bound on the probability of insolvency.

Recall that Chebychev's inequality states that, for any symmetric distribution, the probability that the outcome of a random variable  $X$  falls more than  $k$  standard deviations away from its mean is at most  $\frac{1}{k^2}$ . Formally,

$$P(|X - \mu| > k\sigma) \leq \frac{1}{k^2} \quad (6)$$

It is easy to see that the number of standard deviations between the expected value of  $roa$  and those values in the distribution of  $roa$  that would imply  $roa \leq -car$  is

$$zscore \equiv z = \frac{E(roa) + car}{\sigma_{roa}} \quad (7)$$

So that the probability of insolvency is at most

$$p \leq \frac{1}{2z^2} \quad (8)$$

where the  $1/2$  comes from the fact that these values only happen in one tail of the distribution.

So the  $z$ -score is inversely related to an upper bound of the probability of default for a certain bank. Hence, the higher the  $z$ -score, the safer the bank.

Since the distribution of the  $z$ -score is skewed, a standard practice in the literature is to use its logarithm instead. Thus, we use  $\log(z)$ , computed over a 2-year rolling window.

## References and Notes

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