

# Wishful Thinking or Effective Threat?

## Tightening Bank Resolution Regimes and Bank Risk-Taking\*

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June 2013

### Abstract

We propose a framework for testing the effects of changes in bank resolution regimes on bank behavior, particularly on a variety of risk- and business-model measures. By exploiting the differential relevance of recent changes in U.S. bank resolution laws (i.e., the introduction of the Orderly Liquidation Authority, (OLA)) for different types of banks, we are able to simulate a quasi-natural experiment to test otherwise endogenous effects in a difference-in-difference framework. To the best of our knowledge, this identification strategy is unique in its application to regulatory changes in bank resolution. To test our hypotheses, we assemble a three-level dataset: holding aggregates (including stock market data), bank-level data, and loan-level data. We find that banks that are more affected by the introduction of the OLA significantly decrease their overall risk-taking and shift their business model and new loan origination towards lower risk, indicating the overall effectiveness of the regime change. This effect, however, does not hold for the largest and most systemically important banks, indicating that the application of the OLA does not represent a credible threat to these institutions, leaving the too-big-to-fail problem unresolved. Finally, we find no evidence of gambling between the announcement and the enactment of the OLA, presumably because the legislation was passed relatively quickly. Our results contribute to the emerging literature evaluating the implications of new regulatory policies and yield relevant conclusions for the design of bank resolution law, e.g., in the context of the European Banking Union.

JEL classification: G21, G28, G33

Keywords: bank resolution, bank insolvency, Orderly Liquidation Authority, FDIC, bank behavior, risk-taking

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\*This paper has been prepared by the authors under the Lamfalussy Fellowship Program sponsored by the ECB. Any views expressed are only those of the authors and do not necessarily represent the views of the ECB or the Eurosystem. The authors wish to thank Reint Gropp, Sascha Steffen and Mark Wahrenburg and participants at the 3rd International FEBS/LabEx Conference in Paris, the 15th INFER Annual Conference in Orléans, the Bank Resolution Mechanisms Conference in Dublin, the 10th Euroframe Conference on Economic Policy Issues in the European Union in Warsaw, and the Finance Brown Bag Seminar at the Goethe University Frankfurt for their suggestions and helpful comments.

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## *Prelude*

*On June 30, 2010, bank resolution law - under which the Federal Deposit Insurance Corporation (FDIC) was able to close any insured depository institution in the U.S. - was applicable to approximately 10.9% of the Goldman Sachs Group's subsidiaries. At the end of the next reporting quarter, the FDIC had been enabled by the U.S. Congress to eventually resolve 100% of the Goldman Sachs Group or any financial holding company according to an extension to bank insolvency law termed the Orderly Liquidation Authority (OLA) that had been introduced as part of the Dodd-Frank Act (DFA).*

*At the time, the Financial Times applauded that "the Dodd-Frank bill makes important strides in ending government guarantees [...] and disincentivising risky behaviour. [...] In place of government bail-outs (like AIG) and painful bankruptcies (like Lehman Brothers) comes a new 'Orderly Liquidation Authority'".<sup>1</sup> The Economist concluded that "the most important provision [of Dodd-Frank] is the resolution authority under which federal regulators can seize any financial company [...]. This is an improvement on the status quo."<sup>2</sup> Have these expectations come true? Did such a dramatic change in bank resolution powers influence bank behavior, risk-taking, and business model choices? Do banks operate differently when the threat of being resolved and liquidated by the regulator becomes more realistic in legal, operational, and financial terms?*

## **1 Introduction**

When governments were confronted with seriously distressed banks during the global financial crisis of 2008 and the subsequent European sovereign debt crisis, existing resolution tools proved mostly inappropriate, either because they did not take into account distinctive features of banks or authorities lacked to some extent empowerment, financial resources, and cross-border cooperation to effectively resolve failed banks. A comparison of the failure resolution of Lehman Brothers and Washington Mutual in September 2008 illustrates the importance of effective and appropriate bank resolution mechanisms.<sup>3</sup> Following these recent crisis experiences, bank regulators and legislators have discussed and brought into force significant changes to bank resolution regimes<sup>4</sup> in an effort to improve bank failure resolution and ultimately to prevent future crises (e.g., Dodd-Frank Act in 2010, German Bank Restructuring Act in 2011, and Financial Stability Board in 2011).

Effective and enforceable bank resolution mechanisms not only are of vital importance in dealing with failing banks and minimizing costs associated with bank failures but also can have a disciplining effect and thus reduce the probability of bank failure ex ante. Bagehot (1873) already noted the moral hazard effect and excessive risk-taking induced by banks' expectation for bailout. Although various rationales for bailout policies can be formulated (e.g., Acharya and Yorulmazer (2007); Diamond and Dybvig (1983); Diamond and Rajan (2005)), several recent studies provide empirical evidence regarding the moral hazard effect of bailout (expectations) on risk-taking, e.g., Black and Hazelwood (2012); Dam and Koetter (2012); Duchin and Sosyura (2012). Conversely,

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<sup>1</sup>See *Financial Times*, July 12, 2010.

<sup>2</sup>See *The Economist*, July 3, 2010.

<sup>3</sup>When Lehman Brothers filed for Chapter 11 bankruptcy protection on September 15, 2008, the bankruptcy filing constituted a default action in derivative contracts, leading to the massive terminations of derivative positions. Because Lehman Brothers was not allowed to provide liquidity to its subsidiaries, its foreign legal entities also entered bankruptcy proceedings. At the time of Lehman Brothers' failure, Washington Mutual experienced a bank run and was put into Federal Deposit Insurance Corporation (FDIC) receivership by its regulator, the Office of Thrift Supervision (OTS), on September 25, 2008. The FDIC sold Washington Mutual's assets, deposit liabilities and secured debt immediately to JPMorgan Chase; the remaining holding company filed for bankruptcy protection the next day. Although Washington Mutual's business had been materially different from Lehman Brothers' business, its banking business continued to operate without major interruptions, unlike the failure of Lehman Brothers. FDIC (2011) provides an extensive discussion of the differences between Lehman Brothers' bankruptcy under Chapter 11 and a hypothetical resolution under a special bank resolution regime, i.e., the Orderly Liquidation Authority.

<sup>4</sup>We interpret the term 'bank resolution regime' with a wide meaning, referring not only to the actual legal provisions but also to the (financial or operational) empowerment of resolution authorities. In addition, with regard to affected institutions, we refer not only to banks in their form as insured deposit-taking intermediaries but also to financial institutions with bank features in general (e.g., financial or bank holding companies).

when bailout guarantees cease to be implicit through a credible and enforceable improvement in bank resolution regimes, we expect banks to change their behavior towards less risk-taking and a lower probability of distress. This hypothesis is proposed in a recent model by DeYoung et al. (2013), which suggests that a credible improvement in resolution regimes can increase overall bank discipline. This discipling effect follows from a clear economic rationale. When depositors and creditors cease to believe that the regulator will have to bail out the bank due to insufficient resolution technology, they have more incentives for monitoring and discipling. Likewise, equity holders and bank management that fear losing their investment or their positions in case of resolution both have incentives to avoid failure when the resolution thread becomes more credible.

The introduction of the Orderly Liquidation Authority provides an ideal setup to study this discipling effect on bank behavior. The OLA, which was established through the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 (DFA), authorizes the FDIC to seize control and liquidate any financial institution in distress through its administrative resolution regime. Before the DFA enactment, the FDIC's resolution authority only comprised insured depository institutions. With the OLA, the FDIC's authority has been extended to institutions that were previously exempted from any specific bank resolution regime, namely, bank holding companies (BHCs) and non-bank financial companies. In this paper, we distinguish between BHCs with large nonbank financial asset holdings and BHCs with mainly depository bank holdings and independent banks. By exploiting the differential relevance of the OLA for these groups, we are able to simulate a quasi-natural experiment that allows us to test otherwise endogenous effects in a difference-in-difference framework.

We address a series of important and novel questions in this paper. Do banks change their behavior when bailout expectations vanish and the threat of being resolved in case of failure becomes more realistic? More precisely, is the OLA a credible and effective improvement to the resolution regime that leads to a reduction in the return volatility, asset risk, and default probability of affected institutions? Is the reduction in risk also perceived by market participants? Do banks adjust their business models following the OLA, e.g., with regard to their securities investments, trading activities, or funding structure? Is there a change in risk-taking regarding new business, or more specifically, do banks approve and originate less risky mortgage loans? Is the improvement in the resolution regime effective for all banks, and is the resolution threat credible and effective even for banks that are deemed 'too-big-to-fail'? Finally, can we observe a reverse effect between the announcement and the enactment of the resolution policy change, which would correspond to theories on gambling (compare, e.g., Fischer et al. (2012); Murdock et al. (2000))?

These questions are addressed using a three-level dataset: holding aggregates (including stock market data), bank-level data, and loan-level data. We find that banks that are more affected by the introduction of the Orderly Liquidation Authority significantly decrease their overall risk-taking after the OLA becomes effective relative to the control group of non-affected banks. More precisely, our results suggest an economically considerable impact: Affected banks increase their z-score, for example, by around 11% on average, while non-affected banks change by less than 1%. This risk reduction for affected banks after the introduction of the OLA is also perceived by market participants as reflected in lower stock return volatility. On a more detailed level, we find that affected banks shift their business model and new loan origination towards lower risk. Our results indicate the overall effectiveness of the regime change, which can indeed be interpreted as an improvement in available resolution technology. However, we find that bank size moderates the credibility of the resolution thread to financial institutions and the overall effect does not hold for the largest and most systemically important institutions. Hence, even the introduction of the OLA appears to leave the 'too-big-to-fail' problem unresolved, at least for the largest banks. Finally, we find no evidence of gambling around the announcement and the enactment of the OLA, presumably because the legislation was passed and enacted relatively quickly.

We focus our analysis on the U.S. because of the unique identification opportunity and the availability of data, but our results have wider implications. The findings not only are of concern

in evaluating the effectiveness of resolution policy change in the U.S. but also can contribute to regulatory discussions in the context of an EU-wide joint bank recovery and resolution policy framework that has been proposed as part of the planned European Banking Union (European Commission, 2012).

Our paper contributes to the recent literature on the effects of regulatory actions on bank behavior, particularly risk-taking, e.g., Berger et al. (2012); Black and Hazelwood (2012); Dam and Koetter (2012); Duchin and Sosyura (2012). Whereas these papers focus primarily on the effects of government bailout policies, we investigate the effects of an ex ante disciplining regulatory approach. Although an economic rationale for such disciplining resolution policies has previously been modeled (Acharya, 2009; Acharya and Yorulmazer, 2008; Perotti and Suarez, 2002), empirical evidence is limited with regard to the (non-)application of resolution rules by regulators (Brown and Dinç, 2011; Kasa and Spiegel, 2008; Korte, 2013). One vital implication of resolution regimes, however, has thus far mostly been unevaluated: the effects of their tightening on bank behavior. Therefore, this paper provides an empirical test of the credibility and effectiveness of changes in resolution regimes with regard to their implications for bank behavior. As a methodological contribution, we propose an identification setup that is, to the best of our knowledge, novel to testing the effects of changes in resolution regimes.

The remainder of this paper is organized as follows. Section 2 provides an overview of the related theoretical literature and the core findings of existing empirical research. Our key hypotheses are proposed against this background. In Section 3, we introduce our identification strategy and present initial indicative evidence. Our full model and dataset are described in Section 4. Section 5 presents the results of the analysis and several extensions, complemented with robustness tests. Section 6 concludes and provides policy implications.

## 2 Background, related literature and key hypotheses

### 2.1 How regulation drives bank risk-taking

Financial economics literature has identified several determinants for bank risk-taking, among them the degree of competition, the degree of information transparency in bank risks, and ownership structure as well as incentives created by bank regulation and safety nets. In this section, we revisit theoretical and empirical literature to investigate how regulation – and particularly the resolution regime – interacts with bank risk-taking.

In general, the literature has focused primarily on the following four main forms of bank regulation: deposit insurance, capital regulation, restrictions on bank activities, and the resolution of banks. Deposit insurance schemes are often described as safety nets against bank runs. However, deposit insurance at a fixed rate (independent of the risk of banks' assets) creates a moral hazard problem because banks can borrow funds inexpensively through insured deposits and invest them in risky assets (Kareken and Wallace, 1978; Merton, 1977). Moreover, insured depositors have little incentive to monitor the bank.<sup>5</sup> This moral hazard problem can be mitigated by making the deposit insurance explicit and leaving some creditors uninsured (Calomiris, 1999; Gropp and Vesala, 2004). Other design features of deposit insurance, such as funding, premium structure, or membership requirements, can also alleviate the moral hazard problem (Barth et al., 2004).

The purpose of capital regulations is to reduce banks' – more precisely bank owners' – risk-taking incentives by forcing banks to leave a portion of their capital at risk as a buffer for future losses. However, a simple capital-to-asset ratio provides incentives to shift to riskier asset portfolios, thus increasing risk-taking behavior (Koehn and Santomero, 1980). A risk-based capital ratio that accounts for asset quality can reduce this asset-substitution problem (Kim and Santomero, 1988;

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<sup>5</sup>Empirical cross-country studies strongly confirm the moral hazard incentive of deposit insurance. Demirgüç-Kunt and Detragiache (2002) show that the existence of deposit insurance increases the probability of banking crises and that this effect is even stronger when the deposit insurance provides more coverage. Demirgüç-Kunt and Huizinga (2004) provide evidence regarding the adverse effect of explicit deposit insurance on market discipline.

Repullo, 2004). However, several further theories suggest the negative effects of capital regulation on bank behavior.<sup>6</sup>

Similar to capital regulation, restrictions on bank activities also aim at more prudent risk behavior by restraining banks from engaging in other risky businesses outside their original activities (Boyd et al., 1998). Empirical studies provide mixed evidence regarding the risk-mitigating effect of activity restrictions (e.g., Barth et al. (2004)).

The resolution of distressed banks is likely the most intricate regulatory area regarding risk-taking incentives. Overall, there are two (opposing) regulatory approaches to handling a distressed bank: bailing out the bank to preserve it as a going concern and resolving the bank through either acquisition by another financial institution (i.e., purchase and assumption) or straightforward closure and liquidation. One line of theory predicts that the expectation of being bailed out increases banks' moral hazard because creditors anticipate loss protection in case of bank failure and have little incentive to monitor the bank (or to adjust risk premiums as indicated in Sironi (2003) and Gropp et al. (2006)). A different theoretical approach suggests that bailout guarantees can increase charter values (i.e., through lower funding costs) and hence decrease incentives for excessive risk-taking because banks fear losing these charter values (Keeley, 1990). Connecting both theories, Cordella and Yeyati (2003) and Hakenes and Schnabel (2010) develop models in which the positive charter value effect can actually outweigh the negative moral hazard effect and thus lead to more prudent risk-taking behavior of banks protected through bailout guarantees. However, these models depend on specific economic circumstances, banking sector characteristics and/or bailout policy designs. Empirical evidence tends to support the view that bailout policies increase rather than decrease bank risk-taking and moral hazard in the long run.<sup>7</sup>

A credible resolution threat of closing or selling banks in case of failure should decrease excessive risk-taking incentives *ex ante*. However, theoretical models predict certain caveats. According to Davies and McManus (1991), the effect of the closure threat on bank risk-taking depends on the bank's 'healthiness' (i.e., capital base) and the regulator's closure rule (i.e., specifying closure at a certain capital level). Mailath and Mester (1994) model a time-inconsistency problem in which the regulator's bank closure decisions interact with banks' asset choices, leaving the regulator unable to credibly commit to closure policies. Apart from *ex ante* incentives, closing or selling banks in case of failure can also affect the *ex post* incentives of surviving banks. Perotti and Suarez (2002) consider a model in which the acquisition of failed banks enhances the charter values of surviving banks (i.e., through greater market concentration) and thus increases surviving banks' incentives for prudent risk behavior. Another conceivable implication for bank behavior could be 'gambling for resurrection'. As theoretically shown in Murdock et al. (2000), banks' incentives to gamble increase when banks lose their charter values. The withdrawal of an (implicit) bailout guarantee because of the introduction of a credible resolution threat can imply higher funding cost and thus a loss in charter values. Hence, banks might begin to gamble in a reaction to a change in resolution policy.

Taken together, the existing literature proposes, models, and evaluates several effects of bank failure resolution (bailout or closure) on bank behavior. To the best of our knowledge, however, there has not been any study thus far that empirically investigates the effects of tightening resolution regimes on bank risk-taking.

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<sup>6</sup>Capital regulations might actually increase bank risk-taking (Besanko and Kanatas, 1996; Blum, 1999; Murdock et al., 2000) and decrease lending activity (Thakor, 1996). Moreover, the recent financial crisis revealed the shortcomings of risk-based capital regulation, which scored well neither on predicting failure (Berger and Bouwman, 2012; Blundell-Wignall and Atkinson, 2010) nor on preventing regulatory arbitrage. Rather, when the risk of certain assets is not properly estimated by a regulator, banks have strong incentives to acquire and hoard these assets, thus increasing systemic risk (Acharya et al., 2013).

<sup>7</sup>Black and Hazelwood (2012) and Duchin and Sosyura (2012) provide evidence that (at least large) TARP-funded U.S. banks increased risk-taking after the capital injection. Dam and Koetter (2012) exploit a dataset on capital injections in Germany and find that bailout expectations (through observed capital injections) increase risk-taking in the entire banking sector (measured as the probability of default). However, using the same dataset, Berger et al. (2012) show that banks receiving capital injections decrease risk-taking (measured as the ratio of risk-weighted assets to total assets). The results in Gropp et al. (2011) are also mixed, finding no evidence of increased risk-taking by banks protected by bailout guarantees.

## 2.2 A theoretical model of bank closure

In the theoretical literature, bank resolution regimes have attracted increasing interest in recent years. One of the most comprehensive theoretical models of the interaction between resolution law, its credibility and application, and bank behavior was recently offered by DeYoung et al. (2013). Building on the time-inconsistency problem of bank closure decisions formulated by Mailath and Mester (1994) and Acharya and Yorulmazer (2007), the authors model the regulatory closure of a bank as a trade-off between short-term liquidity and long-term discipline. The model assumes banks that are inherently fragile to suffer from moral hazard with regard to excessive risk, complexity, and volatility. Essentially, there are two alternatives for the regulator to deal with this. On the one hand, banks can be disciplined by a strict closure and resolution policy in case of failure. Unfortunately, this discipline only materializes in the long run. On the other hand, whereas available resolution technologies help to establish discipline, they usually suffer from limitations. These limitations, which include slow processes, missing information, or legal limits to available regulatory instruments, might (temporarily) lead to illiquidity in the case of bank closures. This scenario might result in a detrimental impact on the economy as a whole (e.g., Ashcraft (2005)). Hence, despite knowing about the long run benefits of discipline, the regulator also has an intrinsic motivation to prefer bailouts or forbearance over straightforward closure.

DeYoung et al. (2013) model the outcome of this trade-off as being determined by two parameters. The first parameter is the time discount rate of the regulator: the higher the rate, the stronger the regulator's preference for liquidity, i.e., bailout. Effectively, this discount rate proxies for the pressure for immediacy that regulators and economic policy makers are experiencing, e.g., political pressure to preserve liquidity during a crisis.<sup>8</sup> The resolution technology available to the regulator is the second parameter determining the trade-off. The better this technology is, the faster and more efficiently a bank closure can be executed and the more liquidity is preserved. Consequently, under the assumption of equal time discount rate, regulators with better resolution technologies at hand have more incentive to enforce discipline, i.e., closure.

This model provides several testable implications. First, improvements in resolution technology, such as legal changes or the operational empowerment of the regulator, make a regulatory policy preferring discipline (i.e., closure in case of failure) more likely. If the technological improvement is known and credible, banks will act rationally by adjusting their behavior towards more discipline *ex ante*. Hence, an improvement in resolution technology should induce less excessive risk-taking and the adoption of more conservative business models, *ceteris paribus*. Second, this outcome depends on the credibility of the application of the new resolution technology. The new policy instruments will only be effective when complemented by political will, i.e., a low time discount rate that increases the willingness of regulators to accept potential short-term illiquidity following bank resolution for long-term gains in discipline. Using these general implications as our theoretical foundation, we test whether the change in specific resolution technologies is indeed an effective and credible improvement that alters the behavior of affected banks.

## 2.3 Hypotheses on the effects of tightening resolution regimes

Building on the theory of bank resolution and the previous findings discussed above, we yield the following hypotheses and subject them to econometric testing.

**Main hypothesis:** If a change in bank resolution regimes (e.g., in the legal provisions governing bank resolution) indeed represents a credible and effective improvement to bank resolution technology, it will change the behavior of the affected financial institutions towards less risk-taking

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<sup>8</sup>Several empirical studies confirm the tendency for bailout and forbearance in times of macroeconomic or systemic stress. Brown and Dinç (2011) and Kasa and Spiegel (2008), for example, find that regulators are less likely to close a bank if the entire banking system is in a crisis.

and safer business models. We thus expect a decrease in risk measures for affected banks after the change becomes effective.

**Extended hypothesis I:** The above effect might vary with the credibility and the political will to truly resolve failed institutions. Both credibility and political will can be influenced and hence proxied by exogenous variables (e.g., elections, overall state of the economy) or endogenous variables (e.g., characteristics of the bank such as systemic importance that influence the discipline-liquidity trade-off). If the application of the new regime is not credible because of bank-specific characteristics, we expect to find a lower effect or even no effect on the respective banks' risk-taking.

**Extended hypothesis II:** Changes in bank regulation that reduce banks' charter value might lead to gambling, particularly during the period after the public announcement and before legal enactment of the regulation. If the political and legislative procedures concerning the introduction of changes in bank resolution regimes provide opportunities for gambling, we expect to see an increase in risk measures for the affected banks after the announcement and before effective enactment of the change.

### 3 Identification strategy - An application to changes in the U.S. bank resolution regime

Although the existing literature and the theoretical model of DeYoung et al. (2013) provide testable implications of changes in resolution regimes, actual empirical testing is challenging because of the endogenous relation between bank behavior and resolution. To overcome these endogeneity concerns and to test our hypotheses as formulated above, we apply the theory of failed bank resolution to a specific change in the U.S. bank resolution regime, the introduction of the Orderly Liquidation Authority. We argue that the circumstances of the OLA introduction resemble a natural experiment setup that can be exploited using a difference-in-difference model. This section describes the fit of this specific resolution regime change and the identification strategy as follows: (1) by discussing whether the OLA indeed constitutes an improvement in resolution technology (i.e., whether it can indeed be taken as a relevant treatment), (2) by timing the introduction of the OLA (i.e., the treatment effect), and (3) by defining differentially affected financial institutions (i.e., treatment and control group). Finally, we present initial evidence that supports our identification setup and merits the more formal evaluation that is shown in the following sections.

#### 3.1 Identifying the treatment - Is the Orderly Liquidation Authority an improvement in resolution technology?

When the financial crisis occurred in 2008 (and surely before), U.S. bank resolution law suffered from two significant shortcomings. We will argue that the Orderly Liquidation Authority represents a significant technological improvement to these two issues.

First, financial institutions in the U.S. were subject to two different insolvency and resolution regimes. One pillar of bank insolvency legislation was the Federal Deposit Insurance Act (FDIA) that covered all insured depository institutions, particularly commercial banks, thrifts, and savings banks holding a national or state charter. The FDIA stipulates a special resolution regime for these institutions - an administrative insolvency procedure. The existence of this special bank resolution regime stems from the conviction that banks are somewhat distinctive, particularly with regard to insolvency. Marin and Vlahu (2011) provide a detailed analysis of the characteristics of banks that advocate a special resolution regime. The following characteristics are among the most important: (1) the inherent instability of banking and the threat of runs, (2) the particularly

negative externalities of bank failures, and (3) the potential for moral hazard due to deposit insurance schemes or implicit guarantees. Whereas the corporate insolvency law does not cover these aspects explicitly, the FDIA regime takes into account the special role and functioning of financial institutions. The act is designed to allow the timely intervention and resolution of insolvent banks while limiting moral hazard and potentially detrimental effects to liquidity, sound banks, and the real economy. To achieve the goal of a least cost (and least adverse effects) resolution, the special resolution regime deviates significantly from the regular, judicial insolvency procedure with regard to insolvency triggers and initiation conditions, resolution instruments, financing, and possibilities for appeal and review (Bliss and Kaufman, 2006; Marin and Vlahu, 2011). Under these provisions, the FDIC has powers to promptly intervene upon certain initiating conditions, such as critical undercapitalization, without having to wait for the filing of a default event or for a court decision. In this case, the license of the bank can be revoked by its primary regulator, and the FDIC can be determined as the conservator or receiver, ousting management and shareholders, taking over the bank, and ultimately preparing the bank for purchase and assumption by another financial institution or for closure and liquidation. To preserve the liquidity, charter value, and operations of the bank, the FDIC typically intervenes overnight or over the weekend and is able to pay off all insured depositors if needed from the Deposit Insurance Fund previously collected from insured institutions (Bliss and Kaufman, 2006; DeYoung et al., 2013).

Whereas the FDIA covers insured depository institutions under national and state bank charters, the FDIC did not have legal powers for intervention in regard to the failure of bank holding companies, financial holding companies, or other non-bank financial institutions. Instead, the default legal provisions of corporate insolvency law, i.e., the insolvency procedures according to Chapter 7 and Chapter 11 of the U.S. Federal Bankruptcy Code, applied. These procedures typically protect the owners from creditors, take long time periods for resolution, during which funds for depositors and borrowers might not be available, and require a restructuring plan as a precondition before making decisions on larger asset sales (DeYoung et al., 2013). Because the financial holdings and non-bank financial institutions in question, among them several of the institutions that have been identified as systemically important, exhibit similar characteristics to those of banks, as described by Marin and Vlahu (2011), an application of these corporate insolvency procedures might cause severe disruptions.<sup>9</sup> When these institutions were effectively exempted from the special bank resolution regime, the default corporate law was apparently inappropriate to efficiently resolve their insolvency. Hence, this situation was widely considered to be a major deficiency in the resolution regime for financial holdings and non-bank financial firms, which might have even protected these institutions from actual failure by making bailout the only available choice (FDIC, 2011; Marin and Vlahu, 2011).

Moreover, even if the FDIC had been legally empowered to apply its resolution procedure to non-bank financial institutions, there would have been a financial limit as to which institutions could have effectively been taken over. Although the Deposit Insurance Fund contained to a record high USD 52.4 billion at the onset of the financial crisis, the deposits of Bank of America alone were approximately 10 times larger than the fund (albeit not all insured). The sheer order of magnitude of this difference illustrates the second significant issue gripping the resolution technology available to U.S. regulators before 2010: not only incomprehensive legal provisions but also the insufficient financial endowment of the regulator prevented an effective application of bank resolution and made bailout the regulator's preferred choice in the majority of cases for financial holdings and non-bank financial companies.<sup>10</sup>

Recognizing the need for alterations in bank resolution law and for improvements in the operational and financial capabilities of the regulator, U.S. federal legislators passed the Orderly

<sup>9</sup>In fact, several studies examine the inapplicability of corporate insolvency law to financial institutions, e.g., by referring to one of the few bankruptcy cases of financial firms: Lehman Brothers Holding Inc. (FDIC, 2011).

<sup>10</sup>It should be noted that bailout was not preferred for a myriad of smaller banks that were covered by the FDIA and for which the Deposit Insurance Fund proved large enough: between 2008 and 2010, the FDIC resolved a record number of more than 300 banks.

Liquidation Authority as part of a wider financial sector reform package, the Dodd-Frank Act (DFA, Title II). The new provisions stipulated by the OLA can be considered an improvement to resolution technology in several dimensions. First, the provisions extend a special insolvency and resolution regime to financial institutions previously uncovered by bank resolution law. Specifically, the legislation stipulates that any firm determined to be a covered financial company according to Sec. 201 and 203 of the DFA can be placed under an administrative insolvency and resolution procedure. Effectively, this provision covers any financial institution in the U.S..<sup>11</sup> The determination of a financial institution as a covered financial company is made by the Secretary of the Treasury, following the vote of the Federal Reserve Board and the FDIC board and in consultation with the President. This determination initiates the orderly liquidation procedure with only limited judicial appeal *ex ante*.<sup>12</sup> Technically, this procedure is similar to the existing FDIA regime, with the FDIC being appointed as receiver of the financial company. Once under receivership, the FDIC is empowered to close and liquidate the firm, to pursue a purchase and assumption resolution, or to set up a bridge financial institution. These resolution instruments also resemble the FDIA regime insofar as they cause losses to shareholders and unsecured creditors, replace the management, and protect liquidity in a way that is superior to regular insolvency law.

Second, Title II of the DFA sets up a new Orderly Liquidation Fund that also financially enables the FDIC to act as the receiver and to pursue the orderly liquidation of covered financial companies. Although the fund is set up in the Treasury, the FDIC is authorized to borrow from the fund to cover the cost of orderly liquidation and administrative expenses. Moreover, the FDIC is empowered to charge *ex post* risk-based assessments to financial companies<sup>13</sup> to repay the Orderly Liquidation Fund (DFA, Title II, Sec. 210).

The Orderly Liquidation Authority can be interpreted as an improvement to resolution technology (in the sense of DeYoung et al. (2013)) in at least two dimensions. First, we interpret the OLA to be an improvement in terms of legal authorities by alleviating the previous limitation of the FDIC to only place a certain group of financial institutions into a special bank resolution procedure. Rather than focusing only on insured depository institutions, the special resolution regime is now extended to other financial companies. Second, the establishment of the Orderly Liquidation Fund significantly improves the financial and operational capacity of the FDIC to effectively act as a receiver and liquidity guarantor. There is now less reason to prefer bailout over resolution when large financial institutions fail, at least theoretically. These improvements might not establish an optimal and ultimate resolution regime; rather, there is a broad discussion in the literature suggesting changes that might be even more appropriate (Bliss and Kaufman, 2011; Edwards, 2011; Fitzpatrick et al., 2012; Scott et al., 2010; Scott and Taylor, 2012; Zaring, 2010). However, the majority of these commentators (and the leading financial press quoted in the prelude of this paper) agree that the Orderly Liquidation Authority at least represents a theoretical improvement to the pre-existing regime. In fact, DeYoung et al. (2013) describe the OLA as a ‘positive technological shock for U.S. bank regulators’ and add the prediction that (if effective) this will make the resolution of insolvent financial institutions more likely and hence reduce their incentives to choose high risk business strategies.

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<sup>11</sup>The determination as a covered financial company essentially requires three conditions to be fulfilled. First, the firm in question must be a financial company, i.e., a bank holding company, a non-bank financial company supervised by the FED board, or any company predominantly engaged in financial activities. Second, the firm is not an insured depository institution covered by the FDIA regime. Finally, the determination is made provided the existence of all criteria outlined in Sec. 203b, i.e., the firm is in (danger of) default, the resolution according to otherwise applicable legal provisions would have adverse consequences for financial stability, there is no viable private sector alternative, the impact on creditors and shareholders is appropriate, all convertible debt has been ordered to be converted, and the OLA is deemed effective (DFA, Title II, Sec 201, 203).

<sup>12</sup>In fact, the board of the determined covered financial company can ask the Secretary of the Treasury to petition for a formal authorization by the U.S. district court in the District of Columbia. This court can order the authorization after finding that the determination as a covered financial company is not arbitrary and capricious. If the court does not decide within 24 hours, the authorization is automatically granted by the operation of law (DFA, Title II, Sec. 202).

<sup>13</sup>Specifically, Sec. 210 stipulates that the assessments are to be imposed on large non-bank financial institutions, that is, bank holding companies with consolidated assets exceeding USD 50 billion and non-bank financial companies supervised by the FED board.

Hence, we argue that the introduction of the OLA is indeed a significant improvement to resolution technology and use it as the treatment whose effect we will test.

### **3.2 Timing the treatment - When did the treatment take place?**

As with any legislative process, the introduction of the OLA stretched over a significant timespan from the generation of the idea to the passage of the bill and its signing into law by the President. The earliest proposal for legislation regarding an Orderly Liquidation Authority was contained in the financial sector reform package suggested by the Obama administration in June 2009 (Department of the Treasury, 2009). A revised proposal for the Orderly Liquidation Authority was announced as part of the reform package that was later named the Dodd-Frank Act in December 2009. The major legislative process occurred in the following six months in the House of Representatives and the Senate. Finally, the Dodd-Frank Act (and with it the OLA) was passed by the U.S. Congress in July 2010 and was signed into law by President Barack Obama on July 21 with immediate effect. For our purposes, the treatment period can be understood as the first indication when banks were confronted with the likely change of regulation planned by the Obama administration (June 2009) until the actual enactment of the legislation (July 2010).

Because our dataset is constructed from quarterly data, we define all periods before and including the second quarter of 2009 as pre-treatment periods and all periods after and including the third quarter 2010 as post-treatment periods.<sup>14</sup>

### **3.3 Identifying the treatment and control groups - Were financial institutions differentially affected?**

An important pillar of our identification strategy is the differential effect of the OLA on financial institutions that was indicated above. Whereas insured depository institutions were subject to bank resolution law previously, other financial institutions, specifically bank holding companies (BHCs) and non-bank financial companies, were de facto not resolvable in an appropriate manner because of the legal inapplicability of the FDIA and the economic inapplicability of corporate bankruptcy law. Essentially, the introduction of the OLA only affected the latter group by exposing them to a credible threat of resolution for the first time.

However, the actual situation is less clear cut because the majority of holding companies own bank subsidiaries that fall under the FDIA resolution authority.<sup>15</sup> In some cases, the bank subsidiary even comprises 99% of the holding company's assets, with the holding company merely serving as a legal mantle used for accounting, tax, and other purposes. To avoid treating the constructs that have 99% of assets regulated by the FDIA and those that only have 10% in the same manner, we propose an indicator that measures the share of assets of a holding company not subject to the FDIA resolution regulation. In our view, this indicator has the advantage of capturing the essence of our identification idea and is simple to compute. Although we can also use the continuous indicator to build an interaction term, we will start with a pure difference-in-difference setup by defining cutoffs that identify the treatment and control groups. We define all BHCs (and banks belonging to a BHC) that hold more than 30% non-FDIA-regulated assets as particularly 'affected' by the regulatory change, i.e., as the treatment group. Conversely, we define all BHCs (and banks belonging to a BHC) that do not have any assets or have less than 10% non-FDIA-regulated assets as 'not affected', i.e., as the control group. However, because these cutoffs are admittedly arbitrary, we test several alternative cutoffs and use the continuous indicator in our robustness checks.

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<sup>14</sup>Because of data availability and data quality, we must define slightly different pre- and post-treatment periods in the loan level dataset. The following section provides additional details.

<sup>15</sup>As indicated in the prelude, even Goldman Sachs Financial Holding owned subsidiaries (such as Goldman Sachs Bank) that fall under the definition of an insured depository institution and were hence subject to resolution procedures governed by the FDIA.

Selecting the differential exposure to FDIA regulation as the criterion for distinguishing the treatment and control groups enables us to employ a difference-in-difference setup to estimate the effect of OLA on risk-taking. As our key identifying propositions, we assume that (1) the treatment and control groups are developing in parallel (but not necessary at the same level) and that (2) only the treatment affected the treatment and control groups differently (i.e., what we are measuring is actually the treatment effect and not something else). We argue that both are the case and present evidence for (1) in the following sections. Regarding the differential treatment effect (2), we assume that the majority of other changes that occurred simultaneously to the introduction of the OLA concerned banks independently of their share of assets under FDIA regulation. The first argument supporting this assumption is that among several regulatory changes that occurred at that time, the introduction of the OLA is regarded as the most influential change (see, e.g., the quote from *The Economist* in the prelude). Second, although other changes might have been discussed or passed in the context of the Dodd-Frank Act, many of them only became effective at later dates.<sup>16</sup> Third, even if other important changes (in regulations or other aspects of the banking business) became effective at the same time, none of those changes arguably affected banks differentially depending on their share of FDIA-regulated assets. Finally, one might argue that BHCs with large unregulated shares run a very different business model and hence (assuming that this cannot be controlled for by covariates and fixed effects, which we will actually do) experience a differential effect from other regulatory or financial market changes that occurred at the same time. For example, this would be the case for holdings with large investment banking or trading units, which were a particular target of the regulation that was passed at that time. Following this line of reasoning, we select the bank level (in addition to using the level as a robustness check), at which these effects should not be pronounced. Instead, the business models of insured depository banks (the ones that are individual banks or belong to an affected group vs. the ones that belong to a non-affected group) should be far more comparable than the business models of a holding company with large investment banking departments and a holding where depository banking represents 99% of the assets.

Nevertheless, to the extent that parallel changes also might have affected banks' risk-taking proportionally to their non-FDIA-regulated share, we would also detect their effect in our estimates. Although we are convinced that we will not find such effects outside the regulatory reform area, regulatory attention to mostly non-FDIA-regulated institutions admittedly increased with the introduction of the new resolution law. Hence, we should be aware that we are measuring not only the effect of a mere change in the law but also the entire resolution regime, including the credibility, the capability (e.g., the Orderly Liquidation Fund), and the attention of the regulator that this legal change evoked.

### 3.4 Initial evidence - Does it really make a difference?

Is the OLA a technological improvement that is credible and effective? Is there enough political will to use the OLA? Does this new threat invoke a change in bank behavior, particularly for the most affected institutions, i.e., those institutions covered by a special resolution regime for the first time?

Figure 1 provides the first indication that the non-FDIA-regulated share could indeed be related to changes in bank risk-taking after the introduction of the OLA. We plot the average difference in overall bank risk between the pre- and post-treatment over ranges of non-FDIA-regulated share. As a measure for bank risk, we use the average z-score, which is a composite measure approximating the inverse of the default probability, i.e., higher z-scores indicate less overall bank risk.<sup>17</sup> Although this is only a very rough indication, it is interesting to note that the higher ranges of non-FDIA-regulated shares correspond to higher increases in the z-score, i.e., lower overall bank risk after the

<sup>16</sup>See, for example, the detailed overviews of implementation timelines and effective dates produced by Anand (2011); CCH Attorney-Editor (2010); DavisPolk (2010).

<sup>17</sup>Refer to the following section for a detailed description of the composition of the z-score.

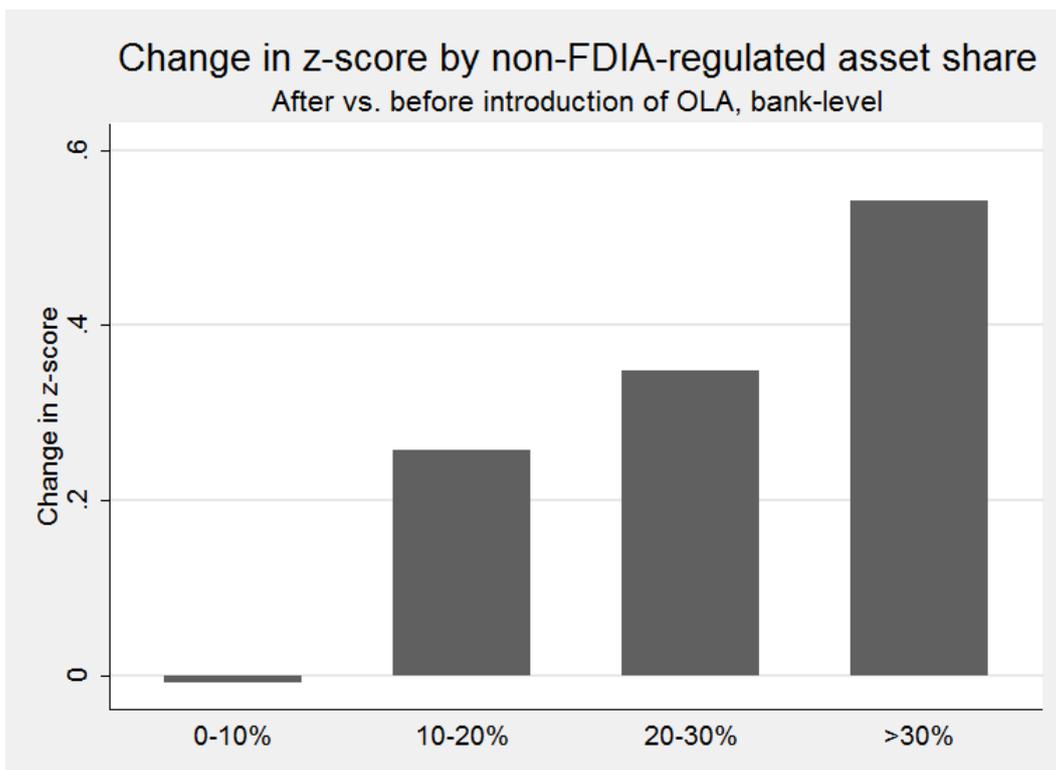


Figure 1: Change in z-score by non-FDIA-regulated asset share

introduction of the Orderly Liquidation Authority.

Figures 2 and 3 provide a perspective regarding the way in which affected (i.e., treatment) and non-affected (i.e., control) banks' overall risk develops over a longer time and reacts to the introduction of the Orderly Liquidation Authority. Again, we depict the average z-score of each group as a measure for overall bank risk, taking the absolute values and evaluating them over time. Because the z-score incorporates the standard deviation of returns, we must compute the score over a period of several quarters. We do this for 8-quarter periods (Figure 2) and 4-quarter periods (Figure 3) both pre- and post-treatment, excluding the treatment period as defined above (Q3 2009 - Q2 2010).

Admittedly, these figures provide only a very crude evaluation that does not control for potentially omitted variables and other sources of endogeneity beyond the bivariate difference-in-difference setup. However, several interesting patterns emerge from the two figures. First, the differential behavior of affected and non-affected banks around the treatment is evident. In both figures, the affected banks experience a much stronger increase in the z-score between the pre-treatment and the post-treatment periods. However, the key identifying assumption of difference-in-difference is that the two groups would exhibit a parallel development in the absence of treatment. We can test this parallel trend assumption by including additional periods of data before and after the pre- and post-treatment periods. Indeed, we find a parallel trend before the treatment. In both graphs, affected and non-affected institutions develop approximately in parallel in the absence of treatment. Figure 3 even allows us to add an additional period after the post-treatment period, which again exhibits a parallel trend. It is interesting to observe that affected banks consistently exhibit higher risk (lower z-score) before the treatment and reverse this pattern after the treatment. Overall, in the absence of treatment, both affected and non-affected banks appear to develop in parallel. It is only at the introduction of the OLA that the treatment group of affected banks experiences a materially different behavior, i.e., a larger decrease in risk-taking compared to the control group of non-affected banks. Consequently, these results are a first indication that our main hypothesis might be correct. We test both the main hypothesis and the parallel trend assumption in a more rigorous empirical framework below.

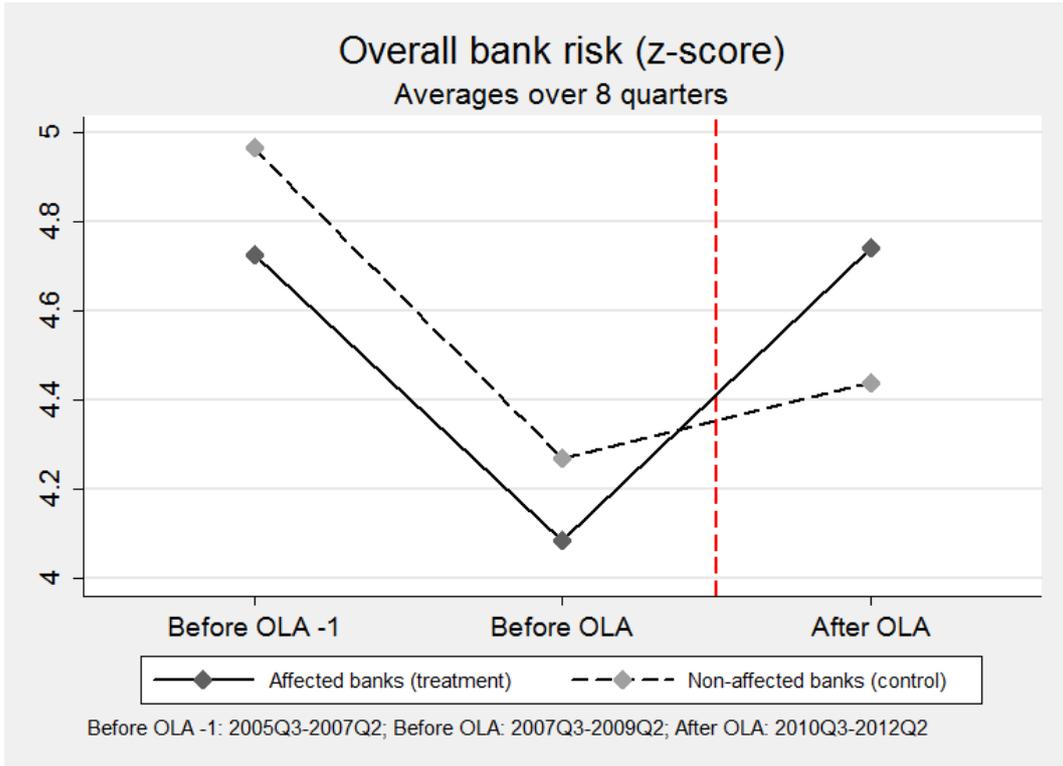


Figure 2: Bank risk-taking before and after OLA

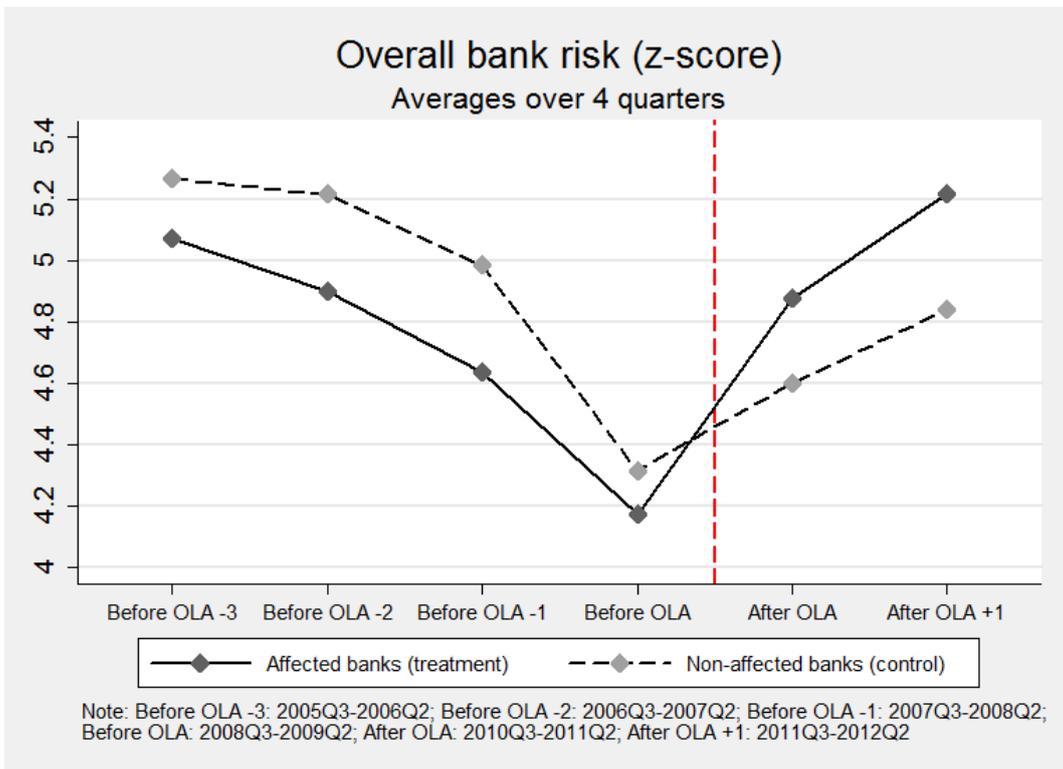


Figure 3: Bank risk-taking before and after OLA

## 4 Model and dataset

### 4.1 Baseline model

To conduct more rigorous empirical testing, we construct a difference-in-difference model whose baseline version is depicted in equation 1. The main dependent variable of the model is  $Risk_{i,t}$ , one of the risk measures outlined below. The core explanatory variables are  $afterOLA_t$ , indicating before or after treatment (i.e., improvement in resolution technology), and  $AFFECTED_i$ , a dummy variable set to 1 for those institutions affected by the improvement in resolution technology and to 0 for the control group (non-affected). Bank ( $\gamma_i$ ) and time ( $\delta_t$ ) fixed effects are used to control for influences constant either over time (e.g., time-invariant bank characteristics) or across banks (e.g., the state of the economy or the financial system in a specific quarter). The model is complemented by a set of control variables ( $X_{i,t}$ ) to control for additional covariates that might vary by treatment and control group and influence bank behavior. If our main hypothesis holds true, we expect to see a decreasing effect of the difference-in-difference term on risk, expressed in the direction and significance of coefficient  $\beta_3$ .

$$\begin{aligned} Risk_{i,t} = & \alpha + \beta_1 * afterOLA_t + \beta_2 * AFFECTED_i \\ & + \beta_3 * (afterOLA_t * AFFECTED_i) \\ & + \gamma_i + \delta_t + X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{1}$$

To ensure the robustness of our results, we test our hypotheses on different levels and using alternative empirical setups and datasets. First, we identify bank level data from quarterly call reports that we merge with data from quarterly BHC reports to construct a dataset covering financial data on the bank level and the BHC level. This dataset enables us to compute and test bank level risk measures as dependent variables in the above setup. Additionally, we define several measures for business model choices (e.g., regarding portfolio decisions or funding structure) that can be tested on the bank level. Second, we investigate risk-taking decisions on the level of new mortgage loan business. Therefore, we construct a loan level dataset using the Home Mortgage Disclosure Act (HMDA) Loan Application Registry.

### 4.2 BHC and bank level dataset

We construct the bank level dataset based on two main sources. On the individual bank level, we assemble data from the Consolidated Reports of Condition and Income (FFIEC031/041), commonly known as call reports. These reports cover several hundred items of financial data that any bank with a state or national charter is required to file on a quarterly basis with the FFIEC. We construct a sample that contains the full set of banks (up to 8,943 individual institutions) and financial data for the period covering the first quarter of 2005 until the second quarter of 2012. In addition, we assemble a second dataset on the bank holding company level. BHCs are required to file quarterly financial reports on a consolidated and parent-only level (FR Y-9C/LP/SP), which are available from the Federal Reserve Bank of Chicago. Regarding the individual bank data, we construct a sample that contains the full set of BHCs (up to 5,756 individual institutions) and selected financial data for the period covering the first quarter of 2005 until the second quarter of 2012. In a third step, we obtain identifiers for the top holders, i.e., the ultimate owner of any individual bank from the FDIC’s Statistics on Depository Institutions (SDI), to match both the individual bank level and the BHC level datasets. This matched dataset enables us to identify and compute all necessary variables as defined below. Panel A of Table 1 provides summary statistics and the sources of the data on the BHC level, whereas Panel B focuses on the bank level data.

**Dependent variables (I): Overall bank risk (accounting/regulatory data)** To conduct a series of robustness checks, we use several measures of risk-taking on the overall bank (or

BHC) level. Our primary measure is the z-score of each bank, which is defined as  $Z = (\overline{RoA} + CAR)/\sigma RoA$ , where  $\overline{RoA}$  is the mean return on assets,  $CAR$  is the capital asset ratio, and  $\sigma RoA$  is the estimated standard deviation of the return on assets. The mean and standard deviation of return on assets are computed over 8-quarter periods (and additionally over 4-quarter periods for robustness tests). Very few banks for which less than 3 datapoints in one of the periods are available for this computation are removed from the sample. The z-score has been widely used in the empirical literature as a proxy for overall bank risk (e.g., Boyd et al. (2010); Dam and Koetter (2012); Gropp et al. (2010); Laeven and Levine (2009); Roy (1952)). Essentially, the z-score captures both channels through which a reduction in overall bank risk can take place, i.e., asset and liability side, and measures the number of standard deviations by which a bank's return on assets would have to fall below its mean to deplete the available capital. If we define default as losses exceeding capital, the z-score can be interpreted as a measure for distance to default or the inverse of the default probability (Laeven and Levine, 2009; Roy, 1952). Hence, a higher z-score indicates that a bank is more stable, i.e., associated with less overall risk. We follow Laeven and Levine (2009) in computing the natural logarithm of the z-score.<sup>18</sup>

In addition, we use the  $\sigma RoA$  as an alternative risk measure that focuses exclusively on the volatility of banks' return on assets. The return volatility has been used as a measure for overall bank risk in several previous empirical studies (e.g., Dam and Koetter (2012); Laeven and Levine (2009)). We complement the z-score and  $\sigma RoA$  with an alternative overall risk measure - average asset risk - that is defined as  $RWA/assets$ , with  $RWA$  being the risk-weighted assets. This measure provides an indication of average asset risk (albeit only in a pre-defined, regulatory sense) and has also been used in the empirical literature (e.g., Berger et al. (2012); De Nicolò et al. (2010)). Whereas the average asset risk is a relatively simple measure and risk weights have been criticized as an inadequate expression of true risk, this measure offers the advantage of being computable on an individual quarterly level. In any case, we use alternative risk measures as dependent variables to test the robustness of our results.

**Dependent variables (II): Overall bank risk (market data)** All of the dependent variables thus far are calculated from accounting data, using the call report and BHC report datasets. Despite their shortcomings, we prefer accounting data over market data because the latter significantly reduce our sample size, particularly for banks. However, we find stock market data for 471 listed BHCs that we accessed via Thomson Reuter's Datastream. Hence, we use a proxy for overall bank risk based on stock market data and available for only a subsample as a complement to the accounting and regulatory data risk measures that are available for our full sample of banks and BHCs. Following Konishi and Yasuda (2004) and Laeven and Levine (2009), we define risk as the volatility of stock returns, which we compute on a quarterly basis as the standard deviation of weekly stock returns. We use Datastream's total return index that includes reinvested dividends to calculate the stock return.

**Dependent variables (III): Bank business model** To test the impact of the regulatory change on the business model of banks, we also define a set of additional dependent variables that proxy for business model choices. With regard to portfolio risk choices, we use some of the measures suggested by Duchin and Sosyura (2012). In detail, these are the trading asset ratio (defined as the ratio of assets held in trading accounts to total assets), the low-risk securities ratio (defined as the ratio of securities of U.S. government agencies and subdivisions to total securities), and the high risk securities ratio (defined as the ratio of equity securities, asset-backed securities, and trading accounts to total securities). Additionally, we use the CRECD loan ratio, which is defined as the sum of commercial real estate loans (CRE) and construction and development loans (CD) divided by total loans. This ratio is used as a proxy for the degree of complex and risky

<sup>18</sup>Because the z-score is highly skewed, its natural logarithm is assumed to be approximately normally distributed.

loans on a bank’s balance sheet and has been shown to be associated with risky business models more prone to bank failure (e.g., DeYoung (2013)).

Beyond the asset side, we also take into account a measure from the liability side of banks’ balance sheets. More precisely, we test the effect on the deposit funding ratio, which is simply defined as deposits divided by assets. This measure is intended to capture the riskiness of the funding structure and the vulnerability to liquidity shocks.

Finally, we also define a measure for risk in income structure. For this measure, we use the non-interest income ratio, which we compute as average non-interest income divided by average total income.<sup>19</sup> Non-interest income, particularly from non-core activities such as investment banking, venture capital and trading activities, has been shown to be relatively volatile compared to interest income (DeYoung and Roland, 2001) and to be associated with higher overall bank risk (Brunnermeier et al., 2012; DeJonghe, 2010; Demirgüç-Kunt and Huizinga, 2010).

**Explanatory variables and controls** In accordance with the identification strategy and the baseline model outlined above, the treatment dummy  $AFFECTED_i$ , the treatment-period indicator  $afterOLA_t$ , and particularly the interaction between the two are defined as our main explanatory variables. To identify the affected (i.e., treatment) group, we compute an indicator capturing the non-FDIA-regulated share of total assets of a bank holding company. We do this by summing up the total assets of all insured depository institutions (i.e., the ones that fall under the FDIA-regulation and hence are subject to FDIC resolution authority) and scaling it by the total consolidated assets of the BHC (including the non-bank, non-FDIA-regulated assets). For independent banks (i.e., insured depository institutions that do not belong to a BHC), we set the non-FDIA-regulated share to 0. The dummy indicating affiliation to the treatment group,  $AFFECTED_i$ , is set to 1 for all BHCs (and banks belonging to a BHC in the bank level dataset) that hold more than 30% non-FDIA-regulated assets, i.e., the group of BHCs and banks that is particularly affected. Although the non-FDIA-regulated share of assets varies between 0 and 100%, it is rather skewed towards the lower end because the majority of holding companies own bank subsidiaries that fall under the FDIA resolution authority, some even exclusively. A cutoff at 30%, however, delivers a sufficiently large treatment group. Moreover, a share of 30% is arguably a significant size of the total business of a bank, which will reasonably influence overall business decisions and consequently affect institutions’ behavior. At the lower end, we set  $AFFECTED_i$  to 0 for all BHCs (and banks belonging to a BHC) that do not have any or less than 10% non-FDIA-regulated assets. Admittedly, these cutoffs are highly arbitrary. Thus, we use not only several alternative cutoffs but also an interaction with the continuous variable of the non-FDIA-regulated share of total assets to perform additional robustness tests.

The second main explanatory variable,  $afterOLA_t$ , is set to 1 for all periods between the third quarter 2010 and the second quarter 2012. The variable is set to 0 for the eight quarters preceding the treatment, i.e., from the third quarter 2007 to the second quarter 2009. To formally test the parallel trend assumption, we define a second pre-pre-treatment period stretching over the eight quarters from the third quarter 2005 to the second quarter 2007. As a robustness check, we use a second set of  $afterOLA_t$  and all variables referring to it, which defines  $afterOLA_t$  over 4 quarters around the treatment period.

In addition to the main explanatory variables, we control for a host of additional covariates that might influence bank risk-taking and business model decisions and that vary over banks and quarters (i.e., that are not captured by the bank and time fixed effects in our model). In detail, these are total assets as a proxy for bank size, capital ratio (defined as equity capital to total assets), return on assets as a proxy for earnings capability, and liquidity ratio (defined as cash and balances at other depository institutions to total assets). All of these variables are computed from the call report and BHC report datasets. Furthermore, several recent analyses have shown that banks tend to increase risk when they receive bailout assistance from the government, e.g.,

<sup>19</sup>Note that we average over the 4- or 8-quarter periods defined above to balance single-quarter effects.

from the Capital Purchase Program (CPP) as part of the Troubled Asset Relief Program (TARP) (Black and Hazelwood, 2012; Duchin and Sosyura, 2012). We follow these studies and add an indicator for the CPP status of a bank that is 1 if a bank is a current recipient of CPP funds in a given quarter and 0 otherwise. The data for this indicator are obtained from the U.S. Department of the Treasury CPP Transactions Report.

### 4.3 Loan level dataset

To test our hypotheses on risk-taking concerning new business operations, specifically new mortgage loan business, we use the HMDA Loan Application Registry as our loan level dataset. HMDA requires most mortgage lenders to collect and report data on all mortgage loan applications on an annual basis. According to Dell’Ariccia et al. (2012), the HMDA dataset comprises approximately 90% of all U.S. mortgage loan applications. The HMDA dataset is a comprehensive registry containing loan information (e.g., loan purpose and loan amount), applicant information (e.g., race and gross annual income), information on the status of the loan application (e.g., sold, originated, denied, withdrawn) including purchaser type or reasons for denial, and information on regional demographics. Moreover, the dataset allows us to distinguish between supply and demand effects in the mortgage loan market. The information regarding whether the loan has been sold in the calendar year of origination is very valuable in our definition of actual risk-taking. Because approximately 60% of originated mortgage loans are securitized (Loutskina and Strahan, 2009), we need to distinguish in our analyses between loans that have been sold and loans that have been held on the balance sheet at least for a certain time period, because the former do not represent actual balance sheet risk-taking.<sup>20</sup> A major disadvantage of the HMDA dataset is that it does not provide more precise information on the time of loan application, purchase, or origination than the calendar year.

We obtain all loan applications for the years 2009 to 2011 from the FFIEC.<sup>21</sup> We remove three sub-samples from the raw data. First, we exclude all loan applications that have been denied in the pre-approval process, withdrawn or not accepted by the loan applicant or closed for incompleteness to focus on those loans that have either been approved and originated or denied in the loan approval process. Second, we drop all purchased loans from the sample to focus on true loan origination (and to avoid the double counting of loans because the dataset does not allow for the exact matching of sold and purchased loans). Finally, we eliminate all loan applications aimed at refinancing an existing loan because these loans usually have a different pricing and underwriting structure than new home purchase or home improvement loans (Avery et al., 2007).<sup>22</sup> We supplement the HMDA dataset with data on the regional housing price index obtained from the Federal Housing Finance Agency. We match the annual appreciation as well as the average annual level of the housing price index based on the Metropolitan Statistical Area (MSA) in which the property is located.<sup>23</sup> In a final step, we match this dataset with the bank level dataset based on an individual and universal bank identifier to identify the treatment and control groups and to derive bank control variables.<sup>24</sup> We use the bank level dataset because mortgage loans are almost exclusively granted through bank subsidiaries or individual banks.<sup>25</sup> Panel C of Table 1 provides summary statistics for the resulting

<sup>20</sup> However, loans that remain on the balance sheet do not necessarily represent balance sheet credit risk either, because lenders can issue synthetic collateralized debt obligations on their loan portfolio to insulate credit risk while still retaining loan servicing. The HMDA dataset does not provide information on synthetic collateralized debt obligations. As a robustness check we calculate the ratio of mortgage loans securitized but with servicing retained to total mortgage loan portfolio from the bank level data and exclude all banks in which this ratio is larger than 30%.

<sup>21</sup> This period is marked by a decrease in housing prices following the subprime mortgage crisis. We account for these adverse conditions and for varying developments in the regional housing markets by adding regional housing market controls and regional fixed effects.

<sup>22</sup> Moreover, refinancing loans could be biased because of ‘evergreening’ effects: Refinancing loans can exhibit a higher risk pattern when intended to prolong non-performing home purchase loans that would be otherwise written off.

<sup>23</sup> We use data for State Nonmetropolitan Areas when information regarding MSA is missing.

<sup>24</sup> HMDA does not provide these identifiers for loans in 2009. We use identifiers from 2010 and 2011 and match lenders manually based on name and address when lenders are only present in the 2009 sub-sample.

<sup>25</sup> We identify two lenders with BHC status. For consistency, we exclude those observations from our analyses.

loan application sample.

**Dependent variables** We calculate the loan-to-income ratio (*LIR*) of each loan application as the main risk measure in the loan level dataset. The *LIR* represents the loan applicant’s ability to repay the loan amount considering his gross annual income and indicates riskier loans by increasing loan-to-income ratios. This measure is commonly used in the mortgage business to assess borrower risk, e.g., it is a criterion for eligibility for loans to be insured by the Federal Housing Administration. According to Dell’Ariccia et al. (2012), the measure is also used in lenders’ loan decision processes. The *LIR* usually correlates strongly with other measures of individual loan risk: As shown by Rosen (2011), loans with lower loan-to-income ratios tend to have stronger FICO scores.<sup>26</sup> Therefore, we are confident that the loan-to-income ratio is an appropriate risk measure in our loan sample. Because the distribution of the loan-to-income ratio displays some distant outliers on the high end, we drop all loan observations with loan-to-income ratios above the 99.5th percentile to ensure that our results are not driven by those outliers.<sup>27</sup> We perform this trimming for the sample of loan applications as well as for the sample of originated loans, so that the loan-to-income ratio ranges between 0 and 7.2 in our prepared sample. For the sample with originated loans, we use the loan-to-income ratio as the dependent variable. For the sample of loan applications, we exploit an approach similar to that of Duchin and Sosyura (2012). We simulate risk ranges by dividing the full loan application sample into ranges with  $\Delta = 0.5 LIR$  (0.0-0.5 being the safest and  $>3.0$  the riskiest loan-to-income range) and run our multivariate baseline model regression for each range separately with the loan approval indicator as a dependent variable. The loan approval indicator is set to 1 if a loan application has been approved and originated and set to 0 if the loan application has been denied. To exclude the possibility that our results are driven by loan demand rather than by loan supply, we calculate the natural logarithm of the total number of loan applications received by a bank from each loan-to-income range in each year and run our multivariate baseline model regression with this dependent variable as in Duchin and Sosyura (2012).

**Explanatory variables and controls** We use the same explanatory variables in the loan level dataset as described above. To identify the treatment and control groups in the loan level dataset, we use the treatment dummy  $AFFECTED_i$  with the previously mentioned 10%/30% non-FDIA-regulated asset share cutoffs. We also utilize the treatment dummy with different cutoffs as a robustness check and construct a continuous variable exploiting the share of non-FDIA-regulated assets. To distinguish before and after treatment periods, we set the variable  $afterOLA$  to 1 for all loan applications in 2011 and to 0 for all loan applications in 2009.<sup>28</sup>

We control for several groups of additional covariates that might influence risk-taking in the new mortgage loan business. First, we use the set of bank control variables described above to account for bank size, capital adequacy, profitability, and liquidity. To capture further individual bank characteristics, we exploit bank fixed effects. Second, we add dummy variables to control for certain loan characteristics that indicate whether the loan has been sold and whether the loan is government-guaranteed or government-insured.<sup>29</sup> Third, we control for demographic conditions by adding the log of total population and the share of minority population for each U.S. Census tract. Fourth, we take into account economic conditions, particularly the state of the housing markets, because these conditions can vary significantly across U.S. regions. We control for the log

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<sup>26</sup>FICO scores are provided by the Fair Isaac Corporation and measure a borrower’s creditworthiness before obtaining a mortgage loan.

<sup>27</sup>We assume that these outliers primarily stem from misentries because of observed unrealistically high requested loan amounts or very low annual incomes.

<sup>28</sup>Because the calendar year is the only time designation in the HMDA dataset, we cannot match loans to particular quarters. Due to current data availability from the FFIEC, we could not obtain loan applications for years before 2009.

<sup>29</sup>Certain borrowers can receive loans that are insured by the Federal Housing Administration or guaranteed by the Veterans Administration, Farm Service Agency, or Rural Housing Services. Historically, these programs have allowed lower income U.S. borrowers to obtain mortgage loans that would otherwise not be affordable.

of median family income and the change and average level of the house price index for each MSA. To further capture heterogeneity in demographic and economic conditions that is not time-varying, we use regional fixed effects on a very detailed geographical level, namely, the U.S. Census tract.

Table 1: Summary statistics

<b>Panel A: BHC sample</b>						
Variable group and name	Source	Mean	SD	Min	Max	N
<i>Dependent variables (risk and business model)</i>						
Bank z-score	BHC	4.57	(1.27)	-2.76	11.96	46043
$\sigma$ RoA	BHC	19.09	(54.99)	0	2709	77613
Asset risk (RWA/assets)	BHC	73.08	(11.98)	0	126.2	15395
$\sigma$ Stock (total return index)	DS	6.95	(6.26)	0	104.24	6523
Trading assets ratio	BHC	0.33	(2.29)	0	42.75	14663
Low risk securities ratio	BHC	0.21	(2.91)	0	100	15547
High risk securities ratio	BHC	2.46	(9.37)	0	97.81	8797
CRECD loans ratio	BHC	0.48	(1.64)	0	31.32	15642
Deposit funding ratio	BHC	67.66	(13.41)	0	99.81	14663
Non-interest income ratio	BHC	23.56	(14.29)	0.03	99.53	16679
<i>Explanatory variables</i>						
BHC non-FDIA-regulated share	BHC, SDI	12.23	(9)	0	100	46569
Affected bank dummy (treatment)	BHC, SDI	0.05	(0.22)	0	1	19467
After OLA dummy		0.49	(0.5)	0	1	86038
<i>Additional bank- and quarter-varying control variables</i>						
Total assets (in USD mn)	BHC	5040.52	(72044.57)	0	2358266	49112
Capital ratio	BHC	10.04	(6.55)	-57	100	47410
Earnings (RoA)	BHC	0.1	(0.84)	-41.95	81.82	47359
Liquidity ratio	BHC	6.57	(6.61)	0.02	97.12	44375
CPP recipient bank-quarter	TR	0.03	(0.18)	0	1	86038
<b>Panel B: Bank sample</b>						
Variable group and name	Source	Mean	SD	Min	Max	N
<i>Dependent variables (risk and business model)</i>						
Bank z-score	SDI	4.44	(1.17)	-9.46	8.83	126104
$\sigma$ RoA	SDI	25.58	(50.23)	0	2014.1	126427
Asset risk (RWA/assets)	SDI	67.67	(14.72)	0	231.97	127022
Trading assets ratio	SDI	0.07	(1.11)	0	77.17	126936
Low risk securities ratio	SDI	71.36	(26.25)	0	100	123346
High risk securities ratio	SDI	1.86	(9.17)	0	100	112917
CRECD loans ratio	SDI	32.89	(20.88)	0	112.5	126209
Deposit funding ratio	SDI	69.29	(11.45)	0	98.66	126785
Non-interest income ratio	SDI	16.41	(12.65)	0	99.95	122973
<i>Explanatory variables</i>						
BHC non-FDIA-regulated share	BHC, SDI	7.68	(9.18)	0	100	89547
Affected BHC dummy (treatment)	BHC, SDI	0.03	(0.16)	0	1	56464
After OLA dummy		0.47	(0.5)	0	1	127170
<i>Additional bank- and quarter-varying control variables</i>						
Total assets (in USD mn)	SDI	1703319.62	(31321571.09)	66	1842568960	127170
Capital ratio	SDI	11.72	(7.37)	-13.52	100	126788
Earnings (RoA)	SDI	0.11	(1.02)	-28.38	93.5	126788
Liquidity ratio	SDI	7.31	(7.93)	0	100	126936
CPP recipient bank-quarter	TR	0.03	(0.17)	0	1	127170
<b>Panel C: Loan sample</b>						
Variable group and name	Source	Mean	SD	Min	Max	N

Continued on next page

Table 1 – *Continued from previous page*

<b><i>Dependent variables</i></b>						
Loan-Income-Ratio (loan appl.)	HMDA	2.04	(1.37)	0	7.22	4145701
Loan-Income-Ratio (orig. loans)	HMDA	2.15	(1.29)	0	7.22	3106212
Loan-Income-Ratio (sold loans)	HMDA	2.5	(1.13)	0.01	7.22	2021819
Loan-Income-Ratio (unsold loans)	HMDA	1.5	(1.31)	0	7.22	1084393
Approval indicator	HMDA	0.75	(0.43)	0	1	4329647
<b><i>Explanatory variables</i></b>						
BHC non-regulated share (continuous)	BHC, SDI	0.23	(0.21)	0	1	4089198
BHC non-regulated share (dummy)	BHC, SDI	0.42	(0.49)	0	1	1876201
After OLA (2011/2009)		0.46	(0.5)	0	1	4329647
<b><i>Additional bank control variables</i></b>						
Total assets (in USD mn)	SDI	401968.92	(564608.08)	18.13	1788146.13	4329291
Capital ratio	SDI	10.19	(2.6)	-1.01	40.2	4329224
Earnings (RoA)	SDI	0.12	(0.32)	-6.08	2.36	4329224
Liquidity ratio	SDI	5.69	(3.93)	0	77.74	4328745
CPP recipient bank	TR	0.57	(0.49)	0	1	4329647
<b><i>Additional loan, demographic and economic control variables</i></b>						
Government-guaranteed/-insured loan	HMDA	0.3	(0.46)	0	1	4329647
Sold loan (orig. loans)	HMDA	0.63	(0.48)	0	1	3242987
Total population in tract	HMDA	5487.1	(2676.24)	1	36146	4280501
Minority population in tract	HMDA	23.97	(25.29)	0.23	100	4280395
Median family income (in USD)	HMDA	65698.53	(14446.18)	16100	111900	4280666
House price index level in MSA	FHFA	183.56	(28.94)	110	338.02	4228877
House price index appreciation in MSA	FHFA	-3.67	(3.72)	-19.49	9.21	4228877

**Notes:** This table reports variable names, sources, means, standard deviations, minimum and maximum values, and the number of observations for which data is available in our sample. The sources are: FED Chicago BHC database (BHC), Thomson Reuters Datastream (DS), Federal Housing Finance Agency (FHFA), Home Mortgage Disclosure Act Loan Application Registry (HMDA), FDIC SDI database and call reports (SDI), U.S. Department of the Treasury (TR).

## 5 Results and robustness

This section presents and discusses our main results. We begin with the effect of the improvement in resolution technology on overall bank risk and continue by evaluating the effects on bank business model and loan decisions. These results are complemented by several extensions, e.g., testing the parallel trend assumption using a placebo treatment event, conducting tests for too-big-to-fail effects, and searching for gambling behavior. Finally, we also discuss a set of robustness checks.

### 5.1 Overall bank risk-taking (accounting/regulatory data)

We first test the hypothesized effect of the OLA as an improvement in resolution technology on overall bank risk, using a univariate version of our baseline model. Table 2 presents the results of these univariate difference-in-difference comparisons, with Panel A focusing on a sample containing individual bank data and Panel B comprising a sample of aggregated BHC data. The treatment group includes all institutions that are particularly affected by the OLA and is defined as all banks (or BHCs in Panel B) belonging to a BHC with more than 30% of its assets not subject to the FDIA resolution procedure. Conversely, the control group contains non-affected institutions, i.e., all independent banks (that are hence fully subject to the FDIA resolution regime) and banks (or BHCs) that are part of a holding with 10% or less non-FDIA-regulated assets.

For both the affected and non-affected institutions, we compute the means of the overall bank

risk measures before (Q3 2007 - Q2 2009) and after (Q3 2010 - Q2 2012) the introduction of the Orderly Liquidation Authority. The resulting differences are tested for their statistical significance and displayed in columns (3) and (6). As a first result, it is interesting to note that all measures of overall bank risk are significantly decreasing across the board - for the treatment and control groups on both the bank and BHC levels - between the pre- and the post-treatment periods. This result, however, is not necessarily driven by the changes in regulation. Rather, it could be an overall trend towards less risk-taking that is influenced by, e.g., macroeconomic trends.<sup>30</sup> To test our hypothesis of a significant difference between the treatment and control groups, we compute the univariate difference-in-difference results in column (7). Interestingly, for both the  $z$ -score and  $\sigma RoA$  measures, the treatment group experiences a significantly larger decline in overall risk between pre- and post-treatment compared to the control group. This finding is fully in line with our main hypothesis. However, the picture for the asset risk measure is less conclusive because we do not find a significant effect in the univariate difference-in-difference estimates. Hence, these results may be interpreted, at most, as suggestive evidence, and therefore, we need to proceed with more conclusive tests.

Because these results may also be driven by unobserved variables, we run multivariate difference-in-difference estimations, adding two sets of fixed effects capturing both individual bank effects and quarter effects and a set of time-variant control variables as outlined in the previous section.<sup>31</sup>

Panel A of Table 3 presents the results of the multivariate difference-in-difference estimations.<sup>32</sup> These results show a highly significant decline in overall risk between pre- and post-treatment for affected banks compared to non-affected banks. In particular, the coefficient on the interaction term  $afterOLA_t * AFFECTED_i$  is positive for the  $z$ -score (i.e., more stable), negative for  $\sigma RoA$  and asset risk (i.e., less volatile/risky), and statistically significant at the 1 percent level for all risk measures. These results hold both at the level of individual banks and at the level of BHCs and strongly support our main hypothesis. Beyond statistical significance, the results also suggest an economically considerable impact: Affected banks increase their  $z$ -score, for example, by more than 11% on average, while non-affected banks change by less than 1%.

To move beyond the arbitrary cutoffs defining the treatment and control groups, we also estimate our model by replacing the treatment dummy with the actual share of assets not subject to FDIA resolution. As before, we included bank and time fixed effects as well as time-variant controls in our estimation. The results are displayed in Panel B of Table 3 and are very much in line with our dummy results in Panel A. Again, the coefficient on the interaction term indicates a significant increase in overall bank stability and a significant decrease in overall bank risk. We also estimated alternative cutoffs (e.g., 50 vs. 10 percent non-FDIA-regulated share of business) as robustness tests, which are not reported but are consistent with our main hypothesis.

The analyses presented thus far have shown a significant difference-in-difference effect, indicating that risk-taking decreases with the degree to which a bank is affected by the improvement of resolution technologies. However, the validity of the difference-in-difference approach also relies upon the identifying assumption of a parallel trend between the treatment and control groups in the absence of treatment. While we presented some suggestive evidence underlining this assumption in the previous section, we now apply a more rigorous approach in testing it. We extend our dataset to cover another 8-quarter period stretching from Q3 2005 to Q2 2007, which we define as a pre-placebo period. We now test the effect of a placebo treatment between the pre-placebo period and the pre-treatment period, using essentially the same model as in the analyses above. If the parallel trend assumption holds, we do not expect to find a significant difference-in-difference effect between the affected and non-affected banks or BHCs across both periods. The results of this placebo test are displayed in Table 4. Indeed, no significant difference-in-difference effect is

<sup>30</sup>One could, for example, argue that the outbreak of the financial crisis in 2008 increased volatility and that markets calmed down after 2010, thus causing the effect that we find.

<sup>31</sup>Note that for brevity in the tables, we do not report the regression coefficients on all of these control variables (which are generally in line with expectations and previous empirical findings).

<sup>32</sup>Note that the level effect on the  $afterOLA_t$  dummy drops as it is captured by the time fixed effects.

Table 2: **Bank risk-taking: Univariate Difference-in-Difference analyses**

<b>Panel A: Bank level</b>							
Dep. variable	(1)	(2)	(3)=(2)-(1)	(4)	(5)	(6)=(5)-(4)	(7)=(3)-(6)
	Affected banks Before OLA	After OLA	Dif	Non-affected banks Before OLA	After OLA	Dif	Dif-in-Dif
<b>Z-score</b>	4.086	4.741	0.655*** (0.0608)	4.270	4.440	0.170*** (0.0108)	0.485*** (0.0668)
<b><math>\sigma</math> RoA</b>	0.521	0.234	-0.287*** (0.0349)	0.321	0.252	-0.0697*** (0.00503)	-0.218*** (0.0312)
<b>Asset risk</b>	0.694	0.631	-0.0618*** (0.0014)	0.681	0.630	-0.0517*** (0.00132)	-0.0101 (0.00822)

<b>Panel B: BHC level</b>							
Dep. variable	(1)	(2)	(3)=(2)-(1)	(4)	(5)	(6)=(5)-(4)	(7)=(3)-(6)
	Affected banks Before OLA	After OLA	Dif	Non-affected banks Before OLA	After OLA	Dif	Dif-in-Dif
<b>Z-score</b>	4.051	4.554	0.503*** (0.0896)	4.17	4.37	0.196*** (0.0202)	0.307*** (0.0986)
<b><math>\sigma</math> RoA</b>	1.119	0.409	-0.71*** (0.196)	0.214	0.193	-0.0212*** (0.00477)	-0.689*** (0.0475)
<b>Asset risk</b>	0.697	0.632	-0.0644*** (0.0159)	0.762	0.682	-0.0801*** (0.00292)	0.0157 (0.0109)

**Notes:** This table presents univariate difference-in-difference estimates. Panel A reports the results for the bank sample, Panel B for the bank holding company (BHC) sample. Banks (or BHCs) are classified into two groups. The treatment group comprises affected banks (BHCs) that are part of a BHC with more than 30% of non-FDIA-regulated assets. The control group comprises non-affected banks (BHCs) that are independent or part of a BHC with less than 10% of non-FDIA-regulated assets. Treatment is defined as the introduction of the Orderly Liquidation Authority (OLA). Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets),  $\sigma$  *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Difference-in-difference estimates are displayed in column (7).

Heteroskedasticity consistent standard errors in parentheses

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

Table 3: **Bank risk-taking: Multivariate Difference-in-Difference analyses**

<b>Panel A: Dummy variable (treatment and control group definition)</b>						
Level	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	Bank level Z-score	$\sigma$ RoA	Asset risk	BHC level Z-score	$\sigma$ RoA	Asset risk
Affected bank	0.131** (0.0559)	0.0459 (0.0285)	0.0142 (0.00903)			
Affected BHC				-0.991*** (0.253)	-0.0649 (0.148)	-0.195 (0.141)
Affected bank x after OLA	0.476*** (0.0410)	-0.181*** (0.0277)	-0.0220*** (0.00536)			
Affected BHC x after OLA				0.545*** (0.0730)	-0.504*** (0.153)	-0.0131** (0.00645)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	55,811	55,894	56,140	17,726	17,995	5,560
R-squared	0.813	0.810	0.889	0.858	0.717	0.894
<b>Panel B: Continuous variable (unregulated share in %)</b>						
Level	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	Bank level Z-score	$\sigma$ RoA	Asset risk	BHC level Z-score	$\sigma$ RoA	Asset risk
Unregulated share (parent BHC-level)	0.390*** (0.0673)	0.0151 (0.0277)	0.0675*** (0.00948)			
Unregulated share (BHC-level)				-0.869*** (0.244)	0.162 (0.202)	-0.110 (0.0775)
Unregulated share x after OLA	0.772*** (0.0537)	-0.133*** (0.0276)	-0.0635*** (0.00690)			
Unregulated share x after OLA				1.766*** (0.155)	-1.316*** (0.391)	-0.0338* (0.0199)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	88,710	88,795	89,194	43,050	43,338	14,221
R-squared	0.786	0.797	0.885	0.809	0.743	0.877

**Notes:** This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk. Panel A reports the results for the difference-in-difference estimation, Panel B for the estimation using a continuous explanatory variable interaction. *Affected bank (BHC)* takes a value of 1 if the bank (BHC) is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank (BHC) is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *Unregulated share* is defined as the share of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets),  $\sigma$  *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

Table 4: **Bank risk-taking: Multivariate Difference-in-Difference analyses with placebo test**

Level Dep. variable	(1)	(2)	(3)	(4)	(5)	(6)
	Bank level Z-score	$\sigma$ RoA	Asset risk	BHC level Z-score	$\sigma$ RoA	Asset risk
Affected bank	0.160** (0.0639)	-0.0706 (0.0468)	-0.00704 (0.00900)			
Affected BHC				-1.084*** (0.242)	0.382** (0.169)	0.0586** (0.0237)
Affected bank x af- ter placebo	-0.0177 (0.0367)	0.106*** (0.0214)	0.00590 (0.00362)			
Affected BHC x af- ter placebo				0.0699 (0.0804)	0.172 (0.131)	0.000800 (0.00473)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	62,757	62,792	63,122	20,017	20,075	7,740
R-squared	0.755	0.819	0.901	0.787	0.774	0.933

**Notes:** This table presents multivariate difference-in-difference estimates for a placebo treatment. *Affected bank (BHC)* takes a value of 1 if the bank (BHC) is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank (BHC) is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After placebo* is 1 for the quarters Q3 2007 - Q2 2009 and 0 for the quarters Q3 2005 - Q2 2007. Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets),  $\sigma$  *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

found for the z-score and asset risk measures, neither in the bank nor in the BHC panel. While the coefficient on the interaction is also insignificant with  $\sigma RoA$  as a dependent variable in the BHC sample, return volatility appears to increase for individual banks belonging to an affected BHC after the placebo treatment. One potential explanation why the return volatility (and only the return volatility) is significantly higher for affected banks could be offered by the rational behavior that we would presume for these banks: Because there was a lower threat of resolution for these banks before the enactment of the OLA, they had incentives to take on higher risks during the pre-placebo period (and before). When the financial crisis hit (which coincides with the placebo treatment), this additional risk materialized in an overproportional increase in volatility. Admittedly, this is only a vague explanation and further research is warranted to investigate this effect. Apart from this one reaction of  $\sigma RoA$ , however, the presented evidence is mostly consistent with the parallel trend assumption.

Furthermore, it is interesting to note that the level effects for affected BHCs appear to confirm the presumption of higher overall risk of this group before the introduction of the OLA. This finding is consistent with our hypothesis that holdings with high unregulated shares are less subject to FDIA resolution and hence enjoy more of an implicit bailout guarantee - before the OLA. The effect does not occur for individual banks, presumably because these banks were already subject to FDIA resolution, even if they were part of a BHC (implying that BHC risk-taking largely occurred through the non-FDIA-regulated parts). When the resolution threat becomes realistic for banks and BHCs alike (even if they hold high previously non-FDIA-regulated shares), the difference in risk-taking and business model decisions is hypothesized to occur both in affected banks and affected BHCs, which is remarkably consistent with the results in the previous and following tables.

Table 5: **Bank risk-taking: Market data (uni- and multivariate analyses, placebo tests)**

Level	(1)	(2)	(3)	(4)	(5)
Model	BHC level				
Dep. variable	Univariate $\sigma$ Stock	Multivariate $\sigma$ Stock	$\sigma$ Stock	Placebo test $\sigma$ Stock	$\sigma$ Stock
Affected BHC	-0.00118 (0.00687)	0.00154 (0.0328)		0.0424 (0.0338)	
Unregulated share (BHC-level)			0.0293 (0.0278)		0.00118 (0.0208)
Affected BHC x af- ter OLA	-0.0419*** (0.00813)	-0.0314*** (0.00927)			
Unregulated share x after OLA			-0.0569*** (0.0146)		
Affected BHC x af- ter placebo				0.00756 (0.00708)	
Unregulated share x after placebo					0.0136 (0.0130)
Constant	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	YES
Bank FE	NO	YES	YES	YES	YES
Time FE	NO	YES	YES	YES	YES
Observations	1,728	1,632	5,466	2,136	6,651
R-squared	0.020	0.690	0.635	0.719	0.690

**Notes:** This table presents uni- and multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk. *Affected BHC* takes a value of 1 if the BHC has more than 30% of non-FDIA-regulated assets and a value of 0 if the BHC has less than 10% of non-FDIA-regulated assets. *Unregulated share* is defined as the share of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. *After placebo* is 1 for the quarters Q3 2007 - Q2 2009 and 0 for the quarters Q3 2005 - Q2 2007. The dependent variable is  $\sigma$  Stock, defined as standard deviation of the weekly total stock return. Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). The models include bank and time fixed effects as indicated.

Heteroskedasticity consistent standard errors in parentheses

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

Taken together, the results presented thus far confirm our main hypothesis: Banks or BHCs that were largely not subject to the FDIA resolution regime before are particularly affected by the introduction of the OLA and decrease their overall risk accordingly.

## 5.2 Overall bank risk-taking (market data)

Thus far, we have primarily relied on risk measures that are based on accounting and regulatory data, because these are available for the total population of banks and BHCs in the U.S. However, as accounting data suffer from several limitations, we also test our main models for overall bank risk with a risk measure based on market data, i.e., the volatility of total stock returns ( $\sigma$  Stock). The results of these tests using  $\sigma$  Stock as the dependent variable are displayed in Table 5.<sup>33</sup> In the first column, we repeat the univariate difference-in-difference analysis. We extend this analysis to the multivariate model that includes fixed effects and controls (columns (2) and (3)), using the dummy variable for the treatment and control groups and the continuous variable (non-FDIA-regulated share of assets) in turn. Any of these specifications result in a positive and highly significant coefficient on the interaction term, indicating that the stock return volatility of more affected

<sup>33</sup>Note that the tests can only be conducted on the BHC level because of stock market data availability.

BHCs decreases more strongly than the volatility of less affected BHCs after the introduction of the OLA.

Additionally, we also employ the market data risk assessment in the placebo setup that was outlined in the previous section, testing for the identifying assumption of a parallel trend between the treatment and control groups in the absence of treatment. Again, we define a placebo treatment between a pre-placebo period and the pre-treatment period and run the multivariate difference-in-difference model. The results shown in columns (4) and (5) support our previous findings of an insignificant placebo effect and are consistent with the parallel trend assumption.

Hence, the results presented in Table 5 confirm the robustness of our findings to using stock market data instead of accounting data in measuring overall bank risk. In the next step, we move beyond overall bank risk and analyze in more detail how banks change their behavior with regard to business model and investment choices as well as new loan origination.

### 5.3 Bank business model choices and loan origination

As outlined above, we define and compute several indicators for bank business model and investment choices that have been suggested in the previous literature (Brunnermeier et al., 2012; DeJonghe, 2010; DeYoung, 2013; Duchin and Sosyura, 2012). We test the difference-in-difference effect by using these indicators as dependent variables in our multivariate baseline model, including fixed effects and additional controls. Because data for these measures are in large part only available at the bank level (particularly for the loan data), we conduct our tests for the bank dataset. Table 6 presents the results, which are consistent with the hypothesized decrease in risky activities and investment choices for the affected banks after the introduction of the OLA. We begin with the effect on the trading assets ratio (column (1)). In line with the expectation that affected banks decrease risky and volatile activities (such as proprietary trading), we find a negative and significant coefficient on the interaction term. A similar result holds for the effect on the low and high risk securities ratios, presented in columns (2) and (3). Whereas affected banks appear to decrease investments in risky securities, they appear to increase their exposure towards low-risk securities classes. This shift in the securities portfolios is consistent with the expectation that affected banks will rush for safer investments and business models after the introduction of the OLA. In a similar vein, we would expect the treatment group of banks to decrease its exposure towards highly complex and risky loans (such as the CRECD loans) relative to its total loan portfolio. The negative and significant coefficient on the difference-in-difference term in column (4) suggests that we cannot reject this hypothesis.

Turning to the liability side of the bank business model, we would expect affected banks to opt for sources of funding that are considered more stable and that carry less interest rate risk. If the deposit funding ratio correctly proxies for this, we find our expectation confirmed by a positive and significant coefficient on the interaction term. Finally, we examine the effect on the sources of income of the bank. The negative coefficient on the interaction term in column (6) suggests that affected banks decrease their non-interest income relative to interest income more strongly than the control group after the introduction of the OLA. If non-interest income is indeed more volatile and associated with overall (systemic) risk, as claimed in the previous literature, the results found in column (6) are consistent with our main hypothesis.

The data and evidence presented thus far largely draw upon aggregated accounting data. To complement this with actual risk-taking in business operations on banks' micro-level, we extend our analysis to the mortgage loan business. We use our multivariate baseline model to test the difference-in-difference effect on risk-taking in newly originated mortgage loans. Table 7 presents the results exploiting the loan-to-income ratio as the risk measure. Column 1 displays an analysis of the entire sample of newly originated loans, yielding a negative and significant coefficient on the interaction term that confirms our main hypothesis. In a second step, we split this sample into loans that have been sold in the same calendar year (column (2)) and loans that have not been

Table 6: **Bank business model and investment choices: Multivariate Difference-in-Difference analyses**

Level	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	Bank level Trading assets ratio	Low risk securities ratio	High risk securities ratio	CRECD loan ratio	Deposit funding ratio	NII ratio
Affected bank	0.00101 (0.00318)	-0.0171 (0.0225)	0.0404*** (0.0151)	-0.00354 (0.00862)	-0.0109 (0.00720)	-0.000635 (0.00647)
Affected bank x af- ter OLA	-0.00605*** (0.00136)	0.0584*** (0.0118)	-0.0377*** (0.00926)	-0.0108*** (0.00312)	0.0307*** (0.00610)	-0.00927** (0.00447)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	56,140	54,000	44,050	55,384	56,137	53,737
R-squared	0.776	0.778	0.784	0.961	0.907	0.921

**Notes:** This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on bank business model and investment decisions. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. Several measures of bank business model and investment decisions are taken as dependent variables: *trading asset ratio* (defined as ratio of assets held in trading accounts to total assets), *low risk securities ratio* (defined as the ratio of securities of U.S. government agencies and subdivisions to total investment securities), *high risk securities ratio* (defined as the ratio of equity securities, asset-backed securities, and trading accounts to total investment securities), *CRECD loan ratio* (defined as the sum of commercial real estate loans and construction and development loans, divided by total loans), *deposit funding ratio* (defined as deposits divided by assets), and *non-interest income ratio* (defined as average interest income divided by average total income). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

sold in the same calendar year (column (3)). We assume that loans in the latter sample have been held on balance sheets at least for a certain time period so that they measure risk-taking more accurately. We find that affected banks significantly decrease loan-to-income ratios of new loans after the introduction of the OLA for both sold and unsold loans.

One further caveat could be loans that remain on the balance sheet for servicing but are de facto securitized (e.g., through synthetic collateralized debt obligations) and hence do not necessarily represent risk-taking. Because the HMDA dataset does not provide information on synthetic collateralized debt obligations, we calculate the ratio of mortgage loans securitized but with servicing retained to total mortgage loan portfolio from the bank level dataset and exclude all banks in which this ratio of synthetic loans is larger than 30%. We rerun our multivariate baseline model and find that affected banks with a low share of synthetic loans in fact reduce the risk of new loans that remain on their balance sheet after the introduction of the OLA, whereas this effect is not significant for sold loans (see Panel B of Table 7).

Our results on the sample of originated loans could possibly stem from loan demand rather than loan supply effects, i.e., only high-quality borrowers demand loans from affected banks after the introduction of the OLA. To account for potential loan demand effects, we include rejected loan applications, divide the loan application sample into different risk ranges based on the loan-to-income ratio, and test our main hypothesis using the application approval indicator as a dependent variable. The results for the analysis on the approval rate of loan applications are shown in Panel A of Table 8. We find that the probability of loan approval by affected banks decreases after the introduction of the OLA compared to non-affected banks. However, this decrease is not significant for the safest risk range with a loan-to-income ratio below 0.5, whereas it is significant for all remaining risk ranges. Additionally, we test for systematic differences in loan demand across risk ranges by employing the total number of loan applications per bank, year, and risk range as dependent variable and find that the loan demand at affected banks did not significantly decrease after the introduction of the OLA (see Panel B of Table 8).

We present evidence that after the introduction of the resolution threat, affected banks decreased risk-taking in new loan business by approving fewer loans from higher risk ranges, and we can exclude that our results are driven by loan demand effects. In sum, the presented results are consistent with the interpretation that affected banks decrease their overall risk-taking after the introduction of the Orderly Liquidation Authority and do so by shifting their investments, business models, and loan decisions towards more prudent behavior.

Table 7: Risk taking in new mortgage loan business: Multivariate Difference-in-Difference analyses

<b>Panel A: Newly originated loans from all banks in sample</b>			
	(1)	(2)	(3)
<b>Level</b>	<b>Loan level</b>		
<b>Sample</b>	<b>All originated loans</b>	<b>Sold loans</b>	<b>Unsold loans</b>
<b>Dep. variable</b>	<b>Loan-to-income ratio</b>		
Affected bank	-0.685*** (0.0767)	-0.170 (0.135)	-0.701*** (0.0984)
After OLA	0.00146 (0.00367)	-0.0581*** (0.00506)	0.0458*** (0.00554)
Affected bank x after OLA	-0.0691*** (0.00477)	-0.0352*** (0.00603)	-0.0459*** (0.00918)
Constant	YES	YES	YES
Bank controls	YES	YES	YES
Loan controls	YES	YES	YES
Demogr. controls	YES	YES	YES
Economic controls	YES	YES	YES
Bank FE	YES	YES	YES
Tract FE	YES	YES	YES
Observations	1,366,242	913,178	453,064
R-squared	0.324	0.219	0.367
<b>Panel B: Newly originated loans from banks with share of synthetic loans &lt;30%</b>			
	(1)	(2)	(3)
<b>Level</b>	<b>Loan level</b>		
<b>Sample</b>	<b>All originated loans</b>	<b>Sold loans</b>	<b>Unsold loans</b>
<b>Dep. variable</b>	<b>Loan-to-income ratio</b>		
Affected bank	-0.698*** (0.0824)	-0.194 (0.136)	-0.747*** (0.110)
After OLA	-0.0193*** (0.00514)	-0.0624*** (0.00732)	0.00752 (0.00769)
Affected bank x after OLA	-0.0470*** (0.00817)	-0.0192 (0.0118)	-0.0406*** (0.0128)
Constant	YES	YES	YES
Bank controls	YES	YES	YES
Loan controls	YES	YES	YES
Demogr. controls	YES	YES	YES
Economic controls	YES	YES	YES
Bank FE	YES	YES	YES
Tract FE	YES	YES	YES
Observations	830,560	532,525	298,035
R-squared	0.350	0.229	0.387

**Notes:** This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on risk-taking in new originated mortgage loans. Panel A reports the results for the sample with all banks, Panel B restricts the sample to banks where the ratio of mortgage loans securitized but with servicing retained to total mortgage loan portfolio is less than 30%. Sold loans are originated loans that were sold in calendar year of origination; unsold loans are originated loans that were not sold in calendar year of origination. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for all loans originated in 2011 and 0 for all loans originated in 2009. The dependent variable to measure risk-taking in new loans is the *loan-to-income ratio*. Bank control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, and liquidity ratio. Loan control variables comprise two indicator variables: sold loan is equal to 1 if the loan has been sold (all originated loans sample) and guaranteed/insured loan is equal to 1 if the loan is guaranteed or insured by the government. Demographic control variables comprise the natural logarithm of total population in tract and share of minority population in tract. Economic controls comprise the natural logarithm of median family income in tract, appreciation and level of regional house price index. All models include bank and regional (tract) fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

Table 8: Approval of mortgage loan applications and loan demand along risk ranges: Multivariate Difference-in-Difference analyses

Panel A: Approval rate of loan applications								
Level	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Loan level							
Sample	Loan applications within loan-to-income ratio range							
Dep. variable	All appl.	0.0-0.5	0.5-1.0	1.0-1.5	1.5-2.0	2.0-2.5	2.5-3.0	>3.0
	Application approval indicator							
Affected bank	0.102*** (0.0221)	0.0270 (0.0532)	0.0901 (0.0647)	0.157** (0.0640)	-0.0146 (0.0614)	0.147* (0.0872)	0.172** (0.0819)	0.155* (0.0936)
After OLA	-0.0043*** (0.00103)	-0.0233*** (0.00345)	-0.0117*** (0.00358)	-0.0117*** (0.00317)	-0.00251 (0.00266)	0.00423 (0.00254)	-0.00275 (0.00272)	-0.00112 (0.00218)
Affected bank x after OLA	-0.0465*** (0.00127)	-0.00640 (0.00491)	-0.0167*** (0.00481)	-0.0529*** (0.00406)	-0.0630*** (0.00336)	-0.0599*** (0.00319)	-0.0540*** (0.00339)	-0.0563*** (0.00253)
Constant	YES	YES	YES	YES	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES	YES	YES
Loan controls	YES	YES	YES	YES	YES	YES	YES	YES
Demogr. controls	YES	YES	YES	YES	YES	YES	YES	YES
Econ. controls	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,839,672	193,601	164,310	189,605	242,163	257,310	234,283	491,291
R-squared	0.443	0.425	0.446	0.469	0.493	0.514	0.539	0.581

Panel B: Total number of loan applications								
Level	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Loan level							
Sample	Loan applications within loan-to-income ratio range							
Dep. variable	All appl.	0.0-0.5	0.5-1.0	1.0-1.5	1.5-2.0	2.0-2.5	2.5-3.0	>3.0
	Log of total number of loan applications per bank, year, and range							
Affected bank	-0.196 (0.180)	0.605 (0.410)	-0.216 (0.278)	-0.420* (0.242)	-0.230 (0.299)	0.101 (0.313)	-0.825*** (0.242)	-0.814** (0.341)
After OLA	-0.171*** (0.0153)	-0.222*** (0.0269)	-0.166*** (0.0238)	-0.119*** (0.0247)	-0.214*** (0.0256)	-0.188*** (0.0253)	-0.237*** (0.0272)	-0.305*** (0.0297)
Affected bank x after OLA	-0.127 (0.122)	-0.229 (0.166)	-0.211 (0.133)	-0.198 (0.149)	-0.119 (0.178)	-0.109 (0.214)	-0.185 (0.238)	-0.0855 (0.202)
Constant	YES	YES	YES	YES	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	33,762	4,510	4,492	4,338	4,225	4,060	3,791	4,261
R-squared	0.015	0.085	0.078	0.072	0.097	0.104	0.108	0.157

**Notes:** This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on approval rate of mortgage loan applications and loan demand along risk ranges. Column (1) shows the full sample of loan applications, columns (2)-(8) contain the sub-samples of loan applications based on loan-to-income ratio ranges. The dependent variable in Panel A is the *application approval indicator* which equals 1 when loan application succeeded in loan origination (and 0 when the application was denied). Panel B employs the natural logarithm of *total number of loan applications* per bank, year, and risk range as dependent variable. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for all loan applications in 2011 and 0 for all loan applications in 2009. Bank control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, and liquidity ratio. Loan control variables comprise two indicator variables: sold loan is equal to 1 if the loan has been sold and guaranteed/insured loan is equal to 1 if the loan is guaranteed or insured by the government. Demographic control variables comprise the natural logarithm of total population in tract and share of minority population in tract. Economic controls comprise the natural logarithm of median family income in tract, appreciation and level of regional house price index. Models in Panel A include bank and regional (tract) fixed effects; models in Panel B include bank fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

## 5.4 Extensions and robustness

**Is the OLA a credible threat for all banks?** We have thus far tested our main hypothesis and found that affected banks indeed reduced their risk-taking after the introduction of the OLA relative to non-affected banks. However, we also postulated in the beginning that this effect might vary with credibility, effectiveness, and the political will to apply the new improvement in regulatory technology. As formulated in the context of the model by DeYoung et al. (2013): When the political will or preference for discipline is low or the liquidity trade-off is high, we expect to find a lower effect or even no effect from the introduction of the OLA on the behavior of affected banks. In other words, if financial institutions do not think that the OLA represents a credible threat, they will not change their behavior in response.

Which factors might moderate the credibility of the resolution threat to a financial institution? One straightforward - and admittedly simple - way of testing the above prediction is by using bank size as a moderator variable. Essentially, we take the total assets of a bank as a proxy for high liquidity trade-off, hypothesizing that the treatment effect decreases with bank size.<sup>34</sup> The argument is simple: Winding down a larger institution might produce high liquidity costs, making discipline less favored by regulators, which ultimately results in the low credibility of the threat of resolution - even after the introduction of the OLA.

We implement this idea in our model by using total bank assets ( $assets_{i,t}$ ) as a third source of identifying variation. Adding the total assets as a moderator variable augments our multivariate difference-in-difference model by a triple interaction term  $AFFECTED_i*afterOLA_t*assets_{i,t}$ , as well as second level interactions of total assets,  $AFFECTED_i*assets_{i,t}$  and  $assets_{i,t}*afterOLA_t$ , and the secular effects of  $assets_{i,t}$ . This augmented model is run for several overall risk measures as dependent variables; the results are presented in Table 9. As a first observation, the coefficient on the difference-in-difference term remains positive and significant for the z-score, and negative and significant for  $\sigma RoA$  and asset risk, thus supporting the robustness of earlier findings. Our main focus, however, is on the moderated effect, i.e., the coefficient of the triple interaction term. This coefficient is negative for the z-score and positive for  $\sigma RoA$  and asset risk, lending support to the hypothesis of a moderation of the resolution threat by bank size.

### **How do the ‘too-big-to-not-rescue’ institutions react to the introduction of the OLA?**

If bank size (or systemic importance) still protects banks from resolution, can this fully compensate for the threat of a new resolution technology? In fact, it is possible not only that the largest banks are unaffected, but also that the absence of an even stronger threat (i.e., stronger than the OLA) induces additional risk-taking. This would be rational if no additional improvement in resolution technology for these firms is expected any time soon after the passing of the Orderly Liquidation Authority. Because the effect is a priori far from obvious, the question regarding the reaction of the largest and most systemically important banks - the ‘too-big-to-not-rescue’-banks - warrants a closer analysis.

Hence, we separately test whether extraordinarily large or otherwise systemically important institutions are responsive to the improvement in resolution technologies. For robustness, we test two different definitions of systemic importance. For our first test, we isolate all banks that form a part of one of the eight U.S. financial holdings that have been determined as a ‘global systemically important bank’ (GSIFI) by the Financial Stability Board.<sup>35</sup> As an alternative definition, we form a sample of all institutions with asset size larger than USD 50 billion. This cutoff is not entirely arbitrary, but rather chosen according to a threshold above which the Dodd-Frank Act stipulates specific enhanced supervision activities and prudential standards, also in conjunction with the

<sup>34</sup>For clarification: The ‘affected’ bank classification is thus far not defined by size (or any other systemic risk variable) but purely on the grounds of resolvability according to the FDIA. Hence, there are, e.g., large and small banks that are classified as ‘affected’ (and ‘not affected’).

<sup>35</sup>In total, the Financial Stability Board designated 29 institutions to be GSIFI, eight of which are of U.S. origin. These institutions include Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JPMorgan Chase, Morgan Stanley, State Street, and Wells Fargo.

Table 9: **Bank size and bank risk-taking: Moderated Difference-in-Difference analyses**

Level	(1)	(2)	(3)
Dep. variable	Bank level Z-score	$\sigma$ RoA	Asset risk
<b>Secular effects</b>			
Affected bank	0.160** (0.0655)	0.0392 (0.0319)	0.00534 (0.00938)
Total assets	-0.0393 (0.0306)	0.0221** (0.107)	-0.00964*** (0.00321)
<b>2nd level interactions</b>			
Affected bank x after OLA	0.508*** (0.0422)	-0.203*** (0.0281)	-0.0241*** (0.00552)
Total assets x after OLA	0.274*** (0.0348)	-0.0587*** (0.009)	-0.00371* (0.00191)
Affected bank x total as- sets	0.0626** (0.0302)	-0.0239** (0.0105)	0.00891*** (0.0032)
<b>Moderated Dif-in-Dif</b>			
Affected bank x after OLA x total assets	-0.275*** (0.0347)	0.0592*** (0.00899)	0.00386** (0.00191)
Constant	YES	YES	YES
Controls	YES	YES	YES
Bank FE	YES	YES	YES
Time FE	YES	YES	YES
Observations	55,811	55,894	56,140
R-squared	0.807	0.805	0.888

**Notes:** This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk, moderated by bank size. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. *Total assets* is the total asset size of a bank (in USD mn). Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets),  $\sigma$  *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Control variables comprise the bank's capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

Table 10: **Too-big-to-not-rescue effect: Multivariate Difference-in-Difference analyses on TBTNR banks**

Level Sample Dep. variable	(1)	(2)	(3)	(4)	(5)	(6)
	<b>Bank level Part of U.S.-GSIFI Z-score</b>	<b><math>\sigma</math> RoA</b>	<b>Asset risk</b>	<b>Asset size USD 50+ billion Z-score</b>	<b><math>\sigma</math> RoA</b>	<b>Asset risk</b>
Unregulated share (parent BHC-level)	2.466*** (0.948)	-1.816* (0.988)	0.721*** (0.160)	1.133*** (0.367)	-0.892*** (0.238)	0.111* (0.0579)
Unregulated share x after OLA	-1.415** (0.696)	0.0800 (0.295)	0.262*** (0.0643)	-0.815* (0.475)	0.0992 (0.147)	0.0795* (0.0455)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	485	485	492	452	452	454
R-squared	0.824	0.665	0.925	0.863	0.847	0.907

**Notes:** This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall risk of those banks that could be classified as too-big-to-not-rescue. The estimation is conducted for two subsamples of banks: All banks that are part of one of the U.S. GSIFIs as classified by the FSB (columns (1) to (3)) and all banks with total asset size of USD 50 billion or more (columns (4) to (6)). *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets),  $\sigma$  *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

OLA (compare, e.g., DFA, Title II, Sec 210). We use these two definitions as alternative but not mutually repetitive indicators of systemic importance.<sup>36</sup> When we run our model on these separate samples of banks, we must use the continuous version of the explanatory variable since too many institutions would be dropped from the sample otherwise. We are able to conduct these tests on our bank level sample; the results are reported in Table 10.

In line with our expectations, the coefficients of the interaction term emerge as insignificant for the return volatility as dependent variable in both subsamples. However, it is interesting to note that for the z-score and asset risk as dependent variables, the coefficients on the interaction term are significant but in opposite directions compared to our baseline regression results. We interpret this finding as support for the rationale outlined above. More affected systemically important banks do not reduce their risk-taking after the introduction of the OLA; conversely, these banks might even increase their risk-taking. One possible explanation for this finding is that the threat of resolution resulting from the OLA is not credible for these banks. They do not appear to believe that the regulator is indeed fully enabled to resolve such institutions in case of failure - due to lacking financial or operational capabilities, fears of systemic risk and contagion, or other rationales. Moreover, because the OLA was considered the major change in bank resolution law in response to the financial crisis, it appears unlikely that these institutions had to expect a further, perhaps more credible upgrade in resolution technology any time soon. Imagining all financial institutions as a system of corresponding vessels in a situation in which the most affected institutions have to reduce risk, only a few players can assume this risk - and these are the affected institutions for which the resolution threat is still not credible. Hence, a rational strategy for these ‘too-big-to-not-rescue’-institutions would be to increase, rather than decrease, risk-taking (at least as long as the resolution threat does not become more realistic). Although we cannot test this directly, the shift in securities and trading asset holdings that we find in the data is at least suggestive of this rationale. Whereas the majority of affected institutions that are not part of a GSIFI heavily reduce their securities holdings (particularly their high risk securities and trading assets) after the introduction of the OLA, the affected GSIFI institutions even increase their holdings. These results suggest that this particular change in the resolution regime is not a panacea to discipline banks that are deemed ‘too-big-to-fail’.

**Gambling in the meantime?** In a final extension, we test how banks’ risk-taking changed in the post-announcement period, i.e., between the proposal of the OLA (mid 2009) and its actual enactment (mid 2010). Theory and available empirical evidence suggest that gambling might occur in this period if the changes in regulation reduce the affected banks’ charter value (Fischer et al., 2012; Murdock et al., 2000). To the extent that the introduction of the OLA actually reduces the charter value of affected banks, e.g., by removing the previously existing implicit bailout guarantee, we might find evidence of gambling in bank behavior. However, banks would need to shift their behavior twice between the publicly known proposal of the OLA and its signing into law. First, after June 2009, they would need to increase risk to exploit the trade-off between high risk-return and potential failure with likely bailout on the one hand and loss in charter value on the other hand. Very briefly after this, before July 2010, bailout becomes less likely (as the OLA becomes effective), and banks would need to readjust their strategy again. Is this realistic? Was the time horizon long enough to shift behavior twice, or was the legislation passed so quickly that gambling did not occur?

To test for the occurrence of gambling in the intermediate phase, we define a post-treatment period (*after OLA*) and a ‘gambling-period’ (*after announcement*), both of which we run against a pre-treatment period. Whereas the pre-treatment period is defined as Q3 2008 to Q2 2009, the period in which gambling might occur stretches from Q3 2009 to Q2 2010. For comparison, we define another 4-quarter post-treatment period as Q3 2010 to Q2 2011 and use it as a benchmark

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<sup>36</sup>Only 24 institutions in our bank level sample fulfill both criteria, whereas an additional 40 institutions form a part of a GSIFI and an additional 80 institutions report more than USD 50 billion in assets.

Table 11: **Bank risk-taking and business model choices: Multivariate Difference-in-Difference analyses with 4-quarter periods and test of risk-taking in post-announcement period**

<b>Panel A: Benchmark tests</b>							
Level Periods	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Bank level 4-quarter periods				2-quarter periods		
Dep. variable	Z-score	Trading assets ratio	Low risk securities ratio	High risk securities ratio	Trading assets ratio	Low risk securities ratio	High risk securities ratio
Affected bank	0.0889 (0.128)	0.00313*** (0.00115)	-0.0240 (0.0403)	0.0591** (0.0278)	0.00315 (0.00273)	-0.0253 (0.0886)	0.125** (0.0514)
Affected bank x after OLA	0.252*** (0.0600)	-0.00568*** (0.00202)	0.0542*** (0.0145)	-0.0482*** (0.0129)	-0.00390* (0.00202)	0.0517*** (0.0170)	-0.0515*** (0.0148)
Constant	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Observations	28,393	28,579	27,513	21,860	14,597	14,045	11,221
R-squared	0.801	0.749	0.850	0.838	0.801	0.892	0.883

<b>Panel B: Gambling tests</b>							
Level Periods	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Bank level 4-quarter periods				2-quarter periods		
Dep. variable	Z-score	Trading assets ratio	Low risk securities ratio	High risk securities ratio	Trading assets ratio	Low risk securities ratio	High risk securities ratio
Affected bank	0.0882 (0.133)	-0.000280 (0.00162)	-0.0225 (0.0271)	0.0131 (0.0206)	-0.00269 (0.00430)	-0.0493 (0.0328)	0.0269** (0.0119)
Affected bank x after announce- ment	-0.00361 (0.0553)	0.00285 (0.00241)	0.0242** (0.0113)	-0.0275** (0.0113)	0.00607 (0.00414)	0.00546 (0.00977)	-0.0204** (0.00961)
Constant	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Observations	29,276	29,472	28,363	22,581	14,653	14,101	11,217
R-squared	0.822	0.804	0.900	0.869	0.830	0.951	0.933

**Notes:** This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk, using pre- and post-treatment periods that stretch over 4 (columns (1) to (4)) or 2 (columns (5) to (7)) quarters. Panel A presents benchmark tests comparable to our baseline estimations, but with shorter pre- and post-treatment periods. Panel B tests the occurrence of a gambling effect. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2011 (columns (1) to (4)) and Q3 2010 - Q4 2010 (columns (5) to (7)) and 0 for the quarters Q3 2008 - Q2 2009 (columns (1) to (4)) and Q1 2009 - Q2 2009 (columns (5) to (7)). *After announcement* is 1 for the quarters Q3 2009 - Q2 2010 (columns (1) to (4)) and Q3 2009 - Q4 2009 (columns (5) to (7)) and 0 for the quarters Q3 2008 - Q2 2009 (columns (1) to (4)) and Q1 2009 - Q2 2009 (columns (5) to (7)). Several dependent variables are tested: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets, only available for the 4-quarter period), *trading asset ratio* (defined as ratio of assets held in trading accounts to total assets), *low risk securities ratio* (defined as the ratio of securities of U.S. government agencies and subdivisions to total investment securities), and *high risk securities ratio* (defined as the ratio of equity securities, asset-backed securities, and trading accounts to total investment securities). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

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

effect for comparison to the gambling results. For robustness, we define an additional set of pre-, gambling-, and post-treatment periods; each one stretches over 2 quarters: Q1/Q2 2009 as the pre-treatment period, Q3/Q4 2009 as the potential gambling period, and Q3/Q4 2010 as the post-treatment period. We run the main model with the z-score<sup>37</sup> (for overall comparison) and a selection of investment choice risk measures that we deem to be adjustable within a short period, i.e., the trading asset ratio and the low and high risk securities ratios, as dependent variables.<sup>38</sup> Panel A in Table 11 presents the results for the 4-quarter and 2-quarter benchmark regressions (pre-treatment vs. post-treatment). It should be noted that these results can also be interpreted as a robustness test of the initial 8-quarter results. With all overall and investment risk measures indicating less risk-taking by affected banks after the introduction of the OLA in the 4-quarter/2-quarter regressions, these results are fully in line with our baseline model. The findings on potential gambling are displayed in Panel B of Table 11 (pre-treatment vs. gambling-period). Interestingly, we do not find a significant effect in the overall risk regression using the z-score as a dependent variable. Likewise, the coefficient on the interaction term is not significant for the trading assets ratio. However, the results for low and high risk securities ratios (for the 2-quarter periods only the high risk securities) indicate that, if at all, affected banks assume less, not more, risk in the intermediate period. Interpreting these findings, we do not find any evidence of gambling in the intermediate period; rather, the affected banks even start decreasing their risk-taking already by shifting their securities portfolios.

**How robust are these findings?** To test the robustness of the results presented above, we have conducted a host of robustness tests using alternative specifications and variable definitions, sample restrictions, and additional entire datasets. This section briefly summarizes the robustness tests and their main results. For brevity and ease of comparison, some of the results from the robustness tests were already presented in the tables above. All other results, although not presented, are also largely consistent with our hypotheses and confirm the effects we report.

The following robustness tests have been conducted:

- With regard to our dependent variables, we have defined and tested a set of alternative measures for overall bank risk and risk choices in business model/investment decisions, both on the bank level and on the micro-level of business decisions. On the overall bank level, we have used accounting data and market data to compute alternative risk measures. All of our results have been shown to be robust to these alterations and yield similar conclusions, indicating that the results are not driven by specific definitions of individual dependent variables but are largely consistent with each other.
- We acknowledge that the dummy-version of our treatment variable  $AFFECTED_i$  is defined along arbitrary cutoffs. To test the robustness of our main bank risk-taking results, we have also defined alternative cutoffs (0%, 5%, 10% on the lower bound and 30% and 50% on the upper bound).<sup>39</sup> Moreover, we have also used the share of non-FDIA-regulated assets as an explanatory variable, particularly in interaction with  $afterOLA_t$ . With regard to the definition of the treatment period and the pre- and post-treatment periods, we have employed alternative variables computed over 8, 6, and 4 quarters. Running our main bank risk-taking model with these alterations in the key explanatory variables yields results that are comparable in statistical and economic significance.

<sup>37</sup>Note that we cannot reasonably define the z-score for the 2-period regressions because the measure requires the computation of a mean and a standard deviation (for which we defined a minimum requirement of 3 available datapoints above). Hence, z-score results are only presented for 4-quarter period regressions.

<sup>38</sup>We also tested the two models with all other previously used dependent variables and found no immediate adjustment effect for the intermediate period.

<sup>39</sup>Concerning the loan level dataset, varying the lower cutoff bound yields similar results. Applying a 50% cutoff for the upper bound is not meaningful because there are only very few banks in the loan level dataset with a share of non-FDIA-regulated assets above this cutoff.

- To alleviate concerns about endogeneity in our model, we extend beyond the univariate difference-in-difference approach and add bank and time fixed effects for regressions using the bank level dataset and bank and regional (tract level) fixed effects for regressions using the loan level dataset as well as sets of time-varying control variables (as appropriate). We have tested all of our models in alternative specifications, including and excluding the controls and fixed effects, finding consistent results. Particularly, our results hold when rerunning our baseline model excluding bank controls but including bank fixed effects.
- Where appropriate and mandated by theory, we have used alternative model specifications. One important specification is the choice of regression model to test the application approval indicator, which is a binary variable. In Panel A of Table 8 we presents the results using the Linear Probability Model (LPM) as the estimation method. Although the LPM has serious drawbacks (i.e., heteroskedastic, can predict probabilities outside the range [0;1]), this model can be appropriate in a panel-data setting (see Puri et al. (2011) for a detailed methodological discussion). We have rerun these regressions with probit and logit models and obtained results that are consistent with the findings presented in Table 8.
- Like numerous other papers using a difference-in-difference methodology, we rely on a panel dataset with repeated cross sections of banks and several periods of data before and after the treatment. Bertrand et al. (2004) describe how this setup can be prone to autocorrelation problems that may lead to an underestimation of the standard errors. Therefore, we have further corrected standard errors for possible autocorrelation at the bank level (as suggested by Puri et al. (2011) and Wooldridge (2010)) and have rerun our models. The results are comparable in size and significance to our findings in the baseline model.
- Finally, we have addressed concerns related to our samples by correcting for outliers, restricting samples to explanatory variables consistent over time, and using entirely different levels of aggregation. First, there might be concerns that the results are driven by outliers, e.g., in the dependent variable or in the non-FDIA-regulated share that is used to define the treatment variable. In the bank level dataset, we have winsorized the dependent variable, the explanatory variable, and the control variables with one percent in their highest and lowest quantiles. We have run all bank level tests using these winsorized versions of dependent, explanatory, and control variables, all together and individually.<sup>40</sup> All of our results are robust to these alterations and yield very similar outcomes. Second, to address concerns about the consistency of key explanatory variables, we have excluded banks whose *AFFECTED<sub>i</sub>* status changed within our observation period. Our results do not change when applying this restriction. Third, we have tested our hypotheses on bank risk-taking for the following different levels of aggregation: BHC level and bank level. Where possible, based on data availability, we test and present both the bank and the BHC level results in parallel, which are largely comparable in direction and significance.

Taken together, our robustness tests suggest that our main findings are not driven by variable definition, model specification, or sample choice.

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<sup>40</sup>In the loan level dataset, we eliminate all observations with loan-to-income-ratios above the 99.5th percentile to avoid the possibility that our results are driven by potential misentries in the loan application registry.

## 6 Concluding remarks and policy implications

In July 2010, the U.S. legislature enacted the Orderly Liquidation Authority as part of the financial system reform package known as the Dodd-Frank Act. The OLA extends a special bank resolution procedure to financial institutions that were previously not covered by the provisions of the Federal Deposit Insurance Act, which allows the FDIC to resolve failed banks in an administrative procedure that secures liquidity and discipline. Hence, the OLA affects financial institutions differently, raising the resolution threat particularly for those institutions that were in large part not previously subject to the FDIA resolution regime.

Building on a recent theoretical model by DeYoung et al. (2013), we suggest several hypotheses regarding the way in which this regulatory change affects bank behavior, particularly risk-taking and business model choices. We propose a difference-in-difference framework exploiting the differential effect of the OLA to test these hypotheses. First and foremost, we find the results to be consistent with our main hypothesis. The introduction of the OLA changes the behavior of the affected financial institutions towards less risk-taking and safer business models compared to the non-affected institutions. In the absence of treatment, i.e., of the regulatory change, both the affected and the non-affected institutions behave equally, which further corroborates our results. Consistent with the theoretical prediction that the main effect varies with the credibility, capability, and political will of the regulator to indeed resolve failed institutions, we find that the effect vanishes for the largest, most systemically relevant institutions. Finally, we have to reject the hypothesis that affected banks gamble between the announcement and the enactment of the OLA.

Our findings yield several interesting policy implications. If we consider our results to be an evaluation of a specific change in the U.S. bank resolution regime, we confirm that the Orderly Liquidation Authority is indeed an effective improvement to the regulatory arsenal. To the extent that a reduction in overall risk-taking of the previously non-FDIA-regulated financial institutions (as compared to their already regulated peers) was one of the legislature's intentions, our results suggest that the OLA can - at least in parts - be considered successful. However, making OLA's resolution threat credible and thus effective for banks with the highest systemic importance while moderating the liquidity cost of winding down such institutions will remain a crucial challenge for regulators.

Moreover, although our analyses focus on the effects of a specific resolution regime, i.e., the Orderly Liquidation Authority, our results prompt us to also draw general implications for the design or reform of bank resolution regimes around the world. Based on these findings and the previous literature, we propose three fundamental features of effective bank resolution regimes that, in our view, can help to increase and maintain stability in the financial system and prevent future financial crises. First, a bank resolution regime that takes into account the special role of financial institutions (contrary to regular and often inapplicable corporate bankruptcy law) and that commands sufficient legal and financial resources is essential, not only to avoid major interruptions in liquidity provision but also to create a credible resolution threat for financial institutions to discipline them *ex ante*. Second, comprehensive coverage of financial institutions as a whole - that extends beyond the scope of only deposit-taking entities - will avoid incentives to shift risks into non-resolvable entities. Third, implementation speed is crucial. When regulators succeed in implementing the resolution threat quickly after its announcement, excessive gambling behavior in the lag time before enactment might be prevented.

Taken together, a bank resolution regime that incorporates these elements can become more than wishful thinking - it can be an effective threat that disciplines banks towards more prudent behavior.

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