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Wishful thinking or effective threat? Tightening bank resolution regimes and bank risk-taking

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ABSTRACT

We propose a framework for testing the effects of changes in bank resolution regimes on bank behavior. By exploiting the differential relevance of recent changes in U.S. bank resolution (i.e., the introduction of the Orderly Liquidation Authority, OLA) for different types of banks, we are able to simulate a quasi-natural experiment using a difference-in-difference framework. We find that banks that are more affected by the introduction of the OLA (1) significantly decrease their overall risk-taking and (2) shift their loan origination toward lower risk, indicating the general effectiveness of the regime change. This effect, however, does (3) not hold for the largest and most systemically important banks. Hence, the introduction of the OLA in the U.S. alone does not appear to have solved the too-big-to-fail problem and might need to be complemented with other measures to limit financial institutions' risk-taking.

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).

The Financial Times applauded that this "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'".[2] *The Economist concluded that "the most important provision is the resolution authority under which federal regulators can seize any financial company [. . .]. This is an improvement on the status quo".*[3] *Did such a dramatic change in resolution powers influence bank risk-taking?*

1. Introduction

When governments were confronted with seriously distressed banks during the global financial crisis of 2008/2009 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 legal empowerment, financial resources, and cross-border cooperation to effectively resolve failed banks.[4] Following

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[2] See *Financial Times*, July 12, 2010.

[3] See *The Economist*, July 3, 2010.

[4] Among many other examples, 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. 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. At the time of Lehman Brothers' failure, Washington Mutual was put into FDIC receivership by its regulator, the Office of Thrift Supervision. The FDIC sold Washington Mutual's assets, deposit liabilities and secured

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1572-3089/

these recent crisis experiences, bank regulators and legislators have discussed and brought into force significant changes to bank resolution regimes[5] 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 are not only of vital importance in dealing with failing banks and minimizing costs associated with bank failures but can also 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 (instead of resolution). Although various rationales for bailout policies can be formulated (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, 2013; Dam and Koetter, 2012; Duchin and Sosyura, 2013). Conversely, when bailout guarantees cease to be implicit through a credible and enforceable improvement in bank resolution regimes, we expect banks to change their behavior toward less risk-taking. 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 disciplining effect follows from a clear economic rationale. When depositors and creditors cease to believe that the regulator[6] will bail out the bank due to the lack of an appropriate resolution technology, they have more incentives for monitoring and enforcing discipline. 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 threat becomes more credible.

The introduction of the Orderly Liquidation Authority (OLA) provides an ideal setup to study this disciplining 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), their subsidiaries, and non-bank financial companies. In this paper, we distinguish between BHCs with a large share of assets previously not subject to the FDIC resolution regime (which can thus be regarded as particularly affected by the regulatory change) and BHCs with mainly subsidiaries that have already been subject to the FDIC resolution regime (which are less or not affected). By exploiting the differential relevance of the OLA for these groups not only at the BHC but also at the individual bank level, 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 risk and default probability of affected institutions? Is the reduction in risk also perceived by market participants? Is there a change in risk-taking regarding new business, e.g., do banks originate less risky mortgage loans? Is the resolution threat credible and effective even for banks that are deemed too-big-to-fail?

These questions are addressed using a three level dataset – holding aggregates, bank level data, and loan level data – and employing several different measures for risk-taking. Testing risk measures based on both accounting and market 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 more than 7% on average, while non-affected banks hardly change it. 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 for affected bank holding companies. On a more detailed level, we find that affected banks shift their new loan origination toward 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 the overall effect does not hold for the largest and most systemically important institutions. This is in line with the theoretical argument that the effectiveness of improvements in resolution technology also depends on the credibility of their application, i.e., the ultimate resolution threat. Hence, even the introduction of the OLA in the U.S. does not appear to have solved the too-big-to-fail problem and might need to be complemented with other ex ante measures to limit large and complex financial institutions' risk-taking. Our findings are robust to various specifications and we are able to rule out potential alternative explanations. We also conduct placebo tests that provide additional support for our findings.

We focus our analysis on the U.S. because of the unique identification opportunity and the availability of data, but our results might have wider implications. The findings are not only of concern in evaluating the effectiveness of a resolution policy change in the U.S., but also can contribute to regulatory discussions, e.g., in the context of a resolution mechanism that is part of the European Banking Union.

Our paper contributes to the recent literature on the effects of regulatory actions or legal changes on bank behavior, particularly risk-taking (e.g., Berger et al., 2012; Black and Hazelwood, 2013; Dam and Koetter, 2012; Duchin and Sosyura, 2013; Gropp et al., 2014). 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 and Yorulmazer, 2008; Perotti and Suarez, 2002), empirical evidence is mostly limited 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.

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. The 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.

[5] 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).

[6] Unless otherwise noted, we refer to the authority competent for resolution decisions when using the term 'regulator' in the context of this paper.

The remainder of this paper is organized as follows. Section 2 provides an overview of the related theoretical literature and the core findings of previous 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 analyses, complemented with extensions and robustness tests. Section 6 concludes and provides policy implications.

2. Related literature and key hypotheses

Several forms of bank regulation have extensively been discussed in the existing literature, among them, e.g., alternative forms of deposit insurance, capital regulation, and restrictions on bank activities. The resolution of distressed banks, however, is likely the most intricate regulatory area regarding risk-taking incentives. Overall, one can think of two stereotypical (and 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. A different 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 (e.g., Duchin and Sosyura, 2013; Black and Hazelwood, 2013; Dam and Koetter, 2012). A credible resolution threat for banks in case of failure resembles the removal of an (implicit) bailout guarantee and might thus decrease excessive risk-taking incentives ex ante.

A comprehensive theoretical model of the interaction between resolution regimes and bank behavior was recently offered by DeYoung et al. (2013). Building on the time-inconsistency problem of bank closure decisions (Mailath and Mester, 1994; 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. Faced with banks inherently fragile to suffer from moral hazard with regard to excessive risk, complexity, and volatility, the regulator has essentially two alternatives. 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 (e.g., slow processes, missing information, or legal limits to available regulatory instruments). These might (temporarily) lead to liquidity costs – such as disruptions in lending – in the case of bank closures and 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 has an intrinsic motivation to prefer bailouts or forbearance over straightforward closure. The outcome of this trade-off is being determined by the regulator's time discount rate and available resolution technology. The higher the time discount rate, the stronger the regulator's preference for liquidity, i.e., bailout.[7] The better the resolution technology available to the regulator 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.

Taken together, the existing literature models and evaluates several effects of bank failure resolution (bailout or closure) on bank behavior. Empirical evidence on resolution policies is, however, mostly limited to the (non-)application of resolution rules (Brown and Dinç, 2011; Kasa and Spiegel, 2008; Korte, 2013). To the best of our knowledge, there has not been any study thus far that empirically investigates the effects of tightening resolution regimes on bank risk-taking.

Building on the implications suggested by the theoretical literature, we propose the following hypothesis and subject it to econometric testing: If a change in bank resolution regimes (e.g., in the legal provisions governing bank resolution) represents a credible and effective improvement to bank resolution technology, affected banks will adjust their behavior toward more discipline ex ante. We thus expect affected institutions to change toward less risk-taking after the change becomes effective. We do not expect to find an effect on risk-taking if the change in bank resolution technology is not credible or not effective.

3. Identification strategy – an application to changes in the U.S. bank resolution regime

Despite 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 in testing our hypotheses we focus on 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: incomprehensive legal provisions and insufficient financial endowment. We will argue that the Orderly Liquidation Authority represents a significant technological improvement to these two issues.

[7] 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. 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.

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. For bank holding companies, financial holding companies, and other non-bank financial institutions, 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.

The FDIA stipulates a special resolution regime for covered institutions, an administrative insolvency procedure, stemming from the conviction that banks are somewhat distinctive, particularly with regard to insolvency. According to Marinc and Vlahu (2011) the following bank characteristics advocate a special resolution regime: (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; Marinc and Vlahu, 2011). 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).

The procedures of corporate insolvency law 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 exhibit similar characteristics to those described by Marinc and Vlahu (2011), an application of these corporate insolvency procedures might cause severe disruptions. 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 considered to be a deficiency in the resolution regime for financial firms, which might have protected these institutions from actual failure by making bailout the only available choice (FDIC, 2011; Marinc and Vlahu, 2011).

Second, 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). 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 for financial holdings and non-bank financial companies before 2010.[8]

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 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 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, such a determination could be made for any financial company in the U.S.[9] 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.[10] 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.

Moreover, 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.[11] The FDIC is empowered to charge ex

[8] 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 und 2010, the FDIC resolved a record number of more than 300 banks.

[9] 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).

[10] 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 h, the authorization is automatically granted by the operation of law (DFA, Title II, Sec. 202).

[11] The fund is set up as a theoretically unlimited credit line from the Treasury. Sec. 210 allows the FDIC to borrow funds not exceeding 10% of the to-be-resolved financial company's total consolidated assets during the first 30 days of closure. Thereafter the borrowing amount is limited to 90% of the fair value of the total consolidated assets of the to-be-resolved financial company that would be available for repayment of the funds.

post risk-based assessments to financial companies[12] to replenish the Orderly Liquidation Fund (DFA, Title II, Sec. 210).

The Orderly Liquidation Authority can be interpreted as an improvement to resolution technology in at least two dimensions. First, the OLA provides a legal empowerment alleviating the previous limitation of the FDIC to only place a certain group of financial institutions into a special bank resolution procedure. 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. 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.[13]

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. 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. 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 in the sense of 'treatment intensity' 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.

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 in the absence of treatment (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 construct a placebo treatment to test the parallel trend assumption (1). Regarding the differential treatment effect (2), we assume that other regulatory changes either concerned banks independently of their share of assets under FDIA regulation or did not occur simultaneously to the introduction of the OLA. The first argument supporting this assumption is that the introduction of the OLA is regarded as the most influential change at its time of passing.[14] 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.[15] Nevertheless, banks may have adjusted their behavior in anticipation of the effectiveness date, e.g., adjusting to the potential requirements of the Volcker Rule. We explore this argument in the robustness test section. Third, even if other important regulatory changes had become effective at the same time, none of those changes arguably affected banks differently depending on their share of FDIA-regulated assets. In addition, 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 might have occurred at the same time. Following this line of reasoning, we test the effect at the bank

[12] Specifically, Sec. 210 stipulates that the assessments are to be imposed on bank holding companies with consolidated assets exceeding USD 50 billion and non-bank financial companies supervised by the FED board.

[13] Because of data availability and quality, we must define slightly different pre- and post-treatment periods in the loan level dataset. The following section provides additional details.

[14] See, e.g., the quote from *The Economist* in the prelude.

[15] Two other elements of the Dodd-Frank Act that are regarded as crucial are the Volcker Rule and enhanced regulation of systematically important financial institutions. The Volcker Rule is still not fully finalized and implemented. Regarding enhanced regulation of systematically important financial institutions, the designation as systematically important non-bank financial institution was only finalized in April 2012 and key rules and their impact became only clear in December 2011. Therefore, we do not expect these changes to have any significant impact on risk-taking at the time the OLA became effective (July 2010). Likewise, other elements that might have an effect on bank risk-taking, e.g., Swaps Pushout Rule, rules for swap dealers and major swap participants, oversight of systematically important financial market utilities, did not become effective until Q2 2012. Refer, for example, to the detailed overview of implementation timelines and effective dates in Anand (2011) or DavisPolk (2010).

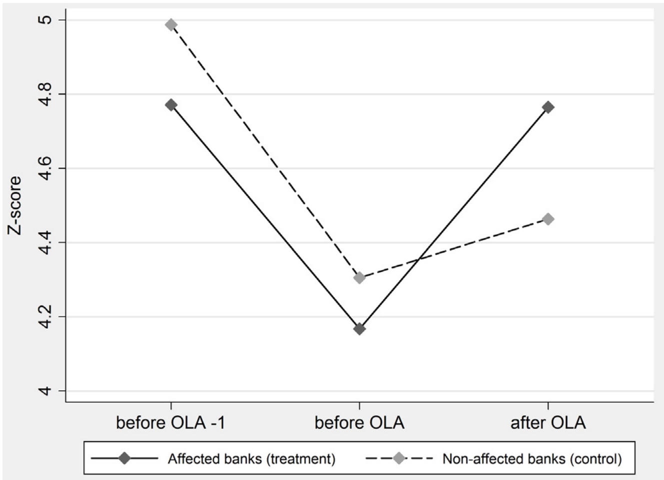

Fig. 1. Bank risk-taking before and after OLA (8-quarter periods).
This figure plots the *z-score* (defined as return on assets plus capital ratio divided by the standard deviation of return on assets and computed over 8-quarter periods) over time for both treatment and control group. The treatment group comprises affected banks that are part of a BHC with more than 30% of non-FDIA-regulated assets. The control group comprises non-affected banks 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), with before OLA-1: 2005Q3–2007Q2; before OLA: 2007Q3–2009Q2; after OLA: 2010Q3–2012Q2.

level (in addition to using the BHC level as a robustness check), at which these effects should not be pronounced. Instead, the general business models of insured depository banks (whether belonging to an affected or non-affected BHC) should be far more comparable – while specific risk-taking could still be influenced by the affected or non-affected holding company.

Still, one might argue that observed changes on bank risk-taking after the introduction of the OLA may be driven by changes that (a) did not take effect simultaneously to the OLA but were already known or anticipated and (b) affected banks differently depending on a variable that is closely proxied by the share of FDIA-regulated assets. For example, the Volcker Rule might have influenced bank behavior already at the time of passing of the OLA although it was scheduled to take effect years later. To the extent that the FDIA-regulated share is a close proxy for affectedness by the Volcker Rule, our estimates might pick up effects of the Volcker Rule. Hence, we conduct additional robustness tests for such alternative explanations.

Finally, to the extent that parallel changes in regulatory behavior might also have affected banks' risk-taking proportionally to their non-FDIA-regulated share, we would also detect their effect in our estimates. Regulatory attention to mostly non-FDIA-regulated institutions admittedly increased with the introduction of the new resolution law. Hence, it is important to note 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?

Fig. 1 provides a first indication 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. As a measure for bank risk, we use the average *z*-score, which is a composite measure approximating the distance to default, i.e., higher *z*-scores indicate less overall bank risk.[16] We depict the average *z*-score of each group as a measure for overall bank risk and evaluate it 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 both pre- and post-treatment around the treatment period as defined above (Q3 2009–Q2 2010).

Admittedly, this figure provides 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. First, the differential behavior of affected and non-affected banks around the treatment is evident. The affected banks experience a much stronger increase in the *z*-score between the pre-treatment and the post-treatment periods. Additionally, one 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 an additional period of data before the pre-treatment period. Indeed, we find a parallel trend before the treatment: Affected and non-affected institutions develop approximately in parallel in the absence of treatment. 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 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.

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 Eq. (1).

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

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 (γ_i) and time (δ_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).[17] The model is complemented by a set

[16] Refer to the following section for a detailed description of the computation of the *z*-score.
[17] Note that the variable $afterOLA_t$ drops out of Eq. (1) when including time fixed effects.

of control variables ($X_{i,t}$) to control for additional covariates that might vary over both time and treatment/control group and influence bank behavior. We cluster the standard errors at the bank level to account for possible autocorrelation. 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 β_3.

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. 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 financial data that any U.S. bank with a state or national charter is required to file on a quarterly basis. We construct a sample that contains the full set of banks (up to 8870 individual institutions) and financial data for the period covering the third quarter of 2005 to the second quarter of 2012.[18] 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 FED Chicago. Our sample contains the full set of BHCs (up to 5,886 individual institutions) and selected financial data for the period covering the third quarter of 2005 to 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 variables as defined below. Table 1 (panels A and B) provides summary statistics of the data, Table 10 in the Appendix A provides an overview of variable sources and definitions.

4.2.1. 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 $Z = (RoA + CAR)/\sigma RoA$, where *RoA* is the return on assets, *CAR* is the capital asset ratio, and σRoA is the estimated standard deviation of the return on assets.[19] The standard deviation of return on assets are computed over 8-quarter periods.[20] The *z*-score has been widely used in the empirical literature as a proxy for overall bank risk (Dam and Koetter, 2012; Gropp et al., 2014; Laeven and Levine, 2009; Roy, 1952). Essentially, the *z*-score captures two channels through which a reduction in overall bank risk can take place, i.e., asset and liability side, measuring the number of standard deviations by which a bank's return on assets would have to fall 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).

In addition, we use the average asset risk as an alternative overall risk measure. *Asset risk* 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 as a measure for overall bank risk in several previous empirical studies (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.

4.2.2. Dependent variables (II): overall bank risk (market data)

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 individual banks. However, we find stock market data for 471 listed BHCs that we accessed via Thomson Reuter's Datastream.[21] Hence, we also construct a proxy for overall bank risk based on stock market data. Following Konishi and Yasuda (2004) and Laeven and Levine (2009), we define risk as the volatility of stock returns, *σStock*, which we compute on a quarterly basis as the standard deviation of weekly stock returns using Datastream's total return index.

4.2.3. 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., 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 toward 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,

[18] We divide the sample in three main time periods each stretching over the eight quarters: (i) pre-pre-treatment period from the third quarter 2005 to the second quarter 2007 (used in a placebo test), (ii) pre-treatment period from the third quarter 2007 to the second quarter 2009, and (iii) post-treatment period from the third quarter 2010 to the second quarter 2012.

[19] We follow Laeven and Levine (2009) in computing the natural logarithm of the *z*-score and using it throughout our analyses. Because the *z*-score is highly skewed, its natural logarithm is assumed to be approximately normally distributed.

[20] Note that these periods are defined in analogy to the $afterOLA_t$ periods as explained in the explanatory variables section.

[21] Since almost all of the listed companies are BHCs, we can only conduct our market data tests on the BHC level.

Table 1
Summary statistics.
This table presents summary statistics, reporting variable names, means, standard deviations, minimum and maximum values, and the number of observations for which data is available in our sample. Unless otherwise stated, the data is reported in percentages, and dummy variables take values of 0 or 1. 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).

Variable group and name	Mean	SD	Min	Max	*N*
Panel A: BHC sample					
Dependent variables					
Bank *z*-score	4.77	1.16	0.65	7.31	67,296
Asset risk (RWA/assets)	73.88	11.73	33.46	100.79	25,510
σ Stock (total return index)	5.34	4.86	0	33.09	9299
Explanatory variables					
Unregulated share (BHC)	12.1	7.39	0	70.85	72,097
Affected BHC dummy (treatment)	0.05	0.22	0	1	19,467
Affected BHC dummy (placebo)	0.05	0.22	0	1	21,942
After OLA dummy	0.49	0.5	0	1	46,569
After placebo dummy	0.48	0.5	0	1	49,471
Additional bank- and quarter-varying control variables					
Total assets (in USD mn)	4737	66,962	0	2,358,266	72,097
Capital ratio	9.79	4.41	0	41.06	68,974
Earnings (RoA)	0.16	0.35	−2.22	1.03	68,926
Liquidity ratio	5.68	5.57	0.78	40.7	67,551
Deposit ratio	69.09	10	15.67	87.47	70,077
Non-performing loan ratio	3.13	3.53	0	23.69	25,724
Real estate loan ratio	74.68	15.62	3.39	99.71	25,724
CPP recipient bank-quarter	0.04	0.19	0	1	72,097
Panel B: Bank sample					
Dependent variables					
Bank *z*-score	4.67	1.08	1.17	6.85	139,714
Asset risk (RWA/assets)	67.8	15.17	20.76	101	141,380
Explanatory variables					
Unregulated share (parent BHC)	7.73	8.69	0	70.85	141,618
Affected bank dummy (treatment)	0.03	0.17	0	1	56,467
Affected bank dummy (placebo)	0.03	0.18	0	1	63,756
After OLA dummy	0.46	0.5	0	1	89,549
After placebo dummy	0.48	0.5	0	1	100,206
Additional bank- and quarter-varying control variables					
Total assets (in USD mn)	1912	33,105	0.07	1,842,569	141,618
Capital ratio	12.07	7.66	3.49	71.44	140,827
Earnings (RoA)	0.13	0.45	−2.74	1.64	140,826
Liquidity ratio	6.29	6.69	0.31	46.21	141,065
Deposit ratio	68.64	12.27	1.34	89.28	140,824
Non-performing loan ratio	3.18	3.6	0	24.1	140,252
Real estate loan ratio	73.64	20.82	0	100	140,263
CPP recipient bank-quarter	0.03	0.16	0	1	141,618
Panel C: Loan sample					
Dependent variables					
Loan-income-ratio (orig. loans)	2.26	1.28	0.04	7.12	1,249,901
Loan-income-ratio (unsold loans)	1.69	1.34	0.04	7.12	416,966
Loan-income-ratio (non-securit.)	2.24	1.32	0.04	7.12	756,721
Explanatory variables					
Affected bank dummy (treatment)	0.40	0.49	0	1	1,249,901
After OLA (2011/2009)	0.40	0.49	0	1	1,249,901
Additional bank control variables					
Total assets (in USD mn)	457,662	690,190	68	1,788,146	1,249,901
Capital ratio	9.37	2.48	4.94	19.33	1,249,901
Earnings (RoA)	0.04	0.32	−1.13	0.73	1,249,901
Liquidity ratio	6.47	3.80	0.44	24.66	1,249,901
Deposit ratio	62.91	13.31	3.56	89.12	1,249,901
Non-performing loan ratio	6.47	4.58	0.44	21.21	1,249,901
Real estate loan ratio	74.91	18.54	25.07	100	1,249,901
Additional loan, borrower, demographic, and economic control variables					
Government-guaranteed/-insured loan	0.36	0.48	0	1	1,249,901
Borrower sex (female)	0.28	0.45	0	1	1,249,901
Borrower race (non-white)	0.13	0.34	0	1	1,249,901
Total population in tract	5541	2606	990	17,189	1,249,901
Minority population in tract	21.81	22.55	1.07	99.54	1,249,901
Median family income (in USD)	67,256	13,674	32,000	106,100	1,249,901
House price index level in MSA	184.66	29.74	119.27	259.1	1,249,901
House price index appreciation in MSA	−3.85	3.81	−16.73	3.1	1,249,901

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. An overview of the total number of banks and BHCs, along with a breakdown into treatment and control group, and the additional observations used in the continuous robustness tests is provided on a quarterly basis in Table 11 in the Appendix B.

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.

In addition to the main explanatory variables, we control for a host of additional covariates that might influence bank risk-taking and that vary over banks and quarters (i.e., that are not captured by the bank and time fixed effects in our model). Most of these are standard in the empirical banking literature. In detail, these are total assets as a proxy for bank size, capital ratio (equity capital to total assets), return on assets as a proxy for earnings capability, liquidity ratio (cash and balances at other depository institutions to total assets), deposit ratio (deposits to total assets), as well as non-performing loan ratio (non-performing loans to total loans) and real estate loan ratio (real estate loans to total loans) as proxies for portfolio quality and composition. 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., from the Capital Purchase Program (CPP) as part of the Troubled Asset Relief Program (TARP) (Black and Hazelwood, 2013; Duchin and Sosyura, 2013). 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.

To address concerns about outliers, we winsorize our variables with one percent at their highest and lowest quantiles.

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), borrower 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. 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.[22] 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 originated loans for the years 2009 to 2011 from the FFIEC.[23] We remove several sub-samples from the raw data. First, 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). Second, we eliminate all originated loans 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).[24] Third, we ignore all banks with less than 10 originated loans per year to focus mainly on banks that are active in the home mortgage market. 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.[25] In a final step, we match this dataset with the bank level dataset based on an individual and universal bank identifier to detect the treatment and control groups and to derive bank control variables.[26] We use the bank level dataset because mortgage loans are almost exclusively granted through bank subsidiaries or individual banks.[27] Panel C of Table 1 provides summary statistics for the resulting loan sample, Table 10 in the Appendix A provides an overview of variable sources and definitions.

4.3.1. Dependent variables

We calculate the loan-to-income ratio (*LIR*) of each loan as the main risk measure in the loan level dataset. The *LIR* represents the borrower'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.[28] Therefore, we are confident that the loan-to-income ratio is an appropriate risk measure in our loan

[22] 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%.

[23] 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.

[24] Moreover, refinancing loans could be biased because of an 'evergreening' effect: Refinancing loans can exhibit a higher risk pattern when intended to prolong non-performing loans that would be otherwise written off.

[25] We use data for State Nonmetropolitan Areas when information regarding MSA is missing.

[26] 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.

[27] We identify two lenders with BHC status. For consistency, we exclude those observations from our analyses.

[28] FICO scores are provided by the Fair Isaac Corporation and measure a borrower's creditworthiness before obtaining a mortgage loan.

Table 2
Bank risk-taking: univariate difference-in-difference analyses.
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 return on assets plus capital ratio divided by the standard deviation of return on assets), *asset risk* (defined as risk-weighted assets divided by total assets), and *σ Stock* (defined as standard deviation of the weekly total stock return). Difference-in-difference estimates are displayed in column (7). Standard errors are reported in parentheses, significance levels are indicated by $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Dep. variable	(1) Affected	(2)	(3) = (2) − (1)	(4) Non-affected	(5)	(6) = (5) − (4)	(7) = (3) − (6)
	Before OLA	After OLA	Dif	Before OLA	After OLA	Dif	Dif-in-Dif
Panel A: Bank level							
z-Score	4.153	4.754	0.601***	4.303	4.462	0.159***	0.442***
			(0.0572)			(0.0102)	(0.0633)
Asset risk	0.688	0.633	−0.0547***	0.681	0.630	−0.0512***	−0.00352
			(0.0105)			(0.0013)	(0.00805)
Panel B: BHC level							
z-Score	4.078	4.536	0.458***	4.189	4.371	0.182***	0.276***
			(0.0854)			(0.0193)	(0.0972)
Asset risk	0.706	0.637	−0.0685***	0.762	0.682	−0.0798***	0.0113
			(0.0142)			(0.00289)	(0.0106)
σ Stock	0.0860	0.04	−0.0459***	0.0855	0.0803	−0.0052	−0.0407***
			(0.00681)			(0.00373)	(0.0102)

sample. To address concerns about outliers, we winsorize the loan-to-income ratio with one percent at its highest and lowest quantile, so that *LIR* ranges between 0.04 and 7.12 in our prepared sample.

4.3.2. 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 loans in 2011 and to 0 for all loans in 2009.[29]

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, liquidity, funding, and portfolio quality and composition. To capture further individual bank characteristics, we exploit bank fixed effects.[30] Second, we add a dummy variable to control whether the loan is government-guaranteed or government-insured.[31] Third, we incorporate borrower characteristics such as the sex and the race/ethnical background. Fourth, we control for demographic conditions by adding the log of total population and the share of minority population for each U.S. Census tract. Fifth, 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 of median family income and the change and average level of the house price index for each MSA. To address concerns about outliers, we winsorize all continuous control variables with one percent at their highest and lowest quantiles. 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.

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. We present several extensions, for example, evaluating the effects on loan origination and conducting tests for a moderation of the effect by bank size. Finally, these results are complemented by robustness tests, e.g., testing the parallel trend assumption using a placebo treatment event, and tests for alternative explanations.

5.1. Baseline results – tightening resolution regime and bank risk-taking

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.

For both the affected and non-affected institutions, we compute the means of the overall bank risk measures before and after 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 decreasing – 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 toward less risk-taking that is influenced by, e.g., macroeconomic trends.[32] To test our hypothesis of a significant

[29] Because the calendar year is the only time designation in the HMDA dataset, we cannot match loans to particular quarters.

[30] We do not include a variable indicating if a bank was recipient of the TARP CPP program in a respective quarter because the data in the loan level dataset is not time-varying on quarterly basis. However, the fact if a bank has received CPP funding is captured in the bank fixed effects.

[31] 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 borrowers to obtain mortgage loans that would otherwise not be affordable.

[32] 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.

Table 3
Bank risk-taking: multivariate difference-in-difference analyses.
This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk. *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 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 return on assets plus capital ratio divided by the standard deviation of return on assets), *asset risk* (defined as risk-weighted assets divided by total assets), and *σ 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, deposit ratio, NPL ratio, RE loan 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. Standard errors are clustered at the bank level and reported in parentheses, significance levels are indicated by $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Level	(1) Bank level	(2)	(3) BHC level	(4)	(5)
Dep. variable	*z*-Score	Asset risk	*z*-Score	Asset risk	σ Stock
Affected bank	0.185*	0.0232**			
	(0.0978)	(0.0117)			
Affected BHC			0.195	0.00562	−0.0345*
			(0.192)	(0.0410)	(0.0195)
Affected bank × after OLA	0.530***	−0.0229***			
	(0.0931)	(0.00862)			
Affected BHC × after OLA			0.467**	−0.0178*	−0.0298***
			(0.229)	(0.0103)	(0.00712)
Constant	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Observations	52,128	52,346	4881	5034	1263
R-squared	0.789	0.891	0.864	0.897	0.676

difference between the treatment and control groups, we compute the univariate difference-in-difference results in column (7). Interestingly, for the *z*-score, the treatment group experiences a significantly larger decline in overall risk between pre- and post-treatment compared to the control group – both on the bank and BHC level. Looking at *σStock*, we also find a significantly larger decline for the treatment 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.[33]

Table 3 presents the results of the multivariate difference-in-difference estimations. 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) and negative for asset risk (i.e., less average risk), and statistically significant at the 1 percent level for both risk measures. These results hold both at the level of individual banks and (with less significance) at the level of BHCs and strongly support our main hypothesis. In addition, using *σStock* as the dependent variable results in a negative and highly significant coefficient on the interaction term, indicating that the stock return volatility of affected BHCs decreases more strongly than the volatility of less affected BHCs after the introduction of the OLA. Beyond statistical significance, the results also suggest an economically considerable impact: Evaluating the multivariate difference-in-difference estimates, we find affected banks to increase their *z*-score by more than 7% on average, while non-affected banks hardly change.

Taken together, the presented tests on overall bank risk 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. Risk taking in new loan origination

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 4 presents the results using the loan-to-income ratio as a 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 rerun our analysis for the sub-sample of loans that have not been sold in the same calendar year (column (2)). We assume that these loans have been held on balance sheet at least for a certain time period so that they measure risk-taking more accurately. We find that loan-to-income ratios in the sub-sample of new unsold loans decrease at affected banks after the introduction of the OLA, however the coefficient for the interaction term is only significant at 10% level.[34] 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 for which this ratio of synthetic loans

[33] 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).

[34] As the coefficient for $afterOLA_t$ is positive and highly significant, we suppose that banks face increasing difficulties to sell loans in the secondary market and might need to keep risky loans on their balance sheet.

Table 4
Risk taking in new mortgage loan business: multivariate difference-in-difference analyses.
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. Unsold loans are originated loans that were not sold in the calendar year of origination. Non-sec. banks are banks where the ratio of mortgage loans securitized but with servicing retained to total mortgage loan portfolio is less than 30%. *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, liquidity ratio, deposit ratio, NPL ratio, and RE loan ratio. Loan control variables comprise an indicator equal to 1 if the loan is guaranteed or insured by the government. Borrower control variables comprise two indicator variables: borrower sex equal to 1 if the borrower is female and borrower race equal to 1 if the borrower is a non-white. 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. Standard errors are clustered at the bank level and reported in parentheses, significance levels are indicated by $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

	(1)	(2)	(3)
Level	Loan level		
Sample	Full sample	Sub-samples	
	All loans	All unsold loans	All loans from non-sec. banks
Dep. variable	Loan-to-income ratio		
Affected bank	−0.736***	−0.665***	−0.724***
	(0.207)	(0.251)	(0.221)
After OLA	0.00201	0.0547***	−0.0131
	(0.00822)	(0.0113)	(0.0104)
Affected bank × after OLA	−0.0608***	−0.0418*	−0.0378**
	(0.0141)	(0.0249)	(0.0148)
Constant	YES	YES	YES
Bank controls	YES	YES	YES
Loan controls	YES	YES	YES
Borrower controls	YES	YES	YES
Demographic controls	YES	YES	YES
Economic controls	YES	YES	YES
Bank FE	YES	YES	YES
Tract FE	YES	YES	YES
Observations	1,249,901	416,966	756,721
R-squared	0.309	0.349	0.334

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 newly originated loans after the introduction of the OLA (column (3)).

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 loan decisions toward more prudent behavior.

5.3. 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, 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.

We assume that bank size alleviates the credibility of the resolution threat to financial institutions. The argument is simple: Winding down a larger institution might produce high liquidity costs, making discipline less favored by regulators, which ultimately results in lower credibility of the threat of resolution – even after 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 warrants a closer analysis.

Hence, we separately test whether extraordinarily large or systemically important institutions are responsive to the improvement in resolution technologies. We test a specific definition of systemic importance by forming 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 enhanced regulatory activities and prudential standards, also in conjunction with the OLA (compare, e.g., DFA, Title II, Sec 210). Since there are only few institutions included in our sample, we additionally use the continuous indicator of ‘treatment intensity’ as an explanatory variable for robustness. Furthermore, we are able to conduct these tests on our bank level sample only; the results are reported in Table 5.

As a first observation, the coefficients of interest are not significant any longer in the model that uses the treatment dummy as explanatory variable (columns (1) and (2)). Interestingly, in the models that use the continuous treatment intensity (columns (3) and (4)), the coefficients on the interaction term even turn to the 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. If they change at all, 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

Table 5
Too-big-to-fail effect: multivariate difference-in-difference analyses on TBTF banks.
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-fail. The estimations are conducted on the subsample of banks with total asset size of USD 50 billion or more. *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. *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. Two measures of overall bank risk are taken as dependent variables: *z-score* (defined as return on assets plus capital ratio divided by the 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, deposit ratio, NPL ratio, RE loan 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. Standard errors are clustered at the bank level and reported in parentheses, significance levels are indicated by $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

	(1)	(2)	(3)	(4)
Level	Bank level			
Sample	Asset size USD 50+billion			
Dep. variable	*z*-Score	Asset risk	*z*-Score	Asset risk
Affected bank	1.277	0.0121		
	(0.978)	(0.0511)		
Affected bank × after OLA	0.553	−0.00227		
	(1.419)	(0.0248)		
Unregulated share (parent BHC-level)			1.969***	0.0548
			(0.755)	(0.0629)
Unregulated share × after OLA			−1.501	0.0776*
			(0.981)	(0.0446)
Constant	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Observations	195	197	399	401
R-squared	0.787	0.961	0.826	0.955

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. So, in essence, too-big-to-fail-banks were never really treated and did not have to expect treatment – inducing them to respond by unchanged or even increased risk-taking.

Taken together, our results suggest that the OLA as a particular change in the resolution regime is not a panacea to discipline banks that are deemed too-big-to-fail.

5.4. Additional robustness tests and alternative explanations

The results presented above are found to be robust to various alterations. First, we have tested our model using alternative proxies for overall bank risk-taking, yielding similar results.[35] Second, we have used accounting as well as market data to confirm our findings. Third, we have run our tests both on the BHC level as well as on the individual bank level to control for similarity of business models. Finally, we have tested all of our models in alternative specifications including and excluding the controls and fixed effects, finding consistent results.[36] These findings indicate that the results are not driven by specific definitions of variables, the level of aggregation, or alternative specifications.

Moving beyond these alterations, the following sections test the identifying assumption of our model, evaluate concerns about sample attrition, and expose our findings to alternative explanations. For brevity, we present these additional tests only for our baseline results from Table 3.

5.4.1. Using continuous treatment intensity

We acknowledge that the treatment variable $AFFECTED_i$ is defined along arbitrary cutoffs. To test the robustness of our results, we have also defined alternative cutoffs (0%, i.e., fully independent deposit-taking institutions, 5%, 10% on the lower bound and 30% and 50% on the upper bound) and found consistent results. Beyond these admittedly 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. This can be understood as a proxy for treatment intensity. As before, we include bank and time fixed effects as well as time-variant controls in our estimation. The results are displayed in Table 6 and are very much in line with the dummy results in Table 3. Again, the coefficient on the interaction term indicates a significant increase in overall bank stability and a significant decrease in overall bank risk and stock volatility.

5.4.2. Applying a placebo treatment

The analyses presented thus far have shown a significant difference-in-difference effect. However, the validity of the difference-in-difference approach 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 Section 3, 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 the 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 7. Indeed, no significant difference-in-difference effect is found for the *z*-score (columns (1) and (3)) and asset risk (columns (2) and (4)) measures, neither at the bank nor at the BHC level. Using

[35] With regard to the definition of the pre- and post-treatment periods, we have also 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.

[36] Note that for brevity, the tables display the results controlling for the most comprehensive set of fixed effects and control variables.

Table 6
Bank risk-taking (Robustness I): multivariate analyses using continuous treatment variable.
This table presents multivariate estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk, using a continuous explanatory variable interaction. *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 return on assets plus capital ratio divided by the standard deviation of return on assets), *asset risk* (defined as risk-weighted assets divided by total assets), and *σ 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, deposit ratio, NPL ratio, RE loan 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. Standard errors are clustered at the bank level and reported in parentheses, significance levels are indicated by $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Level	(1) Bank level	(2)	(3) BHC level	(4)	(5)
Dep. variable	*z*-Score	Asset risk	*z*-Score	Asset risk	σ Stock
Unregulated share (parent BHC-level)	0.900***	0.0887***			
	(0.147)	(0.0145)			
Unregulated share (BHC-level)			3.159***	0.0305	0.0707*
			(0.916)	(0.0388)	(0.0379)
Unregulated share × after OLA	1.035***	−0.0727***			
	(0.127)	(0.0108)			
Unregulated share × after OLA			1.847***	−0.0438*	−0.0659***
			(0.556)	(0.0225)	(0.0166)
Constant	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Observations	82,788	83,061	13,013	13,192	4,626
R-squared	0.757	0.884	0.802	0.875	0.640

market data in column (5) generates a similar finding. This insignificant placebo effect is consistent with the parallel trend assumption.

5.4.3. *Testing for alternative explanations*

Although our results may be robust to the technical tests and alterations described above, there might be various other alternative explanations for our findings.

First, we might simply find a larger reduction in overall risk for treatment banks because more risky treated banks exited the sample during treatment. If that were the case, our results would be driven by sample attrition. In order to test this, we exclude banks that exit the dataset due to failure, identifying them from the FDIC's failed bank list. In addition, we also run our model on a subsample that excludes all banks that exited during the observation horizon, be it due to failure or any other reason (e.g., merger). The results are displayed in columns (1) to (4) of Table 8 and are found to be very consistent with our baseline results. Hence, we exclude sample attrition as a potential driver of our findings.

Second, as the treatment group enters treatment with distinctly higher risk measures (see, e.g., Fig. 1), this might evoke concerns about non-linear responses to insolvency threats driving our findings. Such a solvency constraint is more likely to be binding for the banks that already experience higher risk levels before treatment (i.e., the treatment group). Those banks might react

Table 7
Bank risk-taking (Robustness II): multivariate difference-in-difference analyses with placebo test.
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 return on assets plus capital ratio divided by the standard deviation of return on assets), *asset risk* (defined as risk-weighted assets divided by total assets), and *σ 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, deposit ratio, NPL ratio, RE loan 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. Standard errors are clustered at the bank level and reported in parentheses, significance levels are indicated by $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Level	(1) Bank level	(2)	(3) BHC level	(4)	(5)
Dep. variable	*z*-Score	Asset risk	*z*-Score	Asset risk	σ Stock
Affected bank	0.222***	0.00568			
	(0.0837)	(0.00833)			
Affected BHC			0.0921	0.0610***	0.0775**
			(0.995)	(0.0187)	(0.0347)
Affected bank × after placebo	0.0133	0.00326			
	(0.0766)	(0.00438)			
Affected BHC × after placebo			−0.132	−0.00677	0.0125
			(0.201)	(0.00576)	(0.00866)
Constant	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Observations	59,296	59,577	7261	7321	1957
R-squared	0.761	0.914	0.851	0.933	0.608

Table 8
Bank risk-taking (Robustness III): multivariate difference-in-difference analyses with tests for robustness and alternative explanations.
This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk, performing several robustness checks and testing for alternative explanations. *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 return on assets plus capital ratio divided by the 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, deposit ratio, NPL ratio, RE loan 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. Columns (1) to (4) report the results from our model run on subsamples that control for sample attrition. We exclude banks that either failed at any point in the observation horizon according to the FDIC failed bank list or exited the sample for any reason (e.g., failure, merger). Columns (5) and (6) report the results of our model run on a matched sample. To test for potential non-linearity by the solvency constraint of banks, we match treatment and control banks on pre-treatment *z*-scores and asset risk respectively using 1:1 matching. In columns (7) and (8) we run our model including an alternative explanation by the Volcker Rule. As a proxy for affectedness by the Volcker Rule we use the *trading assets ratio*, which is defined as the ratio of assets held in trading accounts to total assets. Columns (9) and (10) test for another alternative explanation by excluding all banks that are part of a BHC that was affected by the Federal Reserve stress tests. Standard errors are clustered at the bank level and reported in parentheses, significance levels are indicated by $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Level	Bank level									
Robustness test	Sample attrition excl. failed banks		Sample attrition excl. exited banks		Solvency constraint Matched sample		Altern. explanation Volcker Rule		Altern. explanation Stress tests	
Dep. variable	*z*-Score	Asset risk	*z*-score	Asset risk	*z*-Score	Asset risk	*z*-Score	Asset risk	*z*-Score	Asset risk
Affected bank	0.183*	0.0237**	0.145	0.0278**	0.333***	0.0366**	0.191*	0.0236**	0.226**	0.0270**
	(0.0999)	(0.0121)	(0.103)	(0.0117)	(0.108)	(0.0143)	(0.0977)	(0.0118)	(0.0975)	(0.0117)
Affected bank × after OLA	0.508***	−0.0230***	0.578***	−0.0264***	0.487***	−0.0277**	0.512***	−0.0238***	0.336***	−0.0351***
	(0.0922)	(0.00862)	(0.0947)	(0.00915)	(0.151)	(0.0116)	(0.0953)	(0.00883)	(0.0955)	(0.00880)
Trading assets ratio							−0.177	0.0555		
							(0.721)	(0.0842)		
Trading assets ratio × after OLA							2.443**	0.123		
							(1.077)	(0.140)		
Constant	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	51,059	51,251	49,866	50,012	2,689	2,718	52,128	52,346	51,911	52,129
R-squared	0.782	0.890	0.784	0.891	0.817	0.910	0.789	0.891	0.790	0.891

more aggressively in decreasing their overall risk. To eliminate concerns about the solvency constraint causing a non-linear reaction, we match treatment and control banks on pre-treatment *z*-scores and asset risk respectively. We use 1:1 matching, resulting in a matched sample with pre-treatment risk measures that are indistinguishable between the treatment and control groups. Running our model on this matched sample yields results that are similar to our baseline findings (see columns (5) and (6) of Table 8). We conclude that our findings do not appear to be driven by non-linear responses to the solvency constraint.

Third, one could still argue that the observed effects could be driven by other regulatory changes introduced simultaneously to the OLA and affecting bank risk-taking proportionally to FDIA-regulated assets or a close proxy thereof. As argued in Section 3.3 above, this seems very unlikely. However, there were clearly other major regulatory overhauls passed at the time of the OLA, with the Volcker Rule arguably among the most important. Although it is still not fully implemented, to the extent that (a) the Volcker Rule might have influenced bank behavior already at the time of passing of the OLA (i.e., in anticipation) and (b) the non-FDIA-regulated share is a close proxy for affectedness by the Volcker Rule, our findings might pick up effects of the latter. To exclude such an alternative explanation, we define the share of assets held in trading accounts as a rough proxy for the affectedness by the Volcker Rule and include this variable as well as its interaction with $afterOLA_t$ into our baseline model. The results are presented in columns (7) and (8) of Table 8. If it were not the OLA that is driving our results but the proposed Volcker Rule, we would expect an insignificant coefficient on the interaction $afterOLA_t * AFFECTED_i$. This is explicitly not the case as the coefficient on the difference-in-difference term remains nearly unchanged in economic size and statistical significance. The Volcker Rule (if correctly proxied) does not seem to drive the hypothesized effect of the OLA. Rather, the direction and significance of the effect of the Volcker Rule itself does not seem conclusive.[37]

Fourth, apart from changes in the regulatory framework, the stress tests conducted by the Federal Reserve System (i.e., the Supervisory Capital Assessment Program) also took place shortly before the treatment period. These tests could clearly drive bank behavior and risk-taking, particularly for banks that were found to require additional capital. To the extent that these stress tests affected banks or BHCs with a particularly large part of non-FDIA-regulated assets, our findings could simply be driven by the stress tests – providing for yet another alternative explanation. To construct a simple robustness test, we identify all BHCs that were affected by the Supervisory Capital Assessment Program as well as the banks belonging to these BHCs. We exclude these from our sample and rerun the baseline model. Columns (9) and (10) present the results, still displaying a strongly significant decrease in overall risk for the treatment banks after the treatment. We conclude that our findings are unlikely to be driven by banks that were affected by the Federal Reserve's stress tests.

In addition, we conduct all robustness tests for alternative explanations on the sample of all institutions with asset size larger than USD 50 billion. The results are reported in Table 9 and generally confirm our findings regarding the largest and most systemically important banks.

[37] While the effect of the Volcker Rule could indeed be a reduction in risk for the affected institutions, there are also competing theories, e.g., theories of gambling could explain a reverse effect as the Volcker Rule was predicted to not become effective for years to come. Compare, e.g., Fischer et al. (2012) for gambling evoked by regulatory changes that only become effective in the long run. We do not claim to provide a definitive interpretation here, but rather leave this to future research.

Table 9
Robustness tests for too-big-to-fail effect.
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-fail, performing several robustness checks and testing for alternative explanations. The estimations are conducted on the subsample of banks with total asset size of USD 50 billion or more. *Unregulated share* is defined as the share of non-FDIA-regulated assets at the parent BHC level. *After OLA* is 1 for the quarters Q3 2010–Q2 2012 and 0 for the quarters Q3 2007–Q2 2009. Two measures of overall bank risk are taken as dependent variables: *z-score* (defined as return on assets plus capital ratio divided by the 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, deposit ratio, NPL ratio, RE loan 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. Columns (1) to (4) report the results from our model run on subsamples that control for sample attrition. We exclude banks that either failed at any point in the observation horizon according to the FDIC failed bank list or exited the sample for any reason (e.g., failure, merger). Columns (5) and (6) report the results of our model run on a matched sample. To test for potential non-linearity by the solvency constraint of banks, we match treatment and control banks on pre-treatment *z*-scores and asset risk respectively using 1:1 matching. In columns (7) and (8) we run our model including an alternative explanation by the Volcker Rule. As a proxy for affectedness by the Volcker Rule we use the *trading assets ratio*, which is defined as the ratio of assets held in trading accounts to total assets. Columns (9) and (10) test for another alternative explanation by excluding all banks that are part of a BHC that was affected by the Federal Reserve stress tests. Standard errors are clustered at the bank level and reported in parentheses, significance levels are indicated by $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Level	Bank level									
Sample	Asset size USD 50+billion									
Robustness test	Sample attrition excl. failed banks		Sample attrition excl. exited banks		Solvency constraint Matched sample		Altern. explanation Volcker Rule		Altern. explanation Stress tests	
Dep. variable	*z*-Score	Asset risk	*z*-Score	Asset risk	*z*-Score	Asset risk	*z*-Score	Asset risk	*z*-Score	Asset risk
Unregulated share	1.969***	0.0548	2.160***	0.0418	2.352***	0.290***	2.099**	0.0408	−0.0358	0.125
	(0.755)	(0.0629)	(0.746)	(0.0632)	(0.679)	(0.0644)	(0.872)	(0.0678)	(1.466)	(0.0881)
Unregulated share × after OLA	−1.501	0.0776*	−1.309	0.0701	−0.258	0.208	−1.718	0.0856	0.518	0.00459
	(0.981)	(0.0446)	(1.073)	(0.0473)	(1.336)	(0.172)	(1.128)	(0.0582)	(1.834)	(0.0782)
Trading assets ratio							−1.705	−0.121		
							(1.666)	(0.212)		
Trading assets ratio × after OLA							0.452	−0.0784		
							(1.969)	(0.206)		
Constant	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	399	401	379	381	145	145	399	401	232	234
R-squared	0.826	0.955	0.814	0.956	0.943	0.948	0.827	0.955	0.812	0.972

Taken together, our robustness tests suggest that the main findings are not driven by variable definition, model specification, or sample choice, nor do they seem to be caused by various alternative explanations that we tested.

6. Concluding remarks and policy implications

We suggest the hypothesis that the tightening of bank resolution regimes, namely the introduction of the OLA that extends a special bank resolution regime to financial institutions that were previously not covered by a special bank resolution law, has a disciplining effect on bank behavior, particularly risk-taking. We propose a difference-in-difference framework exploiting the differential relevance of the OLA for different banks to test this hypothesis. First and foremost, we find the results to be consistent with our proposition: The introduction of the OLA changes the behavior of the affected financial institutions toward less risk-taking compared to the non-affected institutions. However, 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 and most systemically important institutions. This indicates that the OLA alone did not resolve the too-big-to-fail problem. Our findings are robust to various specifications and we can rule out several alternative explanations. In the absence of treatment, i.e., of the regulatory change, both the affected and the non-affected institutions behave similarly, which further corroborates our results.

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 find mixed answers to the question whether the OLA 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 U.S. regulators.

Moreover, although our analyses focus on the effects of a country-specific resolution regime, our results prompt us to also draw general implications for the ongoing discussions on the design or reform of bank resolution regimes around the world. Based on our 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 discipline and stability in the financial system. First, a bank resolution regime that takes into account the special role of financial institutions (beyond regular and often inapplicable corporate bankruptcy law) is essential, not only to avoid major disruptions in liquidity provision but also to create a credible resolution threat for financial institutions to discipline them ex ante. A credible improvement in the resolution regime should command both sufficient legal resources, i.e., the empowerment of the regulator to intervene promptly and effectively, and sufficient financial resources, i.e., a resolution fund, to increase the resolution threat to financial institutions, hence inducing more discipline. Second, comprehensive coverage of financial institutions as a whole – that extends beyond the scope of only

Table 10
Variable sources and definitions.
This table reports variable definitions and data sources. 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).

Variable	Source	Definition
Dependent variables		
Bank *z*-score	BHC, SDI	Composite measure approximating the distance to default, computed as sum of return on assets and capital ratio, divided by standard deviation of return on assets
Asset risk	BHC, SDI	Average risk weight of assets, i.e., risk-weighted assets divided by total assets
σ Stock	DS	Standard deviation of weekly stock returns using the Datastream total return index
Loan-income-ratio (orig. loans)	HMDA	Ratio of loan amount to borrower's gross annual income for the sample of all originated loans
Loan-income-ratio (unsold loans)	HMDA	Loan-income-ratio for the sample of originated loans that were not sold in the calendar year of origination
Loan-income-ratio (non-securit.)	HMDA	Loan-income-ratio for the sample of banks with less than 30% ratio of mortgage loans securitized but with servicing retained to total mortgage loan portfolio
Explanatory variables		
Unregulated share	BHC, SDI	Share of assets of a holding company not subject to the FDIA resolution regulation
Affected BHC dummy	BHC, SDI	Treatment dummy variable, takes a value of 1 if the bank is part of a BHC with more than X(30)% of unregulated asset share and a value of 0 if the bank is independent or part of a BHC with less than Y(10)% unregulated asset share.
After OLA dummy		Indicator for pre- and post-period
Control variables		
Total assets	BHC, SDI	Total assets in USD million
Capital ratio	BHC, SDI	Total equity divided by total assets
Earnings (RoA)	BHC, SDI	Return on assets, i.e., net income divided by average assets
Liquidity ratio	BHC, SDI	Cash and balances at other depository institutions divided by total assets
Deposit ratio	BHC, SDI	Deposits divided by total assets
Non-performing loan ratio	BHC, SDI	Past due and nonaccrual loans divided by total loans
Real estate loan ratio	BHC, SDI	Loans secured by real estate divided by total loans
CPP recipient bank-quarter	TR	Capital Purchase Program indicator variable, takes a value of 1 if the bank is a current recipient of CPP funds in a given quarter and 0 otherwise
Government-guaranteed/-insured loan	HMDA	Indicator whether the loan is insured by the Federal Housing Administration or guaranteed by the Veterans Administration, Farm Service Agency, or Rural Housing Services
Borrower sex	HMDA	Indicator variable, takes a value of 1 if the borrower is female and 0 otherwise
Borrower race	HMDA	Indicator variable, takes a value of 1 if the borrower belongs to any other race than white and 0 otherwise
Total population	HMDA	Total population in borrower's Census tract
Minority population	HMDA	Share of minority population in borrower's Census tract
Median family income	HMDA	Median family income (in USD) in the borrower's Metropolitan Statistical Area (MSA)
House price index level	FHFA	Average annual level of the house price index in the borrower's MSA
House price index appreciation	FHFA	Annual change rate of the house price index in the borrower's MSA

deposit-taking entities – is important to avoid incentives to shift risks into non-resolvable entities. Finally, to the extent that too-big-to-fail institutions are still unimpressed by improvements in the resolution regime, additional measures increasing their resolvability (and ultimately the resolution threat) might be required.

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 and enforces more prudent behavior.

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Appendix A.

See Table 10.

Appendix B.

See Table 11.

Table 11
Sample overview.
This table presents an overview of the sample size by categories of banks and BHCs over time. *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. The number of institutions for which stock market data is available are reported in parentheses.

Quarter	Banks					BHCs			
	Total	of which affected = 1 (treat)	of which affected = 0 (control)	of which only in continuous robustness test	Asset size USD 50+billion	Total	of which affected = 1 (treat)	of which affected = 0 (control)	of which only in continuous robustness test
Q32005	8870	153	4902	3815	32	5586	83 (14)	2116 (168)	3387 (310)
Q42005	8845	176	4936	3733	32	5886	101 (14)	2210 (166)	3575 (315)
Q12006	8802	172	4882	3748	32	5380	95 (13)	2110 (150)	3175 (325)
Q22006	8788	184	4979	3625	33	5857	116 (12)	2228 (158)	3513 (315)
Q32006	8754	168	4773	3813	33	5379	99 (12)	2053 (131)	3227 (340)
Q42006	8691	162	4686	3843	32	5848	107 (15)	2084 (122)	3657 (337)
Q12007	8660	167	4677	3816	33	5339	93 (13)	1984 (118)	3262 (336)
Q22007	8624	182	4677	3765	34	5830	117 (16)	2093 (115)	3620 (332)
Q32007	8569	173	4528	3868	36	5328	108 (15)	1924 (111)	3296 (330)
Q42007	8544	194	4493	3857	37	5817	132 (15)	1973 (112)	3712 (327)
Q12008	8504	173	4616	3715	38	5311	109 (14)	2013 (128)	3189 (300)
Q22008	8461	170	4728	3563	38	5749	116 (14)	2216 (144)	3417 (280)
Q32008	8392	158	4675	3559	38	5301	94 (13)	2119 (151)	3088 (270)
Q42008	8314	147	4655	3512	41	5691	94 (12)	2190 (142)	3407 (280)
Q12009	8256	151	4598	3507	39	5297	85 (17)	2121 (126)	3091 (286)
Q22009	8204	151	4535	3518	38	5654	91 (15)	2186 (127)	3377 (284)
Q32009	8108	152	4464	3492	37	5028	80 (15)	2074 (124)	2874 (267)
Q42009	8021	125	4551	3345	36	5572	85 (12)	2200 (140)	3287 (261)
Q12010	7943	130	4389	3424	36	5210	79 (13)	2047 (133)	3084 (257)
Q22010	7839	123	4259	3457	35	5488	82 (15)	1994 (119)	3412 (262)
Q32010	7770	112	4259	3399	37	5152	72 (14)	1965 (108)	3115 (266)
Q42010	7667	115	4307	3245	37	5406	76 (16)	2119 (108)	3211 (262)
Q12011	7583	104	4105	3374	36	4839	60 (13)	1880 (90)	2899 (269)
Q22011	7522	103	3974	3445	37	5338	73 (14)	1823 (90)	3442 (274)
Q32011	7446	88	3821	3537	37	5015	64 (13)	1673 (77)	3278 (281)
Q42011	7366	84	3777	3505	37	5284	66 (14)	1705 (79)	3513 (279)
Q12012	7317	86	3656	3575	36	5140	78 (17)	1608 (78)	3454 (276)
Q22012	7254	88	3576	3590	37	5716	102 (17)	1656 (71)	3958 (284)

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