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Debt holder monitoring and implicit guarantees: did the BRRD improve market discipline?

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Abstract

This paper argues that the European Unions *Banking Recovery and Resolution Directive* (BRRD) improved market discipline in the European bank market for unsecured debt. The different impact of the BRRD on bank bonds provides a quasi-natural experiment that allows to study the effect of the BRRD within banks using a difference-in-difference approach. Identification is based on the fact that (otherwise identical) bonds of a given bank maturing before 2016 are explicitly protected from BRRD bail-in. The empirical results are consistent with the hypothesis that debt holders actively monitor banks and that the BRRD diminished bail-out expectations. Bank bonds subject to BRRD bail-in carry a 10 basis points *bail-in premium* in terms of the yield spread. While there is some evidence that the bail-in premium is more pronounced for non-GSIB banks and banks domiciled in peripheral European countries, weak capitalization is the main driver.

Keywords: Bail-in, BRRD, Banking Regulation, Moral Hazard
JEL Codes: G18, G21, H81

1 Introduction

The financial crisis of 2007–2009 resulted in a series of unparalleled public bail-outs for the banking sector[1]. Letting banks fail and enter bankruptcy procedures was deemed to be too socially expensive. The costs of a bailout seemed to be justified given the negative externalities of bank failures, not only on financial stability but also the real economy (McAndrew et al. 2014). While those bailouts presumably restored market confidence in the short-run, they may have further weakened market discipline in the long run. A central goal for banking regulation since the crisis was therefore to provide mechanisms to ensure that equity and debt-holders participate in losses of potential future bank failures without having the respective bank to enter bankruptcy procedures (Avgouleas and Goodhart 2015). In Europe, the *Banking Recovery and Resolution Directive* (BRRD) provides tools to recapitalize banks by converting outstanding debt (unsecured bonds) into equity – a so called *bail-in*. This bail-in threat should ex-ante increase market discipline. Yet it remains unclear whether it achieved its purpose. How credible are banking regulation reforms attempting to strengthen market discipline by eliminating implicit government guarantees?

This paper answers this question by studying the impact of the BRRD on bonds subject to bail-in. It contributes to the literature as it is (to my best knowledge) the first paper that studies how a change in the regulatory architecture —that is a change in the supervisors *ability to enforce* laws, rather than either a change in laws itself or a change in banks risk profiles— is perceived by debt-holders. It capitalizes on a novel identification strategy which allows to study the effect of the BRRD on market discipline using within bank variation. Key to being able to use variation within banks is the fact that for a given bank, bonds are differently affected by the change in the bail-in regulation depending on their respective maturity. The BRRD was passed in the European Parliament on 15/04/2014 and was to be implemented to national law by member states by 01/01/2015, with the exception of the bail-in tool, which was to be implemented by 01/01/2016 to give markets enough time to adjust. Therefore consider two bonds, #1 and #2, both issued by Bank A before June 2012. Bond #1 matures, say 01.06.2016 and bond #2 matures 01.06.2015. If investors did not believe the bail-in tool to be a credible threat, then the introduction of the BRRD should not affect the yield spreads of the two bonds differently. If the BRRD is however perceived as credible, it should only increase the yield spread of

[1]Government support for banking institutions is estimated to have had a fiscal impact of 1.7% of GDP in the Euro Area from 2008–2013 (Maurer and Grussenmeyer 2015).

bond #1, because it is ex-post subject to BRRD bail-in, while bond #2 is explicitly protected. The advantage of this setting is that the variation is plausibly exogenous and that within bank variation allows to control for unobserved heterogeneity since bond level data allows for bond and bank × month fixed effects.

Employing this novel identification strategy, I am able to show that unsecured bank bonds which suffer from unexpected BRRD bail-in exposure face increased yield spreads of about 10–15 basis points compared to the control group – a pattern that cannot be observed for non-bank corporate bonds. This *bail-in premium* is mainly driven by weak capitalization. Also, the effect is less pronounced for *Globally Systemic Important Banks* (GSIB) and for banks of peripheral European countries. The empirical evidence strongly favours the hypothesis that the BRRD indeed improved market discipline, by demanding a premium for unsecured liabilities that are subject to bail-in. A battery of robustness checks is provided, including parallel trends test and placebo tests.

These results contribute to the growing literature on debt-holder monitoring, market discipline and implicit guarantees. As pointed out in Bliss and Flannery (2002) market discipline is characterized by two distinct features: Firstly, the ability of debt holders to monitor risk-taking and secondly their ability to influence the managers behaviour based on their assessment. After the financial crisis a third feature attracted increased attention: Debt holders have to ex-ante believe that they will share the burden of losses in case of a bank failure if they are supposed to have any incentive to monitor risk taking in the first place (Gropp and Vesala 2004). Hence there are two fundamental mechanisms at work here, which have been subject to intensive research in the past. The first is related to debt-holders ability to monitor and influence bank risk taking. Regarding debt-holders ability to understand the risks taken by banks, most studies try to relate market prices of bank securities (shares and bonds) to their underlying risk profile. The majority (and in particular more recent studies) conclude that investors are indeed able to price those risks (Martinez Peria and Schmukler 2001; Jagtiani et al. 2002; Goldberg and Hudgins 2002; Bennett et al. 2015). Less research has been done on debt holders' ability to influence bank risk-taking. There is evidence that both supports (Ashcraft 2008; Ignatowski and Korte 2014; Danisewicz et al. 2016) and rejects (Billett et al. 1998; Bliss and Flannery 2002)) the hypothesis that debt-holders are able to govern bank risk taking. This debate is not only of academic interest but is also important for designing optimal regulatory frameworks (Calomiris 1999). Moreover it is fuelling the debate on debt vs. equity as a disciplining mechanism in the banking sector[2]. The second relevant branch of literature in this context concerns the optimal regulatory architecture and its impact on bank risk taking. The empirical evidence tends to support the view that (expected) government support induces moral hazard (Dam and Koetter 2012; Duchin and Sosyura 2014). The time inconsistency problem at play is fairly simple. Even though ex-ante supervisors would have preferred to let banks fail, ex-post the risks and social costs associated with letting them fail seemed to justify the bailout: Letting banks enter bankruptcy procedures, it is often argued, is not a sub-game perfect strategy for the supervisor as there are states of the world in which ex-post she prefers a bail-out (Acharya and Yorulmazer 2007; DeYoung et al. 2013). This "implicit guarantee" weakens debt holders incentives to monitor bank risk taking in the first place. Quantifying and eliminating this implicit guarantee has been a major goal in banking regulation since the crisis (for an overview see Schich and Lindh (2012)). The BRRD can be thought of as an improvement in resolution technology. By converting debt into equity to recapitalize failing banks (instead of letting banks enter bankruptcy procedures), supervisors are now able to recapitalize these institutions quickly and without risking negative externalities to the real economy. This is because banks will not have to enter socially costly bankruptcy procedures if debt can be converted to equity to restore the required capital ratios (Flannery 2003). The bail-in threat should therefore induce debt-holders to monitor bank risk taking. This should not only incentivize debt-holders to monitor, but also equity holders as their shares will be written off or diluted, diminishing previously existing implicit guarantees for equity holders (Kelly et al. 2016). How credible this threat is remains, however, an open question.[3]

The remainder of this paper is structured as follows. Section 2 explains the identification strategy in greater detail. Section 3 discusses the data used. Section 4 presents the empirical results and provides various robustness checks. Finally, section 5 concludes.

2 Identification Strategy

This section presents the identification strategy employed in this paper. As outlined in the Introduction, establishing causality is one of the major challenges in the literature on market discipline. Therefore, I first provide institutional background

[2] See for example Admati et al. (2013), Hasan et al. (2015) and Jordà et al. (2017)

[3] Schäfer et al. (2016) and Raffaele Giuliana (2018) specifically discuss the credibility of the BRRDs bail-in tool by employing event study methodology based on bail-in events or news regarding legislation.

knowledge on the BRRD and in particular the scope and implementation dates of the bail-in tool. Based on this information, I explain how difference-in-difference methodology can be used in this context to study the impact of the BRRD on market discipline.

2.1 Institutional Background

Ever since the Great Recession various financial market regulation reforms have been brought in place around the world. The BRRD is the EU's effort to provide a common legislative framework to deal with banks which are failing or likely to fail. The European Commission (EC) published their first draft on 06/06/2012. Almost two years later, the final version of the directive passed the European Parliament on 15/04/2014. It defines under which conditions a banking institution is deemed to be failing or likely to fail and which consequences this can trigger. The bail-in tool is one of those. A bail-in (as opposed to a bail-out) occurs when bank creditors (e.g. investors in bank bonds) of a failing bank will either see their debt written off or converted into equity upon supervisory discretion to restore the viability of the institution.[4]

The BRRD provides a detailed account on which liabilities are subject to the bail-in tool (Article 44), the creditor hierarchy (Article 34) and the implementation dates (Article 130). Acording to Article 130 of the BRRD, the directive is to be implemented into national laws by 1 January 2015. However provisions related to Section 5 of Chapter IV of Title IV (Article 43 – 58) are to be implemented by 1 January 2016 (European Parliament 25/04/2014).[5] These articles include the provisions on the bail-in tool and the MREL requirement. This grants an explicit guarantee, that BRRD bail-in will not be applied before January 2016. Of course, debt maturing before 2016 could still suffer losses in case of a bank entering bankruptcy procedures. The change in the regulatory environment therefore does not change the general nature of a given liability. It merely adds to the authorities toolbox to enforce existing regulation. That is, it is now possible to ensure debt-holders participation in losses resulting of bank failures without closing down the bank (i.e.

[4]Under the BRRD regime, a bail-in is a necessary condition for any government support. However, it can also be used to facilitate private sector M&A solutions, such as with Banco Poplare D'Espagne (BPE) and Banco Santander, where junior debt of BPE was written off and Banco Santander purchased BPE for the symbolic price of one Euro. See for example: https://www.economist.com/finance-and-economics/2017/06/10/banco-popular-fails-and-is-bought-by-santander.

[5]The first proposal on the BRRD published by the Comission actually required no use of the bail-in tool before 1 January 2018.

without having the bank enter (socially costly) bankruptcy procedures). This can be understood as an improvement in resolution technology available to authorities. Before and after the BRRD was implemented in January 2016, unsecured bonds are potentially suffering losses from bank failure. In the pre-BRRD regime however, the social costs of forcing debt-holders to participate by letting the failed bank enter bankruptcy procedures, renders that alternative very expensive to the supervisor. Figure 2 graphically depicts how the bail-in tool is supposed to work. If a banking institution is likely too fail (e.g. expected losses are larger than equity), a BRRD bail-in would consist of firstly writing down all existing equity issues and secondly converting outstanding debt into new equity shares. Debt investors (bond holders) would thus become the new owners of the bank, potentially facing immediate losses if asset losses exceed the existing equity level.

2.2 Difference-in-Difference estimation

The staggered introduction of the BRRD serves as a quasi natural experiment in this analysis. Essentially, bonds maturing before January 2016 are protected by an explicit guarantee not to employ the bail-in tool beforehand. This is the ideal setup

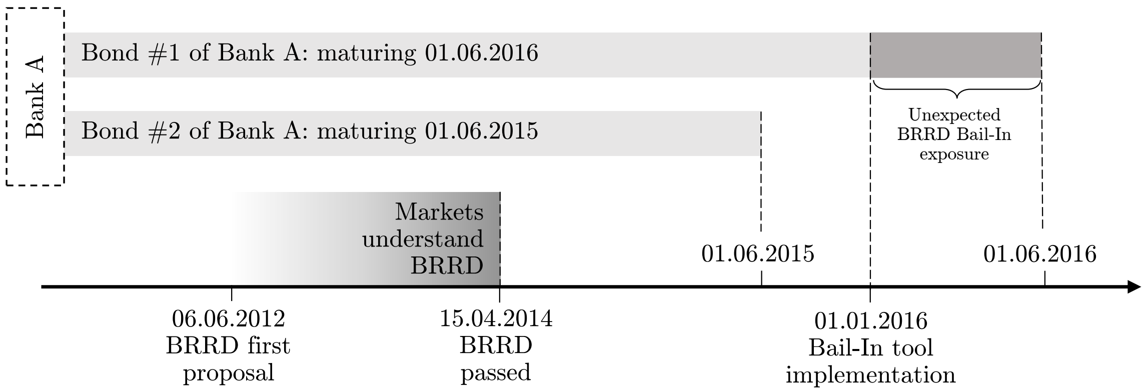

Figure 1: Identification Strategy

This graph plots the evolution of the legislative process. The European commission published the first draft of the BRRD on 06/06/2012. The European Parliament passed the final version on 15/04/2014. The law is to be implemented by 01/01/2015, except for the bail-in tool, which is to be implemented by 01/01/2016. Therefore, from a 2012 perspective, bonds #1 and #2 (both issued by Bank A) appear to have similar risk profiles. After the BRRD was passed however, investors understand that bond #1 is subject to BRRD bail-in. If the BRRD is perceived as a credible threat, it should follow that bond #1 will trade at a discount, that is face higher yield spreads compared to bond #2. This set-up is therefore able to overcome the usual identification challenge by estimating the effect using *within* bank variation.

to employ a difference-in-difference (DiD) analysis. DiD methodology is a frequently used and well documented empirical method for program evaluation (Imbens and Wooldridge 2008; Angrist and Pischke 2009)). It has been used extensivly to study the effects of policies on labor markets (Card and Krueger 2000; Autor 2003). More recently it has been applied in a similar context by Ignatowski and Korte (2014) to evaluate the introduction of the Orderly Liquidation Authority (OLA) on bank risk taking.

To employ a DiD methodology three key questions need to be answered: (1) Is the BRRD bail-in tool an improvement in resolution technology? (2) What is the control group, what is the treatment group? (3) When did the treatment take place? With regards to (1), Conlon and Cotter (2014) analyse how the BRRD would have performed during the financial crisis of 2007 – 2009. They conclude that even in very adverse scenarios, depositors would never have to be bailed-in, limiting the risks of potential bank-runs. The recent bail-in of Banco Popolare's (BPE) junior bond holders provides an excellent example of how the BRRD is meant to work and demonstrates that it indeed provides the technology to force debt holders to participate in losses. On a theoretical level, a bail-in allows to have debt holders participate in losses *without* having the banks to enter bankruptcy procedures (which are socially costly given the associated negative externalities). This should make a bail-in the preferred strategy of the supervisor.

Identifying treatment and control group is fairly simple in this context. Bonds maturing before 2016 constitute the control group, while bonds maturing in 2016 and beyond belong to the treatment group.[6]

Identifying the treatment date is more difficult. In fact, one motivation of this research is that the imprecise event dates regarding the BRRD pose a severe challenge to event studies as performed by Schäfer et al. (2016). This is a well-known issue of event study methodology trying to evaluate the effect of regulation (Binder

[6] There is a valid concern regarding bonds maturing 2017 and later, as for those bonds the identification strategy might potentially yield spurious results. Parallel to the introduction of the bail-in tool, the BRRD also forces banks to fulfil a *Minimum Requirement of Eligible Liabilities* (MREL). After all, having a bail-in tool is only helpful if there is enough outstanding debt, that is subject to bail-in. Therefore the BRRD forces banks to have certain amounts of outstanding bail-in-able securities. MREL is applicable from 01/01/2016 (just as the bail-in tool is). Therefore MREL can be considered as a positive supply shock of bailin-able bank debt. This could, all else equal, increase yield spreads of bonds maturing 2016 and later. It thus seems impossible to disentangle rising yield spreads in a *bail-in premium* and an equilibrium pricing induced *MREL premium*. There is however one important difference to note here. A bond only qualifies as MREL capital if its residual maturity is more than one year. Therefore all bonds maturing in 2016 are never part of any banks MREL capital and therefore not subject to a potential MREL premium. A robustness check verifies that the bail-in premium is also present on bonds maturing in 2016.

1985). To avoid this problem, I define three periods. The pre-treatment period spans from 01/06/2011 to 30/05/2012, i.e. one year period before the European Commission published their first proposal. The post-treatment period ranges from 01/04/2014 to 30/03/2015, i.e. a one year period after the European Parliament passed the BRRD. Observations in between are dropped from the data set, since it is impossible to tell how much information has dispersed at what point in time.

3 Data

To investigate the impact of the BRRD on bonds yield spreads, I construct a data set which is based on all bailin-able euro-denominated bond issues from banks from the European Union that were issued before 01.06.2012 and mature between 01.01.2015 and 31.12.2019 and used in Schäfer et al. (2016), except Swiss and British banks. Swiss banks are not subject to the BRRD (as Switzerland is not a member of the European Union). UK banks are not subject the Single Resolution Mechanism. This data set includes all bond-specific information which I am using in my analysis, including information on seniority level, issue date and maturity date. Table 12 describes the data selection process in detail. I use daily yield data[7] obtained from Bloomberg. For a robustness check, I also use bonds yields for European non-financial corporates. Table 13 describes the data selection process for the non-bank corporate sample in detail. The yield spread is computed as the difference of the yield and the spot rate based on euro denominated European AAA government bonds with identical residual maturity, $\text{Yield Spread}(i,t) = \text{Yield}(i,t) - \text{spotrate}(TTM(i,t))$ where $TTM(i,t)$ is the residual maturity of bond i at time t. To this end I use yield curve data from the ECBs' statistical dataware house. In particular the daily yield curve can be fitted very well using the Svenson (1995) model.[8] Finally monthly averages of the yield spreads are computed. The final sample contains 1,491 bonds of 53 banks. 581 bonds mature in 2015 and will be part of the control group, while 910 mature after January 2016 which will form the treatment group. The average bonds has remaining life of 37 months and a 2.37% yield spread.

[7]The Bloomberg variable code is `YLD_CNV_LAST`. I clean yield data the following way. All bonds that experience negative yields are dropped. The same percentage of bonds that are dropped from the "bottom" are subsequently also dropped from the top of the yield distribution. The reason for this filtering is a problem with stale price on Bloomberg. Price data is sometimes carried forward and not refreshed. Approaching maturity with fixed prices increases (or decreases if the price is above 100) the yield of the respective bond exponentially. My filtering allows me to drop bonds that experience this problem.

[8]The ECB provides daily estimates of the five Svenson model parameters which can be obtained from the ECB's statistical datawarehouse

[Insert Table 1 about here]

Additionally I gather balance sheet data from SNL Financials. In particular the Core Equity Tier 1 ratio is used to gauge bank risk.[9] Table 1 presents summary statistics.

4 Results

This section discusses my empirical results. The first subsection presents the main result of the paper, namely an increase in the yield spread for bonds suffering from BRRD bail-in exposure. I continue by exploring the cross sectional heterogeneity of my dataset and show that the bail-in premium is more pronounced for less capitalized banks. Finally, these results are complemented by a battery of robustness checks, including a parallel trends test and two placebo tests.

4.1 Baseline specification & heterogeneous effects

First the hypothesized effect of the BRRD on bond yield spreads are tested using a multivariate difference-in-difference regression. The treatment dummy is equal to zero if the bonds matures before 2016 and one otherwise. The treatment period dummy is equal to zero for the year prior to the first BRRD proposal (i.e. from June 2011 to May 2012) and equal to one after the BRRD was passed (i.e. from April 2014 to May 2015). The intuition of this specification is that bonds maturing before 2016 are protected from BRRD bail-in and therefore associated with the control group, while bonds maturing after January 2016 are subject to bail-in. The idea of the treatment period dummy variable is to define a one year *pre-treatment* period and a one year *post-treatment* period and drop all observations that happened during the legislative procedure.[10] The interaction between the two is called $\text{DiD}(i, t)$. The hypothesis is that there is no significant difference in the yield spread of the treatment and control group before the legislative process, but a significant (and

[9] To avoid endogeneity concerns, in that banks exposure to BRRD might have influenced their balance sheet structure, I use balance sheet data from 2010–2012.

[10] This is done since it is difficult to tell at which point in time information about the BRRD was released to stakeholders. My approach is robust to including the period dropped for any meaningful choice of treatment date.

positive) difference afterwards.[11] I am running the following baseline regression:

$$\begin{aligned}\text{Yield Spread}(i,t) &= \beta_0 + \beta_1 \cdot \text{DiD}(i,t) + \beta_2 \cdot \text{Treatment Dummy}(i) \\ &+ \text{Treatment Period Dummy}(t) + \beta_4 \cdot X_{it} + \gamma_i + \delta_j \times \mu_t + u_{it} \quad (1)\end{aligned}$$

where Yield Spread(i,t) is bond i's yield spread at month t. X_{it} is a vector of bond controls (in particular residual maturity) and γ_i and $\delta_j \times \mu_t$ are bond and bank–month fixed effects respectively.[12] Standard errors are clustered on bond level (throughout the paper). The coefficient of interest is β_1. If the BRRD improved market discipline, one would expect β_1 to be positive. This would suggest that a bond which matures in 2016 or later (and therefore becomes subject to BRRD bail-in regulation) is viewed as more risky (as measured by the yield spread) after the BRRD is passed than a similar bond of the same institution which matures in 2015 (and is therefore exempt from BBRD bail-in). Note that given bank × month fixed effects explicitly controlling for macro factors (such as Libor or VIX) and bank characteristics (such as size or leverage) is not necessary, as these variables would be differenced out by design. Similarly, there is no need for a treatment period dummy as it would be differenced out by the month fixed effect, just like the treatment group dummy is differenced out by bond fixed effects (used in some specifications).

[Insert Table 2 about here]

Table 2 presents the main results. Specification (1) is the simple difference-in-difference regression (without any controls or fixed effects). Specficiation (2) and (3) add month and bond fixed effects, such that the treatment group dummy and the treatment period dummy are omitted. The difference-in-difference coefficient remains significant across all specifications and is with 15 basis points economically meaningful in magnitude. These three specifications however do not leverage the possibility to exploit variation within banks. The results could therefore be driven by cross sectional differences across banks, rather than differential treatment of bonds by the BRRD. Therefore, the remaining specifications of Table 2 include bank × month fixed effects, which control for a change in the risk profiles of the bonds respective banks. This is a major benefit of my identification strategy. Since bank × month fixed effects allow to difference out any (usually unobservable) change in

[11] The identifying assumption here (parallel trends of treatment and control group in absence of treatment) is intuitively plausible and in more detail discussed in a later subsection, which includes a parallel trends test.

[12] Of course there is no need to include bank and month fixed effects separately once bank × month fixed effects are included.

bank riskiness, the change in the yield spreads must be caused by events which affects bonds of a given institution differently depending on their maturity. The positive and statistically highly significant difference-in-difference estimates in specification (4) – (7) do therefore *not* reflect an increase of the yield spread due to possibly increased riskiness of the respective bank. Instead they capture the differential impact of the BRRD on bonds maturing in 2015 (which are therefore protected from BRRD bail-in) and bonds maturing in 2016 and later (which are subject to BRRD bail-in) after the BRRD is passed compared to the pre-BRRD period. These results indicate that investors indeed perceive BRRD bail-in-able bonds to be more risky and demand a bail-in premium of about 10–15 basis points in terms of the yield spread. The most conservative specification (6) estimates a bail-in premium of 12.9 basis points, which remains virtually unchanged if one drops all observations with a remaining life of less than six months in specification (7) (to mitigate concerns regarding bond retirement effects). Given the 1.76 percentage point average bond yield spread in the sample, this is a sizeable effect of about 7% (=0.129/1.76). Investors seem to perceive bonds maturing in 2016 and later to be more risky than bonds maturing in 2015 after the BRRD was passed, compared to the period before the specifics of the BRRD were known.

As a next step I explore the cross sectional differences in my dataset. Intuitively the *bail-in premium* should be more pronounced for bonds of banks which investors perceive to be more likely to suffer from a bail-in. As explained earlier, those are the banks that are both more likely to fail and less likely to be saved. To proxy for bail-in risk I consider three categories: (i) capitalization level, (ii) GSIB status and (iii) geographic headquarter of the bank.

Since equity represents a residual claim on the assets, capital can be thought of as a distance-to-default measure. As long as equity is not wiped out, debt holders will not suffer losses. Therefore a larger capital base is beneficial to debt investors, since it —ceteris paribus— decreases the likelihood of debt default.

GSIB banks, more commonly referred to as “too-big-too-fail” banks have been studied extensively in the literature. It is argued that a failure of these institutions would endanger the financial system as a whole, which is why the markets expect that the government would not allow a failure of such an institution. Empirical evidence suggests that the “too-big-too-fail” status is linked to lower funding costs (Morgan and Stiroh 2005), suggesting implicit government guarantees for these institutions.

Further it has been shown that market discipline varies across countries. Demirgüç-Kunt and Huizinga (2013) show that banks domiciled in countries with less fiscal capacity for bail-outs are expected to be bailed-out with a lower probability. In addition it has been argued that there might be less political will to enforce stricter rules in some peripheral European countries.[13]

Table 3 presents results for sample splits of my baseline specification described in equation (1). It indicates that the bail-in premium is more pronounced for (i) weakly capitalized[14] banks (13.5 vs. 11.6 basis points), (ii) banks domiciles in peripheral countries (15.6 vs. 10.7 basis points) and (iii) non-GSIB banks (14.2 vs. 1.4 basis points). These findings are quite intuitive. The weaker the capitalization level of the banks, there higher the risk of bank failure is (ceteris paribus). If the BRRD is perceived as a credible threat, then bonds maturing in 2016+ should carry higher yield spreads for weaker capitalized banks. Similarly, if one is willing to accept the "too-big-too-fail" narrative, then one would expect a more pronounced bail-in premium for non-GSIB banks, reflecting their lower importance (and therefore the regulators higher willingness to let them fail). For banks domiciled in peripheral European countries, the effect is less clear ex-ante. On the one hand they are typically perceived as more risky (in particular regarding their exposure to non-performing loans (Messai and Jouini 2013) and the diabolic loop (Brunnermeier et al. 2016)). On the other hand, they are domiciled in countries which displayed some resistance to tighten and enforce resolution regimes.

While sample splits can provide some intuition of cross sectional variation of treatment effects, Equation 2 interacts the difference-in-difference dummy with risk the measures to quantify the heterogeneity of the effect found in Table 3.

$$\text{Yield Spread}(i,t) = \beta_0 + \beta_1 \cdot \text{DiD}(i,t) + \beta_2 \text{DiD}(i,t) \times \text{BailInRisk}(j) + \beta_4 \cdot X_{it} + \gamma_i + \delta_j \times \mu_t + u_{it} \quad (2)$$

where $BailInRisk(j)$ is either the negative average standardized CET1 ratio in 2010–2012, or a dummy variable indicating peripheral domicile or non-GSIB status. The other variables remain unchanged.

Table 4 presents the results. In column (1) of Table 4 the baseline specification (without bail-in risk interaction) is displayed again for reference. Column (2) reveals

[13] Anecdotal evidence supports this view: Stanghellini (2016) states that "*The political will was strong to solve the problem [four bankrupt Italian banks] before 1 January 2016 [that is, before the bail-in implementation date], to avoid four costly bail-ins*". The bank sovereign doom loop might exacerbate moral hazard for those countries (Bolton and Jeanne 2011).

[14] Weakly capitalized in this context means that the bank had below average CET1 capital ratio in 2010–2012 compared all other banks in the sample.

the second major finding of this study: The bail-in premium is primarily driven by the capitalization level of banks. The difference-in-difference term here is interacted with the *negative* average standardized CET1 capital ratio (such that an increase can be interpreted as higher bail-in risk). Both the difference-in-difference term as well as the triple interaction terms are statistically highly significant and economically meaningful. A one standard deviation decrease in equity is associated with a 16 basis points increase in the bail-in premium. Note that this is not driven by a general increase in bank risk (which is absorbed the bank × month fixed effects), but that it affects only those bonds which are not protected from BRRD bail-in. This is a new and conceptually different finding from the usual demonstration that yield spreads respond to changes in capital ratio (as demonstrated in Flannery and Sorescu (1996) and Sironi (2003)). Banks' riskiness (proxied by CET1 capital ratio in this case) affects bonds maturing in 2016 differently than bonds maturing a year earlier, since the latter are explicitly protected from BRRD bail-in. While it is well established that an increase in bank risk taking increases yield spreads of the banks securities, I show that an improvement in the supervisors ability to enforce participation in losses in case of bank failure, affects the yield spreads of applicable securities stronger for weakly capitalized banks.

[Insert Table 4 about here]

Specification (3) and (4) interact the difference-in-difference term with a non–GSIB or a GIIPS dummy. As indicated in the sample splits, the bail-in premiums is more pronounced for non–GSIB banks and banks domiciled in peripheral countries. The difference is *not* statistically significant. This suggests that the driving force of the bail-in premium is related to the risk of bank failure rather than GSIB status or location. Note that this is in and of itself an interesting result: If the likelihood to fail is mainly responsible for the bail-in premium, than the BRRD is perceived as a credible threat across Europe and across institutions. This not a contradiction to the wide held belief that a failure of a major GSIB bank would lead to government intervention – it merely points out, that investors believe that they will be forced participate in the costs of restructuring (via a bail-in) to some degree.

Overall, the presented evidence suggests that the BRRD improved market discipline in the European banking sector by removing implicit guarantees. Bank bonds investors perceive the bail-in tool as a credible threat, which is reflected in applicable securities yield spreads. However it remains challenging to tell just how much bail-out expectations were reduced. This is because it is difficult to map the 12 basis point increase in yield spreads for applicable securities into a change in the perceived probability of future bailouts. Perhaps the best way to put the empirical findings into perspective is to compare the magnitude with those reported in Archarya et al. (2016). Table 8 of their paper investigates how bonds spreads of bonds which were explicitly guaranteed by the FDIC compare to those that were not using a triple-differencing framework similar to the one used in this study. The coefficient of the triple interaction term (distance to default $\times$ guarantee dummy $\times$ post dummy) is equal to -39 basis points. The average yield spread in their sample is 2.371%, i.e. the change in explicit guarantees changed the yield spread by about 16% (= 0.39/2.371). This means that changing the bail-out expectations for those bonds from "somewhat likely" to 100% resulted in yield spread dropping by 39 basis points. Assuming investors perceived bail-outs equally likely in the US and Europe[15] would suggest that bail-in expectations in Europe were diminished, but not completely revoked by the BRRD. This back of the envelope calculation should however be treated with caution. The last subsection of this chapter discusses the scope (and limits) of the findings further.

4.2 Robustness Checks

This subsection presents various robustness checks. It starts with a discussion of the identifying assumption of the difference-in-difference regression, namely parallel trends of control and treatment group in absence of treatment. Next I demonstrate that the same pattern cannot be observed on non-bank corporate bonds. I then turn to explore and MREL related explanation of the empirical results. Finally I discuss potentially distorting effects related to Quantitative Easing and present a placebo test.

Parallel Trends test

The identifying assumption of any difference-in-difference analysis is the parallel trends assumption. In absence of treatment, both control and treatment group ought to have evolved similarly. In the context of this study, this assumption is quite intuitive. The yield spreads of two bonds of a given institution with identical characteristics (except one bond maturing in say 2015 and the other one in 2016) should be similar, as the term premium (for the longer life of the bond) are already accounted for in the spread. An advantage of panel data is that the parallel trends can be verified in the pre-treatment period statistically. To test the validity of

[15] which is a very debatable assumption

the difference-in-difference setup, I evaluate the parallel trend assumption using a standard parallel trends test. This procedure is well established (see for example Autor (2003)). The test is based on checking the statistical difference between treatment and control group in each period by introducing multiple difference-in-difference coefficients. More specifically one can estimate:

$$\text{Yield Spread}(i,t) = \sum_{k=\text{June 2011}}^{\text{May 2012}} m_k \cdot D_{it}(t=k) + \sum_{k=\text{April 2014}}^{\text{March2015}} p_k \cdot D_{it}(t=k) + \beta \cdot X_{it} + \gamma_i + \delta_j \times \mu_t + \varepsilon_{it} \quad (3)$$

where D_{it} is a dummy variable that is equal to one if the bond is in the treatment group and the time is t. γ_i and $\delta_j \times \mu_t$ are bond, and bank × month fixed effects respectively. X_{it} is a vector of bond controls. I exclude May 2012, thus estimating the dynamic effect of the BRRD on yield spreads relative to the last month of the pre-BRRD period, as is standard in the literature. For the difference-in-difference regression to be unbiased, the coefficients m_k should be statistically insignificant different from zero, otherwise the treatment effect appears to take place before the treatment period started, pointing towards omitted variable bias caused by confounding, unobserved factors. The p_k on the other hand should be significantly different from zero (and positive in this specific context).

[Insert Table 5 about here]

Table 5 presents the results. Indeed, there appears to be no difference between control and treatment group before the legislative process. After the BRRD was passed in parliament however, there is a positive and statistically significant difference, between bonds that are protected from BRRD bail-in and those that are not.[16] This suggests that the regression results of the previous section are indeed unbiased and provide consistent estimates. The results are graphically represented in Figure 3. The figure plots the coefficients and their 95% confidence intervals and at the same time foreshadows the results of the next subsection: The bail-in premium can be found only on bank bonds, not on non-bank corporate bonds.

Non-bank corporate bonds

As a first robustness check, I rerun the same regressions on non-bank corporate bonds which are constructed identically as the bank bonds sample. If the increase

[16] Two pre-period interactions terms m_k are significant but negative

in yield spreads of securities which became exposed to BRRD bail-in is indeed driven by the BRRD, then the effect should only be present on bonds issued by banks, not bonds issued by corporates.

[Insert Table 7 about here]

Table 7 presents the parallel trend test of the previous subsection for non-bank corporate bonds. Again, the coefficients in the pre-treatment period are insignificant, indicating a valid research design. The coefficients in the treatment-period however are insignificant for corporate bonds. That is, the bail-in premium of the previous section cannot be found on non-bank corporate bonds. This makse sense, since the BRRD defines a new resolution regime *only* for banks and *not* for corporations and should therefore have no impact on the latter. As the parallel trends test requires a lot of statistical power, the baseline regression is also computed again on the non-bank corporate bond sample.

[Insert Table 6 about here]

Table 6 presents the results. Column (1) and (2) include the entire sample of non-bank corporate bonds, with different fixed effects. Column (3) and (4) focus on bonds maturing in 2015 and 2016, as a robustness check which will be explained in the next subsection. The difference-in-difference coefficient varies between being positive and negative and remains statistically insignificant at any meaningful level across specifications. The fact that there is no effect on non-bank corporate bonds further supports the main result of this study: The BRRD improve market discipline in the European banking sector. The difference between bank and corporate bonds are visualized in Figure 3.

MREL Equilibrium Pricing Effect

Another concern regarding the interpretation of the empirical observations presented so far is related to the *Minimum Requirements for Eligible Liabilities* (MREL) which were introduced in the BRRD.[17] Since a bail-in requires a bank to have outstanding debt which qualifies for a bail-in, the BRRD introduced a minimum requirement of such liabilities (the so called MREL). Starting 2016, authorities had the power to demand certain (at the time still to be determined) amounts of unsecured debt, such that in a banking failure there would be enough qualified debt outstanding to

[17]I would like to thank Ulf Lewrick for pointing this out. See Crespi and Mascia (2018) for an analysis of MREL for the Italian banking sector

revive the bank by writing the debt off or converting it into equity. All else equal, mandating banks to issue more bail-in able debt (i.e. issue more bonds which share the same characteristics as the treatment group bonds in this study) should lead to an increase in the yields (and therefore the yield spreads) of all MREL qualifying bonds. Therefore, the empirical pattern observed so far, could well be explained by an "equilibrium MREL pricing effect". The exogenous shock (an increase in supply of such bonds forced by the regulator) would increase the yield spreads of outstanding securities sharing similar characteristics. Therefore it seems that the results presented sofar could be an *MREL premium* (due to increased supply of applicable securities) rather than a *bail-in premium* (reflecting increased bail-in risk). Fortunately, bonds only qualify as MREL if they have a residual maturity of a year (or longer). Given that MREL was introduced January 2016, any bond maturing in 2016 is potentially subject to a BRRD bail-in, but does not qualify as MREL (and therefore its yield spread should *not* experience an MREL premium). To test whether MREL could distort my results, I rerun the baseline regression, but limit the treatment group to bonds maturing in 2016 (while the control group remains unchanged and consists of bonds maturing in 2015). Table 8 presents the results. Specification (1) displays the main sample (including all bonds) for reference.

[Insert Table 8 about here]

The bail-in premium is still found on bonds maturing in 2016 (albeit the economic magnitude is somewhat smaller). The difference-in-difference coefficients remains significant in specification (2) – (4) and loses significance in specification (5), where the higher order residual maturity control term absorbs too much variation. Limiting the non-bank corporate sample on bonds maturing in 2015 and 2016 only, again finds no signs of a spurious results (as presented in Table 6 specification (3) and (4)).

Quantitative Easing and Maturity Effects

Yet another concern regarding the interpretation of the empirical observations presented so far is related to *Quantitative Easing* (QE) and the term structure of the yield (spread) curve. The sample period in this study coincides with various QE measures undertaken by the European Central Bank. When the short term rate hit the zero lower bound, the ECB tried to lower long term rates using unconventional monetary policy measures. This "lowering of long term rates" may affect the treatment group in the dataset, differently than the control group, since by research design, the treatment group matures later. As described in section 3, I do not just subtract the current short-term rate from each bonds' yield, but the yield of a triple AAA rated government security with identical residual maturity. Therefore, all concerns regarding changes in (the slope of) the yield curve should be relaxed by the use of yield *spreads* rather than yields. Additionally, one might be concerned regarding the residual maturity as a control variable. I therefore rerun the baseline regression using residual maturity and different functions of it to see whether the effect is driven by the choice of residual maturity control variable(s). Table 9 presents the results.

[Insert Table 9 about here]

In specification (1) and (3), no maturity control is included and the difference-in-difference coefficient is quite large (22 basis points without bank controls and 16 basis with bank × month fixed effects). Controlling for different functions of residual maturity (in particular the log of residual maturity and higher order squared residual maturity) in specifications (4) – (7) the coefficient shrinks somewhat in magnitude (with 10 basis points, as "lower bound" estimate), but remains statistically highly significant across all specifications, suggesting the results are not driven by some mechanic effect in the yield spread structure.

Placebo test using 2015 maturing bonds only

As an additional (and final) robustness check, I construct a placebo test. As the BRRD bail-in is applicable from January 2016, one can construct a placebo test on bonds maturing in 2015. In this specification, the control group consists of bonds maturing in the first half of 2015, while the treatment group consists of bonds maturing in the second half of 2015. Note that both treatment and control group are explicitly protected from BRRD bail-in. The treatment group therefore experiences no actual treatment and is more accurately described as a *placebo* treatment group. Running the same baseline regression specification on this sample, one would expect no results, if BRRD bail-in is truly responsible for the observed pattern of the previous sections. Table 10 presents the results. Indeed, the difference in difference coefficient is not statistically significantly different from zero. Specification (4) and (5) additionally limit the sample by dropping all observations where the residual maturity is less than three months (to avoid concerns regarding retirement effects). Table 11 shows a parallel trend test for the placebo regression, which is graphically depicted along side the main sample in Figure 4.

[Insert Table 10 and Table 11 about here]

Scope of this research and policy implications

Albeit the collected evidence strongly favours the hypothesis that the BRRD improved market discipline some limitations shall be pointed out before I conclude. First of all, the study only demonstrates that market discipline improved between 2011/2012 and 2014/15 as a result of the BRRD. Recent bank resolutions in Spain (with the successful bail-in of Banco Popolare d'Espanga junior debt investors) and Italy (with the back-door bail-out of retail investors of Monte de Paschi di Sienna) may have affected market discipline and bail-out expectations. As the identification strategy in this analysis uses bonds maturing in 2015 as the control group, any event after 2015 cannot be studied in this framework. Some evidence regarding the effects of recent bank resolution is presented in Raffaele Giuliana (2018). Second of all, wording matters crucially in this context. This studies only demonstrates that the BRRD *decreased* bail-out expectations. No claim is made regarding the absolute level of bail-out expectations before the BRRD, nor just how credible the bail-in threat is, now that the BRRD is in place. This is of great importance for policy makers. While the presented empirical evidence shows that the BRRD was indeed improving market discipline, it does not necessarily suggest that no additional measures should be undertaken to further diminish any remaining bail-out expectations. Lastly, some avenues for future research shall be pointed out. Since a successful bail-in depends on the creditors ability to suffer losses (and the regulators willingness to enforce those losses), an in depth analysis of the bank debt investor structure seems to be a promising avenue for future research (Boermans and van Wijnbergen 2017). In particular the exposure of retail investors (households) and insurance companies could be potentially dangerous pitfalls in enforcing bail-ins. Continuous monitoring of the debt holdings and their potential for bail-in cascades as documented in Hüser et al. (2017) could further be fruitful endeavour for regulators. Ultimately, the more knowledge supervisors have on the bail-in-able debt investors, the more credible the bail-in threat can be.

5 Conclusion

This paper exploits an unexplored natural experiment to inform the debate about market discipline in the European banking sector. Using the introduction of the BRRD bail-in tool which affects bonds maturing before and after 2016 differently, allows to examine debt holders ability to monitor bank risk taking and their perception of expected public assistance in the event of bank failure. This issue is of great importance to policy makers and supervisors.

The fact that bonds maturing before 2016 were explicitly excluded from BRRD bail-in provides an unique opportunity to overcome the usual identification challenge, as it allows to study the effect of the BRRD on debt holders' perception of implicit guarantees *within* banks. Banks' riskiness is fully accounted for by including bank × month fixed effects. Therefore the regressions only pick up effects that are related to investors' changing their perception of the likelihood that a particular bond will be bailed-in, in case of a bank failure (and *not* the (possibly changing) likelihood of a bank failure itself). This enables me to establish a causal link between a change in the regulators ability to bail-in creditors (which affects only a sub set of the outstanding liabilities) and their response in regard to market prices of applicable securities, independent of the banks' risk profile.

They key finding based on 26,466 observations for 1,481 bonds issued by 50 banks domiciled in 11 European countries illustrates that investors perceive the BRRD bail-in to be a credible threat. On a more fundamental level, this demonstrates their ability to not only monitor the riskiness of the banking sector, but also take into account confounding factors such as public guarantees. While before the BRRD there was no statistically significant difference between bonds maturing in 2015 and 2016 in terms of their yields spreads, there is a positive and significant difference after the BRRD was passed. Consistent with theory, the average treated bond in my main sample increases its yield spread by about 10 basis points compared to the control group, since investors understand the likelihood of being bailed-in in case of bank failure increased. The bail-in premium is mainly driven by the banks' equity level. As the changes in the likelihood to be bailed-in in my paper are plausibly exogenous, my inference remains valid across a battery of robustness checks.

I conclude by pointing out the benefits of capitalizing on my identification strategy. The advantage of the set-up lies in the fact that one is able to estimate effects *within* bank. This allows including bank × month fixed effects, differencing out any unobserved heterogeneity. Treatment effects are therefore simply measured as the difference between yields spreads of bonds that are subject to bail-in before and after the introduction of the BRRD. My empirical results demonstrate that (a) bond holders are able to monitor bank risks and (b) believe that the BRRD is an improvement in resolution technology making future bail-ins more likely. It remains unexplored whether debt holders acted on the updated regulation and actively influenced the banks' managements risk taking, which appears a promising area for future research.

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Figure 2: How bail-in works

This graphs demonstrates how the bail-in tool can be used to recapitalize banks in distress. In plot (1) a stylized bank balance sheet is depicted. If the banks' assets turn out to be less worth than originally thought, equity (as residual claim on the assets) will be reduced until the bank is bankrupt (as in (2)). If raising new equity is not possibly and no private sector M&A solution can be brokered, the resolution authorities had to either let the bank fail and had it enter bankruptcy procedures (risking negative externalities on both financial markets and the real economy) or had to taxpayer money to fund a bailout. The BRRD provides a new option by writing off equity and convert outstanding debt into new equity as depicted in plot (3). By artificially recapitalizing the bank through writing off existing debt, the losses on the balance sheet can be compensated and neither a (potentially socially costly) bankruptcy nor a publicly funded bailout are necessary.

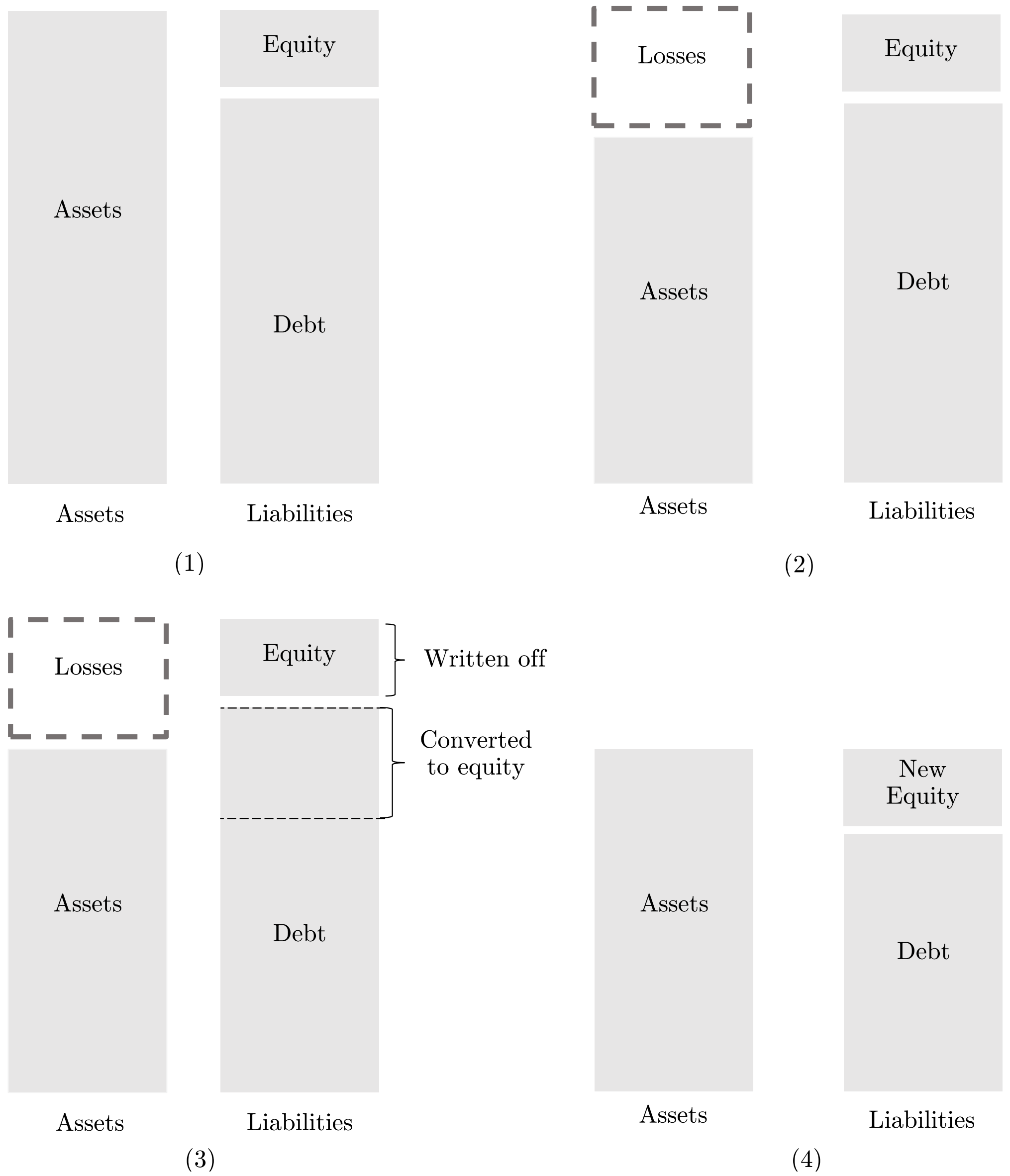


Table 1: Summary statistics

This table presents the summary statistics of the main sample. Panel A describes the yield data. The pretreatment period is June 2011 to May 2012. The post treatment period is April 2014 – March 2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2016 are in the treatment group. Panel B describes the balance sheet data used for sample splits and interactions. The values are the 2010–2012 average of the respective variables.

Panel A: Bond Data (Bloomberg)

	N	Mean	Sd	Min	P1	P50	P99	Max
Banks								
Yield (*in % points*)	26,686	2.37	1.72	0.02	0.18	1.75	6.64	8.68
Yield Spread (*in % points*)	26,686	1.76	1.17	0.00	0.27	1.45	5.36	7.57
Remaining Life (*Months*)	26,686	37.36	23.32	0	1	37	94	102
Corporates								
Yield (*in % points*)	4,534	1.98	1.51	0.04	.09	1.66	5.92	7.69
Yield Spread (*in % points*)	4,534	1.41	1.04	0.02	.17	1.10	4.80	7.65
Remaining Life (*Months*)	4,534	34	22.44	0	1	35	95	101

Yield Spread by groups in % points (Banks)

	non-treated (maturing 2015)	treated (maturing 2016–2019)
Pre-treatment period 06/06/2011 – 06/05/2012	2.43 (1.31)	2.48 (1.37)
Treatment period 15/04/2014 – 15/03/2015	1.23 (.72)	1.32 (.74)

Panel B: Balance Sheet Data (SNL)

Mean in 2010–2012

Variable (*SNL Key*)	N	Mean	Sd	Min	P10	P90	Max
Core Tier 1 Ratio (*235297*)	39	9.58	1.79	5.24	7.43	12.45	12.91

Table 2: The BRRD's effect on market discipline

This table presents multivariate difference-in-difference estimates. The pretreatment period is 06/06/2011 – 06/06/2012, i.e. the 12 month period before the EC first proposal of the BRRD. The post treatmentperiod is 15/04/2014 – 15/04/2015, i.e. the 12 months period after the final version of the BRRD passed the European parliament resulting in the dummy variable "BRRD passed (time dummy)". Bonds maturing before 01/01/2016 are in the control group, since the bail-in tool is not to be used before January 2016. Bonds maturing between 01/01/2016 and 31/12/2019 are in the treatment group, resulting in the variable "Maturing 2016+ (treated dummy). "Difference-in-Difference" is a dummy variable equal to one if the observation lies in the treatment period and the bond is in the treatment group, i.e. if both treated dummy and BRRD time dummy euqal one. The level of observation is bond-month. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Difference-in-Difference	0.059	0.174***	0.187***	0.159***	0.111**	0.130***
	(0.86)	(2.99)	(3.18)	(4.58)	(2.25)	(3.09)
BRRD passed (time dummy)	-1.220***	-1.472***				
	(-22.15)	(-31.01)				
Maturing 2016+ (treated dummy)	0.024				-0.022	
	(0.31)				(-0.47)	
Remaining Life					0.011***	-0.104
					(6.02)	(-1.45)
Remaining Life^2					-0.000***	-0.000
					(-3.94)	(-1.14)
Observations	26466	26466	26466	26466	26466	26466
R^2	0.242	0.740	0.807	0.936	0.825	0.936
Bond Fixed Effects	No	Yes	Yes	Yes	No	Yes
Month Fixed Effect	No	No	Yes			
Bank × Month Fixed Effects	No	No	No	Yes	Yes	Yes

Table 3: Sample split – Equity, GSIB and geographic domicile

This table presents multivariate difference-in-difference estimates for various sub-samples of the dataset. For reference, specification (1) include all banks. Specification (2) includes only weakly capitalized banks, while specification (3) includes only strongly capitalized banks. A bank is considered weakly (strongly) capitalized iff its average CET1 equity ratio from 2010–2012 is below (above) the sample mean. Specification (4) "GIIPS" includes only banks from Greece, Italy, Ireland, Portugal and Spain. Specification (4) "Core" includes all banks that are not included in (5). Specification (6) includes only GSIB banks of 2012, which were: BNP Paribas, Deutsche Bank, Banco Bilbao Vizcaya Argentaria, BCPE, Credit Agricole, ING Groep and Nordea Bank. Specification (7) includes only non-GSIB banks. The pretreatment period is 06/06/2011 – 06/05/2012. The post treatmentperiod is 15/04/2014 – 15/03/2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2019 are in the treatment group. The level of observation is bond-month. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1) All banks	(2) Low Equity	(3) High Equity	(4) GIIPS	(5) Core	(6) GSIB	(7) non–GSIB
Difference-in-Difference	0.130***	0.149**	0.115**	0.156**	0.108**	0.015	0.143***
	(3.09)	(2.09)	(2.58)	(2.35)	(1.99)	(0.09)	(3.31)
Remaining Life	-0.104	-0.206**	0.089	-0.204*	-0.021	0.177	-0.186**
	(-1.45)	(-2.15)	(0.83)	(-1.68)	(-0.24)	(1.45)	(-2.17)
Remaining Life^2	-0.000	-0.000*	0.000	-0.000	-0.000	-0.000	-0.000
	(-1.14)	(-1.91)	(0.99)	(-0.76)	(-0.82)	(-1.11)	(-0.80)
Observations	26466	14769	11697	12229	14237	3960	22506
Adjusted R^2	0.930	0.889	0.963	0.925	0.905	0.890	0.934
Bank × Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bond Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: Heterogenous effects

This table presents multivariate difference-in-difference estimates with additional interaction terms. Specification (1) is the baseline scenario from Table 2 for reference. In specification (2) Bail-in risk is captured by the negative average standardized Core Tier 1 Ratio in 2010–2012 (i.e. less equity implies higher bail-in risk). In specification (3) it is equal to one if the bank is not a GSIB, i.e. neither of BNP Paribas, Deutsche Bank, Banco Bilbao Vizcaya Argentaria, BCPE, Credit Agricole, ING Groep or Nordea Bank. In sepcification (4) it is equal to one if the banks is *not* headquartered in Greece, Italy, Ireland, Portugal or Spain. The pretreatment period is 06/06/2011 – 06/05/2012. The post treatmentperiod is 15/04/2014 – 15/03/2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2019 are in the treatment group. The level of observation is bond-month. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)
Difference-in-Difference	0.130***	0.107***	0.084	0.106**
	(3.09)	(2.58)	(0.70)	(2.08)
Difference-in-Difference × (–CET1)		0.187***		
		(3.44)		
Difference-in-Difference × non–GSIB			0.051	
			(0.42)	
Difference-in-Difference × GIIPS				0.056
				(0.79)
Remaining Life	-0.104	-0.108	-0.105	-0.105
	(-1.45)	(-1.50)	(-1.46)	(-1.46)
Remaining Life^2	-0.000	-0.000	-0.000	-0.000
	(-1.14)	(-1.15)	(-1.16)	(-1.11)
Observations	26466	26466	26466	26466
Adjusted R^2	0.930	0.930	0.930	0.930
Bank × Month Fixed Effects	Yes	Yes	Yes	Yes
Bond Fixed Effects	Yes	Yes	Yes	Yes

Table 5: Parallel Trends test (bank sample)

This table presents the result of a standard parallel trends test. The level of observation is bond-month. The regressors m13,..., m1 are differen-in-difference lags, the regressor p1,..., p12 are difference-in-difference leads. The pretreatment period is 06/06/2011 – 06/05/2012. The post treatmentperiod is 15/04/2014 – 15/03/2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2019 are in the treatment group. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. t statistics are clustered on bond level. p-values are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1)		(2)	
m12 (2011m06)	0.043	(0.91)	0.011	(0.26)
m11 (2011m07)	0.026	(0.55)	0.010	(0.24)
m10 (2011m08)	0.039	(0.84)	0.008	(0.21)
m09 (2011m09)	0.091*	(1.93)	0.070*	(1.88)
m08 (2011m10)	0.052	(1.40)	0.050	(1.46)
m07 (2011m11)	0.048	(1.24)	0.050	(1.40)
m06 (2011m12)	−0.003	(−0.08)	0.001	(0.02)
m05 (2012m01)	−0.053*	(−1.75)	−0.035	(−1.24)
m04 (2012m02)	−0.007	(−0.30)	0.009	(0.38)
m03 (2012m03)	−0.056***	(−2.80)	−0.057***	(−3.04)
m02 (2012m04)	−0.070***	(−4.22)	−0.066***	(−4.31)
m01 (2012m05), ommitted				
p01 (2014m04)	0.093*	(1.86)	0.114***	(2.63)
p02 (2014m05)	0.098*	(1.90)	0.119***	(2.64)
p03 (2014m06)	0.071	(1.36)	0.100**	(2.20)
p04 (2014m07)	0.117**	(2.21)	0.142***	(3.11)
p05 (2014m08)	0.126**	(2.37)	0.155***	(3.39)
p06 (2014m09)	0.110**	(2.12)	0.140***	(3.08)
p07 (2014m10)	0.100*	(1.92)	0.126***	(2.77)
p08 (2014m11)	0.131**	(2.51)	0.126***	(2.75)
p09 (2014m12)	0.143***	(2.75)	0.142***	(3.05)
p10 (2015m01)	0.153***	(2.93)	0.147***	(3.09)
p11 (2015m02)	0.100*	(1.88)	0.098**	(2.00)
p12 (2015m03)	0.088	(1.62)	0.093*	(1.86)
Treated Dummy	−0.025	(−0.49)		
Remaining Life	0.011***	(6.04)	−0.107	(−1.48)
Remaining Life^2	−0.000***	(−3.94)	−0.000	(−1.24)
Observations	26474		26474	
Bank × Month Fixed Effects	Yes		Yes	
Bond Fixed Effects	No		Yes	

Table 6: BRRD and non-bank corporate bonds

This table presents multivariate difference-in-difference estimates for the non-bank corporate bond sample. The pretreatment period is 06/06/2011 – 06/06/2012. The post treatmentperiod is 15/04/2014 – 15/03/2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2019 are in the treatment group. The level of observation is bond-month. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

			maturing 2015 & 2016 only	
	(1)	(2)	(3)	(4)
Difference-in-Difference	-0.098	-0.013	-0.157	0.122
	(-0.68)	(-0.11)	(-1.04)	(0.96)
Treated dummy	-0.029			
	(-0.22)			
Remaining Life	0.026***	0.240	-0.019	-0.153
	(4.18)	(0.69)	(-0.09)	(-0.83)
Remaining Life^2	-0.000***	-0.000***	-0.000**	
	(-3.11)	(-3.65)	(-2.07)	
Observations	4534	4534	3352	3352
Adjusted R^2	0.906	0.947	0.949	0.947
Company × Month Fixed Effects	Yes	Yes	Yes	Yes
Bond Fixed Effects	No	Yes	Yes	Yes

Table 7: Parallel Trends Test (corporate sample)

This table presents the result of a standard parallel trends test for the non-bank corporate sample. The level of observation is bond-month. The regressors m13,. . . , m1 are differen-in-difference lags, the regressor p1,. . . , p12 are difference-in-difference leads. The pretreatment period is 06/06/2011 – 06/05/2012. The post treatmentperiod is 15/04/2014 – 15/03/2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2019 are in the treatment group. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1)		(2)	
m12 (2011m06)	0.210	(1.32)	0.110	(0.88)
m11 (2011m07)	0.135	(0.94)	0.082	(0.68)
m10 (2011m08)	0.033	(0.26)	−0.015	(−0.13)
m09 (2011m09)	0.027	(0.21)	0.023	(0.20)
m08 (2011m10)	0.078	(0.66)	0.070	(0.69)
m07 (2011m11)	0.071	(0.65)	0.065	(0.69)
m06 (2011m12)	−0.014	(−0.09)	−0.022	(−0.15)
m05 (2012m01)	0.026	(0.25)	−0.000	(−0.00)
m04 (2012m02)	0.034	(0.35)	0.002	(0.03)
m03 (2012m03)	0.083	(0.78)	−0.018	(−0.36)
m02 (2012m04)	0.069	(0.70)	−0.007	(−0.17)
m01 (2012m05), ommitted				
p01 (2014m04)	0.019	(0.13)	0.061	(0.48)
p02 (2014m05)	−0.071	(−0.46)	−0.030	(−0.24)
p03 (2014m06)	−0.054	(−0.36)	−0.015	(−0.12)
p04 (2014m07)	0.010	(0.07)	0.045	(0.35)
p05 (2014m08)	0.043	(0.28)	0.077	(0.58)
p06 (2014m09)	−0.008	(−0.05)	0.031	(0.23)
p07 (2014m10)	0.014	(0.09)	0.051	(0.40)
p08 (2014m11)	0.004	(0.02)	0.041	(0.31)
p09 (2014m12)	−0.034	(−0.21)	0.002	(0.01)
p10 (2015m01)	−0.106	(−0.63)	−0.056	(−0.40)
p11 (2015m02)	−0.218	(−1.06)	−0.140	(−0.80)
p12 (2015m03)	−0.278	(−0.99)	−0.226	(−0.96)
Treated dummy	−0.086	(−0.59)		
Remaining Life	0.026***	(4.29)	0.177	(0.57)
Remaining Life^2	−0.000***	(−3.18)	−0.000***	(−3.70)
Observations	4,537		4,537	
Company × Month Fixed Effects	Yes		Yes	
Bond Fixed Effects	No		Yes	

Table 8: MREL Equilibrium Pricing

This table presents multivariate difference-in-difference estimates. The pretreatment period is 06/06/2011 – 06/05/2012. The post treatmentperiod is 15/04/2014 – 15/03/2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2016 are in the treatment group. The level of observation is bond-month. Specification (1) includes all bonds (maturing up to 31/12/2019) for reference. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. log(Remaining Life) is the natural logarithm of the residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

		2015 *vs.* 2016 only			
	(1)	(2)	(3)	(4)	(5)
Difference-in-Difference	0.130***	0.162***	0.112***	0.083**	0.045
	(3.09)	(3.73)	(2.99)	(1.97)	(0.72)
Maturing 2016+ (treated dummy)		-0.104*			
		(-1.73)			
Remaining Life	-0.104	0.010***	0.018		0.041
	(-1.45)	(2.75)	(0.22)		(0.50)
Remaining Life^2	-0.000				-0.000
	(-1.14)				(-1.41)
log(Remaining Life)				0.053	
				(1.44)	
Observations	26466	17301	17301	17301	17301
Adjusted R^2	0.930	0.834	0.933	0.933	0.933
Bank × Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Bond Fixed Effects	Yes	No	Yes	Yes	Yes

Table 9: The effect of bond maturity

This table presents multivariate difference-in-difference estimates using different specifications of residual maturity as control variables. Remaining Life is the bonds remaining life in months. The pretreatment period is 06/06/2011 – 06/06/2012. The post treatmentperiod is 15/04/2014 – 15/04/2015. Bonds maturing before 01/01/2016 are in the control group. Bonds maturing between 01/01/2016 and 31/12/2019 are in the treatment group. The level of observation is bond-month. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. log(Remaining Life) is the natural logarithm of the residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Difference-in-Difference	0.222***	0.227***	0.159***	0.159***	0.101**	0.130***	0.105**
	(5.73)	(5.84)	(4.58)	(4.56)	(2.55)	(3.09)	(2.46)
Maturing 2016+ (treated dummy)	0.014	-0.051					
	(0.36)	(-1.14)					
Remaining Life		0.003***		-0.112		-0.104	-0.061
		(2.86)		(-1.57)		(-1.45)	(-0.83)
log(Remaining Life)					0.078**		0.079**
					(2.56)		(2.22)
Remaining Life^2						-0.000	0.000
						(-1.14)	(0.14)
Observations	26466	26466	26466	26466	26466	26466	26466
Adjusted R^2	0.816	0.817	0.930	0.930	0.930	0.930	0.930
Bank × Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bond Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes

Table 10: Pseudo–BRRD

This table presents multivariate difference-in-difference estimates. The pretreatment period is 06/06/2011 – 06/06/2012. The post treatmentperiod is 15/04/2014 – 15/04/2015. Bonds maturing before 01/06/2015 are in the control group. Bonds maturing between 01/06/2015 and 31/12/2015 are in the treatment group. Since BRRD bail-in is only in effect starting January 2016, the "treatment group" is only a pseudo treatment group. Specficiations (4) and (5) drop all observations with a remaining life of less than three months. The level of observation is bond-month. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

				Retirement effects controlled for	
	(1)	(2)	(3)	(4)	(5)
Placebo Difference-in-Difference	0.047	0.041	-0.113	0.065	-0.119
	(0.444)	(0.501)	(0.348)	(0.294)	(0.322)
Remaining Life		-0.315***	-0.258**	0.008	0.048
		(0.003)	(0.019)	(0.960)	(0.767)
Remaining Life^2			-0.000		-0.000*
			(0.109)		(0.064)
Observations	9309	9309	9309	5945	5945
Adjusted R^2	0.921	0.921	0.922	0.934	0.934
Bank × Month Fixed Effects	Yes	Yes	Yes	Yes	YES
Bond Fixed Effects	No	No	Yes	Yes	Yes

Table 11: Parallel Trend test (Pseudo BRRD)

This table presents the result of a standard parallel trends test. The level of observation is bond-month. The regressors m13,..., m1 are differen-in-difference lags, the regressor p1,..., p12 are difference-in-difference leads. The pretreatment period is 06/06/2011 – 06/06/2012. The post treatmentperiod is 15/04/2014 – 15/04/2015. Bonds maturing before 31/06/2015 are in the control group. Bonds maturing between 01/07/2015 and 31/12/2015 are in the treatment group. Since BRRD bail-in is only in effect starting january 2016, the "treatment group" is only a pseudo treatment group. Remaining Life is the residual maturity in months. Remaining Life^2 is squared residual maturity. Standard errors are clustered on bond level. t statistics are reported in parentheses below the coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively.

	(1)		(2)	
m12 (2011m06)	0.120*	(1.76)	0.051	(0.77)
m11 (2011m07)	0.089	(1.31)	0.026	(0.40)
m10 (2011m08)	0.189**	(2.36)	0.135*	(1.90)
m09 (2011m09)	0.182**	(2.55)	0.134**	(2.13)
m08 (2011m10)	0.036	(0.56)	−0.006	(−0.10)
m07 (2011m11)	−0.044	(−0.67)	−0.080	(−1.23)
m06 (2011m12)	0.040	(0.48)	0.011	(0.14)
m05 (2012m01)	0.038	(0.81)	0.017	(0.39)
m04 (2012m02)	0.087***	(2.61)	0.070**	(2.26)
m03 (2012m03)	0.056**	(2.28)	0.043*	(1.87)
m02 (2012m04)	0.032*	(1.79)	0.025	(1.40)
m01 (2012m05), ommitted				
p01 (2014m04)	−0.048	(−0.46)	0.103	(1.52)
p02 (2014m05)	−0.034	(−0.32)	0.124*	(1.75)
p03 (2014m06)	−0.057	(−0.52)	0.107	(1.51)
p04 (2014m07)	−0.044	(−0.39)	0.126*	(1.80)
p05 (2014m08)	−0.090	(−0.78)	0.087	(1.25)
p06 (2014m09)	−0.070	(−0.59)	0.113	(1.63)
p07 (2014m10)	−0.126	(−1.04)	0.064	(0.94)
p08 (2014m11)	−0.214*	(−1.75)	−0.019	(−0.28)
p09 (2014m12)	−0.247*	(−1.96)	−0.046	(−0.67)
p10 (2015m01)	−0.176	(−1.37)	0.027	(0.39)
p11 (2015m02)	−0.114	(−0.88)	0.087	(1.15)
p12 (2015m03)	−0.097	(−0.74)	0.099	(1.28)
Remaining Life	−0.252**	(−2.25)	−0.317***	(−2.90)
Remaining Life^2	−0.000**	(−1.99)		
Observations	26474		26474	
Bank × Month Fixed Effects	Yes		Yes	
Bond Fixed Effects	No		Yes	

Table 12: Sample construction

This table presents the bond sample selection process for the bank bonds used in my analysis.

Variable	Filter	Value
Issuer Name[(1)]	INCLUDE	*bank name* "current and subs"
Payment Rank[(2)]	INCLUDE	"Junior Unsecured", "Sr Unsecured", "Jr Subordinated" , "Subordinated", "Unsecured"
Issue Date[(3)]	LESS THAN	06/06/2012
Maturity Date[(4)]	IN RANGE	01/01/2015 – 12/30/2019
Maturity Type[(5)]	INCLUDE	Bullet
Currency[(6)]	INCLUDE	Euro (EUR)

Explanation:

(1) The requests above are done for each bank used in Schäfer et al. (2016), except I don't consider Swiss and UK banks. Swiss Banks are not subject to the BRRD since it is a European Regulation. UK Banks are still directly supervised and resolved by the British Authorities (and not the Single Resolution Mechanism). Also, computing the yield spread applicable to GBP denominated bonds is not straight forward. All bonds of the entity and its direct subsidiaries are included. For example for Commerzbank, bonds of Dresdner Bank are also included since Commerzbank acquired Dresdner Bank in 2009. In this respect Dresdner Bank is not considered as a separate bank.

(2) I download all unsecured european bank bonds that are in principle subject to bail-in

(3) Only bonds issued before the reform are considered

(4) Only bonds maturing after the reform was passed are considered.

(5) I include only bullet bonds to avoid distortionary effects of derivatives features as suggested by Archarya et al. (2016)

(6) To be able to compute the yield spread I only use bond yields of Euro denominated bonds.

Table 13: Sample construction (corporate bonds)

This tables describes the bond sample selection process for the corporate bonds used in my analysis.

Variable	Filter	Value
Country of Risk$^{(1)}$	INCLUDE	Europe (except "United Kingdom" and "Switzerland")
BICS Classification$^{(2)}$	INCLUDE	All (except "Financials" and "Government")
Payment Rank$^{(3)}$	INCLUDE	"Junior Unsecured", "Sr Unsecured" "Jr Subordinated", "Subordinated", "Unsecured"
Issue Date$^{(4)}$	LESS THAN	06/06/2012
Maturity Date$^{(5)}$	IN RANGE	01/01/2015 – 12/30/2019
Maturity Type$^{(6)}$	INCLUDE	Bullet
Currency$^{(7)}$	INCLUDE	Euro (EUR)

Explanation:

(1) I choose all corporate bonds whose ultimate risk are located in Europe. Bonds by UK or Swiss companies are dropped to create a sample that is comparable to the bank bonds.

(2) To get a proper control sample I use only non-financials and non-government bonds (the latter would be to correlated with the safe yield to have meaningful information)

(3) Similarly to the bank bond sample, secured bonds are excluded to create a sample that is comparable to the bank bonds.

(4) Only bonds issued before the reform are considered

(5) Only bonds maturing after the reform was passed are considered.

(6) I include only bullet bonds

(7) To be able to compute the yield spread I only use bond yields of Euro denominated bonds

Yield Curve data

To compute the yield spread, I first construct the safe yield (spotrate) of a triple AAA rated Euro denominated government security with residual maturity TTM by fitting the svenson model using the parameters provided by the European Central Bank's Datawarehouse. The keys for the parameters are displayed in the following table:

These parameters can be downloaded from the ECB's statistical data warehouse in daily frequency. Let TTM be the term to maturity (in years), then the spotrate at time t for a triple A rated Euro denominated bond with residual maturity TTM

Table 14: Parameters and keys for yield curve data from the ECB's statistical data warehouse. The parameters can be downloaded in daily frequency.

Parameter	Key
$\beta_0(t)$	YC.B.U2.EUR.4F.G_N_A.SV_C_YM.BETA0
$\beta_1(t)$	YC.B.U2.EUR.4F.G_N_A.SV_C_YM.BETA1
$\beta_2(t)$	YC.B.U2.EUR.4F.G_N_A.SV_C_YM.BETA2
$\beta_3(t)$	YC.B.U2.EUR.4F.G_N_A.SV_C_YM.BETA3
$\tau_1(t)$	YC.B.U2.EUR.4F.G_N_A.SV_C_YM.TAU1
$\tau_2(t)$	YC.B.U2.EUR.4F.G_N_A.SV_C_YM.TAU2

is given by:

$$spotrate(TTM, t) =$$
$$\beta_0(t)+\beta_1(t)\cdot\left[\frac{1-e^{\frac{-TTM}{\tau_1(t)}}}{TTM/\tau_1(t)}\right]+\beta_2(t)\cdot\left[\frac{1-e^{\frac{-TTM}{\tau_1(t)}}}{TTM/\tau_1(t)}-e^{\frac{-TTM}{\tau_1(t)}}\right]+\beta_3(t)\cdot\left[\frac{1-e^{\frac{-TTM}{\tau_2(t)}}}{TTM/\tau_2(t)}-e^{\frac{-TTM}{\tau_2(t)}}\right]$$

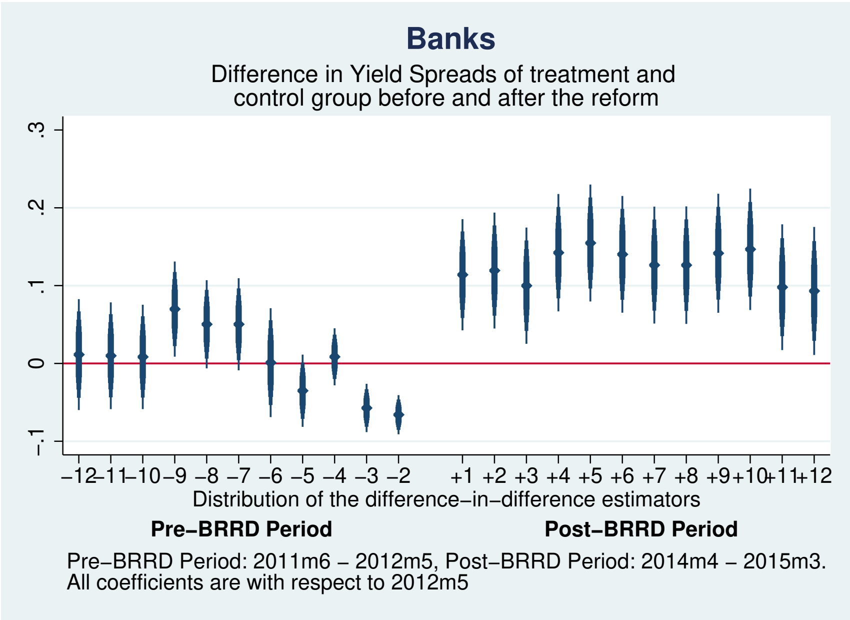
See Svenson (1995) for details. Next for each bond i at each point in time t, I compute:

$$YieldSpread(i, t) = Yield(i, t) - spotrate(TTM(i, t))$$

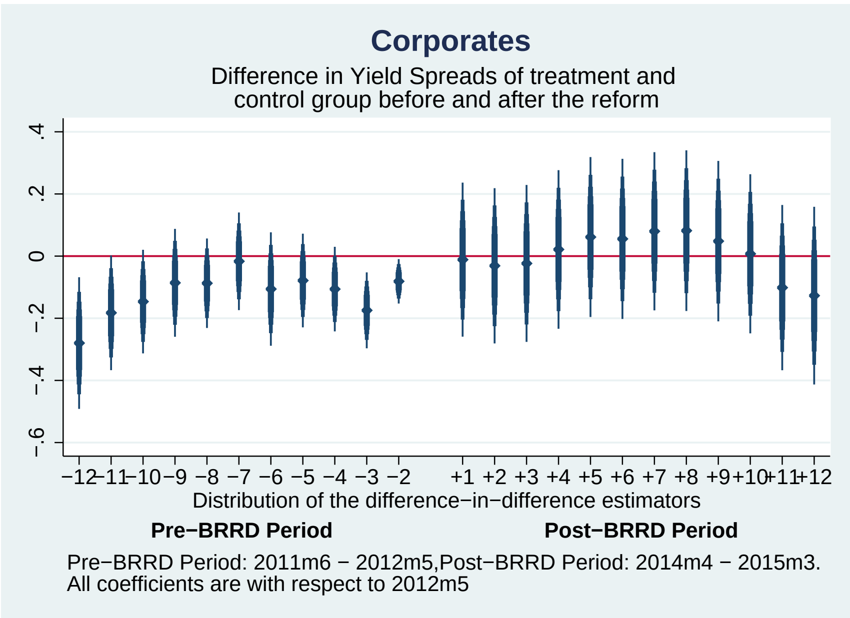
where $TTM(i, t)$ is the residual time to maturity (in years) of bond i at time t.

Figure 3: Parallel Trends Test Banks and Non-Financial Corporates

The graphs plot the difference-in-difference estimators along with their 90% confidence intervals for the (a) bank and (b) the non-bank corporate sample esrimated by equation (3), where standard errors are clustered on bond level. Clearly for both sample the parallel trend assumption can be maintained (all lags are statistically insignificant from zero) in the pre–BRRD period. For banks however, the treatment group (i.e. bonds maturing in 2016 which are subject to BRRD bail-in) face significantly higher yield spreads under the new bail-in regime. All specifications include bond and bank × month fixed effects and control for residual maturity as defined in equation (1).



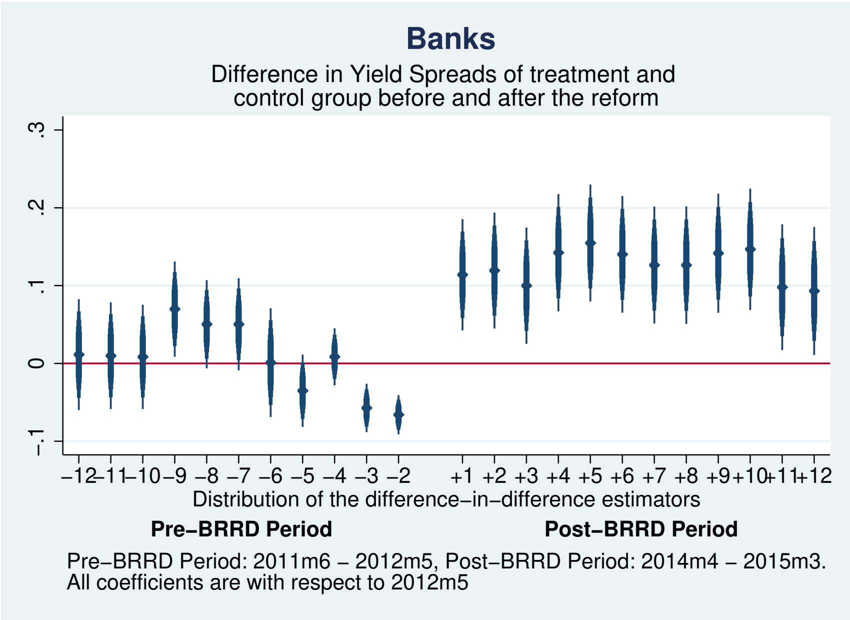
(a) Banks



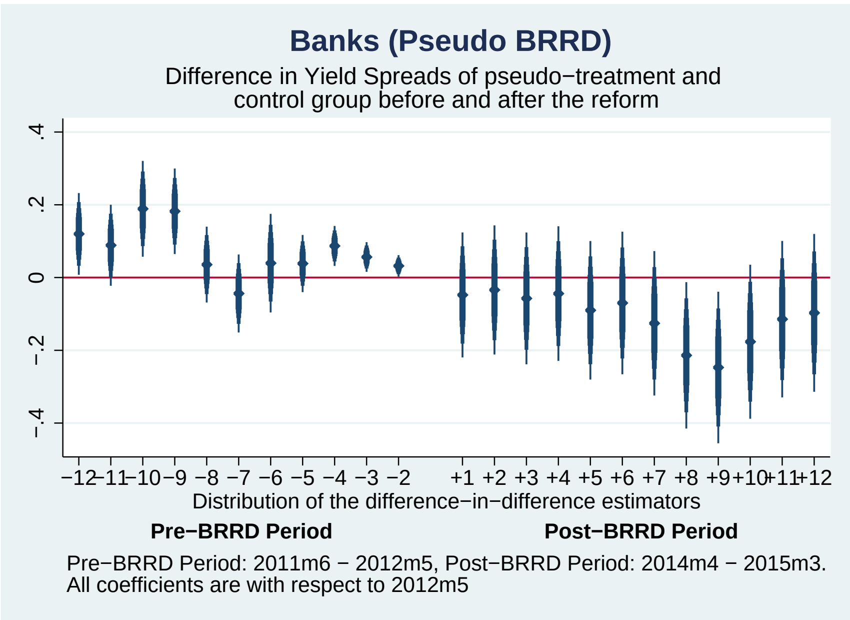
(b) Corporates

Figure 4: Parallel Trends Test Banks and Pseudo-BRRD

The graphs plot the difference-in-difference estimators along with their 90% confidence intervals for the (a) bank and (b) the non-bank corporate sample esrimated by equation (3), where standard errors are clustered on bond level. Clearly for both sample the parallel trend assumption can be maintained (all lags are statistically insignificant from zero) in the pre–BRRD period. For banks however, the treatment group (i.e. bonds maturing in 2016 which are subject to BRRD bail-in) face significantly higher yield spreads under the new bail-in regime. All specifications include bond and bank × month fixed effects and control for residual maturity as defined in equation (1).



(a) Banks



(b) Banks (Pseudo BRRD)

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