How effective are bad bank resolutions? New evidence from Europe

Michael Brei*, Leonardo Gambacorta♦, Marcella Lucchetta♠ and Bruno Maria Parigi♣

Abstract

The paper studies the effectiveness of bank resolutions using a comprehensive database on banks headquartered in 18 European countries over the period 2000–19. By means of difference-in-differences methodology, we find that impaired asset segregations – otherwise known as bad banks – have been more effective than state-funded recapitalisations of distressed banks. While recapitalised banks seem to have used the injected funds mainly to clean up their balance sheets by reducing problem loans and cutting down on lending, banks that segregated assets increased progressively their lending after the creation of the bad bank. For both types of banking crisis interventions, we find a significant ex-post reduction in the cost of bank funding and shift towards deposit funding.

JEL classification: E44; G01; G21.

Keywords: bad banks, resolutions, lending, non-performing loans, rescue packages, recapitalisations.

*University of Lille, CNRS, IESEG School of Management, UMR 9221, LEM, email: michael.brei@univ-lille.fr

♦Bank for International Settlements (BIS) and CEPR, email: leonardo.gambacorta@bis.org

♠Department of Economics, Ca' Foscari University of Venice, email: lucchett@unive.it

♣Department of Economics and Management, University of Padua, email: brunomaria.parigi@unipd.it

For their comments and suggestions, we thank two anonymous reviewers, Konrad Adler, Patrizia Baudino, Thorsten Beck, Simone Boccaletti, Frederic Boissay, Brunella Bruno, Nicolas Debarsy, Sebastian Doerr, Andrew Ellul, Oskar Kowalewski, Ettore Panetti, Andrea Nobili, Amedeo Pugliese, Hyun Song Shin, Eric Strobl, Martin Valdez Quintero, Ratsko Vrbaski, and the audiences at the BIS Research Seminar, at the University of Sassari Summer 2019 Workshop, and at the CefES International Conference on European Studies 2019. The opinions expressed in this paper are those of the authors and do not necessarily reflect those of the Bank for International Settlements.

1. Introduction

The Great Financial Crisis and subsequent Sovereign Debt Crisis in Europe have left many European banks with large burdens of non-performing loans (NPLs).[1] At its peak during 2012-14, the median NPL ratio reached 8% of total loans before falling back to 4% in 2019 for our sample of banks (see Figure 1). One fourth of our banks had NPL ratios that reached 20 percent of total loans. As the ECB (2022) noted the volume of NPLs in the eurozone amounted to 1 trillion EUR in 2015. The sheer size of this phenomenon, and the aim to avoid the use of taxpayer money to bail out distressed banks, induced the European Union (EU) to introduce the Bank Recovery and Resolution Directive (BRRD). The BBRD introduced several major novelties. First, there was the attempt to replace a patchwork of national procedures introducing uniform rules for the entire EU (and countries that choose to adhere to it) to deal with failing banks at the national level, and cooperation arrangements for cross-border banking failures. The second novelty was to replace bailouts with creditors' bail-ins. And third, the BBRD enshrined in the law the possibility to segregate impaired assets in separate companies and severely limited the use of state money to fund them.

Given the size of NPLs, the variability of national resolution regimes and the widespread change in the regulatory landscape, European banks offer an ideal environment to investigate the effectiveness of state-funded recapitalisations and asset segregation tools. As creditors were bailed-in only in a handful of instances due to the political fallout from early bail-ins (Parigi, 2017), state-funded recapitalisations and asset segregations represent two primary bank-level interventions that have been commonly implemented in Europe during our sample period, and in many cases, they have often been used together.

The aim of this paper is thus to investigate their effectiveness for a large sample of European banks over the period 2000–19. According to our view, an intervention is effective when its helps restoring bank stability and lending capacity. It is a multifaceted adjustment that operates through sounder balance sheets and improved financial conditions. For these reasons, we examine a broad set of key financial and performance indicators that aim to capture the evolution of NPLs, the profitability of banks, and their business model in terms asset and liability management after the interventions.

We address three main research questions: (i) Do banks respond differently to state-funded recapitalisations compared to asset segregations? (ii) Which resolution tool is more effective? (iii) Under which conditions and circumstances are asset segregations more effective?

[1] Although the application of the NPL concept is currently not fully harmonised across jurisdictions, a widely accepted definition is any exposure for which repayments are more than 90 days past due, or unlikely to be repaid without recourse to collateral (ESRB, 2017).

Our findings indicate that bad bank restructurings have worked better than state-funded recapitalisations. While recapitalised banks seem to have used the injected funds mainly to clean up their balance sheets by reducing problem loans and cutting down on lending, banks that segregated assets increased progressively their lending after the creation of the bad bank. We also find evidence that the funding structure of recapitalised banks changes in favour of deposits, and that the cost of debt falls after both recapitalization and bank restructuring.

In terms of the impact of different types of asset segregations, we find that resolutions that have used asset disposition vehicles are followed by stronger reductions in the cost of debt compared to those that used asset restructuring vehicles. Our results further indicate that the shift towards deposit funding was stronger in response to asset segregations that occurred within the BRRD framework relative to cases that occurred prior to its establishment. There is evidence that banks deleveraged more ex-post in countries with strong enforceability of contracts where the clean-up process should be facilitated. In the other countries, we observe portfolio rebalancing towards securities and stronger shifts in favour of deposit funding. Finally, we find that system-wide resolution programmes have been associated with a stronger clean-up of the NPL portfolio while the shift towards deposit funding was stronger at banks that received individual impaired asset segregations.

Our study contributes to the literature in at least four dimensions. First, we assemble a comprehensive cross-country data set on bad bank resolution schemes and state-funded recapitalisations in Europe during a period marked by financial distress, raising and persistent stocks of NPLs and changes in bank resolution schemes. Second, we perform an evaluation of major bank resolution interventions using a difference-in-differences approach that allows incorporating counterfactual analysis for two types of bank rescues using propensity score analysis. Third, we provide an assessment of the effectiveness of state-funded recapitalisations and impaired asset segregations using a number of important indicators from banks' financial statements. And lastly, we assess the differential impact of bad bank resolutions schemes depending on the way they were implemented, their complexity and institutional environment.

The remainder of the paper is organized as follows. Section 2 describes some facts on bad banks resolutions, how they work, and recent policy changes to the resolution procedures. In Section 3, we describe the data. In Section 4, we conduct the empirical analysis and describe the results. Section 5 concludes.

2. Some facts on bad banks resolutions

This section first analyses the benefits and drawbacks of asset segregation programs and identifies the main channels through which the widespread presence of non-performing loans in a bank's balance sheet could have undesirable effects. It then describes the main policy changes to the resolution procedures in the European Union that occurred during our sample period.

2.1. Bad banks at work

Asset segregation programs involve the removal of toxic assets from the balance sheets of distressed banks to house them in asset management companies (AMCs) also known as bad banks (BB). Bad banks acquire NPLs at a discount with respect to their book value and manage them with the aim of maximizing the recovery value.[2] As will become clearer below, restructured surviving banks should be relieved from the pressure of non-performing assets, while depositors and financial market participants should regain confidence. Moreover, the higher the discount price, the better it should be for the remaining good bank to perform in the aftermath.

However, when using asset segregation, the implied losses do not disappear as they must be written down and absorbed by bank capital. Then, why segregate impaired assets? To the best of our knowledge there are no theoretical models of the benefits of creating a bad bank. However, we can identify in the literature three channels, not necessarily mutually exclusive, through which a widespread presence of NPLs in a bank's balance sheet could have undesirable effects, ultimately hampering the ability of the burdened bank to function normally and to provide new credits.

First, a large stock of NPLs generates uncertainty about the overall quality of bank assets and therefore raises risk premia and funding costs. If the market has imperfect information about asset quality an *adverse selection* problem will arise making access to finance more difficult and costly (Thomson, 2011; EU Commission, 2018).[3] An indication that *adverse selection* in the NPL market may be a first order problem is the wide gap between the book value of NPLs and their market value (ESRB, 2017).[4] This, in turn, can lead to incentives for banks to delay NPL recognition to avoid increases in their cost of capital.

Second, delayed NPL recognitions are associated with evergreening loans and increased *moral hazard* (Acharya et al., 2021). While the impact of BBs in lowering adverse selection is clear-cut, their impact on *moral hazard* is in principle ambiguous. On one side, the creation of BBs discourages *moral hazard* behaviours of poorly capitalized banks. For example, Kahn and Winton (2004) argue that when a combination of high leverage and asset opacity induces risk shifting and excessive risk taking, incentives can be improved by creating a structure with two subsidiaries. One is supposed to hold safer assets, the other one riskier assets. Each subsidiary's debt has recourse only to that subsidiary's assets. The clear separation of risks and increase in transparency

---

2 AMCs were first used in the late 1980s and early 1990s in the United States (Resolution Trust Corporation) and Sweden (Securum and Retriva) to resolve problems at banks with persistently high stocks of impaired assets. Bad banks were also used during the Asian crisis in the late 1990s (Korea, Malaysia, Indonesia), and more recently, apart from the countries studied, in Turkey and Nigeria. For details, see Cerruti and Neyens (2016).

3 On the theoretical side, Tirole (2012) studies the problem of banks that must sell legacy assets to finance new projects showing that adverse selection in the legacy asset market may prevent trade, and thus, funding for new projects might not be available.

4 For example, when the NPLs of four Italian regional banks (Banca Marche, Banca Popolare dell'Etruria e del Lazio, Cassa di Risparmio di Chieti, Cassa di Risparmio di Ferrara) were segregated in a bad bank in 2015, the value of their NPLs was set at 17.6% of the face value.

should align in turn risk shifting incentives and moral hazard should disappear. On the other side, there are situations in which BB segregations may foster moral hazard. For instance, this could be the case for state-funded BB resolutions as the prospect that the state will assume non-performing loans may encourage banks to take excessive risks, which they otherwise would not. This is similar to the problem raised by Fahri and Tirole (2012) on government bailouts and more likely to happen if state-funded BBs are used regularly instead of selling off bad loans to privately-funded loan collectors at market prices.

Third, delayed NPL recognitions tie up bank capital that could otherwise be used to increase lending to valuable projects and profitability (see e.g. IMF, 2015; Accornero et al., 2017; Marques et al., 2020). Two connected and reinforcing effects are at work. First, NPLs require that the bank puts aside more capital than for performing loans because of the higher regulatory risk weights.[5] This in turn ties up financial resources that could have otherwise been used for lending and other investments. If NPL problems are expected to intensify in the future, banks also must make higher loan loss provisions. Second, negative shocks to banks' balance sheets induce banks to forego profitable lending opportunities as the benefits would mainly accrue to the pre-existing creditors, because of a debt overhang problem (Philippon and Schnabl, 2013; Colliard and Gromb, 2017). When the NPL problem is systemic, such a situation constrains economic growth and can lead to a negative feedback mechanism via borrower downgrades, raising capital requirements and slowdown in lending.

Several empirical papers assess the impacts and costs of bad bank resolutions. There is a well-established literature that shows that their effectiveness depends on the way they were implemented, their complexity and institutional background.

First, bad banks pursue their objective of maximizing recovery value by segregating assets using different strategies. Some are mainly asset restructuring vehicles (ARVs), while others are mainly asset disposition vehicles (ADVs). ARVs aim primarily to restructure bad loans before selling them. Disposition vehicles aim to acquire and dispose of NPLs as soon as possible. In a study of seven banking crises, Klingebiel (2000) shows that AMCs are not effective at expediting corporate restructuring and are more effective at liquidating assets rapidly when they have a narrow and clear objective. In this line, ADVs should be more effective to the extent that they have a shorter time horizon and clear goals.

Second, the source of funding for asset segregation may affect the performance of the "good" surviving bank where the good assets along with the insured deposits remain or are transferred to. A few studies, such as Haldane and Kruger (2002) and Goodhart and Avgouleas (2016), argue that bad bank resolutions with majority private ownership (more than 50% of capital) are more effective than majority state-funded ones in improving key performance indicators of the

[5] NPLs have a risk weight of 150 percent under the standardized approach of Basel III like corporate loans granted to borrowers rated below BB- (BCBS, 2017).

surviving banks.[6] This could have different reasons. One is the *moral hazard* problem linked to managerial incentives: privately funded asset segregations may work better because private funding imposes more monitoring on the management and future behaviour of the originating bank. Another reason is that bad bank resolutions are set up with majority private ownership when the impaired asset problem is less severe or could be considered a sign of a more vibrant economy, which would make the performance of the good bank bounce back faster. [7]

Third, legislation on debt recovery and bankruptcy affect the speed of NPL recognition and their removal from bank balance sheets, and ultimately, the effectiveness of asset segregation. Several studies show that in more efficient institutional environments, banks should be able to realize the value of their impaired assets more quickly and predictably, hence reducing uncertainty and freeing resources for more lending (IMF, 2015; European Commission, 2018). Taking the number of years to foreclosure as an indicator for inefficient insolvency regimes, a cross-country study by the IMF (2015) shows that the time to foreclosure correlates positively with the NPL ratio and negatively with the return on the investment in distressed assets. The lower the ability to enforce credit claims, the poorer are the prospects to recover value by selling or segregating them.[8]

Fourth, asset segregation may involve removing impaired assets from one, or several distressed banks at once, in a given jurisdiction. The resulting bad bank(s) may thus have a different scope and size with, at one extreme, system-wide centralized bad banks and at the other case-specific bad banks (Dado and Klingebiel, 2002; Baudino and Yun, 2017; European Commission, 2018). System-wide bad banks are often created when a large portion of the banking system exhibits significant NPL problems such as in Ireland and Spain. Their advantage are economies of scale and concentrated expertise. When banks have a weak governance, centralization can break the link between banks and borrowers (Klingebiel, 2000). Some evidence suggests that system-wide bad banks have worked better when dealing with non-performing real estate loans (Beck, 2017; European Commission, 2018). It appears, as Baudino and Yun (2017)

---

6 The first privately funded BB, which remained an exception for many years, was the 1988 resolution of Mellon Bank in the United States. Mellon Bank was split into two units with the bad assets moved to a separately chartered and capitalised BB that merely existed to liquidate bad loans (see, New York Times, 1988, and Thomson, 2011).

7 Examples of majority privately funded BB are the Irish National Asset Management Agency (NAMA, in 2010) and the Spanish Sociedad de Gestión de Activos procedentes de la Reestructuración Bancaria (SAREB, in 2012). SAREB was designed by three independent specialists (Oliver Wyman, BlackRock, European Resolution Capital) and funded by private banks and insurance companies (54% of capital) and the public Fund for Orderly Bank Restructuring (FROB, 46%). For details, see https://www.sareb.es/en/about-us/who-we-are/ (consulted on March 5, 2022).

8 This is in line with results for corporate restructuring. For example, Claessens et al. (2003) show that creditors will only force a firm to file for bankruptcy and incur the related legal costs if the judicial efficiency supports an adequate chance of recovery of losses. The relationship between bank distress and efficiency of insolvency regimes is also studied in Demirgüç-Kunt and Detragiache (2005).

observe, that system-wide bad banks are more likely to be set up with state funds, given the scale of resources involved and the coordination capacity needed to run them.[9]

Finally, in a macroeconomic cross-country analysis, Honohan and Klingebiel (2000) assess the fiscal costs of a number of banking crisis resolution tools among which AMCs. They find that unlimited deposit guarantees, open-ended liquidity support, repeated recapitalisations, debtor bailouts and regulatory forbearance significantly increase taxpayers' costs of resolution. Cerruti and Neyens (2016) analyse nine AMC resolutions over the period 1990–2015 and find that they have a mixed track record. The authors conclude that the success of AMCs hinges on institutional efficiency, solid diagnostic and critical mass of impaired assets.

Yet, a comprehensive cross-country study on the bank-level assessing which specific bad bank segregation design is most effective in promoting the integrity and functionality of the banks is absent. Such an analysis should consider these effects also in combination with state-funded recapitalisations that, as we argued above, represent another major bank resolution tool (Bayazitova and Shivdasani, 2012; Mariathasan and Merrouche, 2012; Brei et al., 2013; Giannetti and Simonov, 2013; Homar and van Wijnbergen, 2017). An analysis of banking crisis interventions is perhaps even more relevant as the COVID-19 pandemic will likely result in a significant increase in the stock of NPLs. Importantly, it is impossible to assess the effectiveness of these resolution tools without accounting for the fact that the interventions to repair banks' balance sheets are likely non-random. This calls for the use of econometric techniques that make the banks studied comparable.

The present paper fills this gap in the literature by analysing the impact of both asset segregations and recapitalisations in 18 European countries over the period 2000–19. Our study is based on detailed information on 130 major banks: 40 segregated impaired assets using bad banks, 33 received state-funded recapitalisations without segregating assets, and 57 banks did not receive any of these interventions. We use a difference-in-differences (DiD) methodology that allows us, based on propensity scores, to estimate the impact of the interventions on a comprehensive set of bank balance sheet and performance indicators. The procedure helps us to select similar bank observations from the group of non-intervened banks and thereby to construct a counterfactual for the intervened banks. The DiD methodology has been widely applied in the context of non-experimental policy evaluations in which there is no obvious and comparable control group. For applications of the DiD approach in the banking and finance literature, see among others Beck et al. (2010), Jagtiani et al. (2016), Argimón et al. (2018), Beccalli et al. (2018), Grosse-Rueschkamp et al. (2019).

We corroborated the validity of our results in a number of ways. We first checked whether the timing of bank interventions is unaffected by our outcome variables, since a violation of this would

[9] Examples of recent system-wide BBs are Ireland's NAMA, Spain's SAREB, Italy's National Resolution Fund, and Hungary's Magyar Reorganizációs és Követeléskezelő (MARK) Zrt. Case-specific BBs have been used, among others, in Austria, Belgium and Switzerland.

bias the results. Then we performed balancing tests on the pre-intervention comparability of the intervened and non-intervened banks, once accounting for different types of propensity score adjustment. We also control in our evaluations for a set of observable factors that might affect the way in which banks respond to shocks and the interventions. Following the DiD analysis, we perform an event study analysis and assess the dynamics prior to and in the aftermath of the interventions. This helps us to visually inspect the common trend hypothesis and the dynamic within bank adjustment in response to the interventions. And lastly, we explore the heterogeneity of bad bank resolutions to gauge whether their impact is non-linear and depending on the way they were implemented.

2.2. The Bank Recovery and Resolution Directive

The main policy changes to the resolution procedures in the European Union during our sample period were brought by the introduction of the BRRD.[10] The BRRD, announced in 2014, took effect in 2016 and aimed to provide a unified resolution framework for European credit institutions. Designed to overcome inefficiencies in policy actions stemming from supervisory fragmentation, it aimed to centralise the management of banking crisis resolution and it favoured bail-ins to bailouts (Carletti et al. 2016; Pancotto et al. 2019). However, on both counts the BRRD fell somewhat short of its stated objectives.

As for the provision of a unified framework for bank resolution, the BRRD applies only to banks judged as implicating public interest. However, what constitutes public interest is vague and the BRRD left it up to national regimes to resolve a failing bank that is classified as not of public interest, i.e. a negative Public Interest Assessment (PIA, see Gelpern and Véron (2021)). The lack of a unitary and binding structure of resolution schemes creates potential conflicts between the national and supranational agencies. Some national insolvency regimes are less stringent and leave the door open to state-funded bailouts, contrary to the stated goal. The Single Resolution Board (SRB), which decides on PIA, might have the incentive to adopt a hands-off approach, make a negative PIA, and thereby keep an ailing bank out of the EU resolution regime.[11]

As for creditor bail-ins, Parigi (2017) argues that the European Commission made decisions that caused uncertainties and tensions across national and supranational regulators, thus undermining the enforcement of resolutions that involve bail-ins of shareholders, bondholders and large depositors. An illustration of this is the European Commission's decision of 2015 to prevent the use of state funds in the resolution of four Italian regional banks, obliging shareholders and subordinated bondholders to participate, while allowing a German bank in the same year to benefit from state aid in the form of capital injections and guarantees.

---

10 Switzerland was not required to implement the BRRD but has adopted a very similar resolution framework.

11 This is illustrated by the bailout of the two Veneto banks in 2017. The SRB gave a negative PIA on two mid-sized Italian banks (Veneto Banca, Banca Popolare Vicenza) which were then subjected to liquidation under the Italian law. The latter process was managed by the Bank of Italy with generous financial state support.

To avoid the backlash from the rigid application of the stringent bail-in rules, the European Commission and the Italian monetary authorities interpreted the BRRD provisions in such a way as to minimize the impact on retail creditors of Banca Monte dei Paschi di Siena in 2016, and Veneto Banca and Banca Popolare Vicenza in 2017, but in doing so potentially undermined the effectiveness of these rules (Parigi, 2017). However, while the bail-in provisions proved to be difficult to enforce, the possibility to recourse to AMCs, codified by the BRRD, has been widely used in resolutions that occurred within the BRRD framework. Thus, its potential impact in our study deserves particular attention.

3. Data description

To assess the impact of banking crisis resolution tools, we gather data on the timing and type of intervention along with financial indicators of banks and other macroeconomic and institutional variables.

3.1 Recapitalisations and bad bank segregations

Large, distressed banks are a major obstacle to economic growth and financial stability. In the past, governments and central banks have used large amounts of taxpayers' money to rescue individual banks. During the GFC, authorities resorted to costly bank interventions with the aim of rescuing troubled banks to avoid contagion and a destabilization of the financial system. Typically, these bailouts targeted systemically important banks and they involved in many cases state-funded capital injections, toxic asset purchases and debt guarantees.

State-funded recapitalisations represent the most direct measure of bank rescues and more recently, they tended to be followed by bad bank restructurings. Recapitalisations aim primarily at restoring bank solvency, but they also aim at counteracting credit crunches that amplify economic downturns. To study bank responses to this type of intervention, we collected data on state-funded recapitalisations for our sample of banks over the period 2000-19.[12] We used 2000 as a reference year to start our analysis, because it provided sufficient years of normal cycle conditions, not affected by the financial crisis. We verified the results using 2002 and 2003 as cut-off points and our main conclusions hold (results not shown, but available on request).

Both theoretical and empirical studies emerged post-GFC suggesting that this type of bank bailouts creates incentive distortions, since banks anticipate being rescued in times of stress, particularly large banks – the too-big-to-fail problem. In response, financial regulators in particular with the BRRD gradually adapted their resolution toolkit resorting to impaired asset segregations in which banks transfer and sell toxic assets to asset management companies. The resolution design differs across time and countries, a fact that will be considered in our analysis.

[12] Table B1 in online annex B lists the sample of banks that received state-funded capital injections along with the other types of banks.

Impaired asset segregation has been conducted in many ways, corresponding to different levels of risk transfer and organizational complexity (McKinsey, 2009 a,b; Morrison and Foerster, 2009; HM Treasury, 2013; Gandrud and Hallerberg, 2014; KPMG, 2016). In this paper, we consider only bad bank structures that achieve complete risk transfer. Therefore, we ignore situations in which banks segregate internally impaired assets from the rest of the portfolio.[13] We also neglect risk transfer via the direct sale of impaired assets to specialised operators, a market that started to emerge at the end of our sample period (European Commission, 2021).

In this study, we consider therefore only episodes of asset segregation in which the good assets remained in a surviving "good" bank or in which the distressed bank stopped existing altogether. The rationale for this choice is that we want to evaluate whether asset segregation helps the same bank to bounce back. Thus, we do not include in our sample of asset segregations those cases where another bank takes over the healthy part of the distressed bank.[14] We performed a number of robustness checks, such as excluding banks that stopped existing, removing particular banks and countries from the estimations, and controlling for NPL securitisations. Our results outlined above are robust to such modifications.

For our sample of countries, we gathered information on such events over the period 2000-19 using different sources: academic articles, financial newspapers, press releases, and the European Commission's and central banks' webpages. Table B2 in the online annex B presents more details on the bad bank segregations covered in our study. We further report in Table B3 summary statistics on bank-specific characteristics across banks without intervention, banks with bad bank restructurings, and banks that received state-funded recapitalisations.

3.2 Bank financial indicators

Information on the financial statements of banks was obtained from Fitch Connect, a commercial data provider for harmonized bank financial statements across countries. We focus our analysis on major banks headquartered in 16 European Union countries plus the United Kingdom and Switzerland. Table 1 provides summary statistics by country.

We use banks' consolidated financial statements and exclude foreign bank subsidiaries to avoid double counting. An exception is Hungary where we included three foreign-owned banks because they were subject to bad bank resolutions. We historically reconstruct banks' financial statements adjusting them for mergers, acquisitions and restructurings which limits the number of banks that can be included in our study. We focus on active commercial banks as of end-2019 but also include restructured banks that were subsequently discontinued. We included banks that

[13] Examples of internal BBs include Dresdner Bank and Royal Bank of Scotland.

[14] One example was the Laiki bank experience in Cyprus which was split in two entities, a bad and a good bank where the good assets along with the insured deposits were transferred to the Bank of Cyprus. Similarly, the good assets along with the deposits of Veneto Banca and Banca Popolare di Vicenza were transferred to Intesa Sanpaolo while the NPLs remained in the old banks to be liquidated.

ceased to exist in the empirical analysis because they contain relevant information on the pre-intervention period, although excluding them does not affect our main results. The database was constructed in two steps. First, we identified the set of major banks headquartered in each country, in descending order of size to cover the majority of each banking system. Depending on the degree of competition and other factors, the number of banks thus varies across countries. For these banks, we identified in turn all state-funded recapitalisations and bad bank resolutions. In the second step, we searched for any other episodes of impaired asset segregations and included the surviving "good bank" if there was one.

Table 1 presents the characteristics of our database. The final sample includes 130 banks: 40 segregated impaired assets using bad banks, 33 received state-funded recapitalisations without segregating assets, and 57 banks did not receive any of these interventions. The sample covers all major banking institutions of each country: total assets sum up to EUR 29.7 trillion as of end-2019.

We reconstructed historically the financial statements. First, we appended financial statements under local GAAP to those reported under IFRS to obtain longer time series.[15] Second, we adjusted the financial statements for restructurings, mergers and acquisitions (M&As) by constructing pro-forma banks. We only consider majority takeovers. As in Brei et al. (2013, 2020), we obtain the pro-forma banks by summing the balance sheet components of the involved entities assuming that intercompany holdings are negligible. The adjustment for restructurings is particularly important in Spain where a large part of the banking system has been consolidated in the aftermath of the global financial crisis (FROB, 2011; IMF, 2012; Huerta, 2019).[16] The adjustment for M&As is particularly relevant when considering the growth rates of balance sheet positions as it removes discontinuities. Overall, we consider 121 of such events (see last column of Table 1).

4. Empirical strategy and results

4.1 Hypothesis testing

Because of the previous discussion, we identify two sets of testable hypotheses. The first set of hypotheses concerns to the absolute and relative effectiveness of the two resolution tools, state-funded recapitalisations and bad bank restructurings, in terms of the key financial and performance indicators that we discuss in the paper. Although we expect that the two resolution tools contribute positively to bank stability and lending capacity, we do not have any reason to

[15] In the regressions, we control for this using a dummy because certain items in the reports shift from one accounting method to the other due to e.g. the change in the netting rules of derivatives on the asset and liability side.

[16] For example, BFA Bankia emerged in 2010 from the merger of Caja Madrid, Bancaja, Caja Insular Canarias, Caixa Laietana, Caja Avila, Caja Segovia and Caja Rioja. For more details on Spain, see Table 2 in IMF (2012). For each of these cases, we checked the availability and quality of the financial statements prior to the adjustment. If the availability was weak (meaning that the concerned bank was relatively small), it was not included in the aggregation.

believe a priori that one tool will perform better than the other, and which performance measure will be most relevant.

The second set of hypotheses refers to the way asset segregations were conducted. We explored various alternatives, including state vs. privately-funded schemes, asset restructuring vs. asset disposition vehicles, segregations taking place before vs. after the BRRD, and segregations taking place in weak vs. strong contract enforcement jurisdictions. Based on the literature and theoretical arguments, we anticipate that the second option in each alternative is more effective in terms of the key financial and performance indicators that we discuss in this paper.

4.2 The difference-in-differences approach

The empirical strategy to investigate the impact of recapitalisations and bad bank restructurings on the surviving banks is based on a difference-in-differences approach. For an adequate inference we had to tackle various challenges. Without further adjustment, the evidence would correspond to conditional correlations consistent with the within-bank adjustment in response to the policy interventions. It would thus not reflect the causal effect since the interventions are endogenous for some of our outcome variables y_{it} modifying banks' attempt to repair balance sheets.

For this reason, we employ methods that detect and mitigate endogeneity problems and other biases related to the comparability of banks. Ideally, the aim is to compare the same bank with and without intervention. To get as close as possible to such a situation we will rely on matching procedures that help selecting similar observations from the group of non-intervened banks to construct the counterfactual of intervened banks. It is however important to note that the matching quality depends on observable factors included in our analysis, and it could be undermined if we fail to detect systematic and relevant unobservable factors in the control group. For applications of the DiD approach in the banking and finance literature, see among others Beck et al. (2010), Jagtiani et al. (2016), Argimón et al. (2018), Beccalli et al. (2018), Grosse-Rueschkamp et al. (2019), and for in-depth analyses on the theoretical underpinnings, see Rosenbaum and Rubin (1984), Dehejia and Wahba (1999), Rubin (2001), Zhao (2004), Caliendo and Kopeinig (2008) and Imbens and Wooldridge (2009).

Our baseline DiD specification is based on the following regression:

$$y_{it} = \beta_1 recap_{it} + \beta_2 restruc_{it} + \gamma X_{it} + \mu_i + \varepsilon_{it} \qquad (1)$$

where i refers to banks and t to years. The vector X_{it} contains bank-specific and macroeconomic control variables that absorb time-varying variation affecting banks linked to internal and external shocks, such as banking crises, and we do not include on top year-fixed effects. To absorb time-invariant variation across banks, we include bank fixed effects μ_i. The error term ε_{it} is clustered at the country-level allowing errors to be correlated over time within countries.

The variables of interest $recap_{it}$ and $restruc_{it}$ are dummy variables equal to one in the years after the year of intervention (state-funded recapitalisation and bad bank restructuring, respectively). The coefficients β_1 and β_2 estimate the overall impact of the interventions by comparing the outcome variables,y_{it}, prior to and after the intervention across *treated* and *non-treated* banks. For instance, a positive and significant β_2 indicates that the outcome variable has increased following a bad bank segregation, conditional on the included controls and unobserved bank fixed effects, and relative to banks without intervention. Our estimation approach uses propensity scores to construct counterfactuals of intervened banks along a set of financial indicators and other characteristics. Since we have two types of interventions, we use a multiple treatments approach to derive propensity scores.

Our empirical analysis aims to assess the impact of the interventions on banks by analysing a broad range of indicators from their financial statements. Specifically, we investigate the potential effects on financial risks, lending capacity, and business model. One has to note in this context that causal inferences can only be made on bank indicators for which the timing of bank interventions is unaffected by the outcome variables or the anticipation of it. We test this aspect below using survival regressions and falsification tests. The following outcome variables y_{it} are investigated: (i) loan growth, (ii) non-performing loan growth, (iii) total asset growth, (iv) securities ratio, (v) deposit ratio, (vi) diversification ratio, (vii) cost of debt, (viii) risk density function (risk-weighted assets over total assets), and (ix) return on equity. Table 2 provides details on the precise variable definitions and Table 3 reports summary statistics. We carefully inspected each variable using its 1st, 5th and 95th and 99th percentiles and winsorized the observations when the value did not make sense (e.g., if the ratio of a balance sheet item over total assets was larger than one). In studies as ours in crisis situations, one must be careful about replacing negative outliers as they carry potentially important information.

We control for macroeconomic factors affecting banks' external environment using the change in a country's short-term interest rate, real GDP growth and government debt as a percentage of GDP. Given that most countries in our sample went through financial distress, we include a banking crisis indicator equal to one during crises and zero otherwise. We identify the timing of banking and financial crises based on a range of sources, including Borio and Drehmann (2009), Reinhart and Rogoff (2009), Bech et al. (2014), Laeven and Valencia (2018) and Brei et al. (2020). If countries experience slow recoveries with negative GDP growth in the aftermath of the crisis, we include these years in the crisis indicator. The indicator thus considers economic downturns that might take longer than the financial crisis. Banks' capacity to adjust to shocks and the interventions also depends on their financial situation and business model, and thus we control for a number of bank-specific characteristics including: (i) bank size, (ii) short-term funding ratio, (iii) capital buffer,[17] (iv) liquidity constrained indicator, and (v) leverage constrained

[17] Similar to Brei and Gambacorta (2016), we take into account regulatory differences across countries when calculating the capital buffer. The buffer is defined as the difference between a bank's actual risk-weighted capital ratio and the country-specific regulatory minimum.

indicator.[18] We also include a dummy variable that is equal to one once a bank has adopted the International Financial Reporting Standards (IFRS) and zero otherwise. This indicator controls for changes in the measurement of certain balance sheet items and other differences in accounting due to the introduction of the IFRS standards.

The results for the baseline DiD regressions (1) are reported in panel A of Table 4. The preliminary evidence suggests that recapitalised banks reduce bank lending and non-performing loans in response to the interventions, conditional on the controls and unobserved bank fixed effects, see columns (I) and (II) of Panel A. The growth rate of lending drops by 6.8 percentage points after the capital injection, whereas the growth rate of non-performing loans drops by 24.3 percentage points. Relative to a mean of 5.2 and 19.3 percentage points, respectively, the impact is economically meaningful as well. The evidence seems to suggest that recapitalised banks use the injected funds mainly to clean up balance sheets by reducing problem loans (consistent with the results reported by Brei et al., 2013). Recapitalised banks also appear to deleverage as evidenced by a decline in the growth rate of assets by 6.1 percentage points in the post-intervention period (see column III). The drop of non-performing loans and total assets of recapitalised banks points to the fact that banks write-off of problem loans, deleverage and reduce size.

Banks that segregated impaired assets record a similar adjustment, with the notable difference that the slowdown in lending is less pronounced. More specifically, bad bank restructurings are followed by a reduction in lending by 3.6 percentage points (column I). The adjustment in non-performing loans and balance sheet size is relatively similar to what we have seen for banks that received state-funded capital injections. At the surviving banks, bad bank segregations are followed by a decrease in the growth of total assets by 5.8, while the growth of non-performing loans drops by 28.8 percentage points, see columns (II) and (III) of Panel A. This preliminary evidence suggests that bad bank restructurings have worked better than state-funded recapitalisations: banks still clean-up and shrink balance sheets but the repercussions on the lending business are less severe, other things being equal.

In the remaining regressions, we observe two more significant impacts. Funding of recapitalised banks shifts away from market and other forms of external funding to deposits (column V) and the cost of debt decreases after both recapitalization and bad bank restructuring (column VII). These findings point to another important dimension: interventions are followed by a reduction in banks' financial constraints, as banks are in the condition to finance their operations at lower costs. This finding is intuitive and makes sense, since the interventions on the one hand boosted banks' capital position and on the other relieved them from large burdens of NPLs, thus reassuring depositors and financial market participants.

These results should be considered as a first-pass analysis because for them to be valid two requirements need to be checked (Imbens and Wooldridge, 2009). First, we need to examine

[18] To the extent that the leverage ratio was included in the Basel III agreement in December 2010, we identified banks with regulatory leverage constraints only over the period 2011-19.

whether the timing of bank interventions is unaffected by the outcome variables. And second, we have to make sure that the two types of intervened banks are comparable among themselves and with respect to the banks without interventions. If these checks can be verified, this will allow us to come closer to disentangling the causal impact of the interventions.

4.3. Survival analysis: The timing of interventions and bank characteristics

Our empirical analysis hinges on the assumption that the timing of bank interventions is unaffected by the outcome variables. We formally test whether our outcome variables affected the timing of bank interventions using a survival model.

For each bank i, we count the number of years t it took for the intervention to occur (recapitalization or restructuring). This number is the survival time t_i relative to the start of our observation window. Banks that did not receive any intervention enter as right-censored observations. In turn, we estimate survival regressions of the form:

$$h(t_i) = h_0(t_i)\exp(\alpha + \beta y_i + \gamma X_i) \qquad (2)$$

where $i = 1, \dots, n$ are banks and t_i is the survival time (years to intervention). The hazard function, $h(t_i)$, represents the instantaneous probability of intervention given survival up to time t_i. It depends on a baseline hazard, $h_0(t_i)$, outcome variables y_i, and the control variables X_i (the same used in the baseline regression). Our model is estimated using a Cox proportional hazard model. In this setting, the baseline hazard is estimated non-parametrically and only depends on time, while the risk determinants are estimated parametrically using an exponential function. As before, standard errors are clustered at the country level to allow for correlation in the error term within countries.

We focus on cross-sectional regressions in which each bank enters with one observation. For banks that were intervened, we use as explanatory variables 5-year averages prior to the intervention. For the remaining banks, we use averages over the entire sample period.

The results shown in Table 5 indicate that the timing of interventions does not vary with most of the pre-existing outcome variables. Only in two cases we find the opposite (see columns VI and VII): banks with a higher diversification ratio have a significantly lower probability of intervention (hazard ratio equal to 0.95), while those with a higher cost of debt are more likely to be intervened (hazard ratio equal to 1.10), notably after controlling for bank-specific and macroeconomic factors. These results point to the stabilizing effects for banks of having more diversified sources of income[19] and the destabilizing effects of a too high cost of debt. The coefficients associated with the other outcome variables are statistically not different from zero (see Table A1 in the online annex A). One should thus be careful about interpreting the results on bank diversification and cost of debt in terms of causality.

[19] Using information on internationally active banks, Gambacorta et al. (2014) find that the correlation between income diversification and bank profitability is positive up to 30% of the diversification ratio and is not statistically different from zero afterwards.

4.4. Propensity score approach: The causal impact of bank resolutions

A potential shortcoming of baseline model (1) is that banks subject to intervention may not be comparable to the other banks. Ideally one would like to compare the same bank with and without intervention. To get as close as possible to such a situation we rely on procedures that help selecting similar units from the group of (untreated) banks without intervention to construct the counterfactual of (treated) banks with interventions (Caliendo and Kopeinig, 2008; Imbens and Wooldridge, 2009).

In an ideal set up, we would conduct randomized experiments in which units are randomly allocated across treatment groups. In observational studies as ours, one must rely on methods that help balancing the distributions across the comparison and treatment group. One method to improve the overlap in observed distributions is to drop control units that are very different from the treated subjects in terms of one or more covariates (Rubin, 2006). The selection can be done in many ways. One popular approach is to use propensity scores (PS), the probability of treatment participation. In our setting the propensity score is the probability of a bank to receive an intervention. We will adopt three approaches in using propensity scores to adjust the sample for observable differences across banks: (1) inverse probability of treatment weighting (IPTW), (2) stratification, and (3) PS matching (Rosenbaum and Rubin, 1984; Dehejia and Wahba, 1999; Caliendo and Kopeinig, 2008; Imbens and Wooldridge, 2009; Lunt, 2014).

Propensity scores are typically estimated using discrete choice analysis. In settings with only one treatment, the propensity score can be estimated using binomial probit or logit regressions.[20] In our setting, however, we have multiple treatments (recapitalization and bad bank segregation) and the variable of interest can take multiple discrete values. As a result, we use multinomial logistic regressions (Lechner, 2001; Uysal, 2015; Sloczynski and Wooldridge, 2018).

The estimation involves an iterative procedure with the aim of maximizing the predictive power of the model and its ability to balance the distributions of the included covariates.[21] The included covariates should, on one hand, be important determinants of intervention, given the fact that omitting them can increase bias in the estimates. On the other, one should only include variables that are unaffected by participation (or the anticipation of it). As such, we include in the baseline model the macroeconomic and bank-specific control variables from equation (1). Among others, these controls include banks' capital buffer and information on liquidity and leverage constraints, all of which are known to be important determinants of bank fragility. Because the

[20] The logistic regression has the advantage to have more density mass in the bounds compared to the probit regression (Caliendo and Kopeinig, 2008). Using Monte Carlo experiments, Zhao (2004) provides evidence that the choice of the estimator for the propensity score has little effects on the results.

[21] The explanatory power of the regression can be improved by including interactions and higher moments of the explanatory variables to capture nonlinearities in their relationship with the treatment assignment (Imbens and Rubin, 2015).

capital buffer might be affected by the interventions, in particular the recapitalization, we perform later robustness tests excluding this variable.

A detailed analysis of the propensity score estimation and a description of its utilization can be found in online annex C. We estimate different specifications, but the final one that includes interactions and higher order terms of the explanatory variables has the highest predictive power, as verified by the generalized Hosmer-Lemeshow test. As such, we will base the subsequent analysis on the propensity scores derived from this specification.

The main results for the DiD regressions using propensity score adjustments are reported in Panels B, C and D of Table 4. Panel B reports the results when using inverse probability of treatment weighting (IPTW). As discussed in online annex C, the inverse probability approach performs better in the balancing of the covariates compared with stratification and propensity score (PS) matching. In what follows, we will thus focus our discussion on the results using this method. For robustness, we report also in Panel C and D the results obtained with stratification and PS matching, respectively.

The econometric evidence shows that ceteris paribus, recapitalised banks reduce bank lending and non-performing loans (see columns (I) and (II) of Panel B). The results are not very different with respect to those obtained from the baseline model in Panel A, but we can note a general increase in the statistical significance of the coefficients. The growth rate of lending drops by 8.6 percentage points after the capital injection, whereas the growth rate of non-performing loans drops by 27.3. The impact coefficients are larger than those reported in panel A and confirm the view that recapitalised banks use the injected funds mainly to clean up balance sheets by reducing problem loans. Recapitalised banks also appear to deleverage as evidenced by a significant decline in the growth rate of assets, by 7.1 percentage points (column III).

A similar adjustment is observed for banks that segregated their toxic assets to bad banks, with the notable difference that their lending does not contract. More specifically, bad bank segregations are followed by a decrease in the growth of assets of the surviving bank by 7.2 percentage points, while the growth of non-performing loans drops by 23.2, see columns (II) and (III) of Panel B. Importantly, however, the response of lending is not significantly different from zero. This confirms and reinforces our previous findings: bad bank restructurings have worked better than recapitalisations in helping to restore banks' lending capacity. Banks write-off problem loans and reduce their balance sheet size without cutting back on lending, other things being equal.

Concerning the other outcome variables, there is evidence that banks' funding structure shifts towards deposits away from market forms of funding. However, the impact is only statistically significant for recapitalised banks, which record an average increase of 4.7 percentage points in the ratio of deposits (column IV). In line with this shift in the funding structure, we observe significant declines in the cost of debt for recapitalised and restructured banks in the order of 1.0 to 1.2 percentage points. One should, however, be cautious about interpreting these coefficients, as our previous findings highlighted that the timing of the interventions depends on the cost of debt (Table 5). Nevertheless, as we observe that the cost of debt shrinks, it is a sign that the

resolutions have been effective since one major reason for the segregation of impaired assets and cleaning-up of the balance sheet is to help the bank to regain market confidence (Morrison and Foerster, 2009; Thomson, 2012).

Concerning the control variables, we find that bank size, capital buffer, switch to IFRS accounting, change in the short-term interest rate, and the indicator on banking crisis are significant determinants of our outcome variables (Tables A2-A5 in the online annex A report the complete set of results). For instance, larger banks have lower loan and asset growth, higher securities ratios and lower deposit ratios, and they operate with a lower return on equity. Banking crises tend to be associated with contractions in lending and balance sheets, hikes in non-performing loans, drops in profitability, less deposits and higher cost of debt. All these results make economic sense and are meaningful, thus confirming our specifications.

The results on the impact of the bank resolutions remain largely unaffected when using the propensity score for stratification and direct matching, respectively (see Panel C and D of Table 4). The main differences are two: i) when we use the stratification approach the impact on deposits is no longer significant (see panel C, column V); ii) when we use direct matching the increase in the shareholder return for restructured banks is now significant at the 10% level (see panel D, column IX). This last result is particularly interesting because it suggests that bank profitability recovers when banks are resolved with bad bank segregations but not when banks are recapitalised. While these estimations summarize the average impact in the years that follow the banking crisis intervention, it is also interesting to inspect the dynamic impact using an event study methodology, as we will do below.

We checked for the robustness of our results using bootstrapped standard errors. To the extent that we obtained our weights from a first stage, we bootstrapped the entire process and not only the final regression to ensure that the standard errors account for sampling variability in the intermediate weights. More specifically, we take 500 random samples of the regression observations with replacement and clustered at the country level, and then re-estimate the first and second stages 500 times. The standard error is obtained by the standard deviation of the estimated coefficients. As can be seen in Table A6 in online annex A, the results are very similar. Next, we checked whether excluding the capital buffer in the first stage has an impact on our results to the extent that the capital position of banks could be affected by the interventions, but this ultimately depends on banks' responses. If they use the injected capital to clean up balance sheets and write down non-performing loans, the boost in the capital position might quickly be evaporated. As can be seen in Table A7, our main results remain robust when the capital buffer is excluded from the estimation of the propensity score. We also performed a falsification test that uses data prior to the interventions. More specifically, we shift the year of interventions six years ahead and check whether our outcome variables are impacted by this "pseudo" intervention (see Table A8). Only in the case of the diversification ratio, we find some significance suggesting that one has to be careful in interpreting the results on this variable. Another challenge is to make sure that we did not fail to detect any systematic and relevant unobservable factors in the control

group. We performed three tests of which all support our results. We first exclude countries from different regions and re-run the regressions (see Table A9). Next, we include an indicator variable for NPL securitizations at seven Italian banks, mostly occurring in 2018-19, and last we exclude Banco Popular Espanol from the estimations as it was subject to a bail-in 2017 (results not shown, but available on request).

4.5 Event study approach: a graphical analysis of bank crisis interventions

The regressions in Table 4 allowed us to quantify the overall impact of bank resolutions once they were enacted. In this subsection, we will examine graphically the within-bank adjustment prior and after the crisis intervention episode using an event study methodology or staggered adoption design framework similar to Beck et al. (2010) and Dobkin et al. (2018).[22] This methodology provides visual evidence of pre-intervention trends, on-impact effects and the effects over time.

The specification uses two-way fixed effects regressions that include leads and lags of the intervention using the following form:

$$y_{it} = \sum_{\tau=S}^{-2} \alpha_{i\tau}^{Recap} + \sum_{\tau=0}^{F} \alpha_{i\tau}^{Recap} + \sum_{\tau=S'}^{-2} \alpha_{i\tau}^{Bad\ bank} + \sum_{\tau=0}^{F'} \alpha_{i\tau}^{Bad\ bank} + \gamma X_{it} + \mu_i + \varepsilon_{it} \quad (3)$$

where μ_i are bank fixed effects, X_{it} represents our vector of bank-specific and macroeconomic control variables, and $\alpha_{i\tau}$ are coefficients on indicators for time relative to the interventions (recapitalization vs. creation of a bad bank). As before, we estimate the regressions using as weights the inverse probability of treatment to ensure that the distribution of covariates is comparable across the different groups of banks.

We define event time τ as the number of years relative to the intervention which occurs at $\tau = 0$. The relative time indicators $\alpha_{i\tau}^{j}$ are equal to one for recapitalised/restructured banks ($j = Recap/Bad\ bank$) in the τth year before ($\tau < 0$) and after ($\tau > 0$) the recapitalization/bad bank segregation, and zero otherwise. We include relative time indicators for all available years (S, S', F, F') but exclude the year prior to the resolution ($\tau = -1$). This implies that we estimate the dynamic impact of resolutions on the outcome variable y_{it} relative to the year before the intervention, conditional on the included control variables and unobserved time-invariant differences across banks.

Figure 2 shows the impact coefficients ($\alpha_{i\tau}$) for the different outcome variables along with 95% confidence intervals as a function of relative event time τ. We focus our analysis around the resolution event and depict 15-year event windows starting at $\tau = -5$ and ending at $\tau = 9$ for those bank indicators that were significant in the previous DiD estimates of the average treatment effect: loans, problem loans, assets, deposits, cost of debt, and return on equity.

[22] Beck et al. (2010) investigate the dynamic impact of bank deregulation on income inequality at the state-level in the United States, while Dobkin et al. (2018) examine the impact of hospital admissions on patients' economic situation.

Generally, the results mimic our previous findings. The impact of the two types of resolutions on bank lending is quite different (see panel I). Recapitalised banks progressively reduce the growth of lending, without any apparent recovery. By contrast, restructured banks experience an initially large drop in the growth rate of lending of around 10 percentage points in the year of the bad bank creation followed by a progressive and long-lasting recovery, relative to the pre-resolution year and conditional on the included controls.

The impact of bank restructurings on non-performing loans is less clear. As expected, NPL growth is negative in response to bad bank segregations, but there is important variation across banks and, as a result, the response is most often not significant (panel II). Recapitalised banks show an increase in NPLs in the year of resolution, indicating the use of capital to cover the recognition of loan losses (panel II). This is a clear sign of banks repairing balance sheets. In the following years, the growth rate of NPLs decreases becoming negative from the second year onwards.

Total asset growth drops by more than 5 percentage points in the year of intervention and the subsequent year at both restructured and recapitalised banks (panel III). While asset growth of restructured banks starts recovering progressively, for recapitalised banks, the drop in lending (performing and non-performing) is mirrored by a substantial and prolonged reduction in total assets. For these banks there is a clear sign of deleveraging.

Bank recapitalisations are associated with increasing deposit funding (panel IV), which confirms our previous finding. By contrast, bank restructurings are followed by a decline in deposits, especially during the third year after the resolution. Thereafter, deposits start recovering but the response is never significant. For recapitalised and restructured banks, we observe a steady decrease in the cost of debt (panel V). The larger decline for the cost of debt of recapitalised banks could be related to market expectations about implicit government guarantees linked to bailout policies (Fahri and Tirole, 2012).

Finally, we observe quite different effects of recapitalisations and restructurings on shareholder profitability (panel VI). While the return on equity drops by around 10 percentage points for recapitalised banks in the initial period and remains negative afterwards, we observe an initially negative impact for restructured banks followed by a sustained recovery. For recapitalised banks, this effect is presumably related to the recognition of problem loans and the cleaning-up of balance sheets. Over time, profitability recovers only in the case of restructured banks, which points to a greater effectiveness of this type of resolution tool. Again, the results mirror those of our previous estimations on the average impact.

4.6 The differential impact of bad bank interventions

The impact of bad bank segregations is likely to depend on the way they were implemented, their complexity and the institutional background. To shed more light on the different dimensions, we augment the baseline regression (1) to include an interaction term between the indicator variable on restructuring and a specific characteristic Z_{it} as follows:

$$y_{it} = \beta_1 recap_{it} + \beta_2 restruc_{it} + \beta_2^*(restruc_{it} \times Z_{it}) + \gamma X_{it} + \mu_i + \varepsilon_{it} \qquad (4)$$

where $(restruc_{it} \times Z_{it})$ is the interaction term. The overall impact of bad bank segregations on the outcome variable y_{it} is equal to $\beta_2 + \beta_2^* \times Z_{it}$. This model allows us considering nonlinearities in the responses. For instance, if β_2^* is significant and positive, then the impact of restructurings is increasing in Z_{it}, conditional on the included controls and unobserved bank fixed effects. As before, we weight the regressions by the inverse of the treatment probability and cluster standard errors by country.

We investigate seven dimensions which could possibly influence the impact of restructurings: (1) majority private vs. state-funded restructurings, (2) asset disposition vehicle (ADV) vs. asset restructuring vehicle (ARV), (3) before vs. after the BRRD, (4) large vs. small banks, (5) weak vs. strong enforceability of contracts, (6) system-wide vs. individually-targeted rescues, and (7) universal vs. narrow banks. The exact definitions and results are summarized in Table 6. For comparison, Panel A reports the results of the baseline specification with no interactions and using the IPTW approach.

The regressions reported in Panel B of Table 6 compare majority privately vs. majority state-funded restructurings. Privately funded resolutions occurred in Belgium (1 bank), Ireland (4), Italy (4) and Spain (8). It is worth remembering that in state-funded restructurings the state provides more than 50% of the funding. As discussed above, commenting the existing literature, privately funded bad bank segregation might be in principle more effective. However, there is only weak evidence of a differential response. The only significant interaction term is associated with the risk density function, suggesting that risk-weighted assets increase in response to privately funded restructurings, while they decrease for state-funded asset segregations (column VIII). This could be an indication that governments resolve the more severe cases.

Most bad bank segregations used asset disposition vehicles (27 of 40 interventions), whereas the remaining ones used asset restructuring vehicles. As discussed above in the literature review, ADVs should generally work better. There is some evidence of heterogeneous response (see Panel C). The decrease in the cost of debt seems to be driven by resolutions based on asset disposition vehicles (column VII). This could be an indication that market participants view disposition vehicles bringing in quicker and cleaner resolutions with lower exposures to moral hazard than ARVs. We also find that the diversification ratio increases at banks that used ADVs (+8.9 percentage points), while it drops at the other good banks, by 3.8 percentage points (-12.7+8.9, column VI).

The BRRD was announced in 2014 and provided a unified resolution framework for European credit institutions. Among other things, it favoured bail-ins to bailouts and enshrined in the law the possibility to recourse to AMCs severely limiting the use of state money to fund them. It should be noted, however, as we discussed above that there is scope for national and supranational regulators to circumvent the resolution procedures. An interesting finding is that the shift towards more deposit funding occurred in response to resolutions that were enacted within the BRRD

framework, suggesting that the new resolution procedures have boosted depositor confidence in the intervened banks (column V, Panel D). This indicates that the anticipation of being able to segregate impaired assets did not foster moral hazard, perhaps because of the severe limits to use state funds. Interestingly, also the drop in total assets is less pronounced during the BRRD period.

There are relevant differences when distinguishing bad bank segregations across large and small banks (see Panel E). Large banks do not reduce non-performing loans as much as small banks in the aftermath of the resolutions (column II). At the same time, they reduce their total assets by around 10 percentage points more than small banks (column III). This deleveraging process is mainly achieved through a reduction in securities. It is difficult to draw precise conclusions here, because of lack of more granular data, but it could be that large banks segregated more impaired securities holdings compared to small banks, the latter having higher exposures to problem loans. Small banks on the other hand appear to increase their securities holdings in response to the interventions (column IV). The deleveraging of large banks is accompanied by a reduction in deposit funding, while small banks' deposit ratio increases by 4.9 percentage points (column V). The latter finding could again be due to boosts in depositor confidence which is particularly at stake when it comes to small and troubled banks. Finally, we observe that the fall in the cost of debt is slightly higher for large banks but the difference is not statistically significant.

Panel F shows results across countries with different institutional characteristics in terms of contract enforcement. The latter is relatively weaker in Cyprus, Greece, Hungary, Italy, Slovenia and Portugal, where it takes significantly more days to resolve a legal dispute than in the other countries. We find that resolutions in countries with strong contract enforceability have been followed by more significant reductions in the size of banks' balance sheets. This deleveraging could be an indication of stronger changes in banks' management and recognition of past failure, facilitating by judicial systems in which legal disputes are resolved more efficiently. In countries with weak contract enforcement, we observe a larger increase in securities portfolios and deposit ratios compared to the resolutions in the other jurisdictions. This could indicate a shift in the rescued banks' business model and to a recovery in depositor confidence. Even though not significant, there is some evidence that non-performing loans respond more sluggishly in countries with weak contract enforcement, which could point to less efficient resolutions due to legal constraints and inefficiencies.

Most bad bank restructurings (30 out of 40) occurred within system-wide resolutions in Denmark, Hungary, Ireland, Italy, Slovenia and Spain. In the other cases, banks were resolved individually. As discussed before, system-wide resolutions usually involve more resources in an attempt to resolve systemic issues in the banking sector. Our findings indicate that non-performing loans decrease more strongly in system-wide resolutions together with a significant increase in deposit funding (see Panel G). The former finding could be related to the larger scale

of the resources involved in system-wide resolutions, while the latter to a reestablishment of market confidence. No significant differences are detected for the growth rate of lending or total assets.

Finally, we checked whether the impact of bad bank segregations differs across universal and narrow banks (Panel H). The findings suggest that universal banks' diversification ratios increased in response to bad bank segregations. One should however be cautious about drawing conclusions from this result, since both the survival analysis and the falsification test indicated that one should be careful in making causal inferences about this outcome variable. The other interaction terms are insignificant.

5. Conclusions

Using a comprehensive database for banks headquartered in 18 European countries over the period 2000–19, this paper studies the effectiveness of different types of banking crisis interventions. In total, our sample includes 130 banks and covers 40 bad bank segregation episodes and 33 state-funded recapitalisations.

In the paper, we use a difference-in-differences methodology that allows us, based on propensity scores, to estimate the impact of the two interventions on a set of bank balance sheet and performance indicators. The procedure helps us to select similar bank observations from the group of non-intervened banks and thereby to construct a counterfactual for the intervened banks. Our results show that bad bank segregations have been more effective than state-funded recapitalisations in restoring the lending capacity of banks in the post-resolution period. While recapitalised banks appear to use the injected funds to clean up balance sheets by reducing problem loans and cutting down on lending, banks that segregated assets increased progressively their lending after the creation of the bad bank. For both types of banking crisis interventions, we find a significant ex-post reduction in the cost of bank funding. We corroborate and validate our findings in different ways and check whether the timing of interventions is unaffected by the outcome variables, whether the different groups of banks are comparable prior to the interventions, and we perform an event study to assess the dynamic responses to the interventions. In all cases, the main results are confirmed.

We explore the heterogeneity of the bad bank resolutions to gauge whether their impact depends on the way they were implemented, their complexity and the institutional background. We do not detect significant differences in the effectiveness when the bad bank resolution is in majority privately or state funded, while we find a stronger reduction in the cost of debt when banks are resolved using asset disposition rather than asset restructuring vehicles. We also find evidence that in response to the resolutions occurring during the BRRD period the surviving banks appear to have regained the confidence of depositors. Another interesting finding is that the impact of the resolutions on banks depends on the effectiveness of contract enforcement in a

given country. When the enforceability of contracts is stronger, banks deleverage more importantly ex-post suggesting that the clean-up process is facilitated. In the other countries, we observe instead relatively higher increases in securities portfolios and deposit ratios.

To conclude, our results offer some implications on policy choices regarding bank resolutions. The first policy lesson is that impaired asset segregations have been more effective than state-funded recapitalizations in terms of promoting lending recovery in the intervened banks. The second policy lesson highlights the importance of a predictable framework for bank resolution. Two dimensions stand out. First, our findings show that the phased implementation of the BRRD facilitated a shift toward deposits, which is a more stable and cheaper source of funding for banks. Second, enforceable contracts stimulate the development of a secondary market for NPLs, which can help in the clean-up of bank balance sheets. The third policy lesson stresses the significance of the design of asset segregations in addressing the incentives of the parties involved. Specifically, we found that asset disposition vehicles performed better than asset restructuring vehicles, and that privately funded asset segregations appear to be more effective than state-funded recapitalisation.

Bibliography

Accornero, M., P. Alessandri, L. Carpinelli, L. and A. M. Sorrentino (2017). Non-performing loans and the supply of bank credit: Evidence from Italy. Bank of Italy Occasional Papers, n. 374.

Acharya V.V., L. Borchert, M. Jager, and S. Steffen (2021). Kicking the Can Down the Road: Government Interventions in the European Banking Sector. Review of Financial Studies. vol. 34(9), 4090-4131.

Argimón, I., Dietsch, M., and Á. Estrada (2018). Prudential filters, portfolio composition at fair value and capital ratios in European banks. Journal of Financial Stability, 39, 187-208.

Baudino, P., and H. Yun (2017). Resolution of non-performing loans – policy options. BIS, Financial Stability Institute, October 3.

Bayazitova, D. and A. Shivdasani (2012). Assessing TARP. Review of Financial Studies, vol. 25(2), 377-407.

BCBS (2017). High-level summary of Basel III reforms. Basel Committee on Banking Supervision.

Beccalli, E., P. Frantz, and F. Lenoci (2018). Hidden effects of bank recapitalizations. Journal of Banking and Finance, vol. 94, 297-314.

Bech, M., L. Gambacorta and E. Kharroubi (2014): "Monetary policy in a downturn: are financial crises special?", International Finance, vol. 17, no 1.

Beck, T. (2017) An asset management company for the Eurozone: Time to revive an old idea, VoxEU.org, 24 April.

Beck, T., S. Da-Rocha-Lopes, A. Silva, (2021). Sharing the Pain? Credit Supply and Real Effects of Bank Bail-ins. Review of Financial Studies, 34, (4) 1747-1788.

Beck, T., R. Levine and A. Levkov (2010). Big Bad Banks? The Winners and Losers from Bank Deregulation in the United States, The Journal of Finance, Vol. 65 (5), 1637-1667.

Brei, M., C. Borio, and L. Gambacorta (2020). Bank intermediation activity in a low-interest-rate environment. Economic Notes, Vol. 49(2). Also published as BIS Working Paper no. 807.

Brei, M. and L. Gambacorta (2016). Are bank capital ratios pro-cyclical? New evidence and perspectives, Economic Policy, 31(86): 357-453.

Brei, M., L. Gambacorta, and G. von Peter (2013). Rescue packages and bank lending. Journal of Banking and Finance, 37(2), 490-505.

Caliendo, M. and S. Kopeinig (2008). Some practical guidance for the implementation of propensity score matching. Journal of Economic Surveys (2008) Vol. 22, No. 1, pp. 31–72

Cerruti, C. and R. Neyens (2016). Public asset management companies: A toolkit. A World Bank Study. The World Bank Group.

Claessens, S., S. Djankov, and L. Klapper (2003). Resolution of Corporate Distress: Evidence from East Asia's Financial Crisis. Journal of Empirical Finance 10 (1-2), 199-216

Cochran, W.G. (1968). The Effectiveness of Adjustment by Subclassification in Removing Bias in Observational Studies. Biometrics, 24(2): 295-314.

Cole, S.R. and M.A Hernan (2008). Constructing Inverse Probability Weights for Marginal Structural Models. American Journal of Epidemiology, 168, 656-664.

Colliard, J.-E. and D. Gromb (2017). "Financial Restructuring and Resolution of Banks" HEC mimeo, May.

Dado, M. E. and D. Klingebiel (2002). Decentralized Creditor-Led Corporate Restructuring. Cross-Country Experience. World Bank Financial Sector Operations and Policy Department, Working paper n. 2901.

Dehejia, R.H. and S. Wahba (2002). Propensity Score-Matching Methods for Nonexperimental Causal Studies. The Review of Economics and Statistics. 84(1), 151-161.

Demirgüç-Kunt, A., and E. Detragiache (2005). Cross-country empirical studies of systemic bank distress: a survey. National Institute Economic Review, 192(1), 68-83.

Dobkin C., A. Finkelstein, R. Kluender, M.J. Notowidigdo (2018). The Economic Consequences of Hospital Admissions. The American Economic Review 102, 308-352.

ESRB (2017). Resolving non-performing loans in Europe. European Systemic Risk Board, Report July 2017.

European Commission (2018). AMC Blueprint. Brussels, 14.3.2018, SWD (2018) 72 final.

European Commission (2021). Non-performing Loans - New risks and policies?, IPOL, Economic Governance Support Unit

Farhi, E. and J. Tirole (2012). Collective moral hazard, maturity mismatch, and systemic bailouts. The American Economic Review, 102, 1, 60-93.

FROB (2011). The restructuring of the Spanish Banking System. Section 2, Presentation of the Fondo de Reestructuracion Ordenada Bancaria.

Gambacorta, L., M. Scatigna, and J. Yang (2014). Diversification and bank profitability: A nonlinear approach, Applied Economics Letters, 21(6).

Gandrud, C., and M. Hallerberg (2014). The Bad Banks in the EU: The Impact of Eurostat Rules. Bruegel Working Paper 15, December.

Gelpern, A. and N. Véron (2021). European Banking Reform Should Embrace a Unitary Approach to Failed Banks. ifo Institute, Munich, CESifo Forum 22(1).

Giannetti, M., and A. Simonov (2013). On the Real Effects of Bank Bailouts: Micro Evidence from Japan. American Economic Journal: Macroeconomics, 5(1).

Goodhart, C., and Avgouleas, E. (2016). Critical reflections on bank bail-ins. In The Reform of International Economic Governance, Routledge, 75-99.

Grosse-Rueschkamp, B., S. Steffen, and D. Streitz (2019). A capital structure channel of monetary policy. Journal of Financial Economics, vol. 133.

Haldane, A., and Kruger, M. (2002). The resolution of international financial crises: private finance and public funds. Bank of Canada Review, (Winter), 3-13

HM Treasury (2013). RBS and the case for a bad bank: The Government's review. HM Treasury, United Kingdom.

Homar, T. and S.J.G. van Wijnbergen (2017). Bank recapitalization and economic recovery after financial crises, Journal of Financial Intermediation, vol. 32.

Honohan, P. and D. Klingebiel (2000). Controlling the Fiscal Costs of Banking Crises. Available at SSRN: https://ssrn.com/abstract=282514.

Huerta, J.P. (2019). FROB in the restructuring of the spanish banking system. Where it stands after a decade of activity (2009-2019) and considerations for the banking union. Financial Stability Review, Issue 36, Banco de Espana.

Imbens, G. and D. Rubin (2015). Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction. Cambridge University Press.

Imbens, G. W. and J.M. Wooldridge (2009). Recent Developments in the Econometrics of Program Evaluation. Journal of Economic Literature, 47, 5-86.

IMF (2012). Spain: The Reform of Spanish Savings Banks Technical Notes. IMF Country Report No. 12/141. International Monetary Fund.

IMF (2015). A Strategy for Resolving Europe's Problem Loans. Staff Discussion Notes/15/19. International Monetary Fund.

Jagtiani, J., I. Kotliar, and R.Q. Maingi (2016). Community bank mergers and their impact on small business lending. Journal of Financial Stability, 27, 106-121.

Kahn, C. and A. Winton. (2004). Moral Hazard and Optimal Subsidiary Structure for Financial Institutions, The Journal of Finance, 69, 2531-2576.

Klingebiel, D. (2000). The use of asset management companies in the resolution of banking crises – cross-country experiences, Policy Research Working Paper, 2284, World Bank, Washington DC.

KPMG (2016). European debt sales. Portfolio Solutions Group, KPMG International.

Lechner, M. (2001). Identification and Estimation of Causal Effects of Multiple Treatments under the Conditional Independence Assumption. In Econometric Evaluation of Labour Market Policies, ed. M. Lechner and F. Pfeiffer, 43-58. Heidelberg and New York: Physica; Mannheim: Centre for European Economic Research.

Lunt, M. (2014). Propensity Analysis in Stata. Revision: 1.1. Manuscript, University of Manchester.

McKinsey (2009a). Bad banks: Finding the right exit from the financial crisis. McKinsey Working Papers on Risk 12, August.

McKinsey (2009b). Understanding the bad bank, by G. Brenna, T. Poppensieker, and S. Schneider, McKinsey, Zurich and Munich Offices, December.

Mariathasan, M., and O. Merrouche (2012). Recapitalization, credit and liquidity. Economic Policy, 27, 72, 603-646.

Morrison and Foerster (2009). Good-bank-Bad-bank: A clean break and a fresh start. Attorney Advertisement, Morrison & Foerster LLP.

New York Times (1988). Mellon Set to Shift Bad Loans. July 26.

Pancotto, L., O. ap Gwilym, and J. Williams (2019). The European Bank Recovery and Resolution Directive: A market assessment. Journal of Financial Stability, 44, 100689.

Parigi, B.M. (2017). National and supranational banking regulators: Between delayed intervention and time inconsistency. Banque de France, Financial Stability Review, 21, 87-94.

Philippon, T. and P. Schnabl (2013). Efficient Recapitalization. Journal of Finance, 68, 1-42.

Rosenbaum, P.R. (1989). Optimal Matching for Observational Studies. Journal of the American Statistical Association, 84(408), 1024-32.

Rosenbaum, P.R., and D.B. Rubin (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. Biometrika, 70(1), 41–55.

Rosenbaum, P.R., and D.B. Rubin (1984). Reducing Bias in Observational Studies Using Sub classification on the Propensity Score. Journal of the American Statistical Association, 79(387): 516-24.

Rubin, D.B. (2006). Matched Sampling for Causal Effects. Cambridge and New York: Cambridge University Press.

Sato, T. and Y. Matsuyama (2003). Marginal Structural Models as a Tool for Standardization. Epidemiology, 14, 6, 680-686.

Sloczyński, T. and J.M. Wooldridge (2018). A General Double Robustness Result for Estimating Average Treatment Effects. Econometric Theory, 34, 112-133.

Thomson, J.B. (2011). How to build a bad bank - for the greater good. Forefront, Federal Reserve Bank of Cleveland Winter, 2(1), 12-16.

Tirole, J. (2012). Overcoming Adverse Selection: How Public Intervention Can Restore Market Functioning. The American Economic Review 102, 29--59.

Uysal, S.D. (2015). Doubly Robust Estimation of Causal Effects with Multivalued Treatments: An Application to the Returns to Schooling. Journal of Applied Economics, 30: 763– 786

Zhao, Z. (2004). Using Matching to Estimate Treatment Effects: Data Requirements, Matching Metrics, and Monte Carlo Evidence. Review of Economics and Statistics, 86(1): 91-107.

Tables and Figures

Table 1: Characteristics of the database (2000–19)

	Total assets of banks, end-2019	Banks	Recapitalised banks	Restructured banks	Total amount of recapitalisation	Total assets of bad banks	State-funded bad banks	Loans	NPLs	M&As
	Billion EUR		*No. of entities*		*Billion EUR*	*Billion EUR*	*No. of entities*	*% of assets*	*% of loans*	*No.*
Austria	389	8	4	2	8.0	45.0	2	54.2	9.8	2
Belgium	490	5	1	2	17.2	154.2	1	44.9	3.2	1
Cyprus	45	3	0	0			0	55.5	27.0	0
Denmark	880	14	0	7	1.5	6.5	7	63.3	3.2	9
France	7127	6	5	0	15.9		0	35.2	3.8	10
Germany	3174	14	4	2	40.4	87.5	1	48.8	4.3	10
Greece	285	4	4	0	23.4		0	60.0	33.0	4
Hungary	97	6	0	4		2.0	4	62.1	11.8	2
Ireland	253	5	0	4	43.3	45.0	0	67.9	8.2	0
Italy	2165	15	4	4	4.0	1.2	0	64.0	7.5	22
Latvia	12	4	0	1	0.5	1.3	1	43.4	13.4	1
Netherlands	2005	5	2	1	17.3	4.8	1	63.4	2.2	0
Portugal	231	5	3	1	6.9	2.9	0	66.4	4.9	5
Slovenia	11	4	0	3	1.3	1.0	3	52.8	14.4	1
Spain	3029	17	3	8	41.8	45.8	0	62.9	5.6	35
Sweden	1354	4	1	0	0.7		0	64.6	2.1	4
Switzerland	2010	5	0	1	5.7	54.2	1	56.0	1.7	2
United Kingdom	6192	6	2	0	61.5	0.0	0	55.2	3.2	13
Average*/sum	29748	130	33	40	289.4	451.3	21	56.7*	8.8*	121

The information covers 130 banks over the period 2000-19 (33 received a state-funded recapitalisation (without subsequent bad bank segregation) and 40 received a bad bank resolution (8 without a prior recapitalisation). "Year of intervention" shows the average year in which interventions took place. "Total amount of recapitalisation" shows the amount of capital injections by country. "Total assets of bad banks" indicates the amount of assets transferred from the originating bank to the bad bank. "State-funded bad banks" refer to resolutions where the bad banks are majority-state funded. "M&A" reports the number of mergers, acquisitions and restructurings that have been taken into account. "Average*/sum" indicates unweighted averages (*) or sums over countries. Sources: Fitch Connect; Brei et al. (2013); Press Releases. Authors' calculations.

Table 2: Variable descriptions

Variable	***Definition***	***Sources[(1)]***
	Outcome variables	
Loan growth	Annual growth rate of total loans, local currency	Fitch Connect
NPL growth	Annual growth rate of non-performing loans, local currency	Fitch Connect
Total asset growth	Annual growth rate of total assets, local currency	Fitch Connect
Securities ratio	Total securities/total assets	Fitch Connect
Deposit ratio	Total customer deposits/total funding	Fitch Connect
Diversification ratio	Total non-interest (NI) operating income/(interest income + NI operating income)	Fitch Connect
Cost of debt	Total interest expense/total funding	Fitch Connect
Risk density	Risk-weighted assets /total assets	Fitch Connect
ROE	Net income/total equity	Fitch Connect
	Resolution events	
Recapitalised bank	=1, in the years after a bank received a state-funded recapitalisation without asset transfer	Brei et al. (2013); individual reports
Restructured bank	=1, in the years after a bank transferred assets to a bad bank	See Table B1
	Control variables	
Size (t-1)	Logarithm of total assets	Fitch Connect
Short-term (S-T) funding (t-1)	(Short-term and money market funding)/total assets	Fitch Connect
Liquidity constrained	=1, if liquid asset ratio "(Available-for-sale securities + cash and due from banks + trading securities)/total assets" is in the 1st decile of the distribution	Fitch Connect; authors' calculations
Capital buffer (t-1)	Difference between the actual Tier1 risk-weighted asset ratio and the regulatory minimum	Fitch Connect; central bank reports; authors' calculations
Leverage constrained	=1, if difference between the leverage ratio (Tier1 divided by total assets) and the announced minimum is in the 1st decile of the distribution post-2010	Fitch Connect; authors' calculations
IFRS	=1, once a bank changed accounting standards to IFRS	Fitch Connect
ΔInterest rate (t-1)	Annual change in the 3-month interbank rate	Central banks; BIS
Real GDP (t-1)	Annual growth rate in real GDP	Central banks; BIS
Gov. debt/GDP (t-1)	Market value of government debt as a percentage of GDP	BIS credit statistics
Banking crisis	=1, if banking crisis	Brei et al. (2020)

This table reports the names and definitions of the variables used in the regressions along with the data sources. [(1)] Data from Fitch Connect have been adjusted for mergers and acquisitions.

Table 3: Summary statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Outcome variables					
Loan growth	1809	5.21	15.50	-37.80	133.78
NPL growth	1503	19.32	63.24	-65.27	357.89
Total asset growth	1809	4.98	14.39	-31.61	124.95
Securities ratio	1809	22.94	12.51	1.28	61.09
Deposit ratio	1809	50.50	20.54	0.00	99.94
Diversification ratio	1809	21.54	12.68	-9.94	66.30
Cost of debt	1809	2.44	1.75	0.12	19.51
Risk density	1607	44.87	20.71	8.03	102.77
ROE	1809	3.56	18.65	-99.17	31.00
Bank interventions					
Recapitalised bank	1809	0.15	0.35	0	1
Restructured banks	1809	0.07	0.25	0	1
Control variables					
Size (t-1)	1809	3.36	3.45	-7.52	8.24
Short-term (S-T) funding (t-1)	1809	21.40	12.25	0.04	83.81
Liquidity constrained	1809	0.05	0.21	0	1
Capital buffer (t-1)	1809	7.31	4.55	-7.92	38.30
Leverage constrained	1809	0.06	0.24	0	1
IFRS	1809	0.74	0.44	0	1
ΔInterest rate (t-1)	1809	-0.21	0.95	-4.25	2.95
Real GDP (t-1)	1809	1.56	2.77	-10.13	25.16
Gov. debt/GDP (t-1)	1809	73.70	31.64	4.76	152.19
Banking crisis	1809	0.27	0.45	0	1

Variable definitions are reported in Table 2.

Table 4: The impact of public recapitalization and bad bank restructuring

	(I) Loan growth	(II) NPL growth	(III) Total asset growth	(IV) Securities ratio	(V) Deposit ratio	(VI) Diversification ratio	(VII) Cost of debt	(VIII) Risk density	(IX) Return on equity
Panel A: Baseline model									
Recapitalisation	**-6.83***	**-24.33****	**-6.10****	0.45	**3.90***	1.83	**-0.90*****	-3.80	-0.85
	(3.47)	**(10.86)**	**(2.79)**	(1.42)	**(2.01)**	(1.31)	**(0.27)**	(2.33)	(3.62)
Restructuring	**-3.64***	**-28.78****	**-5.81****	4.13	2.97	3.45	**-0.95*****	3.84	8.11
	(2.08)	**(10.77)**	**(2.26)**	(3.17)	(2.11)	(3.01)	**(0.30)**	(3.23)	(5.25)
Panel B: Baseline model with inverse probability weighting									
Recapitalisation	**-8.61****	**-27.3****	**-7.05****	0.14	**4.67****	1.32	**-1.02*****	-2.90	-2.27
	(4.06)	**(10.9)**	**(2.90)**	(1.31)	**(2.03)**	(1.87)	**(0.30)**	(2.41)	(4.61)
Restructuring	-2.71	**-23.2****	**-7.15****	2.40	1.71	4.50	**-1.19*****	2.39	6.72
	(1.71)	**(9.35)**	**(2.57)**	(3.89)	(1.89)	(4.52)	**(0.31)**	(3.36)	(5.79)
Panel C: Baseline model with stratification									
Recapitalisation	**-6.94***	**-22.2***	**-6.10****	0.33	2.98	1.20	**-0.83*****	-3.26	-1.17
	(3.33)	**(10.8)**	**(2.73)**	(1.41)	(1.79)	(1.43)	**(0.26)**	(2.23)	(3.68)
Restructuring	-3.14	**-23.3****	**-5.51****	3.69	1.69	3.32	**-0.94*****	3.44	8.99
	(2.02)	**(10.9)**	**(2.36)**	(2.85)	(1.86)	(2.80)	**(0.30)**	(2.90)	(5.23)
Obs.	1809	1503	1809	1809	1809	1809	1809	1607	1809
Panel D: Baseline model with PS matching									
Recapitalisation	**-5.95***	**-27.9****	**-5.36****	0.24	**3.79***	2.11	**-0.90*****	-3.61	-0.010
	(3.10)	**(10.9)**	**(2.31)**	(1.33)	**(1.86)**	(1.41)	**(0.25)**	(2.23)	(3.70)
Restructuring	-2.49	**-38.7*****	**-5.03***	3.83	2.46	3.12	**-0.93*****	4.48	**9.04***
	(2.18)	**(10.5)**	**(2.51)**	(3.26)	(1.55)	(3.22)	**(0.30)**	(3.31)	**(4.95)**
Obs.	1367	1152	1367	1367	1367	1367	1367	1207	1367

The table shows the regression results on the impact of bank interventions, as specified in Equation (1). The recapitalisation (restructuring) indicator is equal to one in the years after public recapitalisations (bad bank segregations) and zero otherwise. The regressions include bank-specific and macroeconomic control variables, see Tables 2 and 3. Detailed results can be found in the online annex in Tables A2 to A5. All regressions control for bank fixed effects. Standard errors are clustered at the country-level and appear in brackets. (***, **, *) indicate significance at the 1, 5 and 10% level.

Table 5: The timing of bank interventions and pre-existing outcomes

	(I) Loan growth	(II) NPL growth	(III) Total asset growth	(IV) Securities ratio	(V) Deposit ratio	(VI) Diversification ratio	(VII) Cost of debt	(VIII) Risk density	(IX) Return on equity
Outcome y_{it}	1.014	1.003	1.022	0.989	0.988	**0.954*****	**1.102****	0.995	1.006
	(0.023)	(0.003)	(0.024)	(0.016)	(0.012)	**(0.013)**	**(0.048)**	(0.0166)	(0.013)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	130	120	130	130	130	130	130	125	130
No. of interventions	73	67	73	73	73	73	73	68	73

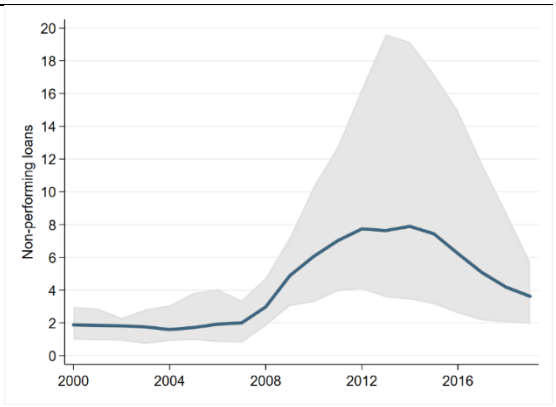
The regressions are estimated by the Cox Proportional-Hazards model where the dependent variable is the hazard of time to intervention (recapitalization/restructuring). The sample covers the period 2000-19 and the banks from our baseline regressions. The explanatory variables (outcomes and controls) are 5-year averages prior to the interventions, if applicable, and averages over the sample period otherwise. The outcome variables are indicated in the top of each column. The included control variables are those shown in Table 2. Hazard ratios are reported with clustered standard errors at the country-level in brackets. (***, **, *) indicate significance at the 1, 5 and 10% level. Detailed results can be found in Table A1 in the online annex A.

Table 6: The differential impact of bad bank restructuring

	(I) Loan growth	(II) NPL growth	(III) Total asset growth	(IV) Securities ratio	(V) Deposit ratio	(VI) Diversif ication ratio	(VII) Cost of debt	(VIII) Risk density	(IX) Return on equity
Panel A: Baseline without interaction terms (Panel B of Table 4)									
Restructuring	-2.71	**-23.2****	**-7.15****	2.40	1.71	4.50	**-1.19*****	2.39	6.72
	(1.71)	**(9.35)**	**(2.57)**	(3.89)	(1.89)	(4.52)	**(0.31)**	(3.36)	(5.79)
Panel B: Private vs. state-funded restructurings									
Restructuring	-4.63	**-42.4****	**-9.01*****	-0.40	3.93	0.24	-1.77	**12.3****	10.1
	(2.85)	**(20.0)**	**(2.64)**	(3.39)	(3.00)	(3.72)	(1.38)	**(5.37)**	(6.02)
Restructuring*public	2.78	29.3	2.69	4.06	-3.21	6.16	0.84	**-13.6***	-4.90
	(3.44)	(25.8)	(3.39)	(6.36)	(4.14)	(6.88)	(1.70)	**(7.45)**	(9.94)
Panel C: Asset restructuring vehicle (ARV) vs. asset disposition vehicle (ADV)									
Restructuring	-2.60	**-27.9****	**-8.37*****	2.88	2.53	**8.94***	**-1.45****	2.73	11.1
	(1.81)	**(11.1)**	**(2.60)**	(4.65)	(2.37)	**(4.78)**	**(0.62)**	(4.88)	(6.48)
Restructuring*ARV	-0.33	14.4	3.48	-1.37	-2.34	**-12.7****	0.76	-0.97	-12.5
	(4.17)	(22.1)	(3.81)	(5.18)	(4.36)	**(5.93)**	(1.03)	(6.00)	(10.4)
Panel D: Before vs. after Bank Recovery and Resolution Directive (BRRD)									
Restructuring	-1.90	**-27.8****	**-10.6*****	0.69	-0.89	3.48	**-1.05****	1.85	5.08
	(1.81)	**(11.8)**	**(3.01)**	(3.13)	(1.52)	(4.99)	**(0.41)**	(2.77)	(5.31)
Restructuring*BRRD	-1.36	7.52	**5.84*****	2.87	**4.36***	1.71	-0.22	0.87	2.76
	(1.67)	(12.3)	**(1.89)**	(2.94)	**(2.08)**	(2.31)	(0.41)	(2.14)	(6.45)
Panel E: Large vs. small banks									
Restructuring	-2.60	**-35.1****	-3.36	**8.18***	**4.85****	2.04	**-0.90****	1.00	10.3
	(3.17)	**(13.2)**	(2.65)	**(4.39)**	**(2.20)**	(3.40)	**(0.41)**	(5.23)	(8.01)
Restructuring*large	-0.30	**29.7****	**-9.79*****	**-14.9*****	**-8.09*****	6.34	-0.74	4.23	-9.14
	(4.46)	**(12.5)**	**(3.08)**	**(4.96)**	**(2.22)**	(10.5)	(0.52)	(6.33)	(8.14)
Panel F: Weak vs. strong enforceability of contracts									
Restructuring	**-3.38****	**-30.0****	**-9.31*****	-1.69	0.25	4.71	**-1.05*****	**5.45***	5.71
	(1.51)	**(12.1)**	**(2.27)**	(2.76)	(1.50)	(5.50)	**(0.32)**	**(2.70)**	(4.35)
Restructuring*weak	2.76	27.7	**8.86*****	**16.8****	**6.00****	-0.86	-0.54	-11.0	4.15
	(3.64)	(22.8)	**(3.05)**	**(7.48)**	**(2.13)**	(8.39)	(0.45)	(8.59)	(16.4)
Panel G: System-wide vs. individually targeted rescues									
Restructuring	-2.35	-6.30	**-9.93*****	-2.38	-0.53	4.34	**-1.58*****	3.13	0.10
	(2.14)	(10.6)	**(2.73)**	(3.17)	(1.70)	(7.64)	**(0.37)**	(2.39)	(6.18)
Restructuring*system	-0.80	**-36.7****	6.12	10.5	**4.95***	0.33	0.86	-1.54	14.6
	(4.01)	**(16.2)**	(3.66)	(6.93)	**(2.75)**	(8.16)	(0.50)	(7.95)	(9.52)
Panel H: Universal vs. narrow banks									
Restructuring	-2.74	**-22.8***	**-6.23****	3.67	1.83	-1.03	**-1.29*****	3.82	6.70
	(2.36)	**(11.5)**	**(2.62)**	(4.54)	(2.29)	(3.08)	**(0.40)**	(4.53)	(6.42)
Restruct.*universal	0.12	-1.09	-3.36	-4.60	-0.41	**20.1*****	0.38	-4.41	0.082
	(2.96)	(18.4)	(3.97)	(6.32)	(3.96)	**(5.85)**	(0.48)	(6.04)	(8.60)
Obs.	1809	1503	1809	1809	1809	1809	1809	1607	1809

The table shows the regression results on the impact of bank interventions, as specified in Equation (4). State-funded restructurings refer to majority state-funding rescues; ARV (ADV) to asset restructuring (disposition) vehicles; BRRD refers the Bank Recovery and Resolution Directive (2014 onwards); large banks are banks with total assets in the upper quartile; weak enforcement refers to SI, IT, CY, GR, PT, and HU; system-wide rescues to ES, HU, IE, SI, IT and DK; and universal banks to banks with a diversification ratio in the upper quartile. All regressions control for bank fixed effects. Only the coefficients on restructurings are shown with standard errors clustered at the country-level in brackets. (***, **, *) indicate significance at the 1, 5 and 10% level.

Figure 1. Credit risks in Europe

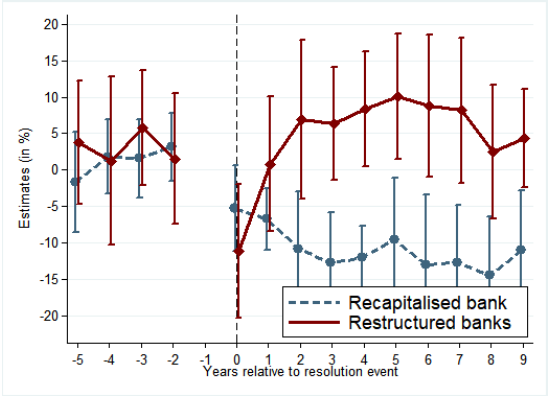


Note: In percent of total loans. The figure shows the annual median along with the 25th and 75th percentile of the non-performing loan ratio for a sample of 130 banks.

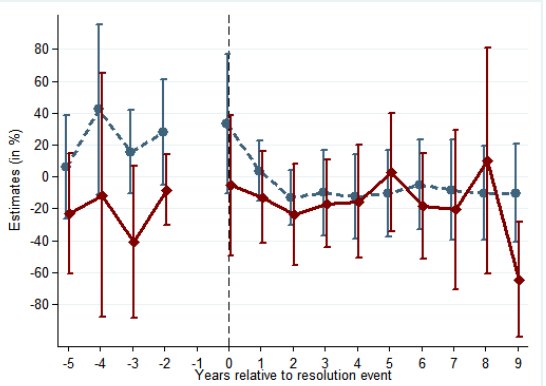
Sources: Fitch Connect, authors' calculations.

Figure 2: The dynamics of bank rescues

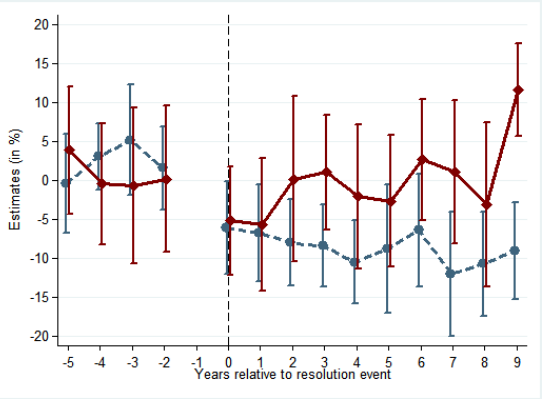
(I) Bank lending growth rate



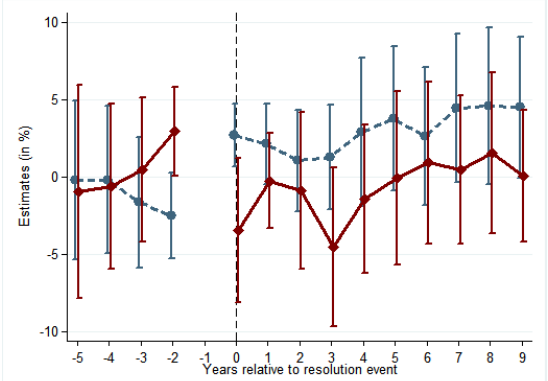
(II) Non-performing loans growth rate



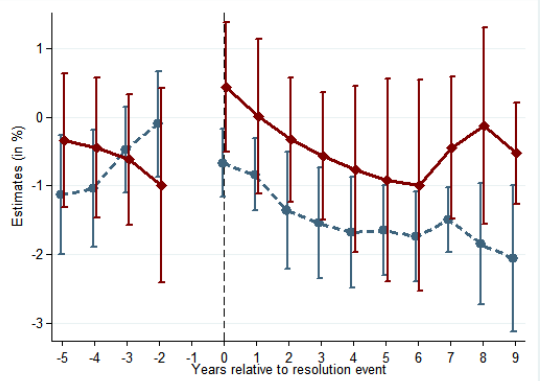
(III) Total assets growth



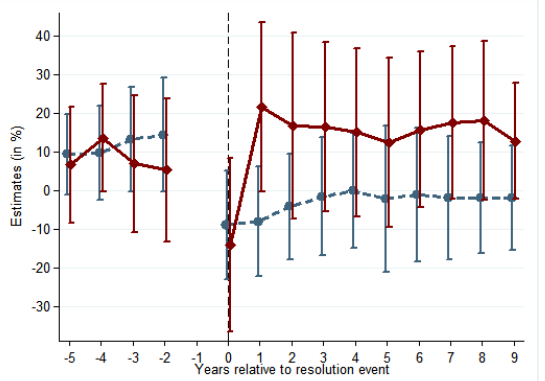
(IV) Deposits ratio



(V) Cost of debt



(VI) Return on equity



The figures plot the impact coefficients α_τ of (i) recapitalisations and (ii) bad bank segregations on various bank indicators. For details, see Equation (3). The results are obtained using OLS with bank-level fixed effects weighetd with the inverse probabilty of treatment. We include impact coefficients for each year relative to the resolutions except for the year before (t-1), thus estimating the dynamic impact of resolutions relative to the year prior to the resolutions. The figures show the impact coefficients for a 15-year window around the resolutions. The vertical lines represent 95% confidence intervals based on standard errors clustered by country. Table 2 provides definitions of the dependent variables and Table A10 in the online annex detailed estimation results.