How Do Bank Resolution Reforms Affect Banks' Implicit Subsidies?

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Abstract: We investigate the effect of the implementation of resolution reforms on the implicit subsidy of banks across 19 jurisdictions. Following Gandhi and Lusting (2015), we measure banks' government implicit subsidy using equity abnormal returns. We find that the implementation of bank resolution reforms does not reduce the implicit guarantee of large banks. However, we document an increase in the abnormal returns of non-large banks after the implementation of these resolutions, suggesting a reduction in their implicit subsidies. Non-large banks also reduce their risk-taking compared to large banks. Both sets of findings indicate that, while resolution regulations alter the perceived implicit guarantees for non-large banks, they do not impact investors' perceptions of Too-Big-to-Fail (TBTF) for large banks. Our inferences are robust to addressing the endogeneity of the implementation of resolutions reforms across countries. These findings alert regulators to the potential ineffectiveness of resolution policies in changing investors' beliefs about TBTF.

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1. Introduction

The turmoil caused by the failure of Silicon Valley Bank (SVB) in March of 2023 has triggered a bank run from medium and small banks to large banks in the US as depositors perceived them as safer (Caglio *et al*., 2024). According to Benoit *et al*. (2023), US largest banks received an additional $120 billion in deposits on the days following SVB's failure, whereas other banks have lost $108 billion in deposits in the period. The flight toward large banks has continued even after the announcement of a blanket guarantee on SVB's liabilities, while SVB's shareholders have been wiped out. In the same month, the Swiss Government used emergency powers to make $280 billion available to support UBS's purchase of Credit Suisse (Ridley *et al*., 2023). Even if Credit Suisse's acquisition was not technically a bailout, public resources were used to guarantee the purchase operation. This heterogeneity in resolution procedures has reignited the debate about the tools to deal with distressed financial institutions. The bank resolution framework, developed in response to the Global Financial Crisis, aims at enhancing authorities' control over bank resolution by establishing a common approach to the recovery and resolution while minimizing reliance on taxpayer money to support financial firms (FSB, 2021a).

Our paper investigates whether bank resolution reforms have succeeded in their objective. If these resolution mechanisms are credible for investors, they must reduce the *ex-ante* implicit subsidies enjoyed by banks. These implicit subsidies are derived from investors' perception that banks will receive some form of governmental support in case of distress. Therefore, we explore how the implementation of resolution regulations affects banks' implicit subsidies. We use the Financial Stability Board (FSB)'s classification to determine the timing of bank resolution reforms across different jurisdictions. Our sample comprises 1,544 publicly traded banks from 19 countries that are members of the FSB community between 2002 and 2021.

We follow Gandhi and Lustig (2015) and use the risk-adjusted equity returns (alphas) of individual banks as a measure of implicit subsidies. The expectation that the government will inject capital to ensure a bank's survival in case of distress results in an abnormally low cost of equity financing for that bank (Gandhi and Lustig, 2015). In this case, the *ex-ante*

equilibrium cost of capital of the bank is lower than that of an otherwise similar firm with identical sensitivity to standard risk factors. Even if bank shareholders are not expected to be bailed out, but there is an expected guarantee to some or all of the bank's liabilities, the bank's cost of equity capital will be lower *ex-ante*, because depositors and other debtholders will accept a lower interest rate, and the rents of this implicit subsidy are transferred to shareholders (Acharya *et al.*, 2014; Oliveira *et al.*, 2015).

Ex-ante implicit subsidies can have detrimental side-effects on financial stability. They reduce banks' borrowing costs (see Berger and Roman, 2020, for an updated review) along with the costs of equity capital (Gandhi and Lustig, 2015; Gandhi *et al.*, 2020), and ultimately increase moral hazard, reduce market discipline, and generate excessive risk taking. Heterogeneous implicit subsidies across banks can also distort competition (Dam and Koetter, 2012), particularly when countries have fiscal capacity to inject substantial resources into the financial system (Acharya *et al.*, 2014; Leonello, 2018; Schiozer *et al.*, 2018). *Ex-post*, bank bailouts deteriorate public finances and produce popular dissatisfaction, as they result in larger sovereign risk (Mian *et al.*, 2014) and reduce market discipline (Berger et al. 2023).

The bank resolution framework aims at transferring the costs of resolving failing banks from the taxpayers to private investors. It includes, among other policies, a bail-in procedure in case of distress. Under the bail-in procedure, regulators can require that the shareholders inject additional funds to ensure a healthy capital structure for the bank if they spot that the bank is in distress (see the Double Liability approach in Anderson *et al.*, 2018). In addition, if a bank approaches bankruptcy, equity holders may be wiped out, while subordinated, unsecured, and contingent convertible debtholders may absorb losses, and these debt instruments may be (fully or partially) converted into equity capital to recapitalize the bank. Therefore, the regulatory prior is that bank resolution reforms should reduce the implicit subsidies of all types of banks, as the regulatory framework does not restrict the application of resolution tools, such as the bail-in mechanism, exclusively to large banks. However, the regulatory frameworks acknowledge that the impact of a bank's failure can vary with its size and systemic importance, and are particularly designed to address the challenges posed by TBTF institutions, while allowing for proportionality in their application. Consequently, the primary effects of resolution reforms should be expected to manifest in large institutions, as

these reforms are aimed mainly at mitigating the risks associated with their failure. Moreover, the credibility and effectiveness about the adoption of bank resolution measures is key to establish market discipline *ex-ante* and reduce the perceptions of an implicit guarantee among investors. Even if bank resolution regulations are in place, investors may still expect some support for banks, and this support may be heterogeneous across banks, depending on their systemic importance (the TBTF doctrine).[4]

Determining which banks (if any) are most affected by these regulations is an empirical issue, as the effect depends on the perception that investors have about possible governmental support to each bank prior to the adoption of these regulations and after it. We analyze separately the effect of the adoption of bank resolution regulations on the implicit subsidies of large and non-large banks in the jurisdictions that are part of the FSB community. We adopt a data-driven procedure and provide several different definitions of large banks. Our results indicate that the average abnormal returns were negative across all bank sizes prior to the adoption of the resolution framework, suggesting that most banks benefited from some form of implicit subsidy in the absence of bank resolution regulations. Notably, our estimates of implicit subsidies are higher for the largest banks in each country[5] in comparison to non-large banks, consistent with the perception that larger banks were "more protected" by governments because the failure of these institutions could cause the entire financial system to collapse.

Gauging the causal effect of a regulation imposes challenges, as identifying a proper control group that is not affected (or less affected) by these regulations is not straightforward. In our baseline specifications, our treatment group is comprised of the banks in the FSB member countries that have fully adopted bank resolution mechanisms (in the definition of the FSB) at some point during our sample period. Our control group is the group of banks in FSB member countries that have never been treated by these regulations. Because the implementation of bank resolution reforms occurs at different points in time for each

[4]For instance, when in 2023 UBS Group AG agreed to take over Credit Suisse Group AG, the Swiss National Bank provided UBS with a 100-billion-franc liquidity line, and the Swiss Department of Finance offered a 9-billion-franc guarantee for potential losses on Credit Suisse assets (Wang, 2023).

[5] In our main tests, large banks are the five with the largest total assets per year in each country. We also use several alternative definitions of large banks in our robustness checks, and our inferences hold.

jurisdiction, we use a staggered differences-in-differences setup[6] to gauge the effect of the adoption of the resolution framework on the implicit subsidy of banks.

We find that the adoption of these regulations is associated with an increase of approximately 4.6 percentage points in the abnormal returns of non-large banks in comparison to jurisdictions that did not adopt such regulations, suggesting that bank resolution reforms reduce the implicit subsidies for these banks. However, we find that these same resolution reforms do not reduce the implicit subsidy of large banks and GSIBs.

In other tests, we also acknowledge the gradual implementation of such reforms, and use a granular measure that captures the degree of implementation of bank resolution regulations in each jurisdiction (FSB's Resolution Reform Index, *RRI*). Our inferences using this granular measure are similar, *i.e.*, the approval of regulations towards the adoption of resolution mechanisms substantially decrease the implicit subsidies of non-large banks, but have a much smaller effect, if any, on large banks' subsidies.

Our findings suggest that the too-big-to-fail problem remains essentially unresolved at present. The results show that investors perceive that bank resolution regulations hinder public authorities from providing support to troubled non-large banks. However, these same regulations are insufficient to significantly alter investors' expectations that governments will provide some form of support for large banks in distress. Indeed, Havemann (2019) shows that bail-in procedures have unintended *ex-post* consequences to the financial system, particularly when banks are sufficiently interconnected (a common characteristic of large, TBTF banks). In such cases, regulators often implement complementary interventions that mitigate investor losses and reduce the systemic impact of a bank liquidation. Therefore, even if bank resolution regulations prevent authorities from providing a plain bailout, investors might expect regulators to step in and provide alternative forms of support to large banks, other than a bailout, before they are forced to induce a socially costly bail-in of these banks. Furthermore, even when regulators have no choice but to impose a bail-in on a large bank, they might still be more likely to undertake other interventions to reduce the losses of investors and mitigate the systemic consequences of the bail-in. In addition, Pandolfi's (2022) model shows that bail-

[6] Specifically, we use both Gormley and Matsa's (2011) stacked approach, and Callaway and Sant'Anna's (2022) approach.

in expectations may reduce the banks' *ex-ante* incentives to monitor loans, therefore increasing moral hazard, rather than decreasing it.

One possible critique to the causal interpretation of our inferences is that countries self-select into adopting bank resolution regulations. Indeed, bank industry lobbying, government ideology, and the relative importance of the financial sector to the economy can heterogeneously affect the likelihood of each country adopting bank resolution regulations. If these features are also correlated with banks' implicit subsidies, the associations we describe above are not necessarily causal relationships. To further ensure the causal interpretation of our results, we follow Beck *et al.* (2020) and use the countries' number of past crises as an instrumental variable for the country's degree of resolution reform adoption (*RRI*). Beck *et al.* (2020) argue that, the more financial crises a country has gone through, the greater is the population's political awareness about their consequences, and thus the larger is the political cost of maintaining policies that do not prevent using taxpayer money to address these crises. The results obtained using this instrumental variable approach confirm and strengthen our previous findings, *i.e.*, bank resolution regulations reduce the implicit subsidies of non-large banks, but have little or no effect on the subsidies of large banks.

We also test the effect of bank resolution regulations on the risk-taking behavior of banks. If these regulations affect implicit subsidies, they should have an effect on bank risk-taking. For non-large banks, we find that resolution reforms decrease bank risk-taking. In other words, non-large banks in countries that fully adopted bank resolution regulations reduce their risk-taking behavior relative to banks in countries that have not fully adopted them. This decrease is economically significant and indicates a possible reduction in moral hazard arising from the loss of expected government protection in case of bankruptcy, consistent with the moral hazard literature (Boyd and Gertler, 1994; Hakenes and Schnabel, 2010; Curti et al., 2022). However, we do not find any effect of resolution reform adoption on the risk-taking of large banks, again suggesting that bank resolution reforms do not seem to affect the TBTF status of large banks.

We also explore heterogeneities across regulations by comparing the effect of bank resolution reforms in North America (US and Canada) versus Europe, as differences among countries are likely to influence market participant perceptions. We find that bank resolution reforms decrease implicit subsidies in both continents, but the effect is economically and

statistically stronger in North America. Again, this effect is mainly driven by non-large banks, as we find no effects for large banks in neither continent. We also run a series of alternative specifications and address several possible confounding effects, and our inferences do not change significantly.

Addressing TBTF is one of the core elements for financial stability as established by the FSB (Duffie, 2017). However, implicit guarantees for large banks remain a contentious issue. Studies by Gao *et al*. (2018) and Pancotto *et al*. (2019) reveal mixed success of reforms like Dodd-Frank and the European Bank Recovery and Resolution Directive in addressing the TBTF issue and the sovereign-bank risk nexus, underscoring ongoing challenges in balancing financial stability and market discipline. Our evidence presents new insights into the literature and complement the results of other studies. Our findings corroborate those of Acharya *et al*. (2016), since we also observe that the subsidy to large banks persists in the post-crisis period. However, we find that non-large banks experience a reduction in their implicit subsidies after the implementation of resolution regulations. Our results are also in line with the research that emphasizes that implicit government guarantees continue to be priced around the world for large financial institutions (Gandhi *et al.*, 2020). However, our study adds more nuanced insights: we estimate a reduction in the implicit subsidy for non-large banks following the implementation of the resolution regulations, especially in the US and Canada.

Our inferences are also consistent with Havemann's (2019), as they suggest that investors expect regulators to be more likely to perform interventions that reduce the losses of investors when resolving a large bank than non-large banks, even if resolution mechanisms are in place. Finally, our findings offer a distinct perspective on the prevailing narrative of reduced implicit government subsidies to large banks, as posited by prior studies (*e.g.*, Atkeson et al., 2018; FSB, 2020; Berndt *et al*., 2024). Specifically, when compared to a control group, our analysis suggests that resolution reforms may not have effectively reduced the implicit subsidies to large banks. Moreover, our findings align with concerns raised in the literature that special procedures for the resolution of large banks may inadvertently entrench the TBTF problem by reinforcing expectations of government intervention (see Gao *et al.*, 2018). Our cross-country analysis explores how variations in regulatory design and implementation impact implicit guarantees, providing a broader perspective on global resolution reforms and their challenges.

We also contribute to the literature that addresses the relationship between implicit guarantees and risk-taking by banks. One strand of the literature asserts that the expectation of a bailout creates moral hazard and results in greater risk-taking by banks (Boyd and Gertler, 1994; Hakenes and Schnabel, 2010). Another strand states that protected banks' charter values reduce the incentives for excessive risk-taking (e.g., Keeley, 1990; Cordella and Yeyati, 2003; Körner and Schnabel, 2013) in large banks. Our results align more closely with the moral hazard literature, which supports a positive relationship between implicit subsidies and risk-taking. Our research also adds indirectly to the literature on the funding and liquidity consequences of TBTF (Acharya *et al.*, 2014; Oliveira *et al.*, 2015). More broadly, our paper also adds to the literature that look at the effect of regulations on banks' cost of capital (e.g., Kovner and Tassel, 2021).

Our results have implications for regulators and other policymakers. The fact that resolution reforms reduce the implicit subsidies for non-large banks, while not substantially affecting large banks, diverge from the regulators' purpose, since resolution regulations were intended to target mainly the largest banks. Because bank resolution regulations affect mostly non-large banks, they may exacerbate the competitive distortion between more protected and less protected banks, which has previously been documented by Gropp *et al*. (2011). Therefore, the heterogeneous effects of the resolution mechanisms on bank risk can further amplify the competitive distortions between large and non-large banks, resulting in lower credit supply and higher cost of borrowing for firms and households that cater mainly from non-large banks.

The paper proceeds as follows. Section 2 presents the Regulatory Framework. Section 3 shows the data, the identification strategy, variable construction, and descriptive statistics. Section 4 exposes the main results on implicit guarantee, starting with the baseline model and then expanding it in several directions. Section 5 explores the effects of these regulations on the risk-taking behavior of banks, section 6 shows the analysis across jurisdictions, and section 7 presents the robustness tests. Section 8 concludes.

2. Regulatory Framework

Driven by the goal of creating a standardized toolkit for bank resolutions worldwide and addressing the TBTF issue, the FSB has established a comprehensive framework that aims at ensuring orderly bank resolution procedures and reduce the reliance on state funds for potential bailouts. The most widely recognized bank resolution tool is the bail-in mechanism, which makes debtholders and shareholders, rather than taxpayers, bear the burden of recapitalizing distressed institutions. The standard hierarchy sees equity as secondary to bonds when a bank is rescued: when a bank failure can affect the financial system, shareholders are wiped out (*i.e.*, common equity instruments are the first to absorb losses) and subordinated creditors, senior unsecured debtholders, contingent convertible debtholders and other uninsured creditors have part of their debt converted into equity for the recapitalization (Berger and Roman, 2020).[7]

While the principles set forth by the FSB have been integrated into the regulations of many member countries, the orderly resolution frameworks adopted by these jurisdictions exhibit variations in their timing and form of implementation (FSB, 2021a). From the 19 countries in our sample, eight have fully adopted bank resolution regulations, according to the FSB. Table 1 shows when each country in our sample fully adopted such set of policies. We summarize below the implementation of the resolution framework for the countries in our sample.

[Table 1 here]

In the US, the bank resolution process is regulated by the Orderly Liquidation Authority (OLA) contained in Title II of the Dodd–Frank Act applied in 2010. If applied, the Federal Deposit Insurance Corporation (FDIC) Board orderly liquidates the top-tier parent company, while transferring solvent subsidiaries to a new bridge corporation. Losses are distributed among shareholders and unsecured creditors. The bridge company is capitalized by converting unsecured debt to equity and it can access financial markets for an additional capital injection. If this process does not ensure recapitalization, the OLA can use the Orderly Liquidation Fund to provide financing (Berger and Roman, 2020).

[7] For example, the bail-in of Bank of Cyprus in 2013 converted approximately 47.5% of uninsured deposits into ordinary stocks (Bank of Cyprus Archives, available at link).

In Canada, the new bank resolution regime has officially come into effect in September 2017. Like the US resolution process, if an important financial institution is in default and reaches a point of non-viability, the Canada Deposit Insurance Corporation (CDIC) is authorized to take temporary control or ownership of the Domestic Systemically Important Banks (DSIB). Any unsecured instrument with an initial term over 400 days is generally eligible to be used to bail-in the bank, with some exclusions like deposits, covered bonds, derivatives, structured notes, and certain liabilities (CDIC, 2023).

Contrary to the OLA, the bank resolution regime in both UK and the European Union permits restructuring or bailout under certain conditions, as noted by Philippon and Salord (2017). The Bank Recovery and Resolution Directive (BRRD) applied to the European Union was finalized on June 1, 2014, and became effective in January 2016.[8] Its bail-in tool empowers regulators to recapitalize the bank by writing-off or converting liabilities to equity and requiring creditors to take losses according to risk hierarchy. However, if resolution objectives are not met using those tools, both the UK and the European Union regulators can follow the BRRD and use public funds to stabilize the bank through temporary public ownership. The shareholders and creditors must bear losses equal to at least 8% of the bank's liabilities before the use of public funds. This is a last resort option in the case of a serious threat to financial stability or to protect public funds previously used to support a failed bank (Philippon and Salord, 2017).

The UK Bank Act of 2013 updated the resolution rules applied to the largest UK banks by the Bank of England as the regulatory authority. Large banks are eligible for a bail-in resolution, in which existing shares are cancelled, diluted, or transferred, and unsecured creditor claims are written down to absorb losses. Creditor claims are then converted to equity to restore the bank's solvency.

In essence, while neither the US nor the Canadian resolution mechanisms explicitly mention the use of public funds, the European bank resolution provides more flexibility to regulators, allowing for the deployment of public funds after shareholders and creditors incur

[8] The resolution applied in Europe underwent several changes between the drafting and implementation of the regulation. For example, the European Commission published the first draft in June 2012. The European Parliament approved the final version in April 2014. The entered into force from January 2015, except for the bail-in instrument, which started to be applied from January 1st, 2016.

losses as outlined in the resolution. The potential reliance on public resources in the resolution process in Europe may increase the perception that governmental funds might be used to save banks in distress.

3. Data and Baseline Empirical Strategy

Our initial sample includes all banks and bank holding companies headquartered in countries that are members of the Financial Stability Board (FSB), as we utilize the FSB's reform dashboard to track the implementation of resolution reforms. We identify banks using the Industry Classification Benchmark (ICB) code 301010 (Banks) and 302020 (Investment Banks and Brokerage Services) in the Refinitiv DataStream database.[9] We remove all observations for firms whose names include the words "fund," "mutual fund," "income," or "income fund" to eliminate data for mutual funds and other such investment services. For simplicity, we refer to all the institutions in our sample as "banks" throughout the paper. Our sample period ranges from 2002 to 2021, a period during which, as we describe in Section 2, there was a staggered implementation of bank resolution regulations in several jurisdictions, beginning with the US in 2010. Therefore, our period encompasses several years before, during, and after the full implementation of resolution regulations in these jurisdictions.

We further collect annual accounting information and weekly stock market returns from Refinitiv DataStream. To ensure that the stock return data is appropriate for use in asset pricing models and for calculating risk variables, we apply the following filters at the stock price level, as suggested by Gandhi *et al*. (2020). We exclude firm-level observations with less than 52 weeks of returns in each year. Additionally, we discard observations with extreme returns followed by reversals: if a stock return is larger than 22.5% in a week and smaller than -22.5% in the following week (or *vice-versa*), we exclude those observations.[10] We exclude penny stocks, classified as observations whose year-end closing price is lower than a unit of local

[9] We include Investment Banks and Brokerage Services because some typical banks (for example J. P. Morgan Chase) are classified in this second category. For robustness, we also re-estimate all our analyses excluding investment banks and brokerage services, and our inferences remain virtually unchanged.
[10] This procedure aims at avoiding using stock prices that possibly contain input errors.

currency. We also remove the smallest banks in each country by excluding those in the bottom 1% of market capitalization. Finally, we exclude highly illiquid stocks, defined as those with more than 80% zero weekly returns in each year.

We also collect data on the following accounting and market variables: total assets, total debt, book value of equity, the market value of equity, total deposits, and return on equity (ROE). Next, we exclude any observations with identical values for total assets, total debt, and ROE in the same country-year to exclude duplicate observations in our sample, and banks that have only one yearly observation.

To measure equity risk factors from around the world, we use the data on risk factors available on the Global Factor Database, following the approach of Jensen *et al*. (2021). In addition, we collect the market yield on 3-month U.S. T-Bills to construct the *proxy* for the risk-free rate from the Federal Reserve Economic Data (FRED), as in Gandhi *et al.* (2020). To standardize stock returns in local currency, we collect official exchange rates for each country across the entire sample period from the International Financial Statistics of the International Monetary Fund (IMF Data). This approach aligns with other cross-country multifactor model studies, such as Gandhi *et al.* (2020) and Jacobs (2016). Additionally, we obtain financial development and macroeconomic data at the country-year level from the World Bank DataBank.[11]

The data regarding the adoption of bank resolution mechanisms is provided by the FSB. To identify whether and when a country adopts the bank resolution mechanism, we collect information from the "Table on implementation of reforms in priority areas by FSB jurisdictions", available on FSB's website, for each country in our sample. Our main variable is a dummy indicating whether a country has fully adopted the legal and regulatory standards for conducting a bank resolution. The FSB states that the country has fully adopted the legal standards for conducting a bank resolution if all the three resolution powers for banks (i.e. "transfer", "bail-in", and "temporary stay") are adopted by a given country (as summarized in Table 1).[12]

[11] For the UK, we collect some macroeconomic variables from the FRED database, as some of these variables are not available at the World Bank DataBank.

[12] According to the Key Attributes of Effective Resolution Regimes for Financial Institutions issued by the FSB: (1) "transfer" refers to "the power to transfer ownership, assets, rights and liabilities, without the consent of

In other tests, we recognize the gradual implementation of bank resolution regulations, and use a granular measure reflecting the degree of adoption of comprehensive bank resolution policies by each country in each year, the FSB's Resolution Reform Index, *RRI*. The FSB (2021b) outlines the methodologies employed to build the index. The *RRI* is calculated as the equally weighted average of three sub-indices: the first sub-index covers resolution powers as well as recovery and resolution planning; the second sub-index pertains to the development of policies and guidance for operationalizing resolution regimes (distinct from the legal framework); and the third sub-index addresses loss allocation, including bail-in powers and the existence of external loss absorbing capacity requirements for Systemically Important Banks.

Lastly, to control for the occurrence of banking crises and to build our instrumental variable, we follow Beck *et al.* (2020) and use the Global Crises Data from the Behavioral Finance and Financial Stability Project (BFFS Project) at Harvard Business School. This dataset includes banking crises and other crises dates for over 70 countries from 1800 to 2016.

After all exclusions, our sample has 1,544 banks from 19 countries with available data. This sample generates a total of 13,971 bank-year observations. Variable definitions and sources are summarized in Table A.1 in the Appendix.

3.1 Baseline Empirical Strategy

To analyze whether bank resolution regulations affect bank's implicit subsidies, we start by estimating a differences-in-differences model, in which a country is defined as treated if, according to the FSB, it has fully adopted bank resolution reforms. We adopt two different estimation methods to deal with the staggered nature of our treatment events over time. The first is the stacked approach (as in Gormley and Matsa, 2011), which pools the data across each

shareholders or creditors, to achieve the sale of all or part of the failing bank, transfer of the bank's critical functions to a temporary bridge institution or transfer of non-performing assets to a management vehicle"; (2) "Bail-in" refers to the power to "write down unsecured liabilities or convert them to equity with the purpose of absorbing losses or providing capital", and (3) "temporary stay" refers to the power to "impose a temporary stay on early termination rights under financial contracts". Although the FSB aggregates these three resolution powers into a single variable, an effective bail-in mechanism can only take place when all resolution powers are available to the regulators in each country.

quarter (cohort) c to estimate our regressions, described by Equation (1). The second is the estimator proposed by Callaway and Sant'Anna (2021).[13]

$$Unlevered\_Alpha_{i,j,t,c} = \omega_0 + \omega_1 Resolution_j \times Post_{j,t,c} + \delta_{i,c} + \delta_{t,c} + \epsilon_{i,j,t,c} \quad (1)$$

$Unlevered\_Apha$ is our measure of banks' implicit subsidy (or equity funding cost advantage). It is defined as the abnormal unlevered return of bank *i*, headquartered in country *j*, in year *t* for the treatment cohort *c*. The estimation of this variable is detailed in section 3.2.

$Resolution$ is a dummy variable that takes the value of 1 if a given country *j* has fully adopted a bank resolution mechanism (*i.e.*, is a treated country) in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after country *j* fully adopted resolution rules, as described in Table 1.

Our interaction variable $Resolution \times Post$ captures the change in the bank resolution framework by the regulatory authorities of each treated country of our sample (*i.e.*, "the treatment"). The coefficient ω_1, which captures the effect of the adoption of bank resolution regulations on the banks' implicit subsidy, is our main parameter of interest. If the implementation of the bank resolution framework in a given country is credible for investors, we expect ω_1 to be positive (meaning a decrease in implicit subsidies). We use a set of bank-cohort and year-cohort fixed effects ($\delta_{i,c}$ and $\delta_{t,c}$, respectively). Bank- cohort fixed effects seek to control for bank unobserved heterogeneity in each cohort, whereas year-cohort fixed effects control for any global time-varying event that affects all banks homogeneously. These fixed effects are different for each cohort (year of treatment), consistent with Gormley and Matsa's (2011) approach. In all our regressions, the standard errors are clustered at the country level to account for any potential correlation of the error term within observations of the same country.

To identify whether bank resolutions reforms affect banks of different sizes heterogeneously, we estimate Equation (1) for subgroups of banks segmented by total assets in the previous year. We classify as "large banks" the five largest banks from each country in

[13] In these regressions, we avoid using bank-level controls. Because they might be affected by the treatment, their inclusion would create the so-called "bad control problem", which is particularly more severe in a difference-in-differences setup (Angrist and Pischke, 2009). Still, to mitigate any existing concerns, we also report the estimation of standard differences-in-differences regressions with controls in the robustness section.

the previous year, whereas the other banks are identified as "non-large banks". We also use additional bank size classifications in our robustness checks.

In addition to our differences-in differences estimations, we also regress the unlevered alphas against the Resolution Reforms Index ($RRI_{j,t}$), a granular measure that captures the degree of implementation of bank resolution policies across countries over time. In these regressions, we estimate regressions without any controls as well as regressions including a series of country- and bank-level controls. Country-level controls are included because factors associated with the stochastic discount factor, such as macroeconomic and financial system structure features, could affect both unlevered alphas and be correlated with the *RRI*. Namely, we include *GDP Growth*, *Inflation*, *Unemployment*, *Banking Crisis*, and *Bank Concentration.*[14] Bank level controls are in line with Gandhi and Lustig (2015), and aim at capturing potential bank-level determinants of abnormal returns. These controls are total deposits over total assets (*Dep./TA*), stock idiosyncratic risk (*Idiosync. Risk)*, Book-to-Market (*B/M*) ratio, Return on Equity (*ROE*) and past-year stock return (*Lagged Return)*. These variables are winsorized at the 5% level, and their operational definitions are provided in Table A.1 in the Appendix. Because the FSB started measuring the *RRI* as of 2010, this set of regressions includes only observations from that year onwards.

One possible concern about our empirical approach is that the adoption of resolution regulations in a country is endogenous to the importance of the financial system, which could hinder a causal interpretation of our coefficients of interest. To address such concern, we adopt an instrumental variable strategy for causal identification, which we describe in detail in section 4.3.

3.2 Measuring the Implicit Subsidy

Our measure of bank implicit subsidy is based on the methodology outlined by Gandhi *et al.* (2020). Securities equally exposed to the same risk factors must have similar expected

[14] In unreported robustness checks, we also include the *Financial System Deposits to GDP* ratio. Our inferences are unchanged in these regressions. We do not use this variable in our main models because it is unavailable for some countries in the sample.

returns, all else equal.[15] In Gandhi *et al.* (2020), the expected return of bank stocks can be explained by the three traditional equity risk factors (market, size, and value) and by an unobservable factor, associated to an implicit governmental protection, which is uncorrelated with the three factors. Therefore, if the government is expected to absorb the risk that would otherwise be borne by the bank's creditors and shareholders, this implicit subsidy is priced by equity investors and the cost of equity of protected banks will be lower than otherwise, controlling for standard risk factors (Gandhi and Lustig, 2015). Therefore, a negative alpha means an implicit subsidy, because investors will price the subsidy *ex-ante*, and this alpha is a proxy for the government's implicit subsidy to a bank.

In our estimation of alphas, we add three other risk factors to those of Gandhi *et al.* (2020): profitability, investments, and momentum. This is consistent with the recent asset pricing literature and mitigates the criticism that omitting relevant risk factors mechanically increases the alpha estimates. This factor regression is generally referred to as the augmented Fama-French 5-Factor Model plus Momentum (Fama and French, 2018). We also follow Doshi *et al.* (2019) and apply a procedure to unlever returns using the parametric transformation proposed by Hamada (1972). Leverage induces heteroskedasticity in the data that causes bias to the coefficients, particularly for highly levered firms (which is the case of most banks). A second reason to use unlevered returns is that most banks have decreased their leverage throughout our sample period to adjust to more stringent capital requirements by regulators. As such, levered returns could suffer from non-comparability along our sample period.[16] Our estimation of alphas is described in Equation (2):

$$\left[r_{i,j,t} - Rf_t\right] = \alpha + \beta_1\left[Rm_{j,t} - Rf_t\right] + \beta_2 Size_{j,t} + \beta_3 Value_{j,t} + \beta_4 Prof_{j,t} + \beta_5 Inv_{j,t} + \beta_6 Mom_{j,t} + \epsilon_{i,j,t} \quad (2)$$

[15] If this relationship does not hold, under a non-arbitrage assumption, the model is incomplete as it does not consider all risk factors or market frictions.

[16] In our robustness section, we also estimate Equation (2) using observed stock returns (*i.e.*, without unlevering) to obtain levered alphas. We then re-estimate Equation (1) using these alphas. Our inferences are qualitatively unchanged (Table A.7).

Where *r* is the unlevered weekly stock return of bank *i*, located in country *j* in week *t*. We winsorize returns at the 2.5 and 97.5% levels. *Rf* is the risk-free rate, measured by the weekly return of the 3-month T-Bill, *[Rm-Rf]* is the weekly equity risk premium, *Size* is the weekly size factor, *Value* is the weekly value factor, *Prof* is weekly profitability factor, *Inv* is the weekly investment factor, and *Mom* is the weekly momentum factor. The six factors are country-specific measures.

We further adapt Gandhi *et al.'s* (2020) approach from a portfolio level to the individual stock level in order to obtain a measure of implicit subsidy at the individual bank-year level. Specifically, we estimate Equation (2) for each bank stock *i* in country *j*, using weekly returns. We run the regressions using one-year windows (*i.e.*, 52-weekly returns for each stock in each year). The alpha obtained for each year for each stock *i* is the abnormal return for that stock within that year. Finally, we multiply this weekly alpha by 52 to obtain the annualized unlevered abnormal return and winsorize it at the 1% level. The result of this computation is our measure of the implicit subsidy enjoyed by bank *i* in year *t*, which we use as the dependent variable (*Unlevered Alpha*) of our baseline regression (Equation (1)).

By employing bank-level alphas to measure implicit subsidy, we take advantage of greater granularity in the data, enabling nuanced insights beyond those achievable with portfolio-level analyses. While Gandhi and Lustig's (2015) research primarily explores differences in implicit subsidies between large and small banks, our study investigates how bank resolution reforms influence implicit subsidies. This data granularity is particularly important for understanding disparities among banks of different sizes and provides policy-relevant findings by facilitating targeted recommendations for regulators. A potential drawback of using individual alphas is the increased noise in stock returns compared to portfolios. There is no reason to believe, however, that the estimation error in unlevered alpha correlates with the regressors in equation (1). As a result, this error does not bias coefficient estimates and only increase standard errors, leading to lower statistical significance (Roberts and Whited, 2011). Nevertheless, to ensure the reliability of our results, we perform additional analyses in our robustness section using a portfolio-based approach, following the methodology outlined in Gandhi and Lustig (2015). The results of these analyses corroborate our findings and provide further robustness to our conclusions.

4. Main Results

In this section, we start by presenting the results of the estimation of implicit subsidies and descriptive statistics in section 4.1. Section 4.2 presents the baseline results on the effect of the implementation of the bail-in regulations on banks' implicit subsidy, section 4.3 explains our IV strategy and presents its results.

4.1 Estimation of Implicit Subsidies and Summary Statistics

We start by describing the results of our estimated implicit subsidies (Equation (2)).

In Figure 1, we compare alphas before and after the implementation of bank resolution regulations in countries that have adopted them. Before the implementation of the resolution policies, both large and non-large banks exhibit negative levered and unlevered alphas on average, suggesting that banks of all sizes enjoy implicit subsidies when bank resolution policies are not fully in place. However, after the full adoption of these resolution regulations, non-large banks' alphas become positive, suggesting that investors no longer perceive these banks as implicitly protected, whereas large banks' alphas remain negative, suggesting that these banks continue to enjoy a protected-bank status. This first set of results suggest that the effect of the full adoption of resolution policies is heterogeneous across banks, and contrary to the main objective of regulators, which is to eliminate (or reduce) the implicit subsidy to large banks.

[Figure 1 here]

Table 2 displays the descriptive statistics for the variables used in the analyses. Panel A shows the statistics for the entire sample, while Panel B reports them for banks in countries that have been treated at some point, and Panel C shows statistics for banks in never treated countries. The average and median unlevered alphas are negative for the entire sample. For treated countries (Panel B), the average unlevered alpha is just slightly positive at 0.6

percentage points, reflecting the balance between overall positive values in the post-treatment period versus negative values prior to treatment, as shown in Figure 1. For never treated countries (Panel C), the average unlevered alpha is negative at 1.9 percentage points, suggesting a large implicit subsidy for banks in these jurisdictions. The average levered alpha is negative on average for both subsamples, consistent with Gandhi and Lustig (2015) and Gandhi *et al.* (2020). However, its magnitude is much smaller for treated countries, again reflecting that these levered alphas are mostly negative prior to treatment, and positive after treatment, as shown in Figure 1. Banks in treated countries are slightly more levered, less reliant on deposits, have a lower ROE, and a much lower book-to-market ratio. The level of past returns and idiosyncratic risks are similar across both subsamples. Finally, the country-level variables are described in Panel D of Table 2.

[Table 2 here]

In Figure 2, we depict the average (solid line) and one-standard deviation band around the average of the Resolution Reform Index (*RRI*) over time. The average *RRI* shows an upward trend, indicating the gradual adoption of bank resolution policies. Furthermore, the increasing dispersion over time highlights that some jurisdictions have advanced more rapidly than others in implementing these policies, suggesting that the index has both cross sectional and time-series variation. Additionally, the data reveals that all jurisdictions began with a relatively low *RRI*, suggesting a uniformity in resolution regulations at the outset.

[Figure 2 here]

4.2 Baseline Results

Table 3 shows our baseline results, obtained from the estimation of Equation (1). Panel A reports the estimations using the stacked approach (Gormley and Matsa, 2011), whereas Panel B reports the results using Callaway and Sant'Anna's (2021) estimator. Both estimation methods are suited to account for the staggered adoption of bank resolution regulations across

countries.[17] We take the results from Panel A as our baseline results. Column 1 shows the regression results using the entire sample of banks. Our estimates of ω_1 indicate that the full implementation of bank resolution policies increase the average annualized abnormal unlevered return by approximately 4.0 percentage points (pp) on average. This effect is economically large, and statistically significant at the 5% level.

We split the sample between non-large banks in column 2 and large banks in column 3. For non-large banks, the adoption of bank resolution regulations increases alphas by 4.6 pp, suggesting a decrease in their implicit subsidies. However, for large banks, we do not observe any significant effect of bank resolution regulations on their alphas. The results shown in Panel B, are qualitatively similar, and suggest a 4.8 pp effect of bank resolution regulations on the implicit subsidies of non-large banks. Similarly to Panel A, the effect for large banks is statistically insignificant.

[Table 3 here]

One potential concern is about confounding effects. If any shocks other than the resolution reform affect the potential outcomes heterogeneously for each group of banks and happen to coincide with our treatment variable, our results could be due to these confounding causes. Because we have staggered treatment events over time, we believe this is unlikely. In any case, we address this concern by regressing *Unlevered_Alpha* against a granular measure capturing the degree of implementation of such reforms (the Resolution Reform Index - *RRI*). We claim that this granular measure is even less likely to be correlated with other confounding shocks.

The results of such estimations are shown in Table 4. In columns 1 through 3 we do not control for any banks or macro characteristics, in columns 4 through 6 we add bank-level controls and in columns 7 - 9 we also include country-level controls. The results in columns 2, 5 and 8 show that a one-unit increase in the *RRI* increases the *Unlevered_Alpha* of non-large banks by between 0.14 and 0.17 pp. The regressions for large banks, reported in columns 3, 6

[17] To mitigate concerns about omitted variables, we also estimate two-way fixed effects regressions with bank-level and country controls. The results are reported in Table A.6 of the Appendix, and our inferences are sustained.

and 9 show that variations in *RRI* affect the *Unlevered_Alpha* for these banks by approximately half as much as for non-large banks. Overall, the results in Table 4 suggest that the adoption of regulations towards a bank resolution regime substantially decrease the implicit subsidies of non-large banks, whereas the effect for large banks, if existent, is much smaller.

The results of Table 4 also show that the inclusion of bank- and country-level controls improves the statistical significance of our coefficient of interest, but has little effect on the magnitude of these coefficients, suggesting that the correlation between these control variables and the degree of implementation of resolution reforms (*RRI*) is low. This mitigates concerns that the adoption of bank resolution reforms is endogenous to country characteristics (*i.e.*, is correlated with observable macroeconomic and financial system structure variables). However, one might still be concerned that country *unobserved* features are driving both a change in alphas and the adoption of bank resolution reforms. We address this possible concern in the next section.

4.3 Addressing Endogeneity

Bank resolutions adoption in each country is arguably not exogenous. In our previous regressions, bank fixed effects (which by definition embed country fixed effects) are able to capture stable omitted variables that could determine the adoption of bank resolution regulations. However, one might still claim that time-varying unobserved country features, like the systemic importance of banks, the government ideology, the perceived importance of the financial sector by the population, and bank-sector political lobbying may be part of the determinants for the implementation of bank resolution regulations. If these determinants are positively correlated with the implicit subsidies and negatively related to the likelihood of adoption of resolution reforms, our coefficient ω_1 likely underestimates the treatment effect of resolution regulations on the implicit subsidy. To address this possible issue, we implement an instrumental variables approach.

Our instrument is based on the logic of Beck *et al*. (2020). The number of past financial crises is an instrument for the implementation of policies towards the adoption of bank resolution reforms. If crises (and therefore, bailouts) have occurred in the past, the government

is more likely to impose a comprehensive bank resolution regime. The occurrence of past crises increases the public awareness of the social costs of bank bailouts (for example, through the impact on public indebtedness and its macroeconomic consequences on unemployment and inflation). If the country has had many banking crises in the past, the political cost of maintaining policies that do not aim to prevent new costly bailouts is larger. Therefore, legislators and policymakers are more likely to adopt regulations that decrease the need of adopting unpopular future bailouts in countries that have suffered past crises. Our data on the cumulative number of bank crises (*CNBC*) come from the Behavioral Finance & Financial Stability (BFFS) Project from Harvard University.

One could still suspect that the *CNBC* is a predictor of future crises, which would violate its exclusion condition as an instrumental variable. We believe this suspicion is unfounded. The literature on forecasting financial crises can be divided into two strands. The first asserts that crises are unpredictable (Cole and Kehoe, 2000), whereas the second stream states that financial crises are predicted by rapid expansions of credit accompanied by asset price booms (*i.e.*, the Kindleberger-Minsky view of boom-bust credit cycles). Therefore, according to both strands of the literature, the accumulated number of past crises is arguably unrelated to the occurrence of a financial crisis in the present. Figure A.1 in the Appendix shows the cumulative number of past crises per country as of the last year in our sample.

Equation (3) shows the first-stage equation of our 2SLS model:

$$RRI_{c,t} = \theta_0 + \theta_1 CNBC_{c,t-1} + \Theta M_{c,t} + K Controls_{i,t} + \delta_t + \epsilon_{i,c,t} \quad (3)$$

Where *CNBC* is the cumulative number of banking crises. *M* is the vector of macroeconomic and financial system structure covariates, and *Controls* are a set of bank-level control variables as described before. δ_t is a set of year fixed effects. θ_0, θ_1, Θ, and *K* are a set of coefficients to be estimated.

In the second stage, we estimate Equation (4):

$$Unlevered\ Alpha_{i,c,t} = \gamma_0 + \gamma_1 \widehat{RRI_{c,t}} + \Gamma M_{c,t} + \Lambda Controls_{i,t} + \delta_t + \epsilon_{i,c,t} \quad (4)$$

Where $\widehat{RRI}$ is the predicted value obtained from the first-stage estimation (Equation 3), and the other variables are defined as before. The set of coefficients to be estimated is represented by γ_0, γ_1, Γ, and Λ. Our parameter of interest is γ_1.

Table 5 shows results of the two-stage model estimation. In column 1, we present the coefficients of the first-stage regression using the entire sample.[18] *CNBC* shows a positive and significant coefficient, indicating that the occurrence of past crises is positively associated with the *RRI*. This coefficient indicates that our IV is a predictor of the degree of adoption of bank resolution reforms, and therefore satisfies the IV relevance condition. In addition, besides being economically relevant, our instrument is strong, presenting an F-test larger than 10 (Stock and Yogo, 2005) for all specifications. In column 2, we report the estimation of a reduced-form instrumental variable estimation (*i.e.*, we simply replace *RRI* with *CNBC*). *CNBC* is positively and significantly associated with *Unlevered_Alpha*, again suggesting that our instrument meets the relevance condition. The second-stage results corroborate the findings of our baseline model: an increase in predicted *RRI* increases the unlevered alpha for non-large banks (column 4), but not for large banks (column 5). According to the estimate in column 4, a one-standard deviation increase in *RRI* increases unlevered alpha by approximately 2.9 percentage points.[19]

[Table 5 here]

In the 2SLS estimations, we do not use bank fixed effects, as they would capture most of the variation in *CNBC*. To provide comparable OLS estimates, we report the results of regressions analogous to those of Table 4, but without bank fixed effects, in Table A.2 in the Appendix. The inferences from these estimations are essentially unchanged relative to those of Table 4, except that the coefficient for the sample of large banks is statistically insignificant.

[18] We estimate the first-stage model for each of the sub-samples and obtain similar results. We do not report these estimations to save space.

[19] The computation is as follows: a 1-unit increase in the predicted RRI increases the unlevered alpha of non-large banks by approximately 0.099 percentage point. The standard deviation of RRI is 28.8. Therefore, the estimated effect is $0.099 \times 28.8 \approx 2.9$ pp

For the sake of comparison with our baseline results in Table 3, we also estimate a 2SLS differences-in-differences model estimation in which the instrumented variable is the dummy $Resolution \times Post$ instead of *RRI*. The results are reported in Table A.3 of the Appendix.[20] Our inferences are sustained.

Taken together, the results of Tables 3, 4 and 5 confirm that the implementation of bank resolution reforms decreases the perception of implicit guarantee for non-large banks. Nevertheless, it appears these regulations have little or no significant impact on the implicit subsidies of large banks. Our results are consistent with the notion that bank resolution regulations hinder governments from providing support to non-large banks, while investors continue to expect some degree of protection for large banks in case of distress.

5. Effects on Bank Risk

After having identified how bank resolution reforms heterogeneously affect banks' implicit subsidy, we examine whether they also affect banks' risk-taking behavior. On one hand, a decrease in non-large banks' implicit subsidies could reduce moral hazard, leading these banks to decrease their risk-taking (Flannery, 1998). In contrast, the charter value theory states that subsidies induce banks to act more conservatively (Keeley, 1990), and therefore a decrease in implicit subsidies for some banks would lead them to increase their risk-taking, because the higher cost of capital of unprotected banks lead them to hold riskier assets with higher expected returns (for example, by selecting riskier borrowers and lending at higher interest rates). To disentangle between the two hypotheses, we estimate a difference-in-differences regression to gauge the effect of the adoption of bank resolution regulations on the risk-taking behavior of banks. Again, because different countries adopted their bank resolution frameworks at different times, we estimate the treatment effect using both Gormley and Matsa's (2011) stacked approach, described by Equation (5) and Callaway and Sant'Anna's (2021) estimator.

[20] In these regressions, we use a standard (*i.e.*, OLS) differences-in-differences regression, because the stacked approach and Callaway and Sant'Anna's estimator are not suited for a two stage estimation.

$$DtoD_{i,j,c,t} = \vartheta_0 + \vartheta_1 Resolution_{j,c,t} \times Post_{j,c,t} + \delta_{i,c} + \delta_{t,c} + \epsilon_{i,j,c,t} \quad (5)$$

Where, for each bank *i*, located in country *j* in year *t* of cohort-treatment *c*, Distance-to-Default (*DtoD*) is our measure of bank risk. We follow Acharya *et al*. (2016) and measure risk-taking using the *DtoD* based on Merton's (1974) structural *DtoD* model. We implement a simplified version proposed by Bharath and Shumway (2008) due to its superior empirical properties: it retains the Merton model's structural form, while simplifying the calculation by avoiding the use of interactions to obtain the implied probability of default.[21] We present the model calculations in Appendix B. The *DtoD* provides a measure of the distance, in terms of asset value standard deviations of the current market value of assets, from a specified default point (the debt value). The independent variable and the fixed effects structure of Equation (5) are the same of Equation (1).

Table 6 shows the results of the estimation of Equation (5). Again, Panel A shows the results using the stacked approach, whereas Panel B shows the results of the Callaway and Sant'Anna's (2021) estimator. Taking the results of Panel A, the result in column 1 shows an average increase in *DtoD* (meaning a decrease in bank risk) upon the implementation of bank resolution regulations. The results in columns 2 and 3, respectively for non-large and large banks, show that the effect comes mainly from the risk reduction in non-large banks. The implementation of bank resolution regulations increases *DtoD* of non-large banks by 13.7 pp, equivalent to more than half of the standard deviation of *DtoD*. The estimated effect for large banks is also positive, but statistically insignificant. The coefficients in Panel B are consistent with those of Panel A, except that the estimated effect for large banks is statistically significant at the 10% level. Still, the estimated effect for large banks (2.8 pp) is much smaller than for non-large banks, and equivalent to less than one fifth of the conditional standard deviation of *DtoD* for large banks.

These results suggest that the bank resolution regulations have real effects in reducing the risk of banks through a reduction in implicit subsidies. Indeed, our evidence shows that the

[21] See Campbell *et al.* (2008) and Bharath and Shumway (2008) for more details about the performance of simplified *DtoD* model.

risk reduction is more pronounced for banks that face an increase in their cost of capital (*i.e.*, the non-large banks) and provide support for the moral hazard hypothesis.

6. Regulatory Heterogeneity

In this section, we analyze whether the effect of the adoption of bank resolution regulations on implicit subsidies is heterogeneous across the jurisdictions that have adopted them. The design of bank resolution regulations is not homogeneous across countries, raising the question of whether the specific design of these resolutions affects their efficacy. This is an important empirical question, as it provides a perspective on the heterogeneity in regulatory frameworks.

Prior research suggests that the impact of resolution reforms could differ across jurisdictions. Gao *et al*. (2018) demonstrate that the Dodd–Frank Act in the U.S. produced mixed reactions, with skepticism about its ability to eliminate implicit guarantees for systemically important financial institutions. Legislative compromises and lobbying efforts during its enactment diluted some of its key provisions, particularly for the largest financial institutions. In contrast, Pancotto *et al*. (2019) highlight that the European Bank Recovery and Resolution Directive (BRRD) has not fully decoupled sovereign and bank risks, emphasizing how discrepancies in implementation across member states undermine its credibility. These findings underscore the importance of exploring how variations in regulatory design influence their perceived effectiveness and justify our approach to compare impacts across regions.

To address this heterogeneity, we segment the bank resolution treatment variable into two groups: the North American (*i.e.*, US and Canada's) and the European bank resolution regimes, which comprises France, Germany, Italy, the Netherlands, Spain and the UK. To explore the effect of the different resolutions, we compare each of these jurisdiction groups to the never treated group by re-estimating Equation (1) excluding one of the jurisdiction groups at a time (*i.e.*, when we analyze the North American reforms, we exclude the European-treated countries and *vice-versa*).

Table 7 shows the results of our analyses for each of the groups of countries. Again, the regressions reported in Panel A use the stacked approach estimation, whereas those in Panel B use Callaway and Sant'Anna's (2021) estimator. Columns 1 to 3 show the results using the North American banks as the treated group (compared to banks in the never treated countries), and columns 4 through 6 use European banks as the treated group. Taking the results from Panel A, we find a 4.9 pp increase in *Unlevered_Alpha* for non-large North American banks compared to banks in never-treated jurisdictions (column 2), whereas the estimated effect for large banks (column 3) is statistically insignificant. The analogous effect for non-large European banks is 2.8 pp, statistically significant at 5% (column 5), whereas for large European banks, the estimated effect is statistically insignificant. The results using Callaway and Sant'Anna's (2021) approach, reported in Panel B, lead to similar inferences. For North America, the estimated effect for non-large banks is a 4.8 pp increase in *Unlevered_Alpha*, statistically significant at 1%, whereas the coefficient for European non-large banks, in column 5, is positive, but smaller than in Panel A, and statistically insignificant.

Overall, the results are qualitatively similar across North America and Europe, in that we find that, for both continents, resolution reforms increase the cost of capital only of non-large banks. However, the magnitude of the reduction in implicit subsidies for non-large banks seems larger in North America than in Europe. In addition, the effect is statistically clearer in North America, as the coefficient for the European subsample is statistically significant in only one of the approaches.

Our findings align with Acharya *et al.* (2016), who find that the OLA/Dodd-Frank Act did not reduce bailout expectations for systemic banks in the US. For European banks, our results are partially consistent with other studies that found low market reaction after the legal implementation of bank resolution mechanisms (Schäfer *et al.*, 2016). However, we provide a nuance to these results by showing that, while the average effect of bank resolution regulations on implicit subsidies is insignificant for European large banks, there is suggestive evidence that these regulations reduce the subsidies for non-large European banks.

We conjecture that the heterogeneous magnitude of effects between jurisdictions likely stems from differences in regulatory design. North American resolutions do not explicitly preclude the use of taxpayers' resources, potentially damaging credibility regarding the non-

adoption of bailouts for large banks. Simultaneously, the absence of explicit provisions for public fund use in North America may lead to the perception that medium and small-sized banks are less protected. Gao *et al*. (2018) highlight that the final design of the Dodd-Frank Act was shaped by political compromises and lobbying efforts from the financial industry, which diluted its provisions and limited its potential to address the TBTF problem. This political influence may have further undermined the credibility of the North American framework in eliminating implicit guarantees for large banks.

In contrast, the European framework explicitly allows public fund use under specific conditions, which may reinforce investors' expectations of implicit guarantees. Pancotto *et al*. (2019) note that discrepancies in implementation across member states have also undermined the BRRD's credibility, contributing to a sustained perception of implicit guarantees in Europe.

7. Robustness Checks

One possible concern that could arise from our previous analyses is the *ad-hoc* classification of large banks. To deal with this concern, we use three alternative definitions of large banks. First, we use the three largest (instead of the five largest) banks of each country. Second, we classify as large banks those in the top ventile of the bank's total assets distribution in each country in each year.[22] Third, we classify as large banks the GSIBs, according to the list published by the FSB in each year.[23] The results of the baseline estimations using these alternative definitions are reported in Table A.4 in the Appendix, and our inferences are qualitatively unchanged.

One could also be concerned about failing banks disappearing from the sample. Several banks failed during our sample period. One possible effect of a bank failure is a sharp drop in the price of its shares before its failure, which could generate negative abnormal returns and

[22] For example, for countries with 20 banks or fewer, only the bank with the largest value of total assets is classified as a large bank. For countries with 21-40 banks, the two with highest total assets are defined as large banks, and so on.

[23] The first version of the GSIBs was published in November 2011, and has been updated ever since in November of each year. We adopt the 2011 classification for the previous years, based on the idea that banks that were considered systemically important as of 2011 were arguably perceived as such prior to that date.

be misinterpreted as an increase in implicit subsidy. To reinforce the robustness of our findings, we remove from our sample all the banks that failed at any time during our sample period. The results of these estimations, in Table A.5, show that our inferences are virtually unaffected by removing these banks.

Our baseline staggered differences-in-differences regressions do not use control variables, because most control variables could be affected by the passage of bank resolution regulations, and therefore our regressions would suffer from a bad controls problem (Angrist and Pischke, 2009). Still, one could be concerned with omitted variable bias due to the absence of control variables in those regressions. To mitigate these concerns, we estimate our baseline differences-in-differences model using a standard two-way-fixed effects approach including controls, and report these results in Table A.6 in the Appendix. The estimated effect of bank resolution reforms on the *Unlevered_Alpha* of non-large banks is only slightly smaller than in the baseline estimation, and the coefficient for the sample of large banks is statistically insignificant as in Table 3.

Furthermore, we estimate equation (2) without unlevering the returns and use the resulting (*i.e.*, levered) alphas to re-estimate our baseline regressions (Equation (1)). We report these results in Table A.7 in the Appendix, and our inferences are practically unchanged. Finally, we estimate our 2SLS regressions using a second instrument (*Fiscal Costs*), which is the ratio between fiscal costs and the overall output loss in the latest crisis in a country as suggested by Beck *et al.* (2021). The results using this overidentified model, reported in Table A.8, are very similar to those reported in Table 5.

One potential concern that could arise from our bank-level analyses is the risk of errors-in-variable bias, given the potential increase in noise in alpha estimates at the individual bank level. To address this concern, we perform an analysis using a portfolio-based approach, which provides a complementary perspective to the granularity of individual-level analysis. Specifically, we build long-short portfolios for each country and year, taking long positions in banks within the largest size decile and short positions in banks within the smallest size decile. Following the methodology of Gandhi and Lustig (2015), portfolio alphas are calculated annually based on weekly returns within each year. We then estimate two-way fixed effects models to examine the results. The results from the portfolio-level analysis, reported in Table

A.9 in the Appendix, corroborate our individual bank-level findings. Specifically, they show that the subsidy for large banks increases relative to non-large banks, driven primarily by a significant reduction in the subsidies of non-large banks. This consistency across approaches strengthens the robustness of our conclusions.

8. Conclusion

This paper provides novel empirical evidence of the causal effect of the adoption of bank resolution frameworks on banks' implicit subsidies and risk-taking behavior. To our knowledge, this is the first paper to explore the effect of such frameworks on banks' implicit subsidies across a large sample of countries, leveraging an international perspective to uncover meaningful cross-country patterns. We employ an instrumental variable strategy designed to mitigate the endogeneity inherent in the voluntary adoption of the resolution by country-specific regulators.

Using a sample of banks across 19 FSB member countries, our findings reveal that non-large banks experience a significant reduction in their implicit subsidies following the implementation of resolution frameworks. In contrast, we do not observe any significant effects on the implicit subsidies of large banks and GSIBs. Remarkably, there is a disparity in how banks are impacted: non-large banks, which have been previously shown to face unfair competition against large banks (e.g., Gropp *et a*l., 2011), are the most affected by the new bank resolution framework, as they face a reduction in implicit subsidies. In this sense, our results strongly suggest that investors perceive that non-large banks are more likely to undergo a bail-in process during distress, whereas large banks seemingly retain expectations of some type of governmental support, perpetuating their implicit subsidies in the event of an imminent failure.

Consistent with the moral hazard theory, we present evidence that banks that lose their implicit subsidy (*i.e.*, non-large banks in countries that implemented the resolution) show a significant reduction in their risk-taking relative to unaffected banks. This underscores a critical limitation: while bank resolution regulations successfully reduce subsidies for non-large banks,

they fail to fully convince investors that large banks will bear the financial consequences of failure. The consequences of not pricing equity adequately can generate excessive risk-taking by large banks, contributing to greater instability in the financial sector.

This study shows a possible path that can be taken in the future. Recent bank failure events, such as those in the US and Switzerland in 2023, have proven that there is strong heterogeneity in how resolution processes are applied in different jurisdictions. For example, while in the US all depositors of Silicon Valley Bank were guaranteed and the bank was subsequently declared bankrupt, the Swiss regulator decided to impose losses on contingent convertible debtholders of Credit Suisse before the bank's shareholders, which appears to violate the implied hierarchical order of risks. These divergent approaches raise questions about the efficacy and consistency of resolution frameworks in addressing systemic risks. Our findings emphasize the importance of designing resolution frameworks that address systemic disparities while fostering consistency across jurisdictions. Understanding how investors' expectations and banks' behaviors respond to these policies offers a promising avenue for future research, particularly in the context of evolving global financial regulations.

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Figures and Tables

Figure 1. Average of Bank-Level Annualized Abnormal Returns Pre- and Post-Resolution Reforms

This chart shows the average abnormal returns for the periods before and after the resolution reform for countries that applied the resolution (*i.e.*, USA, UK, Italy, Spain, Switzerland, France, Germany, Netherlands, and Canada).

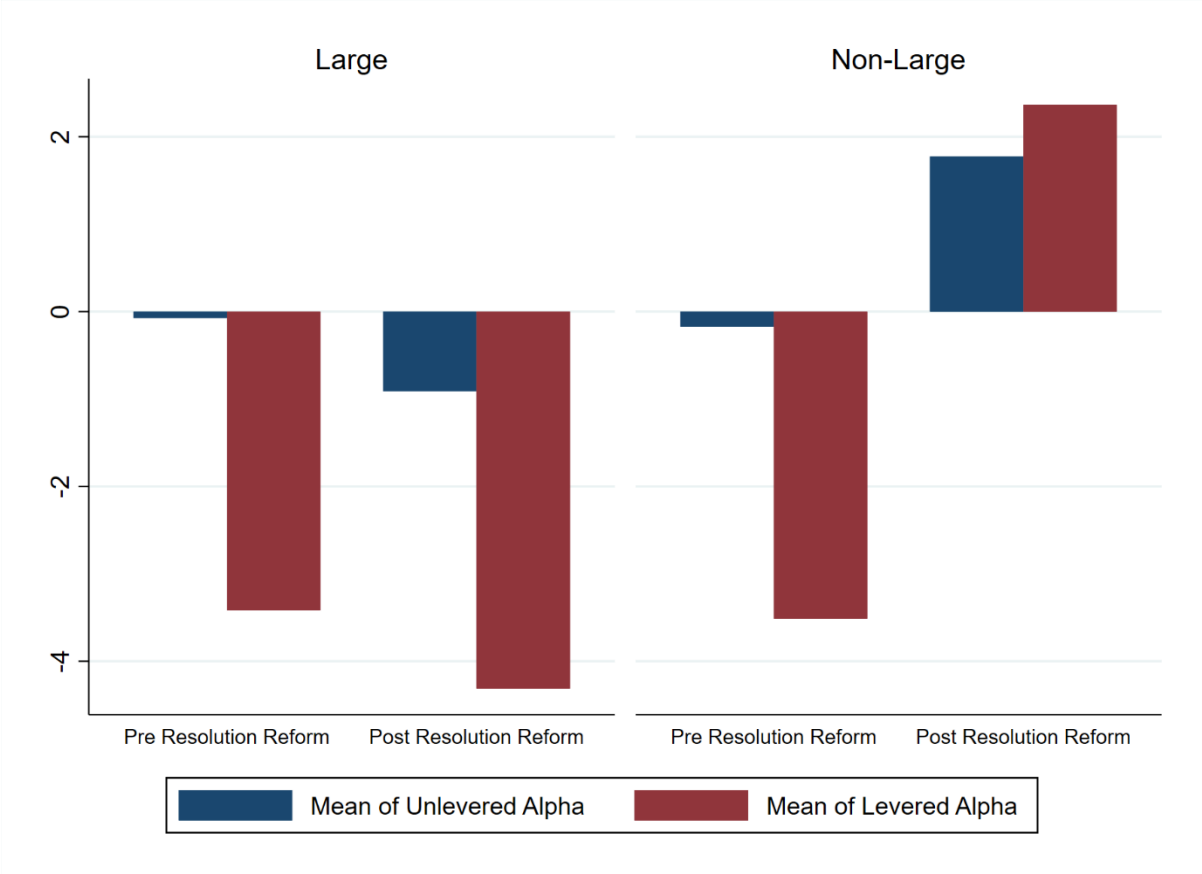

Figure 2. Average Resolution Reform Index

This Figure depicts the average dynamics of the Resolution Reform Index (RRI) for the 19 countries in our sample between 2010 and 2021. The shaded band around the average line indicates the range of plus or minus one standard deviation from the mean.

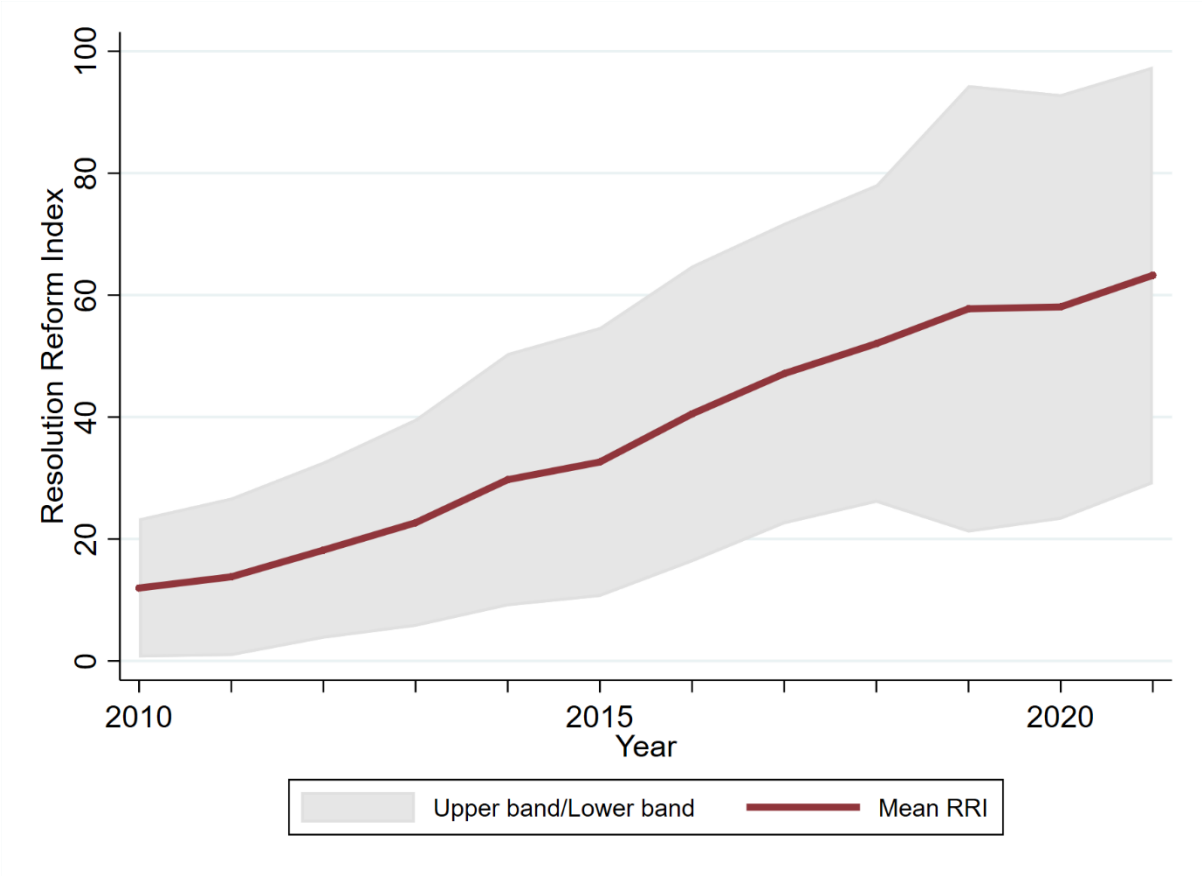

Table 1. Bail-in Resolution by Country

This Table shows in Panel A, for each FSB jurisdiction in our sample, the year of effective implementation of bank resolution reforms. Panel B shows the Never Treated countries.

Panel A: Treated Countries	
FSB jurisdictions	Year of effective implementation
Canada	2017
France	2016
Germany	2016
Italy	2016
Netherlands	2016
Spain	2016
UK	2013
USA	2010
Panel B: Never Treated Countries	
Australia, Brazil, China, India, Indonesia, Japan, Korea, Mexico, Singapore, South Africa, and Turkiye.	

Table 2. Descriptive statistics for sample and subsamples

This Table contains summary statistics of our main variables. We provide a detailed definition of each variable in Appendix A.1 Panel A reports bank-level variables for all banks in our sample. Panel B and C report the subsample of banks in treated and never treated countries, respectively. Panel D reports country-level variables.

Panel A. Bank Level variables of all banks						
	N. obs	**Mean**	**Median**	**25th%**	**75th%**	**Std.Dev.**
Unlevered Alpha	13,971	0.062	-0.352	-6.120	5.828	11.289
Levered Alpha	13,971	-2.004	-1.041	-15.346	12.742	23.046
DtoD	13,134	24.725	19.192	9.763	32.955	20.546
Leverage	13,971	0.524	0.540	0.362	0.695	0.219
Dep/TA	13,971	0.738	0.779	0.679	0.840	0.151
Idiosync. Risk	13,971	2.619	2.479	2.009	3.105	0.844
B/M	13,971	1.115	0.841	0.603	1.235	2.201
ROE	13,971	7.321	8.550	4.510	12.570	11.737
Lagged Return	13,171	7.989	4.823	-12.500	23.934	36.416

Panel B. Bank Level variables – Treated countries						
	N. obs	**Mean**	**Median**	**25th%**	**75th%**	**Std.Dev.**
Unlevered Alpha	10,935	0.613	-0.031	-5.616	6.299	10.972
Levered Alpha	10,935	-1.118	-0.091	-14.014	13.361	22.733
DtoD	10,293	25.180	19.763	10.168	33.456	20.635
Leverage	10,935	0.534	0.549	0.383	0.697	0.213
Dep/TA	10,935	0.730	0.771	0.682	0.825	0.142
Idiosync. Risk	10,935	2.611	2.483	2.010	3.095	0.833
B/M	10,935	0.986	0.794	0.587	1.086	0.726
ROE	10,935	7.182	8.710	4.930	12.250	11.130
Lagged Return	10,329	7.683	5.997	-10.931	24.111	33.012

Panel C. Bank Level variables – Never treated countries						
	N. obs	**Mean**	**Median**	**25th%**	**75th%**	**Std.Dev.**
Unlevered Alpha	3,036	-1.922	-1.424	-7.928	4.074	12.160
Levered Alpha	3,036	-5.195	-4.342	-19.797	9.772	23.871
DtoD	2,841	23.078	16.766	8.551	30.702	20.138
Leverage	3,036	0.487	0.492	0.294	0.689	0.235
Dep/TA	3,036	0.766	0.843	0.662	0.897	0.177
Idiosync. Risk	3,036	2.646	2.460	2.008	3.174	0.882
B/M	3,036	1.580	1.183	0.713	1.905	4.487
ROE	3,036	7.823	7.265	3.625	14.345	13.693
Lagged Return	2,842	9.103	-0.220	-17.417	23.153	46.736

Panel D. Country Level Variables

	N. obs	**Mean**	**Median**	**25th%**	**75th%**	**Std.Dev.**
RRI	5,584	61.516	67.284	45.062	85.802	28.829
Banking Crisis	13,971	0.191	0.000	0.000	0.000	0.393
Bank Concentration	13,971	39.035	35.518	32.808	42.051	12.745
GDP Growth	13,971	4.336	4.132	2.686	5.954	4.090
Inflation	13,971	2.383	2.142	1.465	3.226	2.039
Unemployment	13,971	6.327	5.546	4.620	7.870	2.891
CNBC	13,971	13.153	15.000	10.000	16.000	3.955

Table 3. The Effect of Resolution Reforms on Equity Costs Advantages: Difference-in-Difference

This Table presents estimates from a staggered differences-in-differences model examining whether bank resolution reforms affect bank's implicit subsidies as shown in Equation (1). The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. Panel A shows the results using the stacked approach. Column 1 shows the regression results using the entire sample of banks, columns 2 and 3 show the regression results for non-large banks and large banks respectively. Panel B shows the analogous results using the estimator proposed by Callaway and Sant'Anna (2021). All standard errors are clustered at the country level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Panel A. Stacked Approach Difference-in-Difference

Sample	All	non-large	large
	(1)	(2)	(3)
	Dependent Variable is the *Unlevered Alpha*		
Resolution × Post	3.969**	4.608**	-0.908
	(1.559)	(1.847)	(0.991)
Bank-Cohort	Yes	Yes	Yes
Year-Cohort	Yes	Yes	Yes
N. obs	23,079	19,627	3,358
Adj. R^2	0.169	0.175	0.135

Panel B. Callaway & Sant'Anna's Difference-in-Difference

Sample	All	non-large	large
	(1)	(2)	(3)
	Dependent Variable is the *Unlevered Alpha*		
Resolution × Post	4.820***	4.792***	-0.392
	(1.231)	(1.444)	(1.114)
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N. obs	10,675	9,376	1,108

Table 4. The Effect of Resolution Reforms on Unlevered Alpha: Granular Measure - OLS Results

This Table presents estimates from two-way fixed effects regression examining whether bank resolution reforms affect bank's implicit subsidies. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). *RRI* measures the application of bank resolution regulation and goes from 0 (implementation has not occurred) to 100 (full implementation). Column 1, 4) and 7 shows the regression results using the entire sample of banks, columns 2, 5, 8 and 3, 6, 9 show the regression results for non-large banks and large banks respectively. Columns 4-6 include control variables at bank-level lagged by one year, which include *Dep/TA*, *Idiosync. Risk*, *B/M*, *ROE*, and *Lagged Return*. Columns 7-9 include control variables at country-level, which include *Bank Concentration*, *Banking Crisis*, *GDP Growth*, *Inflation* and *Unemployment*. For a description of these variables, see Table A.1. All the specifications include bank fixed-effects and year fixed-effects. All standard errors are clustered at the country-level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% level, respectively.

Sample	All	non-large	large	All	non-large	large	All	non-large	large
	Dependent Variable is the *Unlevered Alpha*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
RRI_{t-1}	0.131**	0.170*	0.063	0.110**	0.143*	0.061*	0.135***	0.150**	0.075**
	(0.062)	(0.083)	(0.040)	(0.052)	(0.071)	(0.032)	(0.046)	(0.056)	(0.030)
Dep/TA_{t-1}				16.313***	18.449***	10.892	15.824***	16.734**	11.090
				(5.429)	(5.828)	(9.005)	(5.255)	(5.741)	(8.602)
$Idiosync.\ Risk_{t-1}$				-0.363	-0.415	0.447	-0.244	-0.245	0.156
				(0.241)	(0.265)	(0.763)	(0.206)	(0.223)	(0.664)
B/M_{t-1}				1.183*	1.114	1.026	1.496*	1.640*	1.143*
				(0.666)	(0.729)	(0.609)	(0.771)	(0.892)	(0.607)
ROE_{t-1}				0.116***	0.121***	0.133**	0.105***	0.105***	0.129**
				(0.020)	(0.021)	(0.056)	(0.020)	(0.019)	(0.053)
Lagged Return				-0.023***	-0.024***	-0.038***	-0.021***	-0.021***	-0.038***
				(0.005)	(0.006)	(0.013)	(0.005)	(0.007)	(0.013)
Bank Concentration							0.232	0.210	0.148
							(0.158)	(0.285)	(0.122)
Banking Crisis							2.272	2.243	-0.819
							(1.899)	(2.609)	(1.899)

(continued on next page)

GDP Growth							0.032	-0.062	0.071
							(0.135)	(0.194)	(0.121)
Inflation							0.675***	0.870***	0.284
							(0.196)	(0.287)	(0.380)
Unemployment							-0.165	-0.557	0.277
							(0.311)	(0.522)	(0.263)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of obs	5,584	4,902	656	5,584	4,902	656	5,584	4,902	656
Adj. R^2	0.165	0.167	0.178	0.179	0.180	0.212	0.184	0.187	0.222

*** p<.01, ** p<.05, * p<.1

Table 5. The Effect of Resolution Reforms on Unlevered Alpha: 2SLS results

This Table presents estimates from two-stage least square (2SLS) regressions examining whether bank resolution reforms affect bank's implicit subsidies. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). The instrumented variable is the *RRI*, which measures the application of bank resolution regulation and goes from 0 (implementation has not occurred) to 100 (full implementation). The instrumental variable is the Cumulative Number of banking Crises (*CNBC*) defined as in Table A.1. Column (1) reports the first-stage results using the entire sample of banks. Column 2 reports the results of the reduced form regression. Columns 3, 4 and 5 show the second-stage regression results for the entire sample, non-large banks and large banks respectively. All regressions include control variables at bank-level lagged by one year, which include *Dep/TA*, *Idiosync. Risk*, *B/M*, *ROE*, and *Lagged Return* and country-level, which include *Bank Concentration*, *Banking Crisis*, *GDP Growth*, *Inflation* and *Unemployment*. For a description of these variables, see Table A.1. All standard errors are clustered at the country-level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% level, respectively.

Sample	All	All	All	non-large	large
Estimator	2SLS – First Stage	Reduced Form	2SLS – Second Stage	2SLS – Second Stage	2SLS – Second Stage
	Dependent Variable is				
	RRI_{t-1}	*Unlevered Alpha*	*Unlevered Alpha*	*Unlevered Alpha*	*Unlevered Alpha*
	(1)	(2)	(3)	(4)	(5)
$CNBC_{t-1}$	5.144***	0.385**			
	(0.553)	(0.149)			
$\widehat{RRI_{t-1}}$			0.077**	0.099**	-0.016
			(0.028)	(0.042)	(0.030)
Dep/TA $_{t-1}$	6.033	4.637	4.259	5.559	0.783
	(6.475)	(4.786)	(4.722)	(5.429)	(3.906)
Idiosync. Risk $_{t-1}$	-2.178	1.001*	1.160**	1.333***	0.430
	(1.632)	(0.501)	(0.451)	(0.367)	(0.333)
B/M $_{t-1}$	-0.172	0.675	0.725	0.967	0.254
	(1.058)	(0.931)	(0.954)	(1.139)	(0.476)
ROE $_{t-1}$	-0.003	0.152***	0.152***	0.153***	0.151**
	(0.091)	(0.030)	(0.026)	(0.031)	(0.062)
Lagged Return	0.011	-0.000	-0.001	0.001	-0.031
	(0.010)	(0.005)	(0.005)	(0.005)	(0.020)
Bank Concentration	0.263	0.107	0.090	0.100	0.018
	(0.296)	(0.084)	(0.073)	(0.104)	(0.025)
Banking crisis	-15.902**	-1.175	0.345	0.837	-0.223
	(7.437)	(0.835)	(0.942)	(1.159)	(1.108)
GDP Growth	-0.674	-0.084	-0.052	-0.173	0.024
	(0.427)	(0.180)	(0.165)	(0.208)	(0.121)
Inflation	-1.277	0.569*	0.676***	0.951***	0.225
	(1.223)	(0.280)	(0.231)	(0.291)	(0.230)
Unemployment	-0.144	0.009	0.021	0.016	0.013
	(0.712)	(0.084)	(0.086)	(0.211)	(0.032)
Bank FE	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes
F-Stat 1st Stage	.	.	85.141	172.333	30.156
N. of obs	5,584	5,584	5,584	4,902	656
Adj. R^2	0.903	0.074	0.035	0.042	0.058

Table 6. The Effect of Bail-in Resolution on Bank Risk: Difference-in-Difference

This Table presents estimates from a differences-in-differences model examining whether bank resolution reforms affect the risk-taking behavior of banks as shown in Equation (5). The dependent variable *DtoD* is our measure of banks' distance-to-default. *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. Panel A shows the results using the stacked approach. Column 1 shows the regression results using the entire sample of banks, columns 2 and 3 show the regression results for non-large banks and large banks respectively. Panel B shows the analogous results using the estimator proposed by Callaway and Sant'Anna (2021). All standard errors are clustered at the country level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Panel A. Stacked Approach Difference-in-Difference			
Sample	All	non-large	large
	(1)	(2)	(3)
	Dependent Variable is the *Distance-to-Default*		
Resolution × *Post*	11.818***	13.686***	1.899
	(1.952)	(1.737)	(1.412)
Bank-Cohort	Yes	Yes	Yes
Year-Cohort	Yes	Yes	Yes
N. of obs	21,433	18,242	3,116
Adj. R^2	0.600	0.582	0.740

Panel B. Callaway & Sant'Anna's Difference-in-Difference			
Sample	All	non-large	large
	(1)	(2)	(3)
	Dependent Variable is the *Distance-to-Default*		
Resolution × *Post*	13.403***	14.851***	2.808*
	(1.049)	(1.156)	(1.591)
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N. of obs	10,085	8,868	1,041

Table 7. The Effect of Bail-in Resolution on Equity Costs Advantages: Regulatory heterogeneity

This Table presents estimates from a differences-in-differences model examining whether bank resolution reforms affect bank's implicit subsidies heterogeneously across jurisdictions. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. Panel A shows the results using the stacked approach. Panel B shows the results using the estimator proposed by Callaway and Sant'Anna (2021). Column 1-3 show the regression results for North America, and columns 4-6 for Europe. All standard errors are clustered at the country-level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% level, respectively.

Panel A. Stacked Approach Difference-in-Difference

	North American Resolution			European Resolution		
Sample	All	non-large	large	All	non-large	large
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable is the *Unlevered Alpha*					
Resolution × Post	4.716**	4.956**	-0.501	1.452	2.836**	-0.963
	(2.002)	(2.196)	(1.424)	(1.337)	(1.266)	(1.386)
Bank-Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort	Yes	Yes	Yes	Yes	Yes	Yes
N. of obs	15,930	14,324	1,568	7,149	5,303	1,790
Adj. R^2	0.167	0.173	0.131	0.164	0.165	0.139

Panel B. Callaway & Sant'Anna's Difference-in-Difference

	North American Resolution			European Resolution		
Sample	All	non-large	large	All	non-large	large
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable is the *Unlevered Alpha*					
Resolution × Post	4.978***	4.845***	-0.354	1.220	1.066	-0.428
	(1.281)	(1.462)	(1.940)	(0.967)	(2.132)	(0.851)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N. of obs	9,976	9,097	765	3,396	2,090	889

Appendix A

Appendix A provides additional figures and tables supporting the main text.

Figures and Tables

Figure A.1. Number of Cumulative Banking Crises (*CNBC*) by country
This Figure shows the maximum number of cumulative number of banking crises (*CNBC*) in each country as of the last year in our sample.

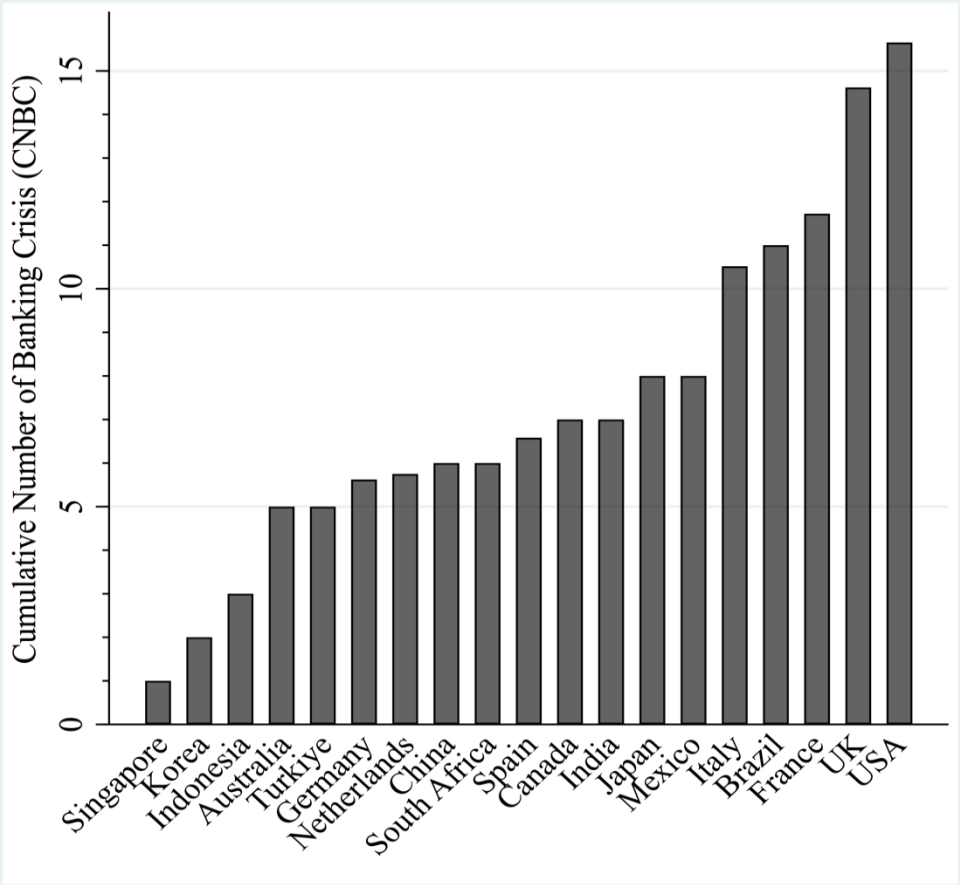

Table A.1. Variable definitions

This Table presents the definitions of the variables used in this paper and their data sources.

Panel A: Firm-level characteristics

Variables	Definition	Source
Unlevered Alpha	The annualized unlevered abnormal returns calculated using Equation (2).	Refinitiv DataStream
Large bank	The 5th banks with the largest total assets per year in each country.	
B/M	Book-to-Market measured as the book value of equity divided by the market value of equity.	
Leverage	Total debt divided by the sum of total debt and market value of equity.	
Dep/TA	Total deposits to total assets.	
DtoD	Distance-to-Default of Bharath and Shumway (2004) based on the Merton model of distance to default following Brogaard et. al (2017).	
ROE	Return on Equity measured as the net income divided by book value of equity.	
Lagged Returns	Annual stock returns lagged by one year.	
Idiosync. Risk	Idiosyncratic Risk measured as the annualized standard deviation of the residuals from the asset pricing regression exposed at Equation (2) using weekly returns.	

Panel B: Country-level variables

Variables	Definition	Source
Resolution	A country-level dummy variable that takes the value of 1 if the country implemented a bail-in resolution.	FSB
RRI	Bank Resolution Reform Index captures the application of bank resolution incrementally. The sub-scores go up by 33 points, according to the following logic. Score 0, indicates that implementation has not occurred (*i.e.*, draft regulation not published). A score up to 33, indicates that resolution is under development (*i.e.*, draft regulation published or submitted to the legislative body, or rulemaking initiated under supervisory powers). A score of up to 67, indicates that partial implementation has occurred (*i.e.*, final legislation published but not yet effective, partially adopted, or introduced only as a pilot). A Score of 100 indicates that full implementation has occurred (*i.e.*, the final rule published and is effective for all relevant banks).	
Banking Crisis	A dummy variable that takes the value of 1 if the country if it had a banking crisis in a given year.	BFFS Project
CNBC	Cumulative Number of Banking Crises.	
Fiscal Costs	The ratio between Fiscal Costs to Gross Domestic Product to Output loss to Gross Domestic Product in the latest crisis in a country. Fiscal costs refer to outlays directly related to the restructuring of the financial sector. Output losses are computed as the cumulative sum of the differences between actual and trend real GDP over the period [T, T+3], expressed in percent of trend real GDP, with T denoting the starting year of the crisis.	Laeven and Valencia (2018)
Risk factors	A vector of equity risk factors: the size factor (*Size*), the value factor (*Value*), profitability (*Prof*), Investments (*Inv*) and a momentum factor (*Mom*).	JKP's Global Factor Data
Exchange rate	The official exchange rate of each country.	IMF database
Mkt	The marker return proxy, measured as the weekly MSCI index return for each country measured in local currency.	Refinitiv DataStream

Rf	The risk-free rate proxy measured as the yield to maturity of a 3-month US T-Bill US dollars.	Federal Reserve Economic Data (FRED)
Bank Concentration	The sum of the total assets of three largest commercial banks as a share of total commercial banking assets.	World Bank
GDP Growth	Growth of Gross Domestic Product in the previous year.	
Inflation	The inflation measured by the consumer price index of each country.	
Unemployment	The country level share of unemployed by the total labor force, estimated by the International Labor Organization.	

Table A.2. The Effect of Resolution Reforms on Unlevered Alpha: Granular Measure - OLS Results without Banks Fixed Effects

This Table presents estimates from a fixed effect regression examining whether bank resolution reforms affect bank's implicit subsidies. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). *RRI* measures the application of bank resolution regulation and goes from 0 (implementation has not occurred) to 100 (full implementation). Column 1 shows the regression results using the entire sample of banks, column 2 and 3 show the regression results for non-large banks and large banks respectively. All specifications include control variables at bank-level lagged by one year, which include *Dep/TA, Idiosync. Risk*, *B/M*, *ROE*, and *Lagged Return,* and control variables at country-level, which include *Bank Concentration, Banking Crisis*, *GDP Growth*, *Inflation* and *Unemployment*. For a description of these variables, see Table A.1. All the specifications include year fixed-effects. All standard errors are clustered at the country-level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% level, respectively.

Sample	All	non-large	large
	Dependent Variable is the *Unlevered Alpha*		
	(1)	(2)	(3)
RRI_{t-1}	0.080***	0.106***	-0.014
	(0.023)	(0.033)	(0.013)
Dep/TA_{t-1}	4.346	5.732	0.833
	(4.564)	(5.203)	(3.621)
$Idiosync.\ Risk_{t-1}$	1.163**	1.346***	0.441
	(0.442)	(0.346)	(0.319)
B/M_{t-1}	0.753	1.025	0.252
	(0.908)	(1.072)	(0.485)
ROE_{t-1}	0.152***	0.153***	0.152**
	(0.027)	(0.030)	(0.066)
Lagged Return	-0.001	0.001	-0.031
	(0.006)	(0.005)	(0.020)
Bank Concentration	0.094	0.112	0.018
	(0.066)	(0.088)	(0.022)
Banking crisis	0.382	0.891	-0.194
	(0.914)	(1.160)	(1.170)
GDP Growth	-0.046	-0.156	0.026
	(0.154)	(0.187)	(0.115)
Inflation	0.692***	0.982***	0.230
	(0.216)	(0.259)	(0.224)
Unemployment	0.016	-0.008	0.013
	(0.088)	(0.207)	(0.034)
Bank FE	No	No	No
Year FE	Yes	Yes	Yes
N. of obs.	5,584	4,902	656
Adj. R^2	0.079	0.088	0.142

Table A.3: The Effect of Bail-in Resolution on Equity Costs Advantages: 2SLS

This Table presents estimates from two-stage least square (2SLS) regressions examining whether bank resolution reforms affect bank's implicit subsidies. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). The instrumented variable is the *Resolution* × *Post,* where *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. The instrumental variable is the Cumulative Number of banking Crises (*CNBC*) defined as in Table A.1. Column 1 reports the first-stage results using the entire sample of banks. Columns 2, 3 and 4 show the second-stage regression results for the entire sample, non-large banks and large banks, respectively. All regressions include control variables at bank-level lagged by one year, which include *Dep/TA, Idiosync. Risk, B/M, ROE,* and *Lagged Return* and country-level, which include *Bank Concentration, Banking Crisis, GDP Growth, Inflation* and *Unemployment*. For a description of these variables, see Table A.1. All standard errors are clustered at the country-level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% level, respectively.

Sample	All	All	All	non-large	large
Estimator	2SLS – First Stage	Reduced Form	2SLS – Second Stage	2SLS – Second Stage	2SLS – Second Stage
			Dependent Variable is		
	Resolution × Post	*Unlevered Alpha*	*Unlevered Alpha*	*Unlevered Alpha*	*Unlevered Alpha*
	(1)	(2)	(3)	(4)	(5)
$CNBC_{t-1}$	0.080***	0.462***			
	(0.006)	(0.123)			
$\widehat{Resolution} \times Post$			5.777***	7.271***	0.000
			(1.231)	(1.533)	(2.083)
Dep/TA $_{t-1}$	0.107	-0.663	-1.283	-1.327	1.733
	(0.089)	(3.714)	(3.811)	(4.573)	(3.728)
Idiosync. Risk $_{t-1}$	0.028*	0.217	0.052	0.156	-0.274
	(0.016)	(0.289)	(0.303)	(0.211)	(0.567)
B/M $_{t-1}$	-0.047**	1.590	1.859*	2.346**	0.641**
	(0.021)	(0.931)	(0.978)	(1.043)	(0.292)
ROE $_{t-1}$	-0.002	0.102***	0.112***	0.114***	0.055
	(0.001)	(0.024)	(0.030)	(0.031)	(0.040)
Lagged Return	-0.000	0.013	0.013	0.013*	0.016
	(0.000)	(0.008)	(0.008)	(0.007)	(0.022)
Bank Concentration	0.006**	0.047	0.014	0.002	0.038
	(0.002)	(0.045)	(0.038)	(0.051)	(0.024)
Banking crisis	-0.407***	-2.472***	-0.119	-0.183	0.342
	(0.063)	(0.827)	(0.789)	(0.923)	(1.534)
GDP Growth	0.019***	0.016	-0.097	-0.257	0.072
	(0.006)	(0.169)	(0.182)	(0.183)	(0.160)

(continued on next page)

Inflation	-0.047***	0.662***	0.935***	1.331***	0.237
	(0.010)	(0.225)	(0.265)	(0.233)	(0.185)
Unemployment	0.010	0.110**	0.051	0.017	0.012
	(0.012)	(0.043)	(0.094)	(0.209)	(0.048)
Bank FE	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes
F-Stat 1st Stage	.	.	174.546	214.672	23.530
N. of obs	11,606	11,606	11,606	10,525	1,057
Adj. R^2	0.843	0.128	0.035	0.040	0.012

Table A.4. The Effect of Bail-in Resolution on Equity Costs Advantages: Alternative Size Classifications

This Table presents estimates from a staggered differences-in-differences model examining whether bank resolution reforms affect bank's implicit subsidies as shown in Equation (1). The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. Panel A shows the results using the stacked approach. Columns 1 and 2 show the results using the non-large banks and large banks, respectively, using the top three largest banks in each country-year. Columns 3 and 4 show the results using the entire non-large banks and large banks, respectively, using the top 5% largest banks in each country-year. Columns 5 to 7 show the results using the GSIB bank status. Panel B shows the analogous results using the estimator proposed by Callaway and Sant'Anna (2021). All standard errors are clustered at the country level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Panel A. Stacked Approach Difference-in-Difference

Sample	Non-Top 3	Top 3	Non-Top 5%	Top 5%	Non-GSIBS	GSIBS
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable is the *Unlevered Alpha*					
Resolution × *Post*	4.600**	-1.840	4.228**	-0.387	4.105**	-0.190
	(1.846)	(1.121)	(1.580)	(1.951)	(1.627)	(0.898)
Bank-Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort	Yes	Yes	Yes	Yes	Yes	Yes
N. obs	20,877	2,101	21,393	1,615	22,813	260
Adj. R^2	0.167	0.203	0.172	0.185	0.169	0.283

Panel B. Callaway & Sant'Anna's Difference-in-Difference

Sample	Non-Top 3	Top 3	Non-Top 5%	Top 5%	Non-GSIBS	GSIBS
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable is the *Unlevered Alpha*					
Resolution × *Post*	5.211***	-1.965	5.243***	-1.826	4.905***	-0.601
	(1.407)	(1.358)	(1.325)	(3.680)	(1.266)	(0.878)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N. obs	9,863	701	9,773	685	10,429	88

Table A.5. The Effect of Resolution Reforms on Implicit Subsidy: Excluding Dead Banks

This Table presents estimates from a staggered differences-in-differences model examining whether bank resolution reforms affect bank's implicit subsidies as shown in Equation (1) excluding banks that fail. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. Panel A shows the results using the stacked approach. Column (1) shows the regression results using the entire sample of banks, columns (2) and (3) show the regression results for non-large banks and large banks respectively. Panel B shows the analogous results using the estimator proposed by Callaway and Sant'Anna (2021). All standard errors are clustered at the country level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Panel A. Stacked Approach Difference-in-Difference

Sample	All	non-large	large
	(1)	(2)	(3)
	Dependent Variable is the *Unlevered Alpha*		
Resolution × *Post*	4.807***	6.038***	-0.723
	(1.431)	(1.675)	(1.001)
Bank-Cohort	Yes	Yes	Yes
Year-Cohort	Yes	Yes	Yes
N. obs	16,320	13,112	3,146
Adj. R^2	0.166	0.174	0.143

Panel B. Callaway & Sant'Anna's Difference-in-Difference

Sample	All	non-large	large
	(1)	(2)	(3)
	Dependent Variable is the *Unlevered Alpha*		
Resolution × *Post*	5.958***	6.297***	-0.420
	(1.405)	(1.712)	(1.169)
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N. obs	7,425	6,225	1,035

Table A.6. The Effect of Bail-in Resolution on Implicit Subsidy: TWFE with controls

This Table presents estimates from two-way fixed effects regression examining whether bank resolution reforms affect bank's implicit subsidies. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized unlevered abnormal returns calculated using Equation (2). *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. Column 1, 2 and 3 shows the regression results using the entire sample of banks, non-large banks and large banks respectively, respectively. All specifications include control variables at bank-level lagged by one year, which include *Dep/TA*, *Idiosync. Risk*, *B/M*, *ROE*, and *Lagged Return*, and control variables at country-level, which include *Bank Concentration*, *Banking Crisis*, *GDP Growth*, *Inflation* and *Unemployment*. For a description of these variables, see Table A.1. All the specifications include bank fixed-effects and year fixed-effects. All standard errors are clustered at the country-level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% level, respectively.

Sample	All	non-large	large
	Dependent Variable is the *Unlevered Alpha*		
	(1)	(2)	(3)
Resolution × Post	1.826**	3.080**	-0.609
	(0.857)	(1.084)	(0.886)
Dep/TA $_{t-1}$	8.767**	9.685***	3.280
	(3.439)	(3.176)	(6.010)
Idiosync. Risk $_{t-1}$	-0.492*	-0.418*	-1.372**
	(0.260)	(0.219)	(0.539)
B/M $_{t-1}$	3.323***	3.615***	1.833***
	(0.602)	(0.587)	(0.569)
ROE $_{t-1}$	0.098***	0.101***	0.056
	(0.019)	(0.020)	(0.032)
Lagged Return	0.003	0.001	0.020
	(0.008)	(0.007)	(0.020)
Bank Concentration	-0.095	-0.173**	0.090*
	(0.057)	(0.075)	(0.048)
Banking Crisis	-2.480***	-2.413**	-0.621
	(0.851)	(0.969)	(2.181)
GDP Growth	-0.059	-0.140	0.038
	(0.187)	(0.252)	(0.142)
Inflation	0.823***	1.140***	0.094
	(0.188)	(0.168)	(0.292)
Unemployment	-0.112	-0.300	0.184
	(0.220)	(0.261)	(0.173)
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N. of obs	11,532	10,450	1,054
Adj. R^2	0.174	0.183	0.151

Table A.7. The Effect of Bail-in Resolution on Equity Costs Advantages: Levered Alphas

This Table presents estimates from a staggered differences-in-differences model examining whether bank resolution reforms affect bank's implicit subsidies as shown in Equation (1). The dependent variable *Levered Alpha* is our measure of banks' implicit subsidy, and it is defined as the annualized levered abnormal returns calculated using Equation (2). *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. Panel A shows the results using the stacked approach. Column 1 shows the regression results using the entire sample of banks, columns 2 and 3 show the regression results for non-large banks and large banks respectively. Panel B shows the analogous results using the estimator proposed by Callaway and Sant'Anna (2021). All standard errors are clustered at the country level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Panel A. Stacked Approach Difference-in-Difference

Sample	All	non-large	large
	(1)	(2)	(3)
	Dependent Variable is the *Levered Alpha*		
Resolution × Post	12.273***	16.999***	-8.196*
	(3.713)	(3.890)	(4.166)
Bank-Cohort	Yes	Yes	Yes
Year-Cohort	Yes	Yes	Yes
N. obs	23,079	19,627	3,358
Adj. R^2	0.184	0.195	0.198

Panel B. Callaway & Sant'Anna's Difference-in-Difference

	(1)	(2)	(3)
Sample	All	non-large	large
	Dependent Variable is the *Levered Alpha*		
		3.735	
Resolution × Post	15.953***	18.263***	-3.737
	(2.673)	(3.049)	(4.578)
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N. obs	10,675	9,376	1,108

Table A.8. The Effect of Resolution Reforms on Equity Costs Advantages: Overidentified Model

This Table presents estimates from 2SLS regressions. The dependent variable *Unlevered Alpha* is our measure of banks' implicit subsidy, and is defined as the annualized unlevered abnormal returns calculated using Equation (2). The instrumented variable is the *RRI*, as in table 4. The instrumental variables are the Cumulative Number of banking Crises (*CNBC*) and *Fiscal Costs* defined as in table A.1. Column (1) reports the first-stage results using the entire sample of banks. Column 2 reports the results of the reduced form regression. Columns 3, 4 and 5 show the second-stage regression results for the entire sample, non-large banks and large banks respectively. All regressions include control variables at bank-level lagged by one year, *Dep/TA*, *Idiosync. Risk*, *B/M*, *ROE*, and *Lagged Return* and country-level, which include *Bank Concentration*, *Banking Crisis*, *GDP Growth*, *Inflation* and *Unemployment*. For a description of these variables, see Table A.1. Standard errors are clustered at the country-level and reported in parentheses beneath coefficient estimates. ***, **, and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)
Sample	All	All	All	non-large	large
Estimator	2SLS – First Stage	Reduced Form	2SLS – Second Stage	2SLS – Second Stage	2SLS – Second Stage
	Dependent Variable is				
	RRI_{t-1}	*Unlevered Alpha*	*Unlevered Alpha*	*Unlevered Alpha*	*Unlevered Alpha*
$CNBC_{t-1}$	5.276***	0.431**			
	(0.404)	(0.183)			
Fiscal Costs	9.315	1.556			
	(7.039)	(1.623)			
$\widehat{RRI_{t-1}}$			0.082**	0.110**	-0.033
			(0.031)	(0.047)	(0.028)
Dep/TA $_{t-1}$	9.000	3.332	2.611	4.171	-2.262
	(5.833)	(5.468)	(5.157)	(5.866)	(3.696)
Idiosync. Risk $_{t-1}$	-1.801	1.096**	1.234**	1.418***	0.219
	(1.254)	(0.489)	(0.465)	(0.333)	(0.537)
B/M $_{t-1}$	0.656	0.815	0.775	1.057	-0.103
	(0.973)	(1.034)	(1.009)	(1.186)	(0.365)
ROE $_{t-1}$	0.045	0.152***	0.147***	0.144***	0.131*
	(0.090)	(0.030)	(0.026)	(0.030)	(0.066)
Lagged Return	0.003	-0.000	-0.001	0.002	-0.028
	(0.008)	(0.006)	(0.005)	(0.006)	(0.022)
Bank Concentration	0.126	0.077	0.071	0.094	0.013
	(0.396)	(0.081)	(0.073)	(0.111)	(0.037)
Banking crisis	-17.336*	-0.584	1.029	1.903	-0.623
	(8.867)	(1.278)	(1.381)	(1.398)	(1.541)
GDP Growth	-1.214**	-0.153	-0.073	-0.136	0.053
	(0.476)	(0.164)	(0.174)	(0.231)	(0.147)
Inflation	-0.638	0.666**	0.729***	0.981***	0.093
	(1.259)	(0.289)	(0.235)	(0.294)	(0.227)
Unemployment	0.887	-0.066	-0.139	-0.273	-0.029
	(0.856)	(0.146)	(0.112)	(0.273)	(0.044)
Bank FE	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes
F-Stat 1st Stage			92.897	100.872	61.972
N. of obs	5,322	5,322	5,322	4,825	476
Adj. R^2	0.920	0.075	0.035	0.043	0.050

Table A.9. The Effect of Resolution Reforms on Equity Costs Advantages: A Portfolio Approach

This Table presents estimates from two-way fixed effects regression examining whether bank resolution reforms affect bank's implicit subsidies. Following the methodology of Gandhi and Lustig (2015), the dependent variable *Portfolio Alpha* is our measure of difference in implicit subsidies between large and small banks. It is defined as the annualized abnormal return for a long-short portfolio in each year in each country. The long-short portfolio has long positions in the banks of the largest size decile and short in the banks in the smallest size decile in each country in each year. *Resolution* is a dummy variable that takes the value of 1 if a given country has fully adopted bank resolution regulations in any year of our sample period, and *Post* is a dummy that takes the value of 1 in the years after a given country fully adopted the resolution rules. *RRI* measures the application of bank resolution regulation and goes from 0 (implementation has not occurred) to 100 (full implementation). Columns 1 and 2 show the regression results using *Resolution* × *Post*, columns 3 and 4 show the regression results using our granular measure *RRI*, respectively. All standard errors are clustered at the country level and reported in parentheses beneath coefficient estimates. *** and ** indicate that the coefficient estimate is significantly different from zero at the 1% and 5% levels, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable is the *Long-short Portfolio Alpha*				
Resolution × *Post*	-6.376***	-9.353**		
	(1.683)	(3.845)		
RRI_{t-1}			-0.113***	-0.194**
			(0.032)	(0.081)
Country FE	No	Yes	No	Yes
Year FE	No	Yes	No	Yes
N. obs.	114	113	65	64
Adj. R^2	0.06	0.05	0.14	0.28

Appendix B

The Distance-to-default of Bharath and Shumway (2008) is a simplified version of the Merton (1974) structural distance-to-default model. Merton's Distance-to-default (*DtoD*) measure considers equity as a call option on the underlying value of the market value of the firm's assets with a strike price equal to the firm's debt par value. A firm will default when its asset value falls below the debt par value. The model calculates the distance between the expected value of the asset and the default point (value of the debt). For each bank *i*, in year *t*, and country *c*, the modified Bharath and Shumway's *DtoD* is computed as follows:

$$DtoD_{i,c,t} = \frac{ln\left(\frac{E_{i,c,t} + D^*_{i,c,t}}{D^*_{i,c,t}}\right) + \left(\widehat{r_{i,c,t}} - \frac{\sigma^2_{V\,i,c,t}}{2}\right)T_{i,c,t}}{\sigma_{V\,i,c,t}\sqrt{T_{i,c,t}}} \quad (1.a)$$

where E is the market value of equity at the end of the year; D* is the par value of short-term and 1/2 of long-term debt at the end of the year; $\hat{r}$ is the bank expected return, given by each bank beta (from $\beta_1$of the Equation (2)) multiplied by the country Total Risk Premium from Damodaran's public dataset; T is set to one year; σ_V an approximation of the volatility of firm assets calculated using the following formula,

$$\sigma_{V\,i,c,t} = \frac{E_{i,c,t}}{E_{i,c,t} + D^*_{i,c,t}}\sigma_{E\,i,c,t} + \frac{D^*_{i,c,t}}{E_{i,c,t} + D^*_{i,c,t}}\left(0.05 + 0.25\sigma_{E\,i,c,t}\right) \quad (2.a)$$

where σ_E is the stock return standard deviation estimated using the weakly stock return from the year t-1.