Credit Risk and Systemic Spillovers from an Unexpected Bank Rescue: Evidence from India

Shashwat Agrawal*

November 17, 2025

*Ph.D. Candidate in Finance, Department of Finance, Fulton Hall, Carroll School of Management, Boston College, Chestnut Hill, MA 02467, USA. Email: agrawasb@bc.edu.

First draft: May 2023. This paper absorbs an earlier draft titled ”Moral Hazard Transmission Due to the Unexpected Rescue of a Private Bank: Evidence from India.” I am grateful to my committee Philip Strahan, Nadya Malenko, Ran Duchin, and Angela Ma, for formative discussions and continuous guidance. I would like to thank Prasanna Tantri, S. Narayan Rao, John C. Chu, Jessica Flagg, Jacob Dowell, and seminar participants at Boston College, the FMA Annual Meeting, and the Global Finance Conference for their helpful comments. I extend my sincere thanks to the Department of Finance at the Carroll School of Management (Boston College) for their financial support. Any errors remain my responsibility.

Credit Risk and Systemic Spillovers from an Unexpected Bank Rescue: Evidence from India

November 17, 2025

Abstract

I exploit an unexpected rescue of a private sector bank to examine spillover effects on other banks. The unprecedented protection of shareholder claims in the failed bank increases bailout expectations among other private sector banks, triggering two opposing channels of systemic risk. Affected banks engage in individual risk-shifting by increasing credit risk, lending more to risky borrowers, and restructuring loans. Yet, they also increase loan specialization due to weaker incentives to correlate assets with peers. On net, their contribution to systemic risk rises by about 23%, highlighting how bailouts can reshape risk-taking and amplify fragility across the banking system.

Keywords: Bank Risk, Bank Bailouts, Risky Lending, Specialization, Systemic Risk, Bailout Expectations

JEL Codes: E58, H81, G21, G28, G32, G33

1 Introduction

Governments around the world often bail out failed banks to reduce the risk of a systemic crisis.[1] However, a side effect of such interventions is that they alter the bailout expectations of other banks, influencing their risk-taking and lending behavior and, in turn, affecting systemic risk.[2,3] This paper is the first to examine how an unexpected bailout affects the systemic risk contribution of other banks, highlighting an underexplored mechanism linking bank bailouts to financial stability through changes in the incentives of other banks.

Theoretically, the impact on systemic risk is ambiguous and operates through two opposing channels. The *individual risk-shifting channel* suggests that higher bailout expectations create a risk-increasing effect, as banks get insulated from negative consequences. If many banks increase their individual risk, their collective behavior can increase systemic risk. Conversely, higher bailout expectations may also encourage specialization. Acharya and Yorulmazer (2007) argue that banks have incentives to correlate their loan portfolios to increase the likelihood of a joint rescue. These incentives weaken if banks anticipate a higher likelihood of individual rescue. Instead, banks will specialize across different borrower types to gain competitive advantages (*specialization channel*). To better understand systemic risk, it is important to examine changes in loan specialization. Although individual bank risk may increase, systemic risk could be mitigated if banks' assets become less correlated through the specialization channel. Given these opposing forces, the net effect remains an empirical question.

Using a novel shock in India, I find that the unexpected rescue of a private sector bank increases the individual risk of other private sector banks. Affected banks raise their credit risk by more than 30%, consistent with the individual risk-shifting channel. They do so by issuing more new loans to risky borrowers, expanding overall lending, restructuring loans

[1]The common belief that bank failures can lead to financial crises (Bernanke (1983); Ashcraft (2005)) motivates government bailouts, e.g. Goldsmith-Pinkham and Yorulmazer (2010).

[2]See, for example, Dam and Koetter (2012) in context of bank bailout expectations and bank risk-taking.

[3]Excessive credit risk poses systemic threats, potentially causing bank failures that harm borrowers (Peek and Rosengren (2000)) and triggering financial crises (Allen and Gale (2000)).

more frequently, and tilting their loan portfolios toward riskier borrowers. At the same time, using novel measures of bank specialization based on cosine distance in loan portfolios, I find that affected banks significantly increase their loan specialization, consistent with the specialization channel. Theoretically, these opposing forces make the net effect on systemic risk ambiguous. Empirically, however, the risk-shifting response dominates: affected banks' contribution to systemic risk rises by more than 23%. Thus, despite greater specialization, the overall spillover effect of an unexpected bailout on financial stability is negative.

The empirical literature on bank bailouts has primarily examined their effect on the rescued banks themselves.[4] In contrast, this paper investigates the spillover effects on the broader banking system, specifically, how bailouts influence the risk-taking and lending behavior of other banks, and the implications for systemic risk. Identifying these spillover effects presents two key challenges. First, a bank failure, its subsequent bailout, and the risk-taking of other banks may all stem from the same underlying economic shock. This makes it difficult to disentangle whether changes in other banks' behavior are due to a shift in bailout expectations (the mechanism of interest in this paper) or simply a response to the economic environment. This problem is especially severe when a bailout is anticipated, since it is difficult to isolate the effect of the change in expectations induced by the bailout itself, or when the analysis relies on only bank-level data, as such data cannot control for confounding factors such as a change in borrower demand driven by an underlying economic shock. Second, even with an unexpected rescue, it is difficult to find a suitable control group of banks whose bailout expectations remain unaffected by the event, which is essential for estimating the spillover effects.

I address these identification challenges in two ways. First, I exploit the unexpected rescue of "Yes Bank", a private sector bank in India, as a natural experiment.[5] This bailout shocked

[4]See, for example, Black and Hazelwood (2013) and Duchin and Sosyura (2014) for risk-taking by TARP banks, and Hryckiewicz (2014) and Poczter (2016) for the effects of government bailouts on risk-taking in other settings.

[5]Indian media highlight the moral hazard risks of government rescues of private sector banks, noting the "Yes Bank" bailout as the first such case in recent times.(https://www.thehindubusinessline.com/opinion/the-moral-hazard-in-banking/article68614584.ece).

financial markets, as evidenced by the bailed-out bank's stock price more than doubling and its credit rating jumping from default to investment grade within days of the bailout announcement. Since there was no precedent for a government-owned institution infusing capital into a failing private sector bank to ensure survival while preserving shareholder value, this induced stakeholders of other private sector banks to revise their future bailout expectations upwards.[6]

Second, the Indian setting provides an appropriate control group in the form of government-controlled banks (GCB). GCB already have implicit rescue guarantee, so will not update their bailout expectations in response to the event.[7] I implement a difference-in-differences (DID) framework that compares private sector banks (treatment group) to GCBs (control group). I exclude the bailed out bank in all my analysis. This design helps isolate the causal effect of the change in bailout expectations on bank outcomes.

To examine how banks adjust their behavior and the mechanisms underlying these effects, granular loan-level data are essential. Aggregate bank-level data make it difficult to disentangle whether an increase in credit risk reflects banks' greater willingness to lend to risky firms or heightened borrower demand. Moreover, without detailed loan information, it is challenging to identify specific lending strategies, such as increased loan specialization, that may influence systemic risk. Using loan-level data allows me to control for unobservable demand factors through borrower-time fixed effects. The results with these fixed effects suggest that the changes in outcomes are driven by altered bank incentives rather than shifts in borrower demand. The granularity of the data also enables the construction of precise measures of loan specialization and other lending patterns, offering deeper insights into the underlying mechanisms.

I start my empirical analysis by examining individual risk-taking for affected banks in

[6]Despite a more than 30% increase in the credit risk of the affected banks, their share of total commercial bank deposits rose slightly from around 30% in 2019 (pre-bailout) to around 31% in 2022 (post-bailout), suggesting that these banks' capital providers became less sensitive to risk due to heightened bailout expectations.

[7]Unlike private sector banks, GCB enjoy multiple capital infusions from the government and implicit/explicit guarantee of rescue (Marisetty and Shoeb (2024)).

response to the unexpected bailout. I use non-performing assets (NPA) as my main measure of credit risk.[8] I find that the NPA of affected banks increase by around 3 percentage points (more than 30% of the unconditional mean) after the unexpected bailout. The findings are consistent with the individual risk-shifting view.

To examine the specific actions through which banks increase their credit risk, I create three measures of bank risk-taking at the bank-quarter level using loan and borrower risk data. Using risky new loan exposure as my first measure, I find that affected banks increase the proportion of new lending to financially weak borrowers by 42% of the standard deviation. Loan restructuring involves modifying loan terms and is another mechanism for higher risk-taking by banks (Caballero et al. (2008)). I find that affected banks increase loan restructuring compared to GCBs post the unexpected bailout. The increase in restructuring suggests higher risk-taking by affected banks with existing borrowers and zombie lending. Overall, the affected banks expand the proportion of active relationships with risky borrowers after the unexpected bailout.

The granular corporate loan data allows me to better understand and answer how banks increase individual risk. I look at new loan issuance. The share of new loans issued by affected banks relative to the total new corporate loans issued by all commercial banks increases from 45% from 2017 to 2019 (pre-bailout) to 52% from 2020 to 2022 (post-bailout). Additionally, I find that banks issue significantly more new loans to risky borrowers compared to GCBs after the bailout. This result holds with borrower-time fixed effects. The findings suggest that affected banks not only increase lending, but also give more new loans to high-risk borrowers.

It could be that although banks extend more new loans to risky borrowers, their existing borrowers become less risky. To assess the overall impact, I examine changes in the outstanding loan portfolio of banks before and after the bailout. I find that the loan portfolio growth rate of affected banks is significantly higher than 2.5 percentage points compared to that of

[8]NPA are loans with no payments for 90 days or more, measured as a percentage of total advances. This metric is similar to non-performing loans (NPL). It is a standard proxy in the literature for credit risk.

GCBs. I next restrict the sample to risky borrowers. I find that the loan portfolio growth rate of affected banks to risky borrowers is significantly higher than 7 percentage points. This suggests that banks increase the size of their loan portfolios more for risky borrowers compared to less risky borrowers. The results hold even after controlling for borrower-time fixed effects. The findings imply that affected banks not only increase outstanding loans, but they also increase their exposure to risky borrowers at a rate that exceeds the rate at which they increase loan exposure to other borrowers.

The individual risk-shifting theory predicts that the effect will be stronger for banks in distress (Merton (1974); Jensen and Meckling (1976)). Consistent with the hypothesis, affected banks with weaker capital buffers and lower market-to-book values prior to the bailout increase risk more. Overall, the findings align with the individual risk-shifting channel.

Having established evidence for the individual risk-shifting channel, I now turn to the competing incentive for specialization. Banks face a strategic choice between correlating their portfolios to increase the likelihood of a joint bailout (Acharya and Yorulmazer (2007)) and specializing to gain greater information advantages and market power (Paravisini et al. (2023); Blickle et al. (2023)). The unexpected bailout, by increasing the likelihood of individual rescues, weakens the incentive to correlate portfolios and instead strengthens the incentive to specialize. To gain a clearer understanding of systemic risk, it is important to study how loan specialization evolves. Even if the risk of individual banks increases, overall systemic risk may decrease when banks' asset portfolios are less correlated. My unique setting and granular data allow me to test this specialization view. I construct three novel measures of loan specialization based on the cosine distance between banks' pairwise lending portfolios across firms, industries, and regions. Across all three measures, affected banks significantly increase their specialization following the unexpected bailout. This implies that banks shift their exposure toward more unique sets of borrowers, industries, and regions, suggesting a positive spillover effect of the unexpected bailout on specialization.

Given these two opposing forces, namely, increased individual risk-shifting and increased loan specialization, the net spillover effects on systemic risk are nuanced. To understand the net effects, I use standard tail-risk measures from the literature, including marginal expected shortfall (MES) and conditional value at risk (CoVaR), along with other predictors such as beta (Adrian and Brunnermeier (2016); Acharya et al. (2017)). I find a significant increase in all these measures of systemic risk for affected banks relative to GCBs following the unexpected bailout. These results suggest that the individual risk-shifting effect dominates, leading to a net rise in systemic risk after the rescue.

Apart from risk-shifting and specialization, I also consider the charter value view, which argues that an increase in bailout expectations will create a risk-reducing effect by increasing the present value of future bank profits as a going concern (Keeley (1990); Demsetz et al. (1996)). My results do not align with this hypothesis, as the affected banks increase risk.[9]

Overall, my paper highlights the unintended consequences of unexpected bank rescues. Even a single unexpected bailout can propagate moral hazard and amplify systemic risk through spillovers to other banks. These results underscore the need for bank resolution frameworks that account for the broader incentive effects of targeted interventions.

Related literature and contribution. Existing literature has predominantly focused on the implications of government guarantees, capital infusions, and bailouts, highlighting how these interventions alter incentives for risk-taking and lending among the rescued banks.[10] However, systematic studies on how such interventions spill over to other financial institutions remain limited. I contribute by studying how an unexpected bailout affect the risk and behavior of other banks.

The closest strand of literature to this paper examines the spillover effects of bailouts on competitor banks (Furfine (2006); Goldsmith-Pinkham and Yorulmazer (2010); Gropp et al.

[9] I rule out further alternative explanations by showing that the relative increase in risk-taking behavior by affected banks post-bailout is not driven by their pre-existing exposure to COVID-19 industries, the series of government bank mergers, or the inclusion of too-big-to-fail (TBTF) banks.

[10] See, for example, Flannery (1998), Hovakimian and Kane (2000), Sironi (2003), Black and Hazelwood (2013), Strahan (2013), Duchin and Sosyura (2014), Hryckiewicz (2014), Poczter (2016), Chari and Kehoe (2016), and Behr and Wang (2020)

(2011); Calderon and Schaeck (2016); Cardillo et al. (2023)). A key mechanism emphasized in this work is the competition channel, which argues that an increase in the market power of a bailed-out bank will decrease the charter value of other banks, creating a risk-increasing effect (Hakenes and Schnabel (2010)). Gropp et al. (2011) use bank-level data to show that competitors of banks with higher bailout expectations increase risk, attributing this to the competition channel. Similarly, Cardillo et al. (2023) apply a matching technique with bank-level data and find that competitor banks increase risk following unconditional bailouts. In contrast, my difference-in-differences design inherently controls for the competition channel by focusing on the relative increase in risk-taking among private sector banks compared to government-controlled banks (GCBs). Hence, the competition channel does not apply in my setting. Furthermore, my paper contributes to this literature in two key ways. First, I exploit a plausibly exogenous and unexpected bailout shock, allowing for cleaner identification of spillover effects, including within borrower-time variation. Second, my granular loan-level data allow me to uncover mechanisms that differ from those in prior work and to analyze bank actions in greater detail. Specifically, I document that the spillover effects operate primarily through the individual risk-shifting channel and the specialization channel. This leads to new findings, such as affected banks increasing lending to risky borrowers while simultaneously specializing their loan portfolios.

The literature on specialization suggests that banks specialize to gain market power and information advantages (Paravisini et al. (2023); Blickle et al. (2023)). In contrast, the literature on commonality in bank asset holdings examines the incentives for and implications of correlated portfolios for systemic risk (Acharya and Yorulmazer (2007); Gandhi and Purnanandam (2021); Goldstein et al. (2024); Jasova et al. (2024)). Acharya and Yorulmazer (2007) argues that banks correlate their portfolios to increase the likelihood of joint bailouts. While prior studies have explored the determinants and consequences of specialization and portfolio commonality, little is known about how these behaviors adjust in response to change in bailout expectations induced by an unexpected bailout. I contribute to this literature by providing novel evidence on changes in loan specialization following an unexpected bailout. Using a new measure of specialization based on the cosine distance of loan portfolios across banks, I show that affected banks increase loan specialization after an unexpected bailout.

Finally, my paper relates to the literature on systemic risk. This literature has primarily developed frameworks and measures to quantify systemic risk (Huang et al. (2009); Adrian and Brunnermeier (2016); Acharya et al. (2017); Brownlees and Engle (2017); Liu (2023); Altermatt et al. (2024)). In contrast, my paper applies these measures in the context of an unexpected bailout to show that the contribution of affected banks to systemic risk increases following the intervention. This highlights how unexpected bailouts can amplify broader economic risks, complementing the measurement focus of prior work.[11]

2 Institutional Details

2.1 "Yes bank" failure

"Yes bank" was the eighth-largest bank in India in March 2018. It had been suffering from increasing bad loans and declining capital ratios since the Reserve Bank of India's (RBI) asset quality reviews in 2017 and 2018. The bank had significant exposure to distressed borrowers. The stock price declined steadily, and depositor withdrawals rose.

The pivotal moment occurred on the evening of 5 March 2020, when the RBI imposed a moratorium restricting the bank's normal operations. The bank's depositors were restricted from withdrawing more than ₹50,000 ($700) from their accounts for thirty days. The market, in anticipation of the bank's collapse, reacted negatively as the stock price of the bank declined by more than 55% from ₹37 to ₹16 on the next trading day. Three major credit agencies-Brickwork, CARE, and ICRA- downgraded the debentures of the bank to default,

[11] This paper also relates to the literature on Indian banking, which has examined how bankruptcy reforms and regulatory interventions shape credit allocation (Cole (2009); Iyer and Puri (2012); Vig (2013); Gopalan et al. (2016); Kumar (2020); Chopra et al. (2021); Mishra et al. (2021); Kalimipalli et al. (2024); Kulkarni et al. (2025)). This body of work highlights the fragility of banks, the role of government-controlled banks, and the economic consequences of regulatory policies.

the lowest possible rating (Table 1). ICRA also downgraded the certificate of deposit of "Yes bank" to Default.[12] This suggests that the market had little or no expectation of a government rescue.

2.2 Unexpected rescue of the bank

The market reaction, combined with credit rating agencies downgrading deposits to default, suggests little or no expectation of rescue. On 6 March 2020, the RBI initiated a reconstruction scheme for the failed bank. A week later, on 13 March 2020, the bank was bailed out under the RBI scheme by a group of eight banks led by India's largest government-owned bank, the State Bank of India (SBI). SBI injected around 70% of the rescue capital. As part of the rescue plan, 75% of shares held by existing shareholders were locked in for three years. If the number of shares held by old shareholders was less than 100, there was no lock-in. Shares purchased after 13 March were not locked in. SBI infused ₹60.5 billion for about a 35% stake in the bailed-out bank. Eight banks, including SBI, acquired almost 49% of the shares at ₹10 per share by infusing ₹100 billion in the bailed-out bank. For three years, SBI and the other banks were mandated to hold at least 75% of the shares they bought as part of the rescue plan.

The market reacted positively to the rescue, as the stock price of the bailed-out bank jumped more than 100%, from ₹26 to ₹54, over the next four trading days (Figure 1). Furthermore, ICRA upgraded the rating for the debentures of the rescued bank from D (Default) to BB+ (Inadequate Safety), as shown in Table 1. The tremendous positive market response and the credit rating upgrade indicate an unexpected rescue. The sharp positive reaction—where the bailed-out bank's stock price more than doubled, ending even higher than its pre-rescue level—highlights the government's unexpected intervention. Furthermore, credit rating agencies upgraded the rescued bank's bond rating by multiple notches after the

[12] ICRA does not explicitly state if they only consider uninsured deposits when rating instruments. On the other hand, CARE Ratings typically rates certificates of deposit (CDs) one notch higher due to the added stability provided by deposit insurance.

bailout. This implies that neither the market nor other agents, such as rating agencies, expected the bailout. This allows me to use the bank's unexpected rescue as a plausibly exogenous shock to bailout expectations of other private sector banks and their stakeholders.

2.3 Indian Banking System

The Indian banking system consists of government-controlled banks (GCBs), private sector banks, foreign banks, cooperative banks, and regional rural banks. The Reserve Bank of India (RBI) is the central bank and regulator of the system. GCBs once dominated the Indian banking system, but over the years, the share of private sector banks has increased. The Indian economy relies heavily on banks for credit, with over 50% of financial assets owned by banks.[13] In my analysis, I consider GCBs and private sector banks, which together account for more than 90% of credit in India (Chopra et al. (2021)).

2.4 Private Sector Banks versus Government Banks

The Indian banking sector can be broadly classified based on ownership into two main categories: private sector banks and government-controlled banks (GCBs). As the name suggests, private sector banks are owned by private entities, whereas GCBs have more than 50% direct or indirect state ownership.

Over the past two decades, distressed government banks have been bailed out either through mergers with larger, better-performing government banks or via capital infusions. This has not been the case for private sector banks. Historically, depositors' money has mostly been protected (though not always in a timely manner), but shareholders of private sector banks have borne the brunt and, in many cases, lost all value. The "Yes Bank" rescue was a rare case in which a government-owned institution injected capital into a failed private sector bank, preventing the destruction of existing shareholder wealth. This sets a precedent, leading other private sector banks to expect similar treatment in the future. As a result,

[13] See https://data.bis.org/topics/TOTAL_CREDIT/tables-and-dashboards

they will revise their bailout expectations upwards.

3 Data

To test the hypotheses, I draw on multiple data sources combining accounting, stock market, and loan-level information. This section describes the data sources and sample selection, details the construction of key variables, and presents summary statistics.

3.1 Data Sources and Sample Selection

I obtain accounting and stock price information for firms and banks from the Prowess database maintained by the Centre for Monitoring Indian Economy (CMIE). The CMIE database contains audited financial statements and stock trading data for both banks and firms.

I gather loan-level and restructuring data from a database maintained by the Ministry of Corporate Affairs (MCA) under the Government of India. The MCA database consists all registered secured loans. Non-registration results in the loss of privileges associated with secured loans, making it likely that nearly all such loans are registered. The MCA data provide details such as the identities of both lenders and borrowers, loan amounts, disbursal dates, any restructuring dates, and final repayment dates.

The final sample spans the period 2017–2022. I consider all scheduled commercial government-controlled and private sector banks that operate in both the pre-bailout (2017–2019) and post-bailout (2020–2022) periods. I exclude "Yes Bank," the private sector bank that was unexpectedly rescued, to ensure that the treated group represents the set of private sector banks other than the rescued institution. Table A.2 reports the detailed sample construction for the bank–quarter data.

I begin with bank–quarter data derived from the quarterly financial statements of banks in CMIE. To obtain loan information such as exposure to risky borrowers and loan specialization, I match the bank–quarter data with aggregated and processed data from both the MCA and CMIE. I obtain 714 observations at the bank–quarter level. The sample includes a total of 16 treated banks and 14 control banks in both pre- and post-bailout periods. The total number of year–quarters is 24. I also construct a bank–month sample using bank stock performance data from CMIE, yielding 2,114 observations at the bank–month level.

Using the MCA data, I create a bank–borrower–quarter-level outstanding loan sample. I begin with all bank–borrower pairs that have an outstanding lending relationship in 2017. A bank–borrower pair enters the sample whenever the bank extends a new loan to the firm and exits once the borrower fully repays the outstanding loan. The final sample consists of 873,218 observations from 2017 to 2022. I match the bank–borrower–quarter sample with the CMIE database to obtain information on financial statements and stock performance for both banks and firms.

3.2 Variable Construction

I construct variables at multiple levels of granularity, namely, bank-quarter, bank-borrower-quarter, and bank-month to capture bank behavior, risk-taking, and systemic exposure. Key outcome variables include measures of credit risk, such as the non-performing assets (NPA) ratio; lending activity, such as new loan issuance; specialization, based on cosine distance in loan portfolios; and systemic risk, such as Marginal Expected Shortfall (MES) and Delta Conditional Value-at-Risk (Delta CoVaR).

NPA is defined as loans with no payments for 90 days or more as a percentage of total advances. Risky lending is measured using borrower financials, such as negative profits after tax (PAT <0). Restructuring activity is captured as the share of a bank's outstanding loans that were restructured in a given quarter.

Specialization is measured using cosine distance between banks' pairwise lending exposure across borrowers, industries, and regions. Detailed variable construction of specialization measures is elaborated in subsection 5.1. Systemic risk measures, namely, MES, Delta

CoVaR, and beta are constructed in spirit of the literature. Details of systemic risk measures are elaborated in subsection 5.2.

Control variables include lagged bank-level characteristics such as the capital ratio, return on assets (ROA), and the log of total assets and deposits. COVID-19 exposure is defined based on the Kamath Committee's classification of vulnerable industries. A detailed description of all variables is provided in Table A.1.

3.3 Summary Statistics

Table 2 presents the descriptive statistics for the main variables used in my analysis. The statistics are reported for three distinct datasets, namely, bank-quarter, bank-borrower-quarter, and bank-month.

For the bank-quarter dataset, the average non-performing asset (NPA) ratio is 8.15%, with a median of 6.66%. This suggests right-skewness in the distribution. The sample is evenly split between treated and control groups with a treat mean of 0.53, and pre and post periods with a post mean of 0.5. Banks exhibit moderate exposure to risky new loans with a mean of 10.1% and restructuring with a mean of 1.9%. The specialization measures, namely, *Specialization*, *Specialization Industry*, and *Specialization Region*, are bounded between 0 and 1, with 1 being perfect specialization and 0 being the limiting value of perfect correlation. Specialization values are high on average with mean ranging from 0.42 to 0.92 across the three measures of specialization. This suggests differentiated lending portfolios. The average bank has a capital ratio of 14.31% and a return on assets of 13%.

In the Bank-Borrower-Quarter loan-level dataset, the average log loan outstanding is 18.82, and about 44.6% of the loans are issued to private firms. Among risky borrowers, 19.5% have negative PBIT, 28.3% have negative PAT, and 24.1% have an interest coverage ratio (ICR) below 1.

For the Bank-Month dataset, systemic risk measures indicate moderate bank interconnectedness. The average marginal expected shortfall (MSE) is 0.03, and the average Delta

CoVaR is 0.01. The average stock return correlation is 0.42, and the average beta is 1.3.

4 Individual risk-taking of banks

My research examines the spillover effects of an unexpected bank bailout on competitor banks' credit risk, specialization, and contribution to systemic risk. The central hypothesis tests whether the rescue of one bank affects the risk-taking behavior of other banks. In this section, I analyze the effect of the unexpected bailout on the individual risk of other private sector banks relative to GCBs, using a difference-in-differences (DID) framework with fixed effects.

4.1 Spillover effects of unexpected rescue on credit risk of other banks

In the first set of results, I regress bank risk measures on the interaction between post bailout and treated bank indicators with bank-quarter data from Q1 2017 to Q4 2022. This is a standard DID model with private sector banks as the treated group and GCBs as the control group. I exclude "Yes Bank" from the sample.

The unprecedented government rescue of a private sector bank, with no precedence of shareholders being bailed out, provides a natural experiment to study spillover effects. Unlike GCBs, which benifit from implicit bailout guarantees, stakeholders of private sector banks increase their bailout expectations in response to the event. The literature highlights the individual risk-shifting channel that argues that bailout expectations protect banks from the consequences of failure, incentivizing higher risk-taking (Jensen and Meckling (1976); Farhi and Tirole (2012)). To test this mechanism, I estimate the following DID specification:

$$Bank\,Risk_{i,t} = \alpha + \beta_1 Treat_i \times Post_t + aX_{i,t} + \gamma_i + \lambda_t + \epsilon_{i,t}, \tag{1}$$

where $Bank\,Risk_{i,t}$ is the main dependent variable for bank i in the quarter t proxied by non-performing assets (NPA), a measure of credit risk. NPA are loans with no payments for 90 days or more, measured as a percentage of total advances. Explanatory variables are $Post_t$ and $Treat_i$. $Post_t$ takes a value of 1 if the period t is greater than March 2020, otherwise 0. $Treat$ takes a value of 1 if bank i is a private sector bank, otherwise 0. The main coefficient of interest is β_1. It gives the average treatment effect on the treated (ATT), where the treated group consists of private sector banks, and the treatment is the spillover effect arising from the unexpected bailout. $X_{i,t}$ is a bank-level control vector that includes one-year lagged values of log of bank assets, log of bank deposits, capital ratio, and return on assets (ROA).[14] γ_i represents bank fixed effects, and λ_t represents time fixed effects. The standard errors are clustered at the bank level.

The coefficient on the interaction term $Treat_i \times Post_t$ is the key variable of interest. In column 1 of Table 3, I estimate OLS without bank level controls and without any fixed effects. This is the baseline specification. To partially address endogeneity issues arising due to confounding by unobservable bank measures and time effects, in my tightest specification, I use bank fixed effects and year-quarter fixed effects in columns 5 and 6 of Table 3. In all six specifications, the coefficient on the interaction term is positive and statistically significant at the 1% level. This implies that post-bailout, treated banks' risk increased by 3 percentage points more than that of the control group. This represents more than a 30% increase in credit risk relative to the unconditional mean risk of banks. The increase in risk is economically significant.[15]

I find that the magnitude of the results does not depend on the fixed effects used. Therefore, one can reasonably conclude that the unexpected bailout leads to an increase in the risk of other private sector banks relative to control banks.

[14] One may argue that the bank-level control variables are 'bad controls,' since they are themselves affected by the treatment. This concern is alleviated, as my core results remain similar even without the control variables (e.g., columns 1, 3, and 5 of Table 3).

[15] I also consider stock return volatility as an alternative measure of bank risk. The findings as shown in Table A.3 are positive and statistically significant. I observe a 0.4 percentage points increase in stock volatility, representing more than a 16% increase in risk.

4.2 Pretrends

To address any concerns about the possibility that my results represent a continuation of a pre-existing trend, I modify the DID specification to include variables that account for pre-trends, i.e., indicator variables representing quarters before and after the unexpected rescue of "Yes bank". The dynamic DID regression specification on bank-quarter data is as follows:

$$Bank\,Risk_{i,t} = \alpha + \sum_{k=-5}^{-2} \beta_k Pre_{i,t}^k + \sum_{k=0}^{8} \beta_k Post_{i,t}^k + aX_{i,t} + \gamma_i + \lambda_t + \epsilon_{i,t}, \tag{2}$$

where $Bank\,Risk_{i,t}$ is the dependent variable for bank i in year-quarter t, measured by non-performing assets (NPA). NPA are loans with no payments for 90 days or more, measured as a percentage of total advances. The main explanatory variables are $Pre_{i,t}^k$ and $Post_{i,t}^k$. k represents the period relative to the treatment, where $k = 0$ is the treatment period, $k = -1$ is the period immediately before treatment, $k = 1$ is the period immediately after treatment, and so on. $Pre_{i,t}^k$ equals 1 for treated banks k quarters before the unexpected rescue and equals 0 otherwise. $Post_{i,t}^k$ equals 1 for treated banks k quarters after the unexpected rescue and 0 otherwise. The coefficients of interest are β_k for $\forall, k \in \{-5, 8\}$. $X_{i,t}$ is a bank-level control vector including one-year lagged values of the log of bank assets, the log of bank deposits, the capital ratio, and return on assets (ROA). γ_i is the bank fixed effect, and λ_t is the time fixed effect. The standard errors are clustered at the bank level. I exclude "Yes Bank" from the sample.

Figure 2 shows the trend plot. It measures the risk difference between private sector banks and GCBs over time. In Figure 2, we see that the risk difference was stable before the unexpected bailout, indicating the absence of a pre-trend.[16] After the bailout, the NPA of affected banks consistently increased relative to that of GCBs. The increase in NPA for

[16] I also conduct dynamic difference-in-differences analysis using *stock return volatility* as an alternative measure of bank risk. I find no pre-trend and a significant increase in monthly return volatility for treated banks relative to government banks after the unexpected bailout.

affected banks reaches 5 percentage points after about six quarters and then stabilizes. The increase in the risk of private sector banks does not reverse even after 8 quarters, suggesting a persistent shift in equilibrium.

4.3 Evidence on Bank Actions

In this section I examine the specific actions through which banks increase their credit risk.

4.3.1 Treated banks issue higher proportion of new loans to risky borrowers

Do banks issue a higher proportion of new loans to firms with poor financial health after the unexpected bailout? To test the hypothesis, I run Equation 1 on bank-quarter data from Q1 2017 to Q3 2022 with *Risky New Loan Exposure* as the dependent variable. I exclude "Yes Bank" from the sample. The dependent variable captures the proportion of new loans to risky borrowers. It is measured as the new loan amount weighted share of new loans granted to risky borrowers, where risky is defined as borrowers with negative profits (Profit After Tax (PAT) <0). The weighting is based on the total new loan amount granted to the borrower f by the bank i in quarter t. The results are shown in columns 1 and 2 of Table 4.

The coefficient on $Post_t \times Treat_i$ is positive and statistically significant at the 1% level. Relative to the standard deviation of *Risky New Loan Exposure*, the affected banks increased their proportion of new loans to risky borrowers by around 42%. This finding suggests that banks increase lending to risky borrowers following the unexpected bailout.

4.3.2 Treated banks restructure more loans

Restructuring of loans involves modification of loan terms to help borrowers who are facing financial difficulty. More restructuring suggests a higher risk-taking by banks with existing borrowers and zombie lending.[17] In the above subsubsection 4.3.1, we saw that banks give more risky new loans. What about the existing borrowers? Do private sector banks do more

[17] See Caballero et al. (2008).

loan restructuring compared to GCBs post the unexpected bailout? To test the hypothesis, I run Equation 1 on bank-quarter data from Q1 2017 to Q3 2022 with *Loan Restructuring* as the dependent variable. I exclude "Yes Bank" from the sample. The dependent variable is the share of a bank's outstanding loans that were restructured in a quarter. The results are shown in columns 3 and 4 of Table 4.

The coefficient on $Post_t \times Treat_i$ is positive and statistically significant at the 5% level. Relative to the mean restructuring rate, affected banks increase restructuring rate by around 26%. This finding suggests that banks increase loan restructuring following the unexpected bailout.

4.3.3 Loan portfolio of affected bank tilts towards risky borrowers

Do banks increase their exposure to risky borrowers after the unexpected bailout? In subsubsection 4.3.1 I showed that treated banks issue a higher proportion of new loans to risky borrowers. However, if the existing borrowers become less risky or if risky borrowers end their relationship with banks, then the banks can overall have a less risky loan portfolio after the bailout. To test whether the loan portfolio of treated banks becomes more risky after the unexpected bailout, I run Equation 1 on bank-quarter data from Q1 2017 to Q3 2022 with *Risky Loan Exposure* as the dependent variable. I exclude "Yes Bank" from the sample. The dependent variable captures the proportion of outstanding loans to risky borrowers. It is measured as the loan amount weighted share of loans outstanding to risky borrowers, where risky is defined as borrowers with negative profits (Profit After Tax (PAT) <0). The weighting is based on the total loan amount outstanding to the borrower f by the bank i in quarter t. The results are shown in columns 5 and 6 of Table 4.

The coefficient on $Post_t \times Treat_i$ is positive and statistically significant at the 10% level. Relative to the mean exposure to risky borrowers, affected banks increase exposure to risky borrowers by around 17%. The result indicates that banks expand their exposure to financially distressed firms after the unexpected bailout. Overall, the findings in subsubsection 4.3.1, subsubsection 4.3.2, and subsubsection 4.3.3 align with the individual risk-shifting channel.

4.4 Loan level analysis

I next analyze the granular loan-level data to provide more direct evidence of bank actions. The use of granular loan data allows me to control for unobservable demand factors by absorbing all time-varying borrower characteristics with borrower-time fixed effects. The findings with these fixed effects support the hypothesis that a change in risk for affected banks is due to changed incentives of banks following the unexpected bailout rather than the change in borrower demand.

4.4.1 Treated banks extend more new loans to risky borrowers following the unexpected bailout

If treated banks perceive higher rescue likelihood, they will relax credit standards and expand lending. Particularly, the treated banks will issue more new loans to risky borrowers. To test whether private sector banks issue more new loans I use secured corporate new loan data for private and public borrowers at the bank-borrower-quarter level from 2017Q1 to 2022Q3. I analyze the share of new secured corporate loans issued by GCBs and private sector banks before and after the unexpected bailout. I exclude "Yes bank" from the sample. The results are illustrated in Figure 4. The share is calculated relative to the total new corporate loans issued by scheduled commercial banks, both GCBs and private sector banks. The 'before' period covers Q1 2017 to Q4 2019, while the 'after' period covers Q1 2020 to Q3 2022. The share of new loans issued by private sector banks increases from 45% before the bailout to 52% after the bailout. This suggests that treated banks increase the size of their loan portfolios as they issue more new loans than GCBs.

To investigate whether treated banks disproportionately issued new loans to less risky borrowers, I estimate Equation 3 using bank-borrower-quarter level data from Q1 2017 to

Q3 2022. The sample is restricted to publicly listed borrowers. The dependent variable is $New\,Loan_{i,f,t}$, an indicator that equals one if bank i issues a new loan to firm f in quarter t. The key independent variable is the triple interaction term $Treat_i \times Post_t \times Risky\,Borrower_{f,t}$. For identifying risky borrower, I use three indicators, namely, $PBIT < 0$, $PAT < 0$, and $ICR < 1$. $PBIT < 0$ is a dummy variable that equals one for firm-quarters where Profit Before Interest and Taxes (PBIT) is negative, and zero otherwise. $PAT < 0$ is a dummy variable that equals one for firm-quarters where Profit After Taxes (PBIT) is negative, and zero otherwise. $ICR < 1$ is a dummy variable that equals one for firm-quarters where the Interest Coverage Ratio (ICR) is less than 1, and zero otherwise.

The specification is:

$$\begin{aligned} New\,Loan_{i,f,t} = \alpha + \beta_1 Post_t \times Treat_i + \beta_2 Treat_i \times Risky\,Borrower_{f,t} \\ + \beta_3 Treat_i \times Post_t \times Risky\,Borrower_{f,t} + aX_{i,t} \\ + \gamma_i + \lambda_t + \delta_{j,t} + \theta_{f,t} + \epsilon^j_{i,f,t}, \end{aligned} \tag{3}$$

where i subscripts bank, f subscripts firms, t subscripts quarter, and j superscripts industry. γ_i is bank fixed effect, λ_t is time fixed effect, $\delta_{j,t}$ is industry-year-quarter fixed effect, and $\theta_{f,t}$ is borrower-year-quarter fixed effect. The remaining variables are as defined for Equation 1. The coefficient of interest is β_3. The triple interaction coefficient informs us about the additional new loans that treated banks issue to risky borrowers compared to control banks, above any differences in lending to safe borrowers.

The results are shown in Table 5. $Risky\,Borrower_{f,t}$ is represented using three indicators, namely, $PBIT < 0$ in columns 1 and 2, $PAT < 0$ in columns 3 and 4, and $ICR < 1$ in columns 5 and 6. A $Risky\,Borrower_{f,t}$ value of one indicates a risky borrower. Across all 6 columns, the coefficient on $Treat_i \times$ $Post_t \times$ $Risky\,Borrower_{f,t}$ is positive and statistically significant at the 1% level. The results hold even after including borrower-year-quarter fixed effects in columns 2, 4, and 6. This indicates that new lending relationships are concentrated among risky firms even after controlling for time variant borrower characteristics. The results in this section suggest that treated banks not only issue more new loans than control banks at the aggregate level but also supply more new loans to risky borrowers.

4.4.2 Treated banks increase the size of the loan portfolio

To test whether treated banks increase the size of their loan portfolios using granular loan data I run the below specification on bank-borrower-quarter data from Q1 2017 to Q3 2022. The dependent variable is $Loan\,Outstanding_{i,f,t}$, which is the log of loans outstanding for a bank-borrower pair in quarter t. The data contains all private and public borrowers.[18] The specification is as follows:

$$Y_{i,f,t} = \alpha + \beta Post_t \times Treat_i + aX_{i,t} + \gamma_i + \lambda_t + \delta_{j,t} + \theta_{f,t} + \epsilon^{j}_{i,f,t}, \tag{4}$$

where i subscripts bank, f subscripts firms, t subscripts quarter, and j superscripts industry.γ_i is bank fixed effect, λ_t is time fixed effect, $\delta_{j,t}$ is industry-year-quarter fixed effect, and $\theta_{f,t}$ is borrower-year-quarter fixed effect. The remaining variables are as defined for Equation 1.

The results are shown in columns 1 to 3 of Table 6. The coefficient on the interaction term $Post_t \times Treat_i$ is positive and statistically significant at 10% level in all three columns. In column 3, I include borrower-year-quarter and bank fixed effects, controlling for time-varying borrower characteristics and time-invariant bank factors, respectively. Within a borrower-year-quarter treated banks increase loan size by 5.2 percentage points compared to GCBs. This implies that affected banks increase the size of their portfolio at a faster rate compared to GCBs.

[18] I also conduct an empirical analysis on the subsample comprising only public borrowers. The results remain qualitatively similar.

4.4.3 Treated banks increase loan outstanding to risky borrowers

The previous subsubsection 4.4.2 does not tell us whether treated banks have increased the outstanding loan size to marginally risky borrowers compared to control banks. It could be the case that although treated banks have increased exposure to more borrowers, the exposure to risky borrowers is less. To test whether treated banks' increased loan exposure to risky borrowers, I run Equation 4 on a sub-sample of risky borrowers using corporate loan data at the bank-borrower-quarter level from Q1 2017 to Q3 2022. A borrower is identified as risky if its Profit Before Interest and Taxes (PBIT) is less than zero, or Profit After Tax (PAT) is less than zero, or Interest Coverage Ratio (ICR) is less than 1. Since I do not have risk measures for private companies, I do risk analysis only for public listed borrowers. The dependent variable is $Loan\,Outstanding_{i,f,t}$, which is the log of loans outstanding for a bank-borrower pair in quarter t.

The results of the subsample analysis for risky borrowers are shown in columns 4 to 6 of Table 6. The coefficient on the interaction term $Post_t \times Treat_i$ is positive and statistically significant in all 3 columns. In column 6, I include borrower-year-quarter and bank fixed effects, controlling for time-varying borrower characteristics and time-invariant bank factors, respectively. Within a borrower-year-quarter, treated banks increase loan exposure to risky borrowers by 20 percentage points compared to GCBs. The results imply that treated banks increase the size of their loan exposure to risky borrowers relative to GCBs.

Additionally, the coefficients of columns 4 to 6 are at least twice as large as those of columns 1 to 3 of Table 6. This suggests that, treated banks not only increase loan outstanding to risky borrowers, but they also increase their exposure to risky borrowers at a rate that exceeds the rate at which they increase loan exposure to other borrowers.

4.5 Cross-sectional evidence on risk-shifting across bank fragility

If the unexpected bailout of a private sector bank induces other banks to revise their likelihood of rescue upwards, then they will be incentivized to take more risks as per the risk-shifting channel. The results presented in subsection 4.1 and subsection 4.4 are consistent with this channel. If the individual risk-shifting channel is one of the driving forces, then more fragile banks will increase risk more than other banks (Merton (1977)). In this subsection I carry out a sub-sample analysis to test this hypothesis.

I measure the fragility of treated banks using two pre-event measures, namely, the market-to-book ratio and the capital ratio. First, I divide the treated banks into two groups. Treated banks with a market-to-book ratio lower than the median are classified as low market-to-book banks, and those with a ratio higher than the median are classified as high market-to-book banks. I run Equation 1 separately on these two sub-samples. Both sub-samples include all the control group banks, but the high market-to-book sample contains only high market-to-book treated banks, and the other sample contains only low market-to-book treated banks. The results are presented in columns 1 and 2 of Table A.4. I find that low market-to-book banks increase credit risk significantly more than high market-to-book banks.

Next, I divide the treated banks based on their capital ratio. Banks with a capital ratio higher than the median are classified as high capital banks, while those with a ratio lower than the median are classified as low capital banks. I run Equation 1 separately on these two sub-samples. Again, both sub-samples include all control group banks, but the high capital sample contains only high capital treated banks, and the other sample contains only low capital treated banks. The results are presented in columns 3 and 4 of Table A.4. I find that low capital banks increase credit risk significantly more than high capital banks. Using both market-based and book-based measures of bank fragility, and in line with the individual risk-shifting view, I find that more fragile treated banks increase risk significantly more than their less fragile counterparts.

5 Specialization and Systemic Risk

The important question is how an unexpected bailout affects systemic risk. The previous section 4 shows that affected banks increase individual risk following the unexpected bailout, consistent with the individual risk-shifting channel. However, an increase in individual risk does not mechanically imply an increase in systemic risk. To better understand systemic risk, it is important to examine how loan specialization changes. Although individual bank risk increases, systemic risk may be mitigated if banks' assets become less correlated. To disentangle these forces, the next two sections examine the spillovers on specialization and systemic risk. First, I test whether affected banks shift toward more differentiated loan portfolios across borrowers, industries, and regions. Next, I evaluate whether the contribution of affected banks to systemic risk increases. Together, these analyses provide a better understanding of how the unexpected bailout reshapes not only individual bank behavior but also the broader stability of the financial system.

5.1 Spillover effects on bank specialization

The specialization view suggests that banks differentiate their loan portfolios to extract information advantage and gain market power (Paravisini et al. (2023); Blickle et al. (2023)). In contrast, the asset correlation view emphasizes that banks correlate loan portfolios to raise the likelihood of joint bailouts (Acharya and Yorulmazer (2007)). Following an unexpected bailout, the relative attractiveness of these two strategies may change. If banks believe that future rescues are more likely to occur regardless of correlated exposures, the marginal benefit of correlating loan portfolio declines, tilting incentives toward specialization. My unique setting and granular data allow me to test this specialization view.

To test the hypothesis, I construct three distinct loan specialization variables, each measuring the degree to which a bank's loan exposure differs from its peers across borrowers, industries, and geographic regions. The construction follows an intuitive methodology based on the cosine distance between vectors of portfolio shares.

Let $L_{i,k,t}$ represent the loan outstanding from bank i to unit k at time t, where k corresponds to a specific borrower, industry, or geographic region depending on the measure. The share of bank i's total loans allocated to unit k is:

$$S_{i,k,t} = \frac{L_{i,k,t}}{\sum_{k=1}^{K} L_{i,k,t}},$$

where K is the total number of units.

The pairwise *Cosine Distance* between banks i and j in quarter t is calculated as:

$$\text{Cosine Distance}_{i,j,t} = 1 - \frac{\sum_{k=1}^{K} S_{i,k,t}\, S_{j,k,t}}{\sqrt{\sum_{k=1}^{K} S_{i,k,t}^2}\,\sqrt{\sum_{k=1}^{K} S_{j,k,t}^2}}.$$

The *Specialization* of bank i is the average of its cosine distances with all other peer banks in its group (either all Treated banks or all GCB banks):

$$\text{Specialization}_{i,t} = \frac{1}{N_g - 1} \sum_{j \neq i} \text{Cosine Distance}_{i,j,t}, \tag{5}$$

where N_g is the number of banks in bank i's group.

I run Equation 1 on bank-quarter data from Q1 2017 to Q3 2022 with three measures of specialization as dependent variables. I exclude "Yes Bank" from the sample. The results are reported in Table 7. The dependent variables are *Specialization* in columns 1 and 2, *Specialization Industry* in columns 3 and 4, and *Specialization Region* in columns 5 and 6, respectively. *Specialization* is measured using the pairwise cosine distance of each bank's loan exposure across different borrowers with those of other banks. *Specialization Industry* and *Specialization Region* are similarly constructed, based on loan exposures across industries and geographic regions, respectively.

My tightest specification is in columns 2, 4, and 6, which include bank fixed effects, time fixed effects, and bank control variables, whereas columns 1, 3, and 5 do not include bank control variables. The coefficient on $Post_t \times Treat_i$ is positive for all 6 columns and statistically significant for 5 out of 6 columns. The results in column 2 indicate that, relative to GCBs, treated banks increase their borrower-level specialization by 3.3 percentage points following the unexpected bailout. Column 4 shows that treated banks increase industry-level specialization by 4.4 percentage points, while column 6 shows that treated banks increase geographic regional specialization by 7.1 percentage points. Relative to the unconditional standard deviation of specialization measures, in my most conservative estimates affected banks increase specialization at the firm level by around 68%, at the industry level by around 19%, and at the region level by around 32%.

To address any concerns about the possibility that my results represent a continuation of a pre-existing trend, I run the dynamic DID specification Equation 2 with *Specialization* as the dependent variable. Figure 3 shows the trend plot. It measures the specialization difference between private sector banks and GCBs over time. In Figure 3, we see that the specialization difference was stable before the unexpected bailout, indicating the absence of a pre-trend. After the bailout, the loan specialization of affected banks consistently increased relative to that of GCBs.

These findings suggest that affected banks increase loan specialization following the unexpected bailout, and shifted their loan exposures across different borrowers, industries, and geographical regions. The findings align with the specialization channel.

5.2 Spillover effects on systemic risk

Do affected banks contribute more to systemic risk after the unexpected bailout? The risk-shifting channel increases bank risk through greater exposure to risky borrowers (section 4), thereby increasing systemic risk, while the specialization channel reduces portfolio correlation (subsection 5.1), thereby reducing systemic risk. Theoretically, the net spillover effect on systemic risk is ambiguous. To empirically test the overall implication for systemic risk, I run Equation 1 on bank-quarter data from Q1 2017 to Q3 2022 with systemic risk measures as the dependent variable. I exclude "Yes Bank" from the sample. I use three measures of systemic risk, namely, *Marginal Expected Shortfall (MES)*, *Delta Conditional Value-at-Risk (CoVaR)*, and *Beta*.

MES is calculated in spirit of Acharya et al. (2017) by first identifying the market's worst days by taking the day with the lowest daily return in each month. For each bank, the average stock return on those distress days is computed. The negative of this return is reported as *MES*, which captures how much the bank typically loses when the market crashes. *Delta CoVaR* is computed in the spirit of Adrian and Brunnermeier (2016) for each bank-month at the 1st percentile. It is obtained by running quantile regressions of market losses on bank losses at the 1-quantile level, then comparing predicted system losses when the bank is distressed (1-VaR) versus when it is at the median state. Using daily return data within each month, I compute the correlation between bank stock and market returns (proxied by Nifty 500 index) and their volatilities, then combine these to obtain *beta*. This measure captures how sensitive a bank's returns are to market movements during that month.

The results are shown in Table 8. The dependent variables are MES in columns 1 and 2, Delta CoVaR in columns 3 and 4, Beta in columns 5 and 6. Across all six columns, the coefficient on $Post_t \times Treat_i$ is positive and statistically significant at the 1% level. Relative to the unconditional mean values of systemic risk measures, affected banks increase *MES* by around 23%, *Delta CoVaR* by around 30%, and *beta* by around 42%. This indicates that the contribution of treated banks to systemic risk rises significantly following the unexpected bailout.

The findings highlight the presence of competing mechanisms. On one hand, the risk-shifting channel increases systemic risk as banks expand lending to risky borrowers. On the other hand, the specialization channel reduces loan portfolio correlations, which could mitigate systemic fragility. On balance, however, the results show that the risk-shifting effects dominate, leading to a significant increase in systemic risk measures for treated banks relative to GCBs.

6 Other Mechanisms and Alternative Explanations

In this section, I discuss the alternative explanations and other plausible mechanisms that could drive the results.

6.1 Charter value channel

In this section, I examine the presence of the charter value channel. The charter value view argues that an increase in bailout expectations will create a risk-reducing effect by increasing the present value of future bank profits as a going concern (Keeley (1990); Demsetz et al. (1996)). My results do not align with this hypothesis, as the affected banks increase risk. However, the subsample analysis in columns 1 and 2 of Table A.4 suggests that the charter value channel acts as a moderating force, since affected banks with higher charter value (proxied by market-to-book ratio[19]) before the event increase risk less than those with lower charter value.

6.2 Competition channel

Gropp et al. (2011) analyzes spillover effects of bailouts, highlighting the competition channel where bailouts increase risk-taking among competitor banks due to intensified market competition. Unlike the other channels, such as risk-shifting, which affects only the treated banks, the competition channel impacts all banks. My DID methodology controls for the competition channel by capturing the relative increase in risk-taking by the treated banks compared to GCBs. Therefore, the competition channel does not apply in my setting.

[19]While market-to-book ratio was initially used to identify potential risk-shifting in subsection 4.5, it also serves as a well-established proxy for a bank's charter value.

6.3 COVID-19 exposure before pandemic

The unexpected bailout happened in March 2020. The post-bailout period of the bank coincides with COVID-19. The increase in risk for private sector banks relative to GCBs could be due to greater exposure of treated banks to borrowers in industries most affected during COVID-19. If the ex-ante lending portfolio of treated banks had a larger share of industries sensitive to COVID-19, then the main result of increasing risk for treated banks could be driven by the COVID-19 shock rather than by risk-shifting induced by an increased likelihood of a bailout. I test whether my results are driven by more exposure of treated banks to COVID-19-exposed industries.

In 2020, the Kamath Committee identified 26 industries that needed additional support and debt relief due to greater exposure to the COVID-19 shock. The list includes sectors such as Hotels, restaurants, Tourism, Automobiles, Auto components, Aviation, Construction, Consumer durables, and Jewellery.[20] I estimate equation Equation 1 on bank-quarter level data for the period from Q1 2017 to Q4 2019 with $COVID, exposure_{i,t}$ as the dependent variable to test whether treated banks were more exposed to COVID-19-sensitive industries than GCBs before the bailout. The results are shown in columns 1 to 3 of Table 9.

$COVID\, exposure_{i,t}$ represents the proportion of a bank's outstanding loans to COVID-sensitive industries as identified by the Kamath Committee. For instance, if a bank has a total of ₹100 in outstanding loans, and ₹53 of those loans are held by firms in COVID-sensitive industries, "COVID exposure" would take a value of 0.53. The independent variable is $Treat_i$ which takes a value of 1 for private sector banks, and zero otherwise. Since the sample includes only the pre-bailout period, $Post_t$ is zero for all observations and is therefore omitted from the results.

I am interested in the coefficient on *Treat*. If the coefficient is positive and significant, then that would imply that treated banks have a higher share of relationships with bor-

[20] More details in https://www.rbi.org.in/Scripts/PublicationReportDetails.aspx?UrlPage=&ID=1157

rowers belonging to COVID-19-affected industries. In columns 1 to 3 of Table 9, I find no positive association between treated banks and exposure to COVID-19 affected industries pre-pandemic. This means that treated banks do not have more exposure to COVID-19 industries compared with GCBs.[21]

This implies that treated banks do not have a greater proportion of active relationships or outstanding loans to COVID-19 industries before the bailout and COVID-19 pandemic. This suggests that my main results of private sector banks increasing risk are not mechanically driven by treated bank's greater ex-ante exposure to COVID-19 sensitive industries.

6.4 Increase in exposure to COVID-19 industries after bailout

In the above section, we have seen that before the unexpected bailout and COVID-19 pandemic, treated sector banks did not have more exposure to COVID-sensitive industries. However, post-bailout treated banks could increase their exposure to the risky COVID-19 sensitive industries. To test this, I estimate equation Equation 1 on Bank-Quarter level data for the period from Q1 2017 to Q4 2022 with $COVID\, exposure_{i,t}$ as the dependent variable. $COVID\, exposure_{i,t}$ represents the proportion of a bank's outstanding loans to COVID-sensitive industries as identified by the Kamath Committee. The results are shown in columns 4 to 6 of Table 9. The coefficients are insignificant in all three columns, implying that private sector banks do not increase their relationships with COVID-sensitive industries post-bailout compared to GCBs.[22]

[21] I further test the hypothesis using more granular pre-bailout bank-borrower-quarter level data by regressing the log of outstanding loan amounts on the interaction between *Treat* and the *COVID indicator*, which takes a value of one if the borrower belongs to a COVID-affected industry, zero otherwise. The results, presented in columns 1 to 3 of Table A.5, are qualitatively similar, suggesting that private sector banks did not have greater exposure to COVID-affected industries prior to the pandemic.

[22] I also test the hypothesis using more granular bank-borrower-quarter level data by regressing the log of outstanding loan amounts on the triple interaction between *Treat*, *Post*, and the *COVID indicator*, which equals one if the borrower belongs to a COVID-affected industry and zero otherwise. The results, presented in columns 4 to 6 of Table A.5 do not suggest that affected banks increase exposure to COVID-affected industries relative to other industries post the bailout.

6.5 Government bank mergers

In September 2019 (six months prior to the unexpected bailout), the merger of 10 government-controlled banks (GCBs) into 4 banks was announced. Since the pre-merger and post-merger periods coincided with my study period (2017–2022), the relative increase in the risk of private sector banks compared to GCBs could reflect GCBs reducing risk rather than private sector banks increasing risk in response to changed incentives. In prior results, the merged banks get excluded, since they ceased to exist in the post period. However, I still include the acquiring banks. Because the four merged banks operated as standalone entities during most of the pre-merger period and as merged entities during most of the post-merger period, there is a concern that the results are driven by the consolidation of the GCBs.

To address this concern, I re-estimate my main regression specified in equation Equation 1, excluding all 10 GCBs involved in mergers from both the pre- and post-merger periods. The results, shown in Table A.6, indicate coefficients that are positive, statistically significant, and similar to the main results, despite the reduced sample size. This strongly supports the conclusion that the relative increase in the risk of treated banks compared to GCBs is not driven by the series of government bank mergers.

6.6 Too big to fail banks

Three banks in India are explicitly recognized by the RBI, the Central Bank of India, as Domestic Systemically Important Banks (D-SIBs), equivalent to being too-big-to-fail (TBTF). [23] TBTF banks are larger in size than non-TBTF banks. Although I control for bank size using assets and deposits, due to other differences between TBTF and non-TBTF banks such as regulatory scrutiny, there could be a concern about the analysis while comparing relative risk changes for treated banks due to an increase in bailout likelihood.

To address the concern, I re-estimate my main regression specified in equation Equation 1 after excluding all the 3 TBTF banks. The TBTF banks are removed from both pre and

[23] For a detailed explanation on TBTF banks in India see Agrawal et al. (2025).

post-periods. The results are shown in Table A.7. The coefficients of interest are positive, significant, and stronger than the main results despite the reduced sample size. This strongly supports that the relative increase in the risk of treated banks compared to government banks is not driven by the inclusion of TBTF banks.

7 Conclusion

This paper examines the spillover effects of an unexpected bank rescue on the individual risk-taking behavior, specialization, and systemic risk contributions of other private sector banks. Using the bailout of "Yes Bank" in India as a natural experiment, I show that the unprecedented government intervention triggers two opposing forces, namely, individual risk-shifting and loan specialization. I find that affected banks increased their credit risk by more than 30%, as measured by non-performing assets. They expanded lending to risky borrowers, engage in more frequent loan restructuring, and reallocate credit toward financially distressed firms. These results are consistent with the individual risk-shifting channel, in which higher bailout expectations lower the perceived downside of taking on additional risk.

At the same time, using novel measures of loan specialization based on the cosine distance of loan portfolios, I find that affected banks significantly increase their portfolio differentiation. This finding supports the specialization channel. When the likelihood of individual bailouts increases and the incentive for joint bailouts diminishes, banks are encouraged to pursue competitive advantages through more specialized lending strategies. Despite this rise in specialization, which could theoretically reduce systemic risk by reducing portfolio correlations, the net effect is a significant increase in the systemic risk contributions of affected banks.

Overall, the findings show that unexpected bank rescues can trigger unintended spillovers. Even one surprise bailout can generate moral hazard and increase systemic risk by influencing the behavior of other banks. These findings highlight the importance of designing resolution frameworks that recognize the wider incentive implications of targeted interventions.

References

Acharya, Viral V, Lasse H Pedersen, Thomas Philippon, and Matthew Richardson, 2017, Measuring systemic risk, *The review of financial studies* 30, 2–47.

Acharya, Viral V., and Tanju Yorulmazer, 2007, Too many to fail—An analysis of time-inconsistency in bank closure policies, *Journal of Financial Intermediation* 16, 1–31.

Adrian, Tobias, and Markus K Brunnermeier, 2016, Covar, *The American Economic Review* 106, 1705.

Agrawal, Shashwat, Nishant Kashyap, Sriniwas Mahapatro, and Prasanna Tantri, 2025, Does systemically important bank status affect loan performance? .

Allen, Franklin, and Douglas Gale, 2000, Financial contagion, *Journal of political economy* 108, 1–33, Publisher: The University of Chicago Press.

Altermatt, Lukas, Hugo van Buggenum, and Lukas Voellmy, 2024, Systemic bank runs without aggregate risk: how a misallocation of liquidity may trigger a solvency crisis, *Journal of Financial Economics* 161, 103929.

Ashcraft, Adam B, 2005, Are banks really special? New evidence from the FDIC-induced failure of healthy banks, *American Economic Review* 95, 1712–1730, Publisher: American Economic Association.

Behr, Patrick, and Weichao Wang, 2020, The (un)intended effects of government bailouts: The impact of TARP on the interbank market and bank risk-taking, *Journal of Banking & Finance* 116, 105820.

Bernanke, Ben S, 1983, Nonmonetary Effects of the Financial Crisis in the Propagation of the Great Depression., *American Economic Review* 73.

Black, Lamont K., and Lieu N. Hazelwood, 2013, The effect of TARP on bank risk-taking, *Journal of Financial Stability* 9, 790–803.

Blickle, Kristian, Cecilia Parlatore, and Anthony Saunders, 2023, Specialization in banking, Working Paper 31077, National Bureau of Economic Research.

Brownlees, Christian, and Robert F Engle, 2017, Srisk: A conditional capital shortfall measure of systemic risk, *The Review of Financial Studies* 30, 48–79.

Caballero, Ricardo J, Takeo Hoshi, and Anil K Kashyap, 2008, Zombie lending and depressed restructuring in japan, *American economic review* 98, 1943–1977.

Calderon, Cesar, and Klaus Schaeck, 2016, The Effects of Government Interventions in the Financial Sector on Banking Competition and the Evolution of Zombie Banks, *Journal of Financial and Quantitative Analysis* 51, 1391–1436.

Cardillo, Giovanni, Franco Fiordelisi, and Ornella Ricci, 2023, Bank Bailouts and Competitive Distortions, *Available at SSRN 3912465* .

Chari, Varadarajan V, and Patrick J Kehoe, 2016, Bailouts, time inconsistency, and optimal regulation: A macroeconomic view, *American Economic Review* 106, 2458–2493.

Chopra, Yakshup, Krishnamurthy Subramanian, and Prasanna L Tantri, 2021, Bank cleanups, capitalization, and lending: Evidence from india, *The Review of Financial Studies* 34, 4132–4176, Publisher: Oxford University Press.

Cole, Shawn, 2009, Fixing market failures or fixing elections? agricultural credit in india, *American Economic Journal: Applied Economics* 1, 219–50.

Dam, Lammertjan, and Michael Koetter, 2012, Bank Bailouts and Moral Hazard: Evidence from Germany, *The Review of Financial Studies* 25, 2343–2380.

Demsetz, Rebecca S., Marc R. Saidenberg, and Philip E. Strahan, 1996, Banks with Something to Lose: The Disciplinary Role of Franchise Value.

Duchin, Ran, and Denis Sosyura, 2014, Safer ratios, riskier portfolios: Banks' response to government aid, *Journal of Financial Economics* 113, 1–28.

Farhi, Emmanuel, and Jean Tirole, 2012, Collective moral hazard, maturity mismatch, and systemic bailouts, *American Economic Review* 102, 60–93, Publisher: American Economic Association.

Flannery, Mark J, 1998, Using market information in prudential bank supervision: A review of the US empirical evidence, *Journal of money, credit and banking* 273–305, Publisher: JSTOR.

Furfine, Craig, 2006, The Costs and Benefits of Moral Suasion: Evidence from the Rescue of Long-Term Capital Management, *The Journal of Business* 79, 593–622, Publisher: JSTOR.

Gandhi, Priyank, and Amiyatosh Purnanandam, 2021, United they fall: Bank risk after the financial crisis, *Available at SSRN 4091626* .

Goldsmith-Pinkham, Paul, and Tanju Yorulmazer, 2010, Liquidity, Bank Runs, and Bailouts: Spillover Effects During the Northern Rock Episode, *Journal of Financial Services Research* 37, 83–98.

Goldstein, Itay, Alexandr Kopytov, Lin Shen, and Haotian Xiang, 2024, Bank heterogeneity and financial stability, *Journal of Financial Economics* 162, 103934.

Gopalan, Radhakrishnan, Abhiroop Mukherjee, and Manpreet Singh, 2016, Do Debt Contract Enforcement Costs Affect Financing and Asset Structure?, *The Review of Financial Studies* 29, 2774–2813, _eprint: https://academic.oup.com/rfs/article-pdf/29/10/2774/24438705/internet_appendix_rfs.pdf.

Gropp, Reint, Hendrik Hakenes, and Isabel Schnabel, 2011, Competition, Risk-shifting, and Public Bail-out Policies, *The Review of Financial Studies* 24, 2084–2120.

Hakenes, Hendrik, and Isabel Schnabel, 2010, Banks without parachutes: Competitive effects of government bail-out policies, *Journal of Financial Stability* 6, 156–168.

Hovakimian, Armen, and Edward J Kane, 2000, Effectiveness of capital regulation at US commercial banks, 1985 to 1994, *the Journal of Finance* 55, 451–468, Publisher: Wiley Online Library.

Hryckiewicz, Aneta, 2014, What do we know about the impact of government interventions in the banking sector? An assessment of various bailout programs on bank behavior, *Journal of Banking & Finance* 46, 246–265.

Huang, Xin, Hao Zhou, and Haibin Zhu, 2009, A framework for assessing the systemic risk of major financial institutions, *Journal of Banking & Finance* 33, 2036–2049.

Iyer, Rajkamal, and Manju Puri, 2012, Understanding bank runs: The importance of depositor-bank relationships and networks, *American Economic Review* 102, 1414–45.

Jasova, Martina, Luc Laeven, Caterina Mendicino, José-Luis Peydró, and Dominik Supera, 2024, Systemic risk and monetary policy: The haircut gap channel of the lender of last resort, *The Review of Financial Studies* 37, 2191–2243.

Jensen, Michael C., and William H. Meckling, 1976, Theory of the firm: Managerial behavior, agency costs and ownership structure, *Journal of Financial Economics* 3, 305–360.

Kalimipalli, Madhu, Olaleye Morohunfolu, and Shankar Ramachandran, 2024, Do repeated government infusions help financial stability? Evidence from an emerging market, *Journal of Financial Stability* 75, 101334.

Keeley, Michael C, 1990, Deposit insurance, risk, and market power in banking, *The American economic review* 1183–1200, Publisher: JSTOR.

Kulkarni, Nirupama, SK Ritadhi, Siddharth Vij, and Katherine Waldock, 2025, Unearthing zombies, *Management Science* .

Kumar, Nitish, 2020, Political interference and crowding out in bank lending, *Journal of Financial Intermediation* 43, 100815.

Liu, Xuewen, 2023, A model of systemic bank runs, *The Journal of Finance* 78, 731–793.

Marisetty, Vijaya B., and Md Shoeb, 2024, Capital infusions and Bank risk-taking behaviour, *Pacific-Basin Finance Journal* 88, 102539.

Merton, Robert C, 1974, On the pricing of corporate debt: The risk structure of interest rates, *The Journal of finance* 29, 449–470, Publisher: JSTOR.

Merton, Robert C, 1977, An analytic derivation of the cost of deposit insurance and loan guarantees an application of modern option pricing theory, *Journal of banking & finance* 1, 3–11, Publisher: Elsevier.

Mishra, Prachi, Nagpurnanand Prabhala, and Raghuram G Rajan, 2021, The relationship dilemma: Why do banks differ in the pace at which they adopt new technology?, *The Review of Financial Studies* 35, 3418–3466.

Paravisini, Daniel, Veronica Rappoport, and Philipp Schnabl, 2023, Specialization in Bank Lending: Evidence from Exporting Firms, *The Journal of Finance* 78, 2049–2085, Publisher: John Wiley & Sons, Ltd.

Peek, Joe, and Eric S. Rosengren, 2000, Collateral Damage: Effects of the Japanese Bank Crisis on Real Activity in the United States, *American Economic Review* 90, 30–45.

Poczter, Sharon, 2016, The long-term effects of bank recapitalization: Evidence from Indonesia, *Journal of Financial Intermediation* 25, 131–153.

Sironi, Andrea, 2003, Testing for market discipline in the European banking industry: evidence from subordinated debt issues, *Journal of Money, Credit and Banking* 443–472, Publisher: JSTOR.

Strahan, Philip E., 2013, Too Big to Fail: Causes, Consequences, and Policy Responses, *Annual Review of Financial Economics* 5, 43–61, Publisher: Annual Reviews.

Vig, Vikrant, 2013, Access to collateral and corporate debt structure: Evidence from a natural experiment, *The Journal of Finance* 68, 881–928.

Figure 1: Stock price of "Yes bank" around bailout benchmarked with total market capitalization of all banks

This figure illustrates the daily closing stock price of "Yes bank" around its moratorium and bailout announcements, benchmarked against the aggregate market capitalization of all banks. The Yes Bank stock price is depicted in *blue*, while the market capitalization of banks is shown in *orange*. The x-axis indicates days relative to the bailout event. The primary y-axis on the left represents Yes Bank's closing stock price and the secondary y-axis on the right reflects the market capitalization of banks, calculated from their respective closing prices.

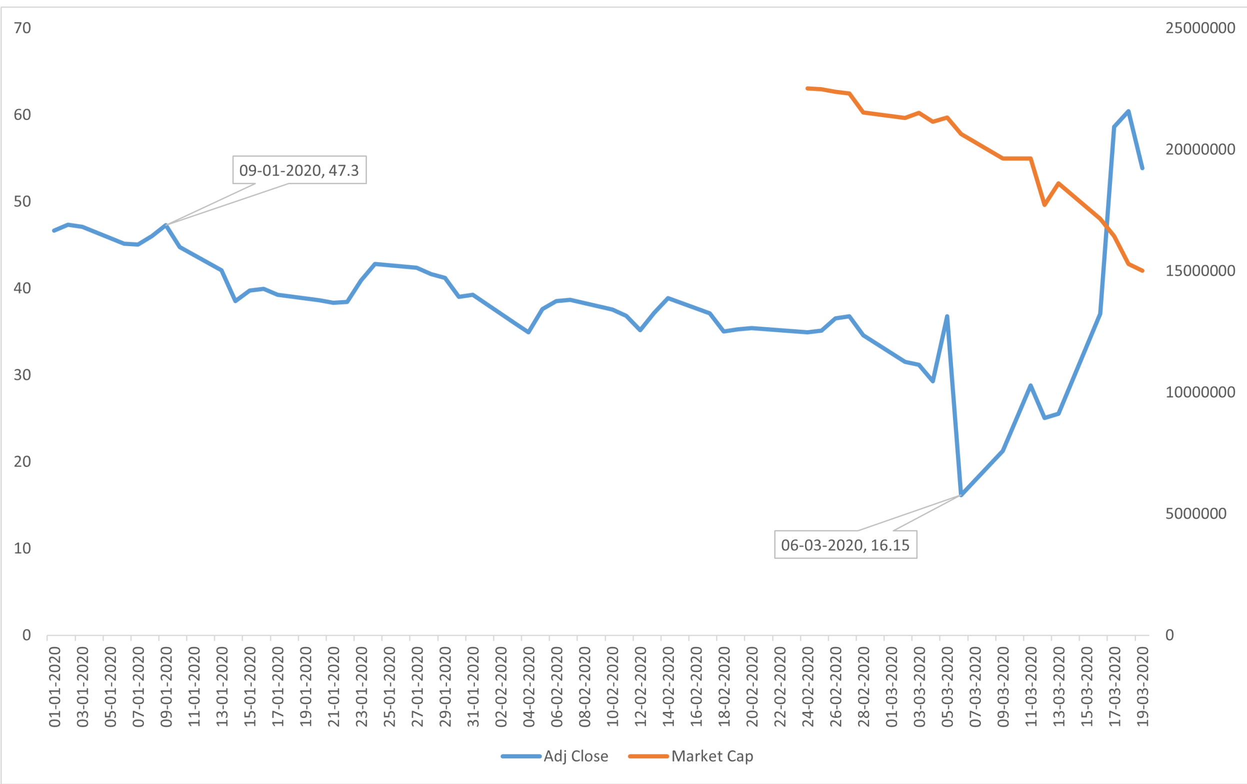

Figure 2: Pretrend and post trend for NPA – Quarterly frequency

The figure plots the coefficients of the dynamic version of the difference-in-differences design of the main regression. The sample is at the bank-quarter level from Q4 2018 to Q1 2022. The dependent variable is non performing asset ratio (NPA). The explanatory variables pre2, pre3, pre4, and pre5 are one for 2, 3, 4, and 5 quarters before the unexpected rescue of Yes bank, and zero otherwise. post0, post1, post2, post3, post5, post6, post7, and post8 are one for current quarter, 1, 2, 3, 4, 5, 6, 7, and 8 quarters after the bank bailout, zero otherwise. The dots represent the point estimates of the coefficient, while the span of the lines represents a 95% confidence interval. Bank and quarter fixed effects are included along with control variables log lagged asset, log lagged deposit, lagged capital ratio, and lagged ROA. The standard errors are clustered at the bank level.

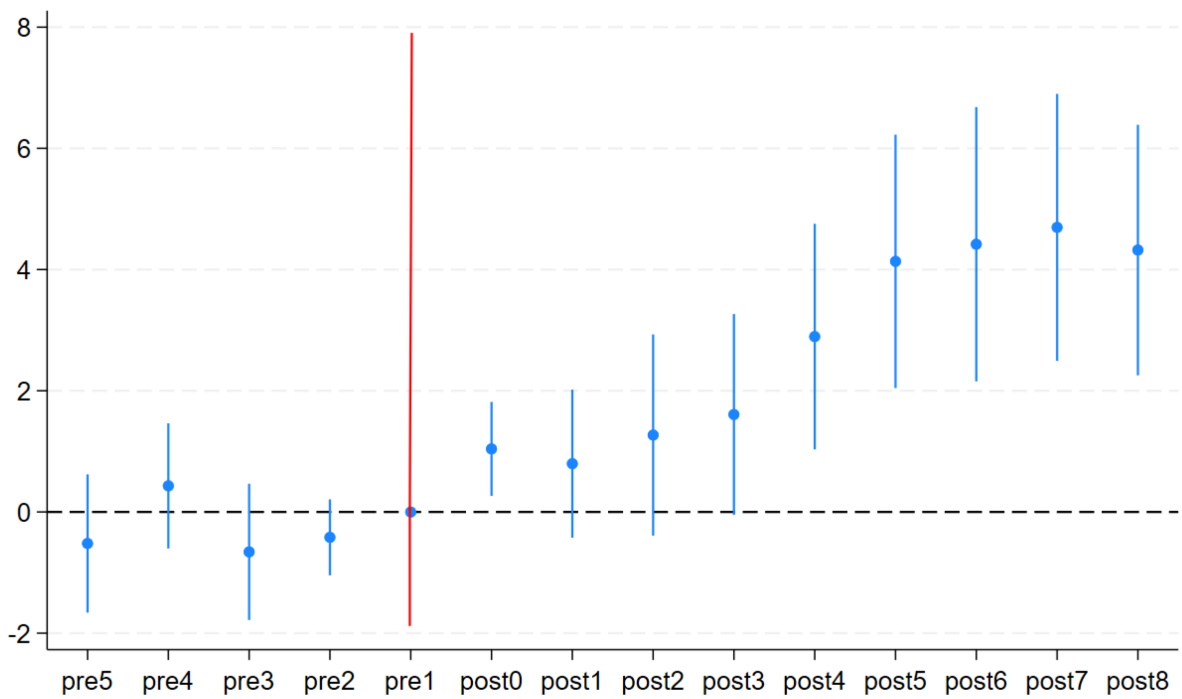

Figure 3: Pretrend and post trend for Specialization – Quarterly frequency

The figure plots the coefficients of the dynamic version of the difference-in-differences design for the loan specialization of banks. The sample is at the bank-quarter level from Q4 2018 to Q1 2022. The dependent variable is *Specialization*. It is the pairwise cosine distance of loan exposure of a bank across borrowers with those of other banks. The measure is computed for all pairs of banks, separately for treated banks and GCBs. Therefore, each treated bank is paired only with another treated bank, and each GCB is paired only with another GCB. The pairwise cosine distance of loan portfolios are averaged at the bank-quarter level. The explanatory variables pre2, pre3, pre4, and pre5 are one for 2, 3, 4, and 5 quarters before the unexpected rescue of Yes bank, and zero otherwise. post0, post1, post2, post3, post5, post6, post7, and post8 are one for current quarter, 1, 2, 3, 4, 5, 6, 7, and 8 quarters after the bank bailout, zero otherwise. The dots represent the point estimates of the coefficient, while the span of the lines represents a 95% confidence interval. Bank and quarter fixed effects are included along with control variables log lagged asset, log lagged deposit, lagged capital ratio, and lagged ROA. The standard errors are clustered at the bank level.

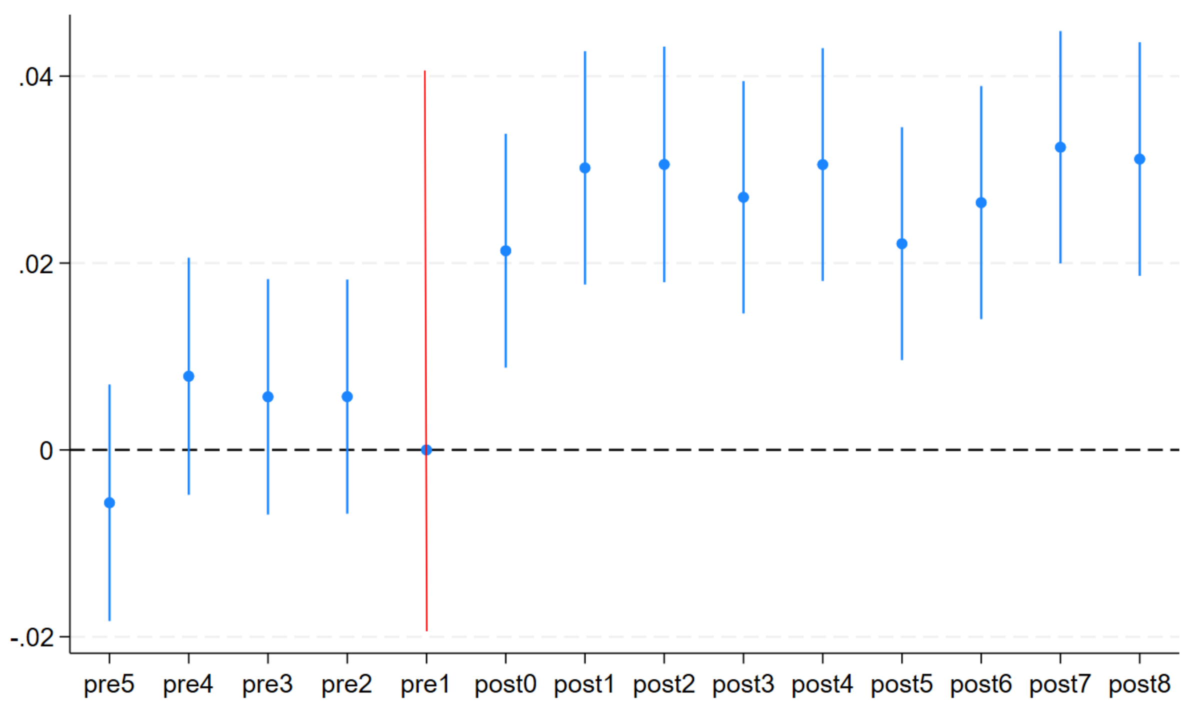

Figure 4: Share of New Loans by Private Sector Banks and GCBs Before and After the Unexpected Bailout

This figure, using two pie charts, illustrates the share of new secured corporate loans issued by government controlled banks (GCBs) and private sector banks before and after the unexpected bailout. The sample excludes "Yes Bank." The share is calculated relative to the total new corporate loans issued by scheduled commercial banks, both GCBs and private sector banks. The 'before' period covers Q1 2017 to Q4 2019, while the 'after' period covers Q1 2020 to Q3 2022. The share of GCBs is depicted in *blue*, while the share of private sector banks is shown in *orange.* The pie chart on the left indicates the share of new loan issuance before the unexpected rescue. The pie chart on the right indicates the share of new loan issuance after the unexpected rescue.

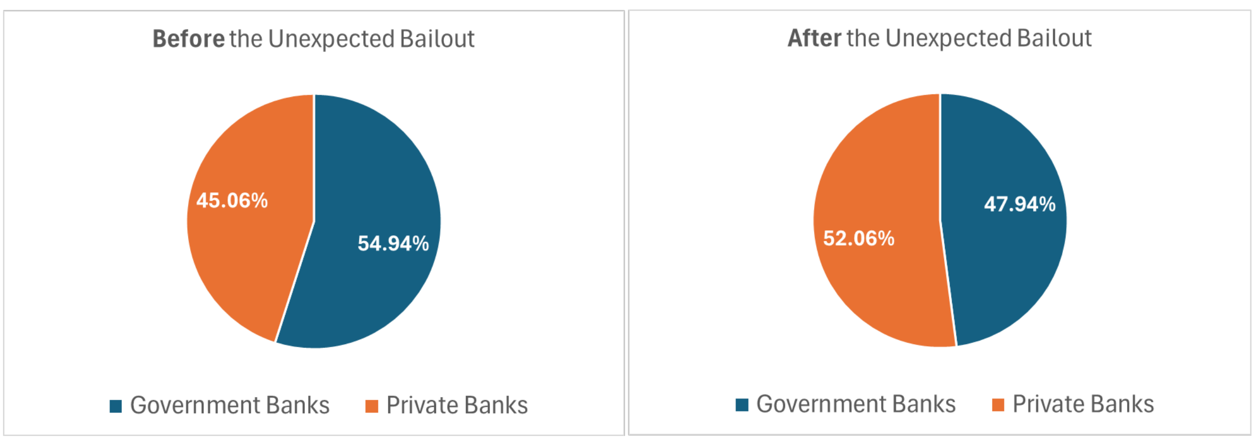

Table 1: Credit rating changes of "Yes bank" securities before and after the event

The table reports the credit ratings of "Yes bank" securities surrounding the unexpected bailout announcement on March 13, 2020, by India's major credit rating agencies, namely, CARE, Brickwork, ICRA, and CRISIL. The data is sourced from Prowess CMIE.

Rating Agency	**Date**	**Security Type**	**Rating**	**Rating Definition**
Before Rescue of Yes Bank				
CARE	6th March 2020	Debentures / Bonds / notes / bills	D	Default
BRICKWORK	6th March 2020	Debentures / Bonds / notes / bills	D	Default
ICRA	6th March 2020	Debentures / Bonds / notes / bills	D	Default
After Rescue of Yes Bank				
CRISIL	19th March 2020	Certificate of deposit	A 2	High Safety
ICRA	24th March 2020	Debentures / Bonds / notes / bills	BB+	Inadequate Safety

Table 2: Descriptive Statistics

In this table, we report the descriptive statistics relating to the main variables used in the paper. The sample period is from 2017 to 2022. The variables have been defined in Table A.1. I exclude "Yes Bank" from the sample.

	Obs	Mean	Median	StDev	P25	P75	Min	Max
Bank-Quarter data								
NPA	714	8.15	6.66	6.11	3.22	11.16	0.51	24.72
Treat	714	0.53	1	0.5	0	1	0	1
Post	714	0.5	1	0.5	0	1	0	1
Risky New Loan Exposure	611	0.101	0.004	0.202	0	0.102	0	0.999
Loan Restructuring	713	0.019	0.015	0.015	0.009	0.024	0	0.071
Risky Loan Exposure	713	0.263	0.239	0.173	0.154	0.333	0	0.989
Specialization	713	0.922	0.92	0.038	0.896	0.951	0.822	0.992
Specialization Industry	713	0.422	0.387	0.137	0.336	0.447	0.271	0.853
Specialization Region	713	0.433	0.441	0.165	0.272	0.540	0.221	0.873
COVID exposure	713	0.396	0.381	0.128	0.324	0.456	0.083	0.762
ln_assets	704	14.57	14.64	1.3	13.66	15.65	11.68	17.69
ln_deposits	696	14.36	14.47	1.29	13.45	15.45	11.32	17.47
capital_lag	707	14.31	13.74	3.3	11.88	15.99	9.16	25.14
roa_lag	708	0.13	0.33	1.1	0.09	0.66	-3.69	2
Bank-Borrower-Quarter loan data								
Loan Outstanding	873218	18.82	19.114	2.321	17.342	20.422	13.122	23.9
Private	873218	0.446	0	0.497	0	1	0	1
Post	873218	0.505	1	0.5	0	1	0	1
New Loan	873218	0.049	0	0.216	0	0	0	1
COVID	832979	0.46	0	0.498	0	1	0	1
Risky Borrower-PBIT <0	170970	0.195	0	0.396	0	0	0	1
Risky Borrower-PAT <0	170970	0.283	0	0.45	0	1	0	1
Risky Borrower-ICR <1	158004	0.241	0	0.428	0	0	0	1
Bank-Month data								
MSE	2114	0.03	0.02	0.03	0.01	0.04	-0.04	0.12
Delta CoVaR	2114	0.01	0.01	0.01	0	0.01	-0.01	0.07
Beta	2114	1.3	1.23	0.79	0.77	1.75	-0.7	3.8

Table 3: Spillover effect of unexpected bailout on credit risk: Bank level data

The table shows the association between credit risk and private sector banks relative to control banks before and after the unexpected rescue of "Yes Bank". The coefficients are of the difference-in-differences (DID) design as specified in Equation 1. The sample is at the bank-quarter level from Q1 2017 to Q4 2022. I exclude "Yes Bank" from the sample. The dependent variable is the non-performing asset ratio (NPA), a measure of credit risk. NPA are loans with no payments for 90 days or more, measured as a percentage of total advances. The explanatory variable is *Treat*, which takes the value of one for private sector banks, and zero for government-controlled banks. *Post* is one for all year-quarters greater or equal to the unexpected rescue period, and zero otherwise. Year-quarter fixed effects are included from columns 3 to 6. Bank fixed effects are included in columns 5 and 6. Control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			NPA			
Treat	-11.357***	-9.224***	-11.358***	-9.131***		
	(0.408)	(0.500)	(1.591)	(1.547)		
Post	-4.274***	-2.920***				
	(0.419)	(0.393)				
Treat × Post	4.588***	3.513***	4.589***	3.394***	4.602***	3.668***
	(0.575)	(0.533)	(1.284)	(1.132)	(1.281)	(0.976)
Observations	714	694	714	694	714	694
R-squared	0.607	0.691	0.624	0.706	0.887	0.906
Bank Controls	No	Yes	No	Yes	No	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No	Yes	Yes
Quarter FE	No	No	Yes	Yes	Yes	Yes

Table 4: Spillover effects of the unexpected bailout on loan portfolio risk and loan restructuring rate: Bank level data

The table shows the association between loan portfolio risk and the loan restructuring rate of treated banks relative to control banks, before and after the unexpected rescue. The sample is at the bank-quarter level from Q1 2017 to Q3 2022. I exclude "Yes Bank" from the sample. The dependent variable in columns 1 and 2 is *Risky New Loan Exposure*, which is the share of new lending by banks to risky firms. It is measured as the new loan amount–weighted share of new loans granted to risky borrowers, where risky is defined as borrowers with negative profits (PAT <0). This captures the proportion of a bank's new credit exposures directed to financially distressed firms in a quarter. The dependent variable in columns 3 and 4 is *Loan Restructuring*. It is the share of a bank's outstanding loans that were restructured in a quarter. The dependent variable in columns 5 and 6 is *Risky Loan Exposure*, which is the share of a bank's loan portfolio exposed to risky borrowers. It is measured as the loan-weighted share of outstanding exposures to risky borrowers, where risky is defined as firms with negative profits (PAT <0). This measure captures the proportion of a bank's loan book that is exposed to financially distressed borrowers in a quarter. The explanatory variable, *Treat*, takes the value of one for private sector banks and zero for government-controlled banks. *Post* is one for all quarters greater than or equal to the quarter of the unexpected rescue and zero otherwise. Bank fixed effects and year-quarter fixed effects are included in all columns. The control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Risky New Loan Exposure		Loan Restructuring		Risky Loan Exposure	
Treat × Post	0.102***	0.084***	0.007**	0.005**	0.056**	0.045*
	(0.029)	(0.027)	(0.002)	(0.002)	(0.026)	(0.026)
Observations	611	590	713	664	713	664
R-squared	0.109	0.116	0.659	0.691	0.887	0.755
Bank Controls	No	Yes	No	Yes	No	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: New loans to risky borrowers: Loan level data

The table presents the relationship between new loans issued and the interaction of borrower financial health and private banks relative to control banks, both before and after the unexpected rescue of Yes Bank. The sample is structured at the bank–borrower–quarter level, covering the period from Q1 2017 to Q3 2022. I exclude "Yes Bank" from the sample. The sample is restricted to public firms, as risk data are only available for these firms. The dependent variable is *New Loan*, which is an indicator variable that takes a value of one if bank b gives a fresh loan to borrower f in quarter t, and zero otherwise. The key explanatory variables are *Treat*, *Post*, and *Risky Borrower*. *Treat* equals one for privately owned banks and zero for government-owned banks. *Post* equals one for all quarters on or after the quarter of Yes Bank's unexpected rescue and zero otherwise. *Risky Borrower* is measured using different indicators: Profit After Tax (PAT) in columns 1 and 2, Profit Before Interest and Taxes (PBIT) in columns 3 and 4, and the Interest Coverage Ratio (ICR) in columns 5 and 6. In columns 1 and 2, *Risky Borrower* equals one for all firm–quarters with PAT less than zero. In columns 3 and 4, *Risky Borrower* equals one for all firm–quarters with PBIT less than zero. In columns 5 and 6, *Risky Borrower* equals one for all firm–quarters with an ICR below one. A *Risky Borrower* value of one indicates that the borrower is risky. All columns include bank, year–quarter, and industry–year–quarter fixed effects. Columns 2, 4, and 6 also include borrower–year–quarter fixed effects. Bank control variables are the one-year-lagged values of assets, deposits, and capital. The standard errors reported in parentheses are robust and clustered at the bank–quarter level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable			New Loan			
Risky Borrower Indicator=1, if	PAT <0		PBIT <0		ICR <1	
Treat × Post	-0.032***	-0.037***	-0.030***	-0.034***	-0.032***	-0.037***
	(0.005)	(0.006)	(0.005)	(0.006)	(0.005)	(0.006)
Risky Borrower	-0.024***		-0.023***		-0.025***	
	(0.002)		(0.002)		(0.002)	
Treat × Risky Borrower	-0.039***	-0.042***	-0.037***	-0.038***	-0.034***	-0.041***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Post × Risky Borrower	-0.013***		-0.017***		-0.007*	
	(0.003)		(0.003)		(0.004)	
Treat × Post × Risky Borrower	0.037***	0.036***	0.040***	0.036***	0.035***	0.037***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)
Observations	166,689	138,215	166,689	138,215	153,947	130,016
R-squared	0.087	0.336	0.086	0.336	0.088	0.333
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower-Year-Quarter FE	No	Yes	No	Yes	No	Yes

Table 6: Analysis of outstanding loans and loan exposure to risky borrowers: Loan level data

The table presents the relationship between outstanding loan amounts and exposure to risky borrowers at private banks, relative to control banks, both before and after the unexpected rescue. The sample is structured at the bank–borrower–quarter level, covering the period from Q1 2017 to Q3 2022. I exclude "Yes Bank" from the sample. Columns 1 to 3 report results for all borrowers. Columns 4 to 6 report results for the subsample of risky borrowers. A borrower is identified as risky if its Profit Before Interest and Taxes (PBIT) is less than zero, or Profit After Tax (PAT) is less than zero, or Interest Coverage Ratio (ICR) is less than one. The dependent variable is the logarithm of the total loan outstanding for a borrower with a given bank in a quarter. The key explanatory variables are *Treat* and *Post*. *Treat* equals one for private sector banks and zero for government-controlled banks. *Post* equals one for all quarters on or after the quarter of the unexpected rescue and zero otherwise. All columns include bank and quarter fixed effects. Industry–year–quarter fixed effects are included in all columns except 1 and 4. Columns 3 and 6 also include borrower–year–quarter fixed effects. Bank control variables are included in all columns. The standard errors reported in parentheses are robust and clustered at the bank–quarter level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Loan Outstanding)					
	All Borrowers			Subsample of Risky Borrowers		
Treat × Post	0.025*	0.026*	0.052*	0.089**	0.070*	0.201***
	(0.014)	(0.014)	(0.028)	(0.036)	(0.040)	(0.055)
Observations	861,412	820,076	577,549	40,898	39,790	34,857
R-squared	0.053	0.149	0.621	0.067	0.301	0.577
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	No	No	Yes	No	No	Yes
Industry FE	No	Yes	Yes	No	Yes	Yes
Industry-Year-Quarter FE	No	Yes	Yes	No	Yes	Yes
Borrower-Year-Quarter FE	No	No	Yes	No	No	Yes

Table 7: Spillover effects of unexpected bailout on specialization

The table examines the spillover effects of unexpected bailout on specialization of other banks. The sample is at the bank-quarter level, covering the period from Q1 2017 to Q4 2022. I exclude "Yes Bank" from the sample. The dependent variables are *Specialization* in columns 1 and 2, *Specialization Industry* in columns 3 and 4, and *Specialization Region* in columns 5 and 6. *Specialization* is based on the pairwise cosine distance of loan exposure of a bank across different borrowers with those of other banks. *Specialization Industry* is based on the pairwise cosine distance of loan exposure of a bank across different industries with those of other banks. *Specialization Region* is based on the pairwise cosine distance of loan exposure of a bank across different regions with those of other banks. The measures are computed for all pairs of banks, separately for treated banks and GCBs. Therefore, each treated bank is paired only with another treated bank, and each GCB is paired only with another GCB. The pairwise cosine distance of loan portfolios are averaged at the bank-quarter level. The explanatory variable is *Treat*, which takes the value of one for private sector banks and zero for government-controlled banks. *Post* is equal to 1 for all quarters on or after the quarter of the unexpected rescue, and 0 otherwise. All columns include bank fixed effects and year-quarter fixed effects. The control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Specialization		Specialization Industry		Specialization Region	
Treat × Post	0.026**	0.033***	0.026	0.044*	0.053***	0.071***
	(0.010)	(0.010)	(0.019)	(0.022)	(0.018)	(0.017)
Observations	713	664	713	664	713	664
R-squared	0.832	0.847	0.937	0.928	0.963	0.967
Bank Controls	No	Yes	No	Yes	No	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 8: Spillover effect of unexpected bailout on systemic risk: Bank level data

The table shows the association between systemic risk measures for private banks relative to control banks, before and after the unexpected rescue of Yes Bank. The coefficients come from the difference-in-differences design of the main regression. The sample is at the bank-month level from Q1 2017 to Q4 2022. The dependent variable in columns 1 and 2 is Marginal Expected Shortfall (MES). *MES* is calculated by first identifying the market's worst days by taking the day with the lowest daily return in each month. Then, for each bank, the average stock return on those distress days is computed. The negative of this return is reported as *MES*, which shows how much the bank typically loses when the market crashes. The dependent variable in columns 3 and 4 is monthly *Delta CoVaR* (Delta Conditional Value-at-Risk) for each bank at the 1st percentile, computed in spirit of Adrian and Brunnermeier (2016). The dependent variable in columns 5 and 6 is *beta*. Monthly *beta* is estimated for each bank. Using daily return data within each month, I compute the correlation between stock and market returns (proxied by Nifty 500 index) and their volatilities, then combine these to obtain *beta*. The *beta* measure captures how sensitive a bank's returns are to market movements during that month. The explanatory variable is *Treat*, which takes the value of one for private sector banks and zero for government-controlled banks. *Post* is one for all month–year observations greater than or equal to the unexpected rescue period, and zero otherwise. Bank and month–year fixed effects are included in all columns. The control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	MSE		Delta CoVaR		Beta	
Treat × Post	0.007***	0.008***	0.003***	0.003***	0.541***	0.572***
	(0.002)	(0.003)	(0.001)	(0.001)	(0.119)	(0.124)
Observations	2,114	2,079	2,114	2,079	2,114	2,079
R-squared	0.519	0.524	0.673	0.673	0.354	0.361
Bank Controls	No	Yes	No	Yes	No	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: COVID exposure before the pandemic and post bailout: Bank level data

The table analyzes the association between COVID industry exposure and private banks relative to control banks, both before the pandemic and the unexpected rescue of Yes Bank, as well as post-bailout. The sample is at the bank-quarter level. Columns 1 to 3 cover the pre-pandemic period from Q1 2017 to Q4 2019, while columns 4 to 6 include data for the full sample period from Q1 2017 to Q3 2022. The dependent variable is *COVID Exposure*, which represents the proportion of a bank's outstanding loans to COVID-sensitive industries as identified by the Kamath Committee. For instance, if a bank has a total of ₹100 in outstanding loans, and ₹53 of those loans are held by firms in COVID-sensitive industries, "COVID exposure" would take a value of 0.53. The key explanatory variable *Treat* equals one for private sector banks and zero for GCBs. The variable *Post* is set to one for all quarters on or after the quarter of the unexpected rescue and zero otherwise. Bank control variables include one-year lagged values of assets, deposits, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			COVID Exposure			
Private	-0.060	-0.060	-0.010	-0.060		
	(0.046)	(0.046)	(0.052)	(0.046)		
Private × Post				0.033	0.033	0.032
				(0.026)	(0.026)	(0.033)
Observations	372	372	337	713	713	664
R-squared	0.050	0.051	0.315	0.046	0.909	0.884
Bank Controls	No	No	Yes	No	No	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No	Yes	Yes
Quarter FE	No	Yes	Yes	No	Yes	Yes

Appendices

Table A.1: Variables Definition

In this table, we present the description of all variables used in the study.

Variables	Definition
NPA	NPA stands for non-performing assets ratio of the bank in the quarter.
Return Volatility	Monthly stock return volatility of Banks
Treat	A variable that takes value of one for all private sector banks except the rescued bank, zero otherwise.
Post	A variable that identifies if the period is after the Yes Bank bailout, zero otherwise
Risky New Loan Exposure	It is the share of new lending by banks to risky firms. It is measured as the new loan amount–weighted share of new loans granted to risky borrowers, where risky is defined as borrowers with negative profits (Profit After Taxes (PAT) <0). This captures the proportion of a bank's new credit exposures directed to financially distressed firms in a quarter.
Loan Restructuring	It is the share of a bank's outstanding loans that were restructured in a quarter. Loan restructuring involves modifying loan terms.
Risky Loan Exposure	It is the share of a bank's loan portfolio exposed to risky borrowers. It is measured as the loan-weighted share of outstanding exposures to risky borrowers, where risky is defined as firms with negative profits (PAT <0). This measure captures the proportion of a bank's loan book that is exposed to financially distressed borrowers in a quarter.
Capital Ratio	Ratio of equity capital to total risk weighted assets of the bank in the quarter reported as per Basel guidelines.
ROA	Stands for return on assets ratio reported by the bank in the quarter.
Assets	Log of value of total assets of the bank in the quarter.
Deposits	Log of value of total deposits of the bank in the quarter.
Loan Outstanding	Logarithm of the total loan outstanding for a firm with a given bank in a quarter.
New Loan	It is an indicator variable that takes a value of one if bank \$i\$ gives a fresh loan to borrower \$f\$ in quarter \$t\$, and zero otherwise.
Risky Borrower - PBIT <0	Risky borrower is set to one for all firm-quarters with Profit Before Interest and Taxes (PBIT) less than zero, zero otherwise
Risky Borrower - PAT <0	Risky borrower is set to one for all firm-quarters with Profit After Taxes (PAT) less than zero, zero otherwise
Risky Borrower - ICR <1	Risky borrower is set to one for all firm-quarters with Interest Coverage Ratio (ICR) less than one, zero otherwise
MSE	It is calculated by first identifying the market's worst days by taking the day with the lowest daily return in each month. Then, for each bank, the average stock return on those distress days is computed. The negative of this return is reported as *MES*, which shows how much the bank typically loses when the market crashes.
Delta CoVaR	Delta Conditional Value-at-Risk for each bank-month at the 1st percentile, computed in the spirit of Adrian and Brunnermeier (2016). It is computed by running quantile regressions of market losses on bank losses at the 1-quantile level, then comparing predicted system losses when the bank is distressed (1-VaR) versus when it is normal (median).
Specialization	It is based on the pairwise cosine distance of loan exposure of a bank across different borrowers with those of other banks. The measure is computed for all pairs of banks, separately for treated banks and GCBs. Therefore, each treated bank is paired only with another treated bank, and each GCB is paired only with another GCB. The pairwise cosine distance of loan portfolios are averaged at the bank-quarter level.
Specialization Industry	It is based on the pairwise cosine distance of loan exposure of a bank across different industries with those of other banks. The measure is computed for all pairs of banks, separately for treated banks and GCBs. Therefore, each treated bank is paired only with another treated bank, and each GCB is paired only with another GCB. The pairwise cosine distance of loan portfolios are averaged at the bank-quarter level.
Specialization Region	It is based on the pairwise cosine distance of loan exposure of a bank across different regions with those of other banks. The measure is computed for all pairs of banks, separately for treated banks and GCBs. Therefore, each treated bank is paired only with another treated bank, and each GCB is paired only with another GCB. The pairwise cosine distance of loan portfolios are averaged at the bank-quarter level.
Beta	Using daily return data within each month, I compute the correlation between bank stock and market returns (proxied by Nifty 500 index) and their volatilities, then combine these to obtain *beta*. The measure captures how sensitive a bank's returns are to market movements during that month.
COVID Exposure	It represents the proportion of a bank's outstanding loans to COVID-sensitive industries as identified by the Kamath Committee. For instance, if a bank has a total of ₹100 in outstanding loans, and ₹53 of those loans are held by firms in COVID-sensitive industries, "COVID exposure" would take a value of 0.53.
COVID	It is set to one for firms in COVID-sensitive industries as identified by the Kamath Committee and zero otherwise.

Table A.2: Sample Construction

In this table, we report details about the sample used. The sample excludes "Yes bank", the private bank that was unexpectedly bailed out. The terms have been defined in Table A.1.

Particulars	Count
Bank-Quarter level	
Sample Period	Q1 2017 to Q4 2022
Number of Quarters	24
Number of Bank-Quarter level Observations	714
Number of Private Bank-Quarter level Observations	378
Number of Government Bank-Quarter level Observations	336
Number of Distinct Banks	30
Number of Distinct Private Sector Banks	16
Number of Distinct Government Banks	14
Number of Distinct TBTF Banks	3
Number of Distinct Private Sector TBTF Banks	2

Table A.3: Spillover effect of unexpected bailout on monthly return volatility as measure of risk

The table shows the association between bank risk and private sector banks relative to control banks before and after the unexpected rescue of "Yes Bank". The coefficients are of the difference-in-differences (DID) design as specified in Equation 1. The sample is at the bank-year-month level from January 2017 to December 2022. The dependent variable is the monthly stock return volatility. The explanatory variable is *Treat*, which takes the value of one for private sector banks, and zero for GCBs. Post is one for all year-months greater or equal to the unexpected rescue period for Yes Bank, and zero otherwise. Year-month fixed effects are included from columns 3 to 6. Bank fixed effects are included in columns 5 and 6. Control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Monthly Stock Return Volatility					
Treat	-0.006***	-0.006***	-0.006***	-0.008***		
	(0.001)	(0.001)	(0.001)	(0.002)		
Post	0.004***	0.005***				
	(0.001)	(0.001)				
Treat × Post	0.003***	0.003***	0.003***	0.004***	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	2,114	2,109	2,114	2,109	2,114	2,109
R-squared	0.081	0.116	0.400	0.429	0.465	0.466
Bank Controls	No	Yes	No	Yes	No	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No	Yes	Yes
Quarter FE	No	No	Yes	Yes	Yes	Yes

Table A.4: Heterogeneous spillover effects of the unexpected bailout on credit risk

This table shows the heterogeneous spillover effects of the unexpected bailout on banks' credit risk. To conduct the cross-sectional tests, I divide the treated banks into two subsamples based on their market-to-book ratio and capital ratio before the unexpected bailout. Treated banks with market-to-book ratio lower than median are classified as low market-to-book banks, and those with market-to-book higher than median market-to-book are classified as high market-to-book banks. Treated banks with capital ratio lower than median are classified as low capital banks, and those with capital higher than median are classified as high capital banks. The sample is at the bank-quarter level, covering the period from Q1 2017 to Q4 2022. I exclude "Yes Bank" from the sample. The dependent variable is the non-performing asset (NPA) ratio. *Treat* is equal to 1 for privately-owned banks and 0 for GCBs. *Post* is equal to 1 for all quarters on or after the unexpected rescue, and 0 otherwise. All columns include bank and quarter fixed effects. Control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	NPA			
	Low Market-to-Book (1)	High Market-to-Book (2)	Low Capital (3)	High Capital (4)
Post × Treat	4.119***	3.068***	4.097***	3.115***
	(1.038)	(0.875)	(1.020)	(0.865)
Observations	497	532	497	532
R-squared	0.888	0.905	0.894	0.901
Bank Controls	Yes	Yes	Yes	Yes
Rescue Bank removed	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Difference between coefficients of Post × Private	(1)-(2):	1.051* (0.617)	(3)-(4):	0.981* (0.557)

Table A.5: Loans to COVID sensitive industries before the pandemic and post bailout

The table analyzes the association between COVID industry exposure and private sector banks relative to control banks, both before the pandemic and the unexpected rescue, as well as post-bailout. The sample is at the bank-borrower-quarter level. Columns 1 to 3 cover the pre-pandemic period from Q1 2017 to Q4 2019, while columns 4 to 6 include data for the full sample period from Q1 2017 to Q3 2022. The dependent variable is the logarithm of the total loan outstanding for a firm with a given bank. The key explanatory variable *Treat* equals one for private sector banks and zero for GCBs. The variable *Post* is set to one for all quarters on or after the quarter of the unexpected rescue and zero otherwise. The variable "COVID" is set to one for firms in COVID-sensitive industries as identified by the Kamath Committee and zero otherwise. Bank control variables include one-year lagged values of assets, deposits, and capital. The standard errors reported in parentheses are robust and clustered at the bank-time level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log(Loan Outstanding)					
Treat × Post				0.002	0.005	0.054**
				(0.023)	(0.023)	(0.027)
Treat × COVID	-0.111***	-0.226***	-0.098***	-0.225***	-0.230***	-0.104***
	(0.029)	(0.025)	(0.023)	(0.025)	(0.026)	(0.023)
Treat × Post × COVID				0.055	0.059	-0.015
				(0.036)	(0.037)	(0.032)
COVID	0.089***					
	(0.012)					
Observations	412,647	411,638	259,238	831,138	819,777	548,609
R-squared	0.062	0.171	0.627	0.163	0.162	0.623
Bank Controls	No	No	No	No	Yes	Yes
Excluding Rescued Bank	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year-Quarter FE	No	Yes	Yes	Yes	Yes	Yes
Borrower-Year-Quarter FE	No	No	Yes	No	No	Yes

Table A.6: Main regression results on excluding government banks from sample with merger announcement in 2019

The table shows the association between bank risk and private banks relative to control banks after and before the unexpected rescue. The coefficients are of the difference-in-differences design of the main regression. The sample is at the bank-quarter level from Q1 2017 to Q4 2022. The sample excludes the government banks for which merger announcements were made in 2019. The dependent variable is the non-performing asset ratio (NPA). The explanatory variable is *Treat*, which takes the value of one for private sector banks, and zero for GCBs. Post is one for all quarter-years greater or equal to the unexpected rescue quarter for Yes Bank, and zero otherwise. The quarter fixed effect is included in columns 3 and 4. Bank and quarter fixed effects are included in columns 5 and 6. Control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			NPA			
Treat	-12.686***	-10.435***	-12.686***	-10.360***		
	(0.441)	(0.544)	(1.884)	(1.705)		
Post	-5.431***	-4.081***				
	(0.486)	(0.461)				
Treat × Post	5.745***	4.533***	5.746***	4.408***	5.759***	4.578***
	(0.622)	(0.586)	(1.555)	(1.417)	(1.552)	(1.310)
Observations	618	594	618	594	618	594
R-squared	0.646	0.720	0.660	0.732	0.897	0.912
Bank Controls	No	Yes	No	Yes	No	Yes
Rescued Bank removed	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No	Yes	Yes
Quarter FE	No	No	Yes	Yes	Yes	Yes

Table A.7: Main regression results on excluding TBTF banks from the sample

The table shows the association between bank risk and private banks relative to control banks after and before the unexpected rescue of Yes Bank. The coefficients are of the difference-in-differences design of the main regression. The sample is at the bank-quarter level from Q1 2017 to Q4 2022. The sample excludes TBTF banks, namely, SBI, ICICI, and HDFC. The dependent variable is the non-performing asset ratio (NPA). The explanatory variable is *Treat*, which takes the value of one for private sector banks, and zero for GCBs. *Post* is one for all quarter-years greater or equal to the unexpected rescue quarter for Yes Bank, and zero otherwise. The quarter fixed effect is included in columns 3 and 4. Bank and quarter fixed effects are included in columns 5 and 6. Control variables are 1 year lagged values of assets, deposits, ROA, and capital. The standard errors reported in parentheses are robust and clustered at the bank level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			NPA			
Treat	-11.942***	-9.343***	-11.945***	-9.258***		
	(0.423)	(0.543)	(1.618)	(1.664)		
Post	-4.279***	-2.895***				
	(0.427)	(0.407)				
Treat × Post	4.874***	3.653***	4.877***	3.510***	4.896***	3.905***
	(0.595)	(0.563)	(1.371)	(1.212)	(1.367)	(1.032)
Observations	642	619	642	619	642	619
R-squared	0.638	0.707	0.656	0.724	0.886	0.904
Bank Controls	No	Yes	No	Yes	No	Yes
Rescued Bank removed	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	No	No	Yes	Yes
Quarter FE	No	No	Yes	Yes	Yes	Yes