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Let a Small Bank Fail: Implicit Non-guarantee and Financial Contagion

Liyuan Liu^{*}, Xianshuang Wang[†]and Zhen Zhou[‡]

Abstract

This paper examines the consequences of Chinese regulators deviating from a long-standing full bailout policy in addressing the distress of a city-level commercial bank. This policy shift led to a persistent widening of credit spreads and a significant decline in funding ratios for negotiable certificates of deposit issued by small banks relative to large ones. Our empirical analysis reveals a novel contagion mechanism driven by reduced confidence in future bailouts (implicit non-guarantee), contributing to the subsequent collapse of other small banks. However, in the longer term, this policy shift improved price efficiency, credit allocation, and discouraged risk-taking among small banks.

Keywords: Implicit guarantee, Bailout, Contagion, Price efficiency, Credit allocation, TBTF JEL: G14, G21, G28, H81

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Non-technical Summary

FOCUS

The classical theory explaining implicit guarantee, the "too big to fail" (TBTF) doctrine, suggests that large bank failures can trigger systemic crises through balance sheet contagion or fire sales. However, it overlooks the frequent bailouts of smaller banks, as seen in the U.S. regulators' recent decision to extend full deposit insurance to Silicon Valley Bank and Signature Bank. This paper explores an unexpected bailout policy shift—from a full bailout to a partial bailout—following a small bank's collapse in China. The study is to examine the impacts of this policy shift and the resulting belief change about the future government bailout (or *implicit non-guarantee*), with a focus on the interbank market.

CONTRIBUTION

This paper contributes to the literature in three ways. First, it highlight a novel contagion mechanism where a small bank's failure, triggered by a bailout policy shift, can spread to other small banks through implicit non-guarantee, independent of direct financial connections or traditional contagion channels, as well as indirect contagion from fire sales and common asset ownership. Second, it adds to the literature on implicit government guarantees by providing empirical evidence on how reduced bailout expectations affect market efficiency, financial stability, and banks' behavior, particularly for small banks. Finally, the study enhances the understanding of implicit guarantees and bailouts in China's banking sector by examining their systemic and persistent impact on risk and efficiency.

FINDINGS

The main empirical finding reveals that departing from the full bailout policy for small banks resulted in a significant deterioration of funding conditions and subsequent failures of several other small banks in the negotiable certificates of deposit (NCDs) market. Specifically, systemically unimportant (SU) banks, relative to systemically important (SI) ones, experience a significant increase in the cost of borrowing through the issuance of NCDs. Despite this pricing effect, the amount of funding SU banks can secure in the NCD market is significantly lower following the policy change. Furthermore, we provide additional supportive evidence that the spillover is caused by implicit non-guarantee. This paper also indicates that reducing the market confidence in government bailout will improve price efficiency and credit allocation, mitigate moral hazard, and prevent banks from taking excessive risks.

1 Introduction

Bank bailouts are common and thus are often well anticipated. The classical theory explaining this anticipation (or *implicit guarantee*), the "too big to fail" (TBTF) doctrine, posits that the failure of a large bank—due to its substantial assets and interconnectedness with other institutions—could trigger a systemic crisis through balance sheet contagion or fire sales. However, this framework fails to account for the frequent bailout of smaller banks. A recent example is the U.S. regulators' decision to invoke the systemic risk exception, extending full deposit insurance to Silicon Valley Bank (SVB) and Signature Bank, neither of which is classified as systemically important (SI) under classical regulatory standards.

What is the rationale behind bailing out small banks? What would be the consequences of not rescuing them? What are the impacts of implicit guarantees that are associated with small banks? This paper aims to address these questions by examining an unexpected bailout policy shift—from a full bailout to a partial bailout—following a small bank's collapse in China.

Our main finding reveals that the bailout policy shift resulted in a significant deterioration of funding conditions and subsequent failures of other banks. Using the difference-indifferences (DiD) methodology, our analysis shows that systemically unimportant (SU) banks, relative to SI ones, faced a significant increase in borrowing costs through the issuance of negotiable certificates of deposit (NCDs).¹ Despite this pricing effect, the total amount of funding SU banks could secure in the NCD market significantly declined following the policy shift.

These market reactions resemble a run on the NCD market. Our analysis indicates that the turmoil in this interbank market is not driven by conventional contagion mechanisms, but rather by declining market confidence in future government bailouts. Our paper, therefore, uncovers a novel channel through which the collapse of a small bank, absent a comprehensive government guarantee, can trigger additional bank failures, heightening systemic risk and undermining ex post financial stability. Consequently, when the risk of such contagion is present, it may be rational for regulators to provide full coverage to protect distressed small banks to safeguard financial stability. These findings justify the U.S. regulators' decision to provide comprehensive deposit coverage to SVB and Signature Bank, effectively preventing depositors from running on other regional banks.²

Our empirical analysis relies on an unexpected policy shift in China, triggered by the regu-

¹NCD issuance is a crucial mechanism for banks to manage liquidity and secure funding in China's interbank market. For example, Hachem and Song (2021) examine the significance of market power in the NCD market regarding the implications of liquidity regulation in China.

²The testimony of Martin Gruenberg, Chair of the Federal Deposit Insurance Corporation, on March 29, 2023 ("Recent Bank Failures and the Federal Regulatory Response," https://www.banking.senate.gov/hearings/ recent-bank-failures-and-the-federal-regulatory-response) highlights the federal financial regulators' concerns about the potential contagion arising from not providing full coverage to depositors in distressed banks: "With the failure of SVB and the impending failure of Signature Bank, absent more immediate assistance for uninsured depositors, [it] could have negative knock-on consequences for depositors and the financial system more broadly."

latory takeover of Baoshang Bank, a distressed city-level commercial bank. Historically, since the failure of Hainan Development Bank in 1998, distressed banks were universally bailed out without undergoing bankruptcy proceedings, with all creditors receiving full repayment. However, following the takeover of Baoshang Bank, a significant bailout policy change occurred on May 24, 2019, when it was announced that only creditors with claims below 50 million RMB would receive full coverage, while those with claims exceeding this amount should anticipate some losses. This announcement marked the first instance of regulatory authorities deviating from the full bailout scheme in the preceding two decades.

Notably, there is no binding commitment or explicit policy stipulating whether full bailouts will or will not be extended to other distressed banks in the future. However, in light of the regulatory authorities' decision not to fully bail out the creditors of this failing bank, rational market participants should anticipate that the likelihood of full bailouts for other distressed banks has significantly decreased. Consequently, this declined confidence in future government support, or *implicit non-guarantee*, will be priced into SU banks' NCD issuance, leading to higher borrowing costs. Additionally, credit spreads on NCD issuance will become more sensitive to the credit risks of the issuing banks, revealing more information about the underlying credit risk and, consequently, enhancing pricing efficiency. We develop a simple theoretical model to formalize this idea and generate testable hypotheses for our empirical analysis (See Section 3). Our theory operates within a "too big to fail" (TBTF) framework, where government guarantees consistently apply to SI banks, implying that this policy shift and the resulting implicit non-guarantee would affect only those SU banks.

NCD Market Reaction We first document the turmoil in the NCD market following the bailout policy change. Despite regulators' prompt intervention by injecting a substantial amount of liquidity into the market, both the credit spreads on NCD issuance and the proportion of banks that failed to issue NCDs increased significantly following the bailout policy change (see Figure 1).³ This pattern is persistent and continues even after the interbank rate (i.e., the Shanghai Interbank Offered Rate, Shibor) began to decline significantly as a result of the central bank's liquidity injection.⁴

To show that systemic importance is a critical factor in determining the spillover of the bailout policy shift, we divide our samples into SU (*treatment* group) and SI (*control* group) banks. Our sample period is from October 1, 2018, to December 31, 2019, which includes roughly a two-quarter window before and after the event date of May 24, 2019. We employ the DiD approach to assess the impact of the bailout policy shift on credit spreads, funded size, and funding ratios in the primary NCD market.

Our baseline regression shows that SU banks experienced a 21.2 basis point (bps) increase

³This turmoil in the NCD market following the bailout policy change is also highlighted in He (2020).

⁴ The Shibor is a daily reference rate based on the interest rates at which banks offer to lend unsecured funds to other banks in the Shanghai wholesale money market. The Shibor comprises eight maturity types, ranging from overnight to a year, calculated from rates quoted by the 18 banks with the highest credit ratings.

in credit spreads on NCD issuance relative to SI banks after the event. This increase is both economically and statistically significant, as the average pre-event credit spread for SU banks was 23.9 bps. Moreover, the widened credit spread gap between SU and SI banks remained persistent, continuing through the end of our sample period (see Figures 2 and 3).

To assess the deterioration in banks' funding conditions following the bailout policy change, we examine the *funding ratio*—the ratio of the actual funded size to the planned size on NCD issuance—for both SU and SI banks around the event. Our DiD regression shows that, despite the increased borrowing costs, the funding ratio for SU banks dropped by 8.7 percentage points relative to SI banks after the event. A similar pattern is observed in the actual *funded size* on NCD issuance. While the impact of the policy shift on the funding ratio gap between SU and SI banks is significant, it is less persistent than the effect on credit spreads (see Figures 2 and 4). These observed market reactions resemble a debt run in the interbank market, contributing to the subsequent collapse of several other small banks.

Additionally, we show that the SU and SI banks exhibit parallel trends for credit spreads and funding ratios on NCD issuance before the bailout policy change, validating the parallel trends assumption. The causal effects on credit spreads and funding ratios are robust to a variety of tests—for example, considering the difference in two groups of banks' pre-event fundamental risks and redefining treatment and control groups based on banks' type or size. (See Section 5.3 for all robustness checks.)

Mechanism: Contagion via Implicit Non-guarantee To further establish that the spillover is caused by the declined confidence in future government bailouts (or *implicit non-guarantee*) extended to SU banks, we conduct additional empirical analyses to rule out other alternative channels. For example, if the observed data patterns are mainly driven by the increased risk awareness for banks that are similar to the failed Bank, then, for SU banks that are not comparable to Baoshang, we would *not* expect to see significant widening of credit spreads or declines in funding ratios for them. To rule out this mechanism, we define privately-controlled banks or city-level commercial banks with a comparable size to Baoshang Bank as *similar banks*. The evidence shows that the observed patterns of credit spreads and funding ratios remain largely unchanged if we exclude similar banks from our treatment group.

Moreover, we find that there is no systemic change to the bank fundamentals surrounding the bailout policy shift for either SI or SU banks, meaning that the change in funding conditions is not primarily driven by the change in the bank's credit risks, which may be directly or indirectly affected by the collapse of Baoshang Bank. In addition, we find that SU banks, relative to SI banks, shrank the size of short-term borrowing and lending in the interbank market after the bailout policy change. Therefore, the observed data pattern cannot be explained by banks' risk exposures, as an endogenous response to this event.

Another potential concern is that our findings are driven by a liquidity shortage on the supply side of funding in the interbank market. However, in the aftermath of the bailout

policy change, the widened gaps between SU and SI banks in credit spreads and funding ratios persist despite massive liquidity injections implemented by bank supervisors to maintain market-wide liquidity (see Section 2.3 for details). To address this concern, we also conduct a placebo test (see Section 6.3), demonstrating that a market-wide liquidity shortage cannot be the primary reason for the observed data pattern in NCD pricing and funding outcomes surrounding the bailout policy shift.

Furthermore, we provide additional evidence supporting the implicit non-guarantee channel. Our theory posits that the likelihood of future government bailouts is positively correlated with local fiscal capacity, given that bailing out distressed banks in China often depends on local government support. We find supportive evidence that SU banks in provinces with weaker fiscal capacity face more pronounced effects on NCD issuance, including higher borrowing costs and sharper declines in funding ratios.

Another piece of evidence supporting the implicit non-guarantee channel, as predicted by our theory, is the increased sensitivity of credit spreads in NCD issuance to SU banks' credit risks. We first provide some supportive evidence: a substantial widening of the credit spread gap between high- and low-rated SU banks following the bailout policy change. This hypothesis is then formally tested using bank fundamentals (e.g., return on assets (ROA) and non-performing loans (NPL)) as proxies for credit risks. Interestingly, the credit spreads exhibit insensitivity to bank fundamentals before the event for both SI and SU banks. However, post-event, a noteworthy shift occurs, but only for SU banks, where they become statistically significant. The triple-difference regressions support this hypothesis, revealing that, for instance, a 1-percentage-point difference in NPL leads to a significantly larger increase in credit spreads for SU banks around the event, amounting to 9.6 bps compared to SI banks.

Broader Impacts of Implicit (Non-)Guarantee By ruling out alternative mechanisms, we establish that the bailout policy shift undermined public confidence in future government bailouts for SU banks, likely serving as the primary mechanism driving spillover effects onto other SU banks. As a result, a reasonable prediction is that this implicit non-guarantee would also affect other financial market participants, including equity investors, bank debt holders and bank managers. This empirical setting, therefore, provides an opportunity to further examine the broader impacts of implicit guarantees.

First, in the interbank market, we find that the implicit non-guarantee induced by the bailout policy shift enhanced *price efficiency* and improved *credit allocation*. Price efficiency improved following the bailout policy change, as the sensitivity of spreads to risk for SU banks increased significantly, making prices more reflective of the underlying credit risk. Credit allocation has improved in the NCD market because SU banks exhibiting weaker (stronger) fundamentals received significantly less (more) funding relative to SI banks after the bailout policy change compared to the period prior to this event.

Moreover, implicit guarantee has a positive impact on bank equity prices. In the stock

market, we observe consistent responses from equity investors akin to those of NCD investors. For example, over a 20-day event window surrounding the bailout policy change, cumulative abnormal returns for listed SU banks were -8.6%, while those for listed SI banks were 3.1% using the market model.⁵

Additionally, our evidence indicates that, in the aftermath of the bailout policy shift, SU banks exhibited significantly higher deposit-to-asset ratios and markedly lower risk-sensitive debt-to-asset ratios compared to SI banks. Furthermore, the risk-taking behavior of banks, measured through the volatility of ROA and its variants, experienced a significant reduction following the event for SU banks, in comparison to SI banks. These results confirm that anticipation of government bailouts indeed promotes excessive risk taking and jeopardizes market discipline for SU banks.

Finally, we provide some suggestive evidence on how the implicit non-guarantee induced by the bailout policy shift may have an impact on the real economy. Following the bailout policy change, the total amount of credit lines extended by SU banks, compared to SI banks, substantially decreased. Firms heavily dependent on credit lines from SU banks, as opposed to those relying more on SI banks, exhibited a lower utilization of credit lines.

Contribution and Related Literature The contribution of this paper is threefold. First, we highlight a novel contagion mechanism through which the failure of a small bank can spread to other small banks. Triggered by a bailout policy shift, this spillover mechanism—driven by implicit non-guarantee—can occur independently of any direct connections between the failing bank and the affected banks, or traditional balance sheet contagion (Allen and Gale, 2000; Eisenberg and Noe, 2001), as well as indirect contagion from fire sales and common asset ownership (Greenwood, Landier and Thesmar, 2015; Duarte and Eisenbach, 2021). Notably, conventional interventions, such as liquidity injections, may be ineffective in mitigating this spillover effect. In the absence of such interventions, it is reasonable to expect that implicit non-guarantee could interact with other contagion mechanisms, potentially exacerbating the negative impact on financial stability.

Secondly, we contribute to the literature on implicit government guarantee.⁶ We present empirical evidence on both the effects of implicit non-guarantees on financial contagion and the impacts of implicit guarantees on banks' behavior and market efficiency. This aligns with the trade-off between financial stability and moral hazard discussed in prior theoretical papers

⁵This finding is consistent with the findings in many previous studies on other banking systems that implicit guarantees increase banks' equity prices (see, among others, O'hara and Shaw (1990), Atkeson, d'Avernas, Eisfeldt and Weill (2019), and Gandhi, Lustig and Plazzi (2020)). It is worth noting that our evidence demonstrates that the equity premium of guarantee is associated with listed SU banks, which should not be interpreted as a "size premium" or "TBTF premium."

⁶For empirical evidence on the effects of *explicit* government guarantees (or deposit insurance) in influencing a bank's ability to retain (or attract) depositors, particularly uninsured ones, see Davenport and McDill (2006), Iyer and Puri (2012), Yagan (2015), Jaremski and Schuster (2024), Kim, Kundu and Purnanandam (2024) and Martin, Puri and Ufier (forthcoming), among others.

(see, among others, Allen, Carletti, Goldstein and Leonello (2015, 2018); Keister (2016); Keister and Narasiman (2016)).

It is worth mentioning that changes in *implicit* guarantees are difficult to observe or quantify.⁷ Similar to the approach adopted in Cutura (2021) and Gropp, Gruendl and Guettler (2014), we leverage the regulatory takeover of Baoshang Bank and the unexpected bailout policy shift to examine the broad impact of implicit guarantees on the interbank market and beyond. Our empirical setting enables us to rule out alternative mechanisms and identify a shift in market beliefs about future government bailouts. In contrast to existing studies, the policy shift examined in our paper involves only a modest deviation from a systemic bailout, as the government continued to bail out the majority of the failing bank's creditors, without enacting an explicit change in future government guarantees. Moreover, while existing studies predominantly concentrate on the price effect of implicit guarantees, our empirical setting offers a unique opportunity to examine its *quantity* impact, including the funded size and funding ratio in the NCD market, as well as credit allocation within and beyond the banking sector.

Notably, most existing papers in this literature consider TBTF as the underlying theory for implicit guarantee and therefore study the impact of implicit guarantee on large banks. For example, see Acharya, Anginer and Warburton (2022) and Berndt, Duffie and Zhu (2022) for how the collapse of Lehman Brothers affected the implied expectation of TBTF.⁸ In contrast, we examine the impact of implicit guarantee with a focus on *small banks*. Interestingly, we find that the bailout policy change had a disproportionate impact on SU banks, raising credit spreads and decreasing the funded size, while SI banks' funding conditions remained stable.

Finally, our paper contributes to the understanding of the implicit guarantee and bank bailout in China's banking sector. Implicit guarantees, provided by central or local government or backed by financial institutions, constitute a fundamental element of China's financial system.⁹ We provide evidence to demonstrate that a slight relaxation of the systemic bailout scheme results in market turmoil in the interbank market, confirming the strong market antic-

⁷For example, Gormley, Johnson and Rhee (2015) provide evidence that even with a promised no-bailout scheme, beliefs of TBTF were not eliminated because investors believed that this policy was not time consistent. In addition, many studies (e.g., Berndt, Duffie and Zhu (2022)) adopt a structural approach to estimate the implied probability of government guarantee from the market data.

⁸Focusing on the CD market in the US, Ellis and Flannery (1992) find that CD rates paid by banks are sensitive to the perceived credit risks. The impacts of TBTF on banks' borrowing costs have also been analyzed for European banks; see, for example, Lindstrom and Osborne (2020) and Neuberg, Glasserman, Kay and Rajan (2018). See Buch, Dominguez-Cardoza and Völpel (2021) for a recent survey on this topic.

⁹See, for example, Jin, Wang and Zhang (2023) and Geng and Pan (2024) for studies on guarantees extended to debts issued by state-owned enterprises, Liu, Lyu and Yu (2021) for the impacts of government guarantees offered to public bonds issued by local government financial vehicles (LGFVs), Allen, Gu, Li and Qian (2023) for the implicit guarantee provided by financial intermediaries on trust products in China, Huang, Huang and Shao (2023) for banks' choices of extending guarantees to investors in wealth management products (WMPs), and Cong, Gao, Ponticelli and Yang (2019) for implicit guarantee on bank loans made to state-connected firms during recessions.

ipation of the government bailout in the banking sector in China.¹⁰ Strong implicit guarantees are widely believed to pose a fundamental challenge to the Chinese banking sector (Song and Xiong, 2018; Zhu, 2016). However, little research has examined the impact of systemic and persistent bailouts on the risk and efficiency of the banking sector, largely due to the lack of significant variation in bailout policies in China. Our study fills this gap. We provide compelling evidence that systemic government guarantees distort pricing, lead to inefficient credit allocation in the interbank market and encourage excessive risk-taking by small banks.

Outline The remainder of this paper is structured as follows. Section 2 provides background on the Chinese banking system, the NCD market, the collapse of Baoshang Bank, and discusses overall market reactions to the bailout policy change. In Section 3, we construct a simple model and develop testable hypotheses to guide our formal empirical analysis. Section 4 describes the data and provides summary statistics. Section 5 outlines the empirical findings on how the bailout policy shift affected credit spreads and funding ratios in the NCD market, with a focus on differences between SU and SI banks. Section 6 addresses alternative explanations for the observed data patterns in the NCD market and provides supporting evidence for the implicit non-guarantee mechanism. Section 7 explores the broader effects of the implicit non-guarantee on pricing efficiency, credit allocation, equity market prices, banks' risk-taking behaviors, and market discipline. Section 8 concludes.

2 Institutional Backgrounds

2.1 Commercial Banks in China

In China, deposit-taking financial institutions, or *commercial banks*, are classified by the China Banking and Insurance Regulatory Commission (CBIRC) as follows: state-owned commercial banks, joint-stock commercial banks, city-level commercial banks, rural commercial banks, private banks, and foreign legal-person banks. The six largest commercial banks in China—the Industrial and Commercial Bank of China, the China Construction Bank, the Bank of China, the Agricultural Bank of China, the Bank of Communications, and the Postal Savings Bank of China—are all state-owned and account for approximately 39.2% of the Chinese banking system's assets by the end of 2020. Twelve medium-sized joint-stock commercial banks account for approximately 18.2% of the Chinese banking system's assets and deposits, while 133 small city commercial banks account for approximately 13.1%.¹¹ In addition, China has over 2,000 rural cooperative institutions (including rural commercial banks, rural cooperative banks, and rural credit unions) with small assets and liabilities.

¹⁰In this way, our study is also related to Kelly, Lustig and Van Nieuwerburgh (2016). The authors use option data to examine the systemic guarantee provided to the financial sector during the global financial crisis.

¹¹If not specified otherwise, the data in this section come from the same sources as the data used in our empirical analysis, which is discussed in Section 4.1.

Banking regulatory authorities in China, including the People's Bank of China (PBOC) and CBIRC, retain considerable influence over banks' lending and deposit-taking activities because of their majority ownership of the largest Chinese banks, in contrast to banking systems that are predominantly privately owned and controlled. For instance, the PBOC establishes benchmark interest rates on bank deposits and loans of various maturities, implements quarterly quotas on bank loans, and sets explicit limits on the proportion of bank loans that can be extended to certain industries (e.g., the real estate sector). As such, bank lending decisions frequently reflect government policy priorities, favoring state-owned enterprises and government infrastructure projects disproportionately.

Full Bailout Scheme More remarkable is that the government has been bailing out the creditors of all distressed banks since 1998, when Hainan Development Bank declared bankruptcy. Given that small community banks and credit unions were also rescued, the underlying reason behind government bailouts should go beyond avoiding systemic financial crises. Rather, the rationale could be linked to these banks' previously assumed obligations to cooperate with central or local government policies, as well as to ex post social harmony and stability. As will be discussed later, such a comprehensive and long-lasting bailout scheme has led to a strong market anticipation of government bailout and may have contributed to an increase in the risks within the Chinese banking sector.

Systemically Important Banks On October 15, 2021, the PBOC and CBIRC released an official list of systemically important banks (see Column (1) of Table D.5 in the Online Appendix), which includes all six state-owned banks, nine joint-stock commercial banks, and four city commercial banks. SI banks operate on a national scale and typically have a much larger asset base than SU banks. At the aggregate level, SI (SU) banks account for 77% (23%) of total Chinese banking assets. In Table 1, we compare SI and SU banks in detail. The regulatory authorities apply more stringent capital requirements to systemically important banks.

2.2 The Market of Negotiable Certificates of Deposit

NCDs are certificates of deposit with a fixed term. The NCD market was established in China on December 9, 2013, and is only available to financial institutions in the interbank market. NCDs must have a minimum size of 50 million RMB, and their maturities range from one month to one year.¹² The primary purpose of introducing NCDs was to increase transparency in the interbank market and liberalize interest rates, particularly deposit and lending rates for commercial banks.

¹²Since September 1, 2017, NCDs have a term limit of one year, according to the amended Article 8 of the "Interim Measures for the Administration of Interbank Negotiable Certificates of Deposit" (Announcement No. 12 [2017], PBOC). Previously, banks were able to issue NCDs with maturities longer than one year, including two and three years. For further details, please refer to the PBOC disclosure available at http://www.pbc.gov.cn/en/3688253/ 3689009/3788480/3860153/index.html.

NCDs are issued in a manner similar to bonds. The issuing bank (seller) first specifies the *planned issuance size* and price of the issuance (interest rate). Following this, investors (buyers) indicate the size of the security they wish to acquire at the specified price. NCD sales begin at 9:00 a.m. and continue until 5:15 p.m. on the same day. The sale automatically ends if the total purchase amount exceeds the planned amount. Therefore, the *funded size*, or total purchase amount, is either less than or equal to the *planned issuance size*. The Shibor serves as a reference rate for NCDs of equivalent maturity.

The NCD market has experienced tremendous growth since its establishment, with NCD issuance exceeding 20 trillion RMB in 2017. Outstanding NCDs now account for approximately 5% of total liabilities in China's banking sector. To understand the reasons behind the boom in the NCD market, it is necessary to highlight the critical distinctions between NCDs and other forms of debt financing. Unlike deposits, NCDs do not have a reserve requirement. Additionally, unlike bond issuances, which require approval from both the PBOC and the CBIRC, commercial banks have greater flexibility in terms of the timing and size of NCD issuance.

The dominant players in the NCD market are joint-stock and city-level commercial banks, which are at a disadvantage in competing with large banks for household and corporate deposits. These banks rely heavily on the issuance of NCDs to grow their balance sheets and manage their liquidity positions. By the end of 2020, joint-stock commercial bank held 40.80% of all outstanding NCDs, while city-level commercial banks held 32%. The major NCD investors include mutual funds (including money market mutual funds, bond funds, and hybrid funds), state-owned banks, and rural banks and credit unions. By the end of 2020, mutual funds in total funded 49.28% of the outstanding NCDs; that ratio is 18.39% for rural banks and credit unions and 8.89% for state-owned banks.

In Section A of the Online Appendix, we show that NCDs are the largest or second-largest component among the four primary short-term financing instruments for both SU and SI banks. On average, they account for 36.5% and 25.0% of short-term financing for SU and SI banks, respectively. This highlights the crucial role that NCDs play as a source of short-term financing for SI and SU banks in managing their liquidity positions.

2.3 The Collapse of Baoshang Bank

Baoshang Bank was established in the Inner Mongolia Autonomous Region in December 1998. On May 24, 2019, the PBOC and CBIRC jointly announced that CBIRC had decided to take over Baoshang Bank in response to serious credit risks. During the takeover, China Bank of Construction was tasked with managing the operation of Baoshang's branches. Prior to the takeover, Baoshang's assets totaled 550 billion RMB, or approximately 0.25% of total Chinese commercial banking assets (see PBOC (2021) for details). In this sense, Baoshang Bank is a small city-level commercial bank in China. **The Causes of Baoshang's Collapse** The inspection by regulators revealed a 220 billion RMB capital shortfall in the bank's assets, posing a significant credit risk to the bank's creditors. Baoshang's collapse was not a result of a macroeconomic recession or market-wide liquidity distress. Rather, it was an idiosyncratic event brought about by the controlling shareholders' misconduct and corporate governance failure.

Tomorrow Holding, Baoshang's largest shareholder, is a private conglomerate that owns 89% of the bank. Tomorrow Holding was reported to have illegally borrowed 150 billion RMB from Baoshang between 2005 and 2019 via 209 shell companies in the form of 347 loans that all ended up becoming non-performing.¹³

The collapse of Baoshang should have been well anticipated; as will be discussed later, however, the bailout plan may not have been. On January 28, 2017, the owner of Tomorrow Holding, the controlling shareholder of Baoshang, was apprehended on charges related to financial misconduct.¹⁴ On April 28, 2018, Baoshang Bank announced its delayed release of the 2017 annual financial statements. On June 28, 2018, Baoshang Bank announced its delayed release of release of the 2017 annual financial statements again, citing its plan to seek strategic investors.¹⁵

The Announced Plan of a "Size-dependent Bailout" Shortly after the takeover, CBIRC assured Baoshang creditors during a Q&A session that all claims under 50 million RMB would be fully repaid, including principal and interest. This promised coverage far exceeds the official deposit insurance plan, which only covers deposits up to 0.5 million RMB.

Creditors with claims exceeding 50 million RMB, however, would not be fully covered, with a planned recovery rate ranging from 70% to 90%.¹⁶ This event marked the first time in two decades that the government refrained from bailing out all bank creditors in full.

In this context, we interpret the shift in bailout policy as a shock to public expectations regarding future government bailouts. However, this policy shift should not be viewed as a market surprise concerning the risks embedded in Baoshang Bank. These risks were gradually uncovered during the investigation of its controlling shareholder, Tomorrow Holding, and

¹³For details, see Zhang Yuzhe, Wu Hongyuran, and Liu Jiefei, "Central Bank Urges Calm After Taking Control of Baoshang Bank," Caixin Global, June 3, 2019, https://www.caixinglobal.com/2019-06-03/central-bank-u rges-calm-after-taking-control-of-baoshang-bank-101423061.html, and Wu Hongyuran, Peng Qinqin, and Denise Jia, "Chinese Government Takes Over Bank Linked to Fallen Tycoon," Caixin Global, May 25, 2019, https://www.caixinglobal.com/2019-05-25/chinese-government-takes-over-fallen-tycoons-bank-101 419763.html.

¹⁴For details, see Jamil Anderlini, Ben Bland, Gloria Cheung, and Lucy Hornby, "Chinese Billionaire Abducted from Hong Kong," Financial Times, February 1, 2017, https://www.ft.com/content/8e54c51c-e7a7-11e6-893 c-082c54a7f539.

¹⁵In fact, Baoshang Bank has not published any annual reports since then. For details, see Cheng Leng, Ryan Woo, and Shu Zhang, "Chinese Regulator to Take Over Baoshang Bank Due to Credit Risks," Reuters, May 24, 2019, https://www.reuters.com/article/idUSKCN1SU1DN.

¹⁶It is noteworthy that creditors holding subordinated debts, such as Tier 2 Capital Bonds, were not covered during the bailout of Baoshang Bank. Throughout the paper, we sometimes employ the term "creditors" specifically to refer to creditors holding non-subordinated debts.

through the bank's delayed release of its annual financial statements.

Regulatory Authorities' Subsequent Responses The regulatory authorities took a number of steps to ensure market liquidity. The PBOC injected a net amount of 150 billion RMB via openmarket operations on May 26 and 27. On June 6, the PBOC expanded its lending to financial institutions by 500 billion RMB through its medium-term lending facility (MLF).¹⁷

Further, on June 9, 2019, the PBOC stated that it had no plans to take over additional banks and would instead use a variety of monetary policy tools to stabilize money markets and boost banking system liquidity.¹⁸ In addition, to ensure the continued normal operation of the failing bank, regulatory authorities provided guarantees for Baoshang's new NCD issuance following the takeover. To prevent the amplification of the policy shift, temporary but explicit guarantees were also extended to other distressed banks, such as the Bank of Jinzhou, which we will discuss shortly.

Subsequent Distress of Other Small Banks As an example, the Bank of Jinzhou, a Hong Kong-listed city-level commercial bank, experienced severe liquidity distress shortly after the event. Between May 25 and June 9, 15 days after the bailout policy change, the Bank of Jinzhou only issued two NCDs successfully, raising 0.2 billion RMB in total. In comparison, prior to the bailout policy shift, the Bank of Jinzhou successfully issued 18 NCDs in 15 days, raising 5.8 billion RMB in total. On June 10, 2019, the regulatory authorities issued temporary guarantees for the Bank of Jinzhou's NCD issuance, which were later revoked on July 30, 2019.¹⁹

Following the bailout policy change, in addition to the Bank of Jinzhou, several other small banks experienced severe funding difficulties, including one joint-stock commercial bank (Hengfeng Bank), one rural commercial bank (Chengdu Rural Commercial Bank), and three city-level commercial banks (Bank of Jilin, Harbin Bank, and Bank of Gansu). All distressed banks have received government assistance, with no creditors suffering losses.²⁰ However, we believe that these bailouts would not have an impact of comparable magnitude or persistence

¹⁷For details, see the Bloomberg report: https://www.bloomberg.com/news/articles/2019-05-28/pboc-a dds-liquidity-as-baoshang-seizure-ratchets-up-bank-stress, and the Reuters report: Winni Zhou and Andrew Galbraith, "China Central Bank Steps Up Liquidity Support for More Banks after Baoshang Takeover," Reuters, June 5, 2019, https://www.reuters.com/article/us-china-economy-mlf-idUSKCN1T706E.

¹⁸For details, see Winni Zhou and Andrew Galbraith, "China Central Bank Steps Up Liquidity Support for More Banks after Baoshang Takeover," Reuters, June 5, 2019, https://www.reuters.com/article/us-china-economy -mlf-idUSKCN1T706E.

¹⁹See the Moody's article reported on S&P Global: Regina Liezl Gambe, "Moody's: PBOC's Credit Enhancement for Bank of Jinzhou Is Credit Positive," S&P Global, June 17, 2019, https://www.spglobal.com/marketintellige nce/en/news-insights/trending/b_4v194ulmaph-fwzyzw_q2 for details.

²⁰To be specific, among the six banks, the Bank of Jinzhou and Hengfeng Bank experienced a direct intervention by the central government, while the other four banks were bailed out by the local government via share purchase or private placement, where all the bailouts happened in the aftermath of the bailout policy change. For details, see the 2020 Annual Report to Congress, the US-China Economic and Security Review Commission, December 2020, https://www.uscc.gov/sites/default/files/2020-12/Chapter_2_Section_2--Vulnerabilities_in_Chinas _Financial_System_and_Risks_for_the_United_States.pdf.

to the policy shift our study focuses on, as it marked the first deviation from the full bailout policy in over 20 years (see Example 1 in Section 3.1).

Resolution and Restructuring On August 6, 2020, Baoshang Bank filed for bankruptcy. According to PBOC (2021), the PBOC and CBIRC provided capital from the national deposit insurance fund to ensure a full repayment for personal deposits and the vast majority of institutional creditors. The support from the deposit insurance fund totaled 184.4 billion RMB, among which 34.4 billion RMB was provided to Huishang Bank to help cover acquisition costs. Additionally, a new bank, Mengshang Bank, was established to acquire the assets and liabilities of Baoshang, in collaboration with Huishang Bank, the Inner Mongolia local government, and other state investors.

The resolution was settled on February 7, 2021. In the end, losses occurred only to institutional creditors with over 50 million claims, and the overall coverage ratio was greater than 90%. Notably, even though the government did not offer a complete rescue to Baoshang's creditors, a tremendous amount of government support has been provided in the resolution process, far exceeding the deposit insurance plan. Nonetheless, the deviation from a complete rescue to a partial bailout and the regulators' decision to allow Baoshang Bank to go bankrupt were still quite unexpected. This is evidenced by the turmoil in the interbank market shortly after the government's announcement, as will be shown shortly.

2.4 Overall Market Responses to the Bailout Policy Shift

Here, we briefly discuss the overall reaction to the bailout policy change in the NCD market. We first show that the overall liquidity condition remained stable because of massive liquidity injections by bank regulators. As illustrated in Panel A of Figure 1, the three-month Shibor interest rate increased by 4.5 bps in the 15 trading days following the bailout policy change and then began to decline significantly after June 17, 2019.

Despite the stable market liquidity, the bailout policy shift has had a significant adverse impact on the NCD market's funding conditions. Panel A of Figure 1 shows that the average credit spreads on NCD issuance for all banks increased from 23.5 bps on May 24 to 36.2 bps on June 24, marking an increase of 54.0%. Apart from the soaring funding costs, banks encountered significant challenges in raising money through NCD issuance. Panel B of Figure 1 suggests that, within seven trading days, the proportion of *NCD issuance* that failed to raise any money jumped from 7.3% on May 23 to 44.7% on June 3. In addition, the proportion of banks that could not obtain financing from the NCD issuance increased from 8.3% on May 23 to 43.1% on June 3, corresponding to an increase of 419.3%. As illustrated in Figure 1, the effects of the bailout policy change on credit spreads and the likelihood of issuance failure were quite persistent, lasting at least three months.



Figure 1. Market Responses after the Bailout Policy Change

Notes: This figure presents the market responses after the bailout policy change. The sample is from February 24, 2019, to August 24, 2019, and the event day is May 24, 2019. Panel A plots the three-month Shibor interest rate (right *y*-axis) and the daily average credit spreads on NCDs successfully issued by all banks (left *y*-axis). *Spread*_{*it*} is the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. Panel B plots the daily failure proportion on NCD issuance. The solid red line plots the proportion of NCD issuance that failed to raise any money on each day. The dashed blue line plots the proportion of banks that could not obtain financing from the NCD issuance on each day.

3 Theory and Hypotheses

In this section, we construct a simple model to demonstrate how the observation that a bank failure without full bailout can change the market perceptions of future government guarantees that are applied to other small banks. In this way, it can adjust market pricing for debt issued by other small banks, even in the absence of any fundamental risk spillover. This theoretical exercise will produce testable hypotheses to guide our empirical analysis.

3.1 A Simple Model

We consider a "normal" episode of time *t* when there is no economic recession or systemic financial distress. At this time, bank distress, if it occurs, is likely to be an idiosyncratic event. Any bank *i* is either an SI bank (s = I) or an SU bank (s = U). The expost realization of $g_{i,t}^s$ governs the guarantee extended to any bank *i* in category *s* at time *t*. More specifically, $g_{i,t}^s = 1$ if the guarantee is provided; otherwise, $g_{i,t}^s = 0$.

The realization of $g_{i,t}^s$ is determined by a time-invariant (stochastic) variable of government guarantee *g* that is universally applied to all banks, and idiosyncratic factors $\alpha_{i,t}^s$ and $\beta_{i,t}^s$ as follows:

(1)
$$g_{i,t}^{s} = g + \mathbb{1}\{g = 0\}\alpha_{i,t}^{s} - \mathbb{1}\{g = 1\}\beta_{i,t}^{s},$$

in which $\mathbb{1}\{\cdot\}$ is an index function.

As discussed, government bailouts in China were previously extended to almost all distressed financial institutions, not just to systemically important banks. If the government guarantee will be extended universally to all banks (except for some bank-specific reasons), then g = 1; g = 0 means the opposite: unless there are bank-specific reasons, no bank will be bailed out.

The terms $\alpha_{i,t}^s \in \{0,1\}$ and $\beta_{i,t}^s \in \{0,1\}$ capture the idiosyncratic characteristics of bank *i* that may make it unique. More precisely, even if no guarantee is universally provided (i.e., g = 0), it will be provided to bank *i* in category *s* (i.e., $g_{i,t}^s = 1$, if $\alpha_{i,t}^s = 1$). This can occur because this bank is systemically important (i.e., $\alpha_{i,t}^I = 1$), or because the failure of an SU bank jeopardizes *regional financial or social stability*. As a result, the likelihood of $\alpha_{i,t}^U = 1$ would increase with the fiscal condition of the local government, where bank *i* is located. Similarly, even if the guarantee is universally provided to banks (i.e., g = 1), it is possible that it is not provided to bank *i* if $\beta_{i,t}^s = 1$, for reasons related to money laundering, mortgage fraud, and other illegal activities, for example. We assume that these idiosyncratic factors are independent across time *t* and across different banks and, thus, cannot be learned from prior experiences.

We denote the public belief about g at time t as $p_t \equiv \mathbb{P}(g = 1|h_t)$, in which $h_t \in \mathcal{H}_t$ stands for the history of observations prior to time t. For SI banks, because they are "too big to fail," $\alpha_{i,t}^I = 1$ and $\beta_{i,t}^I = 0$. Therefore, SI banks are always guaranteed (i.e., $g_{i,t}^I = 1$) regardless of g.

The focus of our theoretical analysis is SU banks. For any SU bank *i*, we assume that the market holds the common belief that $\mathbb{P}(\alpha_{i,t}^U = 1) = \eta_i$ and $\mathbb{P}(\beta_{i,t}^U = 1) = \tau_i$ at any time *t*.²¹ Given that it is unlikely for any SU bank to be a unique one, the values of η_i and τ_i are assumed to be very small, satisfying $\eta_i \in (0, 0.5)$ and $\tau_i \in (0, 0.5)$.

Based on these assumptions, the perceived probability that the government guarantee will be extended to any SU bank *i* at time *t* is

(2)
$$p_{i,t}^{U} \equiv \mathbb{P}(g_{i,t}^{U} = 1|h_{t}) = \mathbb{E}(g_{i,t}^{U}|h_{t}) = \mathbb{P}(g = 1|h_{t}) \left[1 - \mathbb{E}(\beta_{i,t}^{U})\right] + \mathbb{P}(g = 0|h_{t})\mathbb{E}(\alpha_{i,t}^{U})$$
$$= p_{t}(1 - \tau_{i} - \eta_{i}) + \eta_{i}.$$

Since $\tau_i + \eta_i \in (0, 1)$ by assumption, the perceived probability of an implicit guarantee is increasing in p_t , or the expected universal guarantee based on time-*t* information.

Implicit Non-guarantee Now, suppose the market observes $g_{j,t}^{U} = 0$ at time *t*; that is, the government guarantee was *not* extended to a distressed SU bank *j* at time *t*. Assuming that all market participants are Bayesian, they update their beliefs about the universal guarantee

²¹More precisely, it is assumed that $\alpha_{i,t}^{U}$ is independent of $\beta_{i,t'}^{U}$, $\alpha_{i,t'}^{U}$ and $\beta_{i,t'}^{U}$ for $t' \neq t$, as well as the idiosyncratic factors of other bank $j \neq i$ (i.e., $\alpha_{j,t}^{U}$ and $\beta_{j,t}^{U}$ for all t).

parameter *g* at t + 1 as follows:

$$p_{t+1}(g_{j,t}^{U}=0,p_t) \equiv \mathbb{P}(g=1|g_{j,t}^{U}=0,h_t) = \frac{\mathbb{P}(g_{j,t}^{U}=0|g=1,h_t)\mathbb{P}(g=1|h_t)}{\sum_{g=0,1}\mathbb{P}(g_{j,t}^{U}=0|g,h_t)\mathbb{P}(g|h_t)} = \frac{p_t}{p_t + (1-p_t)\frac{1-\eta_j}{\tau_j}}$$

Because $\frac{1-\eta_j}{\tau_j} > 1$, the market is less optimistic about a universal guarantee after observing the failure of an SU bank; that is, $p_{t+1} < p_t$ for any $p_t \in (0, 1)$.

In the opposite scenario, the market observes that a distressed bank *j* is bailed out by the government at time *t* (i.e., $g_{j,t}^{U} = 1$). Then,

$$p_{t+1}(g_{j,t}^{U}=1,p_t) \equiv \mathbb{P}(g=1|g_{j,t}^{U}=1,h_t) = \frac{\mathbb{P}(g_{j,t}^{U}=1|g=1,h_t)\mathbb{P}(g=1|h_t)}{\sum_{g=0,1}\mathbb{P}(g_{j,t}^{U}=1|g,h_t)\mathbb{P}(g|h_t)} = \frac{p_t}{p_t + (1-p_t)\frac{\eta_j}{1-\tau_j}}$$

Clearly, such an observation boosts the confidence of the universal government guarantee because $\frac{\eta_j}{1-\tau_i} < 1$. Therefore, we have

$$p_{t+1}(g_{j,t}^{U} = 1, p_t) > p_t > p_{t+1}(g_{j,t}^{U} = 0, p_t).$$

Given that the perceived probability of a government guarantee that will be extended to any SU bank *i* at time t + 1, $\mathbb{P}(g_{i,t+1}^{U} = 1)$, increases with p_{t+1} (see (2)), a time-*t* observation of a government bailout (no bailout) extended to bank *j* increases (decreases) this perceived probability.

Proposition 1 (Implicit Non-guarantee). For any SU bank *i*, any time *t*, and any history h_t , the perceived probability of future government bailout decreases (increases) after observing $g_{j,t}^U = 0$ ($g_{j,t}^U = 1$); that is,

$$(3) \quad p_{i,t+1}^{U}(g_{j,t}^{U}=0) \equiv \mathbb{P}(g_{i,t+1}^{U}=1|h_{t},g_{j,t}^{U}=0) < p_{i,t}^{U} < p_{i,t+1}^{U}(g_{j,t}^{U}=1) \equiv \mathbb{P}(g_{i,t+1}^{U}=1|h_{t},g_{i,t}^{U}=1).$$

Therefore, SU bank failure without a full bailout makes market participants less optimistic that the full bailout will be extended to other SU banks. Throughout the paper, this belief-updating process is referred to as *implicit non-guarantee*; that is, $p_{i,t+1}^U(g_{j,t}^U = 0) < p_{i,t}^U$.

Next, we present a numerical example to demonstrate the impact of a single observation of no government bailout on market confidence in future government guarantees.

Example 1 (Information Updating under Strong Anticipation of Government Bailout). The strong implicit guarantees in China feature a strong prior belief about a universal guarantee provided by the central bank ($p_0 = 95\%$) and local governments ($\eta = 20\%$), whereas the probability of not receiving a bailout conditional on a universal guarantee (g = 1) is extremely low ($\tau = 1\%$). In this case, the initial perceived likelihood of a government bailout is $p_{j,1}^{U} = 95.3\%$ for any SU bank j. Following a single observation of no bailout ($g_{i,t=1}^{U} = 0$), the perceived likelihood of a government bailout decreases to $p_{j,t=2}^{U} = 39.96\%$. Even with another observation of bailout ($g_{i,t=2}^{U} = 1$), this perceived probability will be $p_{j,t=3}^{U} = 62.05\%$, which is significantly lower than the initial value.

Pricing Bank Debt The belief updating about future bank bailouts should have a pricing impact for the debt issued by SU banks. To examine the impact of the updated belief on credit spreads, consider the following stylized setting. An SU bank *i*, which has a default probability $\phi_i \in (0,1)$ and a recovery rate $\delta_i \in (0,1)$, issues a debt at time t + 1 with principal that is normalized to 1. It is worth noting that our theoretical analysis assumes that *each bank's credit risk is not time varying or responsive to the event of bank failure without full bailout* (i.e., $\phi_{i,t} = \phi_i$ for all *t*).

The maturity of the debt is normalized to 1 unit of time, and the risk-free rate is normalized to 0. Upon maturity, creditors get the promised interest $r_{i,t+1}$ plus the principal 1 conditional on no default, and get δ_i in case of default. Government guarantees, if provided, ensure a full repayment (or $1 + r_{i,t+1}$) even when the bank defaults.

Therefore, given the perceived likelihood of guarantee $p_{i,t+1}^U$, risk-neutral creditors in a competitive market would require an interest rate $r_{i,t+1}^U$ such that²²

(4)
$$(1+r_{i,t+1}^{U})\left(1-\phi_i(1-p_{i,t+1}^{U})\right)+\delta_i\phi_i(1-p_{i,t+1}^{U})=1.$$

It is worth noting that the above break-even condition is dependent on creditors' belief about a government guarantee (or $p_{i,t+1}^{U}$). The interest rate that makes the creditors of SU bank *i* break even is

(5)
$$r_{i,t+1}^{U} = \frac{1 - \delta_i}{\frac{1}{\phi_i(1 - p_{i,t+1}^{U})} - 1}$$

Note that, as the risk-free rate is normalized to 0, $r_{i,t+1}^{U}$ also represents the credit spread. Clearly, the credit spread $r_{i,t+1}^{U}$ is decreasing in the belief of future government bailout, $p_{i,t+1}^{U}$. Therefore, because of the implicit non-guarantee (i.e., $p_{i,t+1}^{U} < p_{i,t}^{U}$), the credit spreads on debts issued by any SU bank will increase following the failure of another SU bank; that is, $r_{i,t+1}^{U} > r_{i,t}^{U}$.

Proposition 2 (Credit Spreads and Bank Failure). Following the failure of an SU bank j (i.e., $g_{j,t}^{U} = 0$) without full bailout, the credit spreads on debts issued by any SU bank i at t + 1 increase (i.e., $r_{i,t+1}^{U} > r_{i,t}^{U}$).

Notably, as we assume that the guarantee will always be extended to SI banks (i.e., $p_{i,t+1}^I = 1$), the interest rate that an SI bank needs to pay is no different from the risk-free interest rate. It is also important to note, throughout the model, we assume both the default risk ϕ_i and the recovery rate δ_i are fixed. In our empirical study, we are going to test if the observed change in the credit spread is driven by the change in bank fundamentals.

Local Government's Fiscal Capacity and Debt Pricing Thus far, our theory demonstrates how implicit non-guarantee affects the credit spreads for bank debt in a competitive market.

²²Assuming the creditors are risk averse will not make any qualitative change to our results, although it adds to the complexity of solving the endogenous interest rates.

We are interested in how the fiscal capacity of local government (where the bank is located in), characterized by η , affects the change in credit spreads caused by the event of no bailout (i.e., $g_{j,t}^{U} = 0$).

First, we observe that the change in the posterior belief of government bailout,

$$\Delta p \equiv |p_{i,t+1}^{U} - p_{i,t}^{U}| = \frac{p_{i,t}^{U}(1 - p_{i,t}^{U})}{\frac{1}{1 - \frac{\tau}{1 - n}} - p_{i,t}^{U}}$$

is decreasing in the local government's fiscal capacity η . Then, based on the determination of the credit spread (see (5)), we have the following result.

Proposition 3 (Change in Debt Pricing and Fiscal Capacity). *If the local government's fiscal capacity,* η *, is higher, the magnitude of the credit spread increase,* $r_{i,t+1}^{U} - r_{i,t}^{U}$, will be smaller.

Price Efficiency Next, we examine how the implicit non-guarantee affects pricing efficiency in the market of bank debts. We say that *price efficiency improves* if the pricing of bank debt becomes *more* sensitive to the issuing bank's credit risk (i.e., $\frac{\partial r_{i,t+1}^{U}}{\partial \phi_i}$ increases) and, thus, more effectively reflects this risk.

First, note that, in the limiting case in which the market believes that full bailout is provided with certainty (i.e., $p_{i,t+1}^{U} = 1$), the credit spread is fixed at 0 (see (4)). This implies that the market price is completely insensitive to the credit risk and, thus, fails to reflect any of the borrower's risk. However, as long as $p_{i,t+1}^{U} < 1$, the credit spread increases with ϕ_i (see (5)), or, equivalently, $\frac{\partial r_{i,t+1}^{U}}{\partial \phi_i} > 0$.

To better understand how pricing efficiency is affected by the perceived strength of the government guarantee, we examine how the credit spread's sensitivity to credit risk is affected by the belief of the government guarantee $p_{i,t+1}^U$. Formally, we observe that

$$\frac{\partial^2 r^{U}_{i,t+1}}{\partial \phi_i \partial p^{U}_{i,t+1}} = -(1-\delta_i) \frac{1+\phi_i(1-p^{U}_{i,t+1})}{\left(1-\phi_i(1-p^{U}_{i,t+1})\right)^3} < 0,$$

which implies that a stronger belief in the government guarantee makes the credit spread less sensitive to credit risk, thereby reducing price efficiency. The next proposition summarizes how an implicit non-guarantee changes price efficiency.

Proposition 4 (Price Efficiency and Implicit Non-guarantee). Following the failure of an SU bank *j* (*i.e.*, $g_{j,t}^{U} = 0$) without full bailout, the credit spread $r_{i,t+1}^{U}$ for debts issued by any SU bank *i* is more sensitive to credit risk ϕ_i compared with the sensitivity of $r_{i,t}^{U}$ to ϕ_i ; that is,

(6)
$$\frac{\partial r_{i,t+1}^{U}}{\partial \phi_{i}}\Big|_{p_{i,t+1}^{U}} > \frac{\partial r_{i,t}^{U}}{\partial \phi_{i}}\Big|_{p_{i,t}^{U}}$$

Therefore, an implicit non-guarantee (i.e., $p_{i,t}^U > p_{i,t+1}^U$ *) improves price efficiency.*

3.2 Hypothesis Development

Based on the theoretical results, we develop testable hypotheses to guide our empirical analyses.

Hypothesis 1 (Credit Spread). *The observation that a distressed SU bank did not receive a full bailout increases the credit spreads on debts issued by SU banks, while not affecting debts issued by SI banks.*

Hypothesis 1 is directly from Proposition 2. The failure of an SU bank will induce an implicit non-guarantee (i.e., $p_{i,t+1}^U - p_{i,t}^U < 0$), which, in turn, leads to an increase in the credit spread on debts issued by SU banks—that is,

(7)
$$r_{i,t+1}^{U} - r_{i,t}^{U} \approx \frac{\partial r_{i,t}^{U}}{\partial p_{i,t}^{U}} \times \underbrace{\left(p_{i,t+1}^{U} - p_{i,t}^{U}\right)}_{\text{implicit non-guarantee}} > 0.$$

As debts issued by SI banks do not follow this pattern, the credit spread gap between SU and SI bank debt widens.

Hypothesis 2 (Fiscal Capacity and Credit Spread). *After the event in which a distressed SU bank did not receive a full government bailout, the increase in credit spreads for a bank in a province with low fiscal capacity is larger than that for a bank in a province with high fiscal capacity.*

This hypothesis follows directly from Proposition 3. Consider a bank *i* located in a province with stronger fiscal capacity and another bank *k* in a province with weaker fiscal capacity. Under the implicit non-guarantee mechanism, after observing the event of no full bailout, market participants revise their beliefs about future bailouts, recognizing that bank *i* is more likely to receive local government support (i.e., $\mu_i > \mu_k$) if the central government refrains from full bailouts (i.e., g = 0). Consequently, the change in the posterior belief of a bailout for bank *i* is smaller than that for bank k, $\Delta p_i = |p_{i,t+1}^U - p_{i,t}^U| < |p_{k,t+1}^U - p_{k,t}^U| = \Delta p_k$, which in turn leads to a smaller increase in interest rates for bank *i* compared to bank k, $r_{i,t+1}^U - r_{k,t+1}^U - r_{k,t}^U$.

Hypothesis 3 (Price Efficiency). *The observation that a distressed SU bank did not receive a full bailout widens the spreads between debts issued by banks with varying credit risks. This pattern is unique to SU banks and does not apply to SI banks.*

This hypothesis translates the theoretical result from Proposition 4 to an empirically testable prediction. To illustrate this underlying logic, consider two SU banks, *i* and *k*, which are identical in all other respects except for credit risk. Assume bank *i* carries a greater credit risk than bank *k* (i.e., $\phi_i > \phi_k$) and that difference is stable during our sample periods. If the implicit non-guarantee mechanism is in effect, then, based on Proposition 4, we should anticipate a widening of the difference in credit spreads between bank *i* and *k* following an observed fail-

ure of SU bank *j* without full bailout. That is,

(8)
$$\left(r_{i,t+1}^{U} - r_{k,t+1}^{U}\right) - \left(r_{i,t}^{U} - r_{k,t}^{U}\right) \approx \underbrace{\left(\frac{\partial r_{i,t+1}^{U}}{\partial \phi_{i}}\bigg|_{p_{i,t+1}^{U}} - \frac{\partial r_{i,t+1}^{U}}{\partial \phi_{i}}\bigg|_{p_{i,t}^{U}}\right)}_{\text{an increase in spread-risk sensitivity}} \times (\phi_{i} - \phi_{k}) > 0.$$

4 Data and Summary Statistics

4.1 Sample Selection

We focus on the primary market of NCDs and collect daily issuance information and quarterly bank characteristics from the Wind Information Co. (WIND), a major financial data provider in China. Data on the daily stock return and yearly bank governance are from the China Stock Market & Accounting Research (CSMAR) Database. We also analyze the secondary market of NCDs, where the trading data of NCDs are from the China Foreign Exchange Trade System (CFETS), the interbank market's official trading platform.

Our event date is May 24, 2019, when the PBOC and CBIRC took over Baoshang Bank and announced the bailout policy shift. Our sample period is from October 1, 2018, to December 31, 2019, with a two-quarter window before and after the event date, respectively.²³

In the analyses for the impact of the bailout policy change on credit spreads of NCD issuance (*price effects*), our sample universe is NCDs issued successfully in the primary market, which we refer to as the *successful sample*. To exploit the variation in cross-bank credit spreads induced by the unanticipated deviation from full bailout, our focus is the bank-day-level analysis. We first merge the daily NCD issuance data with the quarterly bank characteristics, and then we apply the following screening criteria in our data analysis. To avoid the impact of troubled banks, we eliminate samples from Baoshang Bank and six other banks that entered distress (restructuring or bankruptcy) during our sample period (see Section 2.3).²⁴ We keep the NCD issuance sample with the largest planned issuance size if a bank issues more than one

²³We restrict our sample to the end of 2019 in order to rule out some potential concerns caused by the COVID-19 pandemic. For robustness, we change our sample period in Section D.8 of the Online Appendix.

²⁴We also delete NCD issuance that adopted protocols other than those of the issuing bank that determines the price and planned issuance size. Note that only 22 observations in our sample period did not employ the standard protocol of NCD issuance, and 20 of them, issued by the Bank of Jinzhou, have already been removed in the last step. Additionally, to make different NCD issuance interest rates more comparable, we focus on samples with a fixed interest rate. Only 10 observations in our sample period use the floating interest rate as the method of interest payment. Finally, we also exclude NCDs that are not discount bonds. In this step, we only drop one zero-coupon NCD.

NCD on the same day.²⁵ We further drop the issuing dates if there are fewer than five NCDs issued by both SU and SI banks.²⁶ We follow the standard procedure (e.g., Brown, Gustafson and Ivanov, 2021) to fill in the missing data regarding banks' core characteristics (i.e., bank's total assets, debt-to-asset ratio, ROA, and NPL) with the most current available non-missing ones of the corresponding variables.²⁷ To minimize the effect of outliers in regressions, we winsorize all continuous variables at the 1st and 99th percentile levels.

Different from the analyses on price effects, which uses the successful sample, in our analysis of the impact of the bailout policy shift on the funding ratio or funded size of NCD issuance (*quantity effects*), we consider the sample consisting of both successful and failed NCD issuance, which we refer to as the *full sample*.²⁸ The sample filtering processes are the same as those for the successful sample.

4.2 Construction of Credit Spreads and Funding Ratios

In our data analysis, we focus on two key variables: the credit spreads on NCD issuance (*Spread*_{it}), calculated as the difference between the issuance rate on the NCD and the Shibor interest rate with the same term to maturity on the same day, and the funding ratios on NCD issuance (*FdRatio*_{it}), calculated as the ratio of the funded size to the planned size on NCD issuance.

In our primary analysis, the SI banks certified by PBOC are defined as the *control* group $(Treat_i = 0)$, and other banks (i.e., SU banks) are considered as the *treatment* banks ($Treat_i = 1$).²⁹ It is worth noting that, although the official list of SI banks was announced after the bailout policy shift, the market should have a clear anticipation about which banks are large enough to be classified as systemically important ones. In addition, we will show that our results are robust to considering banks with nationwide business (i.e., state-owned commercial banks and joint-stock commercial banks) or of a size that falls in the top 5% percentile as our

²⁵We apply this selection criterion for two reasons. First, we primarily concentrate on the bank-day-level analysis. Second, the security-day-level data have only one-period observations without any time variation since we restrict our main analyses to the primary market. In this step, we delete 12,992 security-day observations, accounting for about 30.9% of total original observations. In the robustness check, we also conduct analysis on the security level in Section D.10 of the Online Appendix.

²⁶We employ this method to ensure proper coefficient estimates in subsequent regression analyses, resulting in the removal of 292 bank-day observations, roughly 0.7% of the total original observations.

²⁷We later conduct a robustness check without filling in the missing bank characteristics, and the results are reported in Section D.1 of the Online Appendix.

²⁸During our sample period, the full sample only covers two more SU banks in comparison to the successful sample. In our following analyses, unless otherwise specified, we use the full sample to explore all the quantity effects (i.e., funding ratios and funded sizes) and use the successful sample in all of the other regressions.

²⁹According to the PBOC, there are 19 systemically important banks; see Column (1) of Table D.5 in the Online Appendix. In our main analysis, we only have 18 SI banks in our control group, since the Industrial and Commercial Bank of China (ICBC), the largest commercial bank worldwide by total assets in 2018, did not issue any NCDs during our sample period.



control group (see Section D.7 in the Online Appendix for details).

Figure 2. Daily Average Credit Spreads and Funding Ratios

Notes: This figure presents the daily average credit spreads and funding ratios on NCD issuance from October 1, 2018, to December 31, 2019, and the event day is May 24, 2019. $Spread_{it}$ is the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day, which is calculated using the successful sample. $FdRatio_{it}$ is the ratio of the funded size to the planned size on NCD issuance, which is calculated using the full sample. $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. Panel A plots the simple average of $Spread_{it}$. Panel B plots the NCD planned issuance size-weighted average of $Spread_{it}$. Panel C plots the simple average of $FdRatio_{it}$. Panel D plots the moving average of $FdRatio_{it}$ with a 15-day window.

Figure 2 provides suggestive evidence that the unexpected bailout policy shift increased (decreased) the credit spreads (funding ratios) for SU banks while having little influence on SI banks. Specifically, Panel A of Figure 2 compares the simple average of daily credit spreads issued by SU and SI banks from October 1, 2018, to December 31, 2019, while Panel B of Figure 2 plots the NCD planned issuance size-weighted average. Panels A and B of Figure 2 both suggest that the bailout policy change pushed up the differences in NCD issuance interest

rates between the SU and SI banks, and the differences are quite persistent.

Table 1. Summary Statistics

Notes: This table reports the summary statistics for the main variables. The credit spreads (*Spread_{it}*), funding ratios (*FdRatio_{it}*), funded size (*FdSize_{it}*), planned issuance size (*PlanSize_{it}*), and duration of the NCD (*Duration_{it}*) are on a daily basis, and all other variables are on a quarterly basis. The sample period is from October 1, 2018, to December 31, 2019. Panels A and B describe the summary statistics of the successful sample and the full sample defined in Section 4.1, respectively.

Panel A. The "Successful" Sample									
	All			SU Bank			SI Bank		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	SD	Ν	Mean	SD	Ν	Mean	SD	Ν
NCD Characteristics									
Spread (%)	0.303	0.307	21,032	0.357	0.306	17,543	0.034	0.108	3,489
PlanSize (Billion RMB)	0.881	1.450	21,032	0.527	0.676	17,543	2.661	2.566	3,489
Duration (Year)	0.533	0.363	21,032	0.517	0.359	17,543	0.615	0.373	3,489
Bank Characteristics									
TotalAsset (Trillion RMB)	0.632	2.508	1,380	0.158	0.234	1,293	7.677	6.817	87
DebtAssetRatio (%)	91.756	2.038	1,373	91.714	2.091	1,286	92.376	0.731	87
ROA (%)	0.897	0.423	1,268	0.894	0.434	1,181	0.941	0.206	87
NPL (%)	1.884	1.075	1,345	1.913	1.104	1,258	1.470	0.278	87
Panel B. The "Full" Samp	le								
		All		SU Bank			SI Bank		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	SD	Ν	Mean	SD	Ν	Mean	SD	N
NCD Characteristics									
Spread (%)	0.331	0.327	22801	0.385	0.324	19295	0.034	0.107	3506
FdRaio	0.761	0.359	22,801	0.739	0.374	19,295	0.883	0.232	3,506
FdSize (Billion RMB)	0.720	1.363	22,801	0.401	0.636	19,295	2.473	2.493	3,506
PlanSize (Billion RMB)	0.837	1.376	22,801	0.512	0.647	19,295	2.623	2.498	3,506
Duration (Year)	0.539	0.363	22,801	0.525	0.359	19,295	0.616	0.373	3,506
Bank Characteristics									
TotalAsset (Trillion RMB)	0.625	2.494	1,397	0.157	0.233	1,310	7.677	6.817	87
DebtAssetRatio (%)	91.711	2.146	1,389	91.667	2.201	1,302	92.376	0.731	87
ROA (%)	0.895	0.422	1,283	0.892	0.434	1,196	0.941	0.206	87
NPL (%)	1.888	1.078	1,355	1.917	1.106	1,268	1.470	0.278	87

Panel C of Figure 2 plots the simple average of funding ratios issued by SU and SI banks during the same sample period. As illustrated, these funding ratios are relatively volatile, since the number of SU or SI banks issuing NCDs varies significantly from day to day. For that

reason, we create a series of an *n*-day moving average of $FundRaito_{it}$; that is, $FdRatio_{it}^{MA(n)} \equiv \frac{1}{n}\sum_{u=t-n+1}^{t} FdRatio_{iu}$. Panel D of Figure 2 plots the average $FdRatio_{it}^{MA(15)}$ across banks. The patterns of the average $FdRatio_{it}^{MA(5)}$ and $FdRatio_{it}^{MA(10)}$ over our sample periods are found to be remarkably similar. The key observation is that the funding ratio on NCD issuance declines significantly for SU banks following the bailout policy shift but remains stable for SI banks.

4.3 Summary Statistics

The descriptive statistics for the main variables are presented in Table 1, where we winsorize all continuous variables at the 1st and 99th percentile levels. Credit spreads, funding ratios, funded size, planned issuance size, and duration of NCDs are all observed on a bank-day basis, while all other variables are measured quarterly. Panels A and B present summary statistics for the successful sample and the full sample from October 1, 2018, to December 31, 2019, respectively.

Here, we focus our discussion on the successful sample (Panel A) since the full sample (Panel B) closely resembles the successful sample. The mean and median of the planned NCD issuance size are 881 and 420 million RMB, respectively, which suggests that the planned issuance size is right skewed. The median maturity of the issued NCDs in our sample is 0.5 years, indicating that NCDs are mostly short-term. During our sample period, the average daily credit spreads on NCDs are 30.3 bps, with a standard deviation of 23.2 bps. The NCDs issued by SI (SU) banks account for approximately 16.6% (83.4%) of the total number of NCD issuance. In Panel B, we show that, on average, banks issuing an NCD can raise 720 million RMB, and the average funding ratio is 76.1%. There are economically and statistically significant differences in some variables between SU and SI banks, which will be further discussed in Section 5.3.

5 The Impact of Bailout Policy Change on the NCD Market

We begin by investigating the effect of the bailout policy change on credit spreads and funding ratios on NCD issuance. Recall that our theory predicts that the absence of a full bailout for a distressed SU bank would have a differential impact on market beliefs regarding government bailouts for SI and SU banks, and consequently, on the pricing of NCDs. Accordingly, we divide the samples into SU (*treatment* group) and SI (*control* group) banks and examine the impact of the bailout policy shift using the difference-in-differences (DiD) methodology.

5.1 Credit Spreads

Panels A and B of Figure 2 provide some preliminary evidence that the differences in credit spreads on NCD issuance between SU and SI banks are quite stable before this event. SI banks

enjoy an average of 21.2 bps lower credit spreads during the pre-event period.³⁰ However, the bailout policy change significantly increased the credit spreads for SU banks while leaving them largely unchanged for SI banks. As a result, the difference in credit spreads between the two groups of banks significantly widened after this event. On average, this difference increased by 21.5 bps, reaching 42.7 bps during the post-event period.

The widening of the difference in NCD credit spreads is not a temporary market response. Rather, as can be seen in Panels A and B of Figure 2, this gap persists. It lasts until December 2019, seven months after the bailout policy change. We extend our sample periods, depicting daily average credit spreads on NCD issuance for SU and SI banks in Panel A of Figure B.1 in the Online Appendix. The evidence suggests that the impact on credit spreads persists until December 2021, 30 months post-event.

To formally test Hypothesis 1, we conduct the following DiD regression model:

(9)
$$Spread_{it} = \beta_0 + \beta_1 Treat_i \times Post_t + X_{it}\Gamma + \mu_i + \lambda_t + \epsilon_{it},$$

where the subscripts *i* and *t* denote bank and day, respectively. The dependent variable *Spread*_{*it*} is the difference between the issuance interest rate on the NCD issued by bank *i* and the Shibor interest rate with the same term to maturity on the same day *t*. The variable *Treat*_{*i*} is a dummy equal to one if bank *i* is systemically unimportant and zero otherwise, and *Post*_{*t*} is a dummy equal to one if date *t* is after the event day and zero otherwise. In our regression, X_{it} stands for a vector of time-varying control variables including the natural logarithm of the planned issuance size of the NCD (*LnPlanSize*_{*it*}), the natural logarithm of the duration of the NCD (*LnDuration*_{*it*}), and the credit rating (*Rating*_{*it*}).³¹ In addition, μ_i and λ_t are the bank fixed effect and day fixed effect, respectively, and ϵ_{it} is the bank-day specific error term. Note that β_1 is our main parameter of interest, and we expect the estimate of β_1 to be positive, as predicted by Hypothesis 1.

Table 2 reports the baseline regression results as specified by Equation (9). We start with a parsimonious model that only adds bank and day fixed effects in Column (1) of Table 2. The estimate of β_1 in Column (1) is positive and statistically significant at the 1% level, which indicates that the SU banks suffer a 21.2 bps increase in the credit spreads on NCD issuance relative to the SI banks after the bailout policy change. This increase is of an economically significant magnitude since the mean of credit spreads for the SU banks is 23.9 bps in the

³⁰It is worth noting that the majority of SI banks are state-owned or joint-stock commercial banks with larger total assets. Lower financing costs are common among these banks, which is consistent with our theory. That is, the perceived probability of having full bailouts is $p_{i,t}^{I} = 1$ for SI banks, while that probability is $P_{j,t}^{U} < 1$ for SU banks.

³¹To be clear, throughout the paper, if the dependent variable is on a daily basis and the quarterly bank characteristics are added as control variables in our regressions, we use the bank characteristics in the last quarter. For example, for the NCDs that are issued from April to June, we use this bank's total assets and debt-to-asset ratio reported in the first quarter of the calendar year.

pre-event period.

Table 2. Impact of the Bailout Policy Change on Credit Spreads

Notes: This table provides the estimation results from a DiD regression in which the dependent variable, $Spread_{it}$, is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and $Post_i$ is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD ($LnPlanSize_{it}$), the natural logarithm of total assets ($LnTotalAsset_{it}$), the debt-to-asset ratio ($DebtAssetRatio_{it}$), and the credit rating ($Rating_{it}$). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	Spread	Spread	Spread
Treat \times Post	0.212*** (0.02)	0.219*** (0.02)	0.212*** (0.02)
LnPlanSize		0.001 (0.00)	0.002 (0.00)
LnDuration		0.094*** (0.00)	0.091*** (0.01)
LnTotalAsset			-0.448** (0.18)
DebtAssetRatio			0.005 (0.01)
Day FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Rating FE	No	No	Yes
Pre-event Treatment Mean	0.239	0.239	0.227
Clusters (Bank)	390	390	296
Ν	20,953	20,953	19,626
R-squared	0.7294	0.7780	0.7809

In Column (2), we add two security-level controls, including the natural logarithm of the planned issuance size and the natural logarithm of the duration of the NCD; and in Column (3), we further control for some bank-level characteristics: the natural logarithm of total assets, the debt-to-asset ratio, and the credit rating. The key observation is that the estimate of β_1 , in either column, is statistically significant at the 1% level and is very close to the estimator in Column (1) in terms of magnitude. Additionally, the results in Column (3) show that NCD issuance of a longer term-to-maturity is associated with larger credit spreads.

The Dynamic Impact on Credit Spreads The causal inference in DiD is valid only when the "parallel trend" assumption holds. This assumption implies that, in the absence of the bailout policy shift, the trends in credit spreads are identical for the treatment and control groups.

Without any controls, Panels A and B of Figure 2 present some preliminary evidence that supports this assumption.

Next, we run the following regression to validate the parallel trend assumption by including a series of dummy variables:

(10)
$$Spread_{it} = \alpha + \sum_{t=-90}^{90} \beta_t Treat_i \times Relative Day_t + X_{it}\Gamma + \mu_i + \lambda_t + \epsilon_{it},$$

where $RelativeDay_t$ is a dummy equal to one if the observation is on the *t*-th day relative to the event day. At the end points, $RelativeDay_{-90}$ equals one for all days that are 90 or more days before the event, while $RelativeDay_{90}$ equals one for all days that are 90 or more days after the event. Other variables are the same as those in Equation (9). Following Freyaldenhoven, Hansen and Shapiro (2019), we normalize the point estimate immediately before the event date to zero. This approach enables us to estimate the dynamic effects of the bailout policy change on the credit spreads relative to the day before and after the shock.

Figure 3 plots these coefficient estimates and the 95% confidence intervals, whereby standard errors are clustered at the bank level. As shown, the coefficient estimates for *Treat* × *RelativeDay* are insignificantly different from zero for almost all days before the event day, with no trends in credit spreads prior to the event. In contrast, the differences in the credit spreads between SU and SI banks increase immediately following the shock. The impact of the bailout policy change on credit spreads on NCD issuance grows for about 20 days after the shock, and then the effect levels off, indicating a steady-state increase of about 29.4 bps in the differences in credit spreads on NCD issuance between the two groups of banks. In summary, this evidence validates the parallel trend assumption and establishes that the DiD regression produces reliable estimates of the effect of the bailout policy shift on the credit spread gap between SI and SU banks.

The Impact on Credit Spreads in the Secondary Market In addition to NCD issuance in the primary market, we also examine the impact of the bailout policy change on credit spreads in NCDs traded in the secondary market in Section C in the Online Appendix. We conduct a similar DiD regression, and the results show that the treatment effect on credit spreads is statistically significant but relatively smaller than that found in Table 2 for the primary market. A plausible explanation for this disparity is that NCDs issued in the primary market may not be directly comparable to those traded in the secondary market.

To investigate this, we refine our analysis by restricting the sample to NCDs issued in the primary market and also traded in the secondary market during the sample period. The results in Columns (1) and (2) of Table C.2 indicate a similar impact of the bailout policy change on credit spreads in both markets, with a difference of approximately 2.2 bps. Furthermore, recognizing that NCDs in the secondary market may not be actively traded,³² we narrow our

³²Throughout our sample period for all NCDs traded in the secondary market, trade counts at the 25th, 50th, and 75th percentiles are 1, 2, and 7, respectively.



Figure 3. Dynamic Impact of the Bailout Policy Change on Credit Spreads

Notes: This figure presents the dynamic impact of the bailout policy change on the credit spreads on NCD issuance. We consider a 90-day window before and after the event, respectively. All continuous variables are winsorized at the 1st and 99th percentile levels. The dashed lines correspond to 95% confidence intervals, which are based on standard errors clustered at the bank level. Specifically, the *x*-axis shows the day relative to the event, and the *y*-axis plots the coefficient estimates for *Treat*_i × *RelativeDay*_t estimated from the equation

$$Spread_{it} = \alpha + \sum_{t=-90}^{90} \beta_t Treat_i \times Relative Day_t + X_{it}\Gamma + \mu_i + \lambda_t + \epsilon_{it},$$

where $Spread_{it}$ is the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. $RelativeDay_t$ is a dummy equal to one if the observation is on the *t*-th day relative to the event day. X_{it} is a vector of control variables, including the natural logarithm of the planned issuance size of the NCD ($LnPlanSize_{it}$), the natural logarithm of the duration of the NCD ($LnDuration_{it}$), the natural logarithm of total assets ($LnTotalAsset_{it}$), the debt-to-asset ratio ($DebtAssetRatio_{it}$), and the credit rating ($Rating_{it}$). μ_i and λ_t are bank and day fixed effects, respectively. The point estimate immediately before the event date is normalized to zero.

sample to include only NCDs issued in the primary market and traded with above-median frequency in the secondary market. The results in Columns (3) and (4) of Table C.2 suggest that the bailout policy shift had a comparable impact on credit spreads in both markets, differing by just 0.9 bps.

5.2 Funding Ratios

Thus far, our data analysis has been limited to the successful sample in which the funded size on NCD issuance is strictly positive. However, in some instances, the funded size is zero, indicating that the issuing bank failed to raise any money. Recall that the proportion of NCD issuance that failed to raise any money jumped from 7.3% on May 23 to 44.7% on June 3, corresponding to an increase of 512.3%. In this section, we employ the full sample to examine the impact of the bailout policy change on the funding ratios on NCD issuance.

Notably, Hypothesis 1 makes predictions about the prices—credit spreads on NCDs—but not about the quantities—the funded sizes. However, if we interpret the zero issuance size as all investors requesting a considerably high credit spread, then, based on the underlying logic of Hypothesis 1, we would expect more issuance failures for SU banks relative to SI banks in the aftermath of the bailout policy change.

As mentioned in Section 4.2, Panels C and D of Figure 2 have already provided some preliminary evidence that supports this conjecture. The key observation is that the funding ratios on NCD issuance decline significantly for SU banks following the bailout policy shift, but they remain stable for SI banks. As a result, the difference between SI and SU banks increased by 11.7 percentage points, reaching 19.9 percentage points during the post-event period. The widening of the difference in funding ratios on NCD issuance is not a temporary market response. Rather, as can be seen in Panels C and D of Figure 2, this gap lasts until December 2019, seven months after the bailout policy change.³³

Next, we perform a DiD estimation similar to Equation (9) but with the funding ratios ($FdRatio_{it}$) as the dependent variable. Table 3 summarizes the findings. Taking Column (1) as an example, with controls of only bank and day fixed effects, the coefficient estimate for *Treat* × *Post* is -0.087 and is significant at the 1% level, suggesting that the funding ratio of SU banks on NCD issuance is 8.7 percentage points lower following the event, as compared with SI banks. This decrease is of an economically significant magnitude since the mean of funding ratios for the SU banks is 80.1 percentage points in the pre-event period. Column (2) contains the same controls as Equation (9), and Columns (3)-(5) set the dependent variables as the 5-day, 10-day, and 15-day window of the moving average of *FdRatio_{it}*, respectively. All coefficient estimates for *Treat* × *Post* are negative and statistically significant at the 1% level. In summary, we find that following the bailout policy change, the amount of money successfully

³³In Panel B of Figure B.1 in the Online Appendix, we plot the daily average funding ratios on NCD issuance for SU and SI banks, extending our sample from January 1, 2017, to December 31, 2021.

raised by SU banks relative to the planned size decreased significantly, whereas this pattern does not hold true for SI banks.

Table 3. Impact of the Bailout Policy Change on Funding Ratios

Notes: This table provides the estimation results from a DiD regression in which the dependent variable is the funding ratios on NCD issuance. In Columns (1)-(2), the dependent variable, $FdRatio_{it}$, is the ratio of the funded size to the planned size on NCD issuance. In Columns (3)-(5), the dependent variables are the 5-day, 10-day, and 15-day moving average windows of $FdRatio_{it}$, respectively. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPlanSize_{it}*), the natural logarithm of the duration of the NCD (*LnDuration_{it}*), the natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)
	FdRatio	FdRatio	FdRatio ^{MA(5)}	FdRatio ^{MA(10)}	FdRatio ^{MA(15)}
Treat \times Post	-0.087*** (0.02)	-0.077*** (0.02)	-0.083*** (0.02)	-0.076*** (0.02)	-0.073*** (0.02)
LnPlanSize		0.004 (0.01)	-0.006* (0.00)	-0.007** (0.00)	-0.006* (0.00)
LnDuration		-0.019*** (0.01)	-0.008*** (0.00)	-0.006** (0.00)	-0.004 (0.00)
LnTotalAsset		0.334** (0.13)	0.298** (0.13)	0.319** (0.14)	0.315** (0.14)
DebtAssetRatio		-0.015* (0.01)	-0.014* (0.01)	-0.016* (0.01)	-0.018** (0.01)
Day FE	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.801	0.802	0.804	0.803	0.802
Clusters (Bank)	393	301	297	288	275
Ν	22,723	21,258	21,237	21,121	20,946
R-squared	0.4367	0.4514	0.6952	0.7685	0.8029

The Dynamic Impact on Funding Ratios Next, we investigate the dynamic impact of the bailout policy shift on funding ratios on NCD issuance as well as test the parallel trend assumption. Without any controls, Panels C and D of Figure 2 present some suggestive evidence that supports this assumption. Finally, we rerun the regression (10) but with the 15-day moving average of funding ratios ($FdRatio_{it}^{MA(15)}$) as the dependent variable to validate the parallel trend assumption.³⁴

Figure 4 plots these coefficient estimates and the 95% confidence intervals, whereby stan-

³⁴As illustrated in Panel C of Figure 2, the funding ratios are relatively volatile, since the number of SU or SI banks issuing NCDs varies significantly from day to day. Therefore, we consider the 15-day moving average of funding ratios as the dependent variable here.

dard errors are clustered at the bank level.³⁵ As shown, the coefficient estimates for *Treat* × *RelativeDay* are insignificantly different from zero for all days before the event day, with no trends in credit spreads prior to the event. However, the differences in the funding ratios between SU and SI banks decrease immediately following the shock. The negative impact of the bailout policy change on funding ratios grows for about 25 days after the shock, and then the effect levels off for the following 50 days. After that, the funding ratio gap gradually disappears. This suggests that the effect of the bailout policy change on the funding ratio gap between SI and SU banks is less enduring than that on the credit spreads. A plausible explanation is that, as time passed, SU banks adjusted the credit spread on NCD issuance to match market beliefs so that the effect of the bailout policy shift decayed over time.

5.3 Robustness Checks

Admittedly, there are economically and statistically significant differences in certain dimensions between SU and SI banks. During our sample period, as shown in Table 1, NCDs issued by SI banks have a lower credit spread, a larger scale, and a longer maturity, compared with those issued by SU banks. Moreover, SU banks have fewer total assets, a lower debt-to-asset ratio, and a higher NPL than SI banks do. In this regard, our DiD setting is not as ideal as the randomized controlled trial. In Section D of the Online Appendix, we provide a variety of robustness checks to strengthen our main results on the causal effect of the bailout policy change on credit spreads and funding ratios on NCD issuance.

One important concern is that the difference in banks' pre-event fundamental risks between SU and SI banks could drive observed gap changes in credit spreads and funding ratios. For instance, during the pre-event period, SU banks have a statistically significantly higher NPL relative to SI banks, whereas there is no significant difference in ROA between SU and SI banks. To address this issue, we conduct three robustness tests: (1) we adopt the inverse probability weighting (IPW) method to ensure that SU and SI banks are comparable (see Section D.3); (2) we consider the subsample with high credit ratings (i.e., AA+ and AAA), where there is no significant difference in bank fundamental risks at the end of 2018, the year before the event (see Section D.4); and (3) we non-parametrically control for banks' pre-event NPL and implement a DFL-reweighting method (DiNardo, Fortin and Lemieux, 1996) to make the SU and SI banks more comparable in terms of NPL (see Section D.5).

In addition, we conduct a battery of additional robustness tests, including weighting each observation by its planned issuance size or total assets (see Section D.6), adopting two different ways to distinguish treatment and control banks (see Section D.7),³⁶ changing the sample pe-

³⁵Similar to discussions on the dynamic impact on credit spreads, we also change the time window, as illustrated in Panels C and D of Figure D.1 in the Online Appendix, and the results are quite robust (see Section D.2).

³⁶In the robustness check, we also define treatment and control groups based on banks' type or size. We find a great deal of overlap among the three grouping methods, suggesting a high correlation between bank size and systemic importance.



Figure 4. Dynamic Impact of the Bailout Policy Change on Funding Ratios

Notes: This figure presents the dynamic impact of the bailout policy change on the funding ratios on NCD issuance. We consider a 90-day window before and after the event, respectively. All continuous variables are winsorized at the 1st and 99th percentile levels. The dashed lines correspond to 95% confidence intervals, which are based on standard errors clustered at the bank level. Specifically, the *x*-axis shows the day relative to the event, and the *y*-axis plots the coefficient estimates for *Treat*_i × *RelativeDay*_t estimated from the equation

$$FdRatio_{it}^{MA(15)} = \alpha + \sum_{t=-90}^{90} \beta_t Treat_i \times RelativeDay_t + X_{it}\Gamma + \mu_i + \lambda_t + \epsilon_{it},$$

where $FdRatio_{it}^{MA(15)}$ is a 15-day moving average of funding ratios calculated as the ratio of the funded size to the planned size on NCD issuance. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. *RelativeDay_t* is a dummy equal to one if the observation is on the *t*-th day relative to the event day. X_{it} is a vector of control variables, including the natural logarithm of the planned issuance size of the NCD (*LnPlanSize_{it}*), the natural logarithm of the duration of the NCD (*LnDuration_{it}*), the natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). μ_i and λ_t are bank and day fixed effects, respectively. The point estimate immediately before the event date is normalized to zero.
riod (see Section D.8), truncating all continuous variables at the 1st and 99th percentile levels or without any winsorization (see Section D.9), repeating the main analyses at the securityday level (see Section D.10), using the full (successful) sample when the dependent variable is credit spreads (funding ratios) (see Section D.11 and D.12), and adding additional control variables for bank fundamental risks and bank governance (see Section D.13).

6 Mechanism: Contagion through Implicit Non-guarantee

Thus far, we have shown that funding conditions in the NCD market deteriorated significantly following the bailout policy shift. When banks are classified according to their systemic importance, our empirical analyses in Section 5 clearly demonstrate that the bailout policy change primarily affected the SU banks, whereas the market environment remained relatively stable for SI banks. For SU banks, credit spreads on NCD issuance increased; still, they cannot raise as much funding as they could before the event.

These findings are consistent with our theoretical prediction: the bailout policy shift signals an implicit non-guarantee—a reduced confidence in government bailout for SU banks, which negatively impacts their funding conditions and contributes to the distress of certain banks. Our study therefore underscores an interesting contagion channel—the implicit nonguarantee—through which the failure of one bank without full bailout can cause a deterioration in the funding conditions of other banks.

However, we cannot take it for granted that the implied non-guarantee is the only channel through which the bailout policy shift can affect other banks. After all, SU and SI banks may differ in many other aspects beyond their systemic importance, and their responses to the failure of a peer bank may be markedly different. That said, other possible explanations could account for the observed data pattern reported in Section 5.

Next, we discuss these alternative channels.

6.1 Risk Awareness

An alternative mechanism could be that the bailout policy change served as a wake-up call for NCD investors, raising their awareness of the risks associated with the issuing banks. If so, NCD investors would be more aware of the risks of banks that are "similar" to Baoshang Bank. As a result, it is possible that the observed pattern for credit spreads and funding ratios on NCD issuance is mainly due to the heightened awareness of the risk associated with issuing banks that are similar to Baoshang Bank. To address this concern, we provide three pieces of evidence suggesting that our main findings are not primarily driven by this mechanism.

First, note that the ownership of Baoshang Bank is privately controlled, and it is a small city-level commercial bank. Consequently, we label a subgroup of banks as similar banks that belong to either private banks or city-level commercial banks with a size similar to Baoshang

Bank (with total assets from 0.2 to 1.8 times that of Baoshang Bank). We first compare the differences in credit spread and funding ratio changes between SI banks and similar banks. As shown in Columns (1) and (2) of Table 4, we find that, in comparison with SI banks, the subgroup of similar banks by our first definition experienced extremely higher credit spread changes and funding ratio changes in terms of magnitude after the bailout policy change. This finding is consistent with the idea that the bailout policy shift could have made the market more aware of the risk associated with similar banks ($\phi_{i,t}$ in our model) as compared with non-similar SU banks. However, this observation can also be explained by the implicit non-guarantee channel: the market is less confident about the government bailout extended to similar banks as compared with non-similar banks. Interestingly, in Columns (3) and (4) of Table 4, according to our second definition of similar banks, the treatment effects are slightly smaller than those in Column (3) of Table 2 and Column (2) of Table 3.

We then exclude those similar banks from our treatment group, which, supposedly, are more subject to the increased risk awareness, and reexamine the impact of the bailout policy change on credit spreads and funding ratios on NCD issuance. The results show that the coefficient estimates for *Treat* \times *Post* in Columns (5) and (6) of Table 2 are quite close to what we find in our main analyses, indicating that our main findings are not primarily driven by the increased risk awareness.

Second, we investigate whether three other important events, which can potentially raise investors' awareness of the risks of Baoshang Bank and other similar banks, have a dramatic impact on credit spreads and funding ratios on NCD issuance. As discussed in Section 2.3, Tomorrow Holding, the controlling shareholder of Baoshang Bank, created significant credit risks for the bank through fraudulent transactions. In this regard, the first event that we are looking into is that the owner of Tomorrow Holding, the controlling shareholder of Baoshang, was apprehended on charges related to financial misconduct on January 28, 2017. The second event is Baoshang Bank's announcement of delayed release of the 2017 annual financial statements on April 28, 2018. The third event is Baoshang Bank's announcement of delayed release of the 2017 annual financial statements on June 28, 2018, again citing its plan to seek strategic investors. All these events signify the possibility of financial distress for Baoshang Bank, making investors more concerned about the risks embedded in banks that are similar to Baoshang. However, these events do not reveal any information about government guarantees or bailouts provided that Baoshang fell into distress. In Figure E.1 of the Online Appendix, we show that following each event, the differences in credit spreads and funding ratios on NCD issuance among similar banks, other non-similar SU banks, and SI banks do not exhibit notable and persistent changes, unlike the impact observed surrounding the bailout policy change. In this way, we provide another piece of evidence to dismiss the risk awareness mechanism.

Table 4. Risk Awareness of Banks Similar to Baoshang

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance. In Columns (1), (3), and (5), *Spread_{it}* is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2), (4), and (6), *FdRatio_{it}* is the ratio of the funded size to the planned size on NCD issuance. In Columns (1) and (2), banks similar to Baoshang Bank are defined as privately controlled banks at the end of 2018. In Columns (3) and (4), banks similar to Baoshang Bank are defined as city-level commercial banks with total assets from 0.2 to 1.8 times that of Baoshang Bank at the end of 2018. In Columns (5) and (6), banks similar to Baoshang Bank, both privately controlled banks and city-level commercial banks with similar size, are excluded. *Treat_i* is a dummy equal to one if the type of bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPlanSize_{it}*), the natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Sample		Similar B	Evoludo Similar Banka			
Jampie	Privately	Controlled Bank	City-level Bank with Similar Size		Exclude Similar Darks	
	(1)	(2)	(3)	(4)	(5)	(6)
	Spread	FdRatio	Spread	FdRatio	Spread	FdRatio
Treat \times Post	0.368*** (0.04)	-0.182*** (0.03)	0.198*** (0.02)	-0.066** (0.02)	0.210*** (0.03)	-0.060*** (0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.339	0.609	0.184	0.820	0.253	0.819
Clusters (Bank)	58	59	81	81	198	202
Ν	4,977	5,509	11,403	11,768	10,617	11,435
R-squared	0.8321	0.5427	0.7741	0.4017	0.8135	0.4678

Third, in Section E.2 of the Online Appendix, we also explore the heterogeneous impact of the bailout policy change across provinces, considering that NCD investors may exhibit heightened risk awareness regarding banks in geographical proximity to Baoshang Bank. If NCD investors become more aware of the risks associated with banks registered near this failing Bank, we would expect the bailout policy shift to have a greater impact on banks located close to the failing bank. Conversely, banks located far away from this bank would experience a weaker impact. However, when we examine the data shown in Figure E.2 of the Online Appendix, we do not observe such a pattern across issuing banks located in different provinces. This finding further alleviates concerns related to the risk awareness channel.

6.2 Balance Sheet Contagion

Another potential concern is that the failure of Baoshang Bank could adversely affect the fundamentals of other banks through various direct or indirect financial connections, potentially increasing their credit risks via fundamental-based contagion. As a result, the observed changes in funding conditions might simply reflect the impact of the bank's failure on other banks' credit risk (or $\phi_{i,t}$ in our model), even if the confidence on future government bailouts remains unchanged. Due to the lack of data on Baoshang's direct and indirect financial connections with other banks, we were unable to directly test this channel.

However, if this channel can explain the observed difference in credit spread and funding ratio changes between SI and SU banks, we should observe that the bailout policy shift has a more pronounced negative impact on SU banks' fundamentals relative to SI banks. We first run the following regression to determine whether the impact of the bailout policy change on bank fundamentals varies significantly between SI and SU banks:

(11)
$$FD_{it} = \beta_0 + \beta_1 Treat_i \times Post_t + X_{it}\Gamma + \mu_i + \lambda_t + \epsilon_{it},$$

where the subscripts *i* and *t* denote bank and quarter, respectively. The dependent variable FD_{it} is bank *i*'s fundamental variables in quarter *t*, including the return on assets ratio (ROA_{it}) and the non-performing loan ratio (NPL_{it}). The variable $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and $Post_t$ is a dummy equal to one if date *t* is after the event date and zero otherwise. The term X_{it} stands for a vector of time-varying and bank-specific control variables, including the natural logarithm of total assets ($LnTotalAsset_{it}$), debt-to-asset ratio ($DebtAssetRatio_{it}$), and credit rating ($Rating_{it}$). The terms μ_i and λ_t are the bank fixed effect and quarter fixed effect, respectively, and ϵ_{it} is the bank-quarter specific error term. The primary parameter that we are interested in is β_1 . If the bailout policy change has a greater adverse effect on SU banks relative to SI banks, we would expect the estimate of β_1 to be statistically significant and negative (positive) for ROA (NPL).

The results in Table F.1 of the Online Appendix show that the coefficient estimates for *Treat* \times *Post* are all statistically insignificant in Columns (1) and (2), indicating that there is no significant difference in the changes in bank fundamental risks following the event, including ROA and NPL, between SU and SI banks. As a result, the changes in bank fundamental risks cannot account for the observed widening changes in credit spreads and funding ratios between SU and SI banks.

To further examine whether there is a significant difference in the way bank fundamental risks vary between SU and SI banks surrounding the bailout policy shift, we plot the kernel densities of bank fundamental risks in the successful sample. As illustrated in Figure 5, we see the distributions of bank fundamental risks of SU banks are strikingly similar during the pre- and post-event periods, and this also holds for SI banks before and after the bailout policy change, which confirms the results in Table F.1 of the Online Appendix.



Figure 5. Distribution of Bank Fundamental Risks

Notes: This figure presents the kernel densities of bank fundamental risks in the successful sample. The solid red line is for SU banks during the pre-event period, the solid orange line is for SU banks during the post-event period, the dashed blue line is for SI banks during the pre-event period, and the dashed green line is for SI banks during the post-event period. The pre-event period is from 2018Q4 to 2019Q1, and the post-event period is from 2019Q2 to 2019Q4. All continuous variables are winsorized at the 1st and 99th percentile levels. The bank fundamental risk is the return on assets ratio (ROA_{it}) in Panel A and the non-performing loan ratio (NPL_{it}) in Panel B.

In addition, we rerun our main regression (9) by controlling for the two bank fundamental variables mentioned above (namely, ROA and NPL), with credit spreads and funding ratios as the dependent variable, respectively. In Columns (1) and (4) of Table D.9 in the Online Appendix, we find qualitatively and quantitatively similar results, although their magnitude becomes slightly smaller than those in Column (3) of Table 2 and Column (2) of Table 3. These results suggest that our main findings are *not* mainly driven by the fundamental contagion.

6.3 Market-wide Liquidity Shortage

In this section, we address the concern that the observed pattern in credit spread and funding ratio changes across SI and SU banks may be related to the deterioration in market liquidity conditions following the bailout policy shift. First, as can be seen in Figure 2, the increased gaps between SU and SI banks in the credit spreads and funding ratios are persistent and lasting, remaining stable in magnitude for more than six months. Given that the liquidity problems should be temporary, it is hard to believe that this sustained gap is the result of a liquidity shortage. Moreover, as discussed in Section 2, the PBOC implemented a variety of measures, including massive liquidity injections to maintain market-wide liquidity (see Section 2.3 for details). However, even though the PBOC provided ample liquidity, the widening trend in the credit spread (funding ratio) gap between SU and SI banks continued for about 20 (25) days after the shock, and the gap has remained quite persistent since then. This further demonstrates that the observed increase in the credit spread and funding ratio gap is unlikely to be driven

by a liquidity shortage on the supply side of funding.

In Section **G** of the Online Appendix, we further conduct a placebo test to ascertain whether the observed data pattern is primarily driven by the liquidity shortage. On October 25, 2016, it was rumored that the PBOC intended to incorporate off-balance-sheet wealth management products into the monitoring checklist of the macroprudential assessment (MPA) framework. As can be seen in Figure G.1 in the Online Appendix, this event this event triggered a significant market liquidity shortage.³⁷ As illustrated in Figure G.1 of the Online Appendix, our placebo test provides additional evidence to refute the alternative explanation that the difference in both credit spread and funding ratio changes between SU and SI banks following the bailout policy change is mainly a result of its impact on liquidity conditions.

6.4 Endogenous Interbank Exposures

Our theory assumes away banks' endogenous responses following the no-full-bailout event, which may not hold true in reality. It is possible that SI and SU banks adjusted their financial positions differently following the bailout policy change. Here, we discuss one possibility that SU banks endogenously choose to have a greater interbank exposure than SI banks, which can result in a higher credit risk $\phi_{i,t+1}$, accounting for the observed difference in credit spreads on NCD issuance.

Arguably, if SU banks chose to borrow more from the interbank market following the bailout policy change, exposing themselves to more rollover risk, then the market may have become unwilling to lend to SU banks and required higher credit spreads for NCDs issued by SU banks, even if market confidence of future government guarantees remained unchanged. Moreover, if SU banks ended up lending more to other banks, as compared with SI banks, then the increased exposure to counterparty risks might have accounted for the observed market responses in the NCD market.

To address these concerns, we take a look at banks' total borrowing and lending positions in the interbank market.³⁸ Specifically, we run a DiD regression similar to Equation (11) by substituting the measures of interbank borrowing and lending for the bank fundamental risks. As in Table 5, we use the natural logarithm of total interbank borrowing (*LnIBBorrow_{it}*) in Column (1) and the ratio of total interbank borrowing to total liability (*IBBorrowRatio_{it}*) in Column (2), respectively. Both coefficient estimates for *Treat* × *Post* in Columns (1) and (2) are

³⁷We choose October 25, 2016, as the date of our placebo event based on the search intensity for news on the terms "off-balance-sheet wealth management products" and "macroprudential assessment," which is validated by the Baidu search index—China's most popular search engine.

³⁸To clarify, the definitions of interbank borrowing and lending may be broader than those used in the classic literature. The data available to us allow us to examine only interbank exposures that meet these definitions. To be more precise, interbank borrowing here includes NCDs outstanding, securities sold under repurchase agreements, interbank loans, and deposits made by other banks and financial institutions; and interbank lending includes securities purchased under repurchase agreements, interbank loans extended to other banks, and deposits with other banks and financial institutions.

negative and statistically significant at the 1% level, indicating that the SU banks decreased their borrowing from the interbank market after the event more than the SI banks did. As a result, SU banks' overall short-term borrowing, including NCDs, decreased more following the bailout policy shift, as compared with SI banks.

Next, we also check banks' total lending positions in the interbank market, which capture the exposure to counterparty risks. Likewise, for each bank, we calculate the natural logarithm of total interbank lending ($LnIBLend_{it}$) in Column (3) and the ratio of total interbank lending to total assets ($IBLendRatio_{it}$) in Column (4), respectively. We find that the SU banks became more conservative relative to SI banks in terms of extending credit to other banks following the bailout policy change. As a result, SU banks overall had a lower exposure to counterparty risks following the bailout policy shift, as compared with SI banks.

Table 5. Impact of the Bailout Policy Change on Interbank Exposure

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are proxies for interbank exposure. In Column (1), $LnIBBorrow_{it}$ is the natural logarithm of the total interbank borrowing (including interbank loans, securities sold under repurchase agreements, NCDs, and deposits made by other banks and financial institutions). In Column (2), $IBBorrowRatio_{it}$ is the ratio of the total interbank borrowing to total debts. In Column (3), $LnIBLend_{it}$ is the natural logarithm of the total interbank lending (including interbank loans extended to other banks, securities purchased under repurchase agreements, and deposits with other banks and financial institutions). In Column (4), $IBLendRatio_{it}$ is the ratio of the total interbank lending to total assets. *Treat_i* is a dummy equal to one if the type of bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and $Post_t$ is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of total assets ($LnTotalAsset_{it}$), the debt-to-asset ratio ($DebtAssetRatio_{it}$), and the credit rating ($Rating_{it}$). The sample is from 2018Q4 to 2019Q4. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
	LnIBBorrow	IBBorrowRatio	LnIBLend	IBLendRatio
Treat \times Post	-0.136*** (0.03)	-2.390*** (0.53)	-0.136** (0.05)	-0.670*** (0.25)
Controls	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	5.743	23.433	4.540	7.238
Clusters (Bank)	174	174	157	157
Ν	615	615	511	511
R-squared	0.9935	0.9202	0.9805	0.9024

In summary, SU banks reduced both their short-term borrowing and short-term lending in the interbank market more than SI banks. This evidence tends to refute the presumption that the size of short-term borrowing and the exposure to counterparty risks account for the observed change in credit spreads and funding ratios on NCD issuance.

6.5 Local Fiscal Capacity and Implicit Guarantee

After ruling out alternative mechanisms, we next present additional evidence to test Hypothesis 2, which posits that if the underlying mechanism operates through the implicit nonguarantee, the increase in credit spreads for small banks should be more pronounced if they are located in provinces with weaker fiscal capacities.³⁹

We employ two measures as proxies for the provincial government's fiscal capacity, in line with Liu, Lyu and Yu (2021).⁴⁰ One is the *GDP portion* measured as the provincial GDP to national GDP, and the other is the *fiscal balance* measured as the provincial general budget revenue scaled by expenditure. Leveraging the measures of fiscal capacity at the end of 2018, we can divide SU banks into two groups based on their registration addresses: SU banks located in the provinces below the median of fiscal capacity (*LFCSU_i*) and SU banks located in provinces above the median of fiscal capacity (*HFCSU_i*).

Table 6 summarizes the heterogeneous impact of the bailout policy change among banks registered in provinces with different fiscal capacities. We first use the GDP portion as a proxy for the fiscal capacity in Columns (1) and (2). In Column (1), the coefficient estimates for $LFCSU \times Post$ and $HFCSU \times Post$ are both positive and statistically significant at the 1% level, suggesting that SU banks located in provinces with lower (higher) fiscal capacities suffered a 27.8 bps (18.3 bps) increase in the credit spreads on NCD issuance relative to the SI banks after the bailout policy change, respectively. Likewise, in Column (2), the coefficient estimates for $LFCSU \times Post$ and $HFCSU \times Post$ are both negative and statistically significant at the 1% level, indicating that the funding ratio of SU banks located in provinces with lower (higher) fiscal capacities was 10.9 (6.3) percentage points lower following the event, as compared with SI banks, respectively. Similar results can be found when using the fiscal balance to measure fiscal capacity in Columns (3) and (4).

These findings suggest that the impact of the bailout policy shift was more pronounced in small banks located in provinces with lower fiscal capacities, as evidenced by the comparison of credit spreads and funding ratios on NCD issuance between SU and SI banks. This result is line with Hypothesis 2 and further supports the implicit non-guarantee channel.⁴¹

³⁹It is worth noting that fiscal capacity may endogenously affect the probability of bank failure through other channels, such as the stringency of bank stress tests (see Faria-e Castro, Martinez and Philippon (2017)). However, this concern should be alleviated in our case, as in most instances, the regulatory framework and supervisory policies are established by the central government, which arguably should not be influenced by the local government's fiscal capacity.

⁴⁰In China, local governments' capacity to offer a bank bailout is closely tied to their fiscal conditions (see, among others, Liu, Lyu and Yu (2021); Bai, Hsieh and Song (2016); Zhang, Li and Tian (2022); Ang, Bai and Zhou (2023)).

⁴¹When we compare the difference in coefficient estimates between $LFCSU \times Post$ and $HFCSU \times Post$ in Table 6, the *F*-statistics are 7.85, 2.34, 2.49 and 0.97 in Column (1)-(4), respectively.

Table 6. Impact of the Bailout Policy Change with Different Fiscal Capacity

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance. In Columns (1) and (3), Spread_{it} is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2) and (4), FdRatio_{it} is the ratio of the funded size to the planned size on NCD issuance. Treat_i is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and Post_t is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (LnPlanSizeit), the natural logarithm of the duration of the NCD (LnPlanSizeit), the natural logarithm of total assets (LnTotal Asset_{it}), the debt-to-asset ratio (Debt AssetRatio_{it}), and the credit rating (Rating_{it}). In Columns (1) and (2), LFCSU_i is a dummy equal to one if the ratio of GDP in the province of SU bank i's registration address to national GDP is below the median at the end of 2018 and zero otherwise, and $HFCSU_i$ is a dummy equal to one if the ratio of GDP in the province of SU bank i's registration address to national GDP is above the median at the end of 2018 and zero otherwise. In Columns (3) and (4), LFCSU_i is a dummy equal to one if the ratio of the government's general budget revenue to its general budget expenditure in the province of SU bank i's registration address is below the median at the end of 2018 and zero otherwise, and HFCSU_i is a dummy equal to one if the ratio of the government's general budget revenue to its general budget expenditure in the province of SU bank i's registration address is above the median at the end of 2018 and zero otherwise. The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Fiscal Capacity	GDP	GDP Portion Fise		scal Balance	
	(1)	(2)	(3)	(4)	
	Spread	FdRatio	Spread	FdRatio	
$LFCSU \times Post$	0.278*** (0.03)	-0.109*** (0.03)	0.246*** (0.03)	-0.096*** (0.03)	
$HFCSU \times Post$	0.183*** (0.02)	-0.063*** (0.02)	0.194*** (0.02)	-0.067*** (0.02)	
Controls	Yes	Yes	Yes	Yes	
Day FE	Yes	Yes	Yes	Yes	
Bank FE	Yes	Yes	Yes	Yes	
Rating FE	Yes	Yes	Yes	Yes	
Pre-event Treatment Mean	0.227	0.802	0.227	0.802	
Clusters (Bank)	296	301	296	301	
Ν	19,626	21,258	19,626	21,258	
R-squared	0.7847	0.4521	0.7821	0.4517	

6.6 Summary

We have provided extensive empirical evidence demonstrating that creditors' risk awareness, changes in credit risk due to a market-wide liquidity shortage, interbank exposures, and balance sheet spillovers cannot be the driving force behind the observed data pattern in the NCD market surrounding the bailout policy shift. While it is impossible to rule out all plausible explanations, the evidence strongly suggests that the pattern observed in the NCD market is primarily driven by a change in market confidence regarding future bailout policies for other

small banks.

More importantly, this spillover mechanism is independent of other traditional contagion mechanisms, such as balance sheet contagion via direct or indirect financial connections, fire sales, or liquidity crises. In a crisis characterized by bank failures, other contagion channels may interact with the one of implicit non-guarantee to exacerbate the situation. However, as our empirical evidence has documented, even when other mechanisms of contagion are largely absent because of government intervention, an implicit non-guarantee can continue to have a detrimental effect on other banks. Furthermore, as our empirical findings clearly demonstrate, the adverse impact of this spillover is not temporary, and conventional government interventions such as liquidity injections are likely to be ineffective at mitigating such a contagion.

7 Broader Impacts of Implicit (Non-)Guarantee

Since implicit government guarantee is largely unobservable and unquantifiable, conducting empirical research on its consequences is exceedingly difficult. However, for regulatory policy design, it is critical for bank supervisors to understand the impact of implicit guarantee on the banking sector.

Our theoretical and empirical analyses thus far demonstrate that the bailout policy shift reduced investors' confidence in the government bailout of SU banks but not SI banks. As a result, this setting enables us to investigate the impacts of implicit guarantee by examining the behavior and performance of SI and SU banks, as well as those of their stakeholders, prior to and following the bailout policy change. In this section, we investigate how implicit nonguarantee changes price efficiency, credit allocation, equity prices, banks' risk-taking behavior, and market discipline implemented by debt holders.

7.1 Price Efficiency

We first examine the impact of the implicit non-guarantee on credit spreads on NCD issuance across different credit ratings. Since all SI banks have the highest credit rating, AAA, with no cross-sectional and time-series variation, our analysis of credit rating sensitivity is limited to SU banks.⁴² Figure 6 plots the daily average credit spreads on NCD issuance with high credit ratings (AA+ and AAA) and low credit ratings (below AA+) for SU and SI banks, respectively. Our findings are twofold: (1) the implicit non-guarantee increased the credit spreads for SU banks while having little influence on SI banks, and (2) the shock increased the credit spreads much more for SU banks with low credit ratings compared with SU banks with high credit ratings. These findings suggest that, following the bailout policy shift, SU banks' borrowing costs became more sensitive to their credit ratings, thereby enhancing price efficiency.

⁴²The variation in credit ratings for SU and SI banks is summarized in Section H.2 of the Online Appendix.



Figure 6. Daily Average Credit Spreads with Different Credit Ratings

Notes: This figure presents the daily average credit spreads on NCD issuance with different credit ratings from October 1, 2018, to December 31, 2019, and the event day is May 24, 2019. $Spread_{it}$ is the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. $HRating_{it}$ is a dummy equal to one if the credit rating is AA+ or AAA and zero otherwise.

Next, we examine the spread-to-risk sensitivity of NCDs issued by SU and SI banks before and after the bailout policy shift. Hypothesis 3 demonstrates that if the implicit non-guarantee is the mechanism underlying the observed data pattern in the NCD market, then the bailout policy change should increase the spread-to-risk sensitivity of NCD issuance for SU banks but not for SI banks. Here, to further validate the channel of the implicit non-guarantee, we employ a triple-difference (DDD) specification to test Hypothesis 3:

(12)
$$Spread_{it} = \beta_0 + \beta_1 FD_{it} \times Post_t + \beta_2 Treat_i \times Post_t \times FD_{it} + \beta_3 Treat_i \times Post_t + \beta_4 Treat_i \times FD_{it} + X_{it}\Gamma + \mu_i + \lambda_t + \epsilon_{it},$$

where FD_{it} are the time-varying and bank-specific fundamental variables. A reasonable assumption is that fundamental measures such as ROA and NPL are correlated with, and thus can be good proxies for, banks' credit risks. Other variables are the same as those in Equation (9).

Our findings are summarized in Table 7 with ROA in Columns (1)-(3) and NPL in Columns (4)-(6), respectively. Take ROA for example. Here, we only include the SU banks in Column (1) and the coefficient estimate for ROA is statistically insignificant, while the coefficient estimate for $ROA \times Post + ROA$ is -0.206 and statistically significant at the 1% level (*F*-statistic is 17.12). This implies that, for SU banks, ROA had no impact on the credit spreads on NCD

issuance before the bailout policy change, but it imposed a negative association with credit spreads afterward. Moreover, the negative coefficient estimate for $ROA \times Post$, which is statistically significant at the 1% level, implies that the negative association between ROA and credit spreads became more pronounced after the bailout policy change. However, as shown in Column (2), ROA did not play any role in determining the pricing of credit spreads either before or after this event for SI banks. Finally, in Column (3), we compare the sensitivity of credit spreads to ROA for both SU and SI banks around the event. The coefficient estimate for *Treat* × *Post* × *ROA* is negative and statistically significant at the 1% level, indicating that, in comparison to SI banks, credit spreads on NCD issuance were priced much more by ROA after the event for SU banks.

The regressions on NPL exhibit a similar pattern. Our evidence indicates that, prior to the bailout policy change, the credit spreads on both SU and SI banks were insensitive to banks' credit risks. This is consistent with the fact that the government previously extended bailouts to all banks, and the market anticipated that all banks would receive bailouts in the future. However, following the bailout policy shift, the market began to price credit risks associated with SU banks but not those associated with SI banks.

Recall that, as shown in Table F.1 of the Online Appendix and Figure 5, the differences in these fundamental measures between SU and SI banks are stable overall around the event. Therefore, the increased spread-risk sensitivity in fact largely accounts for the observed increase in the change in credit spreads between SI and SU banks.

To summarize, we employ the DDD approach and demonstrate that the implicit nonguarantee induced by the bailout policy change increases spread-risk sensitivity by using bank fundamentals as proxies for banks' credit risks. In this way, price efficiency is improved. These findings are consistent with Hypothesis 3 and reconfirm that the underlying mechanism is the diminished confidence in the government guarantee of SU banks.

7.2 Credit Allocation

Evidence from the primary market of NCD issuance allows us to further explore the impact of the implicit non-guarantee on credit allocation in the interbank market. In this subsection, we examine credit allocation both in the NCD markets and across firms through credit line extension and usage.

Table 7. Price Efficiency: Spread-Risk Sensitivity

Notes: This table provides the estimation results from a DDD regression in which the dependent variable, *Spread*_{*it*}, is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. *Treat*_{*i*} is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and *Post*_{*i*} is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPlanSize*_{*it*}), the natural logarithm of the duration of the NCD (*LnDuration*_{*it*}), the natural logarithm of total assets (*LnTotalAsset*_{*it*}), the debt-to-asset ratio (*DebtAssetRatio*_{*it*}), and the credit rating (*Rating*_{*it*}). The measures of fundamental risks (*FD*_{*it*}) are the return on assets ratio (*ROA*_{*it*}) in Columns (1)-(3), and the non-performing loan ratio (*NPL*_{*it*}) in Columns (4)-(6). In Columns (1) and (4), only SU banks are included. In Columns (2) and (5), only SI banks are included. In Columns (3) and (6), all banks are included. The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, ***, and ***, respectively.

Fundamental Risk		ROA			NPL	
	(1)	(2)	(3)	(4)	(5)	(6)
	SU Bank	SI Bank	All	SU Bank	SI Bank	All
$FD \times Post$	-0.178*** (0.04)	-0.014 (0.01)	0.008 (0.02)	0.086*** (0.02)	-0.009 (0.02)	-0.011 (0.02)
Treat \times Post \times FD			-0.187*** (0.04)			0.096*** (0.02)
Treat \times Post			0.367*** (0.04)			0.043 (0.04)
Treat \times FD			0.038 (0.09)			0.058 (0.11)
FD	-0.028 (0.04)	0.122 (0.10)	-0.059 (0.09)	-0.002 (0.02)	-0.078 (0.07)	-0.063 (0.11)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event Treatment Mean			0.219			0.226
Clusters (Bank)	252	18	270	267	18	285
Ν	15,684	3,478	19,162	15,690	3,478	19,168
R-squared	0.7622	0.5849	0.7853	0.7664	0.5844	0.7885

Credit Allocation in the NCD Market We first show that, in comparison to SI banks, SU banks did raise less money in the NCD market after the event in Section H.1 of the Online Appendix. We then explore how the implicit non-guarantee affected the relationship between the funded size and banks' credit risks before and after the bailout policy shift. As before, we employ the same fundamental measures—ROA and NPL—as proxies for banks' credit risks. We rerun the regression (12) but with the natural logarithm of the funded size on NCD issuance (*LnFdSize*_{it}) as the dependent variable.

Table 8. Credit Allocation in the NCD Market

Notes: This table provides the estimation results from a DDD regression in which the dependent variable, $LnFdSize_{it}$, is the natural logarithm of the funded size on NCD issuance. $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and $Post_i$ is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD ($LnPlanSize_{it}$), the natural logarithm of the duration of the NCD ($LnDuration_{it}$), the natural logarithm of total assets ($LnTotalAsset_{it}$), the debt-to-asset ratio ($DebtAssetRatio_{it}$), and the credit rating ($Rating_{it}$). The fundamental risks (FD_{it}) are the return on assets ratio (ROA_{it}) in Columns (1)-(3), and the non-performing loan ratio (NPL_{it}) in Columns (4)-(6). In Columns (1) and (4), only SU banks are included. In Columns (2) and (5), only SI banks are included. In Columns (3) and (6), all banks are included. The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Fundamental Risk		ROA			NPL	
	(1)	(2)	(3)	(4)	(5)	(6)
	SU Bank	SI Bank	All	SU Bank	SI Bank	All
$FD \times Post$	0.166*** (0.05)	0.045 (0.09)	0.120 (0.12)	-0.039* (0.02)	0.028 (0.04)	0.052 (0.05)
Treat \times Post \times FD			0.037 (0.13)			-0.090* (0.05)
Treat \times Post			-0.089 (0.12)			0.071 (0.08)
Treat \times FD			-0.093 (0.19)			0.080 (0.23)
FD	-0.043 (0.07)	0.000 (0.22)	0.050 (0.18)	-0.011 (0.04)	-0.001 (0.17)	-0.090 (0.23)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event Treatment Mean			1.425			1.409
Clusters (Bank)	256	18	274	271	18	289
Ν	17,178	3,495	20,673	17,145	3,495	20,640
R-squared	0.7646	0.9392	0.8469	0.7681	0.9392	0.8494

Our findings are summarized in Table 8 with ROA in Columns (1)-(3) and NPL in Columns (4)-(6), respectively. Take ROA for example. Here, we only include the SU banks in Column (1) and the coefficient estimate for ROA is statistically insignificant, while the coefficient estimate for $ROA \times Post + ROA$ is 0.123 and statistically significant at the 10% level (*F*-statistic is 3.10). This implies that, for SU banks, ROA had no impact on the funded size before the bailout policy change, but it imposed a positive association with the funded size afterward. Moreover, the positive coefficient estimate for $ROA \times Post$, which is statistically significant at the 1% level, implies that the positive association between ROA and the funded size became significantly more pronounced after the event. However, as shown in Column (2), ROA did

not play any role in determining the funded size either before or after this event for SI banks. Finally, in Column (3), we compare the sensitivity of the funded size to ROA for both SU and SI banks around the event. The coefficient estimate for *Treat* \times *Post* \times *ROA* is positive but statistically insignificant. We obtain similar results for NPL except that the coefficient estimate for *Treat* \times *Post* \times *NPL* in Column (6) is negative and statistically significant at the 10% level, indicating that, in comparison to SI banks, the funded size on NCD issuance was determined much more by NPL after the event for SU banks.

Our evidence suggests that, prior to the bailout policy shift, the amount of money raised by NCD issuance was insensitive to banks' credit risks for both SU and SI banks.⁴³ However, after the bailout policy change, the funded size through NCD issuance became negatively associated with the credit risk for SU banks but not for SI banks. In this way, we have found supportive evidence indicating that the implicit non-guarantee prompted by the bailout policy change has enhanced credit allocation in the NCD market for SU banks.

Thus far, our analysis of credit allocation about funding sizes has focused on the aggregate pattern observed in the primary market of NCDs. By looking into money market mutual fund managers ' portfolio choices, we further provide some direct evidence on how institutional investors responded to the bailout policy change in terms of NCD holdings. Suggestive evidence presented in Section H.3 of the Online Appendix shows that market mutual fund managers unloaded more NCDs issued by SU banks relative to those issued by SI banks after the event.

Firms' Credit Lines Granted by Banks We then investigate the allocation of funding that is extended to firms through banks' credit lines.⁴⁴ To do this, we collect the total credit line and used credit line data at the firm-bank-quarter level from WIND for the period between 2018Q4 and 2019Q4, where the underlying data resource is firms' bond issuance prospectuses. This dataset allows us to take a further look into firms' usage of credit lines. In this way, we provide some suggestive evidence of the real impacts of the implicit non-guarantee.

Notably, our sample is limited to non-financial firms with bond issuance. Following specific sample selection criteria in Section H.4 of the Online Appendix, our sample universe, restricted by availability, includes 105 non-financial firms with credit line data from 2018Q4 to 2019Q4. It is also worth noting that the mean of total assets for these firms is 132.4 billion RMB, suggesting that the firms in our sample are of large sizes.

From the perspective of firms, we compare credit line extension and utilization for firms with higher and lower reliance on SU banks around the bailout policy change. Specifically, we categorize our sample into two groups based on firms' exposure to SU banks prior to the

⁴³In Section H.2 of the Online Appendix, we further examine the credit allocation for SU banks in terms of credit ratings.

⁴⁴To be more specific, the bank line of credit is a prearranged financial arrangement that provides the firm with access to a specified amount of funds over a specified time interval. In Section H.4 of the Online Appendix, we provide a detailed definition of credit lines in China and highlight their distinctions from those in the US

bailout policy shift. We define a dummy variable, $HighExpSU^{j}$, which equals one (zero) if firm *j*'s ratio of total credit lines granted by all SU banks to total credit lines granted by all banks falls within the top (bottom) 1/3 percentile at the end of 2018. We then conduct a DiD estimation similar to Equation (11) at the firm-quarter level, where the dependent variables of interest are the total credit line ($FTotalCL_{t}^{j}$) and used credit line ($FUsedCL_{t}^{j}$) obtained from all banks for firm *j* at quarter *t*. Note that, in Table 9, the dependent variables include, at the firm level, $FTotalCL_{t}^{j} = \sum_{i} TotalCL_{it}^{j}$ and $FUsedCL_{t}^{j} = \sum_{i} UsedCL_{it}^{j}$, and, at the bank level, $BTotalCL_{it} = \sum_{j} TotalCL_{it}^{j}$ and $BUsedCL_{it} = \sum_{j} UsedCL_{it}^{j}$, where *i*, *j*, and *t* denote bank, firm, and quarter, respectively.

Columns (1) and (2) of Table 9 report that the coefficient estimate for $HighExpSU \times Post$ is -13.890 (-2.338) and is statistically significant at the 1% (10%) level. These findings suggest that firms with greater exposure to SU banks, compared to those more reliant on SI banks, experienced a significant reduction in their total credit lines (13.9 billion RMB) and used credit lines (2.3 billion RMB) following the bailout policy change. This reflects a decrease in both credit line extensions by banks and credit line utilization by firms more dependent on SU banks, relative to those less dependent. Thus, the evidence indicates that the funding difficulties faced by SU banks after the policy shift negatively affected firms' credit usage. In this way, our findings demonstrates some real impacts of the implicit non-guarantee induced by the bailout policy change.

On the other hand, we aggregate credit line extension and utilization at the bank-quarter level among firms in our sample universe and compare the changes in these measures between SU and SI banks surrounding the bailout policy change. In total, there are 215 banks in our sample, where each bank extends credit lines to an average (median) of 8.7 (1.3) firms. In detail, we perform a DiD estimation similar to Equation (11) but with the total credit line (*BTotalCL_{it}*) granted to all firms by bank *i* and the amount of used credit line (*BUsedCL_{it}*) of all firms drawn from bank *i* at quarter *t* in our sample universe as the dependent variables. Our findings are summarized in Columns (3) and (4) of Table 9. The coefficient estimate for *Treat* × *Post* is - 25.721 (-3.790) and is statistically significant at the 1% (5%) level, indicating that, on average, the total credit lines granted to all firms by an SU bank (the amount of used credit lines of all firms drawn from an SU bank) are reduced by 25.7 (3.8) billion RMB more after the bailout policy shift compared to an SI bank. This confirms a contraction in credit line extension and utilization from the perspective of banks.

7.3 Other Impacts of Implicit Non-guarantee

Stock Market Response We next examine how stock market investors would respond to the implicit non-guarantee induced by the bailout policy change. We consider all banks in our sample period that are publicly listed in the Chinese A-share stock market, which covers 13 listed SU banks and 17 listed SI banks in total. To measure the stock market reactions to the

bailout policy change, we calculate the cumulative abnormal return (*CARet*) centered on the event date on May 24, 2019, using two risk models: the market model and the market-adjusted return model. In addition, we also calculate the standardized cumulative abnormal return (*SCARet*). (For details on the construction of *CARet* and *SCARet*, see Section I of the Online Appendix.)

Table 9. Credit Allocation through Credit Lines Granted by Banks

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit lines. In Column (1), *FTotalCL*^{*j*}_{*i*} is the total credit line granted by all banks for firm *j*. In Column (2), *FUsedCL*^{*j*}_{*i*} is the used credit line drawn from all banks for firm *j*. In Column (3), *BTotalCL*_{*it*} is the total credit line granted to all firms by bank *i*. In Column (4), *BUsedCL*_{*it*} is the used credit line of all firms drawn from bank *i*. In Columns (1) and (2), *HighExpSU^j* is a dummy equal to one if firm *j*'s ratio of total credit lines granted by all SU banks to total credit lines granted by all banks is at the top 1/3 percentile at the end of 2018, and zero if firm *j*'s ratio of total credit lines granted by all SU banks to total credit lines granted by all banks is at the bottom 1/3 percentile at the end of 2018. In Columns (3) and (4), *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. In Columns (1) and (2), control variables include firm *j*'s natural logarithm of total assets (*LnFTotalAsset_i*), debt-to-asset ratio (*DebtAssetRatio_t*), and credit rating (*Rating_t*). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	Firm Level		Bank Level	
	(1)	(2)	(3)	(4)
	FTotalCL	FUsedCL	BTotalCL	BUsedCL
HighExpSU \times Post	-13.890*** (5.03)	-2.338* (1.27)		
Treat \times Post			-25.721*** (5.67)	-3.790** (1.52)
Controls	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes		
Bank FE			Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	19.750	11.671	4.428	2.378
Clusters (Firm)	70	70		
Clusters (Bank)			117	117
Ν	350	350	532	532
R-squared	0.9890	0.9932	0.9982	0.9989

Table 10 reports the event study results of the bailout policy change using different event windows. For the listed SU banks, there is a statistically significant and positive $CARet^{[-3,3]}$ within the 3-day event window, which becomes negative from the 10-day to 60-day event windows. The 60-day event window of $CARet^{[-60,60]}$ is -14.2% (but insignificant) and -4.2%

(significant at the 5% level) using the market model and market-adjusted return model, respectively. For the listed SI banks, the *CARet* is always positive regardless of whatever risk models and event windows are employed. We conjecture that these positive stock market reactions are related to the fact that large banks were now in a better position in competing with small banks (e.g., for deposits and other means of financing) after the bailout policy change.

The stock market generally reacted negatively to diminished market confidence in the government bailout following the bailout policy shift. These adverse reactions affected only SU banks and not SI banks, as they were unaffected by the change in market confidence. This observation demonstrates that the expectation of a government guarantee is a significant factor in supporting the equity market valuation of SU banks.

Banks' Risk-taking Behavior Next, we examine the impact of implicit non-guarantee on banks' risk-taking behavior by considering the bailout policy change as a shock that reduces the common belief in an implicit guarantee extended to small banks.⁴⁵ We follow Laeven and Levine (2009) in using the natural logarithm of the *z*-score ($LnZsocre_{it}$) and the volatility of ROA ($StdROA_{it}$) as banks' risk-taking measures. The *z*-score is calculated as ROA plus the capital asset ratio divided by the standard deviation of ROA, which measures the distance from insolvency. A higher *z*-score indicates that the bank is more stable and less inclined toward risk taking.

Results are shown in Columns (1) and (2) of Table 11. The positive coefficient estimate for *Treat* × *Post* in Column (1) is significant at the 5% level, which means that, compared with SI banks, SU banks became much less inclined toward risk taking after the bailout policy change. The statistically significant and negative coefficient estimate for *Treat* × *Post* in Column (2) presents similar evidence. Our findings show that banks respond to the reduced market confidence of government bailouts by reducing their risk-taking behavior. Therefore, the evidence suggests that a long-standing implicit guarantee in China contributed to the excessive risk-taking behavior of small banks.

Market Discipline After documenting that SU banks became less inclined toward risk taking after the bailout policy change relative to SI banks, we now assess the impact of this event on market discipline.

First, in principle, debt holders can discipline banks that engage in excessive risk taking by demanding higher interest rates or becoming reluctant to lend more money. Recall that the

⁴⁵Previous theoretical studies examine two effects of an implicit guarantee on banks' risk taking that work in opposite directions. The first one is the *moral hazard effect*: banks protected by a government bailout usually seek more risk taking since creditors have less incentive to monitor in the presence of an implicit guarantee (e.g., Flannery, 1998; Sironi, 2003; Ruckes, 2004; Gropp, Vesala and Vulpes, 2006). The second one is the *charter value effect*: banks with higher charter values resulting from government bailouts would decrease the incentives for risk taking because of the threat of losing future rents (e.g., Keeley, 1990; Hakenes and Schnabel, 2010). However, the net effect of an implicit guarantee on the risk taking of banks depends on the relative strength of the two channels. As can be seen later, we find evidence confirming the dominance of the *moral hazard effect* over the *charter value effect*.

results in Table 7 and Table 8 indicate a significant change in spread-risk sensitivity and credit allocation after the event in terms of fundamental risks for the SU banks. Focusing on the price and quantity of the NCD issuance, these results also suggest a stronger market discipline after the bailout policy change.

Table 10. Stock Market Response to the Bailout Policy Change

Notes: This table provides event study results for the impact of the bailout policy change on the stock market on May 24, 2019. The sample includes all bank stocks listed on the Chinese A-share stock market within the event window. The estimation window is [-160, -41] with a minimum observation of 30, and the event windows are [-1, +1], [-5, +5], [-10, +10], [-20,20], and [-30, +30], respectively. Each event window requires a minimum of three observations. Panel A and Panel B show the cumulative abnormal return, standardized cumulative abnormal return, and number of listed banks using the market model and market-adjusted return model, respectively. Columns (1)-(3) of each panel report the results of listed systemically unimportant banks as certified by the PBOC, and Columns (4)-(6) report the results of listed systemically important banks as certified by the PBOC. The corresponding *t*-statistics are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

		SU Bank			SI Bank		
	(1)	(2)	(3)	(4)	(5)	(6)	
Event Window	CARet	SCARet	Bank #	CARet	SCARet	Bank #	
[-1, 1]	1.660*** (2.87)	0.418*** (5.38)	13	0.258* (1.75)	0.156** (2.05)	17	
[-5, 5]	-3.715*** (-2.91)	-0.365** (-2.44)	13	0.805 (1.39)	0.279* (1.67)	17	
[-10, 10]	-8.641** (-2.51)	-0.517*** (-3.21)	13	3.114*** (4.32)	0.668*** (4.06)	17	
[-20, 20]	-7.359 (-1.31)	-0.034 (-0.19)	13	5.373*** (4.79)	0.794*** (5.07)	17	
[-30, 30]	-14.231 (-1.58)	-0.175 (-0.79)	13	4.538*** (4.20)	0.562*** (4.17)	17	

Panel B.	Market-Adjusted	Return Model	

		SU Bank			SI Bank		
	(1)	(2)	(3)	(4)	(5)	(6)	
Event Window	CARet	SCARet	Bank #	CARet	SCARet	Bank #	
[-1, 1]	2.251** (2.46)	0.434*** (5.73)	13	0.242** (1.97)	0.124** (2.11)	17	
[-5, 5]	-2.064* (-1.89)	-0.348** (-2.14)	13	1.288** (2.27)	0.354** (2.46)	17	
[-10, 10]	-4.393*** (-3.92)	-0.421*** (-3.17)	13	2.890*** (4.07)	0.538*** (4.04)	17	
[-20, 20]	-0.786 (-0.68)	-0.013 (-0.11)	13	6.736*** (5.63)	0.900*** (5.76)	17	
[-30, 30]	-4.193** (-2.13)	-0.133 (-0.97)	13	6.294*** (6.76)	0.692*** (6.56)	17	

Second, we find direct evidence that SU banks adjust their liabilities more in reaction to

tightening market discipline than SI banks do. If there were stronger market discipline after the event, we should expect a larger increase in deposits and a greater decrease in risk-sensitive debt. We test the impact of the implicit non-guarantee on the non-financial deposit-to-asset ratio (*DepositRatio_{it}*) and the risk-sensitive debt-to-asset ratio (*RSDebtRatio_{it}*). Results are reported in Columns (3) and (4) in Table 11. Specifically, Columns (3) and (4) report a larger and statistically significant increase in the deposit-to-asset ratio and a larger and statistically significant decrease in the risk-sensitive debt-to asset ratio after the bailout policy change for SU banks compared with SI banks. In general, our findings suggest that debt holders' intensity of monitoring increased and market discipline became more stringent for SU banks following the bailout policy shift.

Table 11. Impact of the Implicit Non-guarantee on Risk Taking and Market Discipline

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are proxies for banks' risk-taking behavior or market discipline. In Column (1), $LnZscore_{it}$ is the natural logarithm of the sum of ROA and the capital asset ratio divided by the volatility of ROA. In Column (2), $StdROA_{it}$ is the volatility of ROA. In Column (3), $DepositRatio_{it}$ is the non-financial deposit-to-asset ratio. In Column (4), $RSDebtRatio_{it}$ is the risk-sensitive debt-to-asset ratio. *Treat_i* is a dummy equal to one if the type of bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and $Post_t$ is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of total assets ($LnTotalAsset_{it}$), the debt-to-asset ratio ($DebtAssetRatio_{it}$), and the credit rating ($Rating_{it}$). The sample is from 2018Q4 to 2019Q4. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
	LnZscore	StdROA	DepositRatio	RSDebtRatio
Treat \times Post	0.163** (0.07)	-0.019*** (0.01)	1.514*** (0.47)	-1.585*** (0.47)
Controls	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	4.059	0.231	68.759	21.854
Clusters (Bank)	220	239	235	219
Ν	706	842	806	701
R-squared	0.9370	0.9295	0.9487	0.9494

7.4 Summary

In summary, we find evidence showing that China's long-standing implicit government guarantees distort SU banks' risk-taking incentives, impair market discipline, reduce spread-to-risk sensitivity, and erode credit allocation efficiency in the interbank market. When confronted with a reduced confidence in future government bailouts and the resulting higher financing costs on debt issuance, SU banks decrease (increase) their reliance on risk-sensitive debt (retail deposits) for funding, shrink the size of credit lines to non-financial firms, and also contain their risky investments to reduce the volatility of their asset returns. Additionally, our evidence indicates that implicit guarantees boost the stock prices of SU banks. While the bailout policy shift challenges financial stability ex post, our findings indicate that implicit non-guarantee induced by this policy change improves price efficiency, mitigate moral hazard, and prevent banks from taking excessive risks, all of which contribute to ex ante efficiency.

8 Conclusion

The long-standing government guarantee that applies to the entire banking system in China, independent of the bank's systemic importance, came to an end on May 24, 2019, when Baoshang Bank was taken over by the Chinese bank regulators. Although public funds were used to bail out the majority of Baoshang's creditors, the government, for the first time in two decades, refrained from a complete bailout policy and allowed a bank to go bankrupt.

In this paper, we document the consequences of this deviation from the full guarantee and reflect on the impact of the long-lasting government guarantee on the Chinese banking system. We find that, despite the massive liquidity injections and various guarantees provided to the banking system, the bailout policy shift significantly worsened the funding conditions in the interbank market, resulting in surging credit spreads and tremendous NCD issuance failures. We conduct extensive empirical analyses to rule out other possible mechanisms and provide additional evidence on local fiscal capacity and spread-risk sensitivity to identify the underlying reason for the observed data pattern in the NCD market—the diminished confidence in future government bailouts of SU banks.

These findings confirm the strong public belief in government bailouts that were expected to be extended to small banks in China before the bailout policy shift. Our empirical setting is unique in that it enables us to examine the impact of the government guarantee on *small* banks. In practice, even for countries other than China, the implicit guarantee applies, to a certain degree, to small banks. We provide evidence showing that the government bailout and the anticipation of it resulted in small banks taking on excessive risk, reducing price efficiency, compromising market discipline, impairing credit allocation, and increasing bank equity prices.

A clear contribution of this study is the empirical documentation of a contagion mechanism driven by updating beliefs about future bailouts following the regulator's decision to address a small bank's collapse. Through this mechanism, the failure of a systemically unimportant bank can potentially contribute to systemic risk. Moreover, we find that the impact of government guarantees on small banks mirrors the trade-off seen in TBTF: it safeguards financial stability ex post while challenging the ex ante efficiency.

From this perspective, it may be rational for regulators to bail out a small distressed bank, as the associated costs are likely far less than rescuing a larger institution or addressing contagiondriven failures of multiple banks. These findings help explain the U.S. regulators' decision to invoke the systemic risk exception (made with respect to compliance with the least-cost resolution requirement), insuring all depositors in SVB and Signature Bank. Nonetheless, determining the optimal bailout policy for small banks—balancing ex ante efficiency and ex post stability while accounting for the contagion channel via implicit non-guarantees—lies beyond the scope of our study and remains a promising avenue for future research.

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ONLINE APPENDIX: Let a Small Bank Fail: Implicit Non-guarantee and Financial Contagion

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A Additional Institutional Details

Section 2 of the main text outlines the institutional background of the Chinese banking system. In this section, we offer additional background on the primary short-term financing channels in the interbank market. In China, the interbank market is the most important marketplace where banks can have their short-term financing in order to manage their liquidity positions. For the sake of illustration, we term the total liability from the interbank market as *interbank borrowing*, a definition that may be broader than those used in the classic literature. By our definition, interbank borrowing here includes four items: outstanding NCDs, securities sold under repurchase agreements (i.e., repos), interbank loans, and deposits made by other banks and financial institutions (i.e., interbank deposits). These items are the four main short-term financing channels from the interbank market.

Next, we provide evidence to show that NCDs are a major short-term financing source for both SI and SU banks. Thus, this market is not negligible when considering the funding conditions faced by both SI and SU banks. We first calculate the proportion of each short-term financing source (i.e., NCDs outstanding, repos, interbank loans, and interbank deposits) for each bank, at the end of 2018, the year before the bailout policy change. Figure A.1 plots the average ratios for SI and SU banks, respectively. Results show that the outstanding NCD is the largest and second-largest part among the four main short-term financing instruments for SU and SI banks, accounting for an average proportion of 36.5% and 25.0%, respectively. This means that NCDs are arguably the most important short-term financing source for SU banks to manage their liquidity positions. Conversely, the largest proportion for SI banks is the one related to interbank loans.



Figure A.1. Proportion of Short-term Financing Channels from Interbank Market

Notes: This figure presents the average proportion of each short-term financing channel from the interbank market to the interbank borrowing for SI and SU banks at the end of 2018, respectively. The short-term financing channels from the interbank market include four items: NCDs outstanding (red bar), securities sold under repurchase agreements (blue bar), interbank loans (orange bar), and deposits made by other banks and financial institutions (green bar). The interbank borrowing is the sum of the four items.

B Raw Data Plots: Extending Sample Period

In order to rule out some potential concerns caused by the COVID-19 pandemic, we restrict our sample to the period from October 1, 2018, to December 31, 2019, in Figure 2 of the main text. In this section, to explore how persistent the effects of the bailout policy shift are, we plot the daily average credit spreads and funding ratios on NCD issuance with an extended sample period ranging from January 1, 2017, to December 31, 2021. As shown in Panel A of Figure B.1, the widening of the difference in NCD credit spreads still exists even 2 years and 7 months after the bailout policy change.

However, Panel B of Figure B.1 indicates that the funding ratio gap between SU and SI banks disappears temporarily about 9 months after the shock, and then this gap widens again and lasts for another 10 months. This means that the funding ratio gap induced by the bailout policy change remains at most 20 months in total.

In sum, the impact of the bailout policy shift on credit spreads on NCD issuance is persistent for a long period of time, whereas the impact on funding ratios is less enduring. A possible explanation for this observation is that the funding ratio would be affected by banks' planned issuance size and credit spreads, which are adjusted over time as a response to market reactions.



Figure B.1. Daily Average Credit Spreads and Funding Ratios: Longer Sample Period

Notes: This figure presents the daily average credit spreads and funding ratios on NCD issuance from January 1, 2017, to December 31, 2021, and the event day is May 24. The gray area is the sample period in the main analysis, ranging from October 1, 2018, to December 31, 2019. *Spread*_{*it*} is the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day, which is calculated using the successful sample. *FdRatio*_{*it*} is the ratio of the funded size to the planned size on NCD issuance, which is calculated using the full sample. *Treat*_{*i*} is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. Panel A plots the simple average of *Spread*_{*it*}. Panel B plots the simple average of *FdRatio*_{*it*}.

C Impacts on Credit Spreads in the Secondary Market

In Section 5.1 of the main text, we mainly focus on the impact of the bailout policy change on credit spreads on NCD issuance in the primary market. In this section, we explore the impact of the bailout policy shift on credit spreads in the secondary market. Following the same sample selection procedures as described in Section 4.1, we have 17,225 unique NCDs (11,964 issued by 297 unique SU banks and 5,261 issued by 18 unique SI banks) traded in our sample period with 112,615 bank-day observations.

We first explore the dynamic effect of the bailout policy change on credit spreads in the secondary market. Panel A of Figure C.1 plots the daily average of credit spreads in the secondary market. Before the bailout policy change, SI banks enjoyed an average of 15.2 bps lower credit spreads, whereas the difference in credit spreads between the two groups of banks significantly widened after this event. On average, this difference increased by 5.6 bps, reaching 20.8 bps. Panel B of Figure C.1 shows the dynamic effect of the bailout policy shift on credit spreads in the secondary market.

Next, we rerun regression (9) except that the dependent variable *Spread*_{*it*} in the secondary market is the difference between the close-price-implied yield to maturity of the NCD and the Shibor interest rate with the same term to maturity on the same day, and the two security-level controls are replaced by the natural logarithm of the number of years to maturity as of the day of trade (*LnMaturity*_{*it*}) and the traded values over the total market values of the NCD (*Turnover*_{*it*}).¹ Table C.1 reports the results for credit spreads in the secondary market. The setting in Columns (1)-(3) is the same as that in Columns (4)-(6), except that the first three columns control the bank fixed effect while the last three columns control the NCD fixed effect. The coefficient estimates for *Treat* × *Post* are all positive and statistically significant at the 1% level. For instance, the result in Column (1) indicates that SU banks suffered a 7.2 bps increase in the credit spreads traded in the secondary market relative to SI banks after the bailout policy change.

It is worth noting that the coefficient estimates for $Treat \times Post$ in Table C.1, focusing on the secondary market, are relatively smaller than those presented in Table 2 for the primary market. A plausible explanation for this disparity is that NCDs issued in the primary market may not be directly comparable to those traded in the secondary market. To investigate this, we refine our analysis by restricting the sample to NCDs issued in the primary market and also traded in the secondary market during the sample period. We then rerun regression (9) separately for the primary and secondary markets. In Columns (1) and (2) of Table C.2, the results indicate a similar impact of the bailout policy shift on credit spreads in both mar-

¹In principle, CFETS provides the Shibor interest rate with eight maturities: overnight, one week, two weeks, one month, three months, six months, nine months, and one year. For NCDs with other maturities, we employ the linear interpolation method to calculate the corresponding Shibor interest rate, which is inspired by the chart titled *Shibor Curve* from CFETS's website, https://www.chinamoney.com.cn/english/bmkshb/.

kets, with a difference of approximately 2.2 bps. Furthermore, recognizing that NCDs in the secondary market may not be actively traded,² we narrow our sample to include only NCDs issued in the primary market and traded with above-median frequency in the secondary market. Results in Columns (3) and (4) of Table C.2 suggest that the bailout policy change had a comparable impact on credit spreads in both markets, differing by just 0.9 bps.

²Throughout our sample period for all NCDs traded in the secondary market, trade counts at the 25th, 50th, and 75th percentiles are 1, 2, and 7, respectively.



Figure C.1. Credit Spreads in the Secondary Market

Notes: This figure presents the robustness results for credit spreads in the secondary market. The sample contains NCDs traded from October 1, 2018, to December 31, 2019, and the event day is May 24, 2019. *Spread_{it}* is the difference between the close-price-implied yield to maturity of the NCD and the Shibor interest rate with the same term to maturity on the same day. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. All continuous variables are winsorized at the 1st and 99th percentile levels. Panel A plots the daily average credit spreads on NCD issuance. Panel B shows the dynamic impact of the bailout policy change on the credit spreads in the secondary market, and the setting is the same as in Equation (10) except for replacing the two security-level controls by the natural logarithm of the number of years to maturity as of the day of trade (*LnMaturity_{it}*), and the traded values over the total market values of the NCD (*Turnover_{it}*).

Table C.1. Impact of the Bailout Policy Change in the Secondary Market

Notes: This table provides the estimation results from a DiD regression in which the dependent variable, *Spread_{it}*, is the credit spreads on the traded NCD equal to the difference between the close-price-implied yield to maturity of the NCD and the Shibor interest rate with the same term to maturity on the same day. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the number of years to maturity as of the day of trade (*LnMaturity_{it}*), the traded values over the total market values of the NCD (*Turnover_{it}*), the natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). The sample is NCDs traded in the secondary market from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Spread	Spread	Spread	Spread	Spread	Spread
Treat \times Post	0.072*** (0.02)	0.083*** (0.02)	0.086*** (0.02)	0.068*** (0.02)	0.070*** (0.02)	0.071*** (0.02)
LnMaturity		0.073*** (0.00)	0.073*** (0.00)		0.127*** (0.01)	0.127*** (0.01)
Turnover		-0.000*** (0.00)	-0.000*** (0.00)		-0.001*** (0.00)	-0.001*** (0.00)
LnTotalAsset			-0.041 (0.14)			0.020 (0.17)
DebtAssetRatio			-0.009 (0.01)			-0.007 (0.01)
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	No	No	No
NCD FE	No	No	No	Yes	Yes	Yes
Rating FE	No	No	Yes	No	No	Yes
Pre-event Treatment Mean	0.222	0.222	0.219	0.221	0.221	0.219
Clusters (Bank)	284	284	227	231	231	197
Ν	112,584	112,584	111,811	105,353	105,353	105,092
R-squared	0.1984	0.2587	0.2496	0.4414	0.4695	0.4652

Table C.2. Impact of the Bailout Policy Change: Primary vs. Secondary Market

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads in the primary and secondary markets. In Columns (1) and (3), Spread_{it} is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2) and (4), Spread_{it} is the credit spreads on the traded NCD equal to the difference between the close-price-implied yield to maturity of the NCD and the Shibor interest rate with the same term to maturity on the same day. Treat_i is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and Post_t is a dummy equal to one if the observation is after the event day and zero otherwise. In Columns (1) and (3), control variables include the natural logarithm of the planned issuance size of the NCD (LnPIssSize_{it}), the natural logarithm of the duration of the NCD (LnDuration_{it}), the natural logarithm of total assets (LnTotal Asset_{it}), the debt-to-asset ratio (DebtAssetRatio_{it}), and the credit rating (Rating_{it}). In Columns (2) and (4), control variables include the natural logarithm of the number of years to maturity as of the day of trade (LnMaturity_{it}), the traded values over the total market values of the NCD (*Turnover*_{it}), the natural logarithm of total assets (*LnTotal Asset*_{it}), the debt-to-asset ratio (Debt Asset Ratio_{it}), and the credit rating (Rating_{it}). The sample period is from October 1, 2018, to December 31, 2019. In Columns (1) and (2), the sample includes NCDs issued in the primary market and traded in the secondary market. In Columns (3) and (4), the sample includes NCDs issued in the primary market and traded with above-median frequency in the secondary market. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Sample	Issued	& Traded	Issued & F	Issued & Frequently Traded		
	(1) (2)		(3)	(4)		
	Primary	Secondary	Primary	Secondary		
Treat \times Post	0.132*** (0.01)	0.110*** (0.02)	0.090*** (0.01)	0.099*** (0.02)		
Controls	Yes	Yes	Yes	Yes		
Day FE	Yes	Yes	Yes	Yes		
Bank FE	Yes	Yes	Yes	Yes		
Rating FE	Yes	Yes	Yes	Yes		
Pre-event Treatment Mean	0.222	0.225	0.189	0.220		
Clusters (Bank)	230	220	137	156		
Ν	12,446	81,366	5,939	73,619		
R-squared	0.7951	0.2432	0.8186	0.1986		

D Robustness Checks

In Sections 5.1 and 5.2 of the main text, we explore the impact of the bailout policy change on credit spreads and funding ratios. In this section, we provide a variety of robustness checks to strengthen our main results on the causal effect of the bailout policy shift on credit spreads and funding ratios on NCD issuance.³

D.1 Missing Bank Characteristics

In the main analyses, we fill in the missing data regarding banks' core characteristics (i.e., bank's total assets, debt-to-asset ratio, ROA, and NPL) with the most current available nonmissing data of the corresponding variables. Without dealing with the missing bank characteristics, we repeat the main regressions in Table D.1. The results show that our results are robust to the way that we cope with missing bank characteristics.

D.2 Dynamic Impact with Different Window Lengths

In the main analyses, we employ a 90-day window around the bailout policy shift to examine the dynamic effect of the bailout policy change on credit spreads and funding ratios on NCD issuance. We rerun Equation (10) with different window lengths. Figure D.1 plots those coefficient estimates for *Treat* × *RelativeDay* and the 95% confidence intervals, with a 60-day window in Panels A and C, and a 120-day window in Panels B and D, respectively. As illustrated, the parallel trend assumptions for credit spreads and funding ratios are also valid under different window lengths.

D.3 Inverse Probability Weighting

We adopt the inverse probability weighting (IPW) method and reweight our data to match the distribution of bank fundamental risks between SU and SI banks. To do this, we first generate propensity scores for being treated by estimating a logit model (Hirano, Imbens and Ridder, 2003), with ROA and NPL as independent variables. We use the information at the end of 2018, the year before the event, to conduct this analysis. Figure D.2 plots the kernel densities of the estimated propensity scores for treatment and control banks, respectively, showing that the distributions of propensity scores overlap. Then we drop banks that are not in the common support of the propensity scores. Figure D.3 shows that after reweighting by the propensity scores, SU and SI banks are balanced in all observable bank fundamental risks, including ROA and NPL, at the end of 2018. Finally, we weigh observations by the propensity scores in our

³As mentioned in Section 4.1 of the main text, during our sample period, the full sample only covers two more SU banks in comparison to the successful sample. In our following analyses of this section, unless otherwise specified, we use the full sample to explore all the quantity effects (i.e., funding ratios and funded sizes) and use the successful sample in all of the other regressions.
main regressions. Columns (1) and (2) of Table D.2 summarize the results. Despite a slight variation in magnitudes, our baseline estimates are robust.

D.4 Sample with High Credit Ratings

In addition to matching samples with the estimated propensity scores, we also conduct the matching process using another non-parametric method to get balanced bank fundamental risks, including ROA and NPL, between SU and SI banks. Specifically, we select observations with high credit ratings (i.e., AA+ and AAA), since fundamental risks are usually embedded in credit ratings. Figure D.3 shows that in this high credit ratings sample, SU and SI banks are balanced in all observable bank fundamental risks, including ROA and NPL, at the end of 2018. When we repeat the main regressions using the sample with high credit ratings, our main findings remain stable, as illustrated in Columns (3) and (4) of Table D.2. On the other hand, this also indicates that our main results are robust even after restricting our observations to those with high credit ratings.

D.5 DFL Reweighting

We employ the method of DiNardo, Fortin and Lemieux (1996) to flexibly control for any timevarying bank-level fundamental risk shocks, a method that is widely used in the literature (e.g., Yagan, 2015). The DFL-reweighting procedure is less parametric but similar to the matching algorithm, and reweighting is useful when comparing outcomes across banks that differ along observable characteristics, such as NPL, given that SU banks on average have higher pre-event NPL than SI banks do. We want to reweight the sample to hold the distribution of observable traits fixed across groups. To do so, we first divide all observations into equal-sized bins, *q*, according to the traits. Then to make the within-group distribution of weights across bins equal to the original cross-bin distribution of weights in the base group, we inflate or deflate weights in every group-bin. For example, if the 2018Q4 treatment group had relatively higher NPL than the 2018Q4 control group, then the DFL method will down-weight banks with higher NPL and up-weight banks with lower NPL in the 2018Q4 treatment banks. In this way, the distribution of observable traits is fixed across the two groups.

In our case, we compare outcomes across treatment groups and over time, so we DFLreweight across 10 (2 groups \times 5 quarters 2018Q4-2019Q4) groups, *g*. We define the base group, *g*_b, as the 2018Q4 treatment group, the year before the bailout policy change. Then we divide all observations into three equal-sized bins (terciles), *q*, according to their NPL to control for the bank fundamental differences that are of most concern. Therefore, we use each observation's NPL to bin it into one of the terciles, *q*, where the bins are defined using the 2018Q4 treatment group. Columns (1) and (2) of Table D.3 report the estimates with DFL reweighting. The magnitudes of the coefficient estimates for *Treat* × *Post* are slightly bigger with DFL reweighting but comparable to our main results.

D.6 WLS Estimation with Value Weighting

Following Moon (2022) and Yagan (2015), we weight each observation by its planned issuance size or total assets, in order to make each observation in our sample contribute to the main estimates according to its economic scale. In this sense, our estimates are value weighted, so that banks with higher planned issuance size or total assets will carry higher weights in their credit spread and funding ratio estimates. The key issue with weighting by the planned issuance size or total assets in our setting is that the two factors partially determine whether banks are systemically important or unimportant, so weighting observations by the planned issuance size or total assets might potentially bias our estimates. For example, banks with very high planned issuance size or total assets are mostly control banks (see Table 1), so by overweighting observations of the control banks and under-weighting observations of treatment banks, our estimates would be likely *downward biased*. Columns (3) and (4) of Table D.3 report the results based on weighting by the planned issuance size, and Columns (5) and (6) of Table D.3 summarize the results based on weighting by total assets. As expected, the coefficient estimates for *Treat* × *Post* are all smaller than those without weighting. Overall, our main results are robust to value weighting by both planned issuance size and total assets.

D.7 Changing Treatment and Control Banks

In our previous analyses, we divided banks into treatment and control groups based on the list of systemically important banks released by PBOC on October 15, 2021. We chose this method mainly because SI banks are officially certified by PBOC, the central bank in China, which could be highly correlated with the implicit guarantee. Nevertheless, this approach could potentially lead to look-ahead bias—that is, investors cannot know precisely which banks are systemically important from the perspective of the government. To address this concern, we adopt two different ways to distinguish treatment and control banks. First, we group banks according to their type. Specifically, we label both state-owned and joint-stock commercial banks as our *control* group since these two types of banks run businesses across the country, contributing much to financial stability and embedded with higher expectations of implicit guarantee. Other types of banks, usually operating locally, are considered as the *treatment* groups. Second, we employ a widely used method to split samples into small (*treatment* group) and big (*control* group) banks according to the largest 5th percentile of total assets at the end of 2018, the year before the event.

As shown in Table D.5, we can see a great deal of overlap among the three grouping methods. For instance, in comparison to our initial way, only 3 (4) treatment banks based on the grouping method of the bank type (size) are not SI banks certified by PBOC.⁴ Therefore, we may confidently expect that there is a market-wide consensus about which banks have a high

⁴In fact, following the same sample selection criteria as described in Section 4.1, we have 18, 16, and 19 control banks defined by systemic importance, bank type, and bank size, respectively.

probability of being bailed out by the government, and changing the grouping method is unlikely to affect our main findings. This is also confirmed by the results in Table D.6. The coefficient estimates for *Treat* × *Post* in Columns (1)-(4) are all statistically significant at the 1% level and are very close to those from our main findings in terms of magnitude.

D.8 Different Sample Periods

Recall that our main results in Tables 2 and 3 use the sample from October 1, 2018, to December 31, 2019, in order to rule out a possible concern arising from the COVID-19 pandemic. We now examine whether these results are robust when changing the sample period. Specifically, we use a 3-month, 9-month, and 12-month window before and after the bailout policy shift, respectively. Table D.7 shows that the coefficient estimates for *Treat* × *Post* in all columns are statistically significant at the 1% level. Thus, our main findings are robust to different sample periods.

D.9 Alternative Methods of Dealing with Outliers

To minimize the effect of outliers in regressions, we winsorize all continuous variables at the 1st and 99th percentile levels. We repeat the main analysis by truncating all continuous variables at the 1st and 99th percentile levels in Columns (1) and (2) of Table D.8, or without any winsorization or truncation in Columns (3) and (4) of Table D.8. We find that our main findings are insensitive to different methods of dealing with outliers.

D.10 Security-Level Analysis

We examine whether our main results are robust at the security-day level. In our main analyses, we only keep the NCD issuance sample with the largest planned issuance size if a bank issues more than one NCD on the same day because our main focus is the bank-day-level data and the security-day-level data only have one-period observations without any time variation. Nevertheless, we now keep all the NCD issuance observations and repeat our main analyses using the security-day-level data. Results in Columns (5) and (6) of Table D.8 indicate both qualitatively and quantitatively similar effects of the bailout policy change on the credit spreads and funding ratios on NCD issuance, respectively.

D.11 Credit Spreads with Full Sample

Our main analyses for credit spreads are free of NCD samples with failed issuance. After the bailout policy change, some banks failed to raise any money by issuing NCDs even though the promised interest rates were much higher than before. Therefore, we expect that the impact of the bailout policy shift on credit spreads would be under-estimated in our main analyses where we only consider the successful sample.

In this section, we use the full sample and explore how this would affect the impact of the bailout policy change on credit spreads. The results in Column (7) of Table D.8 confirm our conjecture: the coefficient estimate for *Treat* \times *Post* in the full sample is slightly larger than that in the successful sample.

D.12 Funding Ratios with Successful Sample

Our main analysis for funding ratios includes NCD samples with failed issuance. The impact of the bailout policy change on funding ratios is expected to be of a smaller magnitude if we restrict our attention to the successful sample. The results in Column (8) of Table D.8 confirm our conjecture: the coefficient estimate for *Treat* \times *Post* in the successful sample is slightly smaller than that in the full sample. This also suggests that our main findings are robust to including or excluding failed NCD samples.

D.13 With Additional Control Variables

To further address the concerns that our observed data patterns in the main analyses are driven by changes in bank fundamental risks or poor bank governance, we rerun regression (9) with basic and additional controls, in which the dependent variable is credit spreads and funding ratios on NCD issuance, respectively. Table D.9 presents our findings. Columns (1) and (4) report results with additional control variables proxying for bank fundamental risks, including the return on assets ratio (ROA_{it}) and the non-performing loan ratio (NPL_{it}). Columns (2) and (5) report results with additional control variables proxying for bank governance, including the number of directors in the board ($BoardSize_{it}$), the number of board meetings ($BoardMeeting_{it}$), and the percentage of directors who are independent ($IndDirectorRatio_{it}$).⁵ In Columns (3) and (6), we add basic controls and all the additional controls (i.e. proxies for both bank fundamentals and governance). In Table D.9, we find both qualitatively and quantitatively similar results, although their magnitude becomes slightly smaller than those in Column (3) Table 2 and Column (2) in Table 3.

⁵Unfortunately, our measures of bank governance obtained from CSMAR are at the bank-year level because of data limitations, which might cause measurement errors. Thus, the regression results should be interpreted with caution.



Figure D.1. Dynamic Impact with Different Window Lengths

Notes: This figure presents the dynamic impact of the bailout policy change on the credit spreads and funding ratios on NCD issuance with different time windows before and after the event. The setting is the same as in Equation (10) except with a 60-day window in Panels A and C and a 120-day window in Panels B and D, respectively.



Figure D.2. Distribution of Propensity Score

Notes: This figure presents the kernel densities of the estimated propensity scores for SU banks (solid red line) and SI banks (dashed blue line), respectively. The propensity score is estimated using a logit model with the return on assets ratio (ROA_{it}) and the non-performing loan ratio (NPL_{it}) as independent variables, which is performed using the successful sample data at the end of 2018, the year before the event.



Figure D.3. Mean Difference between Treatment and Control Banks

Notes: This figure presents the mean differences in bank fundamental risks between the treatment group (SU banks) and control group (SI banks) and the 95% confidence intervals at the end of 2018, the year before the event. The bank fundamental risks include the return on assets ratio (ROA_{it}) and the non-performing loan ratio (NPL_{it}). All continuous variables are winsorized at the 1st and 99th percentile levels. The red circle point estimates are the mean differences for the original successful sample. The blue diamond point estimates are the mean differences for the propensity scores, which are obtained from a logit model. The orange square point estimates are the mean differences for the sample with credit ratings of AA+ and AAA.

Table D.1. Impact of the Bailout Policy Change without Filling in Missing Bank Characteristics

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance without filling in missing bank characteristics. In Column (1), the dependent variable, *Spread_{it}*, is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Column (2), the dependent variable, *FdRatio_{it}*, is the ratio of the funded size to the planned size on NCD issuance. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and *Post_i* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnDuration_{it}*), the natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)
	Spread	FdRatio
Treat \times Post	0.188*** (0.02)	-0.065*** (0.02)
Controls	Yes	Yes
Day FE	Yes	Yes
Bank FE	Yes	Yes
Rating FE	Yes	Yes
Pre-event Treatment Mean	0.208	0.823
Clusters (Bank)	246	248
Ν	14,759	15,367
R-squared	0.7968	0.3940

Table D.2. Impact of the Bailout Policy Change with Matched Samples

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance using the matched samples. Columns (1) and (2) use the PSM sample with weighting by the propensity scores, which are obtained from a logit model. Columns (3) and (4) use the sample with credit ratings of AA+ and AAA. In Columns (1) and (3), *Spread_{it}* is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2) and (4), *FdRatio_{it}* is the ratio of the funded size to the planned size on NCD issuance. *Treat_i* is a dummy equal to one if the type of bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPIssSize_{it}*), the natural logarithm of the duration of the NCD (*LnDuration_{it}*). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Sample	PSM		High Credit Rating	
	(1) (2)		(3)	(4)
	Spread	FdRatio	Spread	FdRatio
Treat \times Post	0.175*** (0.02)	-0.056*** (0.02)	0.150*** (0.02)	-0.055*** (0.02)
Controls	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.193	0.827	0.163	0.840
Clusters (Bank)	155	156	133	137
Ν	13,350	13,955	14,433	14,856
R-squared	0.7609	0.4124	0.7377	0.3759

Table D.3. Impact of the Bailout Policy Change with WLS Estimation

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance using the WLS estimation. The weights are the DFL values in Columns (1) and (2), planned issuance size in Columns (3) and (4), and total assets in Columns (5) and (6), respectively. In Columns (1), (3), and (5), $Spread_{it}$ is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2), (4), and (6), $FdRatio_{it}$ is the ratio of the funded size to the planned size on NCD issuance. *Treat_i* is a dummy equal to one if the type of bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnDuration_{it}*), the natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Weights	D	DFL Planned Issuance Size Total		Planned Issuance Size		Assets
	(1)	(2)	(3)	(4)	(5)	(6)
	Spread	FdRatio	Spread	FdRatio	Spread	FdRatio
Treat \times Post	0.215*** (0.02)	-0.079*** (0.02)	0.146*** (0.02)	-0.051*** (0.01)	0.129*** (0.02)	-0.034** (0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.227	0.805	0.227	0.802	0.227	0.802
Clusters (Bank)	287	291	296	301	296	301
Ν	19,243	20,718	19,626	21,258	19,626	21,258
R-squared	0.7736	0.4452	0.7545	0.4994	0.7442	0.3873

Table D.5. The List of Banks in the Control Group

Notes: This table presents different choices for the control banks. Column (1) displays the list of systemically important banks released by PBOC on October 15, 2021. For details, see the PBOC and CBIRC, http://www.pbc.gov.cn/goutongjiaoliu/1134 56/113469/4360688/index.html. Column (2) displays the list of banks whose type is either the state-owned commercial bank or the joint-stock commercial bank. Column (3) displays the list of banks whose total assets are among the largest 5th percentile at the end of 2018.

	(1)	(2)	(3)
Rank by Total Assets in 2018 (Top 25)	PBOC List	Bank Type	Bank Size
Industrial and Commercial Bank of China	\checkmark	\checkmark	\checkmark
China Construction Bank	\checkmark	\checkmark	\checkmark
Agricultural Bank of China	\checkmark	\checkmark	\checkmark
Bank of China	\checkmark	\checkmark	\checkmark
Bank of Communications	\checkmark	\checkmark	\checkmark
Postal Savings Bank of China	\checkmark	\checkmark	\checkmark
China Merchants Bank	\checkmark	\checkmark	\checkmark
Industrial Bank	\checkmark	\checkmark	\checkmark
Shanghai Pudong Development Bank	\checkmark	\checkmark	\checkmark
China CITIC Bank	\checkmark	\checkmark	\checkmark
China Minsheng Bank	\checkmark	\checkmark	\checkmark
China Everbright Bank	\checkmark	\checkmark	\checkmark
Ping An Bank	\checkmark	\checkmark	\checkmark
Hua Xia Bank	\checkmark	\checkmark	\checkmark
Bank of Beijing	\checkmark		\checkmark
China Guangfa Bank	\checkmark	\checkmark	\checkmark
Bank of Shanghai	\checkmark		\checkmark
Bank of Jiangsu	\checkmark		\checkmark
China Zheshang Bank		\checkmark	\checkmark
Bank of Nanjing			\checkmark
Bank of Ningbo	\checkmark		\checkmark
Huishang Bank			\checkmark
Hengfeng Bank		\checkmark	\checkmark
China Bohai Bank		\checkmark	
Shengjing Bank			

Table D.6. Impact of the Bailout Policy Change with Different Treatment Banks

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance with different treatment and control groups. In Columns (1) and (3), *Spread_{it}* is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2) and (4), *FdRatio_{it}* is the ratio of the funded size to the planned size on NCD issuance. In Columns (1) and (2), *Treat_i* is a dummy equal to one if the type of bank *i* is neither a state-owned commercial bank nor a joint-stock commercial bank and zero otherwise. In Columns (3) and (4), *Treat_i* is a dummy equal to one if the total assets of bank *i* are smaller than the largest 5th percentile of total assets at the end of 2018 and zero otherwise. *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPIssSize_{it}*), the natural logarithm of the duration of the NCD (*LnDuration_{it}*), the natural logarithm of total assets (*LnTotal Asset_{it}*), the debt-to-asset ratio (*Debt AssetRatio_{it}*), and the credit rating (*Rating_{it}*). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *,**, and ***, respectively.

Treatment	Bank Type		Banl	k Size
	(1)	(2)	(3)	(4)
	Spread	FdRatio	Spread	FdRatio
Treat \times Post	0.204*** (0.02)	-0.079*** (0.02)	0.210*** (0.02)	-0.073*** (0.02)
Controls	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.221	0.802	0.219	0.804
Clusters (Bank)	296	301	273	277
Ν	19,626	21,258	19,006	20,368
R-squared	0.7780	0.4513	0.7790	0.4388

Table D.7. Impact of the Bailout Policy Change with Different Sample Periods

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance using different sample periods. In Columns (1), (3), and (5), $Spread_{it}$ is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2), (4), and (6), $FdRatio_{it}$ is the ratio of the funded size to the planned size on NCD issuance. *Treat_i* is a dummy equal to one if the type of bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, *Post_t* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPIssSize_{it}*), the natural logarithm of the duration of the NCD (*LnDuration_{it}*). In Columns (1) and (2), the sample is from February 24, 2019, to August 24, 2019. In Columns (3) and (4), the sample is from August 24, 2018, to February 24, 2020. In Columns (5) and (6), the sample is from May 24, 2018, to May 24, 2020. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Sample	$\pm 3 \mathrm{M}$	± 3 Months ± 9 Months ± 12 Mon		± 9 Months		Aonths
	(1)	(2)	(3)	(3) (4)		(6)
	Spread	FdRatio	Spread	FdRatio	Spread	FdRatio
Treat \times Post	0.256*** (0.02)	-0.089*** (0.02)	0.187*** (0.02)	-0.074*** (0.02)	0.194*** (0.02)	-0.071*** (0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.207	0.771	0.258	0.804	0.271	0.803
Clusters (Bank)	251	256	300	305	317	322
Ν	7,899	8,680	22,923	24,658	30,662	32,698
R-squared	0.7926	0.5185	0.7837	0.4317	0.7595	0.4051

Table D.8. Impact of the Bailout Policy Change with Additional Sample Selection Criteria

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are credit spreads or funding ratios on NCD issuance using additional sample selection criteria. In Columns (1) and (2), all continuous variables are truncated at the 1st and 99th percentile levels. In Columns (3) and (4), all continuous variables are neither winsorized nor truncated. In Columns (5) and (6), the data are at the security level. Column (7) uses the full sample, and Column (8) uses the successful sample. In Columns (1), (3), (5), and (7), *Spread*_{ii} is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (2), (4), (6), and (8), *FdRatio*_{it} is the ratio of the funded size to the planned size on NCD issuance. *Treat*_i is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPIssSize*_{it}), the natural logarithm of the duration of the NCD (*LnDuration*_{it}). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels except in Columns (5)-(8). Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

Sample	Т	rim	No Winsor or Trim		n Security Level		Full	Successful
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Spread	FdRatio	Spread	FdRatio	Spread	FdRatio	Spread	FdRatio
Treat \times Post	0.203*** (0.02)	-0.075*** (0.02)	0.213*** (0.02)	-0.079*** (0.02)	0.205*** (0.02)	-0.101*** (0.03)	0.222*** (0.02)	-0.060*** (0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.224	0.806	0.227	0.802	0.216	0.768	0.234	0.856
Clusters (Bank)	273	279	296	301	310	315	301	296
Ν	18,350	20,280	19,626	21,258	32,128	38,400	21,258	19,626
R-squared	0.7646	0.4459	0.7805	0.4513	0.7791	0.5126	0.8000	0.3240

Table D.9. Impact of the Bailout Policy Change with Additional Controls

Notes: This table provides the estimation results from a DiD regression with additional control variables. In Columns (1)-(3), the dependent variable, $Spread_{it}$, is the credit spreads on NCD issuance equal to the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. In Columns (4)-(6), the dependent variable, $FdRatio_{it}$, is the ratio of the funded size to the planned size on NCD issuance. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and *Post_i* is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD (*LnPIssSize_{it}*), the natural logarithm of the duration of the NCD (*LnDuration_{it}*). He natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). Additional control variables proxying for bank fundamental risks include the return on assets ratio (*ROA_{it}*) and the non-performing loan ratio (*NPL_{it}*). Additional control variables proxying for bank governance include the number of directors on the board (*BoardSize_{it}*), the sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Spread	Spread	Spread	FdRatio	FdRatio	FdRatio
Treat \times Post	0.204*** (0.02)	0.202*** (0.02)	0.198*** (0.02)	-0.074*** (0.02)	-0.060*** (0.02)	-0.061*** (0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fundamentals	Yes	No	Yes	Yes	No	Yes
Bank Governance	No	Yes	Yes	No	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event Treatment Mean	0.218	0.191	0.189	0.805	0.802	0.807
Clusters (Bank)	261	155	153	264	156	153
Ν	18,728	13,463	13,276	20,080	14,201	13,992
R-squared	0.7789	0.7866	0.7883	0.4310	0.4344	0.4345

E More Discussion on Risk Awareness

In Section 6.1 of the main text, we provide empirical evidence to address the risk awareness concern. In this section, we provide further discussion and additional findings regarding the risk awareness mechanism.

E.1 Other Relevant Events

The collapse of Baoshang Bank did not occur overnight. In this section, we consider three important events before the bailout policy change in May 2019, which, in theory, should increase the market-perceived probability of Baoshang's failure ($\phi_{i,t}$ in our model). We investigate the reaction in the NCD market surrounding these three events. If the risk awareness channel is the dominant mechanism that accounts for the observed data pattern on credit spreads and funding ratios surrounding the bailout policy change, then we should expect an impact on the NCD market with a greater or similar magnitude during these three episodes.

As discussed in Section 2.3, Tomorrow Holding, the controlling shareholder of Baoshang Bank, created significant credit risks for the bank because of fraudulent loan transactions. In this regard, the first event that we are looking into is that the owner of Tomorrow Holding was apprehended on charges related to financial misconduct on January 28, 2017.⁶ The second event is Baoshang Bank's announcement of the delayed release of the 2017 annual financial statements on April 28, 2018. The third event is Baoshang Bank's announcement of delayed release of the 2017 annual financial statements on June 28, 2018, again citing its plan to seek strategic investors.⁷

In Figure E.1, we show that following each event, the differences in credit spreads and funding ratios on NCD issuance among "similar banks," other SU banks, and SI banks do not exhibit notable and persistent changes, unlike the impact observed in the bailout policy change. Given the aforementioned three events, which might signify a significant change in Baoshang Bank's fundamentals and potentially heighten investors' perception of risks, we provide another piece of evidence to dismiss the risk awareness mechanism.

E.2 Heterogeneous Impact of the Bailout Policy Change across Provinces

Another way of defining "similar banks" is by considering a bank's geographical distance from Baoshang Bank. Therefore, according to the channel of increased risk awareness, NCD investors may become increasingly aware of the risks associated with banks located in close

⁶For details, see Jamil Anderlini, Ben Bland, Gloria Cheung, and Lucy Hornby, "Chinese Billionaire Abducted from Hong Kong," Financial Times, February 1, 2017, https://www.ft.com/content/8e54c51c-e7a7-11e6-893 c-082c54a7f539.

⁷In fact, Baoshang Bank has not published any annual reports since then. For details, see Cheng Leng, Ryan Woo, and Shu Zhang, "Chinese Regulator to Take Over Baoshang Bank Due to Credit Risks," Reuters, May 24, 2019, https://www.reuters.com/article/idUSKCN1SU1DN.

geographical proximity to Baoshang Bank.

To alleviate this concern, we explore the heterogeneous impact of the bailout policy shift across provinces by running the following regression with a series of dummy variables:

(E.1)
$$Y_{ijt} = \alpha + \beta_0 SI_i \times Post_t + \sum_{j=1}^{30} \beta_j ProvSU_{ij} \times Post_t + X_{ijt}\Gamma + \mu_i + \lambda_t + \epsilon_{ijt},$$

where the subscripts *i*, *j*, and *t* denote bank, province, and day, respectively. The dependent variable Y_{ijt} is credit spreads (*Spread*_{ijt}) or funding ratios (*FdRatio*_{ijt}) on NCD issuance, *SI*_i is a dummy equal to one if bank *i* is systemically important as certified by the PBOC and zero otherwise, and *ProvSU*_{ij} is a dummy equal to one if the registration address of SU bank *i* is in province *j* and zero otherwise.⁸ Other variables are the same as those in Equation (9). For ease of interpretation, we normalize the point estimate for *SI* × *Post* to zero, which enables us to estimate the heterogeneous effects of the bailout policy change on the credit spreads and funding ratios among SU banks registered in different provinces relative to the impact of the event for SI banks. The standard errors are clustered at the bank level.

Figure E.2 plots these coefficient estimates and indicates significant variations in the heterogeneous impact of the bailout policy change on credit spreads and funding ratios across provinces. Specifically, Panel A displays the results for credit spreads on NCD issuance. The coefficient estimates for *ProvSU* × *Post* range from 1.0 bps in Beijing to 61.2 bps in Jilin. Panel B displays the results for funding ratios on NCD issuance. The coefficient estimates for *ProvSU* × *Post* range from -44.8 percentage points in Qinghai to 9.9 percentage points in Beijing. In particular, Figure E.2 shows that the effects of the bailout policy shift on credit spreads and funding ratios in Inner Mongolia, where Baoshang Bank was registered, rank as the third and ninth largest, respectively.

If NCD investors become more aware of the risks associated with banks registered near Baoshang Bank, we would expect the bailout policy change to have a greater impact on banks located close to Baoshang Bank. Conversely, banks located far away from Baoshang Bank would experience a weaker impact. However, when we examine the data shown in Figure E.2, we do not observe such a pattern across provinces. This finding alleviates concerns related to the risk awareness channel.

⁸Our sample covers banks registered in 31 provinces of mainland China. Since there is only one bank in Tibet during our sample period, the point estimate for $ProvSU \times Post$ in Tibet is absorbed by the fixed effects. Therefore, we have coefficient estimates for 30 provinces in total.



Figure E.1. Daily Average Credit Spreads and Funding Ratios on Other Events

Notes: This figure presents the daily average credit spreads and funding ratios on NCD issuance from July 1, 2016, to December 31, 2018. The first event day is January 28, 2017, when Jianhua Xiao, the owner of Tomorrow Holding, was arrested by the Chinese government. The second event day is April 28, 2018, when Baoshang Bank announced that it would delay its release of the 2017 annual report. The third event day is June 28, 2018, when Baoshang Bank again announced that it would delay its release of the 2017 annual report. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. Panel A plots the simple average of *Spread_{it}*, which is the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. Panel B plots the simple average of *FdRatio_{it}*, which is the ratio of the funded size to the planned size on NCD issuance.



Figure E.2. Impact of the Bailout Policy Change across Provinces

Notes: This figure presents the heterogeneous impact of the bailout policy change across provinces. Panel A and Panel B present the impact on credit spreads and funding ratios on NCD issuance, respectively. Provinces in darker red have a higher magnitude of coefficient estimates for $ProvSU_{ij} \times Post_t$ estimated from the Equation (E.1), where the point estimate for $SI_i \times Post_t$ is normalized to zero. The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels.

F More Discussion on Fundamental Contagion

In Section 6.2 of the main text, we discuss the potential mechanism of fundamental contagion. In this section, we provide detailed results regarding whether the impact of the bailout policy change on bank fundamentals varies significantly between SI and SU banks by running the regression in Equation (11). The results in Table F.1 show that the coefficient estimates for *Treat* × *Post* are all statistically insignificant in Columns (1) and (2), indicating that there is no significant difference in the changes in bank fundamental risks following the event, including ROA and NPL, between SU and SI banks. As a result, the changes in bank fundamental risks cannot account for the observed widening changes in credit spreads and funding ratios between SU and SI banks.

Table F.1. Impact of the Bailout Policy Change on Bank Fundamental Risks

Notes: This table provides the estimation results from a DiD regression in which the dependent variables are proxies for bank fundamental risks. In Column (1), ROA_{it} is the return on assets ratio. In Column (2), NPL_{it} is the non-performing loan ratio. *Treat_i* is a dummy equal to one if the type of bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and $Post_t$ is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of total assets (*LnTotalAsset_{it}*), the debt-to-asset ratio (*DebtAssetRatio_{it}*), and the credit rating (*Rating_{it}*). The sample is from 2018Q4 to 2019Q4. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)
	ROA	NPL
Treat \times Post	-0.020 (0.01)	-0.022 (0.03)
Controls	Yes	Yes
Quarter FE	Yes	Yes
Bank FE	Yes	Yes
Rating FE	Yes	Yes
Pre-event Treatment Mean	0.912	1.908
Clusters (Bank)	286	304
Ν	1,204	1,269
R-squared	0.9028	0.9514

G Placebo Tests for Market-wide Liquidity Shortage

In Section 6.3 of the main text, we discuss the potential mechanism of a market-wide liquidity shortage. In this section, we provide detailed evidence on a placebo test to rule out this mechanism. On October 25, 2016, it was rumored that the PBOC intended to incorporate offbalance-sheet wealth management products (hereafter, WMPs) into the monitoring checklist of the macroprudential assessment (MPA) framework,⁹ which we refer to as the "WMP event." WMPs are short-term off-balance-sheet products offered by banks to retail investors as substitutes for deposits. They are a form of regulatory arbitrage that enables banks to evade stringent off-balance-sheet regulations (e.g., the loan-to-deposit ratio).¹⁰ This news has the potential to have a significant impact on market liquidity, as banks and other financial institutions in China rely heavily on WMPs to meet their liquidity needs (Acharya, Qian, Su and Yang, 2022).

We employ the Baidu search index—China's most popular search engine—to confirm that October 25, 2016, was the date on which this rumor of "stricter regulations on WMPs" garnered widespread attention. Panel A of Figure G.1 plots the number of online searches in Chinese for the terms "off-balance-sheet wealth management products" and "macroprudential assessment" on Baidu from September 1, 2016, to January 31, 2017. The amount of attention paid to both phrases increases dramatically on October 25, 2016, reaching its first peak the following day. A similar search volume pattern appears around the second peak, when the PBOC officially confirmed the rumor.¹¹

As a result, we choose October 25, 2016, as the date of our placebo event. After the rumor spreads, the market should anticipate a significant reduction in the size of WMPs once monitored and regulated under the MPA framework. That anticipation can cause the market to panic early. Panel B of Figure G.1 confirms that the dissemination of this rumor immediately resulted in market-wide liquidity distress, as the three-month Shibor interest rate began to rise dramatically on October 25, 2016, and thereafter. In this sense, this placebo event is appropriate because it enables us to identify how a liquidity shortage affects the funding conditions in the NCD market, specifically the differences between SI and SU banks.

If the observed pattern in NCD credit spreads and funding ratios around the bailout policy change is entirely a result of market reactions to deteriorating liquidity conditions, then we

⁹For instance, this rumor was formally reported on October 26, 2016, by Tencent Finance. For details, see "Comment on the Central Bank's 'Notice on Including Off-Balance Sheet Wealth Management Business in the "Broad Credit" Calculation': The Impact of the Inclusion of Wealth Management Products in MPA is Very Small," Tencent Finance, October 26, 2016, https://finance.qq.com/a/20161026/031085.htm.

¹⁰Furthermore, unlike deposits, banks can freely adjust the interest rate on WMPs because they are not subject to the PBOC's deposit rate ceiling. See Acharya, Qian, Su and Yang (2022) for a more detailed discussion on WMPs.

¹¹The PBOC officially confirmed the news on December 19, 2016, and announced that the new regulation would be implemented beginning in the first quarter of 2017, as reported by Reuters. See "China Central Bank to Count Off-Balance Sheet Wealth Management Products in Assessing Banks' Risk: Sources," Reuters, December 19, 2016, https://www.reuters.com/article/us-china-pboc-shadowbanking-idUSKBN14813T for details.

should expect to see a pattern similar to that observed around the "WMP event." Nonetheless, the results in Figure G.1 reveal a strikingly different pattern. As illustrated in Panel B of Figure G.1, the liquidity distress increases credit spreads on NCD issuance for both SU and SI banks, with no discernible difference in magnitude. In addition to the price effects, we also find that the liquidity shock has little influence on the funding ratios, as shown in Panels C and D of Figure G.1.

In summary, the placebo test provides additional evidence to refute the alternative explanation that the difference in both credit spread and funding ratio changes between SU and SI banks following the bailout policy change is mainly a result of its impact on liquidity conditions.



Figure G.1. Placebo Test for Liquidity Shortage

Notes: This figure presents the placebo test in which the market is short of liquidity. Panel A shows the search intensity for news on the terms "off-balance-sheet wealth management products" and "macroprudential assessment" in Chinese on Baidu, where the sample is from September 1, 2016, to January 31, 2017. The intensity of the two lines both peak on October 26, 2016, and December 20, 2016. In Panels B-D, the sample is from April 1, 2016, to June 30, 2017, and the placebo event date is October 25, 2016, when the market anticipated banks' off-balance-sheet wealth management products to be included in the monitoring checklist under the macroprudential assessment framework. *Treat_i* is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. Panel B plots the simple average of *Spread_{it}*, which is the difference between the issuance interest rate on the NCD and the Shibor interest rate with the same term to maturity on the same day. Panel C plots the simple average of *FdRatio_{it}*, which is the ratio of the funded size to the planned size on NCD issuance. Panel D plots the moving average of *FdRatio_{it}* with a 15-day window.

H More Discussion on Credit Allocation

In Section 7.2 of the main text, we explore how the implicit non-guarantee affects credit allocation. In this section, we provide further discussion and additional findings regarding the credit allocation.

H.1 Impacts on Funded Sizes

We first examine whether SU banks raised less money through the NCD market after the bailout policy change compared with SI banks. In more detail, we use the full sample and perform a DiD estimation similar to Equation (9) but with the natural logarithm of the funded size on NCD issuance ($LnFdSize_{it}$) as the dependent variable. Table H.1 summarizes our findings. In Column (1), when only controlling the bank and day fixed effects, the coefficient estimate for $Treat \times Post$ is -0.377 and is significant at the 1% level, suggesting that SU banks reduced their funded size on NCD issuance by 37.7 log points after the event, relative to SI banks. In Column (2), we also add a set of controls, including the natural logarithm of the duration of the NCD, the natural logarithm of total assets, the debt-to-asset ratio, and the credit rating, and results show that the coefficient estimate for $Treat \times Post$ is quite similar in terms of magnitude. We have a similar conclusion in Column (3) even when further including the natural logarithm of the planned issuance size as one of the control variables, which is highly correlated with the natural logarithm of the funded size. Overall, Table H.1 suggests that in comparison to SI banks, SU banks did raise less money in the NCD market after the event.

H.2 More Discussion on Credit Rating Sensitivity

In Section 7.1, we find that price efficiency is improved in terms of credit ratings following the bailout policy change. In this section, we provide additional evidence on how the bailout policy shift changed the association between banks' credit ratings and funding ratios or the funded size on NCD issuance.

Before conducting our formal analysis, we first check the variation in credit ratings. For SU banks, the credit rating shows some variation during our sample period, with AA+ accounting for the highest proportion: 34.45% of total observations. However, all SI banks have the highest credit rating, AAA, with no cross-sectional or time-series variation.¹² As a result, our analysis of credit rating sensitivity is limited to SU banks.

Figure H.1 plots the daily average funding ratios and funded size on NCD issuance with high credit ratings (AA+ and AAA) and low credit ratings (below AA+) for SU and SI banks,

¹²During our sample period, only a few SU banks have ever experienced changes in credit ratings. To be precise, only 1 (0.01%) SU bank has been downgraded, and 36 (0.42%) SU banks have been upgraded during the pre-event period; only 11 (0.12 %) SU banks have been downgraded, and 32 (0.36 %) SU banks have been upgraded during the post-event period.

respectively. Panel A summarizes two findings: (1) the bailout policy change decreased the funding ratios for SU banks while having little influence on SI banks; (2) the shock decreased the funding ratios much more for SU banks with low credit ratings compared with SU banks with high credit ratings. This finding suggests that short-term credit was allocated much more to banks with better fundamentals following the bailout policy shift. We find similar patterns for the funded size on NCD issuance in Panel B, although the data pattern is slightly weaker. These results are consistent with those in Section 7.2.

H.3 Impact on Mutual Fund Holdings

In Section 7.2 of the main text, our analysis of credit allocation about funding sizes focuses on the aggregate pattern observed in the primary market of NCDs. In this section, we present some direct but suggestive evidence that investors treated NCDs issued by SU banks differently from those issued by SI banks in the aftermath of the bailout policy change. Data limitations prevented us from being able to track daily purchases or redemptions of NCDs by any type of investor. For that reason, we resort to quarterly money market mutual fund disclosures of the top 10 holdings of fixed-income securities. Notably, mutual funds, particularly money market funds, are a significant player in the NCD market.¹³

In our data analyses, we examine the reported holdings of NCDs issued by SI or SU banks before and after the bailout policy change.¹⁴ Our conjecture is that the implicit non-guarantee induced by the bailout policy change would dissuade fund managers from holding NCDs issued by SU banks, but not those issued by SI banks. The underlying reason is that the implicit non-guarantee applies only to SU banks but not to SI banks.

Using the quarterly disclosed top 10 bond holdings data, we construct several measures directly related to fund managers' holdings of NCDs issued by SU and SI banks. We first calculate the natural logarithm of the total holding shares of NCDs issued by SI (s = I) or SU (s = U) bank for fund *j* at time *t*, $LnHS_{sjt}$. Likewise, we then compute, for each fund *j* at time *t*, the ratio of the total holding shares of NCDs issued by SI or SU banks to fund *j*'s total holding shares of NCDs, $RelativeHS_{sjt}$, and the ratio of the total holding values of NCDs issued by SI or SU banks to fund *j*'s net asset value, $HVNAV_{sjt}$.

Results are reported in Table H.2. The coefficient estimates for *Treat* × *Post* in all columns are negative and statistically significant at the 1% level, indicating that, in the aftermath of the bailout policy change, mutual fund managers unloaded more NCDs issued by SU banks relative to those issued by SI banks. These effects are also economically large. For example, the coefficient estimate for *Treat* × *Post* in Column (1) is -0.281, suggesting that, on average, mutual fund managers decreased their holding shares of NCDs issued by SU banks by 28.1 log

¹³According to the Asset Management Association of China, only market mutual funds should disclose the detailed bond holdings of the top 10 ranked by market value in their quarterly reports.

¹⁴It is worth noting that, while our primary empirical analysis focuses on NCD issuance, implicit non-guarantee should have a similar effect on outstanding NCDs.

points, compared with those issued by SI banks after the bailout policy shift.

From the funding holdings data, we find some direct but suggestive evidence documenting how the financial market responded to the bailout policy change. The evidence that fund managers significantly reduced their holdings of NCDs issued by SU banks in comparison to NCDs issued by SI banks further demonstrates that systemic importance is a critical factor in understanding the market response. Interestingly, when combined with evidence from credit spreads, funding ratios, and funded size on NCD issuance, our empirical findings suggest that the implicit non-guarantee can be the underlying reason for the observed debt "runs." This, in turn, significantly harmed SU banks' liquidity positions, resulting in severe liquidity distress for some of them—for example, Bank of Jinzhou.

H.4 Details on Credit Lines

In this section, we provide more details on credit lines, including the institutional background, underlying data sources, data limitations, sample selection, and summary statistics of credit line data.

Institutional Background on Credit Lines

According to No.31 [1999] of the People's Bank of China, the credit line in China refers to a credit risk management system in which a commercial bank determines a maximum amount of the comprehensive credit line for a single corporate client or region and controls it in a centralized and unified way, including the unified comprehensive granting of credits in domestic and foreign currencies in the forms of loan, trade financing (such as packing loan and import and export finance), discount, acceptance, letter of credit, letter of guaranty, security, and so on, on and off the balance sheet.¹⁵ This definition closely parallels the U.S. concept, where a credit line is described as a forward contract that allows a company to borrow as needed from a predetermined limit over a specified time interval.

Credit lines between China and the U.S. have several distinctions. In the U.S., bank lines of credit primarily serve short-term liquidity needs, whereas in China, they cater to both short-term liquidity management and long-term investments. Second, about 80% (70%) of aggregate bank lending to listed (small) firms arises from credit line drawdowns (Kashyap, Rajan and Stein, 2002; Sufi, 2009), whereas in China, having a credit line is often a prerequisite for bank loans. Lastly, U.S. firms typically incur commitment fees in their credit line contracts, whereas Chinese firms may not pay such fees but might engage in alternative arrangements such as making deposits with the bank.

¹⁵See http://www.pbc.gov.cn/chubanwu/114566/114579/114658/2833904/index.html for details.

Data Sources and Data Limitations

We collect the credit line data at the firm-bank-quarter level from WIND, where the underlying data resource is firms' bond issuance prospectuses.¹⁶ Consequently, our sample is limited to non-financial firms with bond issuance. In China, there are three types of corporate bonds: enterprise bonds, exchange-traded corporate bonds, and debt financing instruments of nonfinancial enterprises (including medium-term notes, commercial papers, asset-backed securities, private placement notes, and so on).¹⁷ To the best of our knowledge, Table H.3 presents the earliest policies on bond issuance requiring the disclosure of credit line data for each of the three bond types.¹⁸ All three types of bond issuance have been required to disclose credit line data since November 30, 2015. Consequently, the credit line data in our sample period from 2018Q4 to 2019Q4 are relatively less affected by disclosure concerns.

Nevertheless, our credit line data have several limitations. One caveat about our data is that among all the bond types, private placement notes face much fewer stringent requirements for information disclosure and their issuance prospectuses are not public, which means we cannot obtain credit line data from the issuance prospectuses of private placement notes. It is also worth noting that corporate bonds are mainly issued by publicly listed firms, large state-owned enterprises (SOEs), and prominent private enterprises (Amstad and He, 2020). Therefore, our sample is biased toward relatively large firms with alternative financing vehicles besides bank loans. Furthermore, the credit line data are available for a firm in certain quarters only if that firm issues a bond according, which means that it is difficult to get credit line data for a firm in each quarter.

Sample Selection and Summary Statistics

We collect quarterly firm and bank characteristics from WIND. Our original credit line data are at the bond-firm-bank-quarter level, and our data analysis involves the following screening criteria. First, we keep samples in which credit lines are obtained from SU or SI banks.¹⁹ Then we filter out credit lines denominated in currencies other than RMB. We also delete samples with missing data for firm names, bank names, total credit lines, or used credit lines. Finally, we eliminate erroneous samples with non-positive total credit lines, negative used credit lines,

¹⁶Over the last two decades, China's bond market has experienced remarkable growth, with bond market capitalization relative to GDP increasing from 35% in 2008 to over 98% in 2019 (Amstad and He, 2020).

¹⁷See Amstad and He (2020) for a detailed discussion on bond types in China.

¹⁸To be specific, the policy mandates that bond issuers disclose the credit lines provided by major banks, including the total credit lines, the used credit lines, and the unused credit lines.

¹⁹In firms' bond issuance prospectuses, there are cases in which credit lines are granted by non-bank financial institutions or policy banks. However, given our primary focus on SU and SI banks, we have removed 18,789 observations involving non-bank entities or policy banks in this step, making up approximately 15.2% of the original total observations.

or negative unused credit lines.²⁰ At this stage, we get credit line data at the firm-bank-quarter level and merge that data with firm or bank characteristics for our subsequent regression analyses.

Because of data availability, our sample is restricted to non-financial firms with bond issuance. Since credit line data are accessible solely when disclosed by firms in their bond issuance prospectuses, our dataset consists of only a limited fraction of firms that continuously provide credit line information every quarter. Given these data limitations, our analysis centers on 105 non-financial firms that report both total and used credit line data in each quarter throughout our sample period from 2018Q4 to 2019Q4. Among the 105 firms, there are 12 central SOEs, 86 local SOEs, and 7 private firms. In terms of listing status, 7 of them are publicly listed firms while 98 of them are not listed.

The descriptive statistics for the main variables are presented in Table H.4, where we winsorize all continuous variables at the 1st and 99th percentile levels. All variables are on a quarterly basis. Panel A presents summary statistics for the 105 non-financial firms mentioned above. The mean of the total assets for all firms is 132.4 billion RMB, suggesting that the firms in our sample are of large sizes. The mean of the total credit line for all firms is 71.0 billion RMB, and about 41.3% of the total credit line is used on average. In our regression analysis, we categorize our sample into two groups based on firms' exposure to SU banks. We define a dummy variable, $HighExpSU_i$, which equals one (zero) if firm *i*'s ratio of total credit lines granted by all SU banks to total credit lines granted by all banks falls within the top (bottom) 1/3 percentile at the end of 2018. In comparison to firms that rely more on SI banks, those with higher exposure to SU banks exhibit lower total credit lines, used credit lines, total assets, and debt-to-asset ratios. Panel B provides summary statistics for the banks extending credit lines to the 105 non-financial firms mentioned above. Our sample includes 215 banks, where each bank extends credit lines to an average (median) of 8.7 (1.3) firms. Compared with SI banks, SU banks have lower total credit lines, used credit lines, total assets ratios.

²⁰In the preceding three steps, we drop 463, 310, and 77 observations, accounting for about 0.4%, 0.3%, and 0.1% of total original observations, respectively.



Figure H.1. Daily Average Plots with Different Credit Ratings

Notes: This figure presents the daily average funding ratios and funded size on NCD issuance with different credit ratings from October 1, 2018, to December 31, 2019, and the event day is May 24, 2019. $FdRatio_{it}$ is the ratio of the funded size to the planned size on NCD issuance, which is calculated using the full sample. $LnFdSize_{it}$ is the natural logarithm of the funded size on NCD issuance, which is calculated using the full sample. $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise. $HRating_{it}$ is a dummy equal to one if the credit rating is AA+ or AAA and zero otherwise. Panel A plots the simple average of $FdRatio_{it}$. Panel B plots the simple average of $LnFdSize_{it}$.

Table H.1. Impact of the Bailout Policy Change on Funded Sizes

Notes: This table provides the estimation results from a DiD regression in which the dependent variable, $LnFdSize_{it}$, is the natural logarithm of the funded size on NCD issuance. $Treat_i$ is a dummy equal to one if bank *i* is systemically unimportant as certified by the PBOC and zero otherwise, and $Post_i$ is a dummy equal to one if the observation is after the event day and zero otherwise. Control variables include the natural logarithm of the planned issuance size of the NCD ($LnPlanSize_{it}$), the natural logarithm of the duration of the NCD ($LnDuration_{it}$), the natural logarithm of total assets ($LnTotalAsset_{it}$), the debt-to-asset ratio ($DebtAssetRatio_{it}$), and the credit rating ($Rating_{it}$). The sample is from October 1, 2018, to December 31, 2019. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the bank level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	LnFdSize	LnFdSize	LnFdSize
Treat \times Post	-0.377*** (0.08)	-0.358*** (0.08)	-0.080*** (0.03)
LnPlanSize			0.720*** (0.02)
LnDuration		-0.036*** (0.01)	-0.019*** (0.01)
LnTotalAsset		0.575** (0.29)	0.333 (0.25)
DebtAssetRatio		-0.025** (0.01)	-0.018 (0.02)
Day FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Rating FE	No	Yes	Yes
Pre-event Treatment Mean	1.351	1.402	1.402
Clusters (Bank)	393	301	301
Ν	22,723	21,258	21,258
R-squared	0.5310	0.5255	0.8463

Table H.2. Impact of the Bailout Policy Change on Mutual Fund Holdings

Notes: This table presents the estimation results from a DiD regression in which the dependent variable is the mutual fund holdings of NCDs. In Column (1), $LnHS_{sjt}$ is the natural logarithm of the total holding shares of NCDs issued by SU (s = U) or SI (s = I) banks for fund *j*. In Column (2), $RelativeHS_{sjt}$ is the ratio of the total holding shares of NCDs issued by SU (s = U) or SI (s = I) banks to fund *j*'s total holding values of NCDs. In Column (3), $HVNAV_{sjt}$ is the ratio of the total holding values of NCDs issued by SU (s = U) or SI (s = I) banks to fund *j*'s total holding values of NCDs. In Column (3), $HVNAV_{sjt}$ is the ratio of the total holding values of NCDs issued by SU (s = U) or SI (s = I) banks to fund *j*'s net asset value. The sample is from 2018Q4 to 2019Q4. All continuous variables are winsorized at the 1st and 99th percentile levels. Robust standard errors clustering at the treatment-fund level are displayed in parentheses. Significance levels at 10%, 5%, and 1% are indicated by *, **, and ***, respectively.

	LnHS	RelativeHS	HVNAV
Treat \times Post	-0.281*** (0.07)	-13.913*** (2.22)	-2.509*** (0.70)
Quarter FE	Yes	Yes	Yes
Treat FE	Yes	Yes	Yes
Fund FE	Yes	Yes	Yes
Pre-event Treatment Mean	6.112	47.007	9.938
Clusters (Treat-Fund)	632	632	632
Ν	2,694	2,694	2,694
R-squared	0.8249	0.3230	0.5593

Table H.3. Policies Regarding the Disclosure of Credit Line Data

Notes: This table presents the earliest policies on corporate bond issuance that require the disclosure of credit line data for enterprise bonds, exchange-traded corporate bonds, and debt financing instruments of non-financial enterprises.

Bond	Document	Document	Issuing	Issuing	Effective	Official
Туре	Number	Name	Authority	Date	Date	Announcement
Enterprise Bond	Annex 2 to No. 3127 [2015]	The Guidelines on Informa- tion Disclosure for Issuance of Enterprise Bonds	NDRC	2015-11-30	2015-11-30	https://www.ndrc.gov.c n/xxgk/zcfb/tz/201512/ t20151202_963511.html
Exchange-traded Corporate Bond	No. 224 [2007]	The Standards for the Con- tents and Formats of Infor- mation Disclosure by Com- panies Offering Securities to the Public No. 23: Prospec- tus for Public Issuance of Corporate Bonds	CSRC	2007-08-15	2007-08-15	https://neris.csrc.gov .cn/falvfagui/rdqsHead er/mainbody?navbarId=3 &secFutrsLawId=4df06a7 742704cc88beda7d5ee502 beb
Debt Financing Instrument of Non-financial Enterprises	No. 2 [2008]	Guidelines for the Prospec- tus for Debt Financing In- struments of Non-Financial Enterprises in the Interbank Bond Market	NAFMII	2008-04-15	2008-04-15	https://www.nafmii.org .cn/ggtz/gg/201204/t20 120406_197876.html

Table H.4. Summary Statistics of Credit Lines

Notes: This table reports the summary statistics of the main variables for firms and banks with credit line data. Panel A describes the summary statistics of the total credit lines ($FTotalCL_{it}$), used credit lines ($FUsedCL_{it}$), total assets ($FTotalAsset_{it}$), and debt-to-asset ratio ($FDebtAssetRatio_{it}$) for firms. Panel B describes the summary statistics of the total credit lines ($BTotalCL_{it}$), used credit lines ($BUsedCL_{it}$), total assets ($TotalAsset_{it}$), used credit lines ($BUsedCL_{it}$), total assets ($TotalAsset_{it}$), and debt-to-asset ratio ($DebtAssetRatio_{it}$) for banks. The sample period is from 2018Q4 to 2019Q4.

Panel A. Firm Characteristics									
	All			High Exposure to SU Bank			Low Exposure to SU Bank		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	SD	Ν	Mean	SD	Ν	Mean	SD	Ν
FTotalCL (Billion RMB)	70.971	118.937	525	20.592	15.520	175	149.361	175.281	175
FUsedCL (Billion RMB)	29.320	41.284	525	12.033	10.383	175	56.785	58.804	175
FTotalAsset (Billion RMB)	132.394	159.576	525	61.832	44.070	175	235.338	221.892	175
FDebtAssetRatio (%)	63.252	10.402	525	62.386	10.553	175	66.037	9.580	175
Panel B. Bank Characteristics									
	All			SU Bank			SI Bank		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	SD	Ν	Mean	SD	Ν	Mean	SD	Ν
BTotalCL (Billion RMB)	43.206	159.584	872	3.121	7.674	777	371.058	337.014	95
BUsedCL (Billion RMB)	17.522	65.313	872	1.674	3.357	777	147.136	142.756	95
TotalAsset (Trillion RMB)	1.596	4.573	648	0.290	0.320	553	9.196	8.656	95
DebtAssetRatio (%)	91.625	2.904	647	91.489	3.104	552	92.410	0.863	95

I Construction of Cumulative Abnormal Return

In Section 7.3 of the main text, we examine how stock market investors would respond to the implicit non-guarantee induced by the bailout policy change. In this section, we provide details on how to measure the cumulative abnormal return (*CARet*). To measure the stock market reactions to the bailout policy shift, we calculate the cumulative abnormal return centered on the event day on May 24, 2019, using two risk models: the market model and the market-adjusted return model. For the market model, we estimate the following regression to obtain the abnormal return:

(I.1)
$$Ret_{it} = \alpha_i + \beta_i RM_t + \epsilon_{it},$$

where the subscripts *i* and *t* denote stock and day, respectively, Ret_{it} is the return on stock *i* on day *t*, and RM_t is the value-weighted market return on day *t*. The model is estimated for each bank over the 120-day window with a minimum of 30 observations prior to the event day to gain the estimators $\hat{\alpha}_i$ and $\hat{\beta}_i$.²¹ Next, we can calculate the abnormal return as $ARet_{i\tau} = Reti\tau - (\hat{\alpha}_i + \hat{\beta}_i RM_{it})$ over the event window ($\tau = -n, \dots, -1, 0, 1, \dots, n$). Then the cumulative abnormal return is calculated as $CARet_i^{[-n,n]} = \sum_{\tau=-n}^n ARet_{i\tau}$ using an *n*-day window around the event. For robustness, we use the market-adjusted return model to calculate the abnormal return, which is defined as the stock return minus the value-weighted market return. In addition, we also calculate the standardized cumulative abnormal return as

(I.2)
$$SCARet_i^{[-n,n]} = \frac{CARet_i^{[-n,n]}}{\sqrt{N \times \sigma_{\epsilon_{ARet_i}}^2}}$$

where the subscripts *i* denote stock, $CARet_i^{[-n,n]}$ is the cumulative abnormal return from a risk model for stock *i* with an *n*-day window around the event, $\sigma_{\epsilon_{ARet_i}}^2$ is the variance of the residual from the risk model estimation for stock *i*, and *N* is the estimation window length.²²

²¹Following standard procedure, we skip 10 trading days as the gap between the end of the estimation period and the beginning of the event window, to prevent the estimation window from including information that might have been leaked to the market well before the event.

²²When events tend to cluster in calendar time (e.g., a growing demand for month-end liquidity), Boehmer, Masumeci and Poulsen (1991) employ the standardized cross-sectional test, which takes into account information from both the estimation and the event windows and allows for event-induced variance shifts. Boehmer, Masumeci and Poulsen (1991) demonstrate that their test statistic is not affected by event-induced variance changes. In our analysis, we calculate the standardized cumulative abnormal return and the corresponding *t*-statistics for a robustness check.

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