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Evidence on the moral hazard effects of bank recapitalizations

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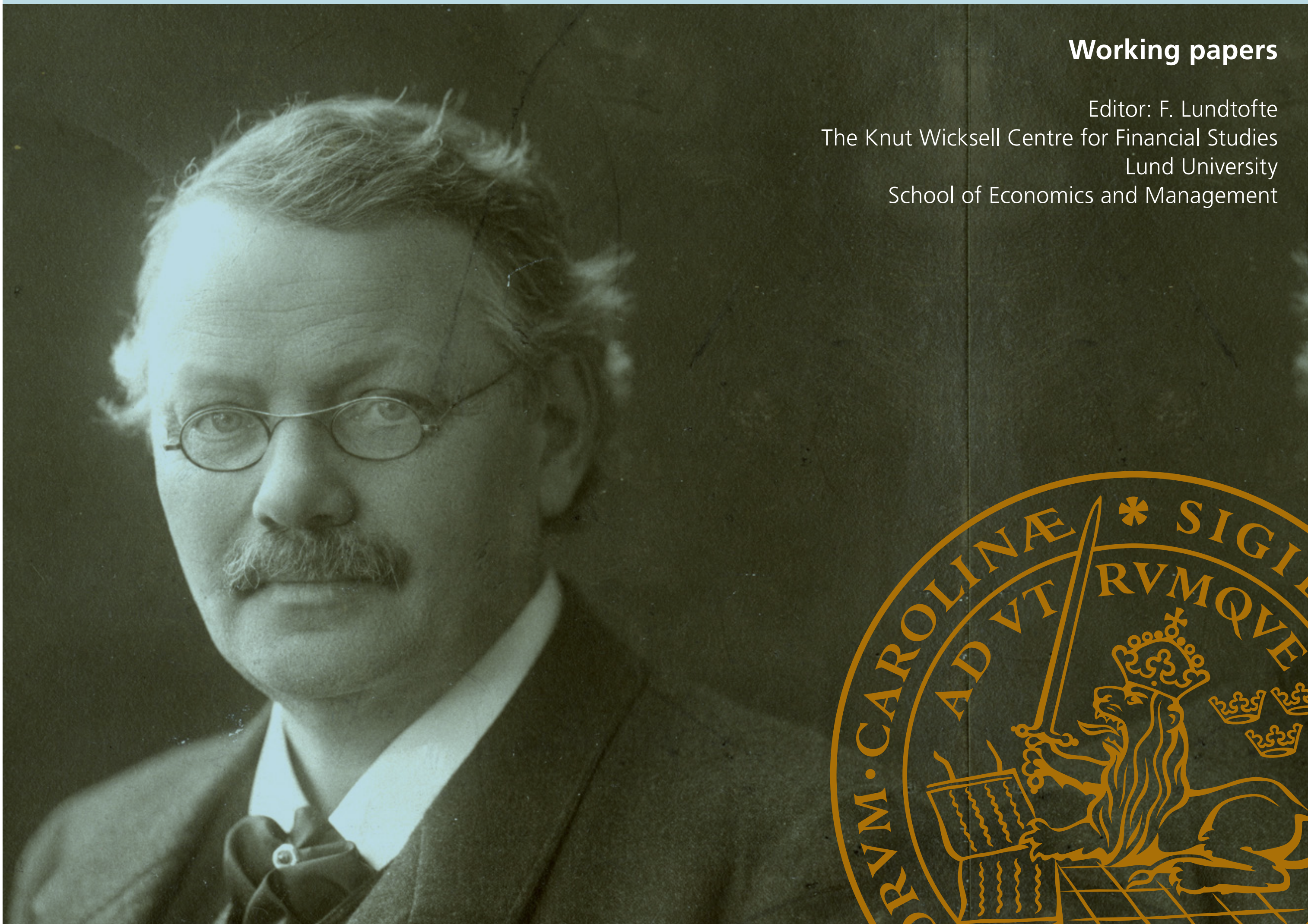

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TARP and market discipline: Evidence on the moral hazard effects of bank recapitalizations☆

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Abstract

We examine the moral hazard effects of bank recapitalizations by assessing the impact of the U.S. TARP program on market discipline exerted by subordinated debt-holders using a sample of 123 bank holding companies over the period 2004-2013. Predicted distress risk has a consistently positive and significant effect on sub-debt spreads, suggesting the presence of market discipline. A higher bailout probability significantly reduces the risk-sensitivity of spreads for the full sample, indicating a moral hazard effect of recapitalizations. This appears to be a too-big-to-fail effect, as it is absent when the largest banks are dropped from the sample. Results indicate that it is transitory. We also find a large effect of the crisis, appearing both as a uniform rise in, and a heightened risk sensitivity of, sub-debt spreads during the crisis.

Keywords: Bank bailouts; moral hazard; distress risk; capital injections; TARP;

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CPP; market discipline; financial crisis
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1. Introduction

A main line of criticism against government support of banking firms in distress is that it may incentivize banks to engage in risk-shifting, since it gives rise to expectations of future bailouts. Such expectations disrupt the disciplinary effects of possible bankruptcy on banks' risk-taking behavior and cause moral hazard: banks have little reason to restrain risk-taking in search of high returns if government guarantees limit downside risk. This effect is well documented in cross-country studies of deposit insurance, which typically find a positive association between the extent of insurance coverage and bank instability (Demirgüç-Kunt and Detragiache, 2002 Hovakimian et al., 2003), but may also be the consequence of implicit guarantees and ad hoc bailouts (Cordella and Levy Yeyati, 2003 Gropp and Vesala, 2004 Wilson and Wu, 2010). Yet, such bailouts may nonetheless be deemed necessary if the threat of failure faces a sufficient number of, and/or sufficiently large, banking institutions to jeopardize the stability of the entire financial system (Rochet and Tirole, 1996 Acharya and Yorulmazer, 2007). This, arguably, was the case when the U.S. Treasury announced the Troubled Asset Relief Program (TARP) at the pinnacle of the financial crisis in October, 2008. The TARP has been labelled "the largest government bailout in U.S. history" (Bayazitova and Shivdasani, 2012), and its centrepiece was the Capital Purchase Program (CPP) under which 707 banks received capital injections from the U.S. Treasury to a total amount of just under USD 205 billion.

An important question in light of the potential adverse incentive effects of bailouts is how this large-scale recapitalization of the U.S. banking sector affected expectations of future government support and, by extension, moral hazard and banks' risk-taking behavior. There are at least two main reasons why this question is not easily answered. One is that the relationship between government support and risk-taking cannot be studied directly: banks do not receive government support at random but precisely because they are close to distress (i.e., risky), so finding that bailed-out banks are risky is not equivalent to identifying government support as a cause of risk-taking.[1] Another is that, even two-way causality between government support and risk aside, it is not necessarily only banks that are actually bailed out that are subject to possible moral hazard. Moral hazard is driven by expectations, and may affect bailed-out and non-bailed-out banks alike. For a non-bailed-out bank it is sufficient to use available information – for instance to observe the conditions under which other banks have received government support in the past – to form such expectations and act on them. Equivalently, a supervisor's threat to close down a bank if it ends up in distress must be credible to prevent moral hazard, whether or not the bank has actually experienced a distress situation (Mailath and Mester, 1994 Angkinand and Wihlborg, 2010). Thus, there is not a one-to-one relationship between the reception *per se* of government support and moral hazard.

In this paper, we examine the possible moral hazard effect of TARP/CPP by assessing its impact on the extent of *market discipline* exerted by uninsured debtholders on participating and non-participating banks. A key finding in the market discipline literature is that the extent to which uninsured bank creditors monitor and discipline banks critically depends on their beliefs regarding the prospects of being bailed out despite being formally uninsured (see, e.g., Flannery and Sorescu, 1996 Gropp and Vesala, 2004 Angkinand and Wihlborg, 2010). Indeed, it is the very absence of punishment from creditors for higher observed risk (via higher risk premiums on debt), when these creditors believe themselves to benefit from

[1]See, e.g., Dam and Koetter (2012) for a thorough general discussion, and Black and Hazelwood (2013) and Duchin and Sosyura (2014) for discussions specifically in the context of TARP/CPP. Several papers have examined the determinants of CPP participation (among them Bayazitova and Shivdasani, 2012 Li, 2013 Cornett et al., 2013), and – by and large – confirm that banks closer to distress were more likely to receive capital injections under the program. Elyasiani et al. (2014) find that recipients of CPP funds had higher systematic risk relative to recipients of capital infusions via private market SEOs.

bailout guarantees, that allows banks to shift risk in the first place (Gropp et al., 2011). Because the extent of monitoring and discipline reflects the market's perception of future bailout probabilities, weakened market discipline should be associated with moral hazard.

The expectation of diminished market discipline due to bailout expectations is not unambiguous, however. Recent evidence (Martinez Peria and Schmukler, 2001) indicates that market discipline may *increase* in times of crisis (when the bailouts are needed, and most likely to occur). A possible reason is that a crisis serves as a "wake-up call" for investors, or that the safety nets are not credible. A partial bank rescue program may also make creditors particularly alert to the distress risk of the rescued banks, since the acceptance of support may signal larger future losses than previously anticipated and put a "crisis stamp" on these banks (Hoshi and Kashyap, 2010). If creditors believe that the government support is insufficient, or unsustainable in the long run, this may cause market discipline to increase rather than to decrease. Examining the effect of government support on market discipline may, therefore, also give an indication of the market's perception of the credibility of the rescue package.

In conformity with a large proportion of the previous empirical market discipline literature (e.g., Flannery and Sorescu, 1996), we test for market discipline by estimating the risk-sensitivity of yield spreads on U.S. bank holding companies' (BHCs) subordinated notes and debentures (SNDs) in a series of panel regressions using quarterly data. The time period spans four years prior to the announcement of the TARP program and four years after its formal closure in December, 2009. In this setting, we examine how the risk sensitivity of SND spreads is affected by bank/period-level observations on the probability of receiving funds under the TARP/CPP program, as well as by a time-variant-only crisis indicator. We address the co-determination of bank risk and government support, as well as possible expectation effects on non-bailed-out banks, by first estimating a system of equations that generate predictions of risk and predicted probabilities of receiving government support based on exogenous variables. We also account for the double selection issues that arise due to a uniform increase in the unconditional bailout probability at the announcement of the TARP program (and the absence of observations of actual bailouts prior to this announcement), and due to the effect of both risk and government support on banks' likelihood of having outstanding subordinated debt.

There are a number of motivations for our approach of testing the potential moral hazard effects of government support via its effect on market discipline, rather than its effect on bank risk as such. In particular, this approach directly targets the mechanism through which banks' risk-shifting in the presence of government support is made possible. In addition, studying the effect of bailout probabilities on market discipline addresses the possible expectation effects of bailouts on *non*-bailed-out banks, and side-steps the need to define "the counterfactual", which is required to isolate the direct effect of actual bailouts on the risk-taking of supported banks only. The market's response to the arrival of new information about future bailout probabilities is also likely to be relatively instantaneous, whereas the effect on banks' observable risk may be gradual and occur with a lag. [2] Finally, in the specific context of the CPP program, holders of subordinated debt are in some sense the perfect claimants to examine: because the support was given in the form of preferred stock, sub-debt holders were provided with a larger buffer against losses; at the same time, they were still formally uninsured, and their claims were junior to those of all other debt holders, and thus still at risk.[3]

[2] Cordella and Levy Yeyati (2003) and Gropp et al. (2011) demonstrate that the overall effect of government support on banks' risk-taking may in fact be ambiguous if the support results in a competitive advantage vis-à-vis non-supported banks that causes the charter values of supported banks to increase, in line with the traditional "charter-value hypothesis" (Keeley, 1990). Berger and Roman (forthcoming) provide evidence of a competitive-advantage effect of TARP).

[3] Veronesi and Zingales (2010) show that for the ten large banks that were the first to be recapitalized under CPP, the bulk of the value created by the capital injections was appropriated by bond-holders via the debt overhang effect. It was thus largely a bailout of bond-holders. The same should apply to other participant banks if – as existing evidence (e.g., Bayazitova and Shivdasani, 2012) suggests – undercapitalization increased the likelihood of participation. Veronesi and Zingales (2010) also argue that the key friction resolved by the recapitalizations was the threat of bankruptcy. Since default risk is what primarily drives market discipline by creditors it appears

Our main results indicate that the direct effect of banks' predicted distress risk (measured as the z-score, a simplified distance-to-default measure common in the banking literature) on SND spreads is consistently positive and highly significant, which gives a general indication of the presence of market discipline by uninsured debt-holders. The effect is not unimportant. In our main specifications, a one standard deviation increase in distress risk raises the spread by about 60 bp. The direct effect of predicted bailout probabilities is equally positive and of a similar order of magnitude. The results further indicate that the interaction of distress risk and bailout probabilities is negative and significant – in other words, a higher bailout probability corresponds to a lower risk sensitivity in SND spreads. Again, the effect is economically important: raising the predicted bailout probability by one standard deviation from the mean reduces the risk sensitivity of spreads by half, consistent with a moral hazard interpretation. We also find a large effect of the crisis on spreads, appearing both as a substantial uniform rise in the intercept term of the spread regressions during the crisis quarters, and as an increased risk sensitivity of spreads during this period. Our main results are thus consistent both with a moral hazard effect associated with the TARP program leading to reduced market discipline by sub-debt holders in banks with a high probability of receiving government support, and with a "wake-up call effect" which increased market discipline across the board during the culmination of the crisis. Both effects are strengthened when they are accounted for simultaneously.

In alternative specifications, we allow for the possibility of a lasting effect of the TARP bailouts, and let bailout probabilities be fully determined at the announcement of TARP and remain unchanged until the end of the sample period. In these results, the effect of bailout probability on market discipline is insignificant when controlling for the crisis, suggesting that the effects of TARP on market discipline were transitory. When the largest banks are dropped from the sample,

likely that a reduction in the threat of bankruptcy will affect market discipline, particularly from creditors with low-priority claims.

the effect of bailout probability on market discipline is likewise insignificant, suggesting that the main results may be largely driven by a too-big-to-fail effect. We further examine this possibility by running "clean" difference-in-difference tests with two possible groups of TBTF banks. In these results, we find no evidence of a lasting reduction in the risk sensitivity of spreads for TBTF banks after TARP. We do find, however, that spreads are on average substantially higher after 2008, but much less so for the TBTF banks. Overall, our results suggest that the recapitalizations under TARP reduced market discipline by uninsured debt-holders, but that this effect primarily affected the largest banks and was transitory. Our results come with a number of caveats at present.

Our paper builds on and extends the existing empirical literature on TARP/CPP, which has hitherto focused on the determinants of entry into, and exit from the program (Ng et al., 2011 Bayazitova and Shivdasani, 2012 Duchin and Sosyura, 2012 Wilson and Wu, 2012 Li, 2013 Cornett et al., 2013), the announcement effects of program participation on stock returns (Bayazitova and Shivdasani, 2012 Farruggio et al., 2013 Kim and Stock, 2012 Fratianni and Marchionne, 2013 Elyasiani et al., 2014), and the effectiveness of the program in terms of stimulating credit supply in supported banks (Li, 2013 Montgomery and Takahashi, 2014). Our paper is most closely related to two recent studies examining the effect of CPP on the supported banks' subsequent asset risk, particularly the risk of new loan originations (Black and Hazelwood, 2013 Duchin and Sosyura, 2014), and a study of the effects of bailout expectations on distress risk in the German banking industry (Dam and Koetter, 2012) – all of which suggest that government support in the form of recapitalizations contribute to increased subsequent risk. Our paper complements these studies by explicitly testing the market discipline channel of moral hazard, by accounting for the effect of changes in bailout *expectations* around the TARP program (rather than the recapitalizations as such), and by substantially extending the time period tested to account for the possibility both of a transitory and a permanent effect. Our paper is also closely related to the substantial amount of literature on market discipline, particularly studies addressing the effect of implicit guarantees on market discipline as a result of too-big-to-fail considerations (Ellis and Flannery, 1992 Morgan and Stiroh, 2001 Völz and Wedow, 2011 Acharya et al., 2013), regulatory changes (Flannery and Sorescu, 1996 King, 2008), or specific events (Balasubramnian and Cyree, 2011). Compared to these studies, our estimation setting allows for a more detailed modeling of implicit guarantees (bailout probabilities), which vary both cross-sectionally and over time. Finally, we address the effect of financial crises on market discipline by uninsured creditors, whereas previous studies have focused on the effect of crises on depositor discipline (Martinez Peria and Schmukler, 2001 Cubillas et al., 2011).

In the following section, we provide background on the TARP/CPP program and a more comprehensive review of related literature. Section 3 describes the methodology and the data. In Section 4, we present the main results, as well as the results of a number of alternative specifications and robustness tests. Section 5 concludes.

2. Background and previous literature

2.1. A brief background on TARP and CPP

The Troubled Asset Relief Program (TARP) formed part of the Emergency Economic Stabilization Act (EESA), which was passed in response to the tumultuous developments following the U.S. Treasury's takeover of mortgage finance companies Fannie Mae and Freddie Mac, the bankruptcy of Lehman Brothers, and the Federal Reserve's bailout of insurance giant AIG in September, 2008. A first announcement of the program, originally designed primarily to purchase distressed assets, was made on 19 September, 2008 (one and a half weeks after the Lehman crash and three days after the AIG bailout), but rejected by Congress. The EESA was passed into law on 3 October with an initial budget for troubled asset purchases of USD 700 billion. At the same time, the FDIC raised the ceiling on deposit insurance coverage by 150 percent to USD 250,000 per depositor.

The revised TARP was announced in mid-October, 2008, with a new main emphasis on direct equity investment by the Treasury in financial institutions and in the automotive industry under 12 different sub-programs. The bulk of capital injections in banks were carried out under the Capital Purchase Program (CPP), which was allocated USD 250 billion.[4] In a first step, half this sum was injected into the ten (nine) largest U.S. banks [5] The "conventional wisdom" is that these ten initial CPP recipients were arm-twisted into participating to gain acceptance for the program (see, e.g., Veronesi and Zingales, 2010). At the announcement of their participation, the CPP also opened for other banks, with an initial time window for publicly held financial institutions to receive capital injections set to one month. The window was later extended, and recapitalizations under TARP were carried out until July, 2009 (but concentrated to the last quarter of 2008 and the first quarter of 2009). Along with the announcement of the CPP, the FDIC announced a three-year guarantee on all new senior unsecured debt issues made by FDIC-insured institutions under the Temporary Liquidity Guarantee Program (TLGP) to improve conditions in wholesale credit markets.

For all banks but the initial ten, participation in CPP was voluntary and subject to regulatory approval. Qualified financial institutions (bank holding companies, savings associations, and some savings and loans holding companies) wishing to participate had to make an application to the relevant regulatory authority (the Federal Reserve, the FDIC, the OCC, or the OTS, depending on the type of institution), upon whose recommendation the Treasury would then make the final

[4]In the paper, we use the acronyms TARP and CPP interchangeably, referring to capital injections in banks under the TARP program.

[5]Citigroup, Bank of America, JP Morgan Chase, Wachovia, Wells Fargo, Bank of NY Mellon, State Street Corp., Goldman Sachs, Morgan Stanley, and Merrill Lynch. At the time of the recapitalization announcement, Wachovia had already signed an agreement to be acquired by Wells Fargo. Merrill Lynch was subsequently acquired by Bank of America. Both deals were finalized by end-of-year, 2008. Three of the ten initial CPP recipients reported as investment banks (not bank holding companies) until September, 2008 (Goldman, Merrill Lynch and Morgan Stanley). Two of the ten, Citigroup and BofA, received additional capital injections of USD 20 billion each under the Targeted Investment Program (TIP) in December 2008 and January 2009, respectively.

decision whether to approve or reject the application. The number of applications for CPP funds is not publicly known, but was "in the thousands" according to the Treasury's publicly available documentation of the program. The number of approved applications is also unknown, as some applicant banks that were approved for recapitalizations later decided not to accept the funds. The Treasury did not reveal the criteria used to assess the CPP applications and what made banks qualify for it, leading to uncertainty about the health of participating institutions and a debate over whether the CPP should be considered a "bailout" of unhealthy banks.[6] A large number of applications were withdrawn, but it is not clear how many of these were withdrawn voluntarily (possibly in response to negative press surrounding the program, see Ng et al., 2011) and how many because the applicant was not qualified to participate.

The capital injections under CPP were carried out in the form of purchases of preferred stock, with an initial dividend rate of 5 percent for five years and 9 percent thereafter, along with a ten-year warrant on common stock to an amount of 15 percent of the injection. The size of the capital injections was restricted to between 1 and 3 percent of risk-weighted assets (but exceeded this amount in some cases). In practice, the variation in the relative sizes of the injections was very small and the percentage injected in most cases close to the maximum, so that the absolute amounts were almost perfectly determined by RWA (see, e.g., Duchin and Sosyura, 2012). The support was initially skewed toward large banks, but in May, 2009, the application window reopened for banks with total assets less than USD 500 million, with the ceiling for capital injections raised to 5 percent of RWA (Cornett et al., 2013). The largest CPP investment was USD 25 billion and the smallest USD 301,000 (USD 25 billion and USD 2.9 million, respectively, in

[6]Later empirical research, in particular, event studies of stock market responses to banks' announcements of CPP participation, produce mixed support of a "crisis stamp" hypothesis. For instance, Bayazitova and Shivdasani (2012) find no significant differences in abnormal returns between banks that participated in the CPP and banks that did not, whereas Farruggio et al. (2013) find a negative effect of banks' announcements of entry into the program, and a positive effect of exit (repayment) announcements on abnormal returns.

our sample).

A number of restrictions related to executive compensation and flexibility for repayment for supported banks were implemented in February-March 2009. According to studies of the determinants of repayment of TARP funds, these restrictions may have been a major factor contributing to banks' decisions to exit the program (Bayazitova and Shivdasani, 2012 Wilson and Wu, 2012). Repayment was subject to regulatory approval, and the primary approval criterion was sufficient capital after repayment. The first repayments were made in March, 2009, by a limited number of banks, and repayments are then relatively evenly distributed over a subsequent period of several years.

Parallel to the toughened requirements on supported banks in terms of, for example, executive pay, the Treasury launched the Financial Stability Plan in February, 2009, which involved a number of initiatives, including the Capital Assessment Program (CAP) and the Supervisory Capital Assessment Program (SCAP), under which it was announced that the nineteen largest U.S. banks (with total assets exceeding USD 100 billion) would have to undergo stress tests. The stress tests were carried out in March, 2009, and the results made public in May. These revealed that ten of the nineteen banks would be required to raise a total of USD 75 billion of additional capital by November, 2009. Bayazitova and Shivdasani (2012) find a strong "certification effect" of the SCAP announcement on the 19 banks' stock returns.

The CPP officially closed in December, 2009, having injected a total of USD 204.9 billion of capital into 707 financial institutions in 742 transactions. Of the total amount invested under the CPP, USD 163.5 billion went to the largest 19 banks. The overall effectiveness of the program remains debated. The inherent conflict of objectives in the rescue plan (and recapitalization programs in general) – to stabilize the financial sector by strengthening its capital base *and* to stimulate lending (to borrowers potentially weakened by the economic downturn) to mitigate the real effects of the crisis – is noted by several authors (including Black and Hazelwood, 2013, and Farruggio et al., 2013). The more immediate potential value-creating effects of the program include a reduction in the probability of bankruptcy for, and the prevention of (inefficient) runs on, major financial institutions (Veronesi and Zingales, 2010), and instilling confidence in financial markets and preventing, or alleviating, credit market freezes (Cheng and Milbradt, 2012). Because recapitalizations under the program were subject to regulatory approval, participation may – at the individual bank level – give rise to a quality signalling/certification effect (Bayazitova and Shivdasani, 2012), if it is believed that regulatory assessments reflect private information (and that the assessment criteria *de facto* used were in line with the stated purpose of supporting healthy institutions). On the potential downside, self-selection into a rescue program may give a negative signal about true firm value (Hoshi and Kashyap, 2010).[7] Further possible negative value effects may be due to government interference in banks' affairs and an increased risk of future regulatory intervention, particularly in the absence of a credible exit plan by the government (Veronesi and Zingales, 2010 Fratianni and Marchionne, 2013), and dilution of equity-holders' cash-flow rights (Kim and Stock, 2012). An additional effect on financial market expectations is the moral hazard effect, manifested as decreased monitoring incentives. The next sub-section reviews the literature on the TARP program in more detail, as well as existing empirical evidence on market monitoring of banks.

2.2. Related literature

A few main directions in the existing empirical literature on the TARP/CPP program can be identified. Several studies examine the determinants of selection into the program (Ng et al., 2011 Bayazitova and Shivdasani, 2012 Duchin and Sosyura, 2012 Li, 2013 Cornett et al., 2013), typically finding that larger, capital- and liquidity-constrained banks were more likely to participate, but also that participating banks generally had higher asset quality. These findings are consistent

[7] A value-signalling effect, positive or negative, is less likely for the major banks "forced" to participate without prior assessment.

with the CPP's stated purpose of supporting banks that were temporarily squeezed by the crisis, but had long-term viable operations. Duchin and Sosyura (2012) as well as Li (2013) find that banks' political connections significantly influenced the likelihood of being approved for the program. A few papers also study the determinants of exit from the CPP. Bayazitova and Shivdasani (2012) and Wilson and Wu (2012) find that, among supported banks, larger, better capitalized banks with higher asset quality, higher pre-crisis CEO compensation levels, and lower shares of commercial and industrial loans were more likely to quickly repay TARP funds. Cornett et al. (2013) find that banks with better pre-crisis financial health, and banks that saw larger performance improvements (particularly in terms of loan quality) during the period they held TARP funds, exited the program faster.

Another main direction in the TARP literature is event studies of the market response to general and bank-specific announcements related to TARP. These studies provide initial evidence on the type of signal that was generated by the recapitalizations – a certification effect (or "stamp of approval") or a "crisis stamp", and may thus give some priors on the expected effect on market discipline. Results, however, are somewhat mixed. Bayazitova and Shivdasani (2012) and Farruggio et al. (2013) find large, positive abnormal returns for bank stocks in general at the Treasury's announcement of the launch of the TARP program, whereas subsequent announcements of program participation for individual banks are associated with insignificant or negative abnormal returns. Kim and Stock (2012) reach similar results for the returns on existing preferred stock. Fratianni and Marchionne (2013), using a cross-country sample of banks (of which more than one-third are U.S. banks) over 2008-09, find little evidence of substantial announcement effects of government support. Elyasiani et al. (2014) find that investors reacted positively to TARP capital injections, but negatively to private market SEOs by financial firms. There is little consistent evidence of significant short-run valuation effects associated with repayment of TARP funds (Bayazitova and Shivdasani, 2012 Farruggio et al., 2013), but Liu et al. (2013) find that CPP banks that had repaid the government support by year-end 2010 earned significant abnormal returns both while holding the funds and, particularly, in the quarter immediately following repayment.

A few papers assess the CPP in light of its explicit objective to limit the real-economy effects of the crisis by preventing banks from having to drastically shrink assets in response to capital shortage created by large losses.[8] Li (2013) finds that CPP participant banks increased lending after recapitalization on an order of magnitude comparable to the drop in credit during the last quarter of 2008, and that this result holds also after accounting for drawdowns of previously unused loan commitments originated prior to the crisis (Ivashina and Scharfstein, 2010). In contrast, Montgomery and Takahashi (2014), expanding the period of time studied and accounting for pre-crisis trends in loan growth, find that TARP fund recipients did shrink assets, particularly in the highest risk-weight class (which includes loans). Mariathasan and Merrouche (2012) study recapitalizations in 15 OECD countries (including the U.S.), and find that capital infusions are only effective in bolstering lending if they are large enough (cf. Diamond and Rajan, 2000 Hoshi and Kashyap, 2010).

The effects of TARP on banks' loan supply are related to the possible moral hazard effects of bank bailouts in the sense that banks that receive, or expect to receive, government support may have a reduced incentive to properly assure the quality of new loan originations. A credit expansion associated with government support may thus, in part, be driven by banks' more offhand attitudes to borrowers' credit risk in the presence of such support.[9] A sharper interpretation of the moral hazard hypothesis is that banks deliberately increase the risk of new lending under the prospect of receiving government support.Two recent studies directly examine the effects of TARP on banks' risk-taking. Black and Hazelwood (2013) find that, relative to non-recipient banks, the internal risk ratings of newly originated commercial and industrial (C&I) loans increased for large banks

[8] See, for example, the Treasury's CPP "Program Purpose and Overview" at financialstability.gov.

[9] Cf. the "evergreening" effect documented by Peek and Rosengren (2005).

recapitalized under CPP (with no accompanying increase in total lending), but decreased for small recipients. These results are consistent with a moral hazard interpretation: government support provided via TARP may have been perceived as a too-big-to-fail (TBTF) guarantee for large banks, leading to an increase in the risk level of new loans.[10] Using a larger sample of publicly listed banks, Duchin and Sosyura (2014) exploit data on both approved and denied CPP applications, and on approved and denied loan applications to the CPP applicant banks, to identify the effect of TARP on supported banks' lending policies. They find a shift at supported banks toward riskier borrowers (retail and corporate) as well as a shift toward larger relative holdings of risky investment securities. Exploring a number of alternative explanations for these findings they conclude that the evidence is most consistent with a moral hazard explanation. In particular, risk-shifting is stronger for larger banks (consistent with a TBTF effect), for banks closer to distress (where the risky option has greater value), and for banks that received multiple signals of forbearance.

Compared to Black and Hazelwood (2013) and Duchin and Sosyura (2014), our paper differs in at least three respects. First, they directly study the effects of TARP on risk-taking, not on market discipline. Second, we test a substantially longer time period before and after TARP, and allow for the possibility that any moral hazard effect of the program is transitory. Third, and perhaps most importantly, their identification strategy targets the effect of a recapitalization on banks that did receive it (compared to banks that did not), i.e., it isolates the effect of reception *per se* of government support. By design, we do not do that. Instead,

[10]The results are also consistent with large banks acting more in line with the intentions of the TARP program to maintain lending, combined with an increase in the average risk level of borrowing firms caused by the economic downturn. However, small TARP banks did not decrease lending more than small non-TARP banks, yet their risk decreased, suggesting that an increase in average borrower risk does not fully explain the results for large banks.

we attempt to capture the *latent* effect of bailout expectations.[11] In other words, we do not model bailouts as discrete events (a bank is either bailed out or it is not); a non-recapitalized bank may face a high probability of receiving government support, and a bank that was bailed out in the past may not be supported again. In this sense, our paper is closer in spirit to Dam and Koetter (2012), who, like Black and Hazelwood (2013) and Duchin and Sosyura (2014), examine the effect of bailouts directly on banks' risk, but in a non-U.S. context. Their sample consists of all distress events (as defined by the supervisory authority) in the German banking industry between 1995 and 2006; they observe whether these distressed banks receive government support or exit, and they use the estimated model of bailout probability for the sub-sample of distressed banks to impute the latent probability of bailout, conditional on distress, for non-distressed banks. Their findings indicate that an increase in predicted bailout expectations results in a significant increase in distress risk, and that this moral hazard effect is only mitigated by "strong" forms of supervisory intervention. Our paper differs from Dam and Koetter (2012) in that we examine the market discipline channel of moral hazard and provide evidence on the effect of bailout expectations for the U.S. during a period spanning the financial crisis. (Our estimation framework also differs, because we do not observe distress events as defined by the regulator).

Besides previous literature on TARP and bank recapitalizations, our paper is closely related to the literature on market discipline in banking, where the primary focus of interest has been on the monitoring aspect of market discipline: whether market prices on various types of bank claim can be relied upon to convey accurate and useful information about banks' risk. There are primarily two reasons why they might not. First, a large proportion of banks' debt holders are insured against losses via formal deposit insurance (and possibly via conjectural guarantees), implying that they have limited incentives to monitor and adequately price

[11] Duchin and Sosyura (2014) provide suggestive evidence of an expectation effect because all banks in their sample that were approved for recapitalizations under CPP show similar increases in risk, whether they actually received the funds or not.

bank risk, since they are (or perceive themselves to be) holding a *de facto* risk-free asset. Second, high leverage and limited monitoring and risk pricing by creditors open up opportunities for equity holders to exploit risk-shifting incentives created by the option value of equity (Merton, 1977), limiting their incentives to discipline bank risk taking and possibly distorting the informativeness of equity-based indicators of risk. Motivated by these arguments, a large body of literature tests the sensitivity of returns on various types of traded bank securities (mostly large CDs or subordinated debt) to a set of benchmark risk measures (typically accounting information and/or ratings).

The evidence piled up over the years paints a mixed picture. Among early studies, for instance, Avery et al. (1988), Gorton and Santomero (1990) and James (1990) find little evidence of accounting risk reflected in debt prices, whereas other studies find that CD rates or SND spreads are significantly determined by various balance sheet ratios (Hannan and Hanweck, 1988 James, 1988 Keeley, 1990). Later studies report relatively consistent evidence in support of market monitoring by sub-debt holders in European banks, using credit ratings, or ratings changes, as benchmark risk measures (Sironi, 2002, 2003 Pop, 2006). On the other hand, studying spreads of U.S. banks' sub-debt of various maturities over the second half of the 1990s, Krishnan et al. (2005) find only weak and inconclusive evidence of a relationship between spreads and benchmark risk measures. A number of studies qualify these results. Of particular interest for our paper are studies directly addressing the effect of implicit government guarantees on market discipline. The conjectural guarantees possibly associated with banks being "too big to fail" are considered by Ellis and Flannery (1992), Morgan and Stiroh (2001) and Acharya et al. (2013). All find evidence that too-big-to-fail considerations cause the market to be softer on bigger banks. Völz and Wedow (2011), examining market discipline in the credit default swap market, find a significant size discount in CDS spreads, suggesting the presence of a too-big-to-fail effect. Examining differences in implicit bailout guarantees under different regulatory regimes, Flannery and Sorescu (1996) find that the sensitivity of yield spreads on subordinated debt to underlying credit risk became stronger after the FDICIA[12] reform of 1991, which committed more credibly to a no-bailout policy for U.S. banks' uninsured creditors. Similarly, King (2008) finds a substantial increase in the sensitivity to default risk of the cost of interbank loans for U.S. banks after regulatory reforms shifting more of the costs of bank defaults onto creditors. Balasubramnian and Cyree (2011) show that in the short term, the risk sensitivity of yield spreads on U.S. banksâ€™ SNDs virtually disappeared and the size discount on SND spreads doubled in response to the Federal Reserve's bailout of the large hedge fund Long-Term Capital Management (LTCM) in 1998, indicating that the event resulted in a shock to bailout expectations, particularly for TBTF banks. Compared to these papers, examining the effect of bailout expectations on market discipline under TARP/CPP allows for cleaner difference tests of possible moral hazard effects – first, because not all banks participated and we are consequently able to discriminate cross-sectionally between "treated" and "non-treated" banks; second, because the program was relatively short-lived and has a well-defined "treatment period" (we are, therefore, also able to test both for a possible transitory effect and a more permanent one).

Finally, a number of papers find that market discipline by depositors changes in response to financial crises. Martinez Peria and Schmukler (2001) study a sample of Latin American banks during the 1980s and 1990s and find that depositors exert greater market discipline (both by requiring higher interest rates and by withdrawing funds) after banking crises. The increase in market discipline is unaffected by deposit insurance, possibly due to limited depositor confidence in the deposit insurance scheme. Hadad et al. (2011), examining Indonesian banks after the 1997-98 Asian financial crisis, and Cubillas et al. (2011), studying a large international sample over a 20-year period, find that accommodative policies (such as extended depositor guarantees) in response to crises reduce depositor discipline.

[12] Federal Deposit Insurance Corporation Improvement Act.

3. Methodology and data

3.1. Methodology

We are interested in estimating market discipline and the extent to which it is affected by bailout probabilities by running panel regressions of the yield spreads on SNDs issued by the sample banks of the following form:

$$S_{jit} = \beta_1 D_{it} + \beta_2 B^*_{it} + \beta_3 D_{it} \times B^*_{it} + \mathbf{\Gamma}'\mathbf{Z_{jit}} + u_{jit} \tag{1}$$

where S_{jit} is the spread over the risk-free rate at time t of SND j issued by bank i, D_{it} is a proxy of i's distress risk, B^*_{it} denotes the bailout probability, and $\mathbf{Z_{jit}}$ is a vector of control variables that may vary across issue, bank, or time, which includes issue-level fixed effects, but which we leave further unspecified for now. In line with previous market discipline literature, we interpret a significantly positive estimate of β_1 as evidence of market discipline (market monitoring): spreads respond to banks' observable distress risk. But Equation 1 can also be loosely interpreted in analogy with a difference-in-difference model, where B^*_{it} represents the *probability* of being "treated" (rather than actual treatment as such). Of particular interest is the coefficient β_3. A significantly negative estimate for this coefficient suggests that the risk sensitivity of spreads on banks' uninsured debt decreases with increased bailout probability, which we interpret as a moral hazard effect (increased bailout probability decreases market discipline). A significantly positive estimate of β_3 suggests the opposite interpretation, whereas if β_3 is indistinguishable from zero, bailout probability does not affect market discipline. The relevant estimation period is Quarter 3, 2004 to Quarter 4, 2013, which corresponds to a period of four years before the announcement of the CPP, the period during which the CPP was open for applications (until the last quarter of 2009), and four years after the program's termination.

We face four estimation challenges. First, B^*_{it} is not observed, but latent. What we observe is an indicator variable B_{it}, which takes on unit value if the U.S. Treasury had a non-zero equity investment under TARP in bank i at time t. In other words, we assume that banks are in a "state of bailout" from the moment they receive government support until it is fully repaid (we discuss this assumption further in subsection 4.3.1). Second, because the assignment to "treatment" is not random but may be determined by banks' distress risk, because a recapitalization could itself affect risk-taking incentives (via the potential moral hazard consequences of the bailout), and because distress risk and bailout expectations (and possibly spreads) may be subject to common shocks, the main regressors of interest in Equation 1 are endogenous.

To address these first two estimation challenges, we define the following structural system of equations:

$$D_{it} = \gamma_1 B^*_{it} + \beta'_{\mathbf{D}} \mathbf{X_{D,it-1}} + \beta'_{\mathbf{C}} \mathbf{X_{C,it-1}} + \epsilon_{1,it} \tag{2}$$

$$B^*_{it} = \gamma_2 D_{it} + \beta'_{\mathbf{B}} \mathbf{X_{B,it-1}} + \beta'_{\mathbf{C}} \mathbf{X_{C,it-1}} + \epsilon_{2,it} \tag{3}$$

where $\mathbf{X_C}$ is a vector of common bank-level determinants of distress risk and bailout probability, and $\mathbf{X_D}$ and $\mathbf{X_B}$ are the exclusion restrictions for distress risk and bailout probability, respectively. All $\mathbf{X}$'s are lagged to avoid simultaneity.[13] This type of partially observed system is discussed by, for example, Maddala (1983) and Rivers and Vuong (1988), and more recently applied in the banking literature in a panel data setting by Buch et al. (2013), and we essentially follow the conventional two-step estimation approach: from Equations 2 and 3, we obtain the reduced-form equations (D_{it} and B^*_{it} as functions of *all* $\mathbf{X}$'s), which are estimated to generate predictions of D_{it} and B^*_{it} that can be used in subsequent estimation:

$$\hat{D}_{it} = \mathbf{\hat{\Pi}'_D X_{it-1}} \tag{4}$$

$$\hat{B}_{it} = \mathbf{\hat{\Pi}'_B X_{it-1}} \tag{5}$$

[13] In particular, as distinct from the indirect, longer-term effects on banks' *ex post* investment policy and risk-taking incentives, a capital injection has an immediate (though small) "mechanical" effect on key balance-sheet ratios, including the capital ratio, the liquidity ratio, total assets (TA), and the return on assets (via TA).

The reduced form for D_{it} (which is a fully observed continuous variable) is estimated as a fixed-effects linear model, whereas B^*_{it} is proxied by the binary indicator B_{it}, and its reduced-form equation estimated by random-effects probit.

An additional complication (and our third main estimation challenge) is that the TARP program did not exist, and we do not observe any recapitalizations, from the start of the sample period until and including the third quarter of 2008. Differently put, the unconditional probability of receiving a capital injection before Quarter 4, 2008, is exactly zero for all sample banks. We address this as a selection problem: the observable counterpart of the bailout probability B^* depends on the existence of a bailout program, and not taking this into account may introduce bias in the estimation of $\hat{B}$. The probability of a bailout program existing is of course the same for all banks, and only varies over time, but simply controlling for it by adding period fixed effects in the prediction of $\hat{B}$ is not feasible, as the fixed effects for Quarter 3, 2004 to Quarter 3, 2008, are unknown. Instead, we formulate the following simple probit model:[14]

$$Pr(T_t = 1) = \Phi(\alpha + \beta'_{\mathbf{T}}\mathbf{X}_{\mathbf{T,t}}) \tag{6}$$

where T_t takes on unit value in all time periods where any bank has a non-zero equity investment from the government under TARP, and zero otherwise, and $\mathbf{X_T}$ is a set of aggregate indicators proxying the likelihood of a systemic event (these are further discussed in subsection 3.2.2). From this, we follow the standard Heckman (1979) approach and calculate the inverse Mills ratio, which is included in the vector $\mathbf{X_B}$ of exclusion restrictions for B.

Note that in this estimation setting, $\hat{B}$ measures the "total" probability of receiving a bailout. Since receiving a bailout usually means being close to distress, this is implicitly a joint probability – the probability of needing a bailout, and of getting it conditional on needing it. Moreover, because $\hat{B}$ and $\hat{D}$ are generated

[14] The model is a simplification, as it does not explicitly account for possible serial dependence in the variables (see, for example, de Jong and Woutersen, 2011).

from the same set of variables, they will contain a common (exogenous) component. We thus rely heavily on the identifying variables $\mathbf{X_B}$ and $\mathbf{X_D}$ to generate sufficient additional variation over $\mathbf{X_C}$, and on the validity of the exclusion restrictions, for the effects of $\hat{B}$ and $\hat{D}$ on spreads not to be confounded. In principle, because we are interested in the effects of $\hat{B}$ and $\hat{D}$ on spreads, we could stop at estimating the reduced form equations for D and B, but as an intermediate step, estimating the structural equations 2 and 3 with the right-hand side variables B^* and D replaced by $\hat{B}$ and $\hat{D}$, respectively, allows us to further examine the relationship between distress risk and bailout probabilities, and to more thoroughly test identification. We devote a separate subsection (4.1) to the issue of identification.

The fourth estimation complication is that we can only test the yield spreads for banks that actually had SNDs issued during the sample period (roughly half the banks in our total sample), but banks self-select into SND issuance. Covitz et al. (2004) make the observation that if riskier banks do not issue sub-debt in the first place, in order to avoid being disciplined, then simply examining the relationship between balance-sheet risk and yield spreads may underestimate the association because of sample selection bias. Whether or not bank i had subordinated debt outstanding at time t may depend on several bank-specific characteristics, including distress risk and if it had received, or expected to receive, a capital injection under the CPP. In addition, a bank's choice between senior and subordinated debt may depend on the guarantee component of TARP (the FDIC guarantee on new senior unsecured debt issues under TLGP, see subsection 2.1). To account for this, we follow Covitz et al. (2004) and define a selection probit equation for outstanding subordinated debt according to the following:

$$Pr(Sub_{it} = 1) = \Phi(\psi_1 Iss_{it-2,t-1} + \psi_2 \hat{D}_{it} + \psi_3 \hat{B}_{it} + \psi_4 G_t + \beta'_{\mathbf{S}} \mathbf{X_{S,it}}) \qquad (7)$$

where Sub_{it} is an indicator that takes on unit value if bank i had subordinated debt outstanding at time t and zero otherwise, $Iss_{it-2,t-1}$ is an indicator equal to one if bank i issued subordinated debt between period $t-2$ and $t-1$, G_t is an indicator that takes on unit value during the time periods the TLGP was active, and $\mathbf{X_S}$ is a vector of bank-specific characteristics that may influence SND issuance, which partly, but not fully, overlaps with $\mathbf{X_C}$. We use the predicted values $\hat{D}$ and $\hat{B}$ of distress risk and bailout probability as these may be subject to the same endogeneity issue as in the spread regression. As previously, we adopt the Heckman (1979) two-step approach and the inverse Mills ratio calculated from the parameters of Equation 7 enters as an explanatory variable in the spread regression.

As a last consideration, the main spread regression (Equation 1) accounts separately for the financial crisis, for two reasons. First, and as previously noted, included variables may be subject to common shocks (the financial crisis is an obvious candidate). Second, besides examining the effect of bailout expectations on market discipline, an additional purpose of our paper is to test the effect of the crisis as such on market discipline (in particular, to test the presence of a possible "wake-up call" effect of the type previously documented in, e.g., Martinez Peria and Schmukler, 2001). Our final, estimable version of the main spread regression to be tested is thus:

$$S_{jit} = \gamma_{0,j} + \beta_1 \hat{D}_{it} + \beta_2 \hat{B}_{it} + \beta_3 \hat{D}_{it} \times \hat{B}_{it} + \gamma_1 I_t + \gamma_2 \hat{D}_{it} \times I_t + \gamma_3 IMR_{sub,it} + u_{jit} \quad (8)$$

where I_t is the financial crisis indicator, and IMR_{sub} is the inverse Mills ratio from (7).

3.2. Data and variables

Our gross sample consists of all U.S. bank holding companies (BHCs) with at least one active bond traded in the U.S. market during the sample period (i.e., between Quarter 3, 2004, and Quarter 4, 2013), as indicated in Standard & Poor's Capital IQ database. There are 123 such BHCs, and we consider these banks to be the "population" of potential SND issuers. Among these banks, there are 58 (57) recipients of government support under the TARP (CPP) program, to a maximum total remaining amount of USD 253 billion (173.1 billion).[15] Although our sample thus only covers 8.5 percent of the *number* of banks supported under CPP, it covers 84 percent of the total *amount* of capital injections carried out under CPP (USD 204.9 billion). The sample thus consists primarily of relatively large, publicly listed banks (which is also the case for previous studies on the TARP program, e.g., Bayazitova and Shivdasani, 2012). In terms of size, our BHC "population" ranges between USD 171 million and 1484 billion in 2004 year-end total assets, and all nine initial recipients of CPP funds (initial ten minus Merrill Lynch) as well as the 19 large SCAP banks are included. In all, banks from 36 states are represented. Of the 123 BHCs in the gross sample, 59 issued a total of 235 SNDs used in our final spread regressions. 26 of the SND-issuing BHCs received TARP funds. All data are collected from Standard & Poor's Capital IQ database, unless otherwise stated.

3.2.1. General bank-level variables

The two dependent variables in the system of equations (2 and 3) initially estimated are the so-called *Z-score* to proxy distress risk, and an indicator variable that takes on unit value if the U.S. Treasury had a non-zero equity investment under TARP in bank *i* at time *t* to proxy bailout probability. Data on remaining TARP investments are collected from the Treasury's Transaction Report for the TARP program from January, 2015. The Z-score is a widely used measure of overall default risk in the banking literature, and is defined as:

$$Z\text{-}score_{it} = \frac{ROA_{it} + (E/TA)_{it}}{\sigma_{ROA,it}} \tag{9}$$

[15] Most of the difference between the total TARP and CPP amounts is accounted for by the USD 20 billion each injected in Citigroup and Bank of America under TIP, and USD 13.8 billion of remaining support from the Auto Industry Finance Program to Ally Financial, which became a BHC (and thus enters our sample) in Quarter 4, 2011.

where ROA equals four multiplied by the ratio of quarterly earnings for period t to the average of total assets for period t and period $t-1$, E/TA is the capital ratio (total equity over total assets), and σ_{ROA} is the standard deviation of ROA over the preceding four quarters. Because the Z-score is essentially a simplified distance-to-default measure, it is negatively related to the bank's distress risk. We enter it with negative sign in the regressions to increase intuitiveness: a higher value indicates a higher distress risk.

For general bank characteristics influencing the probability of bailout and distress risk, we rely on previous studies of the determinants of CPP participation and/or of exit from the program, as well as on the rich literature on the determinants of bank risk. Although the criteria used for approving applications for recapitalizations under the CPP were not revealed, strong evidence suggests that the criteria *de facto* used by the respective regulators of applicant banks and by the U.S. Treasury to assess the applications for CPP funds relied on the so-called *CAMELS* internal supervisory rating system used by banking supervisory authorities in the U.S. and elsewhere to regularly assess banks' conditions (see, in particular, Duchin and Sosyura, 2012 Li, 2013). *CAMELS* is short for Capital adequacy, Asset quality, Management capability, Earnings, Liquidity, and Sensitivity to market risk. Practically all previous studies of the determinants of CPP participation use proxies of these basic components of banks' health to model the probability of receiving a capital injection under the CPP, with only minor variations (e.g., Bayazitova and Shivdasani, 2012 Ng et al., 2011 Cornett et al., 2013). These components are also routinely included as explanatory variables in studies of bank risk (although with somewhat larger variations regarding the components included, and the exact proxies used – see, e.g., Angkinand and Wihlborg, 2010 Shehzad et al., 2010 Forssbæck, 2011 Black and Hazelwood, 2013). Therefore, we use the *CAMELS* framework as a guide to the set of common bank-level characteristics $\mathbf{X_C}$ used as regressors for both bailout probability and distress risk.

Specifically, to proxy *Capital adequacy*, we use the logarithm of the ratio of common equity to total assets. We use the *common* equity ratio rather than the total equity ratio or alternative measures of capitalization to minimize simultaneity concerns: because the recapitalizations under CPP were carried out in the form of preferred equity, the common equity ratio is less affected by the capital injections than the total equity ratio. (All independent variables in the reduced-form regressions are also lagged to further avoid simultaneity, as clarified in Section 3.1.) To measure *Asset quality* we use two proxies: "other real estate owned" to total assets, a measure indicating the extent of foreclosures in the bank's property lending; and loan loss provisions over total loans, a more forward-looking measure of expected loan losses. There is little agreement in the literature on how to measure *Management capability*. We use the ratio of the bank's total cost to its total income – a commonly used control variable in the banking literature, and a simple measure of the bank's operating efficiency. *Earnings* is the return on assets (ROA), as previously defined. To measure *Liquidity*, we construct the variable "liquidity gap", defined as the absolute value of the difference between assets and liabilities maturing within one year divided by total equity. To proxy the *Sensitivity to market risk*, we follow several previous studies and use the share of non-interest income to total income. In addition to the *CAMELS* variables, the set of common determinants also includes size, measured as the natural logarithm of total assets. All financial-statement variables are drawn from quarterly filings of the sample banks' BHC reports (FR Y-9C) to the Federal Reserve, retrieved from S&P Capital IQ.

3.2.2. Identifying variables for risk and bailout probabilities

In the following, we discuss candidate exclusion restrictions for distress risk and bailout probability, respectively. The relevant requirements on these variables are (i) that they generate sufficient unique variation in $\hat{D}$ and $\hat{B}$ for each of these to measure something that the other does not, and (ii) that they do not directly influence the variable they are *not* used to predict (i.e., that they are valid as exclusion restrictions). In several cases, we cannot rule out by argument alone that an identifying variable for distress risk also has a direct effect on bailout probability, and vice versa, which would compromise its validity as an exclusion restriction. We ultimately view it as an empirical question.

One of the most important drivers of subsequent banking system instability is rapid credit growth (see, for example, Dell'Ariccia et al., 2012). Importantly, this is not just an empirical regularity at the aggregate level, but available evidence suggests that loan growth is also a leading indicator of asset as well as solvency risk at the individual bank level with considerable cross-sectional variation unrelated to general macroeconomic developments (see, for example, Foos et al., 2010). Potential mechanisms for this association include that rapid increases in lending may be achieved via relaxed credit standards, softer collateral requirements, and/or the general marginal deterioration of the credit quality of new borrowers (e.g., Berger and Udell, 2004). We therefore include the lagged quarter-by-quarter percentage loan growth as a first possible identifying variable for distress risk.

Besides loan growth and the risk of new assets, banks' overall risk also depends on the concentration of exposure to developments in the different asset markets where it is active. Exposures to different market segments are to some degree diversifiable, suggesting that less diversified banks may be exposed to higher asset return volatility, and, therefore, higher distress risk. Diversification is, on the other hand, unlikely to have a *direct* effect on the probability of receiving government support (although diversification can be a second-order effect of size, which may influence government support). We therefore follow Black and Hazelwood (2013) and construct a measure of loan portfolio concentration, calculated as the Herfindahl-Hirschman index (HHI) based on the shares of real estate loans, commercial and industrial (C&I) loans, consumer loans, and other loans.

Our third identifying variable for banks' distress risk rests on what is commonly known as the "charter value hypothesis" – one of the predominant theoretical explanations of the relationship between competition and risk in the banking sector. The charter value hypothesis states that banks with market power extract rents from valuable bank charters, which makes them less prone to exploit risk-shifting incentives, because the opportunity costs of bankruptcy increase in profitability. Banks with higher charter value should thus be less risky (see, for example, Keeley, 1990). Charter value is commonly measured using Tobin's Q, or simply the market-to-book ratio. We use the market-to-book ratio of the included banks.

Finally, we include ownership concentration (proxying shareholder control) as a potential instrument for distress risk, given the importance of corporate governance factors for shareholders' opportunity to exploit risk-shifting incentives (Iannotta et al., 2007 Forssbæck, 2011), and more generally of ownership structure for banks' risk-taking and performance (Angkinand and Wihlborg, 2010 Shehzad et al., 2010). We measure ownership concentration as the combined common stock ownership share of the five largest public owners.

An important determinant of bailout probabilities for individual banks is the overall, "aggregate" expectation among market participants of the availability of implicit guarantees. Duchin and Sosyura (2014) argue that the wave of recapitalizations that followed in response to the financial crisis (in the U.S. and elsewhere) altered perceptions of the safety net – effectively a shock to (unconditional) bailout expectations, which needs to be taken into consideration in the modeling of bailout probabilities at the individual bank level. In our estimation setting, this consideration is closely related to the more practical point that we do not observe recapitalizations before the announcement of the TARP program. This does not mean, however, that the bailout probability of a given bank was zero during this period, but it is likely that, conditional on individual bank characteristics alone, it was quite a bit lower than after the program had been announced. As clarified in subsection 3.1, we specify a simple probit model to account for changes in the unconditional bailout probability (or, differently viewed, for selection bias in the observation of actual recapitalizations), where the dependent variable assumes unit value for each period when the U.S. Treasury has a non-zero TARP investment in any of the banks in the sample (effectively from Quarter 4, 2008, until the end of the sample period). We assume that the probability of observing government support depends primarily on the likelihood of a systemic event, and look for variables that proxy this. This is challenging, because the likelihood of a systemic event may be closely related to processes driving individual bank distress risk. Again, the validity of the resulting variable as an exclusion restriction for bailout probability is ultimately determined by the identification tests.

First, we use two indicators of general financial market conditions: the quarterly excess return on the S&P500 index over the three-month Treasury bill rate, and the VIX index, which is the implied volatility from S&P500 index options and measures the market expectation of stock-market volatility over a 30-day horizon. End-of-quarter figures for both variables are observed for each period during the sample period. Second, we use a more direct measure of aggregate systemic risk. Specifically, we employ the nonparametric Value-at-Risk measure proposed by Allen et al. (2012), defined as the cut-off point for the lower one percentile of quarterly excess stock returns for financial firms in each quarter.[16] Finally, we use quarterly U.S. real GDP growth as a general indicator of macroeconomic conditions.[17] The resulting inverse Mills ratio from the probit model enters as an identifying variable for bailout probability. Note that this variable only varies over time.

Given our definition of bailout (bank *i* is in a "state of bailout" from the point it receives a recapitalization until it is fully repaid), it is relevant in modeling bailout probabilities to take into account not only what makes a bank receive a capital injection, but that *repayment* (exit from the rescue program) is also to some extent predictable. Bayazitova and Shivdasani (2012) provide the most detailed

[16] Allen et al. (2012) use a monthly, rather than a quarterly, frequency, and consequently use monthly returns. Financial firms are defined as all firms with primary one-digit SIC equal to 6. We also considered the corresponding nonparametric expected-shortfall (*ES*) measure suggested by Allen et al. (2012), defined as the average of the returns on financial firms less than or equal to the 1% nonparametric *VaR*. Our results are insensitive to the choice between *ES* and *VaR*.

[17] We also considered the unemployment rate as an indicator of macroeconomic conditions, and the number of failed FDIC-insured banks in each quarter (available from fdic.gov) as an alternative indicator of systemic risk, but these were excluded because both perfectly predict a large number of outcomes in the binary dependent variable.

analysis of CPP participation to date, estimating both the determinants of banks' application decision, the Treasury's approval, banks' final participation decision (conditional on the Treasury's approval), and banks' repayment decision. They find a significant negative association between pre-crisis CEO compensation levels and the probability of entering CPP and, particularly, a positive effect of CEO compensation on the probability of exit. A positive association between CEO compensation levels and the probability of exiting the program is also supported by the findings of Wilson and Wu (2012). The explanation lies in the increasingly tough requirements on participating banks, including prohibitively high taxation on top management pay beyond a certain threshold, imposed by the Treasury from February 2009 and onward, in the midst of sharp political debate over the justifiability of the bailout program and public outrage at the behavior of banks (not least regarding management compensation practices). Banks' participation and, particularly, exit decisions may therefore have been influenced by the personal cost in terms of forgone salary that top management incurred by accepting government support (and possibly by the cost to shareholders of being unable to compete for top management talent caused by the acceptance of an effective cap on management compensation levels). Although this cost of accepting government support would not have been known until 2009 onward (and thus may have had limited effect on bailout probabilities during the early part of the sample period), the ability of CEO compensation to explain both entry into and exit from the CPP makes it – given our definition of the "state of bailout" – a candidate instrument for bailout probability. Compensation is measured as the natural logarithm of the CEO's "Total calculated compensation" in USD, as reported in Capital IQ. The amount is observed annually (and is therefore the same for all quarters within a calendar year), and is the sum of all executive and director compensation (salary, bonus, and all other forms of cash and non-cash compensation).

As a third potential instrument for bailout probability, we use the quadratic term of the capital ratio. The reasoning is as follows. As a proxy for bailout we use participation in the CPP, but there are two reasons why a particular bank did *not* participate: either because it was deemed unqualified (and possibly did not even apply for that reason), or because it was overqualified and did not apply (or declined to accept CPP funds even after having been approved). Existing evidence on the determinants of participation in the CPP is consistent with the notion that CPP funds were apportioned according to the program's stated purpose of supporting banks that suffered temporary capital and liquidity shortages due to the crisis, but were fundamentally healthy, and had long-term viable operations (Bayazitova and Shivdasani, 2012 Duchin and Sosyura, 2012). The implication is that both the "worst" and the "best" banks did not participate. Because the primary problem solved by a recapitalization is capital shortage, we may thus expect that banks both with the lowest and the highest capitalization did not get bailed out – i.e., the capital ratio has a non-monotonic effect on bailout probability (see, for example, Li, 2013). It is unlikely that the capital ratio has a similarly non-monotonic association with distress risk, which makes the squared capital ratio a candidate identifier for bailout probability.

Finally, a number of studies have shown that banks' political connections influenced their likelihood of receiving capital injections under TARP (Duchin and Sosyura, 2012, 2014 Li, 2013), that political variables influence bailout probabilities more generally (Dam and Koetter, 2012), and that banks with stronger political connections were less likely to repay TARP funds (Bayazitova and Shivdasani, 2012). We use the following proxies of political connections. First, we use a dummy variable that takes on unit value if the elected member of the House of Representatives from the congressional voting district where bank i is headquartered sat on the House's Financial Services Committee in period t (Duchin and Sosyura, 2012, 2014 Li, 2013). Second, we use a dummy variable that is equal to one if the local House representative was a Democrat. This variable measures possible ideological differences between Democrats and Republicans in attitudes toward bailouts (Li, 2013 Ng et al., 2011). Third, we use the percent of campaign contributions from local FIRE (Finance, Insurance, and Real Estate) industries in total contributions received by the House representative of the bank's voting district in the latest preceding electoral cycle (Li, 2013). All political variables are observed bi-annually. We collect data on congressional districts from the U.S. Census Bureau (census.gov), and retrieve names, parties, and committee memberships for local House representatives from the archives of the House of Representatives (history.house.gov) for the 108th-113th Congresses. Campaign contributions for local representatives are collected from the Center for Responsive Politics (opensecrets.org) for electoral cycles 2004-2012.

Throughout the remainder of the paper, we use four different combinations of exclusion restrictions for distress risk and bailout probability, respectively, as summarized in Table 1.

Table 1: Instrumental variables

Variables	Model 1	Model 2	Model 3	Model 4
Distress risk	Loan growth Loan concentration (HHI)	Loan growth Loan concentration (HHI)	Loan growth Loan concentration (HHI) Market-to-book Ownership concentration	Loan growth Loan concentration (HHI) Market-to-book Ownership concentration
Bailout probability	CEO compensation Capital ratio squared Inverse Mills ratio (TARP selection)	CEO compensation Capital ratio squared Inverse Mills ratio (TARP selection) Financial services committee Democrat FIRE campaign contribution	CEO compensation Capital ratio squared Inverse Mills ratio (TARP selection)	CEO compensation Capital ratio squared Inverse Mills ratio (TARP selection) Financial services committee Democrat FIRE campaign contribution

This table lists the exclusion restrictions for distress risk and bailout probability respectively for the four specifications of our model.

3.2.3. Sub-debt selection and spread

As previously defined, the dependent variable in the sub-debt selection equation (7) takes on the value one if bank i had outstanding subordinated debt in period t, zero otherwise. Besides predicted distress risk and predicted bailout probability, the independent variables include two dummy variables – one indicating recent sub-debt issuance, as previously defined, and one accounting for the guarantee on new unsecured debt issues under TLGP. The guarantee was announced in conjunction with the TARP/CPP program, and applied to debt issued until December, 2009, and the guarantee indicator therefore takes on unit value between the last quarter of 2008 and the last quarter of 2009. The remaining independent variables in Equation 7 are closely in line with Covitz et al. (2004), and are as follows. First, we use two indicators of loan quality – the ratio of non-accruing loans to total assets, and the ratio of accruing loans past due ninety days or more to total assets. We also include market leverage, defined as the ratio of book liabilities to the market value of equity. These variables complement predicted distress risk as indicators of general financial soundness, and may be expected to negatively influence the probability of having outstanding sub-debt, insofar as higher asset risk and/or a deteriorating balance-sheet position make uninsured debt a more expensive source of funding for banks. To account for the possibility of an influence from tax shield benefits on banks' propensity to issue uninsured debt, we include a proxy of each bank's marginal tax rate, defined as reported income tax divided by total net income. In addition, we replace the supervisory ratings used by Covitz et al. (2004) by the ratio of total regulatory capital over risk-weighted assets, to account for the possibility that supervisors may put pressure on banks to raise regulatory capital if capital adequacy ratios deteriorate. Last, we use the following variables included in the vector $\mathbf{X_C}$ and defined in subsection 3.2.1: the ratio of other real estate owned to total assets, size, and liquidity gap.

Finally, the only variables used in the final spread regression and not yet defined are the dependent variable and the financial crisis indicator variable. The latter takes on unit value between Quarter 4, 2007, and Quarter 2, 2009, which corresponds to recession quarters during the period, as classified by the NBER. For some of the SNDs in the sample, spreads over the yields on Treasury bonds of corresponding maturity are readily available in Capital IQ, for others not. We therefore opt to calculate all spreads ourselves, to make sure we get consistent spread estimates. To calculate the spreads, we start by collecting end-of-quarter yields for all available SNDs issued by the sample banks during the sample period. The yields used are yields to maturity accounting for possible callability before maturity ("yield to worst"). For each period, we then estimate yield curves for Treasury bonds, using monotone cubic hermite interpolation of the yields on 1-month to 30-year constant-maturity U.S. Treasury bonds.[18] For each SND and time period, we then obtain the spreads by subtracting the estimated Treasury yield for the maturity corresponding to the time to maturity of the SND. For the SNDs where spreads are readily available, our calculated spreads are practically identical to "spread to worst" in Capital IQ.

3.2.4. Descriptive statistics

Summary statistics for included variables appear in Table 2. The "true", observed distress risk (Z-score) has a mean of -98, which – if interpreted directly in terms of (negative) distance to default – is relatively low. It is also quite skewed, with a minimum of -2585 and a maximum of 2.3 (which, again with a distance-to-default interpretation, is equivalent to insolvency). Further down the table, we also report predicted distress risk from Models 1-4, as defined in subsection 3.2.2. The predicted distress risks from all models have means relatively close to the true mean value, but narrower distributions and – predictably – do not reproduce the skew in the observed variable. Predicted bailout probabilities all have an average of approximately 20 percent, and show observations within the full possible inter-

[18]Until Quarter 4, 2005, yields on constant-maturity Treasuries are only available up to 20 years maturity; the estimated yield curves for pre-2006 are therefore extrapolated until 30 years if necessary.

val range between 0 and 100 percent. Other bank-specific characteristics display plausible and relatively un-noteworthy distributions. The zero minimum value for CEO compensation reflects the fact that the CEOs for a few large banks in the years immediately following the crisis announced that they would only accept a token salary of one dollar as a sign of "good faith". The yields on SNDs included in the sample lie on average approximately 2.5 percentage points above the corresponding Treasury yield, but show considerable variation.

Additionally, we report correlations of model predictions of bailout probability and distress risk in Table 3. Importantly, correlations between the predicted values of bailout probability on the one hand, and predicted distress risk on the other, are relatively modest (ranging between 0.44 (Model 2) and 0.49 (Model 3), which gives a preliminary indication that these predictions – although they contain a common component – also do contain a considerable amount of "unique" variation.

Table 2: Summary statistics

VARIABLES	Num. of observations	Mean	Std. dev.	Min.	Max.
Distress risk	2,646	-97.92	141.2	-2,585	2.276
Return on assets	2,551	0.772	2.350	-18.35	19.67
Capital ratio	2,850	2.229	0.435	-2.745	4.393
Capital ratio squared	2,850	5.159	2.120	0.054	19.30
Loan concentration (HHI)	2,855	0.543	0.171	0.259	1.020
Size	2,855	10.08	1.927	5.070	14.72
Loan loss provisions	2,736	1.113	1.731	-15.64	22.74
CEO compensation	2,484	14.83	1.379	0	17.76
Market-to-book	2,684	1.628	1.441	0.013	22.37
Ownership concentration	2,515	14.82	13.51	0.000	92.73
Financial services committee	2,981	0.270	0.444	0	1
Democrat	2,981	0.725	0.447	0	1
FIRE campaign contribution	2,781	0.193	0.100	0.041	0.488
Non-interest income	2,831	0.284	0.221	-1.839	2.254
Cost-to-income ratio	2,831	0.776	0.256	0.329	5.068
Other real-estate owned	2,616	0.286	0.415	0.000	2.919
Liquidity gap	2,853	1.437	1.921	-34.02	8.314
Loan growth	2,332	9.710	20.08	-48.08	159.4
Inverse Mills ratio (TARP selection)	2,981	0.813	0.542	0.001	1.863
$\widehat{\text{Distress risk}}$-Model 1	1,951	-100.8	72.93	-414.7	171.0
$\widehat{\text{Distress risk}}$-Model 2	1,822	-105.3	74.35	-413.9	170.3
$\widehat{\text{Distress risk}}$-Model 3	1,898	-100.7	73.66	-428.0	189.0
$\widehat{\text{Distress risk}}$-Model 4	1,773	-105.2	75.13	-428.0	183.6
$\widehat{\text{Bailout probability}}$-Model 1	1,978	20.41	31.47	0	100
$\widehat{\text{Bailout probability}}$-Model 2	1,846	19.40	30.97	0	100
$\widehat{\text{Bailout probability}}$-Model 3	1,922	20.86	31.87	0	100
$\widehat{\text{Bailout probability}}$-Model 4	1,795	19.73	31.49	0	100
Spread	4783	243.6	367.3	-448.3	9789

This table summarizes the observed distress risk; the common explanatory variables for distress risk and bailout probability, i.e. return on assets (in percent), the log value of capital ratio (in percent), liquidity gap, non-interest income, the log value of size (in millions of dollars), loan loss provisions (in percent), cost-to-income ratio, and other real-estate owned (in percent); the instrumental variables for distress risk, i.e. loan growth (in percent), loan concentration (HHI), market-to-book, and ownership concentration (in percent); the instrumental variables for bailout probability, i.e. the log value of CEO compensation (in dollars), the squared log value of capital ratio (in percent), inverse Mills ratio (TARP selection), financial services committee (dummy variable), democrat (dummy variable), and FIRE campaign contribution; estimated distress risk and bailout probability (in percent); bond spread (in bp).

Table 3: Correlations between the estimated bailout probabilities and distress risk

		Distress risk				Bailout probability			
		Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Distress risk	Model 1	1							
	Model 2	0.9925	1						
	Model 3	0.9825	0.9734	1					
	Model 4	0.9726	0.9801	0.9907	1				
Bailout probability	Model 1	0.4703	0.449	0.4787	0.4573	1			
	Model 2	0.4591	0.4443	0.4732	0.4553	0.9932	1		
	Model 3	0.4693	0.4473	0.4851	0.4644	0.9925	0.9888	1	
	Model 4	0.4564	0.4417	0.4772	0.4606	0.9821	0.9915	0.9926	1

4. Results

4.1. Identification

Although in the main results we rely on a combination of regular fixed-effects linear panel models and random effects probit models, for identification testing we rely exclusively on linear models – i.e., the reduced form equation 5 and the structural equation 3 for bailout probability are estimated using linear probability models – for two reasons. First, linear tests of identification of dichotomous variables is stricter, in the sense that it ensures that identification is based on the considered exclusion restrictions rather than functional form. Second, this approach allows for the use of a richer set of identification tests (the availability in the literature of identification tests applicable to panel probit models is limited). Specifically, we use the GMM continuously updated estimator (CUE) of Hansen et al. (1996), as this estimation method shows better finite-sample properties than alternative (two-step) IV/GMM procedures, particularly in the presence of possible weak instruments (Baum et al., 2007). Identification involves full estimation of the structural equation system for distress risk and bailout probabilities, i.e., estimation of the reduced-form equations 4 and 5 (the first stage), and thereafter estimation of the structural equations 2 and 3 with the endogenous right-hand side variables replaced by their first-stage predictions. To conserve space, we report only the outcome of the identification tests, not the full set of parameter estimates. We report the following. From the first stage, the Kleibergen-Paap LM tests the rank condition for the coefficients of the instruments considered, with the null hypothesis that the model is not identified; the Anderson-Rubin Wald (strictly speaking not a first-stage test) is a (joint) significance test of the endogenous regressors, and can be roughly interpreted as a weak-instruments test; where applicable, we also report tests of the joint relevance of additional instruments (with the null that they are redundant). From the second stage, we report the Hansen J statistic of over-identifying restrictions (joint test of orthogonality restrictions with the null that instruments are valid), and an endogeneity test interpreted in line with the standard Hausman test.

The results are reported in Table 4. They indicate no identification concerns regarding bailout probabilities (Panel A). Tests for under-identification as well as weak identification are clearly rejected, and we are unable to reject the validity of the exclusion restrictions for all models. Panel A also shows that the considered proxies of banks' political connections are redundant as instruments, and that the null of exogeneity of bailout probability in the structural equation for distress risk is strongly rejected. As regards the identification of distress risk (Panel B), a somewhat different picture emerges. First, when instrumented only by past loan growth and loan concentration in Models 1 and 2, distress risk is unidentified (consequently, the redundancy of the market-to-book ratio and ownership concentration in Models 3 and 4 is also rejected). In Models 3 and 4, under-identification is rejected (although in Model 3 only at the 10 percent level) and the weak-instruments-robust inference for distress risk suggests it is highly significant. Nonetheless, the overall impression is that identification of distress risk is weaker than that of bailout probability. The validity of instruments for distress risk is somewhat weaker when the political variables are not included as instruments for bailout probability. Finally, we do not as strongly reject exogeneity of distress risk in the structural equation for bailout probability as vice versa. In what follows, we report full results for Models 1-4, but the discussion will emphasize Models 3 and 4, considering that the model is not fully identified for Models 1 and 2 (as will become apparent, the final results are relatively insensitive to the choice of model, hence, to the exact choice of instruments used).

Table 4: Identification tests

	Model 1	Model 2	Model 3	Model 4
Panel A. Structural equation for distress risk				
(identification of bailout probability)				
First-stage results				
Kleibergen-Paap rank LM-test (χ^2)	26.79***	27.90***	19.26***	18.44***
Anderson-Rubin Wald test (χ^2)	25.95***	33.79***	12.79***	16.27**
LM-test of redundancy of political var's (χ^2)	n/a	2.69	n/a	2.34
Second-stage results				
Hansen J-stat. (χ^2)	1.43	6.04	1.64	5.69
Endogeneity-test of bailout prob. (χ^2)	13.31***	18.11***	10.45***	11.38***
Panel B. Structural equation for bailout probability				
(identification of distress risk)				
First-stage results				
Kleibergen-Paap rank LM-test (χ^2)	0.630	0.200	8.83*	9.77**
Anderson-Rubin Wald test (χ^2)	4.44	3.45	24.39***	23.26***
LM-test of redundancy of M/B and ownership conc. (χ^2)	n/a	n/a	7.57**	9.00**
Second-stage results				
Hansen J-stat. (χ^2)	0.238	0.089	7.14*	5.75
Endogeneity-test of distress risk (χ^2)	0.999	0.329	7.22**	3.63*

This table reports the identification tests on the validity of the exclusion restrictions for distress risk and bailout probability in our model. Kleibergen-Paap LM test is an underidentification test with the null hypothesis that the model is not identified. Anderson-Rubin Wald test is a joint test of the endogenous regressors and orthogonality conditions, and a weak-instruments test (with the null hypothesis that the instruments are weak) when the orthogonality conditions are valid. LM-test of redundancy are for additional instruments (with the null that they are redundant). Hansen *J* statistic of overidentifying restrictions is a test of orthogonality restrictions with the null that instruments are valid, and the endogeneity test is in line with the standard Hausman test. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

4.2. Main results

For the main results, we suppress reporting of the initial and non-essential intermediate steps in the estimation procedure and focus on three sets of results, reported in Tables 5-7. Table 5 reports estimation results of the structural equation for distress risk. Here, as well as in all subsequent tables, parameter estimates for the models are reported with bootstrapped standard errors because regressors are predicted and thus observed with measurement error. Of primary interest is the estimated effect of predicted bailout probability, which is consistently positive and highly significant, with relatively stable coefficient values (regardless of model) around one, suggesting that a one standard deviation increase in bailout probability (between 31 and 32 percentage points for Models 3 and 4) increases distress risk by approximately 1/4 of a standard deviation. The only other bank-specific characteristics that are significant at least at the 5 percent level are the liquidity gap, the cost-income ratio, and the market-to-book ratio – all of which have reasonable signs and magnitudes (a higher liquidity gap implies better asset coverage against short-term liabilities, thus lower liquidity risk). The structural estimation results for bailout probability are reported in Table 6. Again, primary interest lies in estimates for predicted distress risk. The results indicate that distress risk has a considerably weaker effect on bailout probability than the other way around, and is only significant at least at 5 percent in one of the considered model specifications. The magnitude is also much smaller: a one standard deviation increase in distress risk increases bailout probability by about 1/40 of a standard deviation (i.e., the effect is about 1/10 of the size of the effect going the other way). Part of this could come down to the comparatively weak identification of distress risk. However, an interesting and potentially important implication follows from the small effect of distress risk on bailout probability (and particularly its insignificance in Models 1 and 2): because the predictions are made from the reduced-form equation using all $\mathbf{X}$ variables, it shows that the common set of banks characteristics $\mathbf{X_C}$ and the instruments $\mathbf{X_B}$ for bailout probability do *not* give rise to a strong relationship between $\hat{D}$ and $\hat{B}$. The most important other bank characteristic determining bailout probability is loan loss provisions, the effect of which has an intuitive sign (higher projected loan losses increase bailout probability), but is modest in magnitude. The time-varying-only inverse Mills ratio from the program selection equation is also consistently significant, whereas other independent variables are only occasionally and weakly significant.

The results of the main spread regressions, which are the most central of our results, appear in Table 7. For each of Models 1-4 we report two specifications: one including only distress risk and bailout expectations and interaction between the two (as well as the inverse Mills ratio from the sub-debt selection equation), and one including also the crisis dummy and an interaction term between crisis and bailout probabilities to account for possible direct crisis effects on market discipline. A first glance reveals the importance of taking a crisis effect explicitly into account (the right of the two columns reported under each of the models). As can be seen from the table, the results overall are relatively insensitive to the model choice, but point estimates (and significance levels) vary primarily as a reflection of whether or not an explicit crisis effect is included. All main independent variables of interest enter the regression both individually and as part of one or more interaction terms, and are thus assumed to have a non-linear effect on spreads. We focus on Models 3 and 4.

Table 5: Fixed-effect estimation for distress risk in the structural equation 2

VARIABLES	Model 1	Model 2	Model 3	Model 4
Capital ratio	6.227	9.019	-17.12	-20.36
	(20.04)	(18.83)	(22.82)	(25.81)
Return on assets	2.445	2.308	2.943	2.873
	(2.41)	(2.18)	(2.67)	(2.02)
Liquidity gap	-12.21**	-12.38**	-14.30**	-14.16**
	(5.40)	(6.03)	(6.57)	(6.66)
Non-interest income	22.07	18.69	14.74	8.34
	(21.09)	(24.62)	(16.87)	(19.75)
Size	13.66	9.58	-15.88	-25.63
	(26.07)	(21.35)	(20.21)	(24.44)
Loan loss provisions	3.915	3.590*	2.216	1.635
	(2.68)	(1.87)	(2.59)	(2.13)
Cost-to-income ratio	43.83***	36.77**	43.40***	35.88**
	(14.47)	(16.78)	(15.63)	(15.02)
Other real-estate owned	23.67	20.00	12.59	3.16
	(26.01)	(26.92)	(22.92)	(20.16)
Loan growth	0.159	0.106	0.338*	0.319
	(0.20)	(0.18)	(0.19)	(0.21)
Loan concentration (HHI)	-70.72	-33.86	-71.15	-18.49
	(144.5)	(163.2)	(146.0)	(142.6)
Market-to-book			-26.67**	-26.19**
			(11.95)	(12.23)
Ownership concentration			0.371	0.923
			(1.07)	(1.66)
$\widehat{\text{Bailout probability}}$	1.108***	1.151***	0.944***	1.010***
	(0.25)	(0.21)	(0.24)	(0.30)
Constant	-273.3	-252.4	119.9	197.8
	(286.5)	(219.2)	(259.1)	(286.2)
Observations	1,951	1,822	1,898	1,773
R-squared	0.096	0.088	0.106	0.099
Number of banks	91	87	86	82
Chi2	111.5***	123.8***	77.71***	114.3***
Rho	0.274	0.254	0.277	0.329

In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

Table 6: Random-effect estimation for bailout probability in the structural equation 3

VARIABLES	Model 1	Model 2	Model 3	Model 4
CEO compensation	-0.124	-0.127	-0.070	-0.095
	(0.13)	(0.20)	(0.26)	(0.16)
Capital ratio squared	-1.208*	-1.425**	-0.948	-1.237
	(0.66)	(0.69)	(0.76)	(0.95)
Capital ratio	3.105	3.202	2.411	2.746
	(2.85)	(2.90)	(3.30)	(4.27)
Return on assets	0.013	0.024	-0.033	-0.013
	(0.07)	(0.08)	(0.10)	(0.14)
Liquidity gap	-0.013	0.031	0.132	0.138
	(0.07)	(0.08)	(0.09)	(0.10)
Non-interest income	-0.824	-1.271*	-1.181	-1.535*
	(0.86)	(0.71)	(0.91)	(0.88)
Size	0.167	0.225	0.122	0.223
	(0.14)	(0.25)	(0.23)	(0.20)
Loan loss provisions	0.503***	0.500***	0.396***	0.419***
	(0.09)	(0.14)	(0.10)	(0.15)
Cost-to-income ratio	0.170	0.332	-0.429	-0.110
	(0.74)	(0.79)	(0.74)	(0.85)
Other real-estate owned	1.267**	0.953*	0.814	0.695
	(0.57)	(0.55)	(0.69)	(0.56)
Financial services committee		-0.482		-0.411
		(0.39)		(0.51)
Democrat		0.614		0.419
		(0.38)		(0.47)
FIRE campaign contribution		0.365		-1.086
		(2.01)		(2.31)
$\widehat{\text{Distress risk}}$	-0.000	0.001	0.011**	0.010*
	(0.00)	(0.00)	(0.00)	(0.01)
Inverse Mills ratio (TARP selection)	-1.014***	-0.941***	-0.575**	-0.560**
	(0.17)	(0.19)	(0.23)	(0.28)
Constant	-2.805	-2.866	-1.607	-1.862
	(3.58)	(3.69)	(4.53)	(5.59)
Observations	1,951	1,822	1,898	1,773
Number of banks	91	87	86	82
Chi2	335***	243.4***	254.2***	304.4***
Pseudo R2	0.432	0.443	0.428	0.441
Rho	0.722	0.776	0.767	0.802

In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

Table 7: Fixed-effect estimation for spread

VARIABLES	Model 1		Model 2		Model 3		Model 4	
$\widehat{\text{Distress risk}}$	1.977***	0.577***	1.537***	0.511***	2.437***	1.013***	2.018***	1.067***
	(0.20)	(0.18)	(0.22)	(0.17)	(0.23)	(0.23)	(0.22)	(0.26)
$\widehat{\text{Bailout probability}}$	2.221***	2.896***	2.923***	3.062***	1.672***	2.478***	2.255***	2.342***
	(0.51)	(0.44)	(0.59)	(0.41)	(0.50)	(0.47)	(0.55)	(0.43)
$\widehat{\text{Bailout probability}}$ * $\widehat{\text{Distress risk}}$	-0.010**	-0.012***	-0.007	-0.011**	-0.009*	-0.012***	-0.008	-0.013***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
I_{crisis}		273.4***		267.1***		246.3***		241.4***
		(38.45)		(44.35)		(35.07)		(44.22)
$I_{crisis} * \widehat{\text{Distress risk}}$		1.428***		1.388***		1.200***		1.267***
		(0.43)		(0.47)		(0.34)		(0.42)
Inverse Mills ratio (sub-debt selection)	82.5**	172.6***	118.4**	115.2**	65.8	169.7***	10.0	41.1
	(36.96)	(47.79)	(54.00)	(52.25)	(40.40)	(53.39)	(62.86)	(57.51)
Constant	351.6***	145.4***	298.4***	162.8***	407.5***	194.9***	383.6***	248.0***
	(19.06)	(25.69)	(25.76)	(25.07)	(25.74)	(37.29)	(28.02)	(33.21)
Observations	3,618	3,618	3,538	3,538	3,630	3,630	3,534	3,534
R-squared	0.179	0.247	0.179	0.245	0.201	0.257	0.200	0.254
Number of SNDs	235	235	234	234	235	235	233	233
Chi2	283.5***	421.8***	192.5***	538.5***	307.8***	438.8***	270.3***	284.3***
Rho	0.465	0.474	0.646	0.681	0.492	0.477	0.478	0.485

I_{crisis} is a period dummy assuming unit value from Quarter 4, 2007 to Quarter 2, 2009, which is the contraction period defined by NBER. In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

Starting with the effect of (predicted) distress risk, the results suggest the presence of market discipline: SND holders respond to observed distress risk and price SNDs accordingly, with higher distress risk corresponding to higher yield spreads. At the mean value of bailout probability, and assuming no crisis, the total estimated marginal effect of distress risk on yields is approximately 0.8, suggesting that a one standard deviation increase in distress risk increases the yield spread by an average of 61 basis points. If the crisis dummy is "switched on", this effect more than doubles to a total marginal effect of 2.1, corresponding to a 157 basis point increase in the spread per standard deviation increase in distress risk. Theoretically, the stand-alone effect of bailout probability on spreads could go either way: on the one hand, a bank that is more likely to be bailed out is also in worse financial shape, which motivates a higher average spread; on the other hand, the market should be softer on banks with a higher bailout probability. The data shows that the effect of bailout probability is consistently positive and significant, with an order of magnitude comparable to that of distress risk: at the mean value of distress risk, the total marginal effect of predicted bailout probability is approximately 3.7, suggesting that a one standard deviation increase in bailout probability raises the spread by 115 basis points.

The third row in Table 7 constitutes the core results in the paper. With a continued focus on Models 3 and 4, these results show that a higher bailout probability reduces the risk sensitivity of spreads and that, when the substantial direct crisis effects on spreads are accounted for, this effect is strongly significant. Although coefficient estimates appear small, the effect is also economically important. A one standard deviation increase in bailout probability from the sample mean reduces the total marginal effect of distress risk on spreads to 0.4, i.e., the risk sensitivity of spreads is reduced by half. These results are consistent with a nontrivial moral hazard effect, through which the market discipline exerted by banks' uninsured debt-holders is substantially reduced as bailout expectations rise. It should be noted that directly accounting for the crisis, which coincides with the period when the largest number of banks in our sample were in a "state of bailout", and which operates in the opposite direction on market discipline, *strengthens* this effect. It is, therefore, not the result of generally more erratic pricing of debt during the crisis (e.g., as a result of a "panic effect"), which loosens the association between observable risk and spreads. Finally, we note that the crisis did not just increase the risk sensitivity of spreads, but also uniformly raised spreads by at least 240 basis points on average.

In the alternative specifications that follow, we address two questions: first, whether the observed reduction in market discipline as a result of increased bailout probabilities around TARP was temporary or lasting; second, if the reduction applies generally to all banks in the sample, or if the results are driven by the inclusion of the largest U.S. banks (which are also overrepresented in the sub-sample of SND issuers).

4.3. Alternative specifications

4.3.1. A transitory or lasting effect? Cross-sectional tests

The indicator variable used to estimate bailout probabilities in our main results equals one only until the recapitalization is fully repaid. Given that the timing of the sample banks' entry into and exit from TARP differs, this definition makes the best possible use of the available information of when they had access to government support. However, as we have argued, there may be both a specific and a general effect of government support: a bailout affects the supported banks, but also non-supported banks via expectations. The expectation effect is not necessarily related to the specific time period when supported banks actually had access to support funding, but may be more persistent. In addition, our main estimation method relies on the assumption that bailout probabilities before the TARP was announced were determined by the same factors determining participation in the CPP, which is not necessarily the case – particularly considering the exceptional circumstances under which the program was launched, and that it was open for voluntary applications from any banks. For example, assuming that bailout probabilities before TARP were approximately zero may be just as accurate.

The persistence of the expectation effect created by TARP specifically is *a priori* not clear. On the one hand, the official stance of the Treasury was that TARP was not an unconditional bailout of ailing banks, but a means of bridging capital and liquidity shortages of fundamentally sound banks during a period of severe stress in order to avoid a disruption in the flow of credit to the non-financial private sector. Although a (limited) number of banks received more than one capital injection under TARP, the program was not a commitment to keep bailing out supported banks in case of continuing deterioration of their financial health. According to Cornett et al. (2013), by October, 2010, more than 145 participating institutions had missed at least one dividend payment to the Treasury, and four had failed completely. By the end of 2013, five years after the launch of the program, another four CPP-supported, FDIC-insured banks had failed (fdic.gov). Thus, receiving support under TARP was not a guarantee that a bank would not be allowed to fail. This would not have been known at the launch of the program, but, although it may initially have been a fair assumption that the Treasury would not allow any outright losses on taxpayer money invested in the supported banks, it would have become gradually clear to the market that it might. This argument suggests a transitory effect of the program consistent with our main results.[19]

On the other hand, the fact that also apparently unhealthy banks were recapitalized, and the fact that the criteria for the Treasury's approval were not disclosed, may have caused TARP to be perceived as a *de facto* unconditional bailout and dented the credibility of the Treasury's official rhetoric. Existing empirical stud-

[19] Veronesi and Zingales (2010) argue that it is unlikely that any major portion of the value created in the bailout of October 2008 was due to expectations of future bailouts because of the political opposition to the TARP program, and because U.S. government CDS rates dropped at the time, suggesting that the market did not foresee a deterioration of government finances due to future major bailout packages. Duchin and Sosyura (2014) take the opposite view, referring to Standard & Poor's motivation of the negative outlook revision for (and subsequent downgrade of) the U.S. sovereign rating in 2011: increased risks in the U.S. financial sector and higher projected fiscal costs to the U.S. government of resolving another potential crisis.

ies using a difference-in-difference-type approach to examine the effect of recapitalizations mostly assume that the effect is lasting and define treatment periods accordingly (e.g., Mariathasan and Merrouche, 2012, studying the effect of recapitalizations on lending). Black and Hazelwood (2013), estimating the effect of TARP support on the risk of new loan originations, find that this effect is weaker when the treatment indicator is "switched off" after repayment, suggesting that the effect of TARP on risk-taking remained beyond the period of time the banks actually had access to the support.

As a simple approach to accounting for the possibility that bailout expectations were created with the launch of the program, and remained beyond its formal termination, we estimate a cross-sectional probit model for Quarter 3, 2008, where the dependent variable assumes unit value for all sample banks that received government support under TARP at any point during the period when the program was active, and zero otherwise.[20] In subsequent spread regressions, we make the somewhat naïve assumption that the bailout probability equals zero for all banks prior to Quarter 4, 2008, and takes on the predicted value from the cross-sectional probit from Quarter 4, 2008, until the end of the sample period. In other words, we estimate:

$$S_{jit} = \gamma_{0,j} + \beta_1 \hat{D}_{it} + \beta_2 \hat{B}_{cs,it} + \beta_3 \hat{D}_{it} \times \hat{B}_{cs,it} + \gamma_1 I_t + \gamma_2 \hat{D}_{it} \times I_t + \gamma_3 IMR_{sub,it} + u_{jit} \tag{10}$$

where $\hat{B}_{cs,it}$ equals zero for all i before Quarter 4, 2008, and takes on the cross-sectionally estimated bailout probability for all time periods thereafter. Besides accounting for a possible lasting effect of being a bank with a high possibility of being bailed out, this approach gives a "snapshot" of bailout probabilities, which better reflects the true determinants of entry into TARP and may more accurately

[20]This specification closely corresponds to those used in previous studies of the determinants of CPP participation, which rely exclusively on cross-sectional specifications (see Bayazitova and Shivdasani, 2012 Li, 2013, among others).

reflect the moral hazard consequences of TARP specifically.

The results are reported in Table 8. They show both similarities and differences with the main results in the previous section. First, the effect of distress risk is, again, positive and significant, with an order of magnitude similar to that in the main results (or higher, when the crisis is not accounted for). This is an expected result, since the only thing that differentiates predicted distress risk as measured in Table 8 is that it is predicted without the effect of the inverse Mills ratio from the program selection probit in the reduced form (the inverse Mills ratio is redundant when bailout probability is estimated cross-sectionally). The weak identification of distress risk in Models 1 and 2 is evident from the fact that its stand-alone effect is rendered insignificant when the crisis effect is included. The effect of bailout probability, by contrast, is markedly weaker than in the main results, with coefficient estimates ranging from (insignificantly) negative to positive with varying levels of significance. This gives an initial indication of reduced precision in the cross-sectional approach to the measurement of bailout probability. The interaction term between distress risk and bailout probability show coefficient estimates that are consistently negative with values close to those in the main results. However, when the crisis effect is included, they are also consistently insignificant. The likely "mechanics" of this result is that when the crisis is not explicitly accounted for, the substantially higher spreads during this period are picked up by the predicted distress risk (which is also higher during the crisis, and shows considerably higher coefficients in the specification where the crisis indicator is not included). As the crisis subsides, spreads diminish, which shows up as a significantly negative interaction term between risk and bailout probability (as the latter stays constant for the "after" period). It is thus unlikely that this effectively measures a moral hazard effect on market discipline. By contrast, when the crisis indicator *is* included, its estimated effects (directly on spreads and in terms of accentuating market discipline) are close to those estimated in the main results.

Table 8: Fixed-effect estimation for spread with the cross-sectional estimates of bailout probability

VARIABLES	Model 1		Model 2		Model 3		Model 4	
$\widehat{\text{Distress risk}}$	3.817***	1.132*	3.534***	0.776	4.067***	1.925***	3.730***	1.502**
	(0.25)	(0.66)	(0.23)	(0.52)	(0.22)	(0.53)	(0.28)	(0.62)
$\widehat{\text{Bailout probability}}$	-0.068	1.910**	0.187	2.131***	-0.376	1.202**	-0.126	1.537*
	(0.41)	(0.75)	(0.38)	(0.65)	(0.36)	(0.59)	(0.40)	(0.87)
$\widehat{\text{Bailout probability}}$ * $\widehat{\text{Distress risk}}$	-0.015***	-0.002	-0.013***	-0.001	-0.011***	-0.004	-0.009**	-0.002
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
I_{crisis}		323.3***		337.9***		275.6***		291.5***
		(65.36)		(63.10)		(53.94)		(69.54)
$I_{crisis} * \widehat{\text{Distress risk}}$		1.241***		1.308***		1.181***		1.203**
		(0.40)		(0.45)		(0.39)		(0.48)
Inverse Mills ratio (sub-debt selection)	-51.18	-89.32**	-61.70	-219.1***	-59.87	-84.52*	-29.56	-171.1***
	(60.10)	(45.16)	(69.10)	(66.25)	(51.85)	(47.11)	(71.48)	(56.98)
Constant	553.0***	225.0***	531.1***	222.6***	602.1***	326.1***	563.2***	302.9***
	(38.38)	(74.48)	(35.67)	(67.75)	(35.37)	(65.20)	(31.56)	(81.49)
Observations	3,576	3,576	3,478	3,478	3,572	3,572	3,476	3,476
R-squared	0.162	0.248	0.160	0.251	0.205	0.265	0.201	0.267
Number of SNDs	231	231	229	229	231	231	229	229
Chi2	406.5***	332.8***	297.8***	252.3***	479***	552.5***	256.6***	439.9***
Rho	0.420	0.284	0.393	0.323	0.481	0.343	0.436	0.342

I_{crisis} is a period dummy assuming unit value from Quarter 4, 2007 to Quarter 2, 2009, which is the contraction period defined by NBER. In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

The simplified estimation framework in this section notwithstanding, a reasonable conclusion from the results is that they do not speak in favor of a lasting effect of the recapitalizations under TARP on market discipline. Clearly, the assumption that bailout probabilities were approximately zero before TARP, made a discrete jump at the announcement of the program, and then remained constant thereafter, considerably weakens the precision of measurement of bailout probability as such, whereas the effects of other variables remain relatively unchanged compared to the main results.

4.3.2. A too-big-to-fail effect? Two tests

There are a number of differences between how the recapitalizations under TARP were carried out for, and how other components of the program affected, the very largest banks compared to all other banks. First, whereas for the vast majority of recipients of recapitalizations under CPP/TARP, the receipt of government support was voluntary and subject to screening and approval by the relevant supervisor and by the Treasury, the initial ten (nine) banks were coerced into participating without prior regulatory assessment. Second, at a later stage, the largest nineteen banks were subjected to compulsory stress tests under SCAP, with more far-reaching regulatory assessments (and significant "certification" effects on equity values for the banks involved, see Bayazitova and Shivdasani, 2012).[21] It is not unlikely that these circumstances may imply that the signalling effect of large banks' participation in TARP – and, consequently, the effects on the expectations of financial market participants – may differ from that resulting from recapitalizations of other banks. This consideration, as well as the fact that large banks are over-represented among SND issuers, motivates us to further examine the main results. The ques-

[21] These banks were American Express, Bank of America, BB&T, Bank of New York Mellon, Capital One, Citigroup, Fifth Third Bancorp, GMAC, Goldman Sachs, JP Morgan Chase, Key Corp., Met Life, Morgan Stanley, PNC, Regions Financial Corp., State Street, Sun Trust, U.S. Bancorp, and Wells Fargo. Of these, only Met Life did not at any point receive capital injections under TARP.

tion we ask is essentially: are the main results driven by a TBTF effect that applies to the largest banks only? We attempt to answer this question in two ways. First, we simply follow the same estimation method as in the main results, but exclude the nine initial CPP recipients.[22] In a second round of results, we assume that either the announcement of CPP participation by the initial nine in October, 2008, *or* the announcement of detailed forward-looking supervisory assessments of the big-19 under SCAP in February, 2009, constituted effective (exogenous) declarations of which banks the U.S. government considered to be TBTF, with possible permanent subsequent effects on market discipline for these banks only. Specifically, we estimate the following "clean" difference-in-difference model of spreads:

$$
\begin{aligned}
S_{jit} &= \gamma_{0,j} + \beta_1 \hat{D}_{it} + \beta_2 Big_{it} + \beta_3 \hat{D}_{it} \times Big_{it} + \gamma_1 I_{crisis,t} \\
&\quad + \gamma_2 \hat{D}_{it} \times I_{crisis,t} + \gamma_3 I_{after,t} + \gamma_4 IMR_{sub,it} + u_{jit}
\end{aligned} \tag{11}
$$

where Big_{it} is a binary variable assuming unit value either for the initial nine CPP recipients from Quarter 4, 2008, onward, *or* for the nineteen SCAP banks from Quarter 1, 2009, onward, and zero otherwise; $I_{after,t}$ is an indicator assuming unit value from Quarter 4, 2008, onward, or from Quarter 1, 2009, onward, depending on the definition of Big_{it}; and all other variables are defined as previously. Any time-invariant heterogeneity is subsumed by issue-level fixed effects, as before.

The results from the main estimation method without the initial nine recipients are reported in Tables 9-11. The estimation results of the structural equation for distress risk (Table 9) are very close to the full-sample results, and with no material qualitative differences (only the odd marginal significance level differs). The effect of predicted bailout probability remains highly significant and with very similar point estimates. Also, the results of the structural equation for bailout probability are in all essentials similar (Table 10), but with one or two notable dif-

[22]Merrill Lynch is never present in our sample, as it went directly from being an investment bank to being acquired by BofA.

ferences. First, CEO compensation is now significantly negative in Models 1 and 2, whereas the squared capital ratio is not. The effect is in the expected direction (higher CEO compensation reduces the likelihood of bailout). Second, the effect of size becomes significantly positive when the very largest banks are excluded. The average coefficient is about 0.5, suggesting (since size is measured on a log scale) that a one percentage point increase in size increases bailout probability by about half a percentage point.

Results from the main spread regressions without the largest nine banks appear in Table 11. The dominance of the largest banks as SND issuers can be gleaned from the number of observations: although we only drop nine banks, more than half of the total issue-quarter observations are lost. Nonetheless, most of the results are not far off from their full-sample equivalents: the effect of predicted distress risk is positive and significant, likewise for predicted bailout probability; there is a large direct crisis effect on spreads, and an effect of the crisis via accentuated market discipline (higher risk sensitivity of spreads). All these effects also have similar magnitudes as in the main results (possibly with the exception of bailout probability, which appears to systematically take on roughly 50 percent higher coefficient values in the absence of the big-nine). The one major difference, however, is the effect we are most interested in: the effect of distress risk conditional on bailout probability. The interaction term between these two variables with the largest nine banks excluded is smaller than for the full sample, alternately positive and negative, but never significant. In other words, the relatively substantial moral hazard effect observed for the full sample in the form of a reduction in market discipline for higher bailout probability is not present, suggesting that it may indeed be a TBTF effect. A minor difference with the full-sample results is that self-selection into sub-debt issuance appears to be less of an issue when the largest banks are excluded, as indicated by the consistently insignificant inverse Mills ratio from the sub-debt selection equation.

Table 9: Fixed-effect estimation for distress risk in the structural equation 2 without nine big banks

VARIABLES	Model 1	Model 2	Model 3	Model 4
Capital ratio	5.836	8.973	-16.459	-20.139
	(24.34)	(23.33)	(23.89)	(24.35)
Return on assets	3.219	3.021	3.712	3.637
	(3.12)	(2.88)	(2.85)	(2.70)
Liquidity gap	-11.75**	-11.90**	-13.79*	-13.62**
	(5.72)	(5.99)	(7.68)	(6.67)
Non-interest income	23.50	19.52	15.16	7.87
	(25.77)	(29.52)	(19.58)	(28.75)
Size	16.18	10.50	-12.02	-24.42
	(33.28)	(27.12)	(25.22)	(27.33)
Loan loss provisions	4.384*	3.970*	2.579	1.926
	(2.39)	(2.40)	(2.83)	(2.21)
Cost-to-income ratio	46.62***	38.32*	46.68**	38.17*
	(16.63)	(20.72)	(18.88)	(19.82)
Other real-estate owned	21.83	17.71	11.26	1.47
	(30.54)	(26.62)	(26.37)	(24.21)
Loan growth	0.098	0.033	0.321	0.309
	(0.22)	(0.20)	(0.20)	(0.23)
Loan concentration (HHI)	-46.72	-4.57	-50.22	11.30
	(189.5)	(159.7)	(139.5)	(129.7)
Market-to-book			-26.19**	-25.78*
			(10.56)	(13.45)
Ownership concentration			0.330	0.912
			(1.18)	(1.87)
$\widehat{\text{Bailout probability}}$	1.103***	1.153***	0.969***	1.043***
	(0.22)	(0.23)	(0.26)	(0.26)
Constant	-308.6	-276.8	58.4	153.5
	(342.7)	(275.0)	(278.3)	(309.8)
Observations	1,763	1,638	1,710	1,589
R-squared	0.096	0.088	0.106	0.098
Number of banks	84	80	79	75
Chi2	140.5***	74.69***	92.19***	76.19***
Rho	0.267	0.247	0.254	0.297

The nine big banks are the initial recipients of TARP funds, which are Citigroup, Bank of America, JP Morgan Chase, Wachovia, Wells Fargo, Bank of NY Mellon, State Street Corp., Goldman Sachs, and Morgan Stanley. In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

Table 10: Random-effect estimation for bailout probability in the structural equation 3 without nine big banks

VARIABLES	Model 1	Model 2	Model 3	Model 4
CEO compensation	-0.607***	-0.544**	-0.383	-0.386
	(0.21)	(0.21)	(0.25)	(0.24)
Capital ratio squared	-1.148	-1.363	-0.946	-1.235
	(1.08)	(1.21)	(1.07)	(1.10)
Capital ratio	3.086	3.180	2.502	2.839
	(4.44)	(5.00)	(4.48)	(4.64)
Return on assets	0.011	0.027	-0.043	-0.016
	(0.08)	(0.12)	(0.07)	(0.08)
Liquidity gap	0.009	0.039	0.135	0.126
	(0.08)	(0.09)	(0.08)	(0.10)
Non-interest income	-0.423	-0.828	-0.707	-1.039
	(0.97)	(0.85)	(0.95)	(0.80)
Size	0.537**	0.597**	0.397*	0.522**
	(0.22)	(0.27)	(0.24)	(0.26)
Loan loss provisions	0.461***	0.463***	0.364***	0.393***
	(0.11)	(0.11)	(0.13)	(0.10)
Cost-to-income ratio	0.046	0.246	-0.516	-0.151
	(0.71)	(0.94)	(0.71)	(0.91)
Other real-estate owned	1.321	1.029	0.883	0.784
	(0.83)	(0.79)	(0.68)	(0.74)
Financial services committee		-0.372		-0.295
		(0.40)		(0.40)
Democrat		0.706		0.514
		(0.53)		(0.51)
FIRE campaign contribution		0.687		-0.721
		(2.16)		(2.65)
$\widehat{\text{Distress risk}}$	0.000	0.002	0.011*	0.009*
	(0.00)	(0.01)	(0.01)	(0.01)
Inverse Mills ratio (TARP selection)	-1.063***	-0.998***	-0.613***	-0.631**
	(0.19)	(0.30)	(0.23)	(0.28)
Constant	0.590	-0.624	0.148	-0.844
	(6.18)	(5.63)	(5.26)	(5.81)
Observations	1,763	1,638	1,710	1,589
Number of banks	84	80	79	75
Chi2	255.2***	510.9***	219.7***	319.9***
Pseudo R2	0.443	0.452	0.438	0.448
Rho	0.707	0.760	0.763	0.793

The nine big banks are the initial recipients of TARP funds, which are Citigroup, Bank of America, JP Morgan Chase, Wachovia, Wells Fargo, Bank of NY Mellon, State Street Corp., Goldman Sachs, and Morgan Stanley. In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

Table 11: Fixed-effect estimation for spread without nine big banks

VARIABLES	Model 1		Model 2		Model 3		Model 4	
$\widehat{\text{Distress risk}}$	1.571***	0.529	1.129***	0.381	2.147***	1.097***	1.644***	0.940**
	(0.37)	(0.39)	(0.42)	(0.27)	(0.46)	(0.42)	(0.47)	(0.47)
$\widehat{\text{Bailout probability}}$	3.581***	4.083***	4.045***	4.142***	2.813***	3.445***	3.248***	3.384***
	(0.72)	(0.67)	(0.76)	(0.63)	(0.83)	(0.74)	(0.82)	(0.80)
$\widehat{\text{Bailout probability}}$ * $\widehat{\text{Distress risk}}$	0.002	0.000	0.003	-0.000	0.000	-0.002	-0.002	-0.005
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
I_{crisis}		237.5***		244.2***		204.5***		211.9***
		(36.94)		(31.96)		(31.08)		(37.11)
$I_{crisis} * \widehat{\text{Distress risk}}$		0.972***		1.027***		0.740***		0.824***
		(0.30)		(0.27)		(0.23)		(0.32)
Inverse Mills ratio (sub-debt selection)	45.99	77.01*	77.87	74.43**	45.48	71.95	-17.39	-8.99
	(45.39)	(39.74)	(52.23)	(37.79)	(54.85)	(54.07)	(57.66)	(47.83)
Constant	339.8***	164.5***	268.8***	155.8***	416.1***	241.8***	393.1***	278.8***
	(64.73)	(62.60)	(79.07)	(51.55)	(73.69)	(69.59)	(89.51)	(81.05)
Observations	1,466	1,466	1,440	1,440	1,464	1,464	1,436	1,436
R-squared	0.443	0.510	0.431	0.500	0.485	0.538	0.465	0.521
Number of SNDs	87	87	86	86	87	87	85	85
Chi2	283.4***	351.6***	249.3***	386.7***	327.1***	641.2***	299.8***	519.3***
Rho	0.782	0.811	0.905	0.922	0.796	0.818	0.797	0.822

The nine big banks are the initial recipients of TARP funds, which are Citigroup, Bank of America, JP Morgan Chase, Wachovia, Wells Fargo, Bank of NY Mellon, State Street Corp., Goldman Sachs, and Morgan Stanley. I_{crisis} is a period dummy assuming unit value from Quarter 4, 2007 to Quarter 2, 2009, which is the contraction period defined by NBER. In parenthesis are bootstrapped standard errors. In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

The results of the difference-in-difference tests for both groups of large banks (Table 12) do not, however, bear out the presumption of a discrete and lasting shift in market discipline of TBTF banks at the announcement of TARP (or SCAP). Again, the interaction term between distress risk and the implicit guarantee proxy (here, the "Big banks" dummy, rather than predicted bailout probability) is – although it is consistently negative – never significant beyond the 10 percent level, and at 10 percent only in specifications that do not control for the crisis effect. The results yield a number of other interesting insights, however. First, the effect of distress risk is, again, consistently positive and highly significant (and larger than in the main results). Second, the effect of the crisis is virtually identical to previous specifications (with a large direct positive effect on spreads and a substantial effect via increased risk sensitivity of spreads). Third, on average, spreads are substantially higher in the post-TARP/post-crisis period (as indicated by coefficient estimates for the "after" indicator), but not – or at least much less so – for the group of largest banks, regardless of whether this group is defined as the initial nine CPP recipients or as the big-nineteen SCAP banks (indicated by the coefficients for "Big banks").

Table 12: Fixed-effect estimation for spread with big banks (difference-in-difference)

VARIABLES	Model 1-Big 9		Model 3-Big 9		Model 1-Big 19		Model 3-Big 19	
$\widehat{\text{Distress risk}}$	4.956***	2.834***	4.916***	3.079***	5.144***	2.624***	4.957***	2.870***
	(0.57)	(0.57)	(0.64)	(0.67)	(0.54)	(0.63)	(0.43)	(0.63)
Big banks	-120.1*	-123.2**	-68.9	-87.9*	-247.8***	-223.8***	-181.8***	-188.9***
	(62.82)	(57.06)	(62.83)	(51.15)	(61.95)	(77.99)	(54.59)	(59.45)
Big banks * $\widehat{\text{Distress risk}}$	-1.481*	-1.436	-0.981	-1.131	-1.157*	-0.631	-0.442	-0.260
	(0.81)	(0.89)	(1.13)	(1.09)	(0.66)	(0.69)	(0.55)	(0.65)
I_{after}	135.1***	210.6***	62.9	151.2***	207.9***	323.9***	127.9***	254.8***
	(34.96)	(36.91)	(41.85)	(49.01)	(45.75)	(55.98)	(42.10)	(55.75)
I_{crisis}		311.3***		274.8***		327.2***		279.2***
		(71.62)		(42.81)		(60.16)		(59.88)
$I_{crisis} * \widehat{\text{Distress risk}}$		1.467**		1.467**		1.342**		1.270**
		(0.71)		(0.57)		(0.57)		(0.64)
Inverse Mills ratio (sub-debt selection)	-189.7***	-140.4***	-199.5***	-151.6***	-107.9**	-104.3**	-108.6***	-89.6*
	(58.08)	(41.59)	(45.01)	(46.02)	(43.60)	(44.59)	(36.71)	(50.90)
Constant	694.7***	409.4***	721.8***	464.2***	704.7***	380.7***	718.2***	433.3***
	(55.42)	(58.69)	(61.14)	(74.59)	(52.58)	(64.14)	(50.65)	(80.99)
Observations	3,754	3,754	3,766	3,766	3,754	3,754	3,766	3,766
R-squared	0.196	0.249	0.225	0.264	0.197	0.252	0.227	0.264
Number of id	236	236	236	236	236	236	236	236
Chi2	282.3***	261.4***	211.9***	428.5***	246.4***	570***	272.7***	550.2***
Rho	0.553	0.489	0.584	0.514	0.527	0.449	0.558	0.470

"Big-9" refers to the nine initial recipients of TARP funds, which are Citigroup, Bank of America, JP Morgan Chase, Wachovia, Wells Fargo, Bank of NY Mellon, State Street Corp., Goldman Sachs, and Morgan Stanley. "Big-19" refers to the largest nineteen banks subjected to compulsory stress tests under SCAP in 2009. I_{crisis} is a period dummy assuming unit value from Quarter 4, 2007 to Quarter 2, 2009, which is the contraction period defined by NBER. Notice that I_{after} is different for "Big-9" and "Big-19". It is an indicator assuming unit value for Quarter 4, 2008 onward for "Big-9", or from Quarter 1, 2009 onward "Big-19". In parenthesis are bootstrapped standard errors. The superscripts *, **, and *** indicate statistic significance at 10% , 5%, and 1% level, respectively.

In sum, the results of the alternative specifications estimated in this subsection do suggest support for the notion that the main results of decreased market discipline for higher bailout probability are largely driven by a TBTF effect (as the effect is absent when the largest banks are dropped from the sample), but also that this effect is not lasting (in this latter sense the results here are consistent with the alternative specifications in subsection 4.3.1).

5. Conclusions

We examine the possible moral hazard effects of the unprecedented recapitalization of the U.S. banking sector during 2008-09 under the TARP program by assessing its impact on the extent of market discipline exerted by uninsured debt-holders on a sample of approximately 120 (mostly large) U.S. banks. Because uninsured debt-holders have less incentives to exert discipline if they perceive themselves to benefit from (implicit) bailout guarantees, we expect that a possible moral hazard effect of recapitalizations under TARP may manifest itself in terms of reduced risk sensitivity of the yield spreads on subordinated debt issued by banks with a high probability of receiving government support. The main estimation challenge is the co-determination of distress risk and receipt of government support, which we address by first estimating a system of equations that generate predictions of distress risk and bailout probabilities based on exogenous variables. We also address selection issues that stem from a uniform increase in the unconditional bailout probability at the announcement of the TARP program and from the effect of both distress risk and government support on banks' likelihood of having outstanding subordinated debt.

Our main results indicate the presence of market discipline by uninsured debt-holders, showing up as a consistently positive and highly significant effect of predicted distress risk on subordinated debt spreads. The magnitude of the effect is on the order of a 60 bp increase in spreads per standard deviation increase in distress risk. We also find that a one standard deviation increase in predicted bailout probability from the sample mean reduces the risk sensitivity of spreads by half, consistent with a moral hazard interpretation. This effect, however, appears to be a too-big-to-fail effect, as it is absent when the largest banks are dropped from the estimations. All estimations that are specified from an assumption that the substantial increase in implicit guarantees around TARP had a lasting effect on market discipline show weak or insignificant results, suggesting that the effect (even for the very largest banks) may have been transitory. The estimated effects of the crisis as such – a uniform rise in spreads of 2-3 percentage points over the crisis quarters *and* a substantial compounding effect on market discipline – appear to resist any alterations in specifications and choice of included banks, and remain virtually unchanged across all our estimations.

Our paper builds on and extends the existing empirical TARP literature, particularly recent studies examining the effect of recapitalizations under TARP on the risk level of subsequent loan originations in supported banks. It is also closely related to the large body of literature on market discipline, particularly studies addressing the effect of implicit guarantees on market discipline as a result of too-big-to-fail considerations. Our results and conclusions come with a number of caveats at present. First, the identification of distress risk is acceptable when all considered instruments are used, but can still not be considered "strong" (the main results, however, appear largely insensitive to exact instrument choice). Second, given the somewhat weak identification of distress risk, and the fact that predicted distress risk and predicted bailout probability contain a common component (the latter can be regarded as the "total" bailout probability, including the probability of needing a bailout), full separation of the effects of distress risk and bailout probability (particularly the interpretation of the stand-alone effect of bailout probability) remains a concern. Finally, it can be argued that the circumstances under which TARP was implemented were special (for instance, the receipt of government support under the program was voluntary for the vast majority of banks) with possible implications for the generalizability of the expectation effects it created.

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TARP and market discipline:
Evidence on the moral hazard effects of bank recapitalizations

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We examine the moral hazard effects of bank recapitalizations by assessing the impact of the U.S. TARP program on market discipline exerted by subordinated debt-holders using a sample of 123 bank holding companies over the period 2004- 2013. Predicted distress risk has a consistently positive and significant effect on sub-debt spreads, suggesting the presence of market discipline. A higher bailout probability significantly reduces the risk-sensitivity of spreads for the full sample, indicating a moral hazard effect of recapitalizations. This appears to be a too-big- to-fail effect, as it is absent when the largest banks are dropped from the sample. Results indicate that it is transitory. We also find a large effect of the crisis, appearing both as a uniform rise in, and a heightened risk sensitivity of, sub-debt spreads during the crisis.

JEL Codes: E50; G01; G21; G28; H12
Keywords: Bank bailouts; moral hazard; distres srisk; capital injections; TARP; CPP; market discipline; financial crisis

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