Bank CEO careers after bailouts: The effects of management turnover on bank risk

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Abstract

We study whether bank bailouts affect CEO turnover and its subsequent impact on bank risk. Exploiting the Troubled Asset Relief Program (TARP) of 2008, we find that TARP funds temporarily decreased the likelihood of bank CEO turnover during the crisis (2008-2010) but significantly increased CEO changes afterwards. Our results show that replacing TARP CEOs reduced individual bank risk as well as the bank's contributions to the systemic risk. Finally, we find that TARP CEO turnover was mainly driven by a decrease in the bank's political capital. Overall we provide evidence that bank bailouts have important implications for banks' risk-taking and systemic risk, insofar as bailouts affect bank CEO turnover.

Keywords: Bailouts, TARP, CEO turnover, Risk, Political capital

JEL Classification: G21, G28, G33, G34

We are very grateful to Utpal Bhattacharya, Gunther Capelle-Blancard, Ettore Croci, Denis Gromb, Clifford Holderness, Oguzhan Karakas, Ronald Masulis, José Martin-Flores, Colin Mayer, Roni Michaely, Marco Pagano, Arthur Petit-Romec, Ryan Williams and seminar participants at the 2017 FINEST Winter Workshop, Milan, at the third French Corporate Governance Workshop, Paris, at the 2018 AFFI annual meeting, at the 2019 Wolpertinger Conference, Venice, and at the 2019 EFIC Conference, Essex for helpful discussions and comments. We thank Elif Kare for excellent research assistance. The authors gratefully acknowledge financial support from LabEx ReFi.

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1 Introduction

Previous studies have underlined the reliance of the banking industry on government guarantees (see Calomiris and Haber, 2014; Allen, Carletti, Goldstein and Leonello, 2018). Understanding individual banks' risk-taking and systemic risk of the financial sector in general as well as their link to government bailouts in particular, are matters of utmost importance to scholars and regulators.[1] However, little is known about whether and how public intervention affects banks' risk appetite and contribution to systemic risk through bank CEOs' turnover.

In this paper we analyse the effect of bailouts on banks' CEO turnover and we estimate the impact of CEO replacement on banks' specific risk as well as contribution to systemic risk. To the extent that CEO characteristics play a crucial role in corporate decision-making (Bertrand and Schoar, 2003) and affect the exposure of firms to systematic risk (Schoar, Yeung and Zuo, 2020), the replacement of bank CEOs following bailouts should have an impact on the risk profile of their banks. The main contribution of this paper is to provide evidence that bank bailouts increase banks' CEO turnover, which, in turn, has important implications for individual banks' risk as well as systemic risk.

A natural testing ground for our analysis is the Troubled Asset Relief Program (TARP) for U.S. financial institutions, one of the largest interventions by the U.S. government during the financial crisis of 2008-2009. TARP injected $250 billion into the U.S. banking system. The goal was to help troubled banks temporarily stabilize their balance sheets, while maintaining lending to the corporate sector. Banks willing to participate in the program applied to their federal primary regulators for TARP funds. The Treasury would then decide whether to grant a capital injection.

We first investigate whether CEOs of bailed-out banks (TARP CEOs *hereafter*) are more or less likely to retain their jobs compared to non-TARP CEOs. We consider all 453 U.S. banks that were publicly listed as of December 2007. All of them were eligible for TARP funding, with 211 receiving it. We track the employment record of these 211 TARP CEOs as well as the 242 non-TARP CEOs during the crisis (2008–2010) as well as immediately following the crisis (2011–2013).

The first part of our analysis focuses on the crisis period (2008-2010). As some CEOs may have retired voluntarily, we thus focus on CEOs who were 60 years old or younger in 2007 (Eckbo, Thorburn and Wang, 2016). We find that during the crisis, TARP CEOs were significantly more

[1] See Acharya and Mora (2015), Acharya and Yorulmazer (2007), Berger, Roman and Sedunov (2020), Dunchin and Sosuyra (2014), Gropp, Gruendl and Guettler (2014) and Keister (2016) among the others.

likely (12%) to remain in place than non–TARP CEOs. This result is confirmed through several identification strategies, including an instrumental variable regression.

Does management turnover after bailout affect bank's risk? Previous literature has shown that TARP exacerbates moral hazard problems and risk taking (Dunchin and Sosyura, 2012). We find that this is the case only when the CEO is retained. We analyze the individual risk profile of TARP banks (vs. non–TARP banks) in terms of beta, volatility of returns and loan loss provisions before and after TARP was granted. Our results show that only when the CEO remained in (left) the position would the bank's risk profile increase (decrease), compared to non-TARP banks. Similarly, CEO turnover in TARP banks decreases banks' contribution to systemic risk compared to non-TARP banks when measured both in terms of Systemic Expected Shortfall (SES as in Acharya, Pedersen, Philippon, and Richardson, 2017) and expected capital shortfall of banks conditional on a crisis period (SRISK as in Brownlees and Engle, 2017). Overall, we find that CEO turnover after bailout has a real impact on both bank specific risk and its contribution to the systemic risk of the financial sector. CEO change might thus mitigate the moral hazard concerns of bailouts with positive implications for the stability of the financial system.[2]

The fact that not all TARP CEOs were fired during the years 2008-2010 may reflect that banks were unwilling to "punish" their CEOs while other explanations remain still possible. Banks might have been reluctant to change their "captain during the storm", or might have struggled to hire a new CEO during a crisis, given the high risk of the TARP banks and their capped remuneration. If these reasons do affect the probability of TARP CEO departure, we should observe further CEO turnover after the crisis.

We thus turn our analysis to the post-crisis period and we examine the long-term effect of TARP on bank CEOs' careers. We track the employment of CEOs up until December 2013, both within their bank of employment during the crisis and then in the external labor market. To perform this analysis, we use BoardEx, Execucomp, Bloomberg and LinkedIn.

Even when controlling for standard measures of bank performance and risk, we find that TARP CEOs were 15% *less* likely than non-TARP CEOs to remain in their jobs for the post crisis period, and 13% *less* likely to be employed as executives. An analysis of the CAR (-1;1) around the departure of TARP CEOs shows a positive and significantly higher than zero CAR, evidence of forced turnover.

Given the impact of CEO turnover on bank risk taking and systemic risk, it is important to understand the drivers of TARP CEO turnover after the crisis. At least two explanations are

[2] Fahlenbrach, Prilmeier, and Stulz (2012) show that banks' appetite for high level of risk is persistent. In the setting of bailouts, we provide evidence that this is the case as long as the banks' CEO is not replaced.

possible. First, shareholders, who might not have been aware of the excessive risk taken by the CEO, would have finally been able to "punish" them. Alternatively, shareholders might have been aware of their CEO's risky strategies (or even supported them as Cheng, Hong, and Scheinkman, 2015; Fahlenbrach and Stulz, 2011; Ferreira, Kershaw, Kirchmaier, and Schuster, 2021, suggest), but might still have fired their CEOs after the crisis to replace them with less risk-taking managers *only if* future bailouts of their banks seemed less likely.[3] Disentangling the two explanations is not easy and relies on measuring the likelihood of future government aid. We draw from the literature on political connections and bank bailouts, which shows that political connections play a key role in the granting of government support (see Duchin and Sosyura, 2012, 2014; Kostovtesky, 2015). We document that indeed political connections do affect CEO turnover.

Empirically, we analyze the effect of political connections, performance, amount and ability to repay funds, on the probability of TARP CEO turnover. We measure political connections both at the *bank level* and at the *CEO level*.

Banks are classified as politically connected if they are headquartered in the district of one of the House members who served in the Capital Market and Financial Institution Subcommittees. Committee members change every two years and we analyze the effect on CEO turnover of bank connections measured both during and after the crisis (i.e., in the years 2009/2010 and 2011/2012). Similarly, CEO-level political connections are measured via personal political contributions to parties in 2009/2010 and 2011/2012.

We expect political connections (at the CEO and bank level) —measured *during the crisis* — to be positively related to CEO turnover *after the crisis*. Political connection might have rescued the banks during the crisis; however, once they have been exploited, banks and CEOs have exhausted their "political capital" and the ability to receive future bailouts being reduced, the CEO is therefore replaced with a less risk-taking one.

Consistent with our prediction, we find that CEO and bank connections during the crisis are positively related to CEO turnover. Specifically, we find that CEO and bank connections - exploited during the crisis - increase the probability of CEO change by 35% and 23%, respectively.

Our findings suggest that TARP CEOs are replaced when banks have exhausted their political capital and therefore need to lower their risk profile by firing their CEOs. This interpretation is further supported by the fact that we did not find any effect of TARP on board turnover. Additionally, in unreported analyses, we do not find any effect of political connections on CEO turnover in non-TARP banks. Among other drivers of CEO turnover, we show evidence

[3] For example, Fee, Hadlock, and Pierce (2013) find that the managerial effect on company's policies, as shown in previous studies, is largely explained by endogenous changes in corporate policies, which require new management.

of a negative effect of bank performance (market to book ratio) after the crisis on the probability of CEO change. We do not find any relation between the ability to repay TARP funds on schedule with the likelihood of CEO departure. On the contrary, we show that the higher the number of repayment tranches are (i.e. when the payment is spread over time) the lower the likelihood of a CEO being let go is. Finally, we find a negative and significant relation between the probability of departure and the amount of funds (scaled by market capitalization) obtained. If the amount of injected funds proxy for the bank's need via government intervention, then those CEOs that received more funds should have been less likely to remain at the helm of their institution. Thus, our results do not support the hypothesis that CEOs were being "punished" for causing their institutions to require a bailout.

Our results hold if we exclude large banks that were subjected to stress tests under the Capital Assessment Plan (CAP) (Bayazitova and Shivdasani, 2011). Further, Kini, Williams and Yin (2021) show that firms may add clauses to CEOs' contracts that prohibit them from joining other firms within the same industry in the years following their departure. These clauses impact the executive labor market with enforcement depending on the State in which the firm is headquartered. We show that our results are robust if we consider the bank's ability to enforce these clauses.

Our paper makes three major contributions to finance literature. First, the paper contributes to the literature on government bailouts and the moral hazard of financial institutions during the financial crisis (see Calomiris and Khan, 2015; Berger, 2018 for a comprehensive review of this literature), and specifically on the risk-taking of banks (among others, see Cheng, Hong, and Scheinkman, 2015; Fahlenbrach, Prilmeier, and Stulz, 2012; Fahlenbrach and Stulz, 2011; Ferreira, Kershaw, Kirchmaier, and Schuster, 2021). While theoretical and empirical evidence on the effect of government intervention on financial stability is mixed (see, for example, Acharya and Yorulmazer, 2007, 2008; Berger, Roman and Sedunov, 2020; Diamond and Rajan, 2005), we document that the government intervention could exacerbate moral hazard problems with real consequent effects on bank specific and systemic risk of banks when CEOs are retained.

Second, to our knowledge, our study is the first to document the impact of government bailouts on the careers of bank CEOs. Previous studies on managers' careers find that poor performance and especially bankruptcy has a personal cost for CEOs (Cannella, Fraser, and Lee, 1995; Eckbo, Thorburn, and Wang, 2016; Gilson, 1989). In the financial sector, evidence about the effect of poor performance on lower level employees' careers are mixed. Gao, Kleiner, and Pacelli (2020) show that Wall Street bankers underwriting syndicated loans face higher turnover in cases of poor performance, while Griffin, Kruger, and Maturana (2019) show that employees

involved in residential mortgage-backed security deals characterised by high loss and misreporting rates or deals implicated in lawsuits, experienced no adverse internal or external labor market outcomes. Regarding misconduct specifically, Karpoff, Lee, and Martin (2008) and Egan, Matvos, and Seru (2019) show that it has a negative effect on CEO and employee careers. We contribute to this literature by showing that the bailout saved the TARP CEOs in the short run, but they were eventually let go once the political capital tied to the bank or the CEOs themselves was exhausted.

Finally, our paper also contributes to the literature on political connections (Amore and Bennedsen, 2013; Blau, Brough, and Thomas, 2013; Duchin and Sosyura, 2012, 2014; Faccio 2006; Fan, Wong, and Zhang, 2007; Fan, Rui, and Zhao, 2008; Kostovetsky, 2015). We show that banks' political capital affects CEO careers due to its impact on the likelihood of future bailout.

The remainder of the paper is organized as follows: Section 2 describes the sample, the variables and the main analyses; Section 3 presents additional robustness checks. Section 4 concludes.

2. Data and empirical analysis

2.1 Sample construction and main variables

Our sample consists of all listed banks as of December 2007. Following Cheng, Hong, and Scheinkman (2015) we focus on firms with two-digit SIC codes equal to 60 (commercial banks), 61 (non-depository credit institutions), and 6712 (bank holding companies). We obtain accounting data from Compustat and stock return data from the Center for Research in Security Prices (CRSP). We use Execucomp and Boardex to find the name of each bank's CEO as of December 2007. The final sample consists of 453 bank-CEO observations for which we hand-collected their employment up until 2013 using Execucomp, BoardEx, Bloomberg and LinkedIn. We call the banks that received the Treasury funds "TARP banks", while defining all the others as "non-TARP banks". Of the 453 banks in our sample, 211 (46.6%) received capital injections from the US Treasury.

In Table 1, we provide the descriptive statistics of the main variables used in the analyses. Definitions of these variables are provided in the Appendix (Table A1). All financial data are measured as of September 2008 and all financial variables are winsorized at the 1% and 99% level. The median institution in our sample had book assets of $1.2 billion with a return on assets of 0.49%. The median (mean) Tier 1 capital ratio was 9.88% (10.52%). The median (average) institution had a market-to-book value ratio of 1.14 (1.22). The equity beta of the banks is measured estimating a market model of weekly bank returns in excess of 3-month T-bills from

September 2005 to September 2008, where the market is represented by the value-weighted CRSP index. The median and average betas of our sample were 0.57 and 0.71, respectively.

Following Fahlenbrach, Prilmeier and Stulz (2012), we compute the 2006 pre-crisis return using the average daily stock return in 2006. All our banks performed relatively well in 2006, with median and mean annualized returns of 13% and 16%. The median (average) fraction of non-interest income (measured as a ratio of non-interest income over all income) was 18.5% (16.5%) and loan loss provision was 0.54% (0.96%). Accounting and market values were in line with existing literature focusing on samples of listed financial institutions over the period (e.g., Bayazitova and Shivdasani, 2011, Fahlenbrach and Stulz, 2011; Duchin and Sosyura, 2014). We measure the age of our banks as the number of years since they were listed on any major US stock exchange. The average bank age was 10 years. Boards had 10 directors on average and the ratio of independent directors was 0.78. The average age of bank CEOs was 56 with an average tenure of 5 years.

2.2 Summary statistics for bank CEOs' career paths

Figure 1 reports the employment status of the CEOs (that were in office in 2007) at the end of 2013. In our sample, 208 (45.9%) CEOs kept their positions. Among the 245 departing CEOs, 30 CEOs (12.2%) moved to another executive position in other institutions, whereas 190 CEOs (77.5%) took a new position outside the executive labor market. We found no new job positions for 25 departing CEOs.

In Figure 1, we also provide statistics on career paths of TARP versus non-TARP CEOs. Over the period 2008 – 2013, 54.5% (=115/211) of TARP CEOs left their job and among them 9.6% (=11/115) became CEOs in another firm. In comparison, 54% (=130/242) of non-TARP CEOs left their position and 8.5% of them (=11/130) moved to another firm as CEOs. Among the TARP CEOs who departed only 1.8% (=2/115) were able to gain a position as executive (non-CEO) in another firm. This percentage is equal to 4.7% (6/130) for non-TARP CEOs. The fact that only a small number of CEOs who depart are able to find a new executive position is in line with the statistics on the executive mobility over the last decades provided by Graham, Kim and Kim (2020).

2.3 CEO careers during the crisis (2008-2010)

2.3.1 CEO turnover (TARP vs. non-TARP banks) during the crisis

As a first step in our analysis, we study whether the injection of capital by the US Treasury affects the probability of the CEO being retained by a bank.

In Table 2, we analyze the effect of the receipt of TARP funding on the probability of retaining the CEO during the crisis (2008-2010). We show the results of a probit regression where our dependent variable $CEO\ Retained_i^{Crisis}$ is a dummy equal to 1 if the CEO is still at the helm of the bank in 2010 and 0 otherwise. Our main independent variable – *TARP* – is a dummy equal to 1, if the bank received TARP funds, and zero otherwise. As some CEOs retired voluntarily, following Eckbo, Thorburn, and Wang (2016), we restrict our sample to CEOs who were aged 60 or younger in 2007, to address a potential bias caused by retirement effects. This cutoff is also consistent with the study by Jenter and Kanaan (2015) who considered CEOs to be of retirement age if they are between 63 and 66 years old. A 60-year old CEO in 2007 would have turned 63 in 2010—a possible retirement age. It should be noted that the probability of receiving TARP is not significantly different for "old vs young" CEOs, i.e. we are not introducing a bias in our analysis by focusing on CEOs who were 60 years old or younger. In all our regressions we use Huber-White robust standard errors.

In the univariate regression shown in Table 2, Column 1, the coefficient of the *TARP* variable is positive and statistically significant at the 10% level, suggesting that TARP banks are more likely to retain their CEOs than non-TARP banks during the crisis.

In Column 2, we introduce in our regression several measures of bank market and accounting performance (bank size, market-to-book ratio, beta, ROA, and return in 2006), governance (number of directors as well as the ratio of independent directors), bank age, and CEO characteristics (age and tenure). The coefficient on TARP remains positive and statistically significant at the 10% level.

In Column 3, we further control for the Tier 1 ratio, the amount of loan loss provisions over loans, the wholesale debt ratio, and the fraction of non-interest income. The choice of our control variables is driven by previous studies: Fahlenbrach, Prilmeier, and Stulz (2012) show that banks' 2006 returns, beta, market-to-book ratio, and Tier 1 capital ratio and fraction of non-interest income are significantly related to the poor performance of financial institutions during the crisis. Following Bayazitova and Shivdasani (2011), we also include the wholesale debt ratio, which is the ratio of non-deposit liabilities to assets and represents the proportion of non-deposit borrowing by a bank. Banks that rely on non-deposit funding and activities that generate non-interest income are riskier (Demirgüç-Kunt and Huizinga, 2010). We also control for the bank size (log of assets)

and bank age (following Croci, Hertig, and Nowak, 2016). We add the CEOs' age and tenure, as CEOs that have been at their bank longer may be more entrenched in their positions. Finally, we also control for standard governance variables such as the size of the board and the ratio of independent directors.

In the specification shown in column 3, we find that CEOs of banks that received TARP funds were 12.1% more likely to remain in their positions during the crisis period than non-TARP banks (marginal effects of Table 2, column 3). This effect is statistically significant at the 5% level.

Among the control variables, the ratio of loan loss provisions to loans is negatively and significantly related to the probability of the CEO remaining, suggesting that everything else being equal, CEOs who implemented riskier strategies were more likely to lose their jobs. CEO age is negatively related to the probability of remaining in the post.

In Columns 4-6 of Table 2, we perform the same regression as in Columns 1-3 but we include the entire sample of CEOs, therefore not controlling for retirement effects. Column 4 shows results for the univariate regression, while in column 5 and 6 we gradually include all the control variables described above. The coefficient of interest is positive but no longer significant, suggesting that TARP CEOs left primarily for retirement reasons over the period analyzed.

Overall, our results in Table 2 indicate that receiving TARP funds affected the probability of a CEO being retained once we take into account retirement effects.

2.3.2 Instrumental variable regression

Our analysis poses an obvious identification challenge because banks' access to TARP funding was the outcome of a two-stage selection process. In the first stage, a given bank itself decided whether to apply for government support. Some banks might not have applied for funds because they did not need them, or might have been concerned about possible damage to their reputation. On the other hand, some banks might have applied for funds in order to obtain cheaper liquidity while others did so because they were in need of a bailout. At the second stage, the government decided whether to approve TARP funding to banks. Overall, the application for TARP funds and their approval might have been correlated with variables that also affected the probability of retaining the CEO. For example, TARP capital might have been provided to "relatively" more viable banks, which might also have been less likely to replace their CEOs, making the allocation of funds endogenous. To mitigate all these concerns, in this section we perform an instrumental variable regression, whereas in Table OA1 of the Online Appendix we report results using a propensity score matching methodology.

As instrument, we employ the percentage of similar banks (in terms of banks' size and political connections) in the same State that applied for TARP. The intuition is that banks compete for TARP funds with other institutions that have a similar size as well as political connections. (Previous studies show that politically connected banks were more likely to receive funds, see Blau, Brough, and Thomas, 2013; Croci, Herting, and Nowak, 2016; Duchin and Sosyura, 2012, 2014).

The higher the percentage of local banks with similar likelihood of receiving funds, the lower the probability of the bank receiving the injection of funds.

We develop the instrument in a three-step process. First, we identify the number of applicants per State. Since the list of applicants has not been made available publicly, we examine the banks' quarterly filings, annual reports and press releases, and we classify as *non-applicants* all banks that publicly disclosed that they did not intend to apply for TARP. All other banks were considered as applicants (a detailed explanation of how we have collcted bank's application status is provided in Section 3.1). Second, to identify connected banks we follow Duchin and Sosyura (2014), we build a "politically connected" variable (*Connected committee)* based on connections to a member of the House Financial Services Committee. A financial institution is considered to be connected to a member of Congress who sits on one of the Committees if it is headquartered in his or her district. The Committee was involved in the development of the Emergency Economic Stabilization Act of 2008 (and its amendment in 2009), in monitoring TARP programs and approving TARP amendments. This type of connection is not under the control of banks and CEOs, which means that it is unlikely to be correlated to the financial health of the banks in the State. Following Duchin and Sosyura (2014), we focus on two subcommittees: the Subcommittee on Financial Institutions (which supervises all the main banking regulators) and the Subcommittee on Capital Markets (which examines regulations on capital markets and investment banks). The *Connected committee* variable is equal to 1 if a bank was connected to at least one key subcommittee either in 2008 or in 2009, and 0 otherwise.

Large banks are those whose size (measured in terms of total assets) was above the median of the sample of banks in the State. Finally, for each State we define two "groups" of banks: those with a "*high probability of obtaining TARP funds (HPT)*", which were large and/or connected in 2008, and banks with a "*low probability of obtaining TARP funds (LPT)*", corresponding to all other banks in the State. For each bank, we calculate the number of banks in the same "group" that applied for TARP, divided by the total number of banks in the same "group" (excluding the bank observation in the computation), and call this variable the *TARP competition ratio.*

There are at least two reasons why our instrument should satisfy the requirement of relevance. First, since one of the TARP program's objectives was to increase the availability of

credit and improve real economic conditions, a certain number of banks in each State should have received Treasury funds to support the local economic conditions (Berger 2018).[4] This should have generated competition for TARP funds at the State level; a bank has a lower chance of receiving funds if many other banks apply for TARP funds. Second, if the probability of receiving TARP funds was influenced by the probability of other local competitors receiving funds, then the higher the number of similar applicants in terms of size and political connections in the same area, the lower the probability of receiving those funds. As shown in the literature, large banks were more likely to be bailed out in order to prevent such failure from endangering the entire financial system (Kostovetsky, 2015). Additionally, politically connected firms were more likely to receive TARP infusions (Duchin and Sosyura, 2012). Intuitively, given an amount of funds potentially allocated to a State, large and connected banks compete with each other to get government assistance. The remaining funds could then be distributed to small and unconnected banks that also compete with each other for such funds.

Regarding the exclusion restriction, we expect the *TARP competition ratio* not to be related to the probability of CEOs retaining their job, other than through TARP itself. We include the State-fixed effect to control for local economic conditions that might influence the number of TARP applicants in a specific area and, at the same time, the banks' CEO turnover. Importantly, although the instrument is constructed at the State level, the fraction of competitors for TARP funds included in the TARP competition index depends on the percentage of applicants within the specific groups of banks located in the State (i.e., large and connected banks versus other banks). This implies that on the one hand the instrument varies within the State (thus allowing the introduction of State fixed effects), and on the other that the instrument is not a simple proxy of competition. Nonetheless, we also show that results do not change if we use two-digit Zip code fixed effect instead of State fixed effect. We use a two-stage least squares regression approach to determine the effect of TARP on the probability of CEO retention through the TARP competition ratio.

To obtain the predicted probability of receiving funds, we regress our TARP variable on the *TARP competition ratio,* and control for all variables included in Table 2, Column 3: CEOs, banks, and governance characteristics. We also control for a dummy that indicates whether the bank was classified as large or connected as well as for a proxy for CEO personal connections over the crisis period. Specifically, we use the personal contributions of CEOs to political parties in 2007/2008 and 2009/2010 (details on how we collect the data on CEO contribution are provided in Section

[4] One could claim that firms do not necessarily need to borrow from local banks, but this is often the case when lenders need to collect soft information, e.g., for small business (Agarwal and Hauswald, 2010).

2.5.2). The results of the linear probability model comprising our first stage regression results are reported in Table 3, Column 1. We obtain a negative and statistically significant coefficient of -0.638 on the *TARP competition ratio*.

In their survey of the weak instrument literature, Stock, Wright, and Yogo (2002) develop benchmarks for the required magnitude of the F statistic. Following their work, econometric techniques to detect and address weak instruments have been further developed. As recommended by Andrews and Stock (2018), we report the Olea and Pflueger (2013) effective first stage F statistic and compare it to 10. The effective F statistic is above 10 in our first stage regression (14.87), indicating that our measure of competition for TARP funds is not a weak instrument for the receipt of these funds. Since the effective F statistic is above the threshold of 10, we can use the two-stage least square inference (Andrews and Stock, 2018).

Column 2 of Table 3 reports the second stage regression of CEO retention on the predicted TARP variable. The coefficient of the TARP variable is positive (0.816) and strongly statistically significant (t=2.94), suggesting that capital injection has a strong effect on the probability of retaining the CEO.

Restricting this analysis to CEOs who were aged 60 or younger in the first stage in order to address retirement effects would lead to an incorrect estimation of the predicted probability of receiving TARP. Indeed, imposing this restriction in the first stage would bias the *TARP competition ratio* measure as banks with CEOs over 60 years old were as likely as banks with CEOs younger than 60 years old to receive TARP funds, as previously stated. To overcome this problem, we estimate our first stage regression on the whole sample (Column 1) and then focus in the second stage on the restricted sample of CEOs who were aged 60 or younger in 2007. We still include all the control variables used in previous analysis and the State fixed effect. The results of this second stage are reported in Column 3 of Table 3. We still find a positive and significant effect of our instrumented *TARP* variable on the probability of the CEO being retained (t=2.14).

We then modify our model by replacing State fixed effects with the two-digit Zip code fixed effects. Results hold as shown in column 4 and 5.

We also implement two additional tests to prove the robustness of the instrument. First, we use the unbiased estimator of Andrews and Armstrong (2017). This estimator requires that the first stage sign is known ex-ante, which is the case in our setting. As this estimation requires that the sign of the first stage is positive, we rearrange our instrumenmt by multypling the TARP competition ration by (-1). Results under this estimator are unchanged in all models both in terms of statistical significance and in terms of magnitude of the coefficients. For brevity, we only report (in Column 6) results using this estimator in the most saturated model (i.e. the model that includes

Zip-code fixed effects). Second, we use the first-stage F statistic and we calculate the "tF 0.05 standard error" proposed by Lee, McCrary, Moreira, and Porter (2021). Even under this conservative correction, the coefficients remain statistically significant at the 5% level.

Overall, our results on the effect of TARP funds on CEOs' careers during the crisis show that CEOs of TARP banks were rescued by the TARP allocation of funds, at least during the crisis period.

The fact that TARP banks experienced a lower CEO turnover does not necessarily imply that banks were unwilling to punish their CEOs. Banks might have also been reluctant to lose their "captain during the storm", as they might have struggled to hire new CEOs during the of crisis. On the other hand, we note that only six CEO replacements were announced in 2007, the year immediately before the crisis. Therefore, there is no evidence that some CEOs abandoned the "ship" before the crisis began.

Lastly, it is also possible that CEOs were reluctant to accept employment at very risky banks (such as banks in need of bailout) in the midst of the crisis, meaning that TARP banks might have been "stuck" with their old CEOs while non-TARP banks may have found it easier to attract new executives. We investigate this point in greater detail in Section 2.5.

2.4 Effect of TARP CEO retention on bank risk

Does management turnover affect bank risk? Previous literature has shown that TARP exacerbates moral hazard problems and risk taking (Dunchin and Sosyura, 2012). In this section, we investigate the effect of TARP CEO retention on the risk profile of their banks and their contributions to systemic risk. In Table 4 we consider the period 2005-2011 and we run a set of bank fixed-effects regressions. The dependent variables are bank risk measures computed at a yearly frequency, and the main independent variable is *After CPP*TARP,* which is the interaction between *After CPP* (indicator equal to 1 in 2009–2011 and 0 in 2005–2008) and *TARP* (dummy equal to 1 if the bank has been granted TARP funds and 0 otherwise) variables. The *After CPP* and *TARP* variables drop out of the regression due to the inclusion of year and bank fixed effects. Importantly, we divide the sample of banks between those that retained their CEOs and those that replaced them. We then implement our regression models in the two subsamples to test whether the effect of TARP on bank risk depends on CEO retention. In all models we include bank-time control variables (size, market-to-book ratio, ROA, and the ratio of deposits to total liabilities). Standard errors are clustered at the bank level and robust to cross-sectional heteroskedasticity and within-panel serial correlation.

2.4.1 Individual bank risk

Duchin and Sosyura (2014) find that TARP banks increased their Tier I and II compared to non-TARP banks following capital injections. However, the authors find that TARP banks increased their risk compared to non–TARP banks by shifting their lending to less secure borrowers within the same asset class.

Before the detailed analysis of individual bank risk, we first check the effects of TARP funds on Tier 1 and Tier 2 capital ratios following Duchin and Sosyura (2014) and we find similar results (reported in the Online Appendix, Table OA2). The *After CPP*TARP* dummy is significant and positively correlated with the Tier 1 and Tier 2 ratios. When we split the sample into firms that retained their CEO and firms that let them go, we find that the effect of the *After CPP*TARP* interaction is positive and significant for Tier 1 both when the CEO is retained and when the CEO is let go (although the coefficient is higher in this latter case), while for Tier 2, the *After CPP*TARP* interaction is only significant when the CEO is retained.

Next, we investigate whether the CEO retention in TARP banks affects individual bank risk. Our first proxy for bank risk is the market beta. Results are reported in Column 1 and 2 of Table 4 Panel A. We find that the coefficient of our main independent variable is positive (negative) and statistically significant for banks that retained (replaced) their CEO. This suggests a decrease in individual bank risk after TARP only when banks replaced their CEOs. In Columns 3 and 4 we use as outcome variable *Beta (FF3)*, which is the market beta estimated using the Fama–French three-factor model instead of a single-factor model. Results confirm a drop in the market beta only for banks that change the CEO. The coefficient of *After CPP*TARP* is negative and statistically significant at the 5% level for the sample of banks that replace CEOs, while it appears statistically insignificant for banks that kept their CEOs. One concern could be that this result is polluted by the size effect documented in Gandhi and Lustig (2015) and Gandhi, Lustig, and Plazzi (2020), which suggest that the largest banks have lower risk-adjusted returns—especially in bad times—due to implicit government guarantees. To rule out the concern that our findings on market beta are driven by bank size, we run our models excluding banks in the largest size decile (results nor reported) and our findings do not change.

Lastly, we use as outcome variables the ratio of loan loss provisions to loans, and the volatility of stock returns. Results are shown in Columns 5-8. As before, our main independent variable is *After CPP*TARP*. Our regressions show a significant increase in these measures of risk only for TARP banks that retained their CEO. Overall, these results suggest that when risk-taking TARP CEOs were not dismissed, they continued to increase their risk-taking behavior, thus exacerbating the moral hazard problems. Given the existence of strong linkages between government

guarantees, sovereing risk and bank credit risk (Acharya, Drechsler, and Schnabl, 2014; Correa, Lee, Sapriza, and Suarez, 2014), a change in loan loss provisions after a bailout may be due to a perceived change in the government's ability to support the financial sector. Indeed, changes in managers' expectations to receive government support may lead banks to increase the level of provisions (Silva, 2021). Although this mechanism may be at play, it should lead all banks to set a higher level of provisions. Our results that the increase in provisions depends on receiving TARP and keeping the same CEO is rather suggestive that those banks have a higher endogenous risk taking.

Overall, our results suggest that the management turnover affects the individual bank risk when receiving the government assistance.

2.4.2 Systemic risk

A related important question is whether TARP CEO retention causes some banks to have a more destabilizing effect on the financial sector by contributing more to global systemic risk. To investigate this important issue, we build two measures of banks' contributions to systemic risk that we use as outcome variables in our regression.

The first variable we use is the Systemic Expected Shortfall (SES) developed by Acharya et al. (2017). SES captures the bank's expected losses during a crisis and its estimate for bank *i* at year *t* is given by

$$SES_{i,t}=0.02-0.15MES_{i,t-1}-0.04LVG_{i,t-1}$$

where *LVG* is the bank's leverage and *MES* is the banks' equity loss conditional on a market downturn. MES is computed using the average banks' stock return of the days of the year in which the market index experienced the worst returns.[5] As in Berger et al. (2020), we multiply SES by (-1) so that any increase in this measure corresponds to an increase in systemic risk.

In Table 4 Panel B, we report the results of the regression of SES_{it} on our key independent variable, i.e. *After CPP*TARP*. Column 1 shows the results for the sample of banks that retain their CEOs. The variable *After CPP*TARP* enters positively and statistically significant in the regression, suggesting that for this sample of banks, capital injection leads to an increase in their contribution to systemic risk. However, we observe the opposite effect when banks replace their CEOs (Column 2). These banks experience a reduction in their contribution to systemic risk after

[5] Specifically, LVG is defined as book value of liabilities plus market equity scales by market equity. For the computation of MES, we consider the 5% of the worst days of the S&P 500 index return over the year, and over these days, we calculate the average bank's daily stock return.

the receipt of TARP funds. While this finding suggests that capital injections may have positive effects on the stability of the financial sector (see, for example, Berger et al. 2020) , our results show that the replacement of TARP CEOs is likely a key driver of this effect.

The second measure of systemic risk we use is the SRISK developed by Brownlees and Engle (2017) that captures the expected capital shortfall of a bank in market decline. SRISK is influenced by bank's size, leverage and exposure to market risk. Precisely the SRISK estimate for bank *i* at time *t* is derived as follows:

$$SRISK_{i,t}=k(debt_{i,t})-(1-k)\left(1-LRMES_{i,t}\right)equity_{i,t}$$

where $debt_{it}$ is the value of debt of bank i at time t, $LRMES_{it}$ is the long run Marginal Expected Shortfall defined as 1-exp(log(18*MES), $equity_{it}$ is the banks' market value of equity and K is set equal to 8%. As in Berger et al. (2020), we normalize SRISK by the bank's market capitalization and we label our standardized variable as NSRISK.

The results of NSRISK are reported in Columns 3 and 4 of Panel B. The coefficient on *After CPP*TARP* is positive and statistically significant for the sample of banks that retained their CEOs. Thus, results considering both our measures, SES and NSRISK, show that the effect of TARP on systemic risk is strongly affected by CEO turnover. The contribution to systemic risk by bailed-out banks arises when they do not replace their CEOs.

2.5 CEO careers after the crisis (2011–2013)

2.5.1 CEO turnover and career paths after the crisis

This section examines whether Treasury intervention still influenced CEO careers after the crisis. For our sample of CEOs in position at the end of 2007, we track career changes up to 2013.

In this part of analysis, we focus on CEOs who, at the end of 2010, held the same position as before the Treasury allocation of funds, i.e., CEOs who had retained their job during the crisis. We implement a probit model where the dependent variable $CEO\ Retained_i^{After\ Crisis}$ is equal to 1 if the CEO remained in position at the end of 2013, and 0 otherwise. *TARP* is a dummy variable which takes the value of 1 if the bank received TARP funds, and 0 otherwise. We apply the same control variables for bank, governance and CEO characteristics used in the previous sections. However, as we are now focusing on CEO turnover three years after the allocation of funds, we want to control for potential performance, risk-taking behaviors or governance characteristics that might have changed after the implementation of the TARP program. All controls are therefore now measured at the end of 2010. Results hold if we use controls measured in 2008.

Our results are reported in Table 5. Columns 1 and 2 show the results for the sample of CEOs aged 60 years old or younger in 2010 and 2007 respectively. The TARP coefficient is negative and statistically significant. The marginal effect of Column 2 shows that the CEOs of banks that received funds were 15% less likely to retain their position after the crisis than non-TARP banks. This result suggests that the government bailout had two opposite effects on CEO turnover; one during and one after the crisis. Regression results remain statistically significant with the set of control variables. Among these control variables, only return on assets and market-to-book ratio are statistically significant. Bank profitability and market-to-book ratio positively affect CEO retention.

When comparing turnover of TARP and non-TARP CEOs one potential concern is the violation of the Stable-Unit-Treatment-Value-Assumption (SUTVA). In our setting, the SUTVA is violated if the treatment of one bank affects the CEOs turnover of another bank. For example, if banks compete with each other for executives and the bank CEOs moves happen mainly within the financial sector, it could be possible that CEOs of banks that receive capital injections are replaced by non-TARP CEOs (or vice versa). This implies that the turnover of TARP CEOs has spillover effects on the turnover of non-TARP CEOs, leading to a potential violation of the SUTVA. We believe this is unlikely to be the case in our analysis. Indeed, as described in Section 2.2, the probability that a departing CEO becomes later CEO in another firm is very small. Additionally, in our sample only 5 out of 115 (4.3%) departing TARP CEOs joined a non-TARP bank as CEOs, and only 4 out of 130 (3.1%) departing non-TARP CEOs became CEOs of a TARP bank. This suggests that spillovers between CEOs turnover of TARP and non-TARP banks are unlikely to affect our results

Next, we enlarge our analysis by focusing on new positions that CEOs may find in the labour market. The loss of the job position experienced by the CEOs may actually be offset by another executive role. We extend the analysis by studying whether the CEOs who left their bank held an executive position at their bank or another firm. We run a probit regression where the dependent variable $CEO\ Executive\ position_i^{After\ Crisis}$ takes the value of 1 if the CEO held an executive position either at the same bank or at another firm and zero otherwise. The results are reported in columns 3–4 of Table 5. In columns 3 we restrict the sample to CEOs aged 60 or younger in 2010 The variable *TARP* enters negatively and significantly at the 5%. Specifically, TARP CEOs were 13% less likely than non-TARP CEOs to hold an executive position between 2011 and 2013 (marginal effect of Column 3). The lost position was not replaced by new similar positions in the former bank or in another firm. Among the controls, the coefficient of CEO age is negative and statistically significant. Since age affects the probability of leaving the executive market, in Column

4 we further restrict our sample to CEOs who were 60 or younger in 2007. Our main result holds as the *TARP* variable is still negative and statistically significant at almost 5% (t=-1.90)

Baer, Ertimur, and Zhang (2021) show that firms with greater advisory needs are more likely to appoint executives involved in governance failures. Moreover, Graham et al. (2020) in their analysis of executive labour market from 1920 to 2011 report low between-industry mobility from financial services to other industries. Therefore, after they leave their former banks, TARP CEOs might have high likelihood of obtaining a position in another financial institution.

In our sample out of the 115 TARP CEOs that left their bank 52 obtained a position in another bank: 7 (6.1%) became CEOs, 2 (1.7%) executive and 43 (37.9%) directors. Similarly, out of 130 CEOs that left non-TARP banks 51 covered positions in another bank: 4 (3.1%) became CEOs, 5 (3.8%) executive and 42 (32.3%) directors. Indeed, a non-negligible fraction of TARP CEOs remained in the financial sector, especially as board members.[6] Those findings might have important implications for systemic risk, as Gopaland, Gormley and Kalda (2021) show that board members that experience a (mild) bankruptcy in the firms they serve as directors, increase risk in the other firms in which they also serve as board members. In our setting when former TARP CEOs join other banks as new directors they might increase those banks' contribution to systemic risk. In our sample it is difficult to measure the impact on systemic risk of the newly appointed board members that were former CEOs of TARP banks, as 88% of banks they join are unlisted and most of the measures of systemic risk are based on stock performance. However, it is reasonable to believe that this lack of across-industry mobility of bank executives may increasethe moral hazard problem.

Finally, in our analysis we measure the likelihood of TARP CEOs of remaining in the financial sector compared to non-TARP CEOs. Results are shown in Table 5 Column 5. Specifically, we build a new outcome variable, *CEO in other* $FI_i^{After\ Crisis}$, which takes value 1 if the CEO departs to another bank in the role of CEO, executive (non-CEO) or non-executive director, and 0 otherwise. We restrict our sample to CEOs that left their former bank after 2010 and aged 60 years old or younger as of 2007.[7] We regress our outcome variable, *CEO in other* $FI_i^{After\ Crisis}$, on the variable *TARP* and our set of control variables. The coefficient of the variable *TARP* is not statistically significant.

[6] If we focus on the sample of TARP and non-TARP CEOs that became non-executive directors (106 CEOs, as shown in Figure 1) we see that 84.3% (43 out of 51) of TARP CEOs gain at least a directorship in another bank after their departure. This percentage drops to 76.4% (42 out of 55) for non-TARP CEOs.

[7] In untabulated results we find that our results do not change if we restrict our sample to CEOs that left their former institution after 2008 (instead of 2010), and who were 60 or younger in 2010 (instead of 2007).

Overall, we find that TARP CEOs are more likely than non-TARP CEOs to be replaced after the crisis, but, once they leave their institutions, they have similar probability to remain employed with key positions in the financial sector.

2.5.2 Reasons for leaving

In this section we investigate what triggered the departure of the TARP CEOs. Arguably, there are two potential reasons for CEO turnover. First, the punishment for strategies that were too risky pre-crisis. Second, a change in shareholder incentive to take risks, due to the decrease in "political capital" and the consequent lower probability of being bailed out (Dam and Koetter, 2012). In the first case (punishment) we would expect CEO replacement and change in the risk strategies of the bank, while in the second case (likelihood of future bailouts) we would expect turnover of CEOs and change in risk strategies *only* if future bailouts seemed less likely.

Disentangling the two explanations is not easy and relies on measuring the likelihood of future government aid. We draw from the literature on political connections and bank bailouts, which shows that political connections play a key role in the granting of government support (see Duchin and Sosyura, 2012, 2014; Kostovtesky, 2015). We document that indeed political capital affects CEO turnover.

Empirically, we analyze the effect of political connections, performance, amount and ability to repay funds, on the probability of TARP CEO turnover. We measure political connections both at the *bank level* and at the *CEO level*, during and after the crisis.

In Table 6, we study whether the political connections and the consequent changes in the likelihood of bailout plays a role in the turnover of TARP CEOs. When analyzing the effect of political connections, we also control for two additional explanations for CEO turnover, poor bank performance after the crisis, and inability to repay government funds. We focus on the sample of TARP banks, and we run a probit regression where the dependent variable is a dummy equal to 1 if the CEO was retained after the crisis (2011-2013) and 0 otherwise. Columns 1 and 2 show the results only for CEOs who were under the age of 60 at the time of the injection. Columns 3 and 4 show the results for the entire sample.

To test the effect of the banks' political connections on CEO retention, we measure political connections in two different ways: at the *bank level,* and at the *CEO level*, as those measures have different implications. Banks are classified as politically connected if they are headquartered in the district of one of the House members who served in the Capital Market and Financial Institution Subcommittees. Committee members change every two years and we analyze the effect on CEO turnover of bank connection measured both in the years 2009/2010, *Bank connection (crisis)*, and 2011/2012, *Bank connection (after crisis).*

CEO-level political connections are measured via personal political contributions to political parties, and, similarly to the bank connection, we measure the CEO-level contribution in 2009/2010, *CEO connection (crisis),* and 2011/2012, *CEO connection (after crisis)*. We collect data of all campaign contributions made by each CEO to candidates, parties, or PACs. The Federal Election Commission (FEC) collects and publishes campaign contributions greater than $200. Following Fremeth, Richter, and Schaufele (2013), we collect these transactions from the Center for Responsive Politics (www.opensecrets.org).

We expect *past* connection, *CEO connection (crisis)*, at the CEO to be positively related to CEO turnover. At the personal level, connection during the crisis might have helped CEOs by rescuing their banks, however once CEOs have exploited their connections, or in other words have exhausted their political capital, they are less valuable for the bank, and can be fired. For the same reason, CEO connection after the crisis should have negative (if the CEO has changed in the previous period) or no effect on CEO turnover.

Similarly, at the bank level *past* connection, *Bank connection (crisis)*, should be positively related to CEO turnover (having already been exploited during TARP), while we expect *after crisis* political connections (bank connection varies across years as committee members change in the different periods) to be negatively related to CEO turnover.

Consistent with our prediction, we find that past CEO connections are positively related to CEO turnover, while current CEO connections are negatively but insignificantly related to CEO changes. Similarly, past (current) bank connections related positively (negatively) to CEO turnover. Specifically, we find that CEO and bank connections exploited during the crisis (captured by the variables *CEO connection (crisis)* and *Bank connection (crisis)*) reduce the probability of CEO retention by 35% and 23%, respectively (marginal effects of Table 6, Column 2).

To test for the ability to repay on time, we collect information on TARP repayment (amount and date of repayment) from the TARP transaction reports. We introduce a dummy variable into our regression. *Repayment after 2010* equals to one if the bank repaid the funds in full after 2010, and zero otherwise. If the CEO departure was influenced by difficulties in the repayment of funds, there should have been a higher probability of CEO replacement for those banks. This is not what we find. As shown in Table 6, column 1, the *Repayment after 2010* variable is not statistically significant. We also add a variable which captures the number of repayment tranches. Banks could repay the funds in one tranche or rather spread out the repayment over a period of years. However, the repayment of funds over time might have been an entrenchment strategy implemented by CEOs seeking to hold on to their position (Tirole, 2010), with CEOs starting and then delaying full repayment in order to become indispensable for the completion of

the reimbursement strategy. In line with this prediction, we find that the greater the number of tranches, the higher the probability of a CEO staying in office up to 2013.

Next, we add the amount of TARP funds over the banks' market capitalization value. The higher the ratio, the greater the bank's need for TARP funds at the time of injection. Therefore, if the banks punished the CEO for making their institution require a bailout, this ratio should have been negatively related to the probability of retaining them. Instead, we find that the ratio enters positively in the model. This interesting result indicates that CEOs in banks that received more funds were more likely to stay at the helm of their institution, which is strongly at odds with the hypothesis of CEO punishments being applied. Conversely, this finding suggests that CEOs were rewarded after a large capital injection.

Finally, to test for poor performance in our regression we introduce the market-to-book ratio for market performance and ROA for accounting performance and, to analyze the effect of bank risk, we introduce the Tier 1 capital ratio, the beta, and the ratio of loan loss provisions over loans. We also control for the size of the bank, the wholesale ratio, the fraction of non-interest income and bank age. We also introduce governance variables (board size and board independence) and CEO characteristics (age and tenure) into the regression. All variables are measured at the end of 2010. Results show that the risk and performance variables play a marginal role in explaining CEO turnover. The coefficients of the other control variables are statistically insignificant.

In an unreported analysis, we replicate our probit model focusing on CEO retention in the sample of non-TARP banks. None of the political connection variables is statistically significant. This confirms that political connections are more likely to affect the risk-taking behavior of bailed-out banks and thus, in turn, the replacement of CEOs.

Overall, our results suggest that political connections, entrenchment strategies, the amount of funds obtained are important determinants of CEO turnover in banks that received the government assistance.

2.5.3 CARs and CEO turnover after the crisis

In the previous section, we investigate the reasons for CEO departures after the crisis. In the same vein, in this section, we attempt to understand whether the low retention rate of TARP CEOs was indeed driven by forced departures. To do so, we conduct event study by analyzing the CAR(-1;1) around key announcements dates; the CEO's departure and the first repayment date of TARP funds.

We measure abnormal returns using the CAPM with an estimation period of 180 trading days, starting 30 days before the event. We measure CAR(-1;1) in a 3-day window around the announcement date. We first focus on the announcement of the CEO's departure. We collect the dates on which the departures were formally announced via Boardex and press releases. For the entire sample of CEO departures (TARP and non-TARP CEOs), we were able to collect the departure dates of 58 CEOs (31 TARP CEOs and 27 non-TARP CEOs). Results are reported in the Online Appendix, Table OA3. We find that the average CAR (-1;1) around CEO departure is 5% and significantly higher than zero at the 10% level only for the sample of TARP CEOs, while the average CAR(-1;1) around the departures of non-TARP CEO is equal to 1% and is not statistically different from zero. Next, we analyze the market reactions around the first date of repayment of TARP funds. Specifically, we focus on the first repayments dates made by the 57 CEOs who left their banks between 2011 and 2013, and by the 57 CEOs who remained in their posts. The average CAR(-1;1) around the dates of first repayments made by CEOs that remained in their positions is around 1%, significantly higher than 0 at the 10% level. By contrast, the average CAR(-1; 1) around the dates of first repayments made by CEOs who later left their banks is 0.3% and not significantly different from zero. These findings suggest a relation between a positive market reaction around the repayment dates and the probability of CEOs retaining their position. Overall, the market reaction around key announcement dates suggests that the departure of TARP CEOs after the crisis was unlikely to be voluntary.

3 Robustness checks

3.1 CEO turnover based on application status

In this section, we investigate CEO turnover during the crisis (2008-2010) based on the application status of banks. The advantage of these tests is that they take into account the fact that some banks did not apply for funds, most likely because they were not troubled, and the fact that some banks' applications for funds were rejected by the Treasury. In this manner, this analysis could mitigate the problems with the first-stage selection process described before. Precisely based on our first finding that bailout relieves CEOs of the responsibility for poorly performance during the crisis, we expect to observe that the probability of CEO turnover is similar when comparing TARP banks with banks that did not apply for the Treasury injection. By contrast, when comparing TARP banks with other banks that applied but did not receive funds we should observe a higher probability of CEO retention for banks that succeeded in receiving the bailout rather than for banks that did not receive the funds. In both cases, institutions needed the funds and while the

CEOs of the former group (TARP banks) were rescued together with their banks, the CEOs of the latter group (non-TARP banks that applied) could suffer the consequences of the Treasury's rejection together with their banks.

The list of banks that received TARP funds is available on the Treasury Department website (http://www.treasury.gov/initiatives/financial-stability/Pages/default.aspx), although the list of applicants was not made public. Therefore, we hand-collected the application status (whether or not they applied, whether they applied and were approved/rejected) from quarterly filings, annual reports, and press releases. However, banks did not have to disclose their application status, nor whether they applied for TARP funds; any information made available by banks was provided on a purely voluntary basis.

From the information collected and following Bayazitova and Shivdasani (2011), we divide the sample of banks that did not receive funds into two sub-groups, according to their application status and the Treasury decision. The first group of non-TARP banks (141 banks) includes banks that did not apply and banks that applied and were approved but then decided to decline the TARP funds. Most of those which did not apply or did not accept funds declared that the institution did not need government assistance. Following Bayazitova and Shivdasani (2011), we called this group *non-participating* non-TARP banks. These were banks with higher quality assets and who considered the capital injection too costly.

The second group concerns the banks that were rejected by the Treasury (rejected banks) or banks that did not officially report their decision not to apply (undisclosed status). There are 101 banks in this group. We refer to these banks as *participating* non-TARP banks. Reporting the decision not to apply for TARP was a way of signaling that the bank was well capitalized and did not need government funds. Consequently, following Bayazitova and Shivdasani (2011) we firstly consider that if banks did not report their application status, this meant they were willing to participate.

Non-participating non-TARP banks were generally more highly capitalized than their participating counterparts. Tier 1 averages 9.6% for *participating* banks; 12% for *non-participating* banks; and 9.8% for TARP banks. While Tier 1 is statistically lower for TARP banks than for nonparticipating banks, we do not observe any differences in term of capital ratio between TARP and participating non-TARP banks.

We analyze the probability of the CEO being retained in different application subsamples. The dependent variable is the usual $CEO\ Retained_i^{Crisis}$ equal to one if the CEO still occupied his/her position in 2010, and zero otherwise. The main independent variable is the *TARP* dummy equal to one if the bank was granted the funds, and zero otherwise. Results are reported in the

Online Appendix, Table OA4. In Columns 1-2, we exclusively consider TARP banks and non-TARP participating banks, i.e. banks that either applied (and were rejected) or whose status is unknown. In Column 1 we restrict the sample to CEOs who were aged 60 or younger in 2007, whereas in column 2 we show the results for the entire sample. We find that TARP banks were more likely to retain their CEOs than participating non-TARP banks. The *TARP* dummy is positive and statistically significant, and the economic effect is substantial; receiving funds decreases the likelihood of CEO turnover by 20%. It could be argued that the results are affected by the sample of banks for which the application status is unknown. To address this concern, we first check whether the group of rejected banks differed from banks with undisclosed status. Banks are similarly capitalized in these two sub-samples, with a Tier 1 capital ratio of 9.3% for rejected banks and 9.8% for banks with undisclosed status. Similarly, we do not find statistically significant differences between the two groups in terms of return on assets, market-to-book ratio, and market beta. In addition, we replicate the analysis by exclusively comparing TARP banks and rejected banks, thus discarding the sample of banks with undisclosed status. The results are shown in columns 3 and 4. The results hold using this sample: receiving funds decreases the likelihood of CEO turnover by 27%

Next, we compare TARP banks with *non-participating* non-TARP banks (banks that did not apply). In line with prior research, we find that the non-participating non-TARP banks are more viable with sound financial indicators. The decision not to participate or to decline the TARP funds provides important information not only about the financial health of the banks but also about the quality of their CEO. The results (Columns 5 and 6) are interesting, showing that there is no difference in the probability of being retained between TARP CEOs and *non-participating* non-TARP CEOs. These results imply that TARP CEOs were as likely to keep their jobs as CEOs in healthy institutions. The results are similar whether we consider the sample of CEOs aged 60 or younger or the entire sample. In light of the fact that *non-participating* non-TARP banks were healthier than TARP banks, the similar probability of CEO turnover in both samples confirms that TARP funds rescued TARP CEOs during the crisis.

3.2 Non-compete agreements

One important assumption in our analysis is that CEOs who left their bank could have joined another institution upon their departure. Some firms, however, might have put non-compete agreements in place, i.e., the CEOs would not have been allowed to join a competitor for a certain period. Kini, Williams, and Yin (2021) study CEO non-compete agreements and the impact of these agreements on the monitoring and compensation of CEOs. In their analysis of S&P1500

firms, they find that 62.47% firms implement non-compete agreements. Non-compete agreements could have affected our results on CEO turnover (banks might have not been willing to fire CEOs on the grounds that they could disclose important information to competitors) and the ability of CEOs to find another executive position upon being fired. However, in general, it is true that non-compete agreements do not allow former CEOs to join competitors for a maximum period of two years, though we are studying a broader time frame after the CEOs left their financial institution.

As reported by Kini, Williams, and Yin (2021), regulations at the State level play an important role in the enforcement of non-compete agreements and many States have not taken a stand on the enforceability of these. We take into account the differences between States regarding contract enforceability by introducing into our regressions a non-compete enforcement score (*Enforcement score)* developed by Kini et al. (2021), where a value between 0 and 9 is assigned to each State based on its enforcement of these agreements, with 0 being the weakest enforcement and 9 being the strictest.

We check whether our results hold when controlling for (i) State fixed effect to capture for all types of regulation at the State level, and (ii) the *Enforcement score* variable. Results are reported in the Online Appendix OA5. In Columns 1-2, the dependent variable is a dummy variable that takes the value of 1 if the CEO remained in office until at least 2010, and zero otherwise. In Columns 3 and 4 the dependent variable takes value 1 if the CEO held an executive position after the crisis, and zero otherwise. All the governance, bank and CEO control variables used in Table 2, column 3 are included. We restrict the sample to CEOs that are 60 or younger in 2007. Columns 1 and 3 present the results when we add the State fixed effect. In Columns 2 and 4, we control for the *Enforcement score* variable. Since this variable is measured at the State level, we remove the State fixed effect. The results hold. TARP CEOs have a higher probability of remaining in office than non-TARP CEOs up to 2010, whereas they experience a lower probability of having an executive position between 2011 and 2013.

3.3 The influence of CEO compensation on CEO retention

The executive compensation restriction imposed on recipients of TARP funds may have influenced their willingness to participate in the program (Cadman, Carter, and Lynch, 2012). Indeed, CEOs with a higher level of compensation may have been more reluctant to accept capital injections in order to avoid a reduction in their future pay.

We collect compensation data from Compustat ExecuComp. In line with the sample in Fahlenbrach and Stulz (2011), we find data for 98 banks. As the data available clearly shrinks our

sample, we have not included salary as a control variable in our main tables, but we discuss this potential effect on our main result in this section.

Focusing on 2007, we observe an average salary of $670,000 and total CEO compensation of $5.5 million. In our analysis, we first check whether there were any differences between TARP and non-TARP CEO compensation immediately before the allocation of funds. Although non-TARP CEOs received higher total compensation in 2007 ($6.6 million) than CEOs of TARP banks ($4.9 million), the difference is not statistically significant. Moreover, the level of compensation does not differ between the sample of TARP CEOs who were retained and those who left. Nevertheless, we check the robustness of our results by controlling for CEO compensation in the regression analyses. The results hold, suggesting that the level of compensation at the time of TARP implementation does not influence the relationship between program participation and CEO retention.

Focusing on compensation *per se*, one interesting question is whether participation in the TARP program affected CEOs' future pay levels. A precise measure of this effect is difficult to estimate for at least two reasons. First, in 2009 the U.S. Treasury imposed a limit of $500,000 on the total annual compensation of CEOs. Given this cap, it is difficult to estimate the causal effect of TARP on CEO salaries. Second, almost all the departing CEOs left the executive market or took up positions at unlisted firms where we cannot determine their level of compensation. Nevertheless, their total pay in these new positions would certainly have been lower than their former CEO compensation. Following Gao, Lemmon, and Li (2010), Gao and Li (2015), and Eckbo, Thorburn, and Wang (2016), to estimate the salaries at private firms, we apply a reduction of 20% to the salaries paid by public ones. Since the number of retained CEOs was 12% higher (marginal effect of Table 2) for TARP CEOs, this means that they had a higher probability of avoiding reduction in compensation suffered by CEOs who left the executive market. On the other hand, after the crisis, the same TARP CEOs had a lower probability of remaining in an executive position (13% lower, marginal effects of Table 5).

3.4 Excluding large banks/ Including all non-bank institutions

Lastly, we perform two further robustness checks. First, we find that our results hold when excluding large banks that were subjected to stress tests under the Capital Assessment Plan (CAP). We use the list of large banks provided in Duchin and Sosyura (2014). Second, we check the robustness of our results when enlarging the sample to include other financial institutions, which means considering SIC codes 60-63 and 6712, only excluding security brokers following Fahlenbrach and Stulz (2011). All results hold.

4. Conclusion

This paper investigates the effects of government bailouts on the careers of bank CEOs with its subsequent implications for bank specific and systemic risk. We exploit TARP capital injections that resulted in the bailout of hundreds of US banks during the 2008-2009 financial crisis. We perform two steps. First, we investigate whether the allocation of TARP funds affected the probability of CEOs retaining their position during and after the crisis. Second, we investigate the effects of TARP CEO replacements on the risk profile of their respective banks as well as on their contributions to systemic risk.

We find that during the crisis, CEOs at the helm of banks receiving TARP funds were significantly more likely to keep their position compared to CEOs of non-TARP banks. However, after the crisis, TARP CEOs were more likely to be replaced than their fellow non-TARP CEOs. We find that this turnover was mainly driven by a drop in the banks' political capital affecting the expected probability of receiving future government aid.

Importantly, we show that TARP banks retaining their CEOs, experienced a higher increase in their risk profile and in their contribution to systemic risk, vis-à-vis TARP banks that replaced their CEOs. Our evidence suggests that government bailouts have financial stability consequences through bank CEO's incentives. This finding has policy implications that center around the question of whether managers of bailout banks should be replaced, a topic that has attracted much attention during the financial crisis.[8] Our findings shed light on the fact that replacing top managers of financial institutions receiving government aid could mitigate the moral hazard problems with regard to excessive risk-taking behaviors, resulting in positive consequences for the statbility of the financial system.

[8] For example, on April 1, 2009, U.S. Treasury secretary Timothy Geithner said, in an interview with CBS, that he would consider forcing out chief executives of banks receiving government assistance if they were not managing their businesses properly (Reuters, April 2, 2009)

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Figure 1. Employment status of CEOs after the crisis

This figure reports the employment status of CEOs (in office in 2007) at the end of 2013. Data on CEOs' careers are from Execucomp, BoardEx, Bloomberg, Linkedin, and press articles. We classify the departing CEO as *Executive (non-CEO)* if she or he held the role of CFO, CCO or any other executive position. For each employment status, we report the number of CEOs that were at the helm of a bank that received TARP funds (TARP CEOs) and the number of CEOs of banks that did not receive funds (non-TARP CEOs).

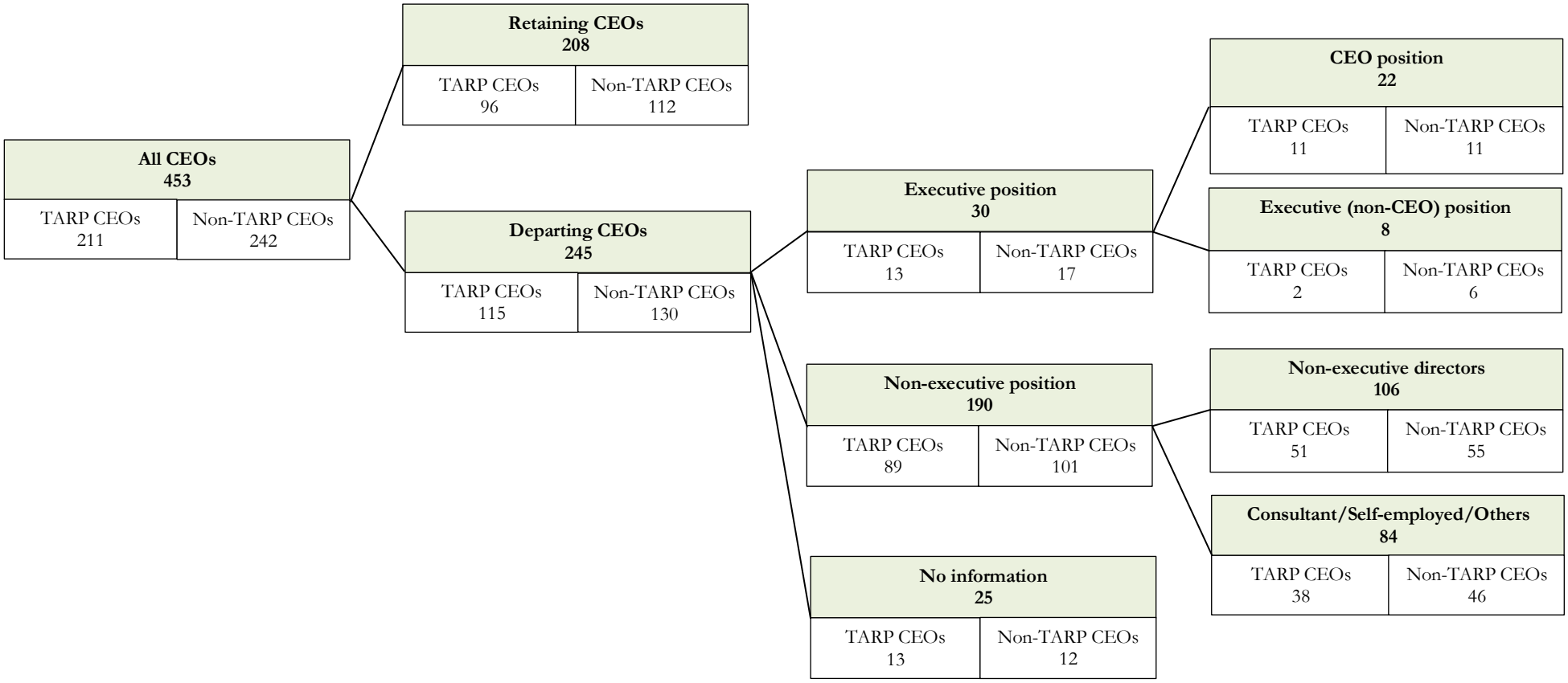

Table 1. Summary statistics

This table provides summary statistics of our sample of listed banks (SIC codes 60, commercial banks, 61, non-depository credit institutions, and 6712, bank holding companies) active in 2007. *CEO Retained*^{crisis} is a dummy variable that takes value 1 if the CEO remains at the same bank from the time of the allocation of TARP funds until 2010, and 0 otherwise. CEO and governance characteristics (CEO age and tenure, number of directors and number of independent directors) are obtained from BoardEx and ExecuComp. Sample bank characteristics are obtained from Compustat. Definitions of all the variables are provided in the Appendix.

	Mean	Median	Standard deviation	N
CEO Retained^{crisis}	0.68	1	0.46	453
CEO age	56	57	0.13	439
CEO tenure	5	6	0.79	439
Total assets ($ million)	19585.42	1235.09	146842	443
Market-to-book ratio	1.22	1.14	0.49	443
Beta	0.71	0.57	0.56	443
Bank age	10	11	0.85	443
Return on assets (ROA) (%)	0.03	0.49	0.02	443
Return 2006	0.16	0.13	0.24	426
Wholesale debt ratio	0.18	0.17	0.09	421
Fraction non-interest income	0.16	0.18	0.23	420
Loan loss provisions/Loans (%)	0.96	0.54	0.01	420
Tier 1 capital ratio (%)	10.52	9.88	2.83	388
Number of directors (log)	2.32	2.30	0.31	438
Fraction of independent directors	0.78	0.80	0.11	438

Table 2. TARP funds and probability of CEO retention during the financial crisis

This table shows the effect of TARP on the probability of CEO retention. The dependent variable of the probit regression $CEO\ Retained_i^{Crisis}$ takes the value of 1 if the CEO remained at the same bank from the time of the allocation of TARP funds until 2010, and 0 otherwise. *TARP* is a dummy variable equal to 1 if the bank received TARP funds, and 0 otherwise. Control variables include CEO, governance, and bank characteristics. Definitions of all variables are provided in the Appendix. Columns 1–3 include the sample of CEOs who were 60 years old or younger, Columns 4–6 include all CEOs. We use Huber-White robust standard errors and z-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Dependent variable	$CEO\ Retained_i^{Crisis}$					
	CEOs aged ≤60			*All CEOs*		
	(1)	(2)	(3)	(4)	(5)	(6)
TARP	0.260*	0.300*	0.364**	0.14	0.125	0.181
	(1.75)	(1.88)	(1.97)	(1.14)	(0.91)	(1.19)
CEO characteristics:						
CEO age		-1.806**	-1.862*		-2.035***	-2.424***
		(-2.05)	(-1.86)		(-3.61)	(-3.78)
CEO tenure		-0.0315	-0.017		-0.033	0.004
		(-0.29)	(-0.14)		(-0.38)	(0.04)
Governance characteristics:						
Number of directors		-0.278	-0.445		-0.251	-0.407
		(-0.95)	(-1.27)		(-1.03)	(-1.47)
Independent directors		-0.687	-0.906		-0.478	-0.557
		(-0.93)	(-1.08)		(-0.76)	(-0.80)
Bank characteristics:						
Bank age		0.059	0.11		0.086	0.149
		(0.47)	(0.73)		(0.83)	(1.24)
Total assets		0.016	-0.008		-0.001	0.006
		(0.19)	(-0.08)		(-0.01)	(0.07)
Market-to-book ratio		0.224	0.097		0.175	0.024
		(1.14)	(0.42)		(1.07)	(0.13)
Beta		-0.286	-0.029		-0.218	0.025
		(-1.50)	(-0.12)		(-1.31)	(0.13)
Return 2006		0.135	0.646		0.211	0.525
		(0.35)	(1.28)		(0.62)	(1.30)
Return on assets (ROA)		14.223***	13.694		14.191***	10.706
		(2.73)	(1.40)		(2.99)	(1.27)
Tier 1 capital ratio			0.056			0.03
			(1.35)			(0.91)
Loan loss provisions/Loans			-26.330**			-26.937***
			(-2.38)			(-2.84)
Wholesale debt ratio			0.435			0.289
			(0.37)			(0.31)
Non-interest income			-0.085			-0.137
			(-0.22)			(-0.38)
Constant	0.480***	8.530**	8.816**	0.416***	9.348***	10.91***
	(4.79)	(2.40)	(2.21)	(4.99)	(3.88)	(4.03)
Observations	332	309	268	453	418	369
Pseudo R^2	0.008	0.098	0.143	0.002	0.087	0.123

Table 3. CEO turnover and TARP: Instrumental variable analysis

This table shows results of the instrumental variable regression. We instrument the *TARP* variable using the local competitors' probability of being granted TARP funds. We split the banks into two groups: banks with a high probability of receiving TARP (HPT banks) and those with a low probability of receiving TARP (LPT banks). The group of HPT banks includes large or politically connected banks. Large banks are those with total assets above the sample median of banks in the State. Connected banks are those headquartered in a district of a House member who served on the Capital Market or Financial Institution subcommittee in 2008 or 2009. The group of LPT banks includes all other banks. For each bank, we compute the ratio of the number of other banks in the same State and group that applied for TARP (e.g. the number of HPT applicants in the State), divided by the total number of banks in the same group and State. We use this ratio, *TARP competition ratio*, as instrument. In Column 1 we show the first stage regression of the instrumental variable analysis. In Columns 2-3 we regress the probability of CEO retention on the *TARP* variable instrumented using the *TARP competition ratio*. $CEO\ Retained_i^{Crisis}$ takes the value of 1 if the CEO remained at the same bank from the time of the allocation of TARP funds until 2010, and 0 otherwise. We add State Fixed effects, all banks and governance characteristics and *other controls* that includes CEOs' personal connection, and dummies indicating whether the bank was classified as large and connected for the construction of the instrument. In Columns 4 and 5 we show the first and second stage regression including two-digit Zip code fixed effects. In Column 6 we present the unbiased estimator of Andrews and Armstrong (2017). ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

	First stage	Second stage		First stage	Second stage	Unbiased estimators
Dependent variable	*TARP*	$CEO\ Retained_i^{Crisis}$		*TARP*	$CEO\ Retained_i^{Crisis}$	
			Age<=60			
	(1)	(2)	(3)	(4)	(5)	(6)
TARP competition ratio	-0.638***			-0.680***		
	(-3.75)			(-3.35)		
TARP (instrumented)		0.816***	0.896**		1.172***	1.093***
		(2.94)	(2.14)		(3.24)	(3.02)
CEO characteristics	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Governance characteristics	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Bank characteristics	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
Other controls	*Y*	*Y*	*Y*	*Y*	*Y*	*Y*
State FE	*Y*	*Y*	*Y*	*N*	*N*	*N*
Zip code FE	*N*	*N*	*N*	*Y*	*Y*	*Y*
Effective F statistic	14.87			11.95		
Observations	343	343	249	343	343	343

Table 4. Panel A. Bank risks, retained CEO and TARP funds

This table reports bank fixed effect regressions. The dependent variables are market beta (Columns 1–4), *Loan Loss Provisions/Loans* (Columns 5-6), and *Volatility of Returns* (Columns 7 and 8). *After CPP*TARP* is the interaction of the *TARP* variable (dummy equal to 1 if the bank was granted TARP funds, and 0 otherwise) with *After CPP* (an indicator equal to 1 in the years 2009–2011, and 0 in the years 2005–2008). The *After CPP* and *TARP* variables drop out of the regression due to the inclusion of year and bank fixed effects. We also include bank-time level controls (size, market-to-book ratio, ROA, the ratio of amount of deposits to total liabilities and Tier 1). Columns 1, 3, 5 and 7 only include banks that retained their CEOs in the years 2008–2010 and Columns 2, 4, 6, 8 only banks that replaced their CEOs. Standard errors are clustered at the bank level and *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Dependent variables	Beta		Beta (FF3)		Loan Loss Provision/Loans		Volatility of Returns	
	CEO retained	CEO replaced	CEO retained	CEO replaced	CEO retained	CEO replaced	CEO retained	CEO replaced
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After CPP*TARP	0.063**	-0.334**	-0.0171	-0.258**	0.001***	-0.002	0.003***	-0.002
	(2.00)	(-2.17)	(-0.465)	(-2.048)	(3.08)	(-0.52)	(3.11)	(-0.42)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,856	723	1835	710	1,891	736	1,856	723
R-squared	0.133	0.215	0.076	0.142	0.697	0.781	0.732	0.805

Table 4. Panel B. Systemic risk, retained CEO and TARP funds

This table reports bank fixed effect regressions. The dependent variables are the Systemic Expected Shortfall (*SES),* (Columns 1–2), and the Normalized Systemic Risk Measure (*NSRISK),* (Columns 3 and 4). *After CPP*TARP* is the interaction of the *TARP* variable (dummy equal to 1 if the bank was granted TARP funds, and 0 otherwise) with *After CPP* (an indicator equal to 1 in the years 2009–2011 and 0 in the years 2005–2008). The *After CPP* and *TARP* variables drop out of the regression due to the inclusion of year and bank fixed effects. We also include bank-time level controls (size, market-to-book ratio, ROA, the ratio of amount of deposits to total liabilities and Tier 1). Columns 1 and 3 only include banks that retained their CEOs in the years 2008–2010 and Columns 2 and 4 only banks that replaced their CEOs. Standard errors are clustered at the bank level and *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Dependent variables	SES	SES	NSRISK	NSRISK
	CEO retained	CEO replaced	CEO retained	CEO replaced
	(1)	(2)	(3)	(4)
After CPP*TARP	0.258***	-0.180**	0.327**	0.388
	(4.336)	(-2.044)	(2.491)	(1.315)
Controls	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	1,824	678	1,808	682
R-squared	0.426	0.470	0.401	0.546

Table 5. TARP funds and CEO careers after the crisis.

This table focuses on the employment status of CEOs after the financial crisis and reports results of probit regressions. In Columns 1-4 we restrict the sample to CEOs that were still at the helm of their banks in 2010. In Columns 1 and 2 the dependent variable, $CEO\ Retained_i^{After\ Crisis}$, takes the value of 1 if the CEO is still in the former position at the end of 2013 and zero otherwise. In Columns 3 and 4 the dependent variable $CEO\ Executive\ Position_i^{After\ Crisis}$ takes the value 1 if the CEO holds an executive position at the former bank or at another firm at the end of 2013 and zero otherwise. In Column 5 we restrict our sample to CEOs that left their former bank after 2010. The dependent variable $CEO\ in\ another\ FI_i^{After\ Crisis}$ takes value 1 if the departing CEO became CEO, executive non-CEO, or non-executive director in another bank, and 0 otherwise. All the controls are measured at end of 2010. In Columns 1 and 3, we restrict the sample to CEOs aged 60 years old or younger as of 2010. In Columns 2, 4 and 5, we restrict the sample to CEOs aged 60 years old or younger as of 2007. Robust χ-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively. Robust z-statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1

	$CEO\ Retained_i^{After\ Crisis}$		$CEO\ Executive\ Position_i^{After\ Crisis}$		$CEO\ in\ another\ FI_i^{After}$
	(1)	(2)	(3)	(4)	(5)
TARP	-0.587**	-0.709**	-0.530**	-0.639*	-0.556
	(-2.35)	(-2.13)	(-2.14)	(-1.90)	(-1.436)
CEO characteristics:					
CEO age	-6.018***	-5.299***	-6.152***	-5.456***	-2.731
	(-4.61)	(-3.10)	(-4.67)	(-3.18)	(-1.382)
CEO tenure	0.343	0.308	0.272	0.191	0.423
	(1.30)	(0.95)	(1.03)	(0.58)	(1.108)
Governance characteristics:					
Number of directors	-0.220	-0.124	-0.284	-0.248	0.812
	(-0.39)	(-0.17)	(-0.50)	(-0.35)	(1.170)
Independent directors	-1.880	-2.772*	-1.726	-2.547	-1.109
	(-1.49)	(-1.68)	(-1.36)	(-1.55)	(-0.785)
Bank characteristics:					
Bank age	-0.278	-0.053	-0.403	-0.222	0.258
	(-0.98)	(-0.17)	(-1.42)	(-0.70)	(0.626)
Total assets	0.109	0.180	0.115	0.180	-0.0450
	(0.83)	(1.09)	(0.87)	(1.06)	(-0.244)
Market-to-book ratio	0.189	0.593*	0.182	0.515*	0.594*
	(1.06)	(1.84)	(1.03)	(1.80)	(1.862)
Beta	0.378	-0.113	0.362	-0.133	0.186
	(1.24)	(-0.31)	(1.18)	(-0.36)	(0.453)
Return on assets (ROA)	17.56	61.04**	16.48	65.32**	-41.76*
	(1.01)	(2.37)	(0.93)	(2.44)	(-1.889)
Tier 1 capital ratio	-0.008	-0.043	-0.005	-0.036	0.0306
	(-0.24)	(-1.10)	(-0.14)	(-0.90)	(0.554)
Loan loss provisions/Loans	-15.77	-7.746	-18.65	-8.428	-42.65**
	(-0.92)	(-0.38)	(-1.08)	(-0.41)	(-1.976)
Wholesale debt ratio	0.949	-0.951	0.806	-1.118	1.710
	(0.57)	(-0.43)	(0.49)	(-0.52)	(0.629)
Non-interest income	-1.041	-2.083	-0.589	-1.371	-0.397
	(-0.89)	(-1.47)	(-0.51)	(-0.96)	(-0.199)
Constant	26.57***	23.92***	27.50***	25.17***	9.115
	(4.69)	(3.31)	(4.77)	(3.41)	(1.071)
Observations	187	144	187	144	68
Prob > Chi^2	0.000	0.001	0.000	0.001	0.409
Pseudo R^2	0.193	0.226	0.194	0.221	0.146

Table 6. CEO turnover, political connections, and TARP repayment.

This table shows the factors that explain the probability of retaining TARP CEOs after the crisis. The dependent variable of the probit regression $CEO\ Retained_i^{AfterCrisis}$, takes the value of 1 if the CEO is still in the same position as of 2007 at the end of 2013, and zero otherwise. *Bank (CEO) connection (crisis)* indicates whether the bank (CEO) is connected in the years 2009/2010, while *Bank (CEO) connection (after crisis)* refer to the years 2011/2012. *N. Tranches Repayment* is the number of tranches used by the bank to repay the funds. *Amount Tarp Funds* is the amount of funds received scaled by the bank's market capitalization value. *Repayment after 2010* takes the value of 1 if the bank repaid funds after 2010, and 0 otherwise. In all Columns we restrict the sample to TARP banks whose CEOs were still at the helm of their banks in 2010. Columns 1-2 show the results for CEOs aged 60 years old or younger, Columns 3-4 include all CEOs. We use Huber-White robust standard errors and z-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

VARIABLES	(1)	(2)	(3)	(4)
	$CEO\ Retained_i^{AfterCrisis}$			
CEO connection (crisis)	-0.872**	-1.189***	-0.811**	-1.217***
	(-2.312)	(-2.831)	(-2.520)	(-3.101)
CEO connection (after crisis)	0.260	0.260	0.445	0.485
	(0.726)	(0.599)	(1.421)	(1.281)
Bank connection (crisis)	-0.659*	-0.782*	-0.571	-0.814*
	(-1.837)	(-1.701)	(-1.537)	(-1.734)
Bank connection (after crisis)	0.986*	1.426**	1.141**	1.682***
	(1.810)	(2.299)	(2.125)	(2.598)
N. Tranches Repayment	0.546**	0.621**	0.728***	0.772***
	(2.351)	(2.181)	(3.165)	(3.135)
Amount TARP Funds	0.566*	0.909**	0.309	0.803**
	(1.784)	(1.970)	(1.149)	(2.276)
Repayment after 2010	-0.467	-0.412	-0.0820	0.292
	(-1.258)	(-0.824)	(-0.271)	(0.782)
Total assets	0.145	0.374*	0.137	0.325*
	(1.288)	(1.812)	(1.426)	(1.755)
Number of directors		0.522		0.429
		(0.589)		(0.678)
Independent directors		-1.180		-0.210
		(-0.639)		(-0.212)
CEO age		-7.055***		-3.256***
		(-3.519)		(-2.639)
CEO tenure		0.262		0.326
		(0.626)		(1.009)
Bank age		-0.736		-0.585
		(-1.587)		(-1.508)
Market-to-book ratio		1.073		1.514***
		(1.520)		(2.621)
Beta		0.00671		0.00795
		(0.0140)		(0.0202)
Return on assets (ROA)		61.90*		30.98
		(1.868)		(1.136)
Tier 1 capital ratio		-0.0406		-0.00339
		(-0.734)		(-0.0741)
Loan loss provisions/Loans		24.77		-12.53
		(0.966)		(-0.543)
Wholesale debt ratio		-2.387		-3.716*
		(-0.901)		(-1.670)
Non-interest income		-1.899		-0.905
		(-1.179)		(-0.584)
Constant	-7.713*	15.76	-5.151	-0.104
	(-1.927)	(1.501)	(-1.474)	(-0.0141)
Observations	107	106	129	128
Prob > chi2	0.0117	0.000142	0.00447	0.000131
Pseudo R2	0.142	0.346	0.114	0.310

Appendix
Table A1

This table provides descriptions and sources of the main variables used in the regression analyses unless otherwise specified.

Variable	***Description***	***Source***
TARP	Dummy variable equal to 1 if the firm received TARP funds	Treasury Department website: http://www.treasury.gov/initiatives/financial-stability/Pages/default.aspx
CEO Retained$_i^{Crisis}$	Dummy variable equal to 1 if the CEO stayed in the same position between 2008 and 2010.	ExecuComp/Boardex/ Linkedin/Bloomberg
CEO Retained$_i^{AfterCrisis}$	Dummy variable equal to 1 if the CEO remained in the same position at the end of 2013.	ExecuComp/Boardex/ Linkedin/Bloomberg
CEO Executive Position$_i^{AfterCrisis}$	Dummy variable equal to 1 if the CEO held another executive position at the end of 2013.	ExecuComp/Boardex/ Linkedin/Bloomberg
CEO in another FI$_i^{After\ Crisis}$	Dummy variable equal to 1 if the departing CEO became CEO, executive non-CEO, or non-executive director in another bank, and 0 otherwise.	ExecuComp/Boardex/ Linkedin/Bloomberg
CEO age	Log of CEO age as of December 2007	ExecuComp/Boardex
CEO tenure	Log of CEO tenure as of December 2007	Boardex /ExecuComp
Number of directors	Log of number of directors as of December 2007	Boardex
Independent directors	Number of independent directors over number of directors as of December 2007	Boardex
Bank age	Log of the bank age, i.e. number of years from the date of market listing until December 2007	Compustat
Total assets	Log of total assets as of September 2008	Compustat
Return 2006	Annualized stock return from July 2005 until December 2006	Compustat
Market-to-book ratio	Market-to-book ratio as of September 2008	Compustat
Beta	Equity beta from a market model of weekly returns from September 2005 to September 2008	Compustat
Tier 1, Tier 2	Tier 1, 2 capital ratio as of September 2008	Compustat
Loan loss provisions/ loans	Sum of the last four quarters of loan loss provision scaled by average loans as of September 2008	Compustat
ROA	Return on assets as of September 2008	Compustat
Fraction of non-interest income	Ratio of non-interest income to the sum of non-interest income and net interest income as of September 2008	Compustat
Wholesale debt ratio	Total liabilities minus deposits scaled by assets as of September 2008	Compustat

Online Appendix

In Table OA1, we implement a propensity score matching (PSM) approach where the treatment sample is banks receiving TARP funds. The idea behind this analysis is that banks with similar characteristics should have had a similar probability of applying for funds, and then a similar probability of receiving them. Following the literature, we select bank characteristics in the pre-treatment period that could have affected the treatment (bank received TARP funds). Bayazitova and Shivdasani (2011) show that approved banks were larger and posed a greater systemic risk but had better asset quality. Therefore, in the first step of the PSM, we include bank size (total assets) and loan loss provision as a measure of expected lending losses. We also add a measure of capital adequacy (Tier 1) and controls for risk and performance (Beta and ROA). In addition, we include a measure for political connections. Previous studies show that politically connected banks were more likely to receive funds (Blau, Brough, and Thomas, 2013; Duchin and Sosyura, 2012, 2014; Croci, Herting and Nowak, 2016) and connections may also influence a bank's risk-taking behavior (Kostovetsky, 2015).

Following Duchin and Sosyura (2014), we developed a "politically connected" variable (*Connected Committee)* based on connections to a member of the House Financial Services Committee. A financial institution is considered to be connected to a member of Congress who sits on one of the Committees if it is headquartered in his or her district. The Committee was involved in the development of the Emergency Economic Stabilization Act of 2008 (and its amendment in 2009), in monitoring TARP programs and approving TARP amendments. Following Duchin and Sosyura (2014), we focus on two subcommittees; the Subcommittee on Financial Institutions (which supervises all the main banking regulators) and the Subcommittee on Capital Markets (which examines regulations on capital markets and investment banks). The *Connected committee* variable is equal to 1 if a bank was connected to at least one key subcommittee either in 2008 or in 2009 and 0 otherwise.

The matching procedures are carried out with replacement, which means that each control (non-TARP) bank can be used as a neighbor for several treated banks (TARP). We use 1:1 matching, which means that each treated bank is matched with the closest non-treated one. The matching procedure was unable to find a good match for three TARP banks and so they are not taken into account in the estimation. We also conduct the analysis using caliper estimation. To further check the robustness of our results we replicate the test using up to three nearest neighbors as a control for each TARP bank.

We compare treated and untreated banks after propensity score matching in order to test the success of the procedure. Panel A of Table OA1 shows that the selected TARP and matched non-TARP banks do not differ in term of size, risk, performance, and political connections in the pre-treatment period.

We exploit the matched sample and compare the probability of the CEO remaining until 2010 in TARP vs matched non-TARP banks. The results are given in Panel B. As in Table 2, we report the results with the restriction on CEO age in Columns 1–3 and without the restriction in columns 4–6.

In Columns 1 and 2, 4 and 5, we show results where each TARP bank is matched with one non-TARP bank. Note that if we use more than one neighboring bank for the control group, the results are similar. In columns 3 and 6, we use caliper estimation. Finally, in Columns 1 and 4, we do not add any control variables, while in all other columns we introduce a full set of controls into the regression as in Table 2, column 3.

By comparing banks with similar characteristics, we find that capital injection by the U.S. Treasury significantly increased the probability of the CEO remaining. The marginal effect of the TARP variable in column 2 shows that CEOs of banks that received TARP funds were 17.6% more likely to remain than non-TARP CEOs. Interestingly, the magnitude of the effect is stronger than that reported in the baseline regression of Table 2, confirming that among

similar banks, the injection of TARP funds reduced the probability of CEO departure. Our results are robust in all specifications; when controlling for governance, CEO, and bank characteristics, when considering the entire sample of CEOs, when taking the closest bank to the treated bank as a neighbor, or when performing the PSM using caliper estimation.

Overall, there appears to be no punishment from the banking side linked to TARP funds, as CEOs did not lose their jobs when banks managed to obtain the funds. On the contrary, the CEO benefited from the government aid.

Table OA1. CEO turnover for TARP and matched non-TARP banks

This table shows the effect of TARP on the probability of CEO retention using propensity score matching analysis. We compare TARP banks (treatment group) and a control group selected among non-TARP banks (matched control group). Panel A presents the differences between TARP and matched non-TARP banks observed in 2008 and a *t*-test to check the quality of the matching procedure. Panel B shows the probability of the CEO remaining by comparing TARP banks and matched banks that did not receive funds, using several matching techniques. In Columns 1, 2, 4, and 5 in the matching procedure, we take the closest control bank for each TARP bank. The matching process is performed with replacement. Columns 3 and 6 show the results using caliper matching. In Columns 1, 2, and 3 we show the results for CEOs who were 60 years old or younger. In columns 2, 3, 5 and 6 we introduce the full set of controls as in Table 2 Column 3. The dependent variable $CEO\ Retained_i^{Crisis}$ takes the value of 1 if the CEO remained at the same bank from the time of the allocation of TARP funds until 2010, and 0 otherwise. *TARP* is a dummy variable equal to 1 if the bank received TARP funds, and 0 otherwise. We use Huber-White robust standard errors and z-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Panel A: Differences between TARP and non-TARP banks

	TARP banks Treatment Group	Non-TARP banks Matched control group	t-test
Total assets	2.435	2.359	0.93
Market-to-book ratio	1.247	1.279	-0.60
Tier 1 capital ratio	9.908	9.762	0.67
Return on assets (ROA)	0.002	0.002	0.58
Loan loss provisions/Loans	0.009	0.009	-0.53
Political connections	0.183	0.203	-0.51

Panel B: Probit regression with TARP banks (treated) and matched non-TARP banks (untreated).

Dependent variable	$CEO\ Retained_i^{Crisis}$					
	CEOs aged ≤60			*ALL CEOs*		
	(1)	(2)	(3)	(4)	(5)	(6)
TARP	0.349**	0.538***	0.651***	0.277**	0.412***	0.428***
	(2.19)	(3.05)	(3.48)	(2.12)	(2.81)	(2.82)
Controls	N	Y	Y	N	Y	Y
Observations	283	271	251	394	379	354
Pseudo R2	0.014	0.169	0.187	0.009	0.164	0.165

Table OA2. Capital ratios, retained CEOs and TARP funds

This table reports bank fixed effect regressions. The dependent variables are Tier 1 capital ratio (Columns 1–3) and Tier 2 capital ratio (Columns 4–6). *After CPP*TARP* is the interaction of the *TARP* variable (dummy equal to 1 if the bank was granted TARP funds, and 0 otherwise) with *After CPP* (an indicator equal to 1 in 2009–2011 and 0 in 2005–2008). The *After CPP* and *TARP* variables drop out of the regression due to the inclusion of year and bank fixed effects. We also include bank-time level controls (size, market-to-book ratio, ROA, and the ratio of amount of deposits to total liabilities). Columns 1 and 4 include all banks, columns 2 and 5 only banks that kept their CEOs, and Columns 3 and 6 only banks that replaced their CEOs in the years 2008–2010. Standard errors are clustered at the bank level and *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

	Tier 1			*Tier 2*		
	All banks	CEO retained	CEO replaced	All banks	CEO retained	CEO replaced
	(1)	(2)	(3)	(4)	(5)	(6)
After CPP*TARP	1.997***	2.135***	4.012***	0.277***	0.387***	-0.0775
	(12.08)	(11.83)	(3.86)	(3.03)	(4.19)	(-0.12)
Controls	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	2,629	1,892	737	2,629	1,892	737
R-squared	0.211	0.242	0.162	0.012	0.022	0.022

Table OA3. CAR: CEO replacement after the crisis and TARP repayment

This table presents abnormal returns around the days of the announcements of CEOs departures (Panel A) and the first repayment of TARP funds by banks (Panel B). In panel A, we distinguish between TARP and Non-TARP CEOs. In Panel B, we distinguish between TARP CEOs who remained in their position until 2013 and CEOs who did not. CAR (-1;1) are abnormal returns calculated with a market model using the S&P 500. The parameters of the model are estimated on a period of 180 trading days, starting 30 days before the event. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively for the t-test of the null hypothesis that the average CAR(-1;1) is zero.

Panel A: CAR (-1-;1) around CEO replacement after the crisis

	CAR(-1;1)	N
TARP CEOs	0.05*	31
Non-TARP CEOs	0.01	27

Panel B: CAR(-1;1) around first TARP repayment day

	CAR(-1;1)	N
CEO is retained	0.010*	57
CEO is replaced	0.003	57

Table OA4. CEO turnover and TARP application status

This table shows the effect of TARP on the probability of CEO retention according to the bank's application status. In Columns 1 and 2, we compare TARP banks with non-TARP participating banks. In Columns 3 and 4, we compare TARP banks with banks that explicitly disclosed Treasury rejection. In Columns 5 and 6, we compare TARP banks with banks that explicitly disclosed that they had not applied for TARP funds. In all models, the dependent variable of the probit regression $CEO\ Retained_i^{Crisis}$ takes the value of 1 if the CEO was retained until 2010, and 0 otherwise. *TARP* is a dummy variable equal to 1 if the bank received TARP funds, and 0 otherwise. We use Huber-White robust standard errors and z-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Dependent Variable	$CEO\ Retained_i^{Crisis}$					
	TARP & Non-TARP Participating banks		*TARP & Rejected bank*		*TARP & Nonparticipating banks*	
	CEOs aged ≤60	*All CEOs*	*CEOs aged ≤60*	*All CEOs*	*CEOs aged ≤60*	*All CEOs*
	(1)	(2)	(3)	(4)	(5)	(6)
TARP	0.668**	0.526**	0.779**	0.717**	0.223	-0.001
	(2.34)	(2.24)	(1.97)	(2.16)	(0.99)	(-0.01)
CEO age	-1.575	-2.672***	-1.396	-3.037***	-2.672**	-2.926***
	(-1.29)	(-3.16)	(-1.11)	(-3.34)	(-2.30)	(-4.05)
CEO tenure	-0.062	-0.028	-0.172	-0.0161	-0.075	0.034
	(-0.44)	(-0.26)	(-1.13)	(-0.13)	(-0.53)	(0.31)
Number of directors	-0.515	-0.442	-0.591	-0.307	-0.559	-0.509*
	(-1.19)	(-1.32)	(-1.31)	(-0.85)	(-1.43)	(-1.66)
Independent directors	-1.105	-0.423	-0.662	0.039	-0.539	-0.325
	(-1.07)	(-0.52)	(-0.64)	(0.05)	(-0.63)	(-0.45)
Bank age	0.024	0.085	0.099	0.183	0.223	0.274**
	(0.12)	(0.58)	(0.47)	(1.12)	(1.32)	(2.01)
Total assets	0.015	0.011	0.0618	0.054	0.024	0.029
	(0.12)	(0.11)	(0.48)	(0.48)	(0.19)	(0.28)
Market-to-book ratio	-0.004	-0.103	-0.0368	-0.081	0.082	0.002
	(-0.02)	(-0.48)	(-0.14)	(-0.34)	(0.33)	(0.01)
Beta	-0.255	-0.121	-0.32	-0.226	-0.107	-0.071
	(-0.96)	(-0.55)	(-1.13)	(-0.92)	(-0.39)	(-0.32)
Return 2006	0.843	0.764	1.066*	0.902*	0.966	0.736
	(1.51)	(1.64)	(1.71)	(1.73)	(1.59)	(1.62)
Return on assets (ROA)	12.28	6.585	7.628	-0.542	1.971	6.82
	(1.27)	(0.76)	(0.73)	(-0.06)	(0.19)	(0.72)
Tier 1 capital ratio	0.038	0.041	0.00613	0.00543	0.041	0.009
	(0.67)	(0.87)	(0.08)	(0.10)	(0.86)	(0.24)
Loan loss provisions/Loans	-21.40*	-23.20**	-27.70**	-34.37***	-35.63**	-30.05**
	(-1.77)	(-2.21)	(-2.03)	(-2.87)	(-2.53)	(-2.51)
Wholesale debt ratio	-0.087	0.103	-0.578	-0.513	0.393	0.0601
	(-0.06)	(0.09)	(-0.36)	(-0.38)	(0.27)	(0.05)
Non-interest income	-0.03	-0.03	-0.41	-0.113	-0.445	-0.186
	(-0.07)	(-0.07)	(-0.79)	(-0.25)	(-0.95)	(-0.43)
Constant	8.311*	11.82***	7.544	12.46***	12.12***	13.01***
	(1.70)	(3.34)	(1.50)	(3.33)	(2.70)	(4.44)
Observations	194	258	194	258	224	307
Pseudo R^2	0.171	0.151	0.171	0.151	0.097	0.103

Table OA5. CEO turnover and State characteristics

This table shows the effect of TARP on the probability of CEO retention (Columns 1 and 2) and on the probability of holding an executive position after the crisis (Columns 3 and 4) controlling for State fixed effect and State-level non-compete enforcement score. $CEO\ Retained_i^{Crisis}$ takes value of 1 if the CEO remained in office in the same financial institution from 2008 to 2010, and 0 otherwise. $CEO\ Executive\ Position_i^{AfterCrisis}$ is equal to 1 if the CEO held an executive position in his/her previous financial institution or in another institution from 2011 to 2013, and 0 otherwise. TARP is a dummy variable equal to 1 if the company obtained TARP funds, and 0 otherwise. *Enforcement score* is a State-level non-compete enforcement score that takes a value between 0 and 9, with 0 being the weakest enforcement and 9 being the strictest. We use Huber-White robust standard errors and z-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

	$CEO\ Retained_i^{Crisis}$		$CEO\ Executive\ Position_i^{AfterCrisis}$	
	CEO Aged ≤ 60	CEO Aged ≤ 60	CEO Aged ≤ 60	Age CEO≤ 60
	(1)	(2)	(3)	(4)
TARP	0.525**	0.395**	-0.532*	-0.442*
	(2.10)	(2.01)	(-1.77)	(-1.77)
Enforcement score		-0.042		0.023
		(-0.79)		(0.41)
Controls	Y	Y	Y	Y
State FE	Y	N	Y	N
Observations	216	243	137	192
Pseudo R^2	0.233	0.155	0.260	0.186