Matching for Risk-Taking: Overconfident Bankers and Government-Protected Banks

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Matching for risk-taking: Overconfident bankers and government-protected banks*

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Abstract

We set up a simple theoretical model in which banks with varying degrees of government support are matched with CEOs that have different degrees of overconfidence. The channel through which the matching occurs is the share of bonus payments offered by banks in their profit-maximizing contracts. This yields a sequence of hypotheses when CEOs can freely choose risk levels: banks with more government support incentivize their CEOs more and this disproportionately attracts overconfident CEOs. In equilibrium this in turn leads to an assortative matching between overconfident managers and banks with a larger bailout probability. We then test the hypotheses derived from this model for U.S. data spanning both the Great Financial Crisis and the Covid Crisis. Our results confirm the hypotheses from our theoretical model for normal years, but not during crises and periods of enhanced regulation.

Keywords: matching, overconfidence, incentive contracts, bailouts

JEL classification: G21, G28, H32

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1 Introduction

Starting with the bankruptcy of Lehman Brothers in September 2008, the global financial crisis caught financial professionals and policymakers by surprise. Despite a buildup of systemic risks and fragility long before the peak, financial professionals were overly optimistic in their forcasts and policymakers were overly confident that no government bailout would be necessary until shortly before the event. This indicates that biased beliefs played a critical role in the buildup of the global financial crisis (Gennaioli and Shleifer, 2018). More recently, the failure of the Silicon Valley Bank in March 2023 similarly originated in biased beliefs and an underestimation of interest rate risks after a long period of monetary expansion and zero interest rates (Bloomberg, 2023).

The psychology of financial top executives has long been an important topic in the finance literature. While overconfidence is a frequently observed behavior even in the general population, it is particularly pronounced among high-earning top executives, who have experienced success in their previous career and attribute this success largely to their own abilities (Gervais and Odean, 2001; Goel and Thakor, 2008; Billett and Qian, 2008). As a result, CEOs are significantly more optimistic than the overall population (Graham et al., 2013). This leads them, for example, to invest more than non-overconfident CEOs (Malmendier and Tate, 2005; Pikulina et al., 2017), a behavioral trait that increases firm value when firms are financially constrained and do not exploit their growth opportunities (Aktas et al., 2019). In crisis times, this overconfidence is likely to backfire, however. For example, Ho et al. (2016) have shown that banks with overconfident CEOs weakened their lending standards prior to the 2007-2009 financial crisis, and performed significantly worse in the crisis, as compared to their non-overconfident counterparts. Taken together, overconfident CEOs overestimate their ability to run a bank and overestimate the likelihood of a high outcome, while underestimating the probability of failure.

A further cause of risk-taking that may be equally important are implicit or explicit government guarantees for banks and other financial institutions. One important form of government guarantees is deposit insurance, which is meant to avoid bank-runs in crisis times. However, this policy instrument has also been shown to systematically increase moral hazard and risk-taking by the protected banks (Demirgüç-Kunt and Detriagiache, 2002). The forced removal of explicit government guarantees for German savings banks caused these banks to significantly reduce their credit risk, indicating that public guarantees are associated with substantial moral hazard (Gropp et al., 2014). More recently, the focus has been on systemically important financial institutions (SIFIs). Ueda and Weder di Mauro (2013) quantify the value of the implicit subsidies for SIFIs, which arise from the possibility of bailouts. They find substantial subsidy-driven reductions in the cost of capital for SIFIs, which increased further during the financial crisis.

In this paper, we bring together these two strands in the literature, which have so far been largely separated.[1] We ask whether the risk-increasing effects of CEO overconfidence on the one hand, and of government guarantees on the other, accumulate via a matching process in which overconfident CEOs are systematically attracted to government-protected banks. The mechanism through which this assortative matching occurs are bonuses and other incentive payments. These bonus payments are well-known to stimulate risk-taking, exploiting the banks' limited liability (Hagendorff and Vallascas, 2011; Bhagat and Bolton, 2014; Efing et al., 2015). Moreover, incentive payments are particularly attractive for overconfident managers, who overestimate their probability of success and therefore the likelihood of receiving the bonus. This in turn allows rational banks and their shareholders to 'exploit' this overconfidence by adjusting their compensation structure and increasing the share of incentive pay.[2]

To study these issues, we first set up a simple theoretical model that connects overconfident mangers and government-protected banks via incentive contracts. Our model draws on the framework of optimal incentive pay developed by Besley and Ghatak (2013). Their model has been extended by Hakenes and Schnabel (2014) to study the effects of bailout expectations, and by Gietl and Kassner (2020) to incorporate overconfidence. The new element in our model is that we incorporate a full matching process that pairs managers with different degrees of overconfidence and banks of different size.[3] Using supermodularity to characterize the matching process generates comparative statics results describing under which conditions matching can be expected to be stronger or weaker.

From our theory, we derive a compact set of hypotheses for empirical testing. Banks of different size choose combinations of bonus payments and fixed pay to hire managers with different levels of overconfidence. Government support is rising in bank size, causing larger - and better protected - banks to use steeper incentive contracts. These incentive contracts are in turn more attractive for more overconfident bank managers, who overvalue the probability of receiving the bonus. In this framework, we derive four hypotheses: (*i*) overconfident managers take more risk; (*ii*) government-protected banks pay higher bonus shares; (*iii*) overconfident managers receive higher bonus shares in equilibrium; and (*iv*) overconfident managers are matched in equilibrium with government-protected banks. Our results also show that these relationships become weaker, if capital regulation is strict or if risk-taking causes high costs to managers, as in periods of crisis.

[1] Lee et al. (2020) find that CEO overconfidence has increased systemic risk in the buildup of the global financial crisis. This paper does not incorporate the effect of government guarantees, however. Another exception is Gietl and Kassner (2020), which is further discussed below.

[2] See Gervais et al. (2011) and De la Rosa (2011) for theoretical analyses. Humphery-Jenner et al. (2016) provide empirical evidence for this positive relationship between overconfidence and the share of incentive pay.

[3] Gietl and Kassner (2020) also include a matching section in an extension of their analysis, but this is confined to a two-bank, two-manager setting in which policy parameters are exogenously assigned to banks.

We then test the hypotheses empirically using the option-exercising behavior of managers to measure overconfidence (Malmendier and Tate, 2005, 2008). We derive the degree of government protection from the U.S. G-SIB scores, which determine the degree of systemic importance of U.S. financial institutions. Our regression results confirm the hypotheses from our theoretical model for periods of moderate regulation and outside of (financial and Covid-related) crises. In these years, banks with more government support incentivize their CEOs more and this disproportionately attracts overconfident CEOs. In equilibrium this in turn leads to an assortative matching between overconfident managers and banks with a larger bailout probability. The same relationships are not confirmed in crises and in periods of enhanced regulation, during which overconfident CEOs do not behave differently from non-overconfident CEOs. This is consistent with our model, which shows that the costs of risk-taking rise in these periods, for both banks and their CEOs.

Section 2 sets up our theoretical model linking overconfidence of CEOs and government support to banks in a model where banks optimize the compensation structure of CEOs. From this model we derive the main hypotheses of our analysis. Section 3 presents the data used in the empirical analysis, describes the main variables, and presents the regression results that test our theoretical propositions. Section 4 concludes.

2 Theory

2.1 Model setup

Our theoretical framework builds on Gietl and Kassner (2020). We extend their analysis to cover a continuous set of managers with heterogeneous degrees of overconfidence, which are matched in equilibrium with banks of varying size.

In our setting there is a continuum of banks, indexed by i, which differ in their level of organizational quality, ϕ_i. Effectively we envisage banks as being composed of identical divisions and the number of these divisions, n_i, determines the size of the bank. Running a larger bank imposes costs that are convex in bank size, n_i, but the cost profiles differ across banks as described by the parameter ϕ_i. In each bank's optimum, bank size will therefore be a monotone function of its organizational quality, $n_i(\phi_i)$, with $dn/d\phi > 0$. Importantly, the organizational quality of a bank affects only its equilibrium size, but not the profitability of each division. The latter depends only on the characteristics of the bank's CEO. Bank size matters only indirectly for each division's profits, because it affects the level of government support that the bank can expect.[4]

The continuum of bank managers (CEOs) is indexed by m. Managers differ in their

[4]Empirical support for our setting comes from Huber (2021), who finds that M&A-driven increases in bank size in postwar Germany were not associated with increases in profitability or efficiency, but did lead to more risky credit operations of the enlarged banks.

level of overconfidence θ_m, with $0 \leq \theta_m < 1$.[5] Each manager m is matched to one bank i. Manager types m are observable to banks. A manager m employed in bank i chooses the risk level q_{im} of the bank's operations, the same in all the n divisions of bank i. We assume that the outcomes of a division's operations are exogenously fixed in each of three different states of the world, with the risk-taking choice q_{im} affecting the probabilities that each state occurs (Besley and Ghatak, 2013). Higher risk-taking increases the probability of a high outcome, but also increases the probability of the low state, leading to bank default. Overconfident CEOs overestimate their ability to run a bank and thus overestimate the likelihood of a high outcome, while underestimating the probability of failure.

Specifically, in the high state the exogenous return to a representative division of each bank is $R + X$, with $R, X > 0$, whereas in the intermediate state it is R. Each bank incentivizes its manager by paying her a bonus in the high state.[6] In the low state the bank defaults and has default costs $\gamma\theta_m$. These default costs are rising in the overconfidence level of the bank's manager, for example because overconfident managers take fewer precautions and this leads to higher costs to the bank when hit by an adverse event. This model element corresponds to empirical evidence that overconfident managers do worse in crisis years, as shown by Ho et al. (2016).

In the low state, the representative division of a bank receives a negative payoff. We assume that the outcomes of all divisions in a given bank i are perfectly correlated and hence the probability of failure of an individual division is the same as the probability of failure of the entire bank. If a bank defaults, there is a probability v_i that the bank is bailed out by the government.[7] The probability of a bailout $v_i(n_i)$ is rising in the size of the bank. This corresponds to the "too-big-to-fail" phenomenon that governments are more likely to save large banks, in order to avoid the potentially severe consequences for other banks and firms that are associated with large banks' defaults. We will provide further evidence for the positive relationship between bank size and bailout probability in the empirical analysis below. From the banks' perspective, the expected payoff in the low state is then $-\gamma\theta_m(1 - v_i)$; hence shareholders' liability for the losses in the low state is limited by the factor $(1 - v_i)$. The shareholders of each bank correctly evaluate the true probability of each state, in contrast to managers. The state-dependent payoffs for each division and the associated probabilities, both the true ones and the ones perceived by overconfident agents, are summarized in Table 1.

As Table 1 shows, the rate at which higher risk-taking increases the likelihood of the high state is parameterized by $1 > \beta > 0$. For any given level of β, the likelihood of

[5] The restriction $\theta_m < 1$ ensures that the bank's optimal bonus choice is well defined; see Section 2.2.

[6] It is straightforward to show that banks will not find it optimal in our setting to pay a bonus in the intermediate state.

[7] These bailouts are exogenous in the present analysis. For models in which optimal bailouts are endogenized, see Niepmann and Schmidt-Eisenlohr (2013), or Haufler (2021).

state	payoff	true probability (shareholders)	perceived probability (managers)
high	$R + X$	βq_{im}	$\beta q_{im}(1+\theta_m)$
medium	R	$(1 - q_{im})$	$(1 - q_{im})$
low	$-\gamma\theta_m[1 - v_i(n)]$	$q_{im}(1-\beta)$	$q_{im}[1 - \beta(1+\theta_m)]$

Table 1: Bank division payoffs in different states

the high state and the likelihood of the low state *both* increase with the manager's risk-taking q_{im}, while the likelihood of the medium state falls by the corresponding amount. Overconfident individuals of type θ_m perceive a higher effectiveness of their risk-taking on the likelihood of a high outcome, as given by the factor $(1 + \theta_m)$, and simultaneously underestimate the probability of default. To ensure that all probabilities are positive, we restrict parameters such that $q_{im} < 1$ for all managers in all banks, and $\beta(1+\theta_m) < 1$ for all manager types m.

Banks compensate and incentivize their managers with a combination of a bonus payment, payable only in the event of a high state, and a fixed payment. Banks are unable to punish managers in the event of a low state. We account for different bank sizes by assuming that a manager (CEO) who runs a bank with n divisions receives n times the bonus payment and n times the fixed wage of a CEO who runs a bank with only one division. This ensures that the share of incentive payments in total pay is structurally (i.e., for fixed θ_m) the same for all CEOs in our model, consistent with the empirical evidence presented in Edmans et al. (2009). Hence the total earnings of managers are increasing linearly in the size of the bank that they run. Finally, we assume that there are as many banks as there are managers, so that the market for bank managers clears. The matching process is perfect and frictionless and matches are formed so as to maximize joint output (Gabaix and Landier, 2008; Bandiera et al., 2015).

We consider a three-stage game. In the first stage, banks of different size, and accordingly with different bailout probabilities v_i, are matched with managers with different degrees of overconfidence. In the second stage, bank shareholders incentivize CEOs by choosing the combination of bonus pay and fixed salary so as to maximize bank profits. In the third stage, overconfident managers choose the risk level q_{im} to maximize their perceived expected utility. We solve the game by backward induction.

2.2 Theoretical analysis

2.2.1 Stage 3: Risk choice

We start with analyzing managers' level of risk-taking, q_{im}. Our model setup ensures that we can analyze all decisions at the level of a representative bank division, which can be part of a larger or a smaller bank i. Decisions at the division level are affected only by the overconfidence level θ_m of the bank's CEO, not by the organizational structure of the bank.

In stage 3, a manager of type m chooses the same risk level q_{im} in each division of their bank, based on their private costs and benefits. The manager's monetary reward comes from their total compensation *per division*, consisting of the fixed payment F_{im} and the expected bonus payment z_{im}.[8] While the true likelihood of receiving the bonus is βq_{im}, a manager of type m perceives a probability of receiving the bonus of $\beta q_{im}(1+\theta_m)$ (see Table 1), and makes its risk choice based on this perceived probability.

At the same time, risk-taking involves psychological costs to the manager, which arise from seeking out risk-taking opportunities above a 'natural' or benchmark level that is here normalized to zero (see Besley and Ghatak, 2013). We disregard individual-specific differences in these psychological costs, as they are closely linked to the CEO's degree of overconfidence, which we capture by the continuum of overconfidence levels θ_m. However, there are general factors that drive up these costs for all managers. The first is that the cost of risk-taking is generally higher in periods of crisis, because the negative consequences of a low outcome tend to be higher, both at the personal and the firm level.[9] Moreover, crises lead to sudden increases in the uncertainty associated with a bank's asset returns (Flannery et al., 2013), which also raise the costs of risk-taking. We label this crisis cost component $\mu^C > 0$.

A second factor is regulation. Strict bank regulation increases the cost of capital for the bank, and particularly so for risky investments. This will affect bank CEOs by lower bonuses and other forms of incentive pay (see below). But higher risk-weighted capital costs for the bank will also affect the manager beyond the share of incentive pay, for example through longer-term career prospects (Eufinger and Gill, 2017). Furthermore, regulation in the form of closer risk monitoring of banks increases the CEO's costs of risk-taking directly; see Saunders and Song (2018) for related evidence. Therefore, there is a regulation component that affects the costs of CEO risk-taking, which we label by $\mu^R > 0$. For analytical tractability, we assume that the manager's cost function for risk-taking is quadratic and given by $(\mu^C + \mu^R)q_{im}^2/2$.

Managers maximize their *perceived* utility per division, which is denoted by $\hat{u}$. Utility per division is the difference between perceived monetary rewards and psychological costs (in monetary equivalents). An important assumption in our setting is that managers differ only in their degree of optimism regarding the likelihood of a high outcome, but neither in their abilities, nor their psychological costs of risk-taking or their reservation utilities. These assumptions are backed by recent empirical evidence. Kaplan et al. (2022) analyze

[8] Hence the total compensation of a manager of a bank with n divisions is $n(z_{im} + F_{im})$.

[9] See Guiso et al. (2018) for econometric evidence from the 2008 financial crisis, and Cohn et al. (2015) for evidence from a lab experiment with financial professionals.

a set of detailed personality assessments for top managers and find that they score worse on several ability-related characteristics, but are overly optimistic with respect to the returns of their investments. Similarly, Chapman et al. (2023) carry out an incentivized survey of the U.S. population and find, among other results, that overconfidence is negatively related to cognitive ability. These findings suggests that overconfident behavior is indeed more closely associated with overestimating the success probability of a given contract, rather than with overstating one's ability or having a higher reservation utility.

To neutralize the effect of bank size n on the manager's risk decision, we assume that both the psychological costs of risk-taking and the monetary rewards to it are multiplied by the number of bank divisions. A manager of type m therefore maximizes his (perceived) utility per division

$$\hat{u}_{im} = \beta q_{im}(1+\theta_m)z_{im} + F_{im} - \frac{(\mu^C+\mu^R)q_{im}^2}{2}. \tag{1}$$

This leads to the optimal risk-taking choices

$$q_{im}^* = \frac{\beta(1+\theta_m)z_{im}}{(\mu^C+\mu^R)}. \tag{2}$$

Hence, the bonus payment z_{im} incentivizes all managers to take higher risks, in order to benefit from a higher probability of receiving the bonus. This effect is stronger for more overconfident managers, because they overestimate the likelihood of attaining the high state, and hence of obtaining the bonus.

We can also ask under which conditions the effect of overconfidence on the level of risk-taking is strong. Taking the derivative $\partial q_{im}^*/\partial\theta_m$ yields:

$$\frac{\partial q_{im}^*}{\partial\theta_m} = \frac{\beta z_{im}}{(\mu^C+\mu^R)}. \tag{3}$$

Overconfidence has a stronger effect on risk-taking, the higher is the manager's bonus share, and the lower are the managers' costs of risk-taking $(\mu^C+\mu^R)$. We summarize in:

Hypothesis 1: *(i) More overconfident bank managers choose higher levels of risk-taking, for any given level of bonus payment. (ii) The effect of overconfidence on risk-taking is rising in the bonus payment z_{im} and it is weaker in periods of crisis (μ^C is large), or under strict regulation (μ^R is large).*

2.2.2 Stage 2: Banks' contract choices

The shareholders of a bank of type i with manager m maximize the profits of each division. This is the sum of expected returns in the different states of the world, net of all (division-specific) payments to the manager, and net of the bank's cost of capital. We assume that the bank's cost of capital consists of a lump-sum component C^0, and a risk-dependent component cq_{im}. The latter term captures the fact that regulation raises the share of costly equity. Since capital regulation targets risk-weighted assets, the share of equity must rise in proportion to the degree of risk-taking q_{im}, whereas the scalar $c > 0$ measures the strictness of capital regulation. In contrast to the manager, the bank's shareholders use the true probabilities of each state in their maximization problem. Using Table 1 yields each bank's division profit as

$$\pi_{im} = \beta q_{im}\left(R + X - z_{im}\right) + (1 - q_{im})R - (1-\beta)q_{im}\gamma\theta_m[1 - v_i(n)] - F_{im} - C^0 - cq_{im}. \quad (4)$$

The two components of manager compensation, the bonus z_{im} and the fixed salary F_{im} are linked by the division-specific participation constraint of each manager that the perceived utility in (1) must weakly exceed a reservation utility $\bar{u}$, which is the same for all manager types m.[10] This determines the fixed wage per division of manager m as a function of their bonus payment:

$$F_{im} = \bar{u} - \frac{\beta q_{im}(1 + \theta_m)z_{im}}{2}. \quad (5)$$

Equation (5) shows that for any given bonus payment z_{im}, a more overconfident manager (with a higher level of θ_m) will receive a lower fixed wage per division. Even when adding the higher bonus, more overconfident managers receive a lower total expected compensation, when the latter is measured by the true probability of receiving the bonus. The "exploitation" of managers' overoptimism by unbiased shareholders is a general result in the literature on overconfidence (De la Rosa, 2011; Gervais et al., 2011). Empirical evidence for this result is presented by Humphery-Jenner et al. (2016), who show that firms indeed tailor their incentive contracts to individual behavioral traits such as overconfidence. This feature is incorporated in our matching model.

Differentiating the division profits in (4) with respect to z_{im}, and using the manager's optimal risk choice (2) and the fixed wage expression (5), gives a reduced form expression for the optimal bonus payment:

$$z^*_{im} = \frac{\Omega_{im}}{\beta(1 - \theta_m)}, \qquad \Omega_{im} \equiv \beta X + (\beta - 1)R - (1 - \beta)\gamma\theta_m(1 - v_i) - c. \quad (6)$$

All parameters in the term Ω_{im}, which represents the net return to bonus incentives from the shareholders' perspective, are exogenous. We assume that these parameters are such that Ω_{im} is positive for all manager types m. Given that $\theta_m < 1$ (see footnote 5),

[10] Again, the CEO of a bank of size n has the reservation utility $\bar{u}$ for *each* division of their bank. This captures higher opportunity costs of running a larger bank and ensures that their total compensation in equilibrium is $n(z_{im} + F_{im})$; see footnote 8.

all managers are then paid a positive bonus in equilibrium. Higher bailout probabilities $v_i(n)$ reduce the costs of managerial risk-taking from the bank's perspective, and therefore incentivize banks to pay higher bonuses.[11] On the other hand, strict capital regulation (measured by c), raises the costs for banks to induce risk-taking by its managers through the bonus payment. We summarize in:

Hypothesis 2: *Bonus compensation z_{im} is rising in the level of government support v_i, and it is falling in the strictness of capital regulation, c.*

Combining Hypothesis 2 with Hypothesis (1)(ii) then also shows that the relationship between the overconfidence of bank managers and their level of risk-taking will be limited in periods of strict regulation by two effects: the higher costs of risk-taking for the bank, which are directly internalized by the manager [eq. (2)], and the reduced incentive for risk-taking that comes from the lower level of bonus pay [eq. (6)].

We can also ask whether more overconfident managers are always paid a higher bonus? Differentiating (6) with respect to θ_m yields

$$\frac{\partial z_{im}}{\partial \theta_m} = \frac{1}{\beta(1-\theta_m)} \left[\frac{\Omega_{im}}{(1-\theta_m)} - (1-\beta)\gamma(1-v_i) \right]. \tag{7}$$

The first term in the squared bracket on the RHS of (7) is positive under our above assumptions. As more overconfident managers value the bonus more, paying higher bonuses (and reducing the fixed payment) is a way for banks to save on total expected compensation. The second term in the squared bracket is negative, however, as the bonus incentivizes more risk-taking and more overconfident bankers induce higher losses in the low state. Therefore, the bonus payment is rising in the overconfidence level m only when the parameter γ is not too large, so that the higher profits caused by overconfidence in the good state dominate the negative consequences of overconfidence in the case of failure. We will develop a formal condition ensuring that (7) is positive in the next section.

We can now use (2), (5) and (6) in (4) to obtain a reduced-form expression for the division profits of a bank i when hiring a manager of type m:

$$\pi^*_{im} = \frac{\Omega^2_{im}(1+\theta_m)}{2(\mu^C+\mu^R)(1-\theta_m)} + R - \bar{u} \tag{8}$$

The optimized division profits in (8) form the basis for our matching analysis below.

[11] Efing et al. (2015) show for a sample of Austrian, German and Swiss banks that higher incentive pay was significantly correlated with more risk-taking in the period preceding the 2007/08 financial crises. They also find that pre-crisis incentive pay was too high to represent an optimal trade-off between higher yield and risk.

2.2.3 Stage 1: Matching

We first ask how optimal division profits in (8) change with the manager's overconfidence level θ_m. Differentiating gives

$$\frac{\partial \pi_{im}^*}{\partial \theta_m} = \frac{\Omega_{im}\Delta}{(\mu^C+\mu^R)(1-\theta_m)}, \qquad \Delta \equiv \left[\frac{\Omega_{im}}{(1-\theta_m)} - (1+\theta_m)(1-\beta)\gamma(1-v_i)\right]. \tag{9}$$

The ambiguity in (9) is similar, but not identical, to that in equation (7): more overconfident managers receive a lower expected total compensation, as measured by true probabilities, and this increases division profits by the first term in Δ. On the other hand, more overconfident managers cause more damage in the low state; this is the negative second term in Δ.[12] If γ is sufficiently low to ensure that more overconfident managers receive higher bonuses, the first effect in Δ dominates the second and hence more overconfident managers also increase banks' profits.

Using the definition of Ω_{im} in (6), the condition for $\Delta > 0$ in (9) yields

$$\Delta > 0 \quad \Leftrightarrow \quad \gamma < \frac{\beta X + (\beta-1)R}{(1-\beta)(1-v_i)[1+\theta_m(1-\theta_m)]}\,. \tag{10}$$

It is straightforward to show that when condition (10) is met, this is sufficient (but not necessary) for the derivative in (7) to be positive. We summarize in

Hypothesis 3: *If condition* (10) *is met, then more overconfident bank managers (with higher θ_m) receive a higher bonus share z_{im}, and they increase banks' profits.*

Finally, we ask how heterogeneous banks and managers are matched in equilibrium. Larger banks have a higher probability v_i of being bailed out, and this is the only relevant heterogeneity at the division level. Since the number of divisions n of each bank is exogenous, bank size does not directly enter into the matching process (but only indirectly via the size-dependent bailout probability). Matching overconfident managers with larger banks is profitable, if and only if the bank's division profits are supermodular in θ_m and v_i, i.e. the second cross-derivative $\partial^2\pi_{im}^*/(\partial\theta_m\partial v_i)$ is positive:

$$\frac{\partial^2 \pi_{im}^*}{\partial \theta_m \partial v_i} = \frac{(1-\beta)\gamma}{(\mu^C+\mu^R)(1-\theta_m)^2}\left[\theta_m\Delta + \frac{1+\theta_m(1-\theta_m)}{(1-\theta_m)}\,\Omega_{im}\right]. \tag{11}$$

For $0 < \theta_m < 1$, equation (11) is indeed unambiguously positive whenever $\Delta > 0$ holds. This leads to our last hypothesis:

[12] The difference between the second terms in the squared brackets of (7) and (9) arises from the fact that overconfidence must increase division profits *net* of the bonus, and the latter increases with higher overconfidence.

Hypothesis 4: *(i) If condition* (10) *is met, then more overconfident managers are matched in equilibrium with larger banks, which receive a higher level of government support. (ii) This matching process is weaker in periods of crisis (μ^C is large), and under strict regulation (μ^R is large and c is large).*

The result in Hypothesis 4 is easily explained. More overconfident managers have lower expected total compensation costs for all banks, but they also cause larger losses in the low state. Since government protection applies precisely in the low state, it is particularly important for banks that employ highly overconfident managers. Moreover, if the only difference between banks is their size, and if larger banks receive higher government support in equilibrium ("too-big-to-fail"), then matching larger banks with more overconfident managers will be privately profitable in equilibrium. Larger banks will then take more risks than their smaller counterparts for two distinct reasons. First, they hire more overconfident managers, who take more risk even in the absence of other differences (H1). Second, larger banks receive more government support, which increases bonus compensation from H2, and this in turn further incentivizes risk-taking (H3).

Note that the matching equilibrium that arises in our model differs in several respects from standard matching models with heterogeneous workers and firms. Standard models match more productive workers with larger and more productive firms, which pay them higher wages in equilibrium (e.g. Gabaix and Landier, 2008; Eeckhout and Kircher, 2018). Moreover, more talented workers receive a higher share of incentive pay (Bandiera et al., 2015). In our framework, more overconfident workers are not generally more productive in (true) expected value terms, and for given levels of government support.[13] However, they receive a higher share of their total compensation as incentive pay. At the same time, all banks are equally productive at the division level in our setting, and the only difference lies in varying levels of (implicit) government support, which favors large firms. In this sense, large firms that are more profitable post-transfer (and only post-transfer) are matched with managers that are not more productive, but have higher risk-taking incentives.

As stated in part (*ii*) of Hypothesis 4, the matching process is again weakened by crises and strict regulation. This occurs through both the managers' costs of risk-taking ($\mu^C + \mu^R$), and through the higher capital cost parameter c (which is incorporated in Ω). More generally, it is important to recall that our entire matching analysis is based on overconfident managers choosing their optimal level of risk-taking [eq. (2)]. If risk-taking choices are constrained, as they are in periods of crisis or strict regulation, then we cannot expect much empirical support for Hypotheses 1-4.

[13] This is in line with the findings of Kaplan et al. (2022) and Chapman et al. (2023) that overconfidence, as conventionally measured, is negatively related to managerial ability.

3 Empirical analysis

In this section, we take the theoretical hypotheses derived in the previous section to the data and test whether we can observe the predicted relationships.

3.1 Data

We use detailed data for the U.S. financial sector for the years 2000 to 2022. Balance sheet data for U.S. financial institutions is obtained via *Compustat Annual North America*. We only keep financial institutions headquartered in the U.S. and drop all firms for which we observe gaps in the data. We want to focus on the traditional financial sector and, thus, only keep financial institutions with Standard Industrial Code (SIC) 6000 to 6282, excluding classification 6282. This leaves us with 1.629 firms and 20.067 firm-year observations. We complement this data with executive compensation data from *Execucomp Annual Compensation* and data on daily stock returns from the *Center for Research in Security Prices (CRSP)*. Keeping only observations that are present in all three databases, we arrive at a base sample of 295 firms and 3.994 firm-year observations.

Further, we only keep observations where the fiscal year coincides with the calendar year to avoid that timing issues affect the results. We drop government-sponsored enterprises, since these are inherently different in nature from the remaining firms. Last, we only keep firms for which we have at least three observations in the data. The final sample amounts to 3.572 firm-year observations from 249 distinct firms.

3.2 Variables

3.2.1 Overconfidence

The first main variable in our empirical analysis is managerial overconfidence. The literature proposes different measures of overconfidence. However, the revealed-beliefs approach using the option exercising behavior of managers, first introduced by Malmendier and Tate (2005), has become standard. The idea behind the option-based approach can be sketched as follows. Through their contract, the value of the CEO's human capital is tied to the firm. Since they are usually contractually detained from taking short positions with respect to the firm, CEOs have limited possibilities to address this under-diversification. To diversify, rational and risk-averse CEOs should exercise stock options, which they receive as part of their compensation package, as soon as their value exceeds a certain, rational benchmark. Therefore, if CEOs do *not* diversify their risks in this way, this may signal that they hold overly optimistic beliefs about their firm's success, and hence about their ability as a CEO.

We consider a CEO as overconfident when they postpone to exercise exercisable options that were at least 100% 'in the money', i.e., the stock price is at least twice as high as the strike price. Using 100% as the cutoff follows previous literature and ensures that only highly overconfident CEOs are classified as overconfident (Campbell et al., 2011).[14] To not capture inattentive behavior, this behavior has to be observed at least twice during tenure. The CEO is then classified as overconfident for the entire period of tenure, to capture the underlying behavioral trait.[15]

By measuring overconfidence of CEOs in this way, we assume that overconfidence is a latent behavioral trait rather than a characteristic that fluctuates with external circumstances, such as the hiring into a systemically important bank. This assumption is backed by previous research. Graham et al. (2013), for example, argue that managerial traits, including overconfidence, are deeply ingrained and can be traced to personal experiences and psychological predispositions rather than situational changes. While the external environment may influence managerial decision-making to some degree, fundamental behavioral biases such as overconfidence are more likely to be a product of stable personality traits (e.g., Deaves et al., 2010).[16]

Malmendier and Tate (2005, 2008) also discuss alternative explanations for the observed late-exercising of options, but conclude that overconfidence is the most consistent explanation. Moreover, a high correlation between the option-based measure and a press-based measure of overconfidence, which classifies CEOs according to their portrayal in the press, also supports the measure (Hirshleifer et al., 2012). Finally, Kaplan et al. (2022) have more recently used detailed personality assessments of CEOs, which provide evidence that the option-based measure indeed reflects overconfident behavior. Taken together, these results provide strong evidence that late exercising behavior is a good proxy for CEO overconfidence.

[14] Since we only have aggregate data available for the option portfolios of CEOs prior to 2006, we follow earlier studies in constructing the overconfidence measure based on the average degree of moneyness of the CEO's option portfolio in a given year (see Ho et al., 2016; Campbell et al., 2011).

[15] If a CEO switches between firms in the observed period, all tenures are taken into account. The average number of years in which we observe CEOs in the sample is 10.4 years (interquartile range 8 years). The average value of exercisable, unexercised options is \$8.1 mn (median \$0.2 mn), while the average value of realized option exercise is \$2.2 mn (median \$0 mn) and the average value of unexercisable, unexercised options is \$1.9 mn (median \$0 mn). Observations with zero options or a value of exercisable, unexercised options of zero are treated as non-overconfident. In contrast, observations where the realizable value per option equals the fiscal year-end stock price, which implies a strike price of zero, are treated as overconfident.

[16] While there exists evidence that previous success affects the degree of overconfidence through, for example, the self-attribution bias (e.g., Billet and Qian, 2008), there is limited evidence in practice that this is true for CEOs in general (see, e.g., Schumacher et al., 2020). Moreover, it might be the case that inherent overconfidence accumulates over the career cycle. However, it is likely that this inherent trait is already fully developed in the case of CEOs (see, e.g., Malmendier and Tate, 2011).

3.2.2 Bailout probability

Our second core variable is the bailout probability of a financial institution. Following our theoretical model, this bailout probability is connected to the bank's systemic importance. Bailout probabilities are determined by government guarantees, which usually are implicit and therefore not directly observable. To construct a proxy for such implicit guarantees we use the U.S. Global Systematically Important Banks (G-SIB) scores, which assess the degree of systemic importance of financial institutions in the U.S. and are based on data from *FR Y-15 Snapshots Reports*.[17] All banks with more than $50 billion in assets are required to file these reports, which contain five main risk categories: size, interconnectedness, complexity, cross-jurisdictional activity, and short-term wholesale-funding. These categories encompass a total of ten risk variables that are used to construct the G-SIB scores. The underlying assumption for using these scores in our analysis is that a financial institution with a higher systemic score also enjoys larger implicit guarantees, and therefore has a higher bailout probability.

These systemic scores are available for the years 2017 to 2022 and for the 24 largest financial institutions in our sample, yielding 112 firm-year observations. Given this low number of directly observed scores, and given that we cannot construct these scores due to lack of data, we have to extrapolate these scores based on other observations in our data. While the underlying measures are not collected from smaller banks, it is reasonable to assume that they are closely linked to the balance sheet data available to us, given that they encompass the same elements. Hence, we use the correlation between the observed scores and the available balance sheet data to extrapolate the G-SIB scores to the remaining banks in our sample. To trace out the variables that are predictive of the observed scores, we run a LASSO on the entire balance sheet data and pick those variables that have the highest explanatory power in the sample for which we observe systemic scores.[18]

Running the LASSO on the subsample with the observed systemic scores yields 40 predictive variables and an adjusted R-squared of .9958.[19] While the LASSO yields all

[17] See https://www.financialresearch.gov/bank-systemic-risk-monitor.

[18] LASSO is a shrinkage estimator with the objective to choose those variables with the highest predictive power for the dependent variable from the set of all possible control variables. It does so by estimating a penalized regression, which minimizes the sum of squared residuals and a penalty term for the sum of the coefficients. We implement this via cross-validation, i.e., the estimator partitions the data into different folds of training and testing data and selects the penalty term that minimizes the out-of sample prediction error in the testing data.

[19] The cross-validation of the model is performed on 10 folds with an optimal penalty parameter of 2.28. We note that the relatively small sample size can be a limitation and may increase variability in cross-validated tuning parameter selection. However, penalized regression methods such as LASSO are specifically designed for settings where overfitting is a concern, and regularization can improve out-of-sample performance relative to unpenalized models. To mitigate instability, we used cross-validation to select the penalty parameter. Moreover, in a further step, we restrict the extrapolation to the significant predictors only and in a further robustness test to the ten most significant variables. The post-LASSO regressions

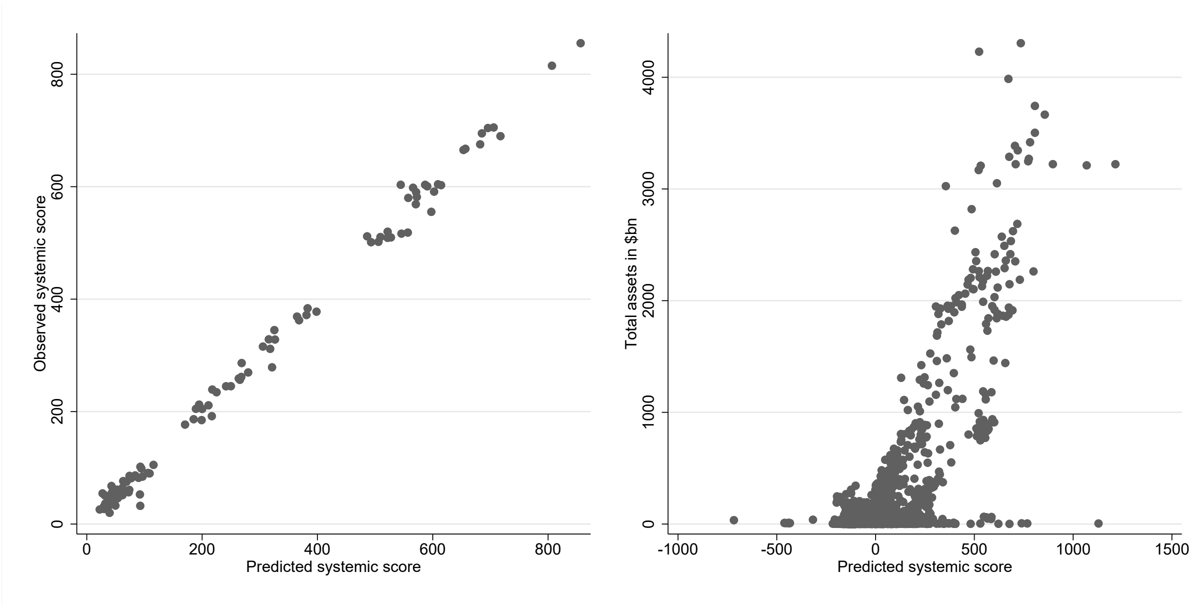

Figure 1: Systemic scores for financial institutions.

The left panel shows the correlation between the systemic scores observed in the data and the systemic scores as predicted by the LASSO model. The right panel shows the correlation between total assets and the predicted systemic scores in the full sample.

remaining variables with a non-zero coefficient, it also yields statistically non-significant coefficients. We exclude all statistically insignificant coefficients and obtain a set of 18 predictive balance sheet variables (see column (2) in Table A.1 in the appendix). These remaining variables are closely related to the risk variables that are used to construct the G-SIB scores.

In the next step we predict the systemic scores for all other observations in our sample, based on the model in column (2) of Table A.1 in the appendix. The results are shown in Figure 1. The left panel shows the fit between our predicted scores and the actually observed scores from the Federal Reserve, with an R-squared of .9943. The right panel of Figure 1 shows the relationship between the size of banks (total assets) and our predicted scores for the entire sample. Even though size is not among the variables used to predict the systemic score, the graph shows a clear, positive correlation between the systemic score and the size of financial institutions and, thus, their systemic importance.

We have thus derived an indicator for the systemic importance of a financial institution, and hence for the degree of implicit guarantees this institution receives. To test the assumption that these systemic scores are positively correlated with bailout probabilities, we follow Gropp et al. (2011) and use Fitch stand-alone and overall credit ratings to calculate a proxy for the bailout probability that is reflected by the ratings. For this we transform the rating notches into default probabilities using the transition matrix for

confirm a high explanatory power of predictors.

Logistic regression			Number of obs	=	104	
			Wald chi2(1)	=	14.70	
			Prob > chi2	=	0.0001	
Log pseudolikelihood = -48.967627			Pseudo R2	=	0.1778	

bailout probability	Coef.	Rob. Std. Err.	z	$P > \|z\|$	95% Conf. Interval	
predicted systemic score	.00454	.00118	3.83	0.000	.00222	.00686
constant	-2.525	.425	-5.95	0.000	-3.357	-1.693

Table 2: Predicted systemic scores and bailout probabilities

This table presents the results of regressing the bailout probability *pr* on the predicted systemic score using a logistic regression in the subsample of banks for which we observe the actual systemic score. Heteroscedasticity-robust standard errors are used.

the U.S. non-financial sector provided by Fitch.[20] A lower rating on the rating scale corresponds to a higher realized historic default rate. If the default probability for the stand-alone rating is higher than the default probability for the overall rating, which takes into account potential external support, this indicates a positive probability that this institutions receives external aid. We calculate the implicit bailout probability according to the formula in Gropp et al. (2011).[21] We then regress this measure for the bailout probability on the predicted systemic scores in a logistic regression.[22] Table 2 shows that the systemic scores are highly positively correlated with the bailout probability. This confirms our underlying assumption that systemically important institutions can expect a higher level of (implicit or explicit) government support.

In a last step, we transform the predicted systemic scores into bailout probabilities using the coefficient from the logistic regression in Table 2.[23] This is essentially a logistic-transformation of the systemic scores, which ensures that they are bound between zero and one. This has the advantage that we do not need to assume a linear relationship between the systemic scores and the bailout probability in the subsequent analysis. This follows

[20]We use the transition matrix for non-financial firms since the default probabilities of financial firms would themselves be affected by the bailout probability of financial institutions. Thus, the observed default probabilities in the transition matrices for financial firms are underestimated.

[21]In principle, this measure could be directly used for the bailout probability as explanatory variable. However, the data overlap between the Fitch data and our *Compustat* sample is too limited, forcing us to map these bailout probabilities to the extrapolated systemic scores.

[22]We restrict this regression analysis to the firm-year observations for which both Fitch credit ratings and systemic scores from the Federal Reserve are available (104 observations). This avoids that measurement errors in the predicted scores affect our analysis.

[23]Note that this assumes a time-invariant relationship between systemic scores and observed bailout probabilities and maps their relationship in the period 2017-2022 to the entire observation period. In Section 3.4 we assess the sensitivity of our results regarding this assumption by relaxing this constraint and testing different time-varying mappings.

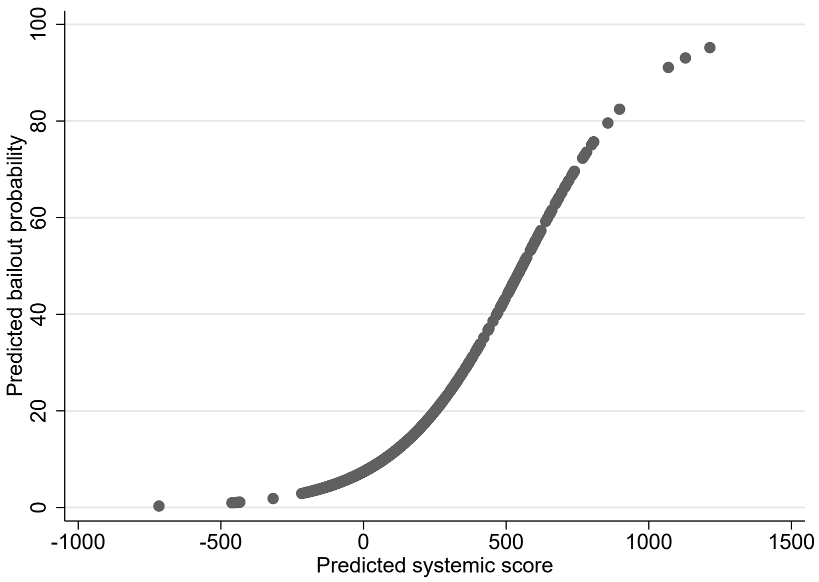

Figure 2: Predicted systemic scores and predicted bailout probabilities

This figure presents the mapping that arises from the regression in Table 2 that transforms the predicted systemic scores in a bailout probability between 0 and 100%.

the observation that banks with medium to low scores have a bailout probability close to zero, whereas the bailout probability of banks with medium to high scores approaches one.[24] Figure 2 shows this non-linear relationship.

3.2.3 Incentive compensation

The third main measure in our analysis is incentive compensation. *Execucomp* offers detailed data on incentive payments and other salary components. These are, however, ex-post observations of realized values that depend on many factors such as firm performance, the achievement of personal goals, or other circumstances. Hence they do not reflect the true incentive component that was contractually agreed upon when hiring the CEO. The latter data is confidential, and is therefore not observable. Nevertheless, we can use the observed realized values of incentive pay to construct a measure for the incentive component that is independent of the performance of the firm. We argue that this is good proxy for the incentive component in the actual hiring contract.

To do so, we regress the observed incentive payment in year t for a CEO at bank i on a vector of variables that are related to firm performance. We generally follow Humphery-Jenner et al. (2016) in their choice of explanatory variables, as far as they are available to us, focussing on financial sector variables. The resulting variables are the age of the CEO,

[24]Since we can only proxy the bailout probability, we cannot rule out that measurement bias attenuates the coefficients in our empirical model towards zero. Given the data constraints, we nevertheless argue that our measure is the best available alternative.

Group	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
0 (non-overconfident)	1,753	0.885	0.034	1.433	0.818	0.953
1 (overconfident)	862	1.219	0.101	2.970	1.020	1.417
combined	2,615	0.995	0.041	2.075	0.916	1.075
diff		-0.333	0.086		-0.502	-0.165

diff = mean(0) - mean(1) t = -3.873
H0: diff =0 degrees of freedom = 2613

Ha: diff < 0 Ha: diff ! = 0 Ha: diff > 0
$Pr(T < t) = 0.0001$ $Pr(|T| > |t|) = 0.0001$ $Prob(T > t) = 0.9999$

Table 3: Excessive incentive compensation for overconfident / non-overconfident CEOs

This table presents the results from a t-test on the difference in the excessive incentive compensation component between overconfident and non-overconfident CEOs. The excessive compensation component is calculated as observed incentive payment over predicted incentive payment. This prediction is based on the relationship between the incentive component and various variables related to firm performance.

the ownership share of the CEO, the bank's book leverage, annualized stock returns, the volatility of daily stock returns, cash-flow, the liquidity ratio, and the size of the executive board. We also include the lags of all performance-related variables as well as year fixed effects. The dependent variable is the sum of all non-equity incentive components recorded in *Execucomp*: these are bonus payments, long-term incentive payments, and other non-equity performance pay (see Table A.2 in the appendix for precise definitions).

From this regression, we then calculate the predicted incentive component. This is the incentive compensation that would have been paid, on average, for the performance parameters in the entire sample of financial institutions, taking into account time trends in wage setting. We then calculate the ratio of the observed realized incentive compensation over this predicted incentive compensation to obtain a measure of excessive incentive compensation. If this measure is above one, the CEO received a higher incentive compensation in this year, as compared to what would have been paid on average for the combination of performance parameters.[25]

Table 3 compares this measure of excessive compensation for overconfident (Group 1) and non-overconfident CEOs (Group 0). The measure is below one for the latter group, which suggests that contracts for non-overconfident CEOs do not have above-average incentive components. In contrast, the measure is above 1.2 for overconfident CEOs (Group 1), indicating an excess incentive component for this group. Since we are ultimately interested in the year of hiring the CEO, and to reduce noise, we take the

[25]Note that the comparison value is not the average of actual levels of incentive compensation, but only of that part which can be explained systematically by the performance variables. See Correa and Lel (2016) for a similar procedure in the context of excessive CEO pay.

average across all observations of the CEO within each tenure as our measure of the incentive component.[26]

We argue that this component reflects the incentive share that was contractually agreed upon, which is the central component in our theoretical model. In the model, the bonus z_{im} represents an additional payment the manager receives in the high state. Our empirical question is therefore whether, conditional on a given state (i.e., high state), overconfident managers receive higher bonuses. In practice, however, this part of the compensation is not a discrete state-contingent payment but a continuously adjusted and multi-dimensional component of total remuneration. It depends on a range of performance measures and is typically paid out in a more granular and heterogeneous manner. Therefore, we are interested in whether, conditional on the same realized performance of a financial institution, overconfident managers receive systematically higher incentives. Empirically, this corresponds to isolating the residual component of incentive compensation after accounting for observable performance determinants. Our measure is constructed effectively as the residual from an industry-wide regression of the incentive component on a set of performance variables, aggregated at the CEO level.[27]

3.2.4 Control variables

We include further control variables in the main analysis in Section 3.3. When estimating the relationship between overconfidence and the matching of CEOs to financial institutions, we use the age of the CEO, the bank's book leverage, the market-to-book ratio, annualized stock returns, the volatility of daily stock returns, cash-flow, the liquidity ratio, and the size of the executive board. These variables are similar to the vector of controls used to construct the measure of the incentive component. However, we do not include the ownership share of the CEO, as we are focusing on newly hired CEOs. When estimating the effect of overconfidence on risk-taking, we use a different vector of controls, following previous econometric models in the literature. Here we additionally include size (logarithm of total assets), return on assets, the deposit ratio, and the year-end stock price, but we do not include the CEO's age, the bank's cash-flow, the volatility of the daily stock returns, and annualized returns. Table A.3 in the appendix shows which controls are employed in the tests of the different hypotheses.

Table 4 presents the descriptive statistics of the main variables used in the analysis. It shows that around one third of the observations relate to an overconfident CEO. Moreover,

[26] Before taking the average, we winsorize the annual values at the 1 and 99 percentile to avoid that outliers influence the results.

[27] An alternative approach would be to use CEO-level pay-performance sensitivity as a proxy for incentives. We implement this robustness test in Section 3.4 and find that the results are qualitatively unchanged. The disadvantage of the alternative measure is that it requires individual-level regressions, and hence a sufficient number of observations per CEO. Our approach relies instead on an industry-wide regression and aggregates the residual components over all CEOs.

	mean	sd	p25	p50	p75	count
overconfidence	0.329	0.470	0.000	0.000	1.000	3572
volatility	0.023	0.014	0.015	0.019	0.026	3495
size	9.469	1.782	8.426	9.247	10.442	3572
return on assets	0.013	0.129	0.007	0.011	0.015	3568
book leverage	9.813	8.440	7.387	9.257	11.345	3535
market-to-book	2.210	7.014	1.074	1.489	2.156	3494
liquidity ratio	0.096	0.130	0.025	0.046	0.106	3572
deposit ratio	0.614	0.281	0.577	0.725	0.801	3391
size of exec. board	5.609	1.135	5.000	5.000	6.000	3572
incentive compensation	1296.158	1979.449	199.946	664.572	1540.000	3572
annualized returns	0.128	0.374	-0.030	0.132	0.311	3495
cash-flow	0.088	3.088	0.043	0.076	0.127	3076
ownership share of CEO	0.019	0.124	0.001	0.004	0.011	3449

Table 4: Descriptive statistics

This table shows the descriptive statistics of the variables used in the analysis. The data is from *Compustat*, *Execucomp*, and *CRSP*.

incentive payments vary widely across CEOs, with the $p75/p25$ ratio being close to 8.

To conclude our description of the data, Figure 3 summarizes the development over time for four of our core variables. These trends reveal several interesting patterns. The upper left panel shows that the share of overconfident CEOs has significantly fallen during the last two decades, from close to 40% in the early 2000s before the financial crisis to around 30% in the period after the financial crisis. The increase in the first years of the sample and the decrease in the last years are caused by a sample effect, as we only observe CEOs in this period for a shorter period of time. As such, these CEOs are less likely to be classified as overconfident under our approach (see section 3.2.1).[28] This sample effect can, however, not explain the drop in the share of overconfident CEOs in the years immediately following the financial crisis.

The upper right panel shows that the average systemic importance of the financial institutions in the U.S. financial sector has simultaneously increased during this period, in response to the financial crisis and to the consolidation of financial institutions that followed it. The lower two panels show the development of the average incentive payment on the left, and of the average fixed salary on the right. Both took a dip in the financial crisis, with the decline being more pronounced for incentive pay. After the crisis, both types of compensation rose back up and meanwhile exceed their pre-2007 levels. However, the ratio of incentive pay to fixed pay remains lower at the end of our sample period, as compared to the pre-crisis period in the early 2000s.

[28]In Section 3.4 we test whether these 'short-tenure' CEOs affect our results.

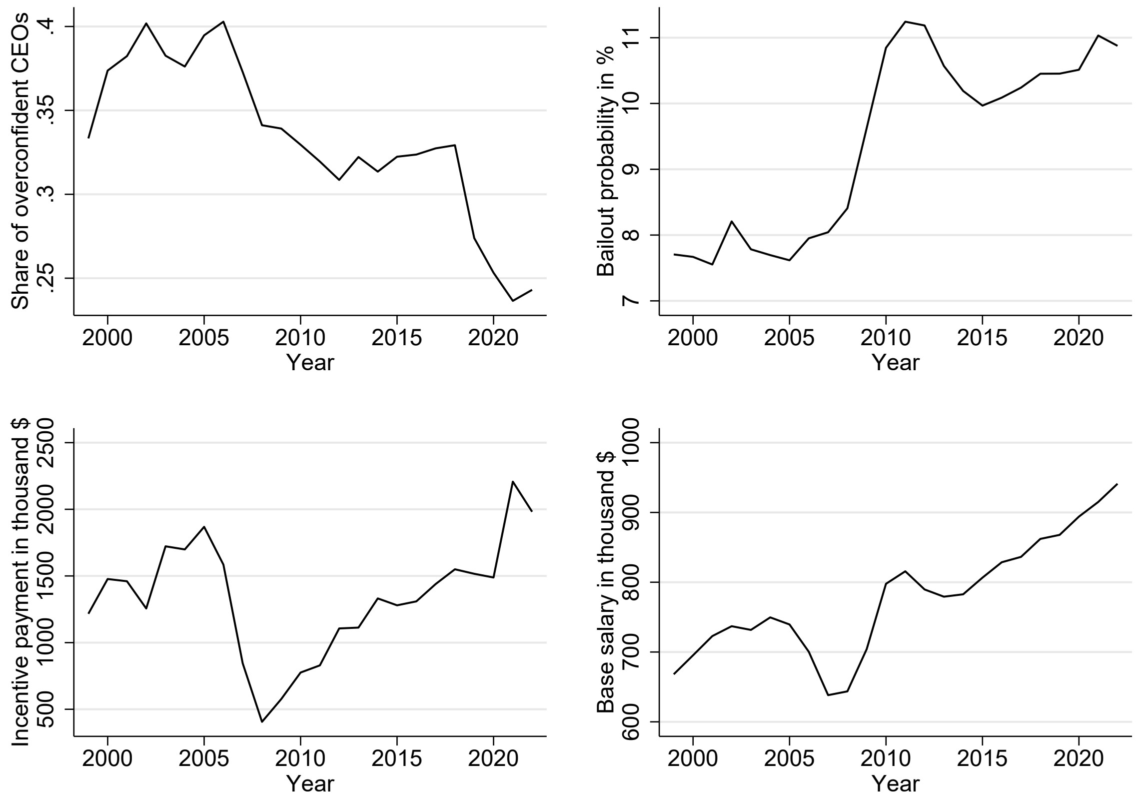

Figure 3: Development of core variables over time

This figure shows the development of the share of overconfident CEOs, the bailout probability, the incentive payments, and the base salary over time.

3.3 Testing the hypotheses

In the following, we will test the theoretical hypotheses developed in Section 2.

3.3.1 Overconfidence and risk-taking

The theoretical model rests on the important assumption that overconfident CEOs increase risk-taking at financial institutions for any given level of incentive compensation (Hypothesis 1). There have been numerous studies in the literature that examine the relationship between CEO overconfidence and risk-taking. For example Ho et al. (2016) show that financial institutions with overconfident CEOs took higher risks before financial crises and performed worse during financial crises. Kassner (2023) examines the relationship between CEO overconfidence and risk-taking in a dynamic setting. He finds that overconfident CEOs took more risk than non-overconfident CEOs in the period before the 2007-2009 financial crises, behaved similarly to non-overconfident CEOs in the post-crisis period of strict regulation, and again took more risk than non-overconfident CEOs once

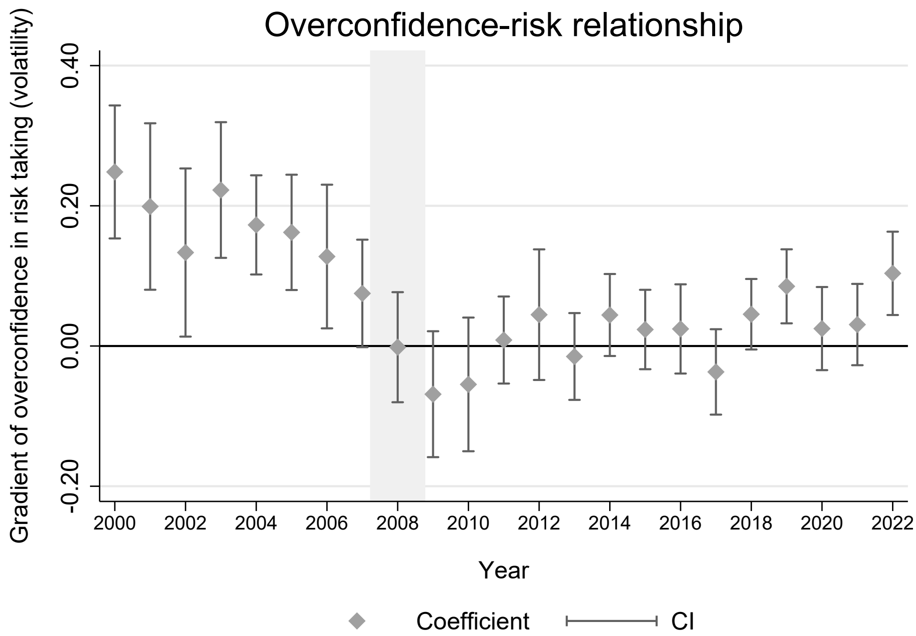

Figure 4: Relationship between overconfidence and risk

This figure shows the gradient of overconfidence in the regression of risk (standard deviation of daily stock returns) on the overconfidence dummy interacted with the year dummies. We include a vector of control variables that is composed of size (logarithm of total assets), return on assets, book leverage, the market-to-book ratio, the liquidity ratio, the size of the executive board, the deposit ratio, and the year-end stock price, as well as firm and year fixed effects. Standard errors are clustered at the firm level. The sample size is N=2,921.

regulation was relaxed after 2018.[29] For the purposes of our matching analysis, we use a similar econometric model as in Kassner (2023), but additionally include the incentive component to control for the level of incentive compensation.

As the primary measure of risk, we use the daily stock return volatility (σ_t), a widely accepted measure within the financial literature.[30] Stock return volatility is particularly suitable for capturing firm-level risk because a company's stock price represents a call option on its underlying assets. As such, volatility in returns (σ_t) reflects the inherent uncertainty and risk associated with those assets. Additionally, beyond capturing fluctuations in the firm's equity, stock return volatility also reflects the market's sensitivity to firm-specific news, such as expectations about future profitability and growth, which are crucial for shareholders (see, e.g., Aabo et al., 2020; Bulan, 2005; Leahy and Whited, 1996). The literature further suggests that stock return volatility has forward-looking characteristics: changes in expected future returns from a firm's assets and growth opportunities are often embedded in variations in stock prices. Because common stock repre-

[29]Similarly, Banerjee et al. (2015) show that economy-wide regulation through the Sarbanes-Oxley Act in 2002 significantly disciplined the behavior of overconfident CEOs across sectors.

[30]These are only calculated if more than 10 daily observations are available within the fiscal year. If multiple securities are assigned to a firm, only the primary security is used.

sents claims on future profits, market reactions to information about future profitability and outlook are embedded in stock price volatility, capturing risk as priced by the market (Berk et al., 1999). Due to the skewed distribution, we use the natural logarithm of the standard deviation of daily stock returns within fiscal year t.

We regress the standard deviation of daily stock returns, i.e., the volatility of the firm, on overconfidence interacted with year dummies along with a vector of controls and firm and year fixed effects using OLS. Standard errors are clustered at the firm level.[31] The coefficients of the interactions between the year dummies and the overconfidence dummy yield the gradient of overconfidence, that is, the additional volatility at firms with overconfident CEOs. Figure 4 presents these gradients of CEO overconfidence on banks' risk-taking, as measured by the standard deviation of daily stock returns in a given year, for each year in our sample period 2000-2022. The results show that there was a statistically significant correlation between CEO overconfidence and risk in the pre-crisis years 2000-2006. However, this relationship is not different from zero during the Great Financial Crisis (2007-2009), and during periods of stricter financial regulation (Dodd-Frank Act 2010-2017). With deregulation starting in 2018, the positive relationship between risk and overconfidence reappears, with a notable interruption in the years of the Covid Crisis (2020-2021). These results correspond to the expectations from our theoretical analysis, which has shown that the effects of overconfidence on risk-taking will be strong only when the managers' costs of risk-taking are limited (Hypothesis 1).

Based on these findings, we define three distinct periods for our subsequent analysis: (*i*) periods with a positive relationship between CEO overconfidence and risk-taking (2000-2006, 2018-2019, 2022); (*ii*) crisis years in which this relationship is zero (2007-2009, 2020-2021); and (*iii*) periods of strict financial regulation in which the relationship is also zero (2010-2017). Column (1) in Table 5 shows the static representation of Figure 4 using an indicator variable representing these three periods. On average, financial institutions with overconfident (OC) CEOs had a 6% higher standard deviation of daily stock returns, and this effect is significant at the 5% level.[32] This confirms Hypothesis 1, but only for non-crisis years that were not characterized by enhanced regulation. We label these 'normal years' in the following. In contrast, the effect of CEO overconfidence falls towards zero in crisis periods, and in periods of strict regulation. This latter finding suggests that tight regulation following the financial crisis has been effective in counteracting the fundamental economic incentives on which our analysis is focused.

[31] In principle, we could also cluster standard errors by both firm and time (Thompson, 2011). However, our sample has fewer time periods than recommended for this approach, implying that double-clustering may lead to false rejection of true hypotheses. Moreover, several of our key regressors, in particular the overconfidence variable, do not vary much over time. In this setting, clustering at the firm level while incorporating time dummies is our preferred choice.

[32] The effect is still significant at the 5% level when influential observations in the data are removed. See Section 3.4 and Table A.10 in the appendix.

	H1	H2		H3		H4
	(1)	(2)	(3)	(4)	(5)	(6)
	Volatility	Incentive	Incentive	Incentive	Incentive	OC
OC_t				1.084***	1.046**	
				(0.363)	(0.453)	
$OC_t \times$ *crisis*				-1.566***	-1.717**	
				(0.575)	(0.670)	
$OC_t \times$ *regulated period*				-1.071**	-1.198**	
				(0.460)	(0.532)	
OC_{t-1}	0.0641**					
	(0.032)					
$OC_{t-1} \times$ *crisis*	-0.0664					
	(0.041)					
$OC_{t-1} \times$ *regulated period*	-0.0692*					
	(0.036)					
pr_{t-1}		0.0416***	0.0432***			0.178**
		(0.014)	(0.014)			(0.089)
$pr_{t-1} \times$ *crisis*		-0.00662	-0.00673			-0.441**
		(0.017)	(0.019)			(0.208)
$pr_{t-1} \times$ *regulated period*		-0.0231	-0.0263*			-0.250***
		(0.015)	(0.015)			(0.093)
Observations	2921	241	221	243	222	180
R^2	0.562	0.161	0.204	0.137	0.167	
$Pseudo - R^2$						0.178
Clusters	219	151	144	151	144	123
Controls	Yes	No	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	firm-level					

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Main results of the analysis

This table presents the main empirical results of the analysis. In column (1) we regress the natural logarithm of the daily stock return volatility on the overconfidence dummy interacted with the three different periods described in Section 3.3.1. In columns (2) and (3) we regress the natural logarithm of the incentive component on the proxy for the bailout probability interacted with the different periods. In columns (4) and (5) we regress the natural logarithm of the incentive component on the overconfidence dummy interacted with the different periods. In column (6) we regress the overconfidence dummy on the proxy for the bailout probability interacted with the three different periods. In all specifications we cluster standard errors at the firm level, which are reported in parentheses. Columns (1) to (5) use OLS, column (6) a logistic regression.

3.3.2 Matching between overconfident CEOs and banks

For our following tests of Hypotheses 2 to 4, we focus only on CEO turnovers. This is because we want to capture the relationship between government protection for banks and CEO overconfidence at the time of hiring a CEO. Hence, we only keep the first year of tenure of each CEO. We further exclude any re-appointments of CEOs as well as 1-year interim positions, which we define as one year tenures with the same CEO before and after, as contracts will arguably differ in these cases. This reduces the sample to 297 observations. Since some of the control variables have missing values, the effective sample will be smaller, depending on the specification of the model. Given this low number of observations, we cannot carry out a dynamic analysis. Therefore, we use the indicator variable representing the three periods differing in the relationship between CEO overconfidence and risk as described above. The results for testing Hypotheses 2 to 4 are summarized in Table 5.

H2: Bonus compensation and government support

Hypothesis 2 states that bonus compensation is rising in the level of government support. This should, however, only be the case when the stakeholders of the bank can assume that risk-taking can be affected via incentives, and therefore in normal years. During periods of strict financial regulation, this may not be the case due to exogenous constraints on risk-taking. Moreover, during such periods the use of incentive compensation might itself be restricted. In the U.S., for example, both the Dodd-Frank Act and the Troubled Asset Relief Programme (TARP) imposed restrictions on bonus payments to CEOs. Therefore, we expect a positive relationship between government support and incentive compensation primarily in 'normal', non-crisis years with moderate regulation.

We test this hypothesis by regressing our measure for the contractual incentive component on our measure of the bailout probability (pr). We interact the bailout probability with the indicator variable reflecting the three periods differing in the relationship between CEO overconfidence and risk-taking. We estimate the model using OLS, with and without a vector of controls. We cluster standard errors at the firm level. For normal years, columns (2) and (3) in Table 5 show a highly significant (1% level) positive relationship between the bailout probability and the incentive component. This positive relationship is still present in crisis years and in years of strict regulation, but it is weaker during the latter periods.[33] In sum, our empirical analysis therefore confirms that government-protected banks do indeed incentivize higher risk-taking by offering contracts with a larger bonus component.

[33]Comparing columns (2) and (3) shows that the R^2 measure increases significantly when controls are added. The most important controls are *liquidity ratio*, *market-to-book ratio*, and *age of CEO*, which are all associated with bonus payments. The same role of controls applies in column (5) that tests H3.

H3: CEO overconfidence and incentive payments

According to Hypothesis 3, banks pay higher incentive compensation if their CEOs are overconfident. Overconfident managers, who overestimate their probability of success, are willing to take higher risks for a given incentive payment, and they accept a lower total compensation by overestimating the expected value of their incentive payment. However, as Hypothesis 3 states, this relationship holds only when the damage caused by overconfident managers in the low state (as parameterized by γ) is not too large. Hence, we expect again that the positive relationship in Hypothesis 3 appears primarily in normal years when bank failure is not a primary concern for the bank's stakeholders and when risk-taking is not constrained by regulation.

We test this hypothesis by regressing our measure for the contractual incentive component on our binary overconfidence variable. We again interact the indicator with the period dummy. We estimate different models including and excluding control variables using OLS and cluster standard errors on the firm level. The results are given in columns (4) and (5) of Table 5. They show that, in normal years, overconfident CEOs are incentivized more than non-overconfident CEOs. This relationship is statistically significant at the 1% level when control variables are absent (column (4)). It remains positive and significant at the 5% level when control variables are added (column (5)). In contrast, the relationship is close to zero in crisis years, or in periods of strict regulation. In sum, Hypothesis 3 is confirmed by our regression analysis.

H4: Matching of overconfident CEOs

Finally, we turn to our main hypothesis that overconfident CEOs are matched, in equilibrium, with government-protected banks (Hypothesis 4). Again, this relationship should only hold in periods where there is a significantly positive relationship between overconfidence and risk-taking by Hypothesis 1. Hence, we expect this assortative matching primarily in normal years, absent crises and years of enhanced regulation.

We test this hypothesis by regressing the binary overconfidence variable on our measure for the bailout probability, interacted with the period indicator in a logistic framework.[34] We include the standard control variables and cluster standard errors on the firm level. For normal years, column (6) in Table 5 shows a positive relationship between the bailout probability of a bank and the overconfidence level of its CEO that is significant at the 5% level. In crisis periods, and in periods of enhanced regulation, the relationship is instead close to zero and the assortative matching does not occur.

Taken together, the results from the empirical analysis support the hypotheses derived from our theoretical model for normal years. They show that banks with larger govern-

[34] We also tested the hypothesis using a linear probability model instead of logit. The results are robust to this change.

ment support, as reflected by a higher bailout probability, incentivize their CEOs more. This attracts overconfident CEOs who overestimate their probability of success and are therefore more likely to accept higher-powered incentive contracts. In equilibrium this in turn leads to an assortative matching between overconfident managers and banks with a larger bailout probability. Importantly, however, these relationships do not hold in crisis periods, and in periods of strict financial regulation. In these periods, risk-taking is either strictly constrained or very costly, thus breaking the link between the overconfidence of managers, and their willingness to take higher risks.

While the empirical analysis supports the hypotheses derived from the theoretical model, our econometric model cannot fully rule out that alternative, and unmodeled, mechanisms may influence the matching of overconfident CEOs. Our baseline model controls for a number of variables that might be correlated with both systemic scores (which underlie our measure of the bailout probability), and the hiring decision of financial institutions. In addition, we examine several alternative explanations in the following.

3.4 Robustness tests

In this section, we assess the robustness of our baseline results in several ways. First, we test alternative hypotheses for our matching mechanism in Hypothesis 4. Overconfident CEOs might sort into larger banks for reasons that are independent of the existence of government guarantees. For example, overconfident managers may be better leaders because their beliefs in their firms' prospects may induce other stakeholders in the firm to also increase their commitment (Phua et al., 2018). In this case the matching between overconfident CEOs and larger banks (with higher bailout probabilities) would be a matter of leadership qualities and larger banks' higher willingness to pay for them, rather than one of risk preferences. To test this, we re-estimate Hypothesis 4 substituting the explanatory variable 'size' for 'bailout probability', and also using both variables simultaneously. Moreover, size might only be one dimension. Complexity and the international scope of banks may be related to both the bailout probability and CEO characteristics. Therefore, we also test for complexity, using the non-interest income share and the share of non-deposit funding as proxies.

Table 6 presents the results from these robustness checks. To ensure comparability of the regression coefficients, we standardize all variables to have mean zero and standard deviation one. Comparing bailout probability and bank size, the findings in columns (1) and (2) indicate that when each variable is included separately, the bailout probability has the larger coefficient and the higher level of statistical significance. When both variables are included, as in column (3), the coefficient of the bailout probability remains significant and largely unchanged, whereas the size coefficient loses all significance. This shows that, even conditional on size, there remains a positive correlation between the bank's bailout

	Baseline	Size		Complexity				Threshold
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OC	OC	OC	OC	OC	OC	OC	OC
pr_{t-1}	1.526**		1.680**		1.593**		1.347*	11.10
	(0.766)		(0.817)		(0.789)		(0.782)	(7.524)
$pr_{t-1} \times crisis$	-3.776**		-3.757**		-1.955**		-2.105*	-15.20*
	(1.783)		(1.728)		(0.878)		(1.124)	(7.766)
$pr_{t-1} \times regulated\ period$	-2.143***		-2.247***		-2.280***		-1.944**	-11.63
	(0.797)		(0.820)		(0.858)		(0.798)	(7.531)
$size_{t-1}$		1.162*	-0.152					
		(0.640)	(0.364)					
$size_{t-1} \times crisis$		-2.466***						
		(0.800)						
$size_{t-1} \times regulated\ period$		-1.377**						
		(0.665)						
$non\text{-}interest\ income_{t-1}$				0.343	1.398***			
				(0.650)	(0.452)			
$non\text{-}interest\ income_{t-1} \times crisis$				1.031				
				(0.852)				
$non\text{-}interest\ income_{t-1} \times reg.\ per.$				1.398				
				(0.944)				
$non\text{-}deposit\ funding_{t-1}$						-0.819	-0.142	
						(0.625)	(0.309)	
$non\text{-}deposit\ funding_{t-1} \times crisis$						0.677		
						(0.936)		
$non\text{-}deposit\ funding_{t-1} \times reg.\ per.$						0.968		
						(0.674)		
Observations	180	180	180	163	163	162	162	121
R^2								
Pseudo - R^2	0.178	0.188	0.179	0.196	0.226	0.135	0.159	0.227
Clusters	123	123	123	115	115	114	114	95
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Testing alternative explanations for Hypothesis 4

This table tests alternative explanations for Hypothesis 4. In column (1) we regress the overconfidence dummy on the proxy for the bailout probability interacted with the three different periods described in Section 3.3.1. In column (2) we use size instead of the bailout probability. In column (3) we add size to column (1). In columns (4) to (7), we proceed analogously for two measures of complexity: the non-traditional income share in columns (4) and (5), and the share of non-deposit funding in columns (6) and (7). In column (8) we only keep financial institutions with less than \$50 bn in total assets to exclude financial institutions that are heavily regulated. For comparability purposes, bailout probability and sizes are standardized to mean zero and standard deviation one. In all specifications we use a logistic regression and cluster standard errors at the firm level, which are reported in parentheses.

probability and its CEO's level of overconfidence. Adding two proxies for complexity in columns (4) to (7) yields similar results: both proxies for complexity are insignificant when included separately and interacted with the period dummies in the regressions, and adding them to the bailout variable does not change the latter's coefficient significantly.

There might also be a concern that the systemic scores capture regulatory requirements, as post-crisis regulation is conditioned on whether a bank is systemically important. As such, the matching between overconfident CEOs and banks with implicit guarantees might be driven by regulation instead of implicit guarantees. However, post-crisis regulation was based on a size threshold rather than the continuous systemic scores used in our analysis (which were introduced only at a later stage). The Dodd-Frank act imposed a size threshold of \$50 bn to define which banks are systemically relevant. According to Labonte and Perkins (2017), this threshold was rather arbitrarily chosen. This arbitrary choice of the threshold provides a setting in which we can test this alternative hypothesis. In column (8) of Table 6, we exclude all financial institutions above the \$50 bn threshold. As Figure 1 shows, there is still substantial variation in predicted systemic scores below this threshold. The results in column (8) of Table 6 show that our qualitative results are upheld for the group of financial institutions that are not subject to enhanced post-crisis regulation.

Concerning Hypothesis 4, a potential further concern is that internally promoted CEOs may systematically differ from externally hired CEOs. To address this issue, we test whether our results are robust to conditioning on the origin of the CEO appointment. Specifically, we include a dummy variable that is one if the CEO had previously appeared in the *ExecuComp* database for the same firm prior to assuming the CEO position, and zero otherwise. The corresponding results are shown in Table A.4 in the appendix. The result in column (2) of Table A.4 indicates that controlling for the origin of the CEO hire does not qualitatively affect our findings. In fact, the point estimates increase slightly in magnitude.

Moreover, pre-turnover firm performance and risk might be correlated with both the bailout probability and the firm's choice of CEO. Although our baseline specification already includes a comprehensive set of control variables, we further examine robustness by conditioning on pre-turnover performance and risk. To do so, we incorporate several proxies for firm performance and volatility measured in the year preceding the CEO turnover. The results of this robustness test are shown in columns (3) through (6) in Table A.4 in the appendix. They demonstrate that even after additionally conditioning on different combinations of pre-turnover risk and performance proxies, our results remain largely unchanged.

As discussed in Section 3.2.2, data limitations required the introduction of a novel proxy for bailout probabilities. For this reason, we extrapolated systemic scores, available over the period 2017-2022 for the largest financial institutions, to the remaining financial institutions. These scores were then transformed into bailout probabilities using a logistic mapping, calibrated with bailout probabilities inferred from credit rating data. As this proxy relies on a two-step procedure, we conduct a series of robustness checks. First, we assess the sensitivity of our results to alternative specifications of the extrapolation of systemic scores. Second, we examine the robustness of the mapping between systemic scores and implied bailout probabilities.

To test the robustness of the extrapolation step, we re-estimate Hypotheses 2 and 4, extrapolating the systemic scores by (i) using the full set of variables selected by the LASSO procedure as informative predictors, and (ii) restricting the model to the ten most significant variables. Table A.5 in the appendix shows that the results remain qualitatively unchanged when including more vs. fewer explanatory variables in the extrapolation.

The mapping between the (predicted) systemic scores and the bailout probability might vary during the observation period. For example, the mapping could differ for systemically important financial institutions in the pre- and post-crisis periods. For this reason, we allow the mapping to vary over time in a series of robustness checks that are summarized in Table A.6 in the appendix. In columns (3) and (4) we allow the mapping to differ before and after the financial crisis (i.e., before and after 2008). In columns (5) and (6) the mapping is allowed to differ in three subperiods: the pre-crisis period, the post-crisis but pre-deregulation period, and the period following the 2018 deregulation. In columns (7) and (8) we allow the mapping to evolve linearly over time and in columns (9) and (10) the mapping between systemic scores and the bailout probability can discretely vary by year. The results show that point estimates decrease in magnitude, but our qualitative conclusions remain unchanged.

One could also consider alternative proxies for the bailout probability to rule out potential measurement error. Using the implicit bailout probability measure by Gropp et al. (2011) is precluded by data limitations (see footnote 21). Alternatively, one could directly use the extrapolated systemic scores rather than relying on the mapping between the two variables. However, the extrapolated systemic scores are not bounded between zero and one and exhibit greater dispersion, rendering them a considerably noisier measure as compared to our bailout proxy. A further approach would be to use a 'too-big-to-fail' indicator based on the size of the financial institution. However, this classifies institutions only coarsely and estimated coefficients then rest on few observations. In some specifications this prevents reliable estimation of coefficients. Taken together, we view the proposed measure as a reasonable and informative proxy given the available data.

Another key variable in our analysis is the incentive pay proxy. As observed incentive compensation may reflect realized firm performance rather than the underlying strength of incentives, we have constructed a performance-adjusted measure of incentive compensation. This measure defines the incentive component as the portion of incentive pay that exceeds the level predicted by firm performance, relative to the industry. We assess the robustness of our results with respect to this approach in several ways in Table A.7 in the appendix. First, we examine the sensitivity of our findings to alternative degrees of winsorization. Our benchmark analysis has winsorized the incentive pay measure at the 1st and 99th percentiles. Columns (3) and (4) in Table A.7 show the results when winsorizing at the 5th and 95th percentiles, and columns (5) and (6) show the results without winsorization. The results remain robust across these specifications.

Second, we use a simpler measure, which is the pay-performance sensitivity. We calculate this pay-performance sensitivity by regressing total CEO compensation on the firm's lagged annualized stock returns for each CEO. The resulting coefficient captures the sensitivity of the CEO's income to the firm's short-term performance. To ensure reliable estimation, we restrict the sample to CEOs with more than five observations. The results in columns (7) and (8) in Table A.7 show that, despite a considerable reduction in sample size, the main findings remain qualitatively unchanged.

We also assess the robustness of our findings concerning the overconfidence measure. As pointed out in Section 3.2 and illustrated in Figure 3, our measure may not accurately reflect overconfidence towards the beginning and the end of the sample, where we observe CEOs only for a short period of time and, thus, CEO tenures are shorter by definition. Therefore, we exclude CEOs from the analysis if they are observed for less than five years at either end of the sample. Figure B.1 in the appendix shows the change in the share of overconfident CEOs. The results presented in Table A.8 in the appendix show that our findings remain qualitatively robust to this adjustment, even though the sample size (and therefore the power to detect significant effects) is decreased substantially.

For Hypothesis 1, we used stock return volatility as a proxy for risk-taking, for the reasons outlined in Section 3.3.1 However, one potential concern is that volatility may capture not only the firm-specific risk of reaching a low state or default, as in our theoretical framework, but also broader factors such as macroeconomic shocks, fluctuations in expectations, and time-varying risk premia. To address this concern, we assess the robustness of our results to alternative measures of risk. One alternative is the default probability implied by long-term issuer credit ratings, which are also used to construct the bailout probability in Section 3.2.2. A limitation of this measure is that it is available only for a subset of the sample. Nonetheless, Table A.9 in the Appendix reports results for Hypothesis 1 using the logarithm of the default probability implied by the overall credit rating as well as by the stand-alone rating. Given the reduced sample size, we additionally report specifications that exclude influential observations (see below for details). The results indicate that, while statistical significance weakens, our qualitative conclusions remain unchanged.

Finally, we identify and address influential observations in the data. Influential observations are those that exert a strong impact on the estimated coefficients and, thus, on the predicted values of the model. To detect such observations, we use DFITS, a diagnostic measure that evaluates the influence of individual data points on the fitted values by quantifying the change in predicted values when a specific observation is excluded. A high DFITS value suggests that an observation significantly influences the regression model's predictions, potentially flagging it as influential.[35] The results, presented in Table A.10 in the appendix, indicate that removing these influential observations does not qualitatively change the estimated coefficients.

4 Conclusion

In this paper we have shown that there is a positive assortative matching between overconfident CEOs and government-protected banks in normal years, absent crises and enhanced regulation. Banks that expect a higher bailout probability due to their systemic relevance attract more overconfident CEOs by means of above-average bonus shares. Hence the moral hazard problem of government-protected banks that has been stressed in the previous literature is exacerbated by the hiring of managers who overestimate their probability of success and accordingly take excessive risks.

Our findings underscore the incentive problems arising in systemically relevant banks, which can expect to be protected in times of crisis. Our positive analysis has also shown that stricter regulation is able to prevent overconfident managers from taking excessive risks, and of breaking the link between the degree of government protection for banks and the hiring of overconfident CEOs. Together, these results offer additional support for the Basel III set of rules requiring systemically important banks to hold extra equity buffers or higher leverage ratios, as is planned under the new Basel IV framework.

At the same time, we emphasize the limitations of our analysis. Our theoretical model makes several partial equilibrium assumptions, such as exogenous financing costs for banks, or bailout probabilities that are mechanically linked to bank size. Moreover, this is a positive analysis that does not model an explicit welfare objective of governments. Our empirical analysis shows that the evidence is consistent with our main hypothesis. However, and despite a number of robustness checks, we cannot fully rule out alternative matching mechanisms.

From a methodological perspective, our analysis is among the few studies that use data on the degree of government protection for banks to empirically analyze the effects

[35] To identify only the most influential observations, we apply a threshold of $4 \times \sqrt{((e(dfm)+1)/e(N))}$, where dfm are the degrees of freedom, N is the number of observations and e is the exponential function. This threshold is a heuristic that adjusts for both model complexity and sample size, ensuring a balance between detecting influential data points and avoiding over-flagging in larger datasets.

of implicit bailout guarantees. To gain an improved understanding for the incentives of the affected banks, it is important to improve the database on the systemic importance of individual banks. The U.S. Global Systemically Important Banks (G-SIB) database could serve as a blueprint to develop a similar database for banks in other parts of the world.

Appendix A: Additional Tables

	Full	Lean	Select
	(1)	(2)	(3)
	Scores	Scores	Scores
Accum Other Comp Inc - Other Adjustments	-0.0105		
	(-1.634)		
Cash and Short-Term Investments	0.000257**	0.000260***	0.000291***
	(2.368)	(8.717)	(7.643)
Debt in Current Liabilities - Total	0.0000158		
	(0.095)		
Long-Term Debt - Tied to Prime	0.000152		
	(0.915)		
Debt - Notes	0.000288		
	(0.477)		
Dividends & Interest Receivable (Cash Flow)	-0.00248		
	(-1.319)		
Exchange Rate Effect	-0.000834		
	(-0.644)		
Property, Plant, and Equipment - Machinery and Equipment at Cost	-0.0392***	-0.0407***	-0.0185***
	(-5.333)	(-6.406)	(-3.033)
Financing Activities - Other	-0.000199***	-0.000247***	-0.000301***
	(-4.189)	(-8.944)	(-8.653)
Gain/Loss on Ineffective Hedges	-0.0331		
	(-1.060)		
Short-Term Investments - Change	0.000259***	0.000214***	
	(2.799)	(3.726)	
Liabilities Level1 (Quoted Prices)	0.0000899		
	(0.291)		
Marketable Securities Adjustment	-0.000667		
	(-1.097)		
Net Interest Margin	-11.99***	-14.57***	
	(-3.201)	(-4.124)	
Nonrecurring Income Tax Basic EPS Effect	1.684		
	(0.586)		

	(1)	(2)	(3)
Operating Activities - Net Cash Flow	0.0000694		
	(0.512)		
Dividend Rate - Assumption (%)	-3.813**	-3.128**	
	(-2.579)	(-2.003)	
Options - Fair Value of Options Granted	0.367***	0.342***	
	(4.494)	(3.772)	
Retained Earnings - Unrestricted	-0.00756**	-0.00688**	
	(-2.420)	(-2.214)	
Sale of Property, Plant and Equipment	0.00232		
and Investments - Gain (Loss)	(1.582)		
Stock Compensation Expense	0.0123		
	(1.317)		
Deferred Taxes (Cash Flow)	0.00127		
	(1.101)		
Deferred Taxes-State	0.0156		
	(0.688)		
Net Deferred Tax Asset	0.0000244		
	(1.240)		
Customers" Acceptance	0.0979**	0.0724**	
	(2.306)	(2.016)	
Risk-Adjusted Capital Ratio - Tier 1	15.89***	18.12***	19.88***
	(17.197)	(24.616)	(22.529)
Commissions and Fees - Other	0.00269**	0.00497***	0.00640***
	(1.998)	(8.554)	(9.798)
Property, Plant, and Equipment	-0.00629***	-0.00699***	-0.00574***
- Machinery and Equipment at Cost	(-2.818)	(-5.447)	(-5.924)
Foreign Exchange Income (Loss)	0.0776***	0.0724***	0.103***
	(5.366)	(5.217)	(6.721)
Interest and Dividend Income	0.00345		
- Sundry	(0.898)		
Other Intangibles	0.000343		
	(0.290)		
Income - Other (Broker Dealer)	0.00295		
	(1.368)		

	(1)	(2)	(3)
Short-Term Investments - Total	0.000106		
	(0.911)		
Preferred/Preference Stock	-0.206***	-0.228***	-0.185***
- Redeemable	(-9.680)	(-10.789)	(-7.094)
Securities In Custody	0.000513**	0.000900***	0.00137***
	(2.126)	(6.764)	(10.560)
Securities Sold Not Yet Purchased	-0.0000697		
	(-0.350)		
Trading/Dealing Account Securities	0.00704		
- Local Governments	(1.313)		
Trading/Dealing Account Securities	0.000342***	0.000459***	0.000500***
- Total	(4.212)	(8.301)	(8.006)
Trust Fees	0.00632***	0.00285**	
	(4.276)	(2.619)	
Interest Expense - Long-Term Debt	0.00743**	0.00925***	
	(2.507)	(4.990)	
Constant	-98.53***	-118.2***	-175.0***
	(-5.646)	(-7.157)	(-14.724)
Observations	112	112	112
R^2	0.997	0.995	0.989
Model DF	40	18	10
Residual DF	71	93	101
F Test	655.7	1071.4	908.7

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.1: The LASSO models

This table shows in column (1) the results of the full model that is chosen by the LASSO approach, leaving out variables with a coefficient of zero. The cross-validation of the model is performed on 10 folds with an optimal penalty parameter of 2.28. Column (2) shows the results of the lean model after all insignificant variables are dropped. Column (3) shows the results of the version of the model that is restricted to the ten most significant variables. Variable definitions can be found in *Compustat*. The model in Column (2) is used for our benchmark analysis, whereas the models in Columns (1) and (3) are used for robustness tests (see Table A.5).

Variable	Definition 1992-2005	Definition change from 2006
Non-stock-based Compensation		
salary	Dollar value of the annual base salary paid during the fiscal year.	No change.
bonus	Dollar value of the annual bonus paid during the fiscal year. The amount of cash or non-cash unrestricted compensation received during the fiscal year, if subject to performance criteria not in excess of one year and/or if contingent only on continued employment.	*ExecuComp* retains variable name BONUS, but it should be BONUS(R) since elements of BONUS are included in the new variable noneq_incent, with BONUS(R) now confined to annual non-performance payments while noneq_incent is contingent on achieving performance targets, often extending beyond one year.
ltip	Cash payment under long-term incentive plan that may include restricted stock (or stock "units") tied to performance criteria such as cash flow or EPS over a period of more than one year (usually three years). If only restricted stock is given, company can opt to disclose ltip under rstkgrnt.	Discontinued and replaced by noneq_incent.
noneq_incent		New variable. Reports amount of all non-equity compensation paid in that year that is triggered by attainment of performance target(s) defined by the incentive compensation plan. noneq_incent excludes stock-based pay, differentiating it from ltip but similar to elements of BONUS.

Table A.2: Compensation variable definitions in Execucomp

This table presents the variable definitions for incentive compensation in the *Execucomp* database. The table is taken from Hopkins and Lazonick (2016), p.40f.

Dependent variable	**H1** Volatility	**H2** Incentive	**H3** Incentive	**H4** Overconfidence
Age of CEO		✓	✓	✓
Annualized Stock Returns		✓	✓	✓
Volatility of Daily Stock Returns		✓	✓	✓
Cash-Flows		✓	✓	✓
Book Leverage	✓	✓	✓	✓
Market-to-Book Ratio	✓	✓	✓	✓
Liquidity Ratio	✓	✓	✓	✓
Size of Executive Board	✓	✓	✓	✓
Return on Assets	✓			
Size (Log Total Assets)	✓			
Deposit Ratio	✓			
Year-End Stock Price	✓			
Incentive Component	✓			

Table A.3: List of control variables in the analysis

This table shows which of the control variables are used in the different hypotheses. The choice of variables follows earlier approaches in the literature.

	Baseline	Origin	Pre-turnover characteristics			
	(1)	(2)	(3)	(4)	(5)	(6)
	OC	OC	OC	OC	OC	OC
pr_{t-1}	0.178**	0.185**	0.176**	0.178**	0.176**	0.183**
	(0.089)	(0.090)	(0.088)	(0.089)	(0.088)	(0.092)
pr_{t-1} × *crisis*	-0.441**	-0.441**	-0.440**	-0.441**	-0.439**	-0.467**
	(0.208)	(0.203)	(0.208)	(0.209)	(0.209)	(0.219)
pr_{t-1} × *regulated period*	-0.250***	-0.259***	-0.247***	-0.249***	-0.247***	-0.250***
	(0.093)	(0.095)	(0.094)	(0.093)	(0.094)	(0.094)
internal hire		-0.425				
		(0.694)				
$volatility_{t-1}$			-0.0818		-0.0812	
			(0.941)		(0.964)	
annualized stock $returns_{t-1}$				-0.0105	-0.00324	
				(0.515)	(0.533)	
returns on $assets_{t-1}$						1.565*
						(0.814)
Observations	180	180	179	179	179	180
R^2						
$Pseudo - R^2$	0.178	0.180	0.177	0.177	0.177	0.185
Clusters	123	123	122	122	122	123
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Robustness of Hypothesis 4 to CEO origin and pre-turnover characteristics

This table presents the sensitivity of the results of Hypotheses 4 to controlling for CEO origin and pre-turnover characteristics. Column (1) shows the baseline results as in Table 5. In Column (2) we additionally control for CEO origin by including a dummy variable that equals one if the CEO had previously appeared in the *ExecuComp* database for the same firm prior to assuming the CEO position, and zero otherwise. In Column (3) we include lagged stock return volatility, in Column (4) lagged annualized stock returns and in Column (5) both. In Column (6) we include lagged returns on assets. In all specifications we cluster standard errors at the firm level, which are reported in parentheses.

	Baseline (lean)		Full		Select	
	H2	H4	H2	H4	H2	H4
	(1)	(2)	(3)	(4)	(5)	(6)
	Incentive	OC	Incentive	OC	Incentive	OC
pr_{t-1}	0.0432***	0.178**	0.0478***	0.173*	0.0373**	0.214**
	(0.014)	(0.089)	(0.015)	(0.104)	(0.014)	(0.097)
$pr_{t-1} \times crisis$	-0.00673	-0.441**	-0.0134	-0.450*	-0.000701	-0.448**
	(0.019)	(0.208)	(0.020)	(0.232)	(0.019)	(0.182)
$pr_{t-1} \times regulated\ period$	-0.0263*	-0.250***	-0.0293*	-0.253**	-0.0256*	-0.290***
	(0.015)	(0.093)	(0.016)	(0.107)	(0.015)	(0.101)
Observations	221	180	221	180	221	180
R^2	0.204		0.205		0.192	
$Pseudo-R^2$		0.178		0.173		0.186
Clusters	144	123	144	123	144	123
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Robustness of Hypotheses 2 and 4 to the LASSO extrapolation

This table presents the sensitivity of the results of Hypotheses 2 and 4 to changes in the extrapolation of the systemic scores. Columns (1) and (2) show the baseline results as in Table 5. In columns (3) and (4) we use the full set of variables as elected by the LASSO procedure as informative predictors, and in columns (5) and (6) we restrict the model to the ten most significant variables. The respective models are shown in Table A.1. In all specifications we cluster standard errors at the firm level, which are reported in parentheses.

	Baseline		Crisis		Period		Trend		Discrete	
	H2	H4	H2	H4	H2	H4	H2	H4	H2	H4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Incentive	OC	Incentive	OC	Incentive	OC	Incentive	OC	Incentive	OC
pr_{t-1}	0.0432***	0.178**	0.0270**	0.123*	0.0273**	0.0984	0.0282***	0.130**	0.0239**	0.158**
	(0.014)	(0.089)	(0.010)	(0.072)	(0.011)	(0.067)	(0.010)	(0.062)	(0.010)	(0.068)
$pr_{t-1} \times crisis$	-0.00673	-0.441**	0.00607	-0.304**	0.00797	-0.331**	0.00485	-0.306***	0.00691	-0.342**
	(0.019)	(0.208)	(0.015)	(0.124)	(0.018)	(0.136)	(0.015)	(0.117)	(0.015)	(0.161)
$pr_{t-1} \times regulated\ period$	-0.0263*	-0.250***	-0.0151	-0.177**	-0.0183	-0.146**	-0.0174	-0.177***	-0.0163	-0.191***
	(0.015)	(0.093)	(0.011)	(0.075)	(0.012)	(0.070)	(0.011)	(0.064)	(0.011)	(0.069)
Observations	221	180	221	180	221	180	221	180	221	180
R^2	0.204		0.200		0.200		0.207		0.189	
$Pseud - R^2$		0.178		0.181		0.182		0.187		0.183
Clusters	144	123	144	123	144	123	144	123	144	123
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Robustness of Hypotheses 2 and 4 to the bailout probability mapping

This table presents the sensitivity of the results of Hypotheses 2 and 4 to changes in the mapping from systemic scores to bailout probabilities over time. Columns (1) and (2) show the baseline results as in Table 5. In columns (3) and (4) we allow the mapping to differ before and after the financial crisis (i.e., before and after 2008), and in columns (5) and (6) we allow the mapping to differ across three subperiods: the pre-crisis period, the post-crisis but pre-deregulation period, and the period following the 2018 deregulation. In columns (7) and (8) we allow the mapping to evolve linearly over time and in columns (9) and (10) we allow the mapping between systemic scores and the bailout probability to discretely vary by year. In all specifications we cluster standard errors at the firm level, which are reported in parentheses.

	Baseline		Winsorization				Pay sensitivity	
	H2	H3	H2	H3	H2	H3	H2	H3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Incentive	Incentive	Incentive	Incentive	Incentive	Incentive	Incentive	Incentive
pr_{t-1}	0.0432***		0.0399***		0.0432***		0.0864***	
	(0.014)		(0.013)		(0.014)		(0.029)	
pr_{t-1} × *crisis*	-0.00673		-0.00537		-0.00755		0.00463	
	(0.019)		(0.018)		(0.019)		(0.092)	
pr_{t-1} × *regulated period*	-0.0263*		-0.0246*		-0.0266*		-0.0915***	
	(0.015)		(0.014)		(0.015)		(0.030)	
OC_t		1.046**		1.002**		1.066**		2.097***
		(0.453)		(0.436)		(0.466)		(0.598)
OC_t × *crisis*		-1.717**		-1.658**		-1.749**		-2.055***
		(0.670)		(0.652)		(0.678)		(0.737)
OC_t × *regulated period*		-1.198**		-1.139**		-1.229**		-2.131***
		(0.532)		(0.513)		(0.543)		(0.629)
Observations	221	222	221	222	221	222	91	92
R^2	0.204	0.167	0.198	0.166	0.197	0.163	0.489	0.524
$Pseudo - R^2$								
Clusters	144	144	144	144	144	144	84	84
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Robustness of Hypotheses 2 and 3 to the choice of the incentive measure

This table presents the sensitivity of the results of Hypotheses 2 and 3 to changes in the incentive measure. While Columns (1) and (2) show the results for the baseline specification, as in Table 5, Columns (3) and (4) show the results when winsorizing the incentive measure at the the and 95th percentiles and Columns (5) and (6) when not winsorizing the measure. Columns (7) and (8) show the results when using pay-performance sensitivity instead, which is defined as the coefficient in the individual-level-regression of total compensation on the firm's returns. In all specifications we cluster standard errors at the firm level, which are reported in parentheses.

	H1		H2		H3		H4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Volatility		Incentive		Incentive		OC	
OC_t					1.046**	0.815*		
					(0.453)	(0.486)		
$OC_t \times crisis$					-1.717**	-1.751**		
					(0.670)	(0.705)		
$OC_t \times regulated\ period$					-1.198**	-0.999*		
					(0.532)	(0.573)		
OC_{t-1}	0.0641**	0.0864**						
	(0.032)	(0.033)						
$OC_{t-1} \times crisis$	-0.0664	-0.109**						
	(0.041)	(0.044)						
$OC_{t-1} \times regulated\ period$	-0.0692*	-0.0882**						
	(0.036)	(0.038)						
pr_{t-1}			0.0432***	0.0635**			0.178**	0.128
			(0.014)	(0.028)			(0.089)	(0.096)
$pr_{t-1} \times crisis$			-0.00673	-0.0560			-0.441**	-0.590*
			(0.019)	(0.034)			(0.208)	(0.348)
$pr_{t-1} \times regulated\ period$			-0.0263*	-0.0495*			-0.250***	-0.249**
			(0.015)	(0.027)			(0.093)	(0.120)
Observations	2921	2670	221	131	222	159	180	114
R^2	0.562	0.574	0.204	0.254	0.167	0.205		
$Pseudo - R^2$							0.178	0.208
Clusters	219	207	144	97	144	112	123	91
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	firm-level	firm-level						

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Robustness of main results to 'short-tenure' CEOs

This table presents the baseline empirical results in the respective first column, and the results excluding the first and last CEO per firm with less than five years of tenure in the respective second column. In columns (1) and (2) we regress the natural logarithm of the daily stock return volatility on the overconfidence dummy interacted with the three different periods as described in Section 3.3.1. In columns (3) and (4) we regress the natural logarithm of the incentive component on the proxy for the bailout probability interacted with the three different periods as described in Section 3.3.2. In columns (5) and (6) we regress the natural logarithm of the incentive component on the overconfidence dummy interacted with the three different periods as described in Section 3.3.2. In columns (7) and (8) we regress the overconfidence dummy on the proxy for the bailout probability interacted with the three different periods as described in Section 3.3.2. In all specifications we cluster standard errors at the firm level, which are reported in parentheses. Columns (1) to (6) use OLS, columns (7) and (8) a logistic regression.

	Baseline		Overall credit rating		Stand-alone rating	
	(1)	(2)	(3)	(4)	(5)	(6)
	Volatility	Volatility	Default	Default	Default	Default
OC_{t-1}	0.0641**	0.0672**	0.524**	0.483***	0.466*	0.363*
	(0.032)	(0.032)	(0.216)	(0.176)	(0.246)	(0.212)
$OC_{t-1} \times crisis$	-0.0664	-0.0770**	-0.0425	-0.107	0.0861	-0.0648
	(0.041)	(0.039)	(0.162)	(0.141)	(0.225)	(0.235)
$OC_{t-1} \times regulated\ period$	-0.0692*	-0.0843**	-0.245	-0.334*	-0.378	-0.468*
	(0.036)	(0.034)	(0.243)	(0.185)	(0.305)	(0.265)
Observations	2921	2909	492	486	498	492
R^2	0.562	0.567	0.745	0.773	0.735	0.763
$Pseudo-R^2$						
Clusters	219	217	38	37	51	50
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	firm-level	firm-level	firm-level	firm-level	firm-level	firm-level
DFITS	no	yes	no	yes	no	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Robustness of Hypothesis 1 to the choice of the risk measure

This table presents the results for Hypothesis 1 when using alternative risk measures. As the alternative risk measures are only available for small subsamples, we also show results when removing influential observations (see Table A.10). Column (1) shows the baseline results as presented in Table 5 and Column (2) the baseline results after removing influential observations as shown in Table A.10. In Columns (3) and (4), we use the overall credit rating as provided by Fitch, and in Columns (5) and (6) we use the stand-alone rating as provided by Fitch. In all specifications we cluster standard errors at the firm level, which are reported in parentheses.

	H1		H2		H3		H4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Volatility		Incentive		Incentive		OC	
OC_t					1.046**	0.557		
					(0.453)	(0.352)		
$OC_t \times crisis$					-1.717**	-1.528**		
					(0.670)	(0.606)		
$OC_t \times regulated\ period$					-1.198**	-0.755*		
					(0.532)	(0.441)		
OC_{t-1}	0.0641**	0.0672**						
	(0.032)	(0.032)						
$OC_{t-1} \times crisis$	-0.0664	-0.0770**						
	(0.041)	(0.039)						
$OC_{t-1} \times regulated\ period$	-0.0692*	-0.0843**						
	(0.036)	(0.034)						
pr_{t-1}			0.0432***	0.0403***			0.178**	0.175*
			(0.014)	(0.013)			(0.089)	(0.090)
$pr_{t-1} \times crisis$			-0.00673	-0.00970			-0.441**	-0.442**
			(0.019)	(0.017)			(0.208)	(0.194)
$pr_{t-1} \times regulated\ period$			-0.0263*	-0.0205			-0.250***	-0.225**
			(0.015)	(0.014)			(0.093)	(0.094)
Observations	2921	2909	221	213	222	214	180	177
R^2	0.562	0.567	0.204	0.171	0.167	0.133		
$Pseudo-R^2$							0.178	0.170
Clusters	219	217	144	142	144	142	123	122
Controls	Yes		Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes		Yes	
Fixed effects	firm-level							

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Robustness of main results to outliers in the data

This table presents the baseline empirical results in the respective first column, and the respective second column excludes influential observations as identified by DFITS, diagnostics meant to show how influential a point is in a linear regression. The threshold is set to $4 \times \sqrt{((e(df_m) + 1)/e(N))}$. In columns (1) and (2) we regress the natural logarithm of the daily stock return volatility on the overconfidence dummy interacted with the three different periods as described in Section 3.3.1. In columns (3) and (4) we regress the natural logarithm of the incentive component on the proxy for the bailout probability interacted with the three different periods as described in Section 3.3.2. In columns (5) and (6) we regress the natural logarithm of the incentive component on the overconfidence dummy interacted with the three different periods as described in Section 3.3.2. In column (7) and (8) we regress the overconfidence dummy on the proxy for the bailout probability interacted with the three different periods as described in Section 3.3.2. In all specifications we cluster standard errors at the firm level, which are reported in parentheses. Columns (1) to (6) use OLS, columns (7) and (8) a logistic regression.

B Additional Figures

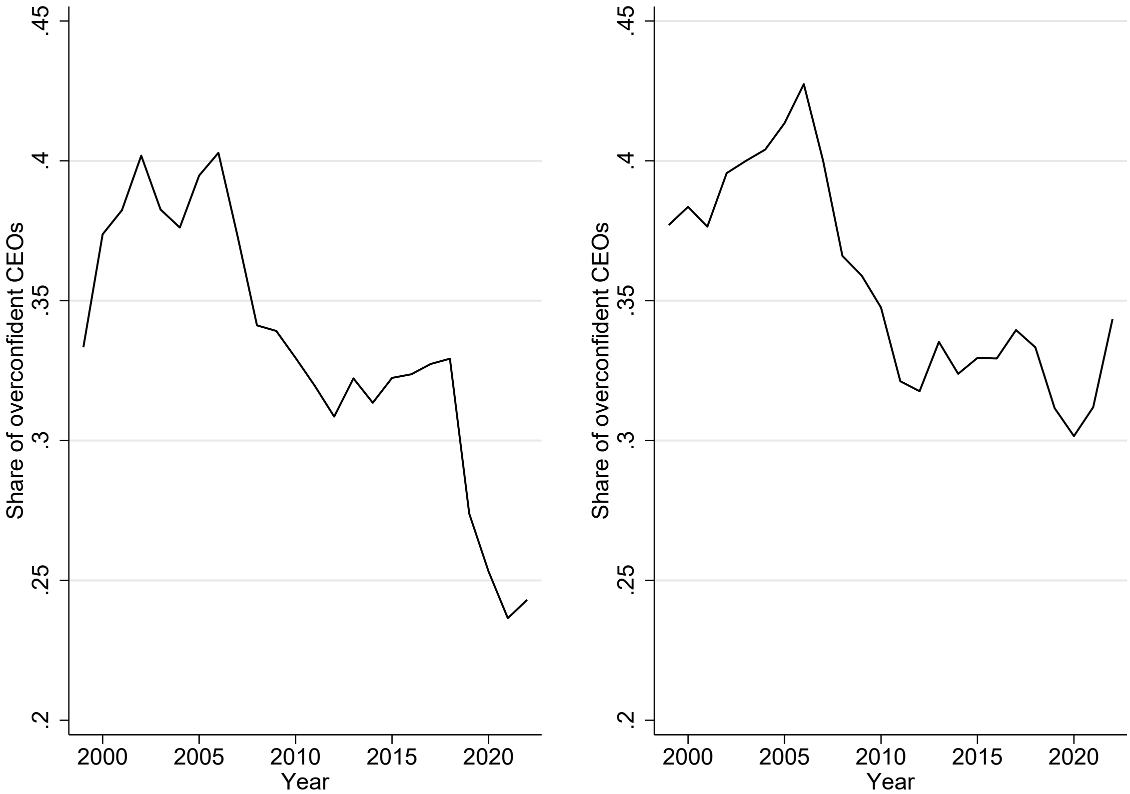

Figure B.1: Development of overconfidence over time

This figure shows the development of the share of overconfident CEOs over time. The left panel shows the share in the original sample and the right panel the share in the sample when excluding CEOs with less than five years of tenure at either end of the sample.

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