

ERASMUS UNIVERSITY ROTTERDAM  
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**CEO Job Security and Firm Performance:  
An Empirical Study of the Effects of CEO Employment Contracts at  
the Time of a Financial Crisis**

Name student: Kim Sterkenburg

Student ID number: 414285

Supervisor: Dr. Daniel Urban

Second assessor: Dr. Stefan Obernberger

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**ABSTRACT**

This paper examines the effects of CEO employment contracts on CEO job security and firm performance during the 2007 financial crisis. Using a unique, manually collected dataset of employment agreements for CEOs of S&P 500 firms in office at the start of the financial crisis, I find that CEOs with (long) explicit employment contracts had greater job security during the financial crisis, measured by CEO turnover rates, compared to CEOs with relatively short employment contract horizons. In theory, contractual protection against dismissal offered by (long) explicit employment contracts may encourage CEOs to make value-enhancing long-term decisions. However, I find that firms with (long) explicit CEO employment contracts performed worse during the financial crisis compared to firms with relatively short CEO employment contract horizons. The findings suggest that (long) explicit CEO employment contracts may offer a wrong type of job security that induces perverse CEO incentives. Overall, this study highlights the importance of contract design for incentive purposes.

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# 1. Introduction

In the past decades, a central question in the corporate governance literature has been how to optimally incentivize managers to act in the interest of shareholders. Agency problems within a firm arise when managers pursue their own interests at the expense of the shareholders (Jensen & Meckling, 1976). Relatively few studies have examined the role of Chief Executive Officer (“CEO”) employment contract horizons in motivating CEOs. A long contractual time horizon may offer job security that alleviates managerial risk aversion and that encourages value-maximizing risky decisions. In this paper, I examine the effects of CEO employment contracts on CEO job security and firm performance at the time of a financial crisis. The aim is to shed a novel light on the role of CEO employment agreements in providing job security and its influence on firm performance.

CEO employment contracts define all terms and conditions of the employment relationship between the firm and the CEO. In the U.S., firms employ CEOs under an explicit (written) employment contract or an implicit (non-written) employment contract. Most explicit employment contracts are for a fixed time period. Dismissal before the expiration date of the employment contract is costly and the costs of dismissal usually increase with the contract horizon. In contrast, implicit employment contracts are presumed to be for *at-will* employment. This essentially means that the firm or the CEO can terminate the employment contract at any time and for any cause (Groen-Xu, 2013). Dismissal of CEOs with implicit contracts is typically less costly than dismissal of CEOs with explicit employment contracts. In other words, implicit CEO contracts have an infinitely short horizon, whereas the horizon of explicit CEO contracts increases with the time remaining until contract expiration.

Explicit CEO employment contracts enable firms to attract and retain CEOs of high quality (Gillan, Hartzell, & Parrino, 2009). The contractual protection against dismissal can align the decision horizon of the CEO with the horizon of the shareholders. For instance, job security mitigates managerial risk aversion and encourages value-enhancing but risky decisions (see, e.g., Manso, 2011). Thus, longer contract horizons may reduce incentives problems. On the other hand, long CEO employment contract horizons protect the CEO against dismissal after poor performance. The high costs of early contract termination reduce the threat of dismissal, and hence might impose less discipline (see, e.g., Bertrand & Mullainathan, 2003).

In consideration of the controversial effects of CEO contract horizons, the purpose of this paper is to examine whether CEO employment contract horizons *ex ante* affect CEO job security and firm performance at the time of a financial crisis. The main challenge is the collection of CEO employment contract data because employment contract data are not readily

available. However, the U.S. Securities and Exchange Commission (SEC) requires firms to file employment agreements with executives. This requirement enables me to manually collect data on explicit and implicit CEO employment contracts for CEOs of the S&P 500 firms in 2007. The empirical strategy is comparable to the empirical design of a study by Almeida, Campello, and Laranjeira (2011), in which they examine the effect of financial contracting on corporate policies during the financial crisis. Accordingly, the empirical strategy is a difference-in-differences (“DID”) approach using the 2007 financial crisis as event. The financial crisis provides for a shock to the economic environment that was generally unanticipated by firms (Almeida et al., 2011). The advantage of using this shock is that the variation in CEO employment contracts is exogenous, because contract terms are predetermined at the start of the financial crisis and are relatively rigid. Therefore, CEO employment contract terms are plausibly exogenous to the performance of the firm during the financial crisis. In this study, I examine whether CEOs with (long) explicit employment contracts at the start of the financial crisis had greater job security and performed better during the financial crisis relative to CEOs with implicit contract horizons (or CEOs with relatively short explicit contract horizons).

The findings suggest that CEO employment contracts are an effective instrument to provide job security. As predicted, firms with explicit CEO employment contracts reduced CEO turnover more during the financial crisis relative to firms with implicit CEO employment contracts. Specifically, the difference-in-differences estimator indicates that the average CEO turnover rate for firms with explicit CEO employment contracts declined by 7.24 percentage points more relative to firms with implicit CEO employment contracts. This estimation is statistically and economically significant. The results hold for long explicit CEO employment contract horizons relative to shorter (explicit and implicit) employment CEO contract horizons.

Furthermore, the findings suggest that firms with explicit CEO employment contracts performed worse during the financial crisis relative to firms with implicit CEO employment contracts. The difference-in-differences estimator indicates that the average *return on assets* (ROA) for firms with explicit CEO contracts declined by 1.32 percentage points more during the financial crisis relative to firms with implicit CEO contracts. This finding is interesting because poor firm performance is expected to be associated with higher CEO turnover (Jenter & Lewellen, 2017). The findings suggest that the job security offered by explicit contracts might have been a wrong type of CEO job security. CEOs possibly realized that they were better protected against dismissal after poor firm performance, which induced less discipline and an agency conflict. Alternatively, in line with incentives predictions, the relatively poor firm performance might have been the result of long-term actions that in the short-term are observationally equivalent to low effort or bad CEO quality. Another side of the story could be that CEOs with implicit contracts experienced a constant job insecurity that incentivized to

exert effort. This could explain the relatively better firm performance of firms with short CEO contract horizons. However, it remains unidentified whether the relatively good performance of firms with short CEO contract horizons is based on short-term gains or long-term shareholder value.

In further analysis, I examine the effects of CEO employment contracts on corporate investments. The analysis provides insights into the effects of CEO contract horizons on investment incentives and the CEO decision horizon. Theory suggests that risk-averse CEOs with job insecurity are more likely to refrain from risky profit-maximizing projects with an unobservable long-term value (Gormley, Matsa, & Milbourn, 2013). However, I find no consistent evidence of the effects of CEO employment contract horizons on investment decisions during the financial crisis.

In a robustness test, I use the age of the CEO as an alternative proxy for career concerns and career horizon. One strand of the literature argues that managers closer to retirement – i.e. with lower career concerns – care less about career prospects, and hence exert low effort (see, e.g., Holmström, 1999). In contrast, Jenter and Lewellen (2015) find that agency conflicts do not worsen with CEO age. Many value-creating decisions are related to future personal costs to CEOs, which might be of lower importance to CEOs close to retirement. However, I find that firms with older CEOs performed relatively worse during the financial crisis. The results point more in the direction of agency conflicts between older CEOs and shareholders. Moreover, the findings suggest that the poor firm performance is associated with higher CEO turnover. However, due to a relatively weak identification strategy, it is ambiguous whether turnovers in the data are for retirement or for poor performance reasons, and whether the outcomes are attributable to the treatment, as examined in a placebo test.

This paper makes several contributions to the existing literature. First, this study contributes to the literature on CEO employment contracts. Schwab and Thomas (2006), and Gillan, Hartzell, and Parrino (2009) examine the determinants of explicit CEO employment contracts from a legal and empirical perspective, and Rusticus (2006), González-Uribe and Groen-Xu (2017), and Cziraki and Groen-Xu (2018) examine the effects of CEO contracts on CEO turnover. The studies report that firms offer explicit contracts when the continuity of the employment relationship is uncertain, and that explicit CEO contracts create a commitment by protecting the CEO against dismissal. In this study, I empirically show that explicit CEO employment contracts are indeed an effective instrument to provide job security.

Second, this study sheds a novel light on the role of CEO employment contracts in incentivizing CEOs. The findings indicate that CEO contractual protection is negatively related to firm performance at the time of a financial crisis. That is, firms with CEOs who are better

protected against dismissal performed relatively worse. The findings suggest that explicit CEO employment contracts might have offered a wrong type of job security that imposed less discipline, which supports the agency problem view of high contractual protection (see, e.g., Bertrand & Mullainathan, 2003; Zhao, 2013).

Furthermore, this study contributes to contract design literature. Most empirical studies link firm outcomes to CEO compensation (see, e.g., Frye, 2004; Matolcsy & Wright, 2011). This study explores the time horizon aspect of CEO contracts and the related contractual protection. Contract horizons differ from long-term equity compensation in setting CEO incentives. Prior literature (see, e.g., Low, 2009) has argued that equity-based compensation helps CEOs to overcome aversion to risky projects by providing benefits from the upside of a risky investment. However, long-term employment contracts provide in addition insurance against downside risk by protecting the CEO against dismissal after poor firm performance, and thereby may lead to optimal risk-taking behavior. Thus, the interaction between CEO compensation and contract horizons might be important for contract design. A thorough understanding of employment contract incentive structures is valuable for the board of directors, who determine contract terms, as well as for regulators, who may consider adjusting the regulation related to executive employment contracts.

Finally, this paper contributes to two recent studies by González-Uribe and Groen-Xu (2017), and Cziraki and Groen-Xu (2018). These studies also use a manually collected CEO employment agreements dataset to examine the effects of CEO contract horizons on CEO turnover and corporate policies. Employment contract durations are likely to be endogenously determined. Therefore, as identification strategy, both studies use within-agreement variation. Within an agreement, the contract horizon decreases over time, with a sharp increase in contract horizon when the agreement is renewed. Compared to these two studies, the novelty of this paper is its research design. I use a difference-in-differences approach around the financial crisis to overcome endogeneity concerns. To the best of my knowledge, this is the first study that examines the effects of CEO employment contract horizons on CEO job security and firm performance at the time of a financial crisis.

The remainder of this paper is organized as follows. Section 2 discusses the theoretical background of CEO employment agreements. Section 3 reviews prior literature and discusses the development of the hypotheses for this study. Section 4 presents the sample and variable construction, and Section 5 describes the empirical strategy. Section 6 presents the results of the analysis, which will be followed by the results of several robustness tests in Section 7. Finally, Section 8 presents the conclusions, limitations of this research, and recommendations for future research.

## 2. Theoretical Background

When a firm appoints a new CEO, the new CEO and board of directors must negotiate the terms of the employment. These employment terms are usually complex and have substantial long-term consequences for the stockholders of the firm. The terms include, for instance, the duties and responsibilities of the CEO, the compensation, the duration of the employment, change-in-control provisions, and the severance package of the CEO. Firms must decide whether to govern the employment terms of the CEO by an explicit (written) or an implicit (non-written) contract. Gillan et al. (2009) report that less than half of the S&P 500 firms in 2000 had an explicit, written employment contract with their CEO. Instead, these CEOs had an implicit, non-written contract or a contract that covered only a few aspects of their employment, such as a change-in-control contract. The employment of CEOs with implicit employment contracts is presumed to be at-will. Under an at-will employment arrangement, a CEO may be dismissed by the firm, or the CEO may choose to leave, for any or no reason (Groen-Xu, 2013).

An explicit employment contract could be beneficial for both the firm and the CEO. First, an explicit contract enables the firm to attract and retain a CEO of high quality, because it creates a commitment and it explicitly specifies the rights of the CEO and the firm. This commitment reduces uncertainty regarding the employment. Besides that, by reducing uncertainty, an explicit employment contract can also reduce agency problems (Gillan et al., 2009). CEOs with explicit contracts experience less immediate dismissal threat and are therefore more likely to take risky value-creating long-term projects. Nonetheless, explicit employment contracts also have downsides. The main disadvantages of explicit employment agreements are the reduced flexibility of the board of directors to adjust contract provisions when changing environmental conditions demand different employment terms, and the possibility that courts misinterpret provisions of the employment contract (Gillan et al., 2009).

Firms usually do not provide the reason for the decision to employ the CEO under an explicit or implicit contract. In annual proxy statements, firms without an explicit CEO employment contract generally state that they do not have an explicit contract, often without reasoning why (Gillan et al., 2009). However, firms sometimes specify the reason. For example, Raytheon Company states in its 2007 annual report that at-will employment offers them the flexibility to remove employees when this is necessary. The quotation can be found in appendix A1.

Explicit CEO employment contracts commonly cover a fixed time period and sometimes contain automatic renewal provisions. The duration of an explicit contract can be considered as a measure of the amount of protection the contract provides (Gillan et al., 2009). CEOs with

long explicit contract durations have legal protection for a longer time period. In contrast, CEOs with shorter explicit contract durations have contractual protection for a shorter time period and might be subject to an implicit contract after the expiration of the current employment contract. Nevertheless, even explicit contracts with a short duration provide more contractual protection than implicit contracts (Schwab & Thomas, 2006).

Explicit fixed-term contracts generally include provisions that protect the CEO against early dismissal or a change-in-control. When the CEO is dismissed by the firm before expiration of the employment contract, the CEO is typically entitled to a multiple of the base salary plus a bonus (Groen-Xu, 2013). Explicit employment agreements contain various clauses that specify the conditions that would allow the firm to dismiss the CEO. These conditions are classified as for “good cause”, which, for instance, could define a willful failure by the CEO to perform his duties, or “without cause”. The “good cause” clause is important in aligning the interests of the firm and the CEO. The threat of “good cause” dismissal should discipline the CEO. The “good cause” provision also legally protects the CEO by explicitly defining the scope of termination for “good cause”. The contract provisions also define any payments the firm is obligated to make when the CEO is dismissed. This payment is usually larger for dismissal “without cause” than for “good cause” (Gillan et al., 2009). CEOs who are dismissed for “good cause” typically lose most severance benefits provided in the employment contract. Therefore, termination “without cause” is more expensive to firms (Schwab & Thomas, 2006).

Instead of termination by the firm, a CEO can decide to leave the firm voluntarily. Voluntary termination of the employment by the CEO is in explicit contracts classified as for “good reason”, or “without good reason”. The clause “good reason” could define, for instance, a material change in the duties that are assigned to the CEO. Again, the definition of “good reason” is important for the potential payments the firm is obligated to make to the CEO. CEOs leaving for “good reason” are typically entitled to larger severance payments, usually identical to the payments after termination by the firm “without cause”, whereas leaving “without good reason” typically leads to less severance payments, often identical to the payments after termination by the firm for “good cause” (Zhao, 2013) (Schwab & Thomas, 2006).

Appendix A2 quotes the termination clauses that are included in the employment agreement of Analog Devices, Inc. with their CEO Jerald G. Fishman. As shown by this explicit fixed-term contract example, firms are obligated to make greater severance payments when the contract is terminated before its expiration date. The severance payments for termination “without cause” and for “good reason” are typically specified as a function of the remaining time until the expiration date of the contract, as shown in Section 5.4. b. of the employment



contract of Jerald G. Fishman in appendix A2. Thus, the costs of termination increase with the number of years left under the fixed-term contract. The relatively large severance payments the firm is obligated to make when it terminates the employment contract in the beginning of its duration may deter the firm from replacing the CEO (Cziraki & Groen-Xu, 2018). In contrast, CEOs with a short time remaining until the expiration date may face a more immediate threat of dismissal because the costs of replacement are relatively low (Groen-Xu, 2013).

Implicit employment contracts provide less contractual protection relative to explicit employment contracts. One reason for the lower contractual protection is that at-will employment is not for a fixed time period. That is, implicit employment contracts have no expiration date. Instead, the employment can be terminated at any time and for any cause, by the firm and by the CEO. Occasionally, explicit employment contracts expressly declare at-will employment (Schwab & Thomas, 2006). For instance, the explicit employment contract of BMC Software, Inc. with their CEO Robert Beauchamp expressly declares at-will employment. This example is shown in appendix A3.

Nevertheless, at-will employment does not involve a costless termination of the contract. The firm might still be obligated to make severance payments to the CEO. However, termination is equally costly at any time from a legal perspective (Groen-Xu, 2013). Moreover, standard at-will employment provides no greater rights to a CEO whose employment is terminated “without cause” than for “good cause”. As mentioned earlier, at-will employment can be expressly declared in an explicit employment contract. Schwab and Thomas (2006) report that CEOs with expressly declared at-will contracts typically have greater rights for termination “without cause” than for “good cause”. Thus, CEOs with explicit at-will contracts are contractually better protected against dismissal than CEOs with implicit at-will contracts.

The question that emerges is whether the theoretical relation between contractual protection and the probability of dismissal holds in practice, and if so, how this job security affects the behavior of the CEO.

### **3. Literature Review & Hypotheses Development**

#### **3.1 CEO Job Security**

The corporate board of directors has the important task to decide whether to retain or dismiss the CEO. Standard CEO turnover models in the literature assume that the board gains insight into the ability of the CEO from the performance of the firm. The board may learn that the CEO is of low quality or a bad match by monitoring performance over time. According to the models, a firm should replace its CEO if the ability of the CEO falls below a threshold, which is usually the expected ability of a new CEO adjusted for the costs of replacement

(Jenter & Kanaan, 2015). The costs of replacement are substantially high for CEOs with an explicit employment contract and are even higher for CEOs with long explicit employment contract horizons.

Besides dismissal of CEOs of low quality, a firm may also dismiss its CEO because the CEO is not the best match anymore. Bertrand and Schoar (2003) report a large and persistent heterogeneity in management styles. These management styles explain a substantial fraction of the corporate behavior and firm performance. An environmental change could require a different management style. For instance, Anderson, Bustamante, Guibaud, and Zervos (2018) argue that an increased product market competition or an innovative technology could demand for replacement of the CEO because such shocks coincidence with growth opportunities.

An environmental change also appears during a financial crisis. Jenter and Kanaan (2015) argue that recessions allow to test the quality of the CEO because boards are able to observe aspects of the CEO skills that are unobservable during non-crisis periods. For instance, the board learns whether the CEO has foreseen and prepared for a recession. Moreover, a recession could have a considerable impact on the firms' environment, which requires the CEO to have different skills. If the CEO does not have those required skills, the CEO may no longer be the best match. Consistent with their view that firm performance during a crisis is more informative about the CEO quality and the CEO-firm match, Jenter and Kanaan (2015) report some evidence that firm performance affects CEO turnover more during crisis periods.

As already discussed in the previous section, (long) explicit CEO employment contracts may offer job security. Most explicit contracts are for a fixed period, which creates a commitment between the firm and the CEO. On the other hand, implicit employment contracts and explicit at-will contracts can be considered as for an infinitely short term because the employment can be terminated at any time and for any reason (Groen-Xu, 2013). Long-term explicit contracts offer CEOs the largest contractual protection because the costs of dismissal usually increase with the number of years left until contract expiration. CEOs with short explicit contract horizons are therefore less costly to dismiss than CEOs with long explicit contract horizons. Still, the literature argues that CEOs with short explicit contract horizons have more contractual protection than CEOs with implicit contracts (see, e.g., Gillan et al., 2009).

Cziraki and Groen-Xu (2018) empirically show that the probability of a CEO turnover decreases with contract horizon. Specifically, their results indicate that the probability of a turnover is 73 percent lower for CEOs with five years remaining until contract expiration compared to CEOs who are in the year before contract expiration. Moreover, they find that the likelihood of a CEO turnover declines by 15 percent from the year before a contract renewal

to the year after a contract renewal. Similarly, González-Urbe and Groen-Xu (2017) find that CEO turnover rates within a fixed-term contract period are around 4 percent, whereas CEO turnover rates at contract expiration are significantly higher at 8 percent. Rusticus (2006) examines the association between severance agreements and CEO turnover. The study finds that higher equity-based severance payments are associated with lower CEO turnover. However, the results are conditional on having a severance agreement, whereas I examine CEO employment agreements instead of standalone severance agreements. Still, the study suggests that severance payments are important determinants of CEO turnover decisions.

To sum up, recessions allow the board of directors to evaluate the quality of the CEO and the CEO-firm match. CEOs with an explicit employment agreement are typically more costly to dismiss than CEOs with an implicit employment agreement. This leads to the prediction that explicit contracts offered more CEO job security during the 2007 financial crisis relative to implicit contracts. The contractual protection is expected to be the greatest for CEOs with long explicit contract horizons because severance payments usually increase with contract horizon. Therefore, I formulate the first hypothesis as follows:

*Hypothesis 1: Explicit CEO employment contracts offer more CEO job security during a financial crisis relative to implicit CEO employment contracts. Explicit CEO employment contracts with long horizons offer the greatest CEO job security.*

### **3.2 Firm Performance**

The finance literature has extensively studied the relationship between firm performance and CEO turnover. Most of the studies have found a close association between firm performance and CEO turnover. For instance, Jenter and Lewellen (2017) find that between 38 and 55 percent of the CEO turnovers in their sample are for performance reasons. They also document that boards assign more weight to recent performance than to performance in the past. Moreover, they report that the beliefs of the board about the quality of the CEO stay sensitive to performance and that this sensitivity slowly decreases with tenure.

Focusing on CEO employment contracts, the literature shows some controversial evidence of the effects of contract horizons on CEO behavior. On the one hand, long explicit CEO employment contract horizons may alleviate managerial risk aversion and encourage risky but value-creating decisions (see, e.g., Cziraki & Groen-Xu, 2018). On the other hand, long explicit CEO employment contract horizons reduce the threat of dismissal after poor firm performance, and hence may impose less discipline (see, e.g., Bertrand & Mullainathan, 2003).

The first strand of literature argues that longer contract horizons align the time horizon of the CEO and the shareholders. The contractual protection against dismissal alleviates CEO

risk aversion and motivates CEOs to make risky long-term strategies that are in the interest of the shareholders. For instance, Manso (2011) argues that job security is important to motivate innovation. Long contract horizons protect CEOs against dismissal after short-term losses and reward CEOs for successes in the long-term. In contrast, CEOs with shorter contract horizons sacrifice long-term projects to avoid short-term losses. Short-term losses would reduce short-term performance, and hence might increase the probability of dismissal.

Zhao (2013) finds that explicit CEO contracts motivate CEOs to make value-creating acquisitions. He reports that acquirers with an explicit CEO contract have higher announcement returns, pay lower premia, and undertake riskier acquisitions but with greater profitability. The findings support the idea that explicit employment contracts alleviate CEO risk aversion by protecting the CEO against downside risk.

The second strand of literature argues that CEO employment contracts increase agency costs. Again, the starting point is that (long) explicit CEO employment contracts protect CEOs against termination after poor firm performance. However, in contrast to incentives predictions, this field of research predicts that CEO contracts reduce the disciplining effect of the market for corporate control and internal governance mechanisms (Lehn & Zhao, 2006). That is, contractual protection reduces the personal costs of value-destroying decisions to CEOs, which may provide perverse incentives.

For instance, Bertrand and Mullainathan (2003) find that CEOs avoid complex decisions and costly efforts after the adoption of state antitakeover laws. Specifically, the increased protection of executives is not associated with more firm-specific or plant-specific investments. Moreover, subsequent to the passage of antitakeover legislation, the productivity and profitability of these firms declined. Another study on antitakeover laws by Masulis, Wang, and Xie (2007) reports lower announcement returns for acquirers with more antitakeover provisions. The findings suggest that CEOs of firms that are protected by more antitakeover provisions are less disciplined by the market for corporate control and are therefore more likely to make value-destroying acquisitions. Similarly, longer contract horizons may also protect CEOs against forced turnovers, which may lead to less discipline.

Another part of the finance literature focuses on CEOs with short contract horizons. Shorter CEO horizons may influence the performance of firms (McClelland, Barker, & Oh, 2012). Antia, Pantzalis, and Park (2010) find that shorter CEO horizons, measured by CEO tenure and age, are related to higher agency costs and lower firm performance. Several studies argue that short contract horizons lead to short-termism. Short-termism implies that CEOs cut long-term investments in order to meet short-term performance objectives (Chen, Cheng, Lo, & Wang, 2015). DeFond and Park (1997) argue that job insecurity motivates CEOs to smooth

earnings over time. Since the likelihood of dismissal is positively related to poor firm performance and corporate boards put more weight on recent firm performance, CEOs raise current earnings by 'borrowing' earnings from the future. Previous literature has shown that managerial short-termism can be alleviated by, for instance, long-term incentive compensation and monitoring by outside directors. However, many studies question the effectiveness of incentive-based compensation and monitoring (see, e.g., Cheng & Warfield, 2005; Duchin, Matsusaka, & Ozbas, 2010).

Several studies argue that CEO employment contracts could be an effective instrument to mitigate short-termism. Chen et al. (2015) find that explicit contracts can reduce the pressure to perform well in the short-run by protecting CEOs against short-term fluctuations in performance. The study confirms that CEO contractual protection reduces the likelihood of a CEO turnover after poor short-term firm performance. Moreover, they report that CEOs with larger contractual protection are less likely to cut R&D expenses to avoid short-term losses. Narayanan (1985) examines the relation between long-term contracts and short-term incentives. The study finds that incentives for short-term performance are inversely related to the contract horizon of the CEO. The study concludes that CEOs benefit more from future cash flows when their contract horizon is long, and hence CEOs with long contract horizons have less incentives for sacrificing long-term profits for short-term profits. Thus, long contract horizons may effectively reduce short-termism.

On the other hand, job insecurity may have a disciplinary effect on potential value-destroying behavior of the CEO. Cziraki and Groen-Xu (2018) empirically show that the probability of a CEO turnover is significantly higher at contract expiration. CEOs whose contract is up for renewal want to show a good firm performance. Therefore, CEOs may behave in the interest of shareholders in order to increase the probability of a contract renewal.

To sum up, prior literature reveals some conflicting evidence of the effects of CEO employment contract horizons on firm performance. (Long) explicit employment contracts may align the time horizon of the CEO and the shareholders, which could lead to long-term value-maximizing behavior. Alternatively, the contractual protection of (long) explicit contracts against dismissal may impose less discipline to the CEO. Based on the literature on short CEO contract horizons, CEOs with short contract horizons may focus on short-term performance, thereby sacrificing long-term profits. Conversely, the higher threat of dismissal for CEOs with short contract horizons may discipline CEOs to act in the interest of the shareholders. The potential effects are not mutually exclusive, and in this study, I attempt to examine which effect dominated during the financial crisis. To be consistent with the literature, the proxy for firm performance is the return on assets (Mehran, 1995). Hypothesis 2 is formulated as follows:

*Hypothesis 2: Firms with explicit CEO employment contracts perform better during a financial crisis relative to firms with implicit CEO employment contracts. Firms with long explicit CEO employment contract horizons perform the best.*

### **3.3 Corporate Investments**

One potential channel through which contractual protection affects corporate behavior is investments. Investment decisions can reveal whether job security affects the behavior of CEOs.

Stein (1989) is among the first to develop a theoretical model in which CEOs are more likely to avoid uncertain, potentially value-creating investments, if the value of the investments is only observable in the long-term. Long explicit CEO contract horizons might mitigate this investment problem. First, longer explicit contract horizons increase the probability that the value of an investment will be realized while the CEO is employed. Second, longer explicit contract horizons give CEOs more time to signal their qualities before a potential contract renewal, which may increase the willingness to make value-creating long-term decisions (González-Uribe & Groen-Xu, 2017).

Cziraki and Groen-Xu (2018) study the effects of CEO employment contract horizons on corporate policies. They show that employment contract horizons are positively associated to firm risk, and that capital expenditures are one potential source of this firm risk. According to their results, CEOs with a contract horizon of more than three years invest statistically and economically significant more than CEOs with a contract horizon of less than one year. Moreover, they report that capital expenditures significantly increase after contract renewals.

Xu (2009) finds some evidence for short-termism by CEOs with short contract horizons. According to the results, firms with short CEO contract horizons invest on average less compared to firms with long CEO contract horizons. However, the firms with short CEO contract horizons report a higher profitability. The findings are consistent with the idea that short-term oriented CEOs avoid long-term investments in order to maximize short-term value.

Major economic shocks, such as the 2007 financial crisis, make business opportunities uncertain. Consequently, firms become less willing to invest in risky long-term projects with uncertain returns. Campello, Graham, and Harvey (2010) survey 1,050 CFOs and find that more than half of the respondents stated that they canceled or deferred investments during the financial crisis. In this research, I expect that (long) explicit employment contracts alleviate CEO risk aversion and that CEOs with (long) explicit employment contracts are more willing to invest during a financial crisis compared to CEOs with relatively short contract horizons. Therefore, Hypothesis 3 is formulated as follows:

*Hypothesis 3: Firms with explicit CEO employment contracts reduce capital investments less during a financial crisis relative to firms with implicit CEO employment contracts. Firms with long explicit CEO employment contract horizons reduce capital investments the least.*

Another type of investments are R&D expenditures. R&D expenditures are usually considered as high-risk investments relative to capital investments. If (long) explicit CEO employment contracts provide incentives to take more risk, one approach to increase firm risk could be to decrease capital expenditures and to increase R&D expenditures (Coles, Daniel, & Naveen, 2006). Dechow and Sloan (1991) examine the relation between innovation and the career horizon of the CEO. They empirically test the behavior of CEOs and find that CEOs reduce R&D expenditures in their last years in office, which suggests a horizon problem. Similarly, González-Uribe and Groen-Xu (2017) argue that longer CEO contract horizons provide long-term incentives. They find that one extra year in the CEO contract horizon is associated with 3 percent more annual R&D expenditures.

The general view is that firms are less willing to invest in innovational projects at the time of a recession due to financial constraints and uncertainties of the economic landscape (Campello et al., 2010). In this study, I hypothesize that long contract horizons reduce aversion to innovational projects during a financial crisis, resulting in more innovation activities for firms with (long) explicit CEO employment contracts relative to firms with shorter CEO contract horizons. Hypothesis 4 is formulated as follows:

*Hypothesis 4: Firms with explicit CEO employment contracts reduce R&D expenditures less during a financial crisis relative to firms with implicit CEO employment contracts. Firms with long explicit CEO employment contract horizons reduce R&D expenditures the least.*

## **4. Data**

This section describes the sample construction process and discusses the data collection as well as the construction of the variables that are relevant for the empirical analysis.

### **4.1 Sample Construction**

The sample consists of U.S.-based firms of the S&P 500 between 2007Q1 and 2007Q3. I exclude utility firms (SIC 4,900 – 4,999), financial institutions (6,000 – 6,999), and services and public administration firms (SIC greater than 8,000). Besides that, the sample excludes firms that are delisted or acquired in 2007 and firms without available annual reports and proxy filings in the U.S. Securities and Exchange Commission (SEC) EDGAR database. This selection procedure results in a sample of 337 firms, as summarized in Panel A in Table 1.

**Table 1: Sample Selection Procedure**

This table summarizes the sample selection criteria that are applied to arrive at the final sample of 337 firms.

Panel A: S&P 500 - 2007 (Q1 to Q3)	525 firms
<i>Dropping:</i>	
Utility firms (SIC 4,900 - 4,999)	-36
Financial Services firms (SIC 6,000 - 6,999)	-98
Services and Public Administration firms (SIC 8,000 - 9,999)	-9
Firms delisted/acquired in 2007	-21
Firms without annual reports/proxy filings in the SEC EDGAR database	-24
<b>Total</b>	<b>337 firms</b>
<hr/>	
Panel B: Number of firm-year observations	
CEO data Execucomp	3,501
Financial data Compustat	3,576
Not matched observations	171
<b>Total matched observations</b>	<b>3,453</b>
<i>Dropping:</i>	
Observations fiscal year 2002	-319
Missing CEO variables	-34
Missing financial variables	-40
<b>Total number of firm-year observations</b>	<b>3,060 observations</b>

I use the variable “CEOANN” from Execucomp to identify the CEOs of the 337 firms for the fiscal year 2007. I also collect variables that specify the date the individual became CEO and the date the individual left the position of CEO. To account for the possibility that the financial crisis has been anticipated by some firms and has endogenously affected CEO replacement decisions, I consider a relatively early start date of the financial crisis which is October 1, 2007. I replace CEOs that left before 2007Q4 with their successor, but only if the new CEO was appointed before October 1, 2007. This results in a sample of 337 CEOs that were in office at October 1, 2007.

I manually collect explicit (written) CEO employment contract information from the U.S. SEC EDGAR database using the central index key (CIK) of the firms. Regulation S-K Item 402 requires that all public companies disclose material employment contracts or the contract terms with their named executive officers (Zhao, 2013). This requirement enables me to obtain an actual copy of explicit CEO employment contracts or information about the terms of contracts. Employment agreements define important aspects of the relationship between the firm and its CEO. However, there is substantial heterogeneity in explicit employment



agreements. I classify an agreement as an explicit CEO employment agreement when it covers at least three important CEO protection aspects, namely compensation, severance, and change-in-control. Agreements that are only effective upon the occurrence of an event, for instance change-in-control contracts, are not classified as explicit CEO employment contracts. Change-in-control contracts protect CEOs only if the employment is terminated by the acquirer in a hostile takeover, and therefore only cover a specific event (Schwab & Thomas, 2006).

The SEC EDGAR database contains many filings for many years and acquiring copies of explicit employment contracts from the SEC's public documents room requires to know the filing date. However, the filing date of CEO employment contract exhibits is not easily observable. Therefore, I use the WRDS SEC Analytics Suite – Filings Search, which searches the contents of SEC filings based on keywords, a filing time period and the CIK of the company. The main keyword I use is "employment agreement". Appendix A4 provides a detailed overview of the keyword search method. This method is a very effective approach to acquire actual copies of explicit CEO employment contracts.

The purpose is to obtain the explicit CEO employment contract that was in place for the CEO at the start of the financial crisis. To ensure the contract I acquire was effective on October 1, 2007, I search the proxy statements DEF-14 or DEF14A and the firm's annual reports of 2007 and 2008. These filings contain information about the terms of the CEO employment contract and typically mention the date of the most actual employment contract with the CEO. The terms and the date of the most actual CEO contract defined in these filings should correspond with those in the acquired copy of the explicit CEO employment contract. Appendix A5 and A6 show an example of an explicit CEO employment contract and a proxy statement that confirms that this explicit employment contract was effective at the start of the financial crisis.

Since the SEC requires that firms disclose material employment agreements with their named officers, I assume that all firms disclose when they have an explicit CEO employment agreement. The first indication that the CEO serves the firm with an implicit (non-written) employment agreement is when the keyword search does not report any result. In that case, I search the proxy statements DEF-14 or DEF-14A and the firm's annual reports of 2007 and 2008 because firms often expressly disclose that they do not have explicit employment contracts with their named executive officers. Appendix A7 provides an example. When firms do not expressly disclose that the CEO serves without an explicit employment agreement, but there is no indication of an explicit employment contract in the filings, I assume that the CEO has an implicit employment contract, because firms are violating SEC regulation when they do not disclose the explicit employment contract or the terms of the explicit employment contract.

The final sample consists of 337 firms. Table 2 reports the CEO employment agreement statistics. On October 1, 2007, 155 firms have an explicit CEO employment agreement (46.0 percent of the sample). 116 of the explicit contracts in the sample cover a fixed period. 64 explicit fixed-term contracts allow for renewal under specified conditions and 13 explicit fixed-term contracts evergreen on a daily or monthly basis. 39 of the explicit contracts are expressly for at-will employment. 182 firms have an implicit (non-written) employment contract with their CEO (54.0 percent of the sample), of which 142 firms expressly disclosed in the SEC filings that their CEO serves without an explicit employment agreement.

**Table 2: CEO Employment Agreement Statistics**

Panel A presents the number of explicit and implicit CEO employment contracts across the sample of 337 firms at the start of the financial crisis (2007Q4). Panel B categorizes the contracts.

Panel A: Explicit versus implicit contracts			
	Number	Percent	
Explicit CEO contract	155	46.0	
Implicit CEO contract	182	54.0	
Total	337	100.0	

Panel B: Contract type classification			
	Total	Fixed-term	At-will
Explicit CEO contract	155	116	39
		Classification of fixed-term contracts	
		<i>Standard:</i>	39
		<i>Automatic Renewal:</i>	64
		<i>Evergreening:</i>	13
		Expressly disclosed	Not expressly disclosed
Implicit CEO contract	182	142	40

## 4.2 Variable Construction

This subsection describes the variable construction. The complete list of variables and their definitions can be found in appendix B.

### 4.2.1 Employment Contract Variables

First, I create an *Employment Agreement* indicator for CEOs with an explicit employment contract, a *Fixed Contract* indicator for CEOs with an explicit fixed-term employment contract, an *At-will* indicator for CEOs with an explicit at-will employment contract, and an *Implicit* indicator for CEOs with an implicit employment contract.

The acquired explicit employment agreements are extensively read in order to collect the required information. First, I gather the *Starting Date* and the *Initial Expiration Date* of the

explicit employment contracts to construct the variable *Initial Term*. Around 40 percent of the explicit fixed-term contracts have an initial term of 3 years, as reported in Table D1 in appendix D.

Many explicit employment contracts contain an automatic renewal provision. In general, these contracts will be automatically extended on a *Renewal Date* (for example on the second anniversary of the *Starting Date*) and on *Renewal Periods* thereafter (for example on each anniversary of the *Renewal Date*), unless the firm or the CEO gives written notice that the employment period will not be extended. I collect information about the *Renewal Date*, *Renewal Periods*, and the *Renewal Term*. In total, 64 explicit fixed-term employment contracts contain a renewal provision. Most of these contracts (62) automatically renew every year after the *Renewal Date* (i.e. *Renewal Periods* of one year) and most agreements (44) have a *Renewal Term* of one year.

One specific type of a contract renewal provision is an evergreen provision. An evergreen provision allows for automatic renewals on a regular basis. This implies that the remaining time of the contract is substantially fixed at its initial duration until one of the parties terminates the contract. 13 explicit fixed-term contracts contain an evergreen provision, of which 10 renew daily and 3 monthly. I collect the fixed *Evergreening Term* of these contracts.

After gathering the relevant contract terms and expiration dates of the explicit fixed-term contracts, I construct the variable *Expiration Date*. This variable specifies the planned expiration date of the explicit fixed-term contract as of October 1, 2007. Thus, *Expiration Date* is equal to *Initial Expiration Date* if the fixed-term contract does not contain a renewal provision or if the *Renewal Date* has not been reached on October 1, 2007. When the *Renewal Date* has been reached, *Expiration Date* is set to the most recent renewal date (i.e. *Renewal Date* plus *Renewal Periods*) plus the *Renewal Term*. So, a contract with *Renewal Date* August 1, 2006, *Renewal Periods* of one year, and a *Renewal Term* of one year is assumed to have *Expiration Date* August 1, 2008, because its most recent renewal date on October 1, 2007, is August 1, 2007. The *Expiration Date* of contracts with an evergreen provision is set to October 1, 2007, plus the *Evergreening Term*. So, a contract with a daily evergreening three-year term is assumed to have October 1, 2010, as *Expiration Date*.

The variable *Expiration Date* is used to construct the variable *Contract Horizon*. *Contract Horizon* measures the duration of the explicit fixed-term employment contract that is left at the start of the financial crisis. Thus, an explicit fixed-term contract with *Expiration Date* October 1, 2009, has a *Contract Horizon* of two years. Table D2 in appendix D presents the distribution of *Contract Horizon*.

#### 4.2.2 CEO Variables

I gather additional CEO data for the time period 2002 – 2012 from Execucomp. Specifically, I collect the date the individual became CEO and the date the individual left as CEO, the CEO ID number, and *CEO Age*. This results in 3,501 firm-year observations. I construct *CEO Tenure* as the number of years the CEO is in office. *CEO Time Remaining* is defined as the number of years remaining as CEO, which is only constructed for the CEO in office at the start of the financial crisis. *CEO Total Term* represents the total number of years the CEO had been in office, which is again only constructed for the CEO in office at the start of the financial crisis. I use the CEO ID number to construct *CEO Turnover*, which indicates whether there was a CEO turnover in that fiscal year. Finally, I create an indicator *CEO Turnover 2008/2009*, which indicates whether there was a CEO turnover in 2008 or 2009.

#### 4.2.3 Financial Variables

The financial data are collected from Compustat's North America – Fundamentals Annual database. The financial variable construction corresponds to the approach of Almeida et al. (2011). The following variables are constructed: *Investment*, *R&D*, *Tobin's Q*, *Cash Flow*, *Size*, *Cash Holdings*, *Leverage*, and *ROA*. *Investment* is defined as the ratio of capital expenditures to the lag of property, plant and equipment (PPE). *R&D* is the ratio of research and development expenses to the lag of total assets. *Tobin's Q* is total assets plus market capitalization minus common equity divided by total assets. *Cash Flow* is the ratio of net income plus depreciation and amortization to the lag of property, plant and equipment. *Size* is the natural logarithm of total assets. *Cash Holdings* is defined as the ratio of cash and short-term investments to total assets. *Leverage* is defined as long-term debt plus debt in current liabilities over long-term debt plus debt in current liabilities plus the book value of common equity. *ROA* is the ratio of EBITDA to the lag of total assets. In a robustness test, I define *ROA* as the ratio of net income to the lag of total assets.

To make it into the final sample, I require the firm-year observation to have both CEO data from Execucomp and financial data from Compustat. Moreover, the observation should have non-missing values for *CEO Age*, *CEO Tenure*, *Tobin's Q*, *Cash Flow*, *Size*, *Cash Holdings*, *Leverage*, and *ROA*. The final sample consists of 3,060 firm-year observations. Finally, all financial variables are winsorized at the top and bottom one percent of the distribution to account for outliers. Panel B in Table 1 gives additional detail on the sample selection procedure.

## 5. Empirical Strategy

The empirical strategy in this paper is the difference-in-differences (DID) approach. This approach uses two groups, the treated and control group, and two periods, the pre- and post-period. If the treated and control group follow a similar trend in the pre-period, the control group serves as counterfactual and the difference between the two groups in the post-period reveals the treatment effect (Roberts & Whited, 2013).

I follow Almeida et al. (2011) and use the 2007 financial crisis to conduct a DID-analysis. The financial crisis provides for a shock to the economic environment that was generally unanticipated by firms. The advantage of using this shock is that the *ex ante* variation in CEO employment contracts is exogenous, because contract terms are predetermined at the start of the financial crisis and are relatively rigid (it is hard to recontract on the short-term).

One can think of various treatment assignment specifications. I choose to use two different treatment specifications in the baseline models. The first treatment specification assigns firms to the treated or control group based on the existence of an explicit CEO employment contract (hereafter: *Contract Treatment*). Treated firms are firms that have an explicit employment contract with their CEO at the start of the financial crisis. Control firms are firms with implicit employment contracts with their CEO. The second treatment specification assigns firms to the treated or control group based on the horizon of the CEO employment contract (hereafter: *Horizon Treatment*). Treated firms are firms with CEOs that have a *Contract Horizon* of at least two years, measured as of the start of the financial crisis. Control firms are firms with implicit employment contracts, firms with explicit at-will contracts, or firms with CEOs that have a *Contract Horizon* of less than two years. Thus, the CEOs of the control firms are assumed to have a relatively short contract horizon. In further analysis, I show the impact of two other treatment assignment specifications on the results.

The baseline models use two-year pre- and post-periods around the financial crisis. The pre-period is the fiscal years 2006 and 2007, and the post-period is the fiscal years 2008 and 2009. Additional models use three-years windows around the financial crisis, with 2005 to 2007 being the pre-period, and 2008 to 2010 being the post-period. The two-year window is expected to capture the real effects of the financial crisis better. That is because 2010 is already three years after the start of the crisis, and hence other events might affect firm outcomes. In a robustness test, I exclude the fiscal year 2007 from the pre-period as this is the transition year from non-crisis to crisis. In the analysis, I require firms to have at least one firm-year observations in the pre-period to ensure the validity of the DID-design. I do not require firms to have two firm-year observations in the pre-period, because this would significantly

reduce the number of observations. Also, I do not require firms to have at least one firm-year observation in the post-period, because this potentially results in a survivorship bias.

The estimated equation of the difference-in-differences models is as follows:

$$y_{i,t} = \beta_0 + \beta_1 * \text{Treatment}_i * \text{Post}_t + \beta_2 * \text{Treatment}_i + \beta_3 * \text{Post}_t + \gamma_i + \delta_t + \sum \beta_k * \text{Controls}_{i,t} + \varepsilon_{i,t} \quad (1)$$

I use four outcome variables  $y_{i,t}$  in various analyses, namely *CEO Turnover*, *ROA*, *Investment*, and *R&D*. The time-invariant firm heterogeneity, captured by  $\beta_2$ , is differenced away by including a treatment dummy. Also, common trends, captured by  $\beta_3$ , affecting both the treated and control group are differenced away by including a post dummy (Roberts & Whited, 2013). However, the treatment dummy will be absorbed by the firm fixed effects  $\gamma_i$  and the post dummy will be absorbed by the year fixed effects  $\delta_t$ . The coefficient of interest,  $\beta_1$ , is the interaction term between the treatment dummy and the post dummy, and captures the average change in the outcome variable, from the pre- to post-period, for the treated group *relative* to the control group. Thus, for instance in the firm performance analysis, the difference-in-differences estimator indicates whether firms with (long) explicit CEO employment contracts performed worse during the financial crisis relative to firms with implicit (or short explicit) CEO contracts. I compare the changes in outcome variables across the treated and control groups, rather than the levels of the outcome variables across the groups. This controls for the possibility that the outcome levels of the groups are different prior to the treatment and may remain different after the treatment. The models also include various control variables and  $\varepsilon_{i,t}$  represents the error term. Finally, the standard errors are robust, and I account for serial correlation by allowing for clustering of the error term at the firm level.

I need to address several remaining issues regarding the empirical strategy. First, the difference-in-differences approach assumes that the assignment of the treatment is random. This essentially means that the treated and control group should be relatively similar with the only difference being the CEO employment contract horizon at the start of the financial crisis (Roberts & Whited, 2013). If the treatment is randomly assigned across firms, it allows to compare the outcomes for the treated firms with the outcomes for the control firms. As mentioned earlier, the firm fixed effects capture time-invariant firm heterogeneity. However, other sources of firm heterogeneity may affect the treatment assignment and firm outcomes (Almeida et al., 2011). To alleviate this concern, I include covariates in the empirical analysis. The inclusion of these covariates is based on covariates balancing tests in the pre-treatment year. I compare the mean values of financials and CEO characteristics for treated and control firms in the pre-treatment year using a t-test.

Furthermore, I test for distributional differences by conducting Kolmogorov-Smirnov tests. I follow Almeida et al. (2011) and use as financial covariates *Tobin's Q*, *Cash Flow*, *Size*, *Cash Holdings*, *Leverage*, and *ROA*. In addition, I include *CEO Age* and *CEO Tenure* to the covariates balancing tests. The tests should reveal no statistically significant differences in the pre-treatment year. However, when the covariates are statistically significant different, I include these unbalanced covariates in the regression models to satisfy the conditional mean zero assumption (Roberts & Whited, 2013). In a robustness test, I include multiple covariates to the regression models to examine whether the results are stable. If the assignment of the treatment is random, including these covariates should have a negligible effect on the treatment estimator (Roberts & Whited, 2013).

Another related concern is the parallel trend assumption. This assumption is the key assumption behind the difference-in-differences approach. It requires that the treated and control group follow the same trend in the outcome variable prior to the treatment. If there are differences in trends between the treated and control firms prior to the treatment, the results might be incorrectly interpreted as being driven by the treatment (Roberts & Whited, 2013). Thus, only if the groups shared the same trend before the financial crisis, the differences in outcomes between the treated and control group during the financial crisis can be ascribed to the treatment. Although this assumption cannot be statistically tested, I visually analyze whether the parallel assumption is satisfied by plotting the outcome variables for both groups over time.

## 6. Results

This section starts with the summary statistics for the full sample and the subsamples of treated and control firms of the *Contract Treatment* and the *Horizon Treatment*. Thereafter, I discuss the results of the difference-in-differences analysis of job security, firm performance, and corporate investments. Finally, several tests check the robustness of the reported results.

### 6.1 Summary Statistics

The descriptive statistics for the full sample and for the subsamples of treated and control firms of the *Contract Treatment* are in Table 3. Panel A reports the employment agreement characteristics. As mentioned earlier, 46 percent of the CEOs in the full sample have an explicit employment agreement. The average *Initial Term* and average *Contract Horizon* of the 116 explicit fixed-term contracts in the sample are 3.29 and 1.81 years, respectively. Panel B presents the CEO characteristics for the CEOs that are in office at the start of the financial crisis. The average CEO is 55 years old, has a *CEO Tenure* of 6.27 years, a *CEO Time Remaining* of 5.33 years, and a *CEO Total Term* of 11.60 years. According to the *CEO Turnover 2008/2009* indicator, around 19 percent of the CEOs left in 2008 or 2009. The

number of observations for the *CEO Turnover 2008/2009* indicator is lower because 17 firms do not have firm-year observations in the data for fiscal years after 2007. Panel C reports the firm characteristics in the pre-treatment year, which is the last fiscal year-end before 2007Q4. The number of observations is slightly lower because 9 firms do not have a firm-year observation in the data with a fiscal year-end before 2007Q4.

Table 3 also reports the descriptive statistics for treated and control firms of the *Contract Treatment*. The number of treated firms is 155 and the number of control firms is 182. The last two columns report the results of the t-test and the Kolmogorov-Smirnov test. The purpose of these tests is to show that treated and control groups are relatively similar along observable characteristics in the year before treatment. The CEOs of the treated and control group are of similar age. However, treated CEOs have on average a shorter *CEO Tenure*, *CEO Time Remaining*, and *CEO Total Term* than control CEOs. These differences are statistically significant. The fact that CEOs with an explicit employment contract have, on average, less time remaining in office is somewhat surprising. Nevertheless, *CEO Turnover 2008/2009* indicates that 18.62 percent of the treated CEOs left during the crisis, compared to 19.43 percent of the control CEOs. This is a first – but weak – indication that treated CEOs experienced more job security during the crisis compared to control CEOs. Panel C shows that the observed firm characteristics are in balance, because the differences are economically small and statistically insignificant, except for *Size* and *ROA*. Treated firms are statistically significant smaller and have lower *ROA* in the pre-treatment year than control firms. The differences in *Size* and *ROA* may affect the selection into the treatment group and the post-crisis outcomes. Therefore, I control in the DID-analysis for *Size* and *ROA*.

Table 4 reports the summary statistics for treated and control firms of the *Horizon Treatment*. The number of treated firms is 51, which is lower than for the *Contract Treatment*. This is because the 65 firms with an explicit *CEO Contract Horizon* of less than two years shift to the control group, as well as the 39 firms with explicit at-will CEO contracts. Consequently, the control group increases from 182 to 286 firms. The explicit fixed-term CEO contracts of the 51 treated firms have an average *Initial Term* of 3.75 years and an average *Contract Horizon* of 2.90 years. As expected, the 65 explicit fixed-term CEO contracts of the control firms have a significantly lower average *Initial Term* of 2.92 years and a significantly lower average *Contract Horizon* of 0.97 year. Surprisingly, treated firms still have a shorter *CEO Tenure*, *CEO Time Remaining*, and *CEO Total Term*. However, the differences are not statistically significant anymore, which could also be due to the lower number of treated firms. Again, *CEO Turnover 2008/2009* indicates that a smaller fraction, namely 18.75 percent, of the CEOs of the treated firms left during the financial crisis, compared to 19.12 percent of the CEOs of the control firms. Nevertheless, this difference is economically very small and statistically insignificant.



**Table 3: Descriptive Statistics for the Contract Treatment**

This table presents CEO employment agreement (Panel A), CEO (Panel B), and firm characteristics (Panel C) for the 337 firms in the sample. The variables are defined in Appendix B. This table also presents the summary statistics for the *Contract Treatment*. CEO characteristics are measured as of 10/1/2007. Firm characteristics are observed in the last fiscal year-end before 10/1/2007. The number of observations is slightly lower for CEO Turnover 2008/2009 because 17 do not have observations for the years after 2007. The number of observations for the firm characteristics is slightly lower because 9 firms have no fiscal year-end before 10/1/2007. Two-tailed p-values for tests of differences in mean values are reported in parentheses below the t-Value. The p-value of Kolmogorov-Smirnov test is the exact p-value to test the (in)equality of the distribution.

	Total Sample			Firms with an explicit CEO employment agreement - Treated firms			Firms with an implicit CEO employment agreement - Control firms			Statistics for Tests of Differences between Mean Values and Distributional Differences	
	N	Mean	Median	N	Mean	Median	N	Mean	Median	t-Value	Kolmogorov-Smirnov Test P-Value
	Panel A: Employment Agreement Characteristics										
Employment Agreement	337	0.4599 (0.4991)	0.0000	155	1.0000 (0.0000)	1.0000	182	0.0000 (0.0000)	0.0000		
Initial Term	116	3.2888 (1.3256)	3.0000	116	3.2888 (1.3256)	3.0000					
Contract Horizon	116	1.8184 (1.2449)	1.7467	116	1.8184 (1.2449)	1.7467					
	Panel B: CEO Characteristics										
CEO Age	337	54.9555 (6.4278)	55	155	54.9097 (6.3126)	55	182	54.9945 (6.5414)	56	0.1209 (0.9038)	0.875
CEO Tenure	337	6.2730 (6.1611)	4.5859	155	5.2741 (5.1935)	3.7919	182	7.1236 (6.7775)	5.6235	2.8324*** (0.0049)	0.008***
CEO Time Remaining	337	5.3250 (3.6878)	4.6680	155	4.8260 (3.4935)	4.0739	182	5.7499 (3.8036)	5.4155	2.3226** (0.0208)	0.078*
CEO Total Term	337	11.5979 (6.9887)	10.4203	155	10.1001 (5.8169)	9.0267	182	12.8735 (7.6369)	11.5702	3.7785*** (0.0002)	0.001***
CEO Turnover 2008/2009	320	0.1906 (0.3934)	0	145	0.1862 (0.3906)	0	175	0.1943 (0.3968)	0	0.1829 (0.8550)	1.000
	Panel C: Firm Characteristics										
Tobin's Q	328	2.3140 (1.1447)	1.9787	148	2.2060 (1.1004)	1.9077	180	2.4028 (1.1755)	2.0694	1.5632 (0.1190)	0.289
Cash Flow	328	0.8556 (1.1649)	0.5790	148	0.8734 (0.9618)	0.5430	180	0.8410 (1.3111)	0.6064	-0.2580 (0.7966)	0.832
Size	328	9.1243 (1.1248)	9.0383	148	8.9881 (1.0765)	8.9121	180	9.2363 (1.1540)	9.1568	2.0115** (0.0451)	0.047**
Cash Holdings	328	0.1387 (0.1395)	0.0817	148	0.1432 (0.1398)	0.0900	180	0.1350 (0.1396)	0.0765	-0.5290 (0.5972)	0.735
Leverage	328	0.3197 (0.2267)	0.3076	148	0.3191 (0.2418)	0.2856	180	0.3202 (0.2142)	0.3158	0.0422 (0.9663)	0.436
ROA	328	0.1862 (0.0928)	0.1697	148	0.1802 (0.1050)	0.1582	180	0.1911 (0.0813)	0.1852	1.0370 (0.3007)	0.014**
Investment	328	0.2699 (0.1765)	0.2203	148	0.2611 (0.1773)	0.1996	180	0.2771 (0.1760)	0.2271	0.8177 (0.4141)	0.152

**Table 4: Descriptive Statistics for the Horizon Treatment**

This table presents CEO employment agreement (Panel A), CEO (Panel B), and firm characteristics (Panel C) for the 337 firms in the sample. The variables are defined in Appendix B. This table also presents the summary statistics for the *Horizon Treatment*. CEO characteristics are measured as of 10/1/2007. Firm characteristics are observed in the last fiscal year-end before 10/1/2007. The number of observations is slightly lower for CEO Turnover 2008/2009 because 17 do not have observations for the years after 2007. The number of observations for the firm characteristics is slightly lower because 9 firms have no fiscal year-end before 10/1/2007. Two-tailed p-values for tests of differences in mean values are reported in parentheses below the t-Value. The p-value of Kolmogorov-Smirnov test is the exact p-value to test the (in)equality of the distribution.

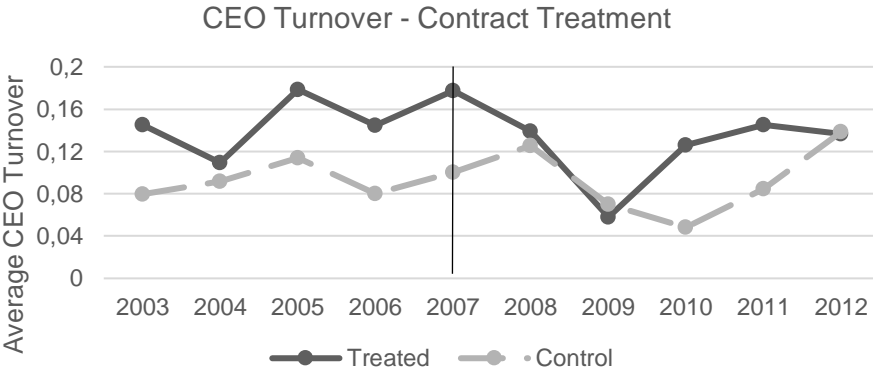
	Total Sample			Firms with an explicit fixed-term CEO employment agreement with a <i>Contract Horizon</i> >= 2 years - Treated firms			Firms with an implicit CEO employment agreement / explicit at-will contract / explicit fixed-term contract with a <i>Contract Horizon</i> < 2 years - Control firms			Statistics for Tests of Differences between Mean Values and Distributional Differences	
	N	Mean	Median	N	Mean	Median	N	Mean	Median	t-Value	Kolmogorov-Smirnov Test P-value
Employment Agreement	337	0.4599 (0.4991)	0.0000	51	1.0000 (0.0000)	1.0000	286	0.3636 (0.4819)	0.0000		
Initial Term	116	3.2888 (1.3256)	3.0000	51	3.7549 (1.1974)	3.0000	65	2.9231 (1.3147)	3.0000	-3.5565*** (0.0006)	0.004***
Contract Horizon	116	1.8184 (1.2449)	1.7467	51	2.8966 (1.0210)	2.7461	65	0.9724 (0.5632)	0.9500	-12.0925*** (0.0000)	0.000***
Panel B: CEO Characteristics											
CEO Age	337	54.9555 (6.4278)	55	51	55.2941 (6.6432)	55	286	54.8951 (6.3987)	55	-0.3973 (0.6924)	0.999
CEO Tenure	337	6.2730 (6.1611)	4.5859	51	5.9436 (6.0867)	3.8330	286	6.3317 (6.1830)	4.7461	0.4185 (0.6769)	0.278
CEO Time Remaining	337	5.3250 (3.6878)	4.6680	51	4.9777 (3.5057)	4.2491	286	5.3869 (3.7218)	4.9938	0.7606 (0.4494)	0.831
CEO Total Term	337	11.5979 (6.9887)	10.4203	51	10.9212 (6.9809)	9.0267	286	11.7186 (6.9954)	10.7091	0.7512 (0.4551)	0.281
CEO Turnover 2008/2009	320	0.1906 (0.3934)	0	48	0.1875 (0.3944)	0	272	0.1912 (0.3940)	0	0.0595 (0.9527)	1.000
Panel C: Firm Characteristics											
Tobin's Q	328	2.3140 (1.1447)	1.9787	47	2.3532 (1.3585)	1.9062	281	2.3075 (1.1076)	1.9957	-0.2190 (0.8274)	0.529
Cash Flow	328	0.8556 (1.1649)	0.5790	47	0.8841 (1.1088)	0.5016	281	0.8509 (1.1759)	0.5816	-0.1887 (0.8509)	0.332
Size	328	9.1243 (1.1248)	9.0383	47	8.9732 (0.9944)	9.0540	281	9.1496 (1.1449)	9.0357	1.1001 (0.2751)	0.435
Cash Holdings	328	0.1387 (0.1395)	0.0817	47	0.1283 (0.1440)	0.0851	281	0.1405 (0.1390)	0.0816	0.5395 (0.5915)	0.658
Leverage	328	0.3197 (0.2267)	0.3076	47	0.3758 (0.2625)	0.3540	281	0.3103 (0.2193)	0.3011	-1.6200 (0.1107)	0.276
ROA	328	0.1862 (0.0928)	0.1697	47	0.2014 (0.1190)	0.1683	281	0.1836 (0.0876)	0.1697	-0.9804 (0.3312)	0.523
Investment	328	0.2699 (0.1765)	0.2203	47	0.2579 (0.1838)	0.1996	281	0.2719 (0.1755)	0.2229	0.4834 (0.6305)	0.138

Panel C shows that the firm characteristics are in balance in the pre-treatment year and supports the assertion that treatment and control groups are comparable and only differ with respect to their CEO employment contract horizons. However, the insignificant differences could also be the consequence of the relatively low number of treated firms. Therefore, I also include *Size* and *ROA* in the DID-analysis for this treatment specification.

**6.2 DID-analysis: Effects on CEO Job Security**

The next subsections discuss the results of the difference-in-differences analysis. I start with the treatment effect on job security, measured by *CEO Turnover*. First, I visually explore the trends in CEO turnover over time for both treatment specifications. Second, I report the results of the difference-in-differences analysis.

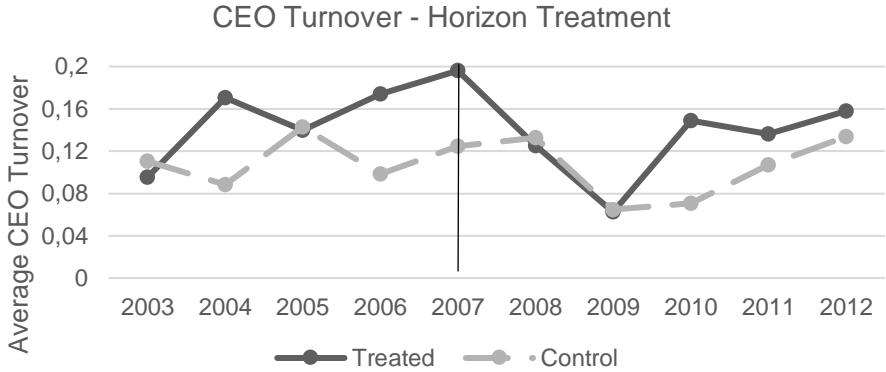
Figure 1 plots the average CEO turnover rates for the treated and control group of the *Contract Treatment* from 2003 to 2012. The figure shows that treated firms had higher absolute turnover rates than control firms in the years before financial crisis. However, the turnover rates moved approximately parallel in the pre-period. So, even though treated firms have a shorter *CEO Tenure*, a lower *ROA* and are smaller, there is no obvious violation of the parallel trend assumption in the pre-period. The figure clearly shows that the average CEO turnover rate for treated firms declined during the crisis, with the sharpest decline in 2009. This downward trend reversed after 2009, with a sharp increase in 2010. Conversely, the average turnover rate for control firms increased in 2008. In 2009, the turnover rate declined, but this decline seemed to be less pronounced relative to that of treated firms. The figure suggests that the CEO turnover rate for treated firms declined more compared to control firms, which implies that CEOs of treated firms had greater job security during the financial crisis than CEOs of control firms.



**Figure 1: CEO Turnover trend for the *Contract Treatment***

Figure 2 shows the trends for the treated and control group of the *Horizon Treatment*. Again, treated firms had higher absolute CEO turnover rates than control firms in the pre-period. The groups followed a somewhat non-parallel trend in the pre-period. For instance, the

CEO turnover rates moved in the opposite direction in 2005. Therefore, the treatment estimations of the DID-analysis should be interpreted with a little caution. In 2007, the average CEO turnover rates for both groups increased in the same manner. The average CEO turnover rate for treated firms declined sharply in 2008 and 2009, whereas control firms display a small increase in 2008 followed by a sharp decline in 2009. Again, treated firms reversed the pattern in 2010 with a sharp increase in the average CEO turnover rate. Based on the figure, it seems that CEOs of treated firms had more job security during the financial crisis.



**Figure 2: CEO Turnover trend for the *Horizon Treatment***

Table 5 presents the results of the DID-regression analysis. Models 1 to 4 report the results of the *Contract Treatment* and Models 5 to 8 report the results of the *Horizon Treatment*. The results are based on two-year and three-year time windows around the start of the financial crisis. The number of treated and control firms slightly differs across the two windows, which is the consequence of the restriction that firms should have at least one firm-year observation in the pre-period. Thus, for the *Contract Treatment* there is one treated firm and one control firm that only has an observation for 2005, but not for 2006 and 2007.

Model 1 reports that the DID-estimator of the *Contract Treatment* is negative and statistically significant at the 10 percent level. In Model 2, I include controls for *CEO Tenure*, *Size*, and *ROA*, because Table 3 suggests that these covariates are significantly different in the pre-treatment year. The treatment estimator in Model 2 indicates that the average CEO turnover rate for treated firms decreased by 7.24 percentage points more during the financial crisis relative to control firms. This is consistent with the observed trends in Figure 1. The treatment estimation is also economically meaningful. The average CEO turnover rate in the pre-period (2006 and 2007) is 12.27 percent for the full sample, 16.16 percent for the treated firms, and 9.01 percent for the control firms. The average CEO turnover rate in the post-period (2008 and 2009) is 9.78 percent for the full sample, 9.68 percent for the treated firms, and 9.86

**Table 5: Results CEO Turnover**

This table presents the results of the DID-analysis for the *Contract Treatment* (Models 1 to 4) and the *Horizon Treatment* (Models 5 to 8). The dependent variable is *CEO Turnover*. [-2;+2] is based on 2006 - 2007 (pre) to 2008 - 2009 (post). [-3;+3] is based on 2005 - 2007 (pre) to 2008 - 2010 (post). Robust t-statistics based on standard errors clustered by firm are within parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Model	1	2	3	4	5	6	7	8
Treatment	Contract	Contract	Contract	Contract	Horizon	Horizon	Horizon	Horizon
Dependent variable	CEO Turnover	CEO Turnover	CEO Turnover	CEO Turnover	CEO Turnover	CEO Turnover	CEO Turnover	CEO Turnover
Window	[-2;+2]	[-2;+2]	[-3;+3]	[-3;+3]	[-2;+2]	[-2;+2]	[-3;+3]	[-3;+3]
Treatment * Post	-0.0672* (-1.751)	-0.0724* (-1.923)	-0.0472 (-1.505)	-0.0564* (-1.739)	-0.0630 (-1.180)	-0.0675 (-1.223)	-0.0176 (-0.385)	-0.0333 (-0.637)
CEO Tenure		-0.0477*** (-7.407)		-0.0401*** (-8.144)		-0.0477*** (-7.374)		-0.0401*** (-8.117)
Size		0.0587 (1.121)		0.0416 (1.232)		0.0619 (1.192)		0.0453 (1.348)
ROA		-0.505*** (-2.749)		-0.273** (-1.969)		-0.487*** (-2.667)		-0.260* (-1.885)
Constant	0.106*** (6.513)	-0.0220 (-0.0466)	0.143*** (7.777)	0.0727 (0.242)	0.106*** (6.532)	-0.0543 (-0.117)	0.143*** (7.764)	0.0372 (0.125)
Observations	1,276	1,276	1,895	1,895	1,276	1,276	1,895	1,895
Treated Firms	153	153	154	154	51	51	51	51
Control Firms	181	181	182	182	283	283	285	285
Total Firms	334	334	336	336	334	334	336	336
Adjusted R-square	0.009	0.219	0.007	0.179	0.007	0.217	0.006	0.177
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

percent for control firms. Thus, the decline in CEO turnover rate during the financial crisis for treated firms is of relatively great magnitude. The estimator in Model 4 is based on the three-year window and is less negative, but still statistically significant at the 10 percent level. The less negative treatment estimator is probably the result of the sharp increase in CEO turnover rate for treated firms in 2010, as shown in Figure 1. However, moving further away from the financial crisis allows other confounding factors to influence CEO turnover rates and threatens the internal validity of the estimated treatment effect (Roberts & Whited, 2013). Therefore, I focus more on the two-year window results, which suggest that CEOs of the treated firms had significantly more job security during the financial crisis relative to control firms.

The DID-estimator of the *Horizon Treatment* in Model 5 is negative, but statistically insignificant with a t-statistic of -1.180. This is probably the consequence of a smaller number of treated firms. However, the size of the estimator is comparable to the size of the estimator in Model 1. Table 4 suggests that the covariates are in balance. However, in Model 6, I include some covariates to show that the result reported in Model 5 is stable. The treatment estimator indicates that the average CEO turnover rate for treated firms declined by 6.75 percentage

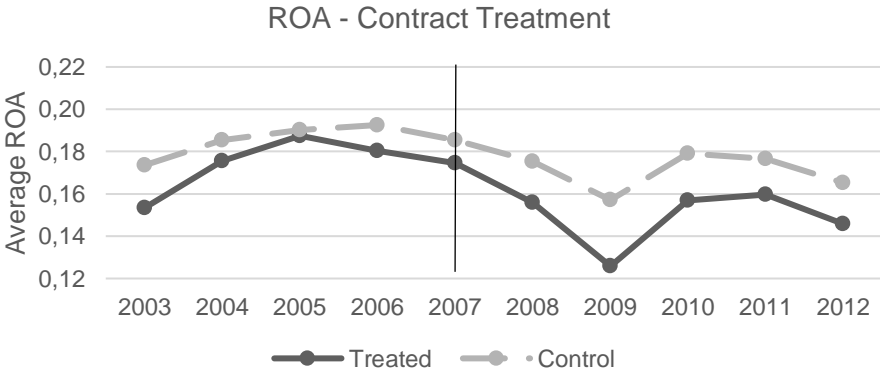
points more during the financial crisis relative to control firms. This estimation is also economically meaningful. However, the treatment estimator might be a little biased, because treated and control firms followed a somewhat non-parallel trend in the pre-crisis period, as illustrated in Figure 2.

To sum up, the findings of the *Contract Treatment* analysis suggest that CEOs with explicit employment contracts had more job security during the financial crisis relative to CEOs with implicit employment contracts. Moreover, the results of the *Horizon Treatment* analysis – weakly – suggest that CEOs with a longer explicit contract horizon had more job security relative to CEOs with shorter horizons. Overall, the results are consistent with Hypothesis 1.

**6.3 DID-analysis: Effects on Firm Performance**

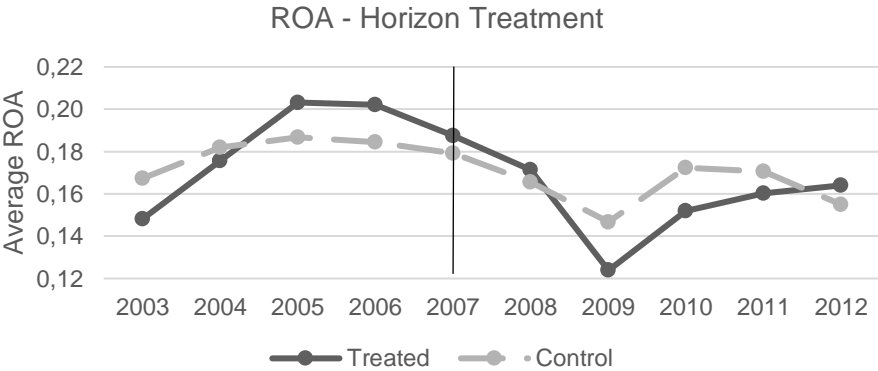
The previous subsection suggests that (long) explicit employment contracts offered more CEO job security during the financial crisis relative to short (explicit and implicit) contract horizons. The question that emerges is what the association between the greater CEO job security and firm performance is. In this section, I measure firm performance by ROA.

Figure 3 plots the average ROA for the treated and control group of the *Contract Treatment* from 2003 to 2012. As already shown in the summary statistics, treated firms had a lower average ROA than control firms in the pre-crisis year. This difference in the level of ROA is not problematic, because the treated and control group shared to a large extent the same trend in the pre-period. Therefore, the parallel trend assumption is plausibly satisfied. The figure illustrates that both treated and control firms performed worse during the financial crisis. However, the decline in ROA seemed to be more pronounced for the treated firms in both 2008 and 2009. The downward trend reversed for both groups in 2010. Overall, the figure suggests that treated firms performed worse during the financial crisis relative to control firms, which would be interesting because I find a larger decline in average CEO turnover rate for those treated firm relative to the control firms over the same period.



**Figure 3: ROA trend for the *Contract Treatment***

Figure 4 displays the trends for the treated and control group of the *Horizon Treatment*. The trends are obviously different in the period before the crisis. The treated firms show a somewhat cyclical pattern over the period 2003 to 2012, whereas the performance of control firms fluctuated less over time. Focusing on the financial crisis, the average ROA for treated firms declined sharply. Especially the performance in 2009 is poor. In absolute terms, the ROA declined from an average of 0.17 in 2008 to 0.12 in 2009. The control firms also performed worse during the financial crisis, but the change in firm performance seemed to be less extreme compared to that of the treated firms. Both groups recovered in 2010 and they reversed their trend in that year in the same manner. Figure 4 shows that is possible that the parallel trend assumption is violated, and I should therefore be very careful with the interpretation of DID-results.



**Figure 4: ROA trend for the *Horizon Treatment***

Models 1 to 4 in Table 6 present the DID-results of the *Contract Treatment*. In all model specifications, the treatment estimator is negative and statistically significant at the 10 percent level. The inclusion of *CEO Tenure* and *Size* in Models 2 and 4 has a negligible effect on the treatment estimator, which implies that the results are relatively stable. The treatment estimator in Model 2 indicates that the average ROA for treated firms declined by 1.32 percentage points more during the financial crisis relative to control firms. This estimation is also economically significant. The average ROA for the full sample is 18.36 percent in the pre-period (2006 and 2007), and 15.50 percent in the post-period (2008 and 2009). The average ROA declined from 17.73 percent in the pre-period to 14.05 percent in the post-period for the treated group, compared to an average decline from 18.90 to 16.68 percent for the control group. Therefore, the treatment estimation of 1.32 percent points is also economically sizeable. Overall, the results suggest that treated firms performed worse during the financial crisis relative to control firms.

**Table 6: Results Firm Performance**

This table presents the results of the DID-analysis for the *Contract Treatment* (Models 1 to 4) and the *Horizon Treatment* (Models 5 to 8). The dependent variable is ROA. [-2;+2] is based on 2006 - 2007 (pre) to 2008 - 2009 (post). [-3;+3] is based on 2005 - 2007 (pre) to 2008 - 2010 (post). Robust t-statistics based on standard errors clustered by firm are within parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Model	1	2	3	4	5	6	7	8
Treatment	Contract	Contract	Contract	Contract	Horizon	Horizon	Horizon	Horizon
Dependent variable	ROA	ROA	ROA	ROA	ROA	ROA	ROA	ROA
Window	[-2;+2]	[-2;+2]	[-3;+3]	[-3;+3]	[-2;+2]	[-2;+2]	[-3;+3]	[-3;+3]
Treatment * Post	-0.0141* (-1.924)	-0.0132* (-1.759)	-0.0132* (-1.791)	-0.0130* (-1.723)	-0.0131 (-1.146)	-0.0120 (-1.027)	-0.0150 (-1.249)	-0.0146 (-1.216)
CEO Tenure		-0.000358 (-0.637)		0.000210 (0.430)		-0.000361 (-0.637)		0.000206 (0.423)
Size		0.0193 (1.623)		0.00309 (0.321)		0.0200* (1.651)		0.00356 (0.369)
Constant	0.189*** (75.11)	0.0149 (0.138)	0.189*** (62.49)	0.160* (1.847)	0.189*** (74.95)	0.00888 (0.0807)	0.189*** (62.42)	0.156* (1.797)
Observations	1,276	1,276	1,895	1,895	1,276	1,276	1,895	1,895
Treated Firms	153	153	154	154	51	51	51	51
Control Firms	181	181	182	182	283	283	285	285
Total Firms	334	334	336	336	334	334	336	336
Adjusted R-squared	0.149	0.153	0.103	0.103	0.146	0.150	0.102	0.101
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Models 5 to 8 present the results of the *Horizon Treatment*. All models report a negative, but statistically insignificant treatment effect. The insignificance of the estimators is again possibly the result of the lower number of treated firms. However, the size of the estimators is comparable to the size of the estimators reported in Models 1 to 4 and, hence, are economically meaningful. According to Model 6, the average ROA for treated firms declined by 1.20 percentage points more during the financial crisis relative to control firms. However, this estimated treatment effect might be biased. As shown in Figure 4, the treated and control group followed a somewhat different trend in the period before the crisis, which could lead to an erroneous treatment inference. The average ROA for treated firms seemed to have moved more cyclical, and the downward trend during the financial crisis might partly be ascribed to this cyclical movement. However, this cyclical, non-treatment effect is unidentified. So, treated firms performed worse during the financial crisis relative to control firms, but this might only partly be the result of the treatment.

To sum up this section, the findings suggest that firms with explicit CEO employment contracts performed worse during the financial crisis relative to firms with implicit CEO contracts. This result is interesting, because the previous section has shown that CEOs with



explicit contracts had relatively more job security during the financial crisis. The prediction is that a relatively poor firm performance is associated with a higher probability of a CEO turnover, but I observe the opposite association. The results of the *Horizon Treatment* analysis – weakly – suggest that firms with longer explicit CEO contract horizons performed worse during the financial crisis compared to firms with relatively short (explicit and implicit) CEO contract horizons. This result is also interesting because I find that CEOs with longer explicit contract horizons had relatively more job security during the financial crisis. Overall, the results are inconsistent with Hypothesis 2.

Furthermore, the results seem to be inconsistent with the incentives predictions of (long) explicit CEO employment contracts. Instead, the results suggest that CEO job security might have led to perverse incentives and agency conflicts at the time of a financial crisis. Alternatively, the relatively poor firm performance might have been the result of long-term actions that in the short-term are observationally equivalent to low effort or bad CEO quality. If this is the true story, the results could still be consistent with the incentives role of (long) explicit CEO employment contracts. On the other hand, focusing on short CEO contract horizons, it might be that short contract horizons and the related relatively higher dismissal threat during the financial crisis incentivized CEOs to increase effort in more pronounced ways relative to CEOs with greater job security. Lastly, the reported relatively better performance of firms with short CEO contract horizons might have been the result of short-termism by CEOs during the financial crisis.

#### **6.4 DID-analysis: Effects on Corporate Investments**

The previous subsections suggest that (long) explicit employment contracts offered more CEO job security at the time of the financial crisis. However, the greater CEO job security is negatively related to the performance of the firm. In this section, I examine whether CEO contractual protection and contract horizons influenced corporate investment behavior during the financial crisis.

Figures C1 and C2 in appendix C show the *Investment* trends for the *Contract Treatment* and the *Horizon Treatment*, respectively. Figure C1 illustrates that the *Contract Treatment* groups followed the same, approximately parallel trend in the pre-crisis period. In contrast, Figure C2 shows more noise in the trends for the *Horizon Treatment* groups. Table 7 reports the results of the corporate investments DID-analysis. Models 1 to 4 have *Investment* (capital expenditures to lagged PPE) as outcome variable. The *Contract Treatment* estimator in Model 2 suggests that firms with explicit CEO employment contracts reduced investments statistically significant less – by 2.40 percentage points – during the financial crisis relative to firms with implicit CEO employment contracts. This is consistent with Hypothesis 3.

**Table 7: Results Corporate Investments**

This table presents the results of the DID-analysis for the *Contract Treatment* (Models 1 and 2, and 5 and 6) and the *Horizon Treatment* (Models 3 and 4, and 7 and 8). The dependent variables are *Investment* and *R&D*. The number of firms in the R&D analysis is lower due to missing data. [-2;+2] is based on 2006 - 2007 (pre) to 2008 - 2009 (post). [-3;+3] is based on 2005 - 2007 (pre) to 2008 - 2010 (post). Robust t-statistics based on standard errors clustered by firm are within parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Model	1	2	3	4	5	6	7	8
Treatment	Contract	Contract	Horizon	Horizon	Contract	Contract	Horizon	Horizon
Dependent variable	Investment	Investment	Investment	Investment	R&D	R&D	R&D	R&D
Window	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]
Treatment * Post	0.0147 (1.351)	0.0240** (2.331)	0.00108 (0.0814)	-0.00373 (-0.269)	0.00252 (0.954)	0.00161 (0.672)	0.00590 (1.636)	0.00609* (1.836)
CEO Tenure	0.000607 (0.702)	0.000240 (0.320)	0.000611 (0.709)	0.000222 (0.292)	-0.000214 (-0.848)	-0.000236 (-1.170)	-0.000219 (-0.885)	-0.000246 (-1.224)
Size	0.0596*** (2.733)	0.0579*** (3.668)	0.0581*** (2.623)	0.0554*** (3.410)	-0.000855 (-0.141)	-0.00481 (-1.384)	-0.00144 (-0.237)	-0.00500 (-1.470)
ROA	0.489*** (7.556)	0.526*** (7.611)	0.482*** (7.456)	0.516*** (7.440)	0.105*** (3.244)	0.0973*** (4.155)	0.107*** (3.289)	0.0980*** (4.231)
Constant	-0.370* (-1.858)	-0.381*** (-2.651)	-0.356* (-1.756)	-0.356** (-2.413)	0.0452 (0.799)	0.0745** (2.331)	0.0503 (0.888)	0.0761** (2.433)
Observations	1,271	1,890	1,271	1,890	893	1,328	893	1,328
Treated Firms	153	154	51	51	104	109	24	26
Control Firms	181	182	283	285	130	132	210	215
Total Firms	334	336	334	336	234	241	234	241
Adjusted R-squared	0.275	0.236	0.273	0.232	0.085	0.090	0.086	0.092
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The results of the *Horizon Treatment* analysis reported in Models 3 and 4 show a statistically insignificant and very low, inconclusive estimated treatment effect. This is surprising, because long explicit CEO contract horizons are predicted to induce the strongest investment incentives. It also – weakly – suggests that the estimated *Contract Treatment* effect reported in Models 1 and 2 can be ascribed to firms with relatively short explicit CEO contract horizons, which is inconsistent with Hypothesis 3.

Figures C3 and C4 in appendix C plot the *R&D* trends for the *Contract Treatment* and the *Horizon Treatment*, respectively. The *Contract Treatment* groups followed the same, approximately parallel trend until 2006. In 2007, treated firms increased R&D expenses on average, whereas control firms decreased R&D expenses. Therefore, the parallel trend assumption is clearly violated. During the financial crisis, treated and control firms seemed to have similar R&D expense patterns. This is also reflected in the very small and statistically insignificant *Contract Treatment* estimator, as reported in Models 5 and 6. However, all R&D model specifications have a small number of observed firms due to missing R&D expenditures

data. Because of the low number of observations and the clear violation of the parallel trend, I refrain from *Contract Treatment* inference.

The number of observed firms is even smaller for the *Horizon Treatment*. This treatment specification does not clearly violate the parallel trend assumption, as illustrated in Figure C4. The average level of R&D expenses for treated firms remained almost unchanged during the financial crisis, whereas control firms slightly reduced R&D expenses. This is also reflected in the positive treatment estimator, as reported in Models 7 and 8. However, the magnitude of the estimator is small. Because of the very low number of treated firms, I also refrain from *Horizon Treatment* inference. Therefore, I cannot confirm nor reject Hypothesis 4.

To sum up, I find no clear results that are consistent with Hypothesis 3 and Hypothesis 4. In terms of capital expenditures, I find some results that weakly suggest that firms with explicit CEO contracts reduce capital investments less during the financial crisis. However, this outcome can possibly be ascribed to firms with relatively short explicit CEO contract horizons, which in fact contradicts the predictions.

## **7. Robustness Tests**

This section presents a series of robustness tests to verify the robustness of the results reported in the previous sections. The findings suggest that explicit CEO employment contracts offered relatively more CEO job security during the financial crisis. However, the firms with these explicit CEO employment contracts performed worse relative to firms with implicit CEO employment contracts. I show that these findings are robust and internally consistent. Table D3 in appendix D reports the results of the robustness tests.

The first robustness test excludes fiscal year 2007 from the analysis, because 2007 can be considered as a transition year. The financial crisis officially began in 2007 with a crisis in the subprime mortgage market in the United States (Campello et al., 2010). Firms might have already anticipated an economic downturn during 2007 and have therefore – endogenously – replaced their CEO. Moreover, the fiscal year-end varies across firms, and hence it is debatable whether 2007 belongs to the pre- or post-crisis period. Models 1 to 4 show the results of DID-analysis leaving out fiscal year 2007. The treatment estimators are very close to the earlier reported estimators with only slightly smaller t-statistics. The influence of the assignment of fiscal year 2007 to the pre-period is therefore negligibly small.

Models 5 and 6 show that the firm performance findings are robust to the measurement of firm performance. The baseline models measure firm performance by EBITDA over lagged total assets. An alternative measurement of firm performance is net income over lagged total assets. This measurement excludes a few non-operating income additions of EBITDA. The

average net income over lagged total assets of the firms in the sample is 8.65 percent, which implies that the negative treatment estimators of 2.06 and 1.11 percentage points are again statistically and economically significant for both treatment specifications. To conclude, the finding that treated firms performed worse during the financial crisis relative to control firms is robust to an alternative firm performance measurement.

One important assumption of the DID-analysis is that treatment assignment is random. This essentially means that the treated and control group should have relatively similar characteristics that are relevant for the treatment assignment. Including covariates to the regression analysis can help if the groups are different (Roberts & Whited, 2013). I have already tested the balancing of some covariates and included the covariates that are not in balance in the pre-treatment year to the model specifications. In Models 7 to 10, I include more covariates to check for randomness. For instance, *CEO Age* may affect the decision of the board to renew the CEO contract or the length of the CEO employment contract and may also affect firm performance. The inclusion of more covariates results in smaller treatment estimators for both outcome variables and treatment specifications. However, the economic significance holds. Therefore, the robustness test suggests that treatment assignment is relatively random.

Furthermore, the covariates balancing tests reported in Table 3 raise concerns about size differences between treated and control firms of the *Contract Treatment*. Treated firms are significantly smaller than control firms in the year prior to the financial crisis. To alleviate concerns about size differences, I exclude very large control firms with a *Size* of at least 10.5 (which is the 90<sup>th</sup> percentile) in the pre-treatment year from the analysis. This restriction removes the statistically significant difference in *Size* between the treated and control group. Also, this restriction has a negligible effect on the treatment estimators and the t-statistics, as shown in Models 11 and 12. Thus, size differences do not significantly affect the estimated treatment effects.

## **7.1 Alternative Treatment Assignment Specifications**

The baseline treatment specifications assign firms to the treated group when the CEO has an explicit employment contract (*Contract Treatment*) or when the CEO has a long explicit employment contract horizon (*Horizon Treatment*), measured as of the start of the financial crisis. One can think of many other treatment assignments. I have experimented with several variations in treatment specifications. The decision to use *Contract Treatment* and *Horizon Treatment* for analysis purposes has not been an arbitrary choice, but has been based on economic intuition, parallel trend comparisons, and the covariates balancing tests. Nevertheless, it is important to verify the impact of alternative treatment assignments on the

results. Therefore, in this section I briefly discuss two alternative treatment assignment specifications and the influence on the empirical results.

As an alternative to the *Contract Treatment*, I consider a treated group that only includes firms with explicit fixed-term CEO employment contracts (*Alternative 1*). Consequently, the 39 firms with an explicit at-will CEO employment contract shift to the control group. The decision to initially assign those firms to the treated group has been based on a study by Schwab and Thomas (2006), in which they argue that explicit at-will employment contracts provide more contractual protection than implicit employment contracts, and are therefore more similar to explicit fixed-term employment contracts. However, in practice it could be that explicit at-will employment contracts are more comparable to implicit employment contracts in terms of job security than to explicit fixed-term employment contracts.

The (unreported) summary statistics are not considerably different from the statistics for the groups of the *Contract Treatment* as described in Section 6.1. CEOs of treated firms still have a significantly lower *CEO Tenure*, *CEO Time Remaining*, and *CEO Total Term*. The covariates balancing tests report statistically significant differences for *Tobin's Q* and *ROA* in the pre-crisis year. Figures C5, C6, and C7 in appendix C show the trends in *CEO Turnover*, *ROA*, and *Investment*, respectively. Table D4 in appendix D presents the results of the DID-analysis. Compared to the *Contract Treatment* estimators reported in Table 5, the CEO turnover treatment estimators of *Alternative 1* reported in Models 1 and 2 are lower and statistically insignificant. One potential explanation for the difference is that explicit at-will employment contracts offered job security more comparable to explicit fixed-term employment contracts. However, Figure C5 illustrates that the CEO turnover trends in the pre-period are noisy and not completely parallel, which makes inference difficult.

The patterns in average ROA for the *Alternative 1* treatment specification are comparable to the patterns in average ROA for the *Contract Treatment*, which is also reflected in an almost identical estimated treatment effect in Models 3 and 4. Thus, firms with explicit fixed-term CEO contracts performed worse during the financial crisis relative to firms with at-will CEOs. The estimated treatment effect of *Alternative 1* on investments is lower than the *Contract Treatment* estimator, and statically and economically insignificant, as reported in Models 5 and 6. This – weakly – suggests that explicit at-will CEOs had investment incentives during the financial crisis. To conclude, the results of this alternative treatment specification are reasonably consistent with the results of the *Contract Treatment*.

As an alternative to the *Horizon Treatment*, I restrict the sample to firms with explicit fixed-term CEO employment contracts only. Treated firms still have explicit CEO contracts with a *Contract Horizon* of at least two years. The control group now only includes firms with explicit

fixed-term CEO employment contracts with a *Contract Horizon* of less than two years (*Alternative 2*). So, firms with explicit at-will CEO employment contracts and firms with implicit CEO employment contracts are excluded from the sample. The consequence of this treatment assignment is a considerably lower number of treated firms (51) and control firms (65). The (unreported) summary statistics are interesting, because the CEO characteristics are more consistent with the predictions. CEOs of treated firms now have, on average, a longer *CEO Tenure*, *CEO Time Remaining*, and *CEO Total Term* than CEOs of control firms. However, *Leverage* and *ROA* are not in balance for this treatment specification. Moreover, as shown in Figures C8, C9, and C10 in appendix C, the trends in all three outcome variables are substantially non-parallel in the pre-period, so the treatment effects remain somewhat unidentified. Because of the lower number of observed firms, the imbalance of some covariates, and the clear violations of the parallel trend assumption, I have chosen *Horizon Treatment* as baseline treatment.

Table D5 in appendix D reports some interesting results. However, I infer with caution. Also, the t-statistics of the treatment estimators are very small, which is probably due to the very low number of observed firms. The *Alternative 2* treatment estimator reported in Model 1 suggests that CEOs of treated firms had more job security during the financial crisis relative to CEOs of control firms, which is not surprising because they had longer contract horizons. However, the estimated *Alternative 1* treatment effect on ROA is negative and economically very small, as reported in Model 3. This suggests that the relatively high CEO job security is not associated with relatively better firm performance. Moreover, the estimated *Alternative 1* treatment effect on corporate investments reported in Model 5 indicates that firms with longer CEO contract horizons reduced investments more during the financial crisis relative CEOs with short contract horizons. Overall, the findings are also not consistent with the incentive predictions of long CEO contract horizons. I acknowledge that the findings are somewhat weak due to the low number of observed firms and the non-parallel trend in the pre-crisis period.

## 7.2 CEO Age

Throughout this paper, CEO employment agreements have been examined as a potential instrument to mitigate career concerns, i.e. CEO job insecurity, and horizon problems. An alternative perspective on managerial career concerns and horizon problems is the distance to retirement. Therefore, this section uses the age of the CEO as a proxy for career concerns and career horizon. The age of the CEO at the start of the financial crisis is assumed to be exogenous, since changing the age of the CEO requires a replacement of the current CEO. The empirical approach in this section is identical to the approach in the previous sections. First, I briefly review the relevant literature. Subsequently, I describe the data

collection process of a new sample and the new treatment assignment specification. Finally, the last subsection discusses the results of the analysis.

In theory, career concerns could mitigate agency conflicts between managers and shareholders. Holmström (1999) is among the first to provide a model in which future career concerns influence managerial incentives. According to his model, managers exert effort in their early years in office because the labor market is assessing the quality of the managers at that time (Gibbons & Murphy, 1992). Managers realize that a poor performance increases the likelihood of dismissal and reduces outside employment opportunities, and hence they put in effort. In contrast to managers with long career horizons, his model predicts that managers closer to retirement care less about career prospects, and hence they reduce effort.

The theoretical prediction that CEO personal characteristics influence corporate policies has been tested in several studies. For instance, Serfling (2014) finds that older CEOs reduce risk-taking behavior through less risky investment policies. Similarly, Dechow and Sloan (1991) empirically show that older CEOs spend less on R&D in their last year in office. These findings are consistent with the prediction that older CEOs avoid long-term projects and instead focus on short-term gains. On the other hand, Brickley, Link, and Coles (1999) argue that career concerns do not end at CEO retirement. Many CEOs stay active after their retirement, for instance, through serving on corporate boards. They find that such opportunities are positively related to the performance of the CEO in the last years of employment. So, even CEOs close to retirement have career concerns that might motivate to exert effort.

A recent study by Jenter and Lewellen (2015) examines the relationship between CEO retirement preferences and takeovers. Their analysis uses the retirement-age of CEOs as a proxy for low career concerns. Takeovers provide valuable insights into career concerns because CEOs usually lose their position during or shortly after a takeover of their firm. For that reason, CEOs often resist a takeover. However, the personal costs of takeovers are lower to CEOs close to retirement, which may reduce their resistance to a takeover. Indeed, Jenter and Lewellen (2015) find that CEOs near the age of 65 are more likely to agree to a takeover. Thus, in contrast to some earlier studies, Jenter and Lewellen (2015) argue that agency conflicts do not increase with CEO age. Instead, many value-creating decisions, such as takeovers, are positively related to future personal costs to CEOs, which could be of lower importance to CEOs close to retirement.

The findings discussed in the previous sections indicate that (long) explicit employment contract horizons reduced CEO job insecurity and firm performance during the financial crisis. Thus, the findings suggest that career concerns, i.e. job insecurity, are positively related to firm performance. To be consistent with these previous findings, I hypothesize that firms with

relatively old CEOs, who have lower career concerns, performed worse during the financial crisis compared to firms with younger CEOs, who have larger career concerns. This prediction is consistent with the idea that greater career concerns mitigate agency conflicts between managers and shareholders (see, e.g., Holmström, 1999; Gibbons & Murphy, 1992).

### 7.2.1 Sample and Data

I collect a new, comprehensive panel of CEOs of the S&P 1,500 firms in the fiscal year 2007 from Execucomp. The sample excludes CEOs of utility firms, financial institutions, and services and public administration firms. Additional CEO data and financial data are collected for the period 2002 – 2012 from Execucomp and Compustat. The construction of the CEO variables and financial variables is identical to the variable construction for the initial sample, as described in Sections 4.2.2 and 4.2.3, and in appendix B. The new sample consists of 1,260 firms.

The treatment assignment specification in this section should distinguish firms with CEOs with low career concerns from firms with CEOs with large career concerns. Table D6 in appendix D reports the summary statistics for the full sample. I use the median *CEO Age* of 55 in the fiscal year 2007 to assign firms to the treated or the control group (*Age Treatment*). This assignment results in a treated group of 647 firms and a control group of 613 firms.

Table D6 also presents the summary statistics for the treated and control group, and the statistics for tests of differences in mean values (t-test) and distributional differences (Kolmogorov-Smirnov test). Panel A shows that the average *CEO age* for the treated firms is 60 years, whereas the average *CEO age* for the control firms is 49 years. Furthermore, *CEO Tenure* and *CEO Total Term* are significantly larger for treated firms. This is not surprising because older CEOs are typically more experienced and their ability is better observable from past performance, which might have led to a good CEO-firm match. Remarkably, *CEO Time Remaining* is significantly lower for CEOs of treated firms. This is also reflected in the *CEO Turnover 2008/2009* indicator of around 26 percent for treated firms, compared to only 13 percent for control firms. The indicator strongly suggests a higher CEO turnover rate for treated firms during the financial crisis than for control firms. The covariates balancing tests indicate that *Tobin's Q*, *Cash Flow*, *Size*, *Cash Holdings*, and *Leverage* are not in balance in the pre-crisis year. Therefore, I include these covariates as well as *CEO Tenure* and *ROA* in the DID-analysis.

### 7.2.2 Results

Figures C11 and C12 in appendix C plot the average *CEO Turnover* and *ROA* trends for the treated and control group over the period 2003 – 2012. The figures suggest that the treated and control group followed an approximately parallel trend in CEO turnover rates



(except in 2005) and ROA in the period before the financial crisis. So, there is no clear indication of a violation of the parallel trend assumption. In 2008, the groups changed their pattern in CEO turnover rate in opposite direction. That is, the average CEO turnover rate for treated firms increased from 7.40 percent in 2007 to 16.48 percent in 2008, whereas the average CEO turnover rate for control firms declined from 16.84 percent in 2007 to 8.14 percent in 2008. This difference in CEO turnover rate patterns during the financial crisis is also reflected in the highly statistically significant and extremely large treatment estimator of around 12.00 percentage points, as reported in Models 1 and 2 in Table D7 in appendix D.

The results suggest that older CEOs had less job security during the financial crisis relative to younger CEOs. However, one explanation for the huge increase in CEO turnover is that many CEOs left for retirement reasons during the financial crisis. Lumsdaine, Stock, and Wise (1996) find that CEOs commonly retire at the age of 65, which creates a discontinuous jump in CEO turnover rate at the age of 65. When I exclude firms with CEOs older than 62 in 2007 from the treated group, the average CEO turnover increased only from 9.79 percent in 2007 to 12.13 percent in 2008. Therefore, it is likely that the CEO turnover findings are partly driven by (mandatory) CEO retirements, which makes inference of the treatment difficult.

Figure C12 shows some interesting firm performance patterns during the financial crisis. The performance of the treated firms seemed to be worse compared to the performance of control firms. The treatment estimators reported in Models 3 and 4 in Table D7 confirm this observation. The estimated treatment effect is statistically and economically significant. According to the estimation in Model 3, the average ROA for treated firms declined by 1.17 percentage points more during the financial crisis relative to control firms. The relatively poor performance of firms with older CEOs during the financial crisis might have been driven by the relatively low career concerns of CEOs, and hence the findings can imply a horizon problem.

Nevertheless, it remains unclear whether the observed patterns in CEO turnover rates and firm performance can be ascribed to the treatment. If the identification strategy is correct, I should not find similar treatment effects on CEO turnover and firm performance during non-crisis periods. To investigate this, I conduct two placebo tests. The first placebo test replicates the strategy for 2003 and 2004 as pre-period and 2005 and 2006 as post-period using a CEO age of at least 55 in 2004 as treatment. The second placebo test replicates the strategy for 2009 and 2010 as pre-period and 2011 and 2012 as post-period using a CEO age of at least 55 in 2010 as treatment.

Figures C13 to C16 in appendix C show the placebo trends and Table D8 in appendix D reports the treatment estimators of the placebo tests. The trends in average CEO turnover rates in the post-period for the 2004 placebo test and the 2010 placebo test are almost identical

to the trends in the post-period for the baseline treatment. That is, the average CEO turnover rate for the treated group increased, whereas the average CEO turnover rate for the control group declined. This is also reflected in the positive treatment estimators, as reported in Models 1 and 3 in Table D8. Therefore, the identification strategy does not allow to completely attribute the observed changes in CEO turnover rates during the financial crisis to the *Age Treatment*.

As mentioned earlier, a plausible explanation is that (mandatory) CEO retirements influence the treatment outcome. Nevertheless, the estimated treatment effect of 12.00 percentage points for the baseline treatment is larger than the estimated treatment effect of 8.26 and 6.21 percentage points for the placebo 2004 and placebo 2010 test, respectively. Thus, it could still be that the changes in CEO turnover rates during the financial crisis are partly attributable to the treatment. However, based on the reported findings, it remains unclear what the exact size of the real treatment effect is. I acknowledge that one should use a different identification strategy to study the real effects, for instance, by using a more complex (triple) difference-in-differences technique.

The plots for the ROA placebo test in 2004 and 2010, shown in Figures C15 and C16, illustrate that treated and control groups followed similar trends in the pre-period as well as in the post-period. Especially the post-period trends in both plots are interesting. The figures suggest that the 2004 and 2010 placebo treatment leave ROA outcomes unaffected. The treatment estimators reported in Models 2 and 4 in Table D8 confirm this observation. The treatment estimator of both placebo treatments is economically very small and statistically insignificant. Thus, the placebo findings – weakly – support the assertion that the age of the CEO negatively affected firm performance during the financial crisis. The negative *Age treatment* effect on ROA is consistent with the predictions and suggests a potential agency conflict between older CEOs and shareholders, possibly due to lower CEO career concerns.

## **8. Conclusion and Discussion**

The primary objective of this paper is to examine whether CEO employment contracts affect CEO job security and firm performance at the time of a financial crisis. The study contributes to the understanding of the effects of contractual protection and contract horizons on CEO job security and firm performance by examining a unique, manually collected dataset of employment agreements for CEOs of the S&P 500 firms in office at the start of the financial crisis. The empirical strategy aims to overcome endogeneity concerns by conducting a difference-in-differences analysis using the 2007 financial crisis as event. I identify heterogeneity in CEO employment contract horizons by exploiting *ex ante* variation in employment agreements. The difference-in-differences approach helps to control for observed firm heterogeneity and time-invariant unobserved firm heterogeneity.

The findings support theoretical and empirical arguments regarding the contractual protection of CEO employment agreements against dismissal. Specifically, I find that CEOs with explicit employment contracts at the start of the financial crisis had greater job security during the financial crisis, i.e. a more pronounced decline in the probability of a CEO turnover, relative to CEOs with implicit employment contracts. Moreover, the findings suggest that CEOs with relatively long explicit employment contract horizons, measured as of the start of the financial crisis, had greater job security during the financial crisis relative to CEOs with shorter (explicit and implicit) employment contract horizons. The findings are statistically significant and economically meaningful. Overall, (long) explicit CEO employment contracts seem to be an effective instrument to provide job security to the CEO.

Further investigation of the effects of CEO employment contracts shows some interesting firm performance results. I find that firms with explicit CEO employment contracts performed worse during the financial crisis relative to firms with implicit CEO employment contracts. This finding also holds for firms with longer explicit CEO contract horizons relative to firms with shorter (explicit and implicit) CEO contract horizons. So, although (long) explicit CEO contracts seem to offer more job security, this CEO job security is not associated with relatively better firm performance. After conducting several robustness tests, the results hold.

One potential explanation for the findings is that (long) explicit contracts provide CEO job security that induces perverse CEO incentives. CEOs possibly realize that they are contractually better protected against dismissal after poor firm performance, and hence they may reduce effort. This story suggests an agency conflict as a result of (long) explicit employment contracts, instead of the aimed alignment of interests of CEOs and shareholders. Alternatively, the relatively poor firm performance might have been the result of long-term actions that in the short-term are observationally equivalent to low effort or bad CEO quality. If this is the true story, the findings could still be consistent with the incentives role of (long) explicit CEO employment contracts. Conversely, another side of the story could be that the constant dismissal threat experienced by CEOs with shorter contract horizons induces effort. Thus, short CEO employment contract horizons may have a disciplining effect on CEOs. Alternatively, the relatively better performance of firms with short CEO employment contract horizons might have been the result of short-termism.

Although the potential stories driving the results are interesting, the objective of this paper is not to examine *how* job security affected CEO behavior during the financial crisis, but merely to identify that CEOs with (long) explicit employment contracts behaved differently relative to CEOs with short (explicit and implicit) contract horizons. In further analysis, I examine whether CEO employment contract horizons affected capital expenditures and R&D

expenditures during the financial crisis. However, I find no consistent results, and hence it remains unidentified what corporate policies and decisions have contributed to the relatively poor performance of firms with (long) explicit CEO contract horizons. I leave the investigation of the channels through which job security affects firm performance for future research.

Furthermore, this paper raises questions about the effects of career concerns, measured by the age of the CEO, on job security and firm performance. The findings suggest that the probability of a CEO turnover for older CEOs increased more pronounced in the financial crisis than in non-crisis periods. However, treatment inference from the data is difficult because older CEOs also leave for (mandatory) retirement related reasons. Moreover, the findings suggest that firms with relatively old CEOs performed worse during the financial crisis compared to firms with younger CEOs. One explanation for the finding could be that agency conflicts increase with CEO age as career concerns diminish with CEO age. Although the identification strategy is somewhat weak, the insights of the analysis offer an interesting area for future research.

Overall, the findings contribute to the literature on CEO employment contracts. The identification strategy of this study allows to show a novel link between CEO employment contracts, CEO job security, and firm performance. Specifically, this study points to the importance of contract design for incentive purposes. Corporate boards should pay attention to the real (incentives) effects of contract types and contract horizons when they determine employment contract terms.

This paper has a few limitations. First, the empirical findings are difficult to extrapolate to non-crisis periods. The setting in this study is the 2007 financial crisis, and treatment effects might have been stronger during the financial crisis. However, the empirical strategy provides a simple and intuitive approach to deal with many endogeneity problems that such a study might face. Another downside is that this study examines relatively few firms. This raises concerns about the external validity. Nevertheless, the firms in the sample represent an important part of the U.S. corporate sector. In addition, the lack of statistically significant results in some analysis may in part reflect the relatively low number of observed firms. It could therefore be valuable to manually collect a larger dataset of CEO employment agreements, for instance for CEOs of the S&P 1,500 firms, to infer more accurately.

To conclude, this paper provides a novel link between CEO employment contracts, CEO job security, and firm performance at the time of a financial crisis. While more work needs to be done in order to conclusively determine how CEO employment contracts affect the behavior of CEOs, this study offers some valuable insights into the potential role of CEO employment contracts in providing job security and incentives.

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# Appendices

## Appendix A – CEO employment contract examples and search method

### A1. Raytheon Company, Form 10-K, filed February 22, 2007

*“Our employees, including our named executive officers, are employees at-will and do not have long-term employment contracts with us. The at-will employment status of our employees affords us the necessary flexibility to remove employees when appropriate under the circumstances.”*

### A2. Analog Devices, Inc., Form 8-K, filed November 11, 2005, Exhibit 10.1

*“5.3. Cause or Without Good Reason. If Executive's employment shall be terminated (i) by the Corporation with Cause, or (ii) by Executive without Good Reason, the Corporation shall pay Executive his Base Salary through the date of termination and accrued vacation pay, and shall have no further obligations to Executive under this Agreement.*

*5.4. Without Cause or With Good Reason. If Executive's employment shall be terminated (i) by the Corporation without Cause, or (ii) by Executive with Good Reason, the Corporation shall:*

*a. pay to Executive, to the extent not previously paid, Executive's Base Salary, and accrued vacation pay, through the date of termination;*

*b. pay Executive, in a lump sum in cash, within thirty (30) days after the date of termination, an amount equal to (i) his Base Salary at the time of termination plus his Target annual bonus (i.e., the agreed upon percentage of his Base Salary) for the fiscal year in which termination occurs, multiplied by (ii) a number equal to the lesser of (A) three (3) and (B) the number of full years (plus a fraction representing any partial year) remaining in the Employment Period immediately prior to such termination.”*

### A3. BMC Software, Inc., Form 10-K, filed June 14, 2001, Exhibit 10.7

*“3. At-Will Employment. (a) No Fixed Term. Executive's employment with the Employer shall be “at will,” and for no fixed term, such that either Executive or the Employer may terminate Executive's employment at any time for any or no reason, with or Without Cause or with or Without Good Reason, by giving notice as provided in Section 3(b) below.”*

### A4. Keyword search method

In the Filing Search of the WRDS SEC Analytics Suite, the contents of SEC filings are searched based on keywords. CIK is the identifier and the date ranges are set from the year the CEO came in office until 2008. I use numerous keywords, ranging from specific to more general keywords.

The list of keywords includes: "Employment agreement", "Employment contract", "Offer letter", "Employment letter", "Employment arrangement", "Management continuity", "Retention agreement", "Severance agreement", "Change-in-Control", "Employment", "Contract", "*last name CEO*".



**A5. ADC Telecommunications, Inc., Form 10-Q, filed September 12, 2003, Exhibit 10.E**

*"1.1 Employment. The Company hereby employs the Executive as President and Chief Executive Officer, and the Executive accepts such employment and agrees to perform services for the Company, for the period and upon the other terms and conditions set forth in this Agreement.*

*1.2 Term. Unless terminated at an earlier date in accordance with Article III of this Agreement, the term of this Agreement ("Term") shall be for a period of three (3) years, commencing on the Commencement Date, and shall extend through August 13, 2006. Thereafter, the Term shall be automatically extended for successive one-year periods unless either party objects to such extension by written notice to the other party at least sixty (60) days prior to the expiration of the initial Term or any extension of the Term.."*

**A6. ADC Telecommunications, Inc., DEF-14A Proxy Statement, filed January 16, 2008**

*"Employment and Severance Agreement with Robert E. Switz. We entered into an employment agreement with Mr. Switz in conjunction with his appointment as Chief Executive Officer effective August 13, 2003. The initial term of the employment agreement continued until August 13, 2006, at which time it began automatically to renew for successive one year periods unless either party elects to terminate the agreement."*

**A7. Target Corporation, DEF-14A Proxy Statement, filed April 9, 2007**

*"Employment Contracts:*

*None of our executive officers, including our CEO, have employment contracts for a guaranteed term of employment."*

## Appendix B – Variable description

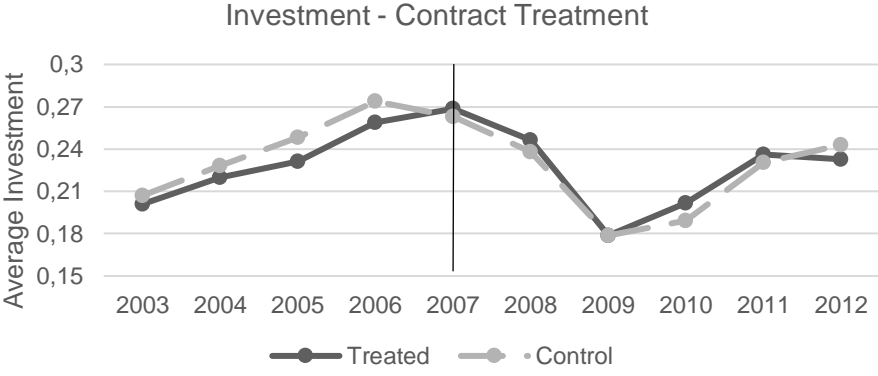
<b><u>Employment Contract Variables</u></b>		<b>Source</b>
Employment Agreement	Indicator which takes the value 1 if the CEO has an explicit employment contract, and 0 otherwise.	SEC filings
Fixed Contract	Indicator which takes the value 1 if the CEO has an explicit fixed-term employment contract, and 0 otherwise.	SEC filings
At-will	Indicator which takes the value 1 if the CEO has an explicit employment contract for at-will employment, and 0 otherwise.	SEC filings
Implicit	Indicator which takes the value 1 if the CEO has an implicit employment contract, and 0 otherwise.	SEC filings
Starting Date	Starting date of the employment agreement.	SEC filings
Initial Expiration Date	Date of the initial expiration of the employment agreement.	SEC filings
Initial Term	Year(Initial Expiration Date) minus Year(Starting Date).	SEC filings
Renewal Date	Date of the first automatic renewal.	SEC filings
Renewal Periods	Time until the next renewal date (in years).	SEC filings
Renewal Term	Time until the expiration date after a contract renewal (in years).	SEC filings
Evergreening Term	Fixed term until the expiration date of the contract (in years).	SEC filings
Expiration Date	Planned expiration date of the explicit fixed-term contract as of 10/1/2007. This variable is only constructed for explicit fixed-term employment contracts.	SEC filings
Contract Horizon	Duration of the explicit fixed-term contract left at the start of the financial crisis, measured by Expiration Date minus 10/1/2007 (divided by 365.25, in years). This variable is only constructed for explicit fixed-term employment contracts.	SEC filings
<b><u>CEO Variables</u></b>		
CEO Age	Age of the CEO (in years).	Execucomp
CEO Tenure	Current fiscal year minus the year the individual became CEO (in years).	Execucomp
CEO Time Remaining	Date CEO left minus 10/1/2007 (divided by 365.25, in years), where Date CEO left is set to 5/1/2019 if the CEO is the current CEO. This variable is only constructed for the CEO in office at the start of the financial crisis.	Execucomp
CEO Total Term	Date CEO left minus Date became CEO (divided by 365.25, in years), where Date CEO left is set to 5/1/2019 if the CEO is the current CEO. This variable is only constructed for the CEO in office at the start of the financial crisis.	Execucomp
CEO Turnover	Indicator which takes the value 1 if the CEO ID number of the CEO differs from the CEO ID number in the previous fiscal year, and 0 otherwise.	Execucomp
CEO Turnover 2008/2009	Indicator which takes the value 1 if there was a CEO turnover in 2008 or 2009, and 0 otherwise.	Execucomp
<b><u>Financial Variables</u></b>		
Investment	Ratio of capital expenditures (Compustat item 128) to the lag of net property, plant and equipment (item 8).	Compustat
R&D	Ratio of research and development expense (item 46) to the lag of total assets (item 6).	Compustat
Tobin's Q	Ratio of total assets (item 6) plus market capitalization (item 24 times item 25) minus common equity (item 60) to total assets (item 6).	Compustat

Cash Flow	Net income (item 188) plus depreciation and amortization (item 133) divided by the lag of property, plant and equipment (item 8).	Compustat
Size	The natural logarithm of total assets (item 8).	Compustat
Cash Holdings	Ratio of cash and short-term investments (item 1) to total assets (item 8).	Compustat
Leverage	Long-term debt (item 9) plus debt in current liabilities (item 34) over long-term debt (item 9) plus debt in current liabilities (item 34) plus the book value of common equity (item 60).	Compustat
ROA	Ratio of EBITDA (item 18) to the lag of total assets (item 6). Only in the robustness test, ROA is defined as the ratio of net income (item 172) to the lag of total assets (item 6).	Compustat

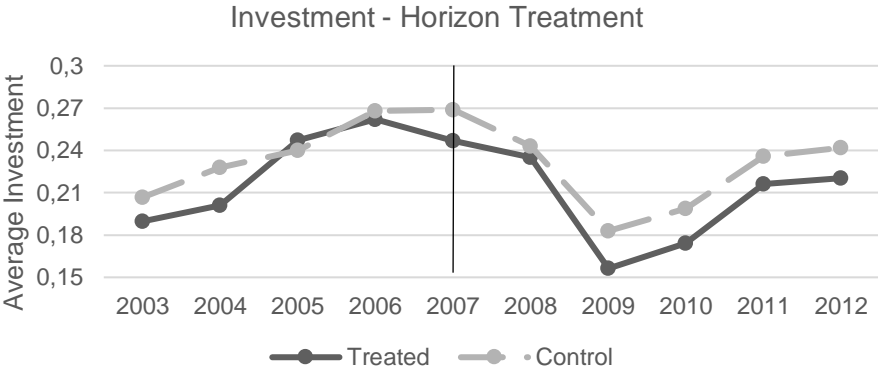
### **Treatment Specifications**

Contract Treatment	Treated firms have an explicit CEO employment contract. Control firms have an implicit CEO employment contract.
Horizon Treatment	Treated firms have an explicit fixed-term CEO employment contract with a <i>Contract Horizon</i> of at least two years, measured as of 10/1/2007. Control firms have an implicit CEO employment contract / explicit at-will contract / explicit fixed-term CEO employment contract with a <i>Contract Horizon</i> of less than two years, measured as of 10/1/2007.
Alternative 1	Treated firms have an explicit fixed-term CEO employment contract. Control firms have an implicit CEO employment contract / explicit at-will CEO employment contract.
Alternative 2	Treated firms have an explicit fixed-term CEO employment contract with a <i>Contract Horizon</i> of at least two years, measured as of 10/1/2007. Control firms have an explicit fixed-term CEO employment contract with a <i>Contract Horizon</i> of less than two years, measured as of 10/1/2007.
Age treatment	Treated firms have a CEO with an age of at least 55 in 2007. Control firms have a CEO with an age below 55 in 2007.
Placebo 2004	Treated firms have a CEO with an age of at least 55 in 2004. Control firms have a CEO with an age below 55 in 2004.
Placebo 2010	Treated firms have a CEO with an age of at least 55 in 2010. Control firms have a CEO with an age below 55 in 2010.

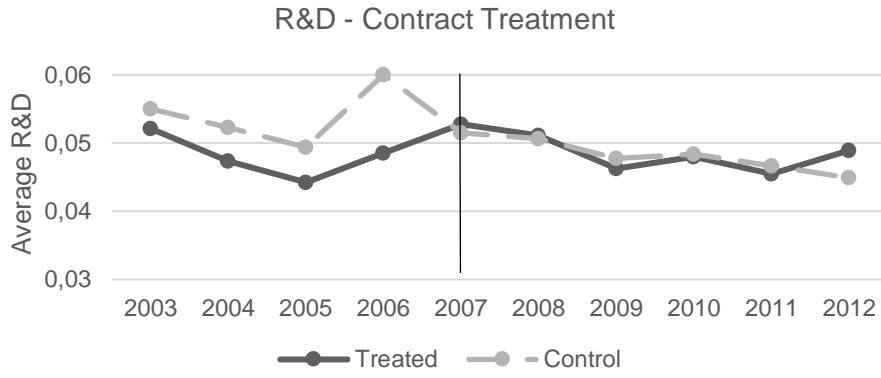
**Appendix C – Additional figures**



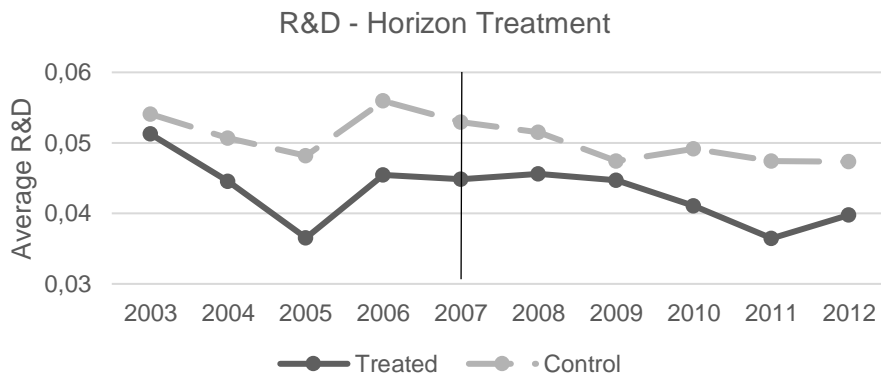
**Figure C1: Investment trend for the *Contract Treatment***



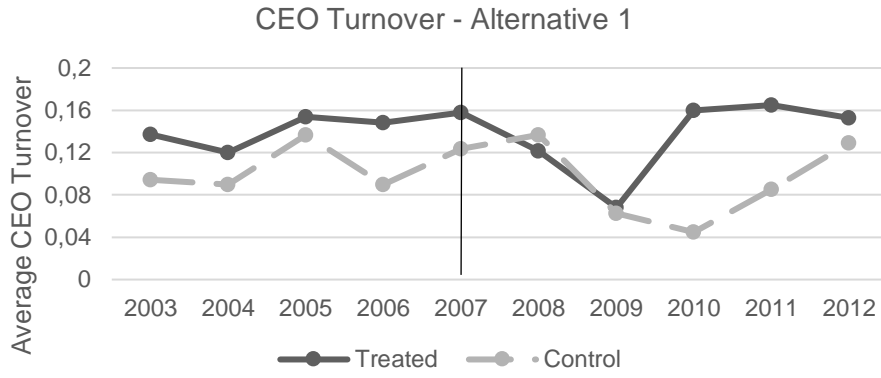
**Figure C2: Investment trend for the *Horizon Treatment***



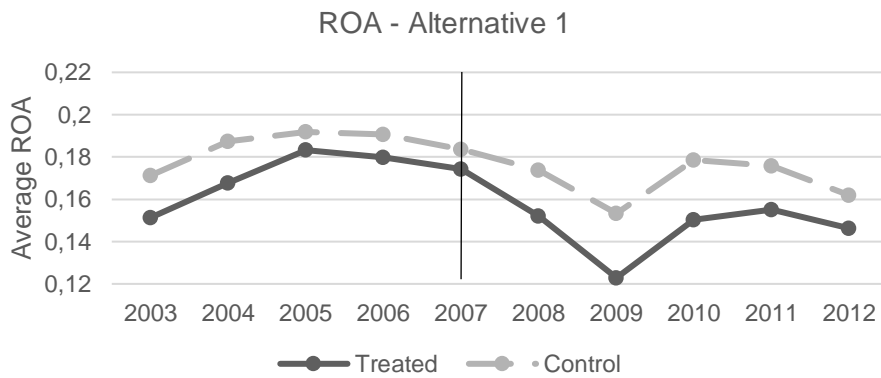
**Figure C3: R&D trend for the *Contract Treatment***



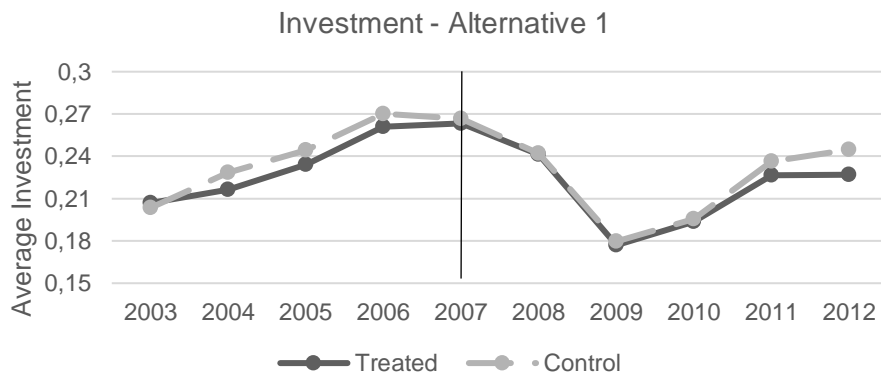
**Figure C4: R&D trend for the *Horizon Treatment***



**Figure C5: CEO Turnover trend for *Alternative 1***



**Figure C6: ROA trend for *Alternative 1***



**Figure C7: Investment trend for *Alternative 1***

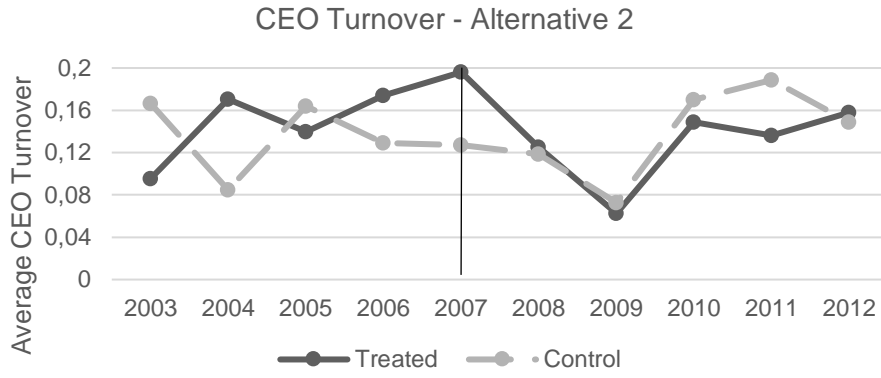


Figure C8: CEO Turnover trend for *Alternative 2*

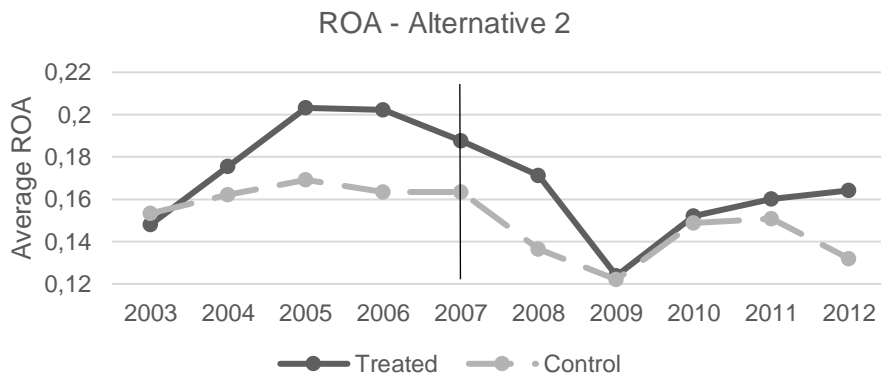


Figure C9: ROA trend for *Alternative 2*

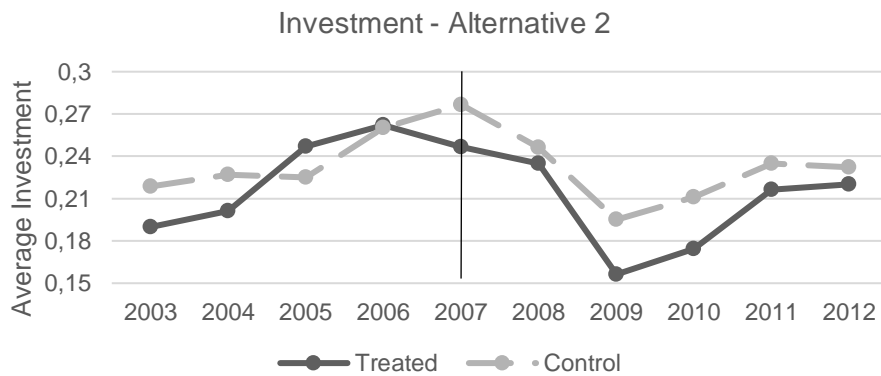
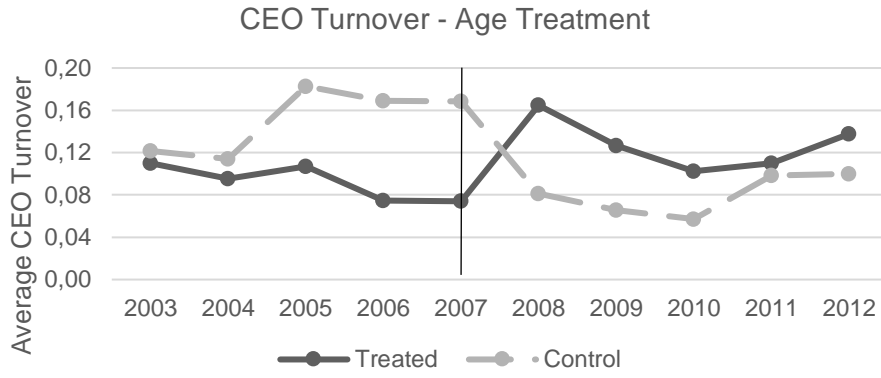
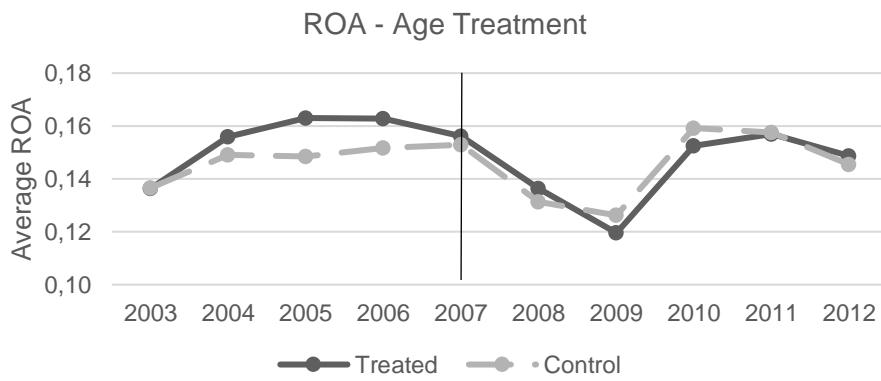


Figure C10: Investment trend for *Alternative 2*

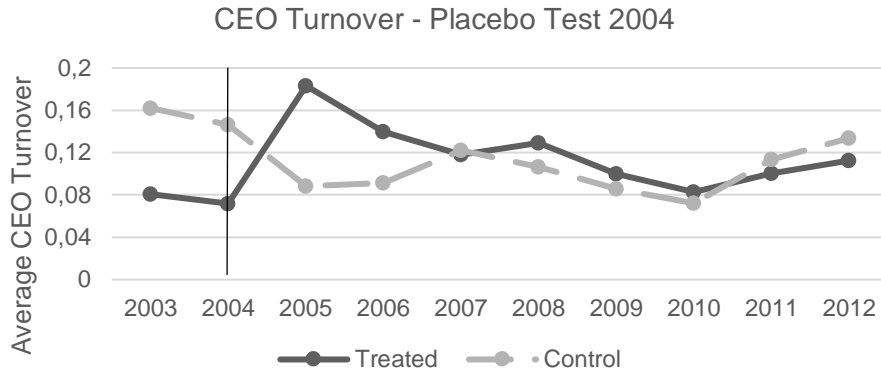


**Figure C11: CEO Turnover trend for the *Age Treatment***

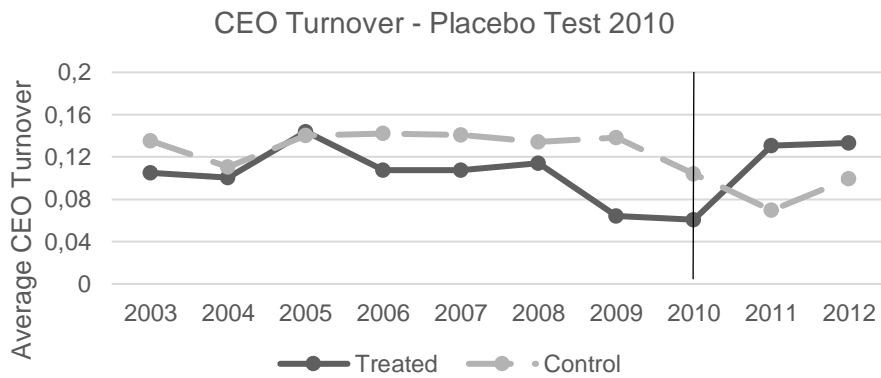


**Figure C12: ROA trend for the *Age Treatment***

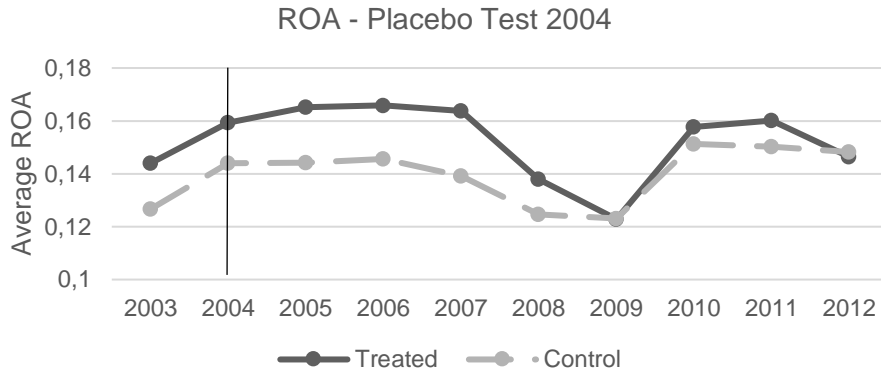




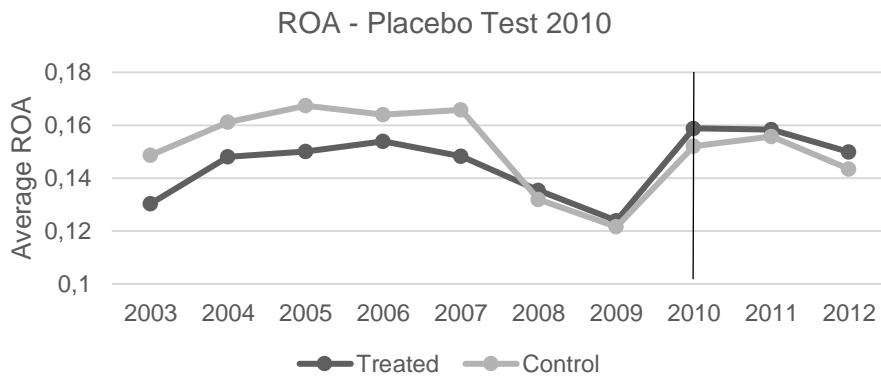
**Figure C13: CEO Turnover trend for the Placebo Test in 2004**



**Figure C14: CEO Turnover trend for the Placebo Test in 2010**



**Figure C15: ROA trend for the Placebo Test in 2004**



**Figure C16: ROA trend for the Placebo Test in 2010**

## Appendix D – Additional tables

**Table D1: Distribution of Initial Term**

This table presents the distribution of the initial contract term for the sample of 116 firms with an explicit fixed-term CEO employment agreement at the start of the financial crisis (2007Q4).

Initial Term	Count	%	Cumulative
0	2	1.72	1.72
1	9	7.76	9.48
2	20	17.24	26.72
3	48	41.38	68.10
4	17	14.66	82.76
5	17	14.66	97.41
6	1	0.86	98.28
7	1	0.86	99.14
8	1	0.86	100.00
Total	116	100.00	

**Table D2: Distribution of Contract Horizon**

This table presents the distribution of the contract horizon for the sample of 116 firms with an explicit fixed-term CEO employment agreement at the start of the financial crisis (2007Q4).

Contract Horizon	Count	%	Cumulative
0	11	9.48	9.48
1	34	29.31	38.79
2	36	31.03	69.83
3	25	21.55	91.38
4	7	6.03	97.41
5	2	1.72	99.14
6	-	-	-
7	-	-	-
8	1	0.86	100.00
Total	116	100.00	

**Table D3: Robustness Tests**

This table presents the results of several robustness tests. Models 1 to 4 leave out fiscal year 2007 from the analysis. Models 5 and 6 use a different firm performance measurement, namely net income divided by lagged assets. Models 7 to 10 include covariates to the regression specification. Models 11 and 12 exclude the largest control firms from the analysis. [-2,+2] is based on 2006 - 2007 (pre) to 2008 - 2009 (post), [-3,+3] is based on 2005 - 2007 (pre) to 2008 - 2010 (post). Robust t-statistics based on standard errors clustered by firm are in parentheses. \*\*\*, \*\*, \*, and \* indicate significance at the 1%, 5%, and 10% level, respectively.

Robustness Test	Performance Measurement						Including covariates						Excluding largest control firms					
	Leaving out fiscal year 2007		5		6		7		8		9		10		11		12	
Model	Contract	Horizon	Contract	Horizon	Contract	Horizon	Contract	Horizon	Contract	Horizon	Contract	Horizon	Contract	Horizon	Contract	Horizon	Contract	Horizon
Dependent variable	CEO Turnover	CEO Turnover	Net Income	Net Income	CEO Turnover	CEO Turnover	Net Income	Net Income	CEO Turnover	CEO Turnover	Net Income	Net Income	CEO Turnover	CEO Turnover	Net Income	Net Income	CEO Turnover	CEO Turnover
Window	[-3,+3]	[-3,+3]	[-3,+3]	[-3,+3]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]	[-2,+2]
Treatment * Post	-0.0579 (-1.586)	-0.0348 (-0.564)	-0.0133 (-1.553)	-0.0143 (-1.058)	-0.0206*** (-2.629)	-0.0111 (-1.068)	-0.0679* (-1.839)	-0.0538 (-1.014)	-0.0402*** (-1.839)	-0.0538 (-1.014)	-0.00990 (-1.494)	-0.00735 (-0.761)	-0.00735 (-0.761)	-0.00990 (-1.494)	-0.0737* (-1.924)	-0.00735 (-0.761)	-0.0737* (-1.924)	-0.0139* (-1.804)
CEO Age																		
CEO Tenure	-0.0335*** (-6.375)	-0.0334*** (-6.356)	0.000263 (0.452)	0.000268 (0.460)	-0.00104* (-1.709)	-0.00104* (-1.685)	-0.0400*** (-4.783)	-0.0402*** (-4.792)	-0.0402*** (-4.792)	-0.0402*** (-4.792)	-0.000457 (-0.779)	-0.000457 (-0.820)	-0.000457 (-0.820)	-0.000457 (-0.779)	-0.0466*** (-6.931)	-0.000484 (-0.820)	-0.0466*** (-6.931)	-0.000280 (-0.471)
Tobin's Q																		
Cash Flow																		
Cash Holdings																		
Size	0.0211 (0.598)	0.0235 (0.660)	-0.00256 (-0.221)	-0.00239 (-0.205)	0.0560*** (3.787)	0.0576*** (3.807)	0.0722 (1.339)	0.0761 (1.428)	0.0761 (1.428)	0.0761 (1.428)	0.0184* (1.667)	0.0190* (1.701)	0.0190* (1.701)	0.0184* (1.667)	0.0722 (1.352)	0.0183 (1.461)	0.0722 (1.352)	0.0183 (1.461)
Leverage																		
ROA	-0.256* (-1.710)	-0.243 (-1.625)																
Constant	0.215 (0.686)	0.190 (0.604)	0.212** (2.029)	0.210** (2.002)	-0.414*** (-3.067)	-0.428*** (-3.102)	0.502 (0.815)	0.446 (0.731)	0.446 (0.731)	0.446 (0.731)	-0.0388 (-0.349)	-0.0474 (-0.422)	-0.0474 (-0.422)	-0.0388 (-0.349)	-0.148 (-0.315)	-0.0423 (-0.217)	-0.148 (-0.315)	0.0243 (0.217)
Observations	1,535	1,535	1,535	1,535	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276	1,276
Treated Firms	147	46	147	46	153	51	153	51	153	51	153	51	153	51	153	51	153	51
Control Firms	179	280	179	280	181	283	181	283	181	283	181	283	181	283	181	283	181	283
Total Firms	326	326	326	326	334	334	334	334	334	334	334	334	334	334	306	306	306	306
Adjusted R-squared	0.143	0.141	0.109	0.107	0.150	0.142	0.231	0.228	0.228	0.228	0.271	0.269	0.269	0.271	0.214	0.214	0.214	0.150
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

**Table D4: Results *Alternative 1***

This table presents the results of the DID-analysis for Alternative 1. The dependent variables are *CEO Turnover*, *ROA*, and *Investment*. [-2;+2] is based on 2006 - 2007 (pre) to 2008 - 2009 (post). [-3;+3] is based on 2005 - 2007 (pre) to 2008 - 2010 (post). Robust t-statistics based on standard errors clustered by firm are within parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Model	1	2	3	4	5	6
Treatment	Alternative 1	Alternative 1	Alternative 1	Alternative 1	Alternative 1	Alternative 1
Dependent variable	CEO Turnover	CEO Turnover	ROA	ROA	Investment	Investment
Window	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]
Treatment * Post	-0.0625 (-1.534)	-0.0149 (-0.440)	-0.0147* (-1.937)	-0.0145* (-1.943)	0.00678 (0.630)	0.00969 (0.932)
Tobin's Q	0.000146 (0.00633)	-0.00108 (-0.0650)	0.0246*** (3.928)	0.0292*** (5.969)	-0.0136 (-1.649)	-0.00611 (-0.712)
ROA	-0.431** (-2.132)	-0.292* (-1.951)			0.527*** (7.457)	0.539*** (7.045)
Constant	0.187*** (2.685)	0.201*** (4.313)	0.132*** (9.342)	0.121*** (10.01)	0.201*** (10.41)	0.155*** (7.805)
Observations	1,276	1,895	1,276	1,895	1,271	1,890
Treated Firms	115	115	115	115	115	115
Control Firms	219	221	219	221	219	221
Total Firms	334	336	334	336	334	336
Adjusted R-squared	0.011	0.007	0.190	0.165	0.264	0.218
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

**Table D5: Results *Alternative 2***

This table presents the results of the DID-analysis for Alternative 2. The dependent variables are *CEO Turnover*, *ROA*, and *Investment*. [-2;+2] is based on 2006 - 2007 (pre) to 2008 - 2009 (post). [-3;+3] is based on 2005 - 2007 (pre) to 2008 - 2010 (post). Robust t-statistics based on standard errors clustered by firm are within parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Model	1	2	3	4	5	6
Treatment	Alternative 2		Alternative 2		Alternative 2	
Dependent variable	CEO Turnover	CEO Turnover	ROA	ROA	Investments	Investments
Window	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]
Treatment * Post	-0.0318 (-0.480)	-0.0175 (-0.307)	-0.00283 (-0.196)	-0.00627 (-0.434)	-0.00968 (-0.591)	-0.0224 (-1.385)
Leverage	-0.271 (-1.430)	-0.120 (-0.887)	-0.0957** (-2.453)	-0.0946*** (-3.133)	-0.0324 (-0.589)	-0.0292 (-0.667)
ROA	-0.854** (-2.399)	-0.591** (-2.354)			0.567*** (6.088)	0.573*** (4.485)
Constant	0.389*** (3.587)	0.300*** (3.620)	0.213*** (14.42)	0.211*** (17.61)	0.174*** (6.093)	0.143*** (5.213)
Observations	431	635	431	635	429	633
Treated Firms	51	51	51	51	51	51
Control Firms	64	64	64	64	64	64
Total Firms	115	115	115	115	115	115
Adjusted R-squared	0.023	0.008	0.188	0.146	0.354	0.280
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

**Table D6: Descriptive Statistics Age Treatment**

This table presents CEO characteristics (Panel A) and firm characteristics (Panel B) for the 1,260 firms in the sample. The variables are defined in Appendix B. This table also presents the summary statistics for the Age Treatment. CEO characteristics are measured as of 2007. Firm characteristics are observed in the last fiscal year-end before 2008. Two-tailed p-values for tests of differences in mean values are reported in parentheses below the t-Value. The p-value of Kolmogorov-Smirnov test is the exact p-value to test the (in)equality of the distribution.

	Total Sample			Firms with CEO age >= 55 year - Treated firms			Firms with CEO age < 55 year - Control firms			Statistics for Tests of Differences between Mean Values and Distributional Differences	
	N	Mean	Median	N	Mean	Median	N	Mean	Median	t-Value	Kolmogorov-Smirnov Test P-value
Panel A: CEO Characteristics											
CEO Age	1,260	54.8905 (7.2005)	55	647	60.4451 (4.7265)	59	613	49.0277 (4.0038)	50	-46.3496*** (0.0000)	0.000***
CEO Tenure	1,260	6.8119 (7.0523)	5	647	8.8903 (8.2886)	6	613	4.6183 (4.5177)	3	-11.4387*** (0.0000)	0.000***
CEO Time Remaining	1,260	6.5032 (4.3236)	6	647	5.5332 (4.2010)	5	613	7.5270 (4.2169)	8	8.4034*** (0.0000)	0.000***
CEO Total Term	1,260	13.2651 (8.3097)	12	647	14.3648 (9.6295)	12	613	12.1044 (6.4455)	12	-4.9197*** (0.0000)	0.000***
CEO Turnover 2008/2009	1,157	0.1979 (0.3986)	0	594	0.2626 (0.4404)	0	563	0.1297 (0.3362)	0	-5.7900*** (0.0000)	0.000***
Panel B: Firm Characteristics											
Tobin's Q	1,260	1.9757 (1.1434)	1.6334	647	1.8923 (1.0349)	1.6003	613	2.0637 (1.2427)	1.6803	2.6542*** (0.0081)	0.067**
Cash Flow	1,260	0.7269 (2.1771)	0.5078	647	0.6072 (1.8373)	0.4921	613	0.8531 (2.4812)	0.5272	1.9904** (0.0468)	0.000***
Size	1,260	7.4783 (1.5765)	7.4154	647	7.6300 (1.6191)	7.5253	613	7.3181 (1.5152)	7.2398	-3.5322*** (0.0004)	0.030**
Cash Holdings	1,260	0.1565 (0.1697)	0.0898	647	0.1385 (0.1558)	0.0738	613	0.1755 (0.1813)	0.1065	3.8709*** (0.0001)	0.002***
Leverage	1,260	0.313 (0.2933)	0.2809	647	0.3228 (0.2824)	0.3050	613	0.3026 (0.3044)	0.2551	-1.2199 (0.2227)	0.036**
ROA	1,260	0.1545 (0.1164)	0.1507	647	0.1562 (0.1113)	0.1537	613	0.1526 (0.1216)	0.1469	-0.5456 (0.5854)	0.215
Investment	1,256	0.3034 (0.2385)	0.2329	645	0.2687 (0.2043)	0.2140	611	0.3400 (0.2653)	0.2535	5.3177*** (0.0000)	0.000***

**Table D7: Results Age Treatment**

This table presents the results of the DID-analysis for the *Age Treatment*. The dependent variables are *CEO Turnover* and *ROA*. [-2;+2] is based on 2006 - 2007 (pre) to 2008 - 2009 (post). [-3;+3] is based on 2005 - 2007 (pre) to 2008 - 2010 (post). Robust t-statistics based on standard errors clustered by firm are in parentheses. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% level, respectively.

Model	1	2	3	4
Treatment	Age	Age	Age	Age
Dependent variable	CEO Turnover	CEO Turnover	ROA	ROA
Window	[-2;+2]	[-3;+3]	[-2;+2]	[-3;+3]
Treatment * Post	0.120*** (6.373)	0.115*** (7.084)	-0.0117*** (-2.840)	-0.0120*** (-3.075)
CEO Tenure	-0.0460*** (-12.53)	-0.0362*** (-14.26)	-0.000602* (-1.879)	-0.000259 (-0.985)
Tobin's Q	0.0189* (1.656)	0.00446 (0.485)	0.0311*** (8.651)	0.0344*** (12.05)
Cash Flow	-0.00299 (-0.838)	-0.000952 (-0.328)	0.0136*** (10.77)	0.0167*** (14.52)
Size	0.0351 (1.264)	0.0356** (1.995)	0.0318*** (4.575)	0.0275*** (5.553)
Cash Holdings	0.143* (1.698)	0.132** (2.124)	0.0263 (1.418)	0.0193 (1.140)
Leverage	0.123*** (2.696)	0.0555* (1.727)	-0.0260* (-1.883)	-0.0173* (-1.655)
ROA	-0.0765 (-0.793)	-0.0874 (-1.192)		
Constant	0.0942 (0.448)	0.0979 (0.722)	-0.143*** (-2.689)	-0.124*** (-3.287)
Observations	4,581	6,714	4,581	6,714
Treated Firms	646	647	646	647
Control Firms	607	613	607	613
Total Firms	1,253	1,260	1,253	1,260
Adjusted R-squared	0.225	0.172	0.297	0.322
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes



**Table D8: Results Placebo Tests**

This table presents the results of the DID-analysis for the Placebo Tests in 2004 and 2010. The dependent variables are *CEO Turnover* and *ROA*. [-2;+2] is based on 2003 - 2004 (pre) to 2005 - 2006 (post) for Placebo 2004, and 2009 - 2010 (pre) to 2011 - 2012 (post) for Placebo 2010. The Placebo 2004 treatment is a CEO age of at least 55 in 2004. The Placebo 2010 treatment is a CEO age of at least 55 in 2010. Robust t-statistics based on standard errors clustered by firm are in parentheses. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% level, respectively.

Model	1	2	3	4
Treatment	Placebo 2004	Placebo 2004	Placebo 2010	Placebo 2010
Dependent variable	CEO Turnover	ROA	CEO Turnover	ROA
Window	[-2;+2]	[-2;+2]	[-2;+2]	[-2;+2]
Treatment * Post	0.0826*** (4.228)	0.000780 (0.222)	0.0621*** (3.961)	0.00261 (0.791)
CEO Tenure	-0.0465*** (-9.824)	0.000472* (1.819)	-0.0483*** (-11.44)	8.35e-05 (0.286)
Tobin's Q	-0.00996 (-0.791)	0.0332*** (9.451)	-0.00968 (-1.070)	0.0307*** (9.447)
Cash Flow	0.000960 (0.163)	0.0155*** (7.507)	0.00120 (0.340)	0.0201*** (11.76)
Size	-0.000314 (-0.0105)	0.0545*** (8.076)	-0.0198 (-0.805)	0.0526*** (7.100)
Cash Holdings	-0.0398 (-0.473)	0.0313* (1.732)	0.0767 (1.103)	0.0491** (2.410)
Leverage	0.0461 (0.958)	-0.0215* (-1.894)	0.0340 (0.953)	-0.0114 (-1.102)
ROA	0.0253 (0.211)		-0.128 (-1.503)	
Constant	0.447* (1.961)	-0.340*** (-6.516)	0.588*** (3.235)	-0.331*** (-5.923)
Observations	3,946	3,946	5,091	5,091
Treated Firms	585	585	809	809
Control Firms	448	448	603	603
Total Firms	1,033	1,033	1,412	1,412
Adjusted R-squared	0.225	0.267	0.232	0.386
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes