Essays on Empirical Corporate Finance

A DISSERTATION SUBMITTED TO THE FACULTY OF THE DEPARTMENT OF FINANCE OF THE NORWEGIAN SCHOOL OF ECONOMICS

BY

P. Johan E. Mellberg

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

Karin S. Thorburn, Advisor

November 2019

Till Ingrid

CONTENTS

Acknowledgments **3**

Summary **4**

Binding Say-on-Pay and CEO Compensation **7**

CEO Wealth, Incentives and Private Asset Allocation **60**

CEO Death, Successor Characteristics and Firm Performance **99**

Acknowledgments

First of all, I wish to express my deepest gratitude to my supervisor Karin Thorburn, whose guidance and support have been instrumental in the writing of this dissertation. Karin has always kept a door open for me, providing me with constructive feedback and encouragement. Her focus on clarity in both thinking and writing have undoubtedly helped me to become a better researcher. For this and much more, I am most grateful.

I would also like to thank Espen Eckbo for his continuous guidance and for giving me the opportunity to visit Dartmouth College during the spring 2016. I am also grateful to Aksel Mjøs for his feedback and support when writing the third chapter of this dissertation and to Martin Holmén for giving me the opportunity to visit University of Gothenburg. In addition, I would like to thank all faculty members and colleagues at Norwegian School of Economics (NHH).

Finally, I am grateful to my family, and especially to my wife Julianna Mellberg, who has always encouraged me to pursue my goals. You are my best friend and life companion. Without you, this dissertation would not have been completed.

Gothenburg, November 2019

Johan Mellberg

Summary

This dissertation consists of three independent papers about the efficiency of chief executive officer (CEO) pay and the impact of the CEO on firm performance.

The first paper studies how CEO pay change following the introduction of binding say-on-pay. In theory, the introduction of binding say-on-pay strengthened the decision power of shareholders and thus changed the contracting environment in which compensation contracts are negotiated. I use the exogenous nature of this regulatory change to study whether CEO compensation is partly the outcome of inefficient contracting. Using firms with highly concentrated ownership as controls, I argue that the reform primarily affected firms with dispersed ownership. My identification strategy hinges on the plausible assumptions that 1) CEO compensation is well-aligned with shareholder interests in firms with concentrated ownership and 2) ownership concentration does not change in response to say-on-pay. Since ownership concentration in Swedish listed firms vary cross-sectionally but is largely time-invariant, the introduction of say-on-pay in Sweden constitutes a close to ideal setting for an experimental research design. I show that treated CEOs suffered a significant compensation loss amounting to approximately MSEK 1.6 (21%) compared to untreated CEOs. The effect is mainly attributable to the year of treatment and resulted in a lasting pay differential between the two groups. Sub-sample analysis shows that the treatment effect is stronger for longer-tenured CEOs and CEOs with high levels of unexplained pay in the year before implementation. The effect is also stronger in firms where the board had more time to revise its compensation guidelines in the first year of implementation (measured as the distance between the announcement date and the date of the AGM in 2006), which suggests that boards are trying to incorporate shareholder preferences ahead of the vote, possibly to avoid the negative publicity associated with explicit shareholder dissent. The adjustment in CEO pay appears to not have influenced the market value of the firm. Specifically, after adjusting for event-date clustering, the CARs around the dates of announcement and implementation are not significantly different from

zero. Also, I find no evidence of say-on-pay adversely effecting either CEO turnover or firm performance. Overall, my results suggest that binding say-on-pay mainly worked to adjust pay levels without changing investors' expectations of the long-term value of the firm in either direction, consistent with prior rent-extraction by powerful CEOs. The paper supports the hypothesis that say-on-say can help reduce the agency costs between shareholders and CEOs in dispersed firms.

The second paper analyzes the link between risk aversion, the provision of incentives and the CEO's ability to hedge the risks embedded in her incentive contract. Using unique data on the level and composition of private wealth, we first show that wealthier CEOs make riskier allocation decisions, which is consistent with wealth being a valid proxy for risk aversion. Second, we show that less risk-averse CEOs receive steeper incentive contracts in equilibrium, which is consistent with standard principal-agent theory. Third, we show that CEOs who receive stronger incentives make safer allocation decision, given their level of risk aversion. This result is consistent with risk-averse CEOs responding optimally to the provision of incentives. Lastly, we show that risky allocations are negatively related to the firm's systematic risk component but unrelated to the firm's idiosyncratic risk component, which suggests that CEOs are mainly using their private wealth to adjust the exposure to market risk. Consistent with this result, we also find that the return of the private portfolio is only weakly correlated with the firm and highly correlated with the market index and that its composition is not sensitive to either incentive strength or firm risk. This suggests that CEOs hold relatively well-diversified portfolios. The main channel through which CEOs adjust their exposure to risk seems to be by allocating funds between the market portfolio and the risk-free asset.

The third paper studies how CEO death affects firm performance in "larger firms" (defined as having 10 or more employees in each of the two years preceding the death of the incumbent CEO). Previous studies on CEO death effects have shown that the impact on firm performance is negative on average and highly heterogeneous with respect to

the characteristics of both the firm and the incumbent CEO, suggesting that assignment is not always efficient *ex ante*. I begin by showing that the average effect survives the exclusion of smaller firms, which reduces the likelihood that the average effect is driven by hard-to-overcome frictional costs associated with finding a suitable successor. The passing of the incumbent CEO has a clear negative effect on firm performance, suggesting that incumbent CEOs are hard to replace. I then show that the treatment effect is surprisingly homogeneous with respect to the characteristics of both the firm and the incumbent CEO, suggesting that assignment is efficient *ex ante* in larger firms. To further examine whether the treatment effect is the result of CEO uniqueness or frictions in the labor market, I test for the effects of different successor traits on the strength of the treatment effect. If the treatment effect is mainly driven by frictions, I conjecture that heterogeneity in successor traits will explain part of the treatment effect, after controlling for firm fixed effects. On the other hand, if incumbent CEOs are pivotal to the extent that they are *de facto* irreplaceable at the time of death, I expect heterogeneity in successor traits to have low explanatory power. In the analysis, I focus on four successor traits, namely whether the successor has previously held the position of CEO, whether the successor has previous firm or industry experience and whether the successor has family ties to the incumbent CEO (family succession). I show that successor traits play only a minor role in determining the outcome in the post-treatment period, suggesting that the average treatment effect is mainly driven by CEO uniqueness.

BINDING SAY-ON-PAY AND CEO COMPENSATION

P. Johan E. Mellberg[†]

Abstract

This paper analyzes how chief executive officer (CEO) pay change following the introduction of binding say-on-pay. Using firms with highly concentrated ownership as controls, I argue that the reform primarily affected (treated) firms with dispersed ownership. I show that treated CEOs suffer a significant loss of income amounting to approximately MSEK 1.6 (21%) compared to untreated CEOs. The effect is mainly attributable to the year of treatment and results in a lasting pay differential between the two groups. The effect is strongest for CEOs with relatively long tenure and high pre-reform unexplained pay. While lowering the pay of treated CEOs, the reform did not affect shareholder value, consistent with prior rent-extraction by powerful CEOs. Lastly, I find no evidence of say-on-pay adversely affecting either turnover rates or firm performance, which is consistent with efficiency. Overall, this study supports the hypothesis that say-on-say can help reduce the agency costs between CEOs, boards and shareholders.

Key Words: Say-on-Pay, Executive Compensation, Corporate Governance, Ownership Structure, Shareholder Monitoring, Shareholder Activism
JEL Classifications: G33

[†]Norwegian School of Economics (NHH)

I Introduction

The question of whether shareholders should be allowed a "voice" on matters regarding executive compensation has been subject to much debate. Growing concerns among both investors and the general public about the efficiency of the pay-setting process have led regulators around the world to take measures to limit the potential for excessive pay. Such measures include enhancing the disclosure requirements of firms and encouraging shareholders to engage more actively in corporate governance.[1] As a result, several countries have instituted rules that give shareholders the right to decide on executive compensation, an initiative commonly referred to as shareholders' "say-on-pay". The exact regulations surrounding say-on-pay vary across countries. While some countries have adopted advisory votes that give shareholders the right to express their view on pay but give boards retained decision power, several European countries have adopted *binding* votes that forces the board to abide to the voting outcome.

While advisory say-on-pay has been researched extensively, evidence on the effects of binding say-on-pay is largely missing in the literature. The aim of this paper is to fill that gap by examining how chief executive officer (CEO) pay change following the introduction of binding say-on-pay in Sweden. The Swedish government amended its Companies Act to include binding say-on-pay in 2006. The law was one of the first to mandate that shareholders cast a binding vote on the pay policy to be applied in the coming period. In theory, the introduction of say-on-pay strengthened the decision power of shareholders and thus changed the contracting environment in which compensation contracts are negotiated. I use the exogenous nature of this regulatory change to study whether CEO compensation is partly the outcome of inefficient contracting.

The question of whether say-on-pay effectively aligns CEO pay with shareholder interests is unsettled. While some studies find evidence of advisory say-on-pay affecting the level and structure of CEO pay (Balsam et al., 2016; Correa and Lel, 2016), others find that it can have value-destroying effects (Cai and Walkling, 2011; Wagner and Wenk, 2017). In particular, despite

[1] Noteworthy examples of such initiatives include the 1992 Cadbury Report in the U.K., the 2002 Sarbanes-Oxley Act in the U.S., the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act in the U.S. and the 2017 Shareholder Rights Directive in the EU.

that the stated purpose of say-on-pay is to provide shareholders with a tool to curb seemingly high levels of pay, to date there exist little empirical evidence of advisory say-on-pay altering subsequent pay levels (Conyon and Sadler, 2010; Sheehan, 2010; Armstrong, Gow and Larcker, 2013; Ferri and Maber, 2013; Alissa, 2015; Cuñat, Gine and Guadalupe, 2016; Mason, Medinets and Palmon, 2016). After reviewing the empirical evidence, both Mason, Palmon and Sudit (2013) and Thomas and Elst (2015) conclude that, overall, advisory say-on-pay seems to have had only a limited impact on the level of CEO pay.

The mixed empirical evidence could potentially be explained by difficulties in identifying a suitable control group. Since say-on-pay is generally imposed on entire populations of firms (e.g. all firms listed on the main stock exchange in a certain country), a suitable counterfactual can be hard to identify, making the results sensitive to the identification strategy used to examine the effect.[2] This study uses a novel identification strategy to test the effects of binding say-on-pay in a setting where ownership is concentrated. Using firms with highly concentrated ownership as controls, I argue that the reform primarily affected firms with dispersed ownership. My identification strategy hinges on the plausible assumptions that 1) CEO compensation is well-aligned with shareholder interests in firms with concentrated ownership and 2) ownership concentration does not change in response to say-on-pay. Since ownership concentration in Swedish listed firms vary cross-sectionally but is largely time-invariant, the introduction of say-on-pay in Sweden constitutes a close to ideal setting for an experimental research design.[3] The idea that concentrated ownership facilities monitoring and activism in the context of say-on-pay is supported by Rapp, Sperling and Wolff (2010), who finds that shareholder approval rates increase with the voting

[2] A couple of papers have proposed solutions to this problem. Balsam et al. (2016) exploit that "smaller reporting companies" (firms with a public float below $75 million) were exempted from say-on-pay in the U.S. in the first two years of its implementation. The results suggest that treated CEOs experienced a relative drop in pay in 2010, with larger decreases found in firms that previously overpaid their CEOs. However, the paper does not analyze the pre-treatment trends in pay, which makes causal interpretation difficult. Similarly, Ferri and Maber (2013) exploits that firms listed on the "Alternative Investment Market" (a sub-market to the London Stock Exchange) were exempted from say-on-pay in the UK. Contrary to Balsam et al. (2016), the authors do not find any evidence that advisory say-on-pay altered the subsequent level and structure of CEO pay in the UK. As pointed out by Ferri and Göx (2018), while this identification strategy helps in alleviating many of the endogeneity issues, the analysis suffers from comparing pay practices in firms of vastly different sizes.

[3] The persistence of ownership concentration in Swedish firms has been documented by La Porta, de Silanes and Schleifer (1999), Högfeldt (2004) and Holmén and Högfeldt (2009). As is common in many European countries, Swedish founding families often retain significant control over their firms, even after a firm goes public, making ownership concentration a largely time-invariant characteristic.

power of block-holders. Similarly, Thomas and Elst (2015) argues that for firms characterized by concentrated ownership, say-on-pay may be less important as a means of mobilizing shareholder opposition against high executive pay levels. When ownership is concentrated, the controlling shareholder has an incentive to discipline ineffective managers. Such owners presumably have significant influence over the board and consequently, compensation practices should already be aligned with shareholder interests prior to the introduction of say-on-pay, leaving this group unaffected.

The results of this paper have direct implications for the ongoing debate on whether say-on-pay should be binding or advisory. Despite that we know little about the effects of say-on-pay outside the U.K. and the U.S., several countries in the EU are currently considering switching from advisory to binding voting regimes. In March 2017, the European Parliament enacted an amended version of the EU Shareholder Rights Directive, in which say-on-pay is made mandatory in all member states. It introduces, among other things, a shareholder say on the remuneration policy for the board of directors and the executive management, as well as a mandatory advisory vote on the compensation report throughout the European Union. Whether or not the vote on remuneration policy should be binding or advisory will be left to each member state to decide. Gaining more insight into the effects of binding say-on-pay is therefore of great interest to policymakers.

The analysis is carried out in three steps. I begin by sorting firms into treatment and control based on the concentration of ownership the year before the introduction of say-on-pay. It turns out that the control rights of the largest shareholder group is high; the median firm has a primary owner that controls 27% of the votes. I then show that the control rights of the primary owner is stable around the event, which suggests that say-on-pay-induced variation in ownership is not a major issue.

In a second step, I use difference-in-differences methodology to compare the level and composition of CEO pay for treated and control firms before and after the introduction of say-of-pay. My results show a significant negative treatment effect. The average CEO received an annual compensation package of approximately MSEK 5.6 in the year prior to say-on-pay. For treated

firms, the relative decrease in compensation was approximately MSEK 1.6, which constitutes a 21% pay drop in the post-treatment period. This result is robust to the inclusion of both firm- and year-fixed effects, various model specifications and placebo tests. The effect is mainly attributable to the year of treatment and results in a lasting pay differential between the two groups. Sub-sample analysis shows that the treatment effect is stronger for longer-tenured CEOs and CEOs with abnormally high pay in the period leading up to say-on-pay.

An often stated concern with say-on-pay is that shareholders may not have the information nor the incentives required to make sound policy decisions, which could lead to inefficient outcomes. To test this, I examine the effects on firm value, performance and CEO turnover. I begin by testing for abnormal returns around both the dates of announcement and enactment using standard event study techniques. It turns out that the market reaction is weak. Specifically, after adjusting for cross-sectional correlations in stock returns, treated firms display positive but insignificant CARs. I then test whether say-on-pay adversely effected firm outcomes, such as turnover rates or firm profitability. I find no evidence of this. In all, these results suggest that say-on-pay mainly worked to correct excessive pay levels without changing investors' expectations of the long-term value of the firm in either direction.

In summary, this paper provides evidence of a downward adjustment in compensation for those CEOs who became more accountable as a result of the introduction of binding say-on-pay in Sweden. I show that the regulatory change was a major determinant of the overall development of CEO compensation during the post-treatment period. By using ownership concentration as a proxy for compliance and by comparing the pay differential around the event, I show that say-on-pay can be an effective governance tool in dispersed firms. Conversely, empowering already strong shareholder groups is largely ineffective. To my knowledge, this is the first paper to show this.

The remainder of the paper is organized as follows. Section II discussed the expected effects of say-on-pay on CEO compensation and firm value. Section III gives an overview of the institutional setting and section IV discusses the sample and data sources and reports summary statistics. Section V presents the results and section VI concludes.

II Expected Effects of Say-on-Pay

Theories based on managerial power argue that CEO pay is partly the outcome of rent-seeking behavior. Weak governance structures and acquiescent boards allow powerful CEOs to decide on their own compensation contracts, which leads to inflated and/or ill-structured pay (Bebchuk, Fried and Walker, 2002; Bebchuk and Fried, 2003; Bertrand and Mullainathan, 2001). In contrast, theories based on efficient contracting argue that observed pay practices are predominantly the result of optimal contracting in a competitive market for managerial talent (Holmström, 1979; Rosen, 1982; Gabaix and Landier, 2008; Terviö, 2008).[4] Proponents of say-on-pay argue that, if boards allow pay practices that do not reflect shareholders' best interests, say-on-pay may help to reduce agency costs, resulting in more efficient contracts (Bebchuk, 2007; Davis, 2007; Burns and Minnick, 2013). On the other hand, opponents argue that increased shareholder voice will only distract the board and reduce its authority, resulting in sub-optimal contracting outcomes.

It is important to clarify that the ability of say-on-pay to abate seemingly high levels of pay depends not only on the efficiency of the pay contract *ex ante*, but also on the efficiency of the vote. Since boards are likely to be better at assessing the quality of the manager and the firm's operating environment, the benefits of retained board decision-power may outweigh the costs (Bainbridge, 2008). In addition, say-on-pay may have adverse effects. For example, say-on-pay could lead to a homogenization of pay practices, forcing boards to impose pay that is perceived as best practice by proxy advisors (Gordon, 2009). Alternatively, say-on-pay could lead to increased CEO turnover, which could have a negative impact on firm value in the long run.

Figure I shows how the expected effects of say-on-pay are linked to the efficiency of the pay contract *ex ante* and the efficiency of the voting outcome. In total, there are four states to

[4] According to this view, pay mainly reflects the productivity of the CEO. The rise in executive compensation over time can be explained by parallel changes in firm characteristics or operating environments, such as changes in firm size (Himmelberg and Hubbard, 2000; Baker and Hall, 2004; Gayle and Miller, 2009), market competition (Hubbard and Palia, 1995; Campbell et al., 2001), the production function of managers as well as the supply and demand of managerial talent (Murphy and Zábojník, 2004, 2007; Frydman, 2007).

consider (I-IV). Intuitively, if CEO pay is efficient *ex ante* (I-II), imposing say-on-pay cannot increase firm value. If CEO pay is efficient *ex ante* and the voting outcome is efficient (I), say-on-pay should have no effects on either pay levels or firm value.[5] This is the assumed state for the control group. If instead CEO pay is efficient *ex ante* but the voting outcome is inefficient (II), I conjecture that say-on-pay will have negative effects on both pay and firm value. An inefficient voting outcome implies that shareholders either vote to reduce pay, which is likely to violate the CEO's participation constraint (since pay is efficient *ex ante*), or distort managerial incentives. In both of these scenarios, say-on-pay is likely to have a negative effect on firm value. If instead, CEO pay is supra-competitive *ex ante* and the vote is efficient (III), I expect the effect on CEO pay to be negative. The effect on firm value can be either positive or zero depending on whether CEOs are under-incentivized *ex ante*. If the inefficiency of CEO pay is mainly attributable to rent extraction, an efficient vote will optimally reduce of the level of pay, which is unlikely to have an effect on firm value.[6] On the other hand, if the inefficiency relates to under-provision of incentives, an efficient voting outcome will strengthen incentives, which may have a positive effect on firm value. Lastly, if pay is supra-competitive *ex ante* but the voting outcome is inefficient (IV), the subsequent adjustment in pay must be either zero, in which case the effect on firm value is also zero, or large enough to cause either CEO turnover or distortion of managerial incentives, in which case the effect on firm value should be negative.

There are broadly two channels through which say-on-pay may cause a change in CEO pay. One possibility is that shareholders vote against the proposal, thereby forcing the board the revise its policy. However, previous studies have shown that approval rates are high in general. Thomas and Elst (2015) report that Sweden has among the highest approval rates in Europe and according to an ISS survey, approval rates in Sweden were as high as 89.1% in 2010 and 99.3% in 2011, which is higher than in most other European countries. The other possibility is that boards will incorporate shareholder preferences ahead of the vote, possibly to avoid the negative publicity associated with low approval rates. Davis (2007) argues that say-on-pay will make

[5]This claim ignores potential costs associated with implementing the vote and managing the relationship with investors.

[6]This assumes that overpaid CEOs earn reasonably low rents, so that the gains from adjusting pay are modest relative to the total firm value.

boards more accountable and thereby provide incentives to communicate complex pay policies to shareholders and implement transparent disclosure practices. Grundfest (1993); Davis (2007); Alissa (2015) also point out that shareholder votes carry a strong symbolic value, which may cause boards to take preemptive action to bring on policy adjustments ahead of the actual vote, fearing the negative publicity and embarrassment associated with explicit shareholder dissent. It has also been suggested that say-on-pay can empower weak boards in their negotiations with powerful managers, through the use of shareholder dissent/support as leverage (Correa and Lel, 2016).

III The Setting

III.A The Swedish Model of Corporate Governance

The Swedish model of corporate governance is firmly rooted in the idea that both capital and labor should assume an active role in the monitoring of corporate insiders. Historically, this has been achieved through retained control by a small set of large shareholders and the formation of organized labor movements.[7]

Panel A of Table I summarizes the central features of the Swedish governance model.[8] Like in most western economies, the Swedish governance structure consists of three main bodies; the shareholders' meeting, the board of directors and the managing director. The shareholders' meeting constitutes the highest decision-making body and with few exceptions, shareholders have the power to decide on any company matter, including issuing instructions to the board. This means that the shareholders' meeting is sovereign to both the board and the CEO, which is

[7] Högfeldt (2004) provides an interesting discussion on why ownership dispersion never materialized in Swedish firms. He argues that The Swedish model can be seen as a concession that the Social Democratic Party had to accept in order to remain in power throughout most of the post-war era. The political agenda of the party, which revolved around building a tax-financed welfare state, relied heavily on income redistribution to finance its reforms. In order to legitimize this agenda, the party needed assurance that the largest firms would remain under Swedish control so that capital would not migrate. The solution was to allow large incumbent owners to retain their corporate control using only a small capital investment. As such, the political legitimacy of entrenched private ownership was traded off against the implicit guarantee that the largest listed firms would not migrate but continue to invest, thereby generating the economic resources needed to finance the political reform agenda.

[8] For a comprehensive description of corporate governance in Sweden, see Eckbo, Paone and Urheim (2010).

different from Anglo-Saxon countries, where shareholders generally have limited influence over board decisions. Apart from the fact that Swedish shareholders can resolve on a wider range of corporate matters compared to their U.S. counterparts, the main difference between the two models of governance is that the former emphasizes active monitoring by large shareholders. The largest shareholders are usually represented on the board and given pivotal roles in the election process.[9]

III.B Binding Say-on-Pay in Sweden

Panel B of Table I summarizes the central features of the Swedish say-on-pay initiative. The rule states that the remuneration package of the CEO has to be approved at the shareholders' meeting. The vote is binding and comprises all Swedish public limited liability companies. Specifically, the law introduced two additional paragraphs to the Swedish Companies Act. Chapter 7, §61-62, states that the decision over executive compensation accrues to the shareholders attending the shareholders' meeting. Moreover, Chapter 8, §51-54, states that the board is responsible for presenting guidelines for the remuneration of the CEO and other senior executives, including all types of cash- and equity-based compensation. Furthermore, these guidelines, combined with a detailed description of previous year's compensation package, have to be expressed clearly in the annual report. Shareholders should cast the say-on-pay vote annually at the annual general meeting and the vote is forward-looking by one year. If shareholders vote against the proposal, the board is responsible for revising the guidelines and administer a new vote, usually by calling to an extra shareholders' meeting.

The main differences between the Swedish and the U.S. say-on-pay regimes is that the former is binding and forward-looking, whereas the latter is advisory and retrospective. In addition, the Swedish say-on-pay rule requires that shareholders vote on the individual components of the compensation package (vs. voting on general guidelines), with a fixed voting frequency of

[9] Half of the directors have to be elected by the shareholders attending the shareholders' meeting, but the rest can be elected by other parties, such as individual shareholders. The power to elect directors cannot be allocated to any individual member(s) of the board though. The nomination committee, which is to be appointed at the shareholders' meeting, shall have at least three members and should be comprised of representatives of the major shareholders. Usually, the nomination committee consists of four or five members.

one year. Taken together, these differences make the Swedish version of say-on-pay stricter compared to its U.S. counterpart.

Panel C of Table I reports the time-line of the initiative. The first day of implementation was July 1, 2006, only three and a half months after the legislation was first proposed by the government. The proposition was announced on March 16, 2006, and on June 1, 2006, it was passed in parliament. An important question is whether say-on-pay was anticipated before March 16? In order to to examine this, I search Retriever for articles relating to say-on-pay dated before March 16, 2006. This search results in zero matches, suggesting that say-on-pay was largely unanticipated prior to this date.[10] The announcement date is likely to have raised awareness among investors, since the ruling red-green coalition government had a 55% majority in parliament at the time and thus, the probability of passing the legislation in the near future was high. In choosing relevant event dates, I follow the literature and focus on the events that are linked to the decision-making process of the parliament and the government. These dates are March 16, the day of announcement, June 1, the day of enactment and July 1, the first day of implementation.

IV Data Description

IV.A Sample Construction

The initial sample consists of all firms listed on the Stockholm Stock Exchange in 2006. A small number of firms that were either delisted or incorporated abroad during the sample period are excluded due to lack of data. I also exclude firms listed on alternative marketplaces, since the disclosure requirements for these firms are less strict and the available data on CEO pay and corporate governance is therefore less detailed.[11] The final data set comprises 236 firms

[10] It could also be argued that if there were information regarding the future adoption of say-on-pay available before March 16, it is unlikely to have significantly impacted market values, since it relates to the formative phase of the legislation.

[11] The main alternative marketplaces in Sweden are Aktietorget, First North and Nordic Growth Market (NGM).

observed over the period 1999-2013. In total, the panel spans seven pre-treatment and seven post-treatment years, allowing me to track within-firm changes in CEO pay over time. The final data set contains approximately 3000 firm-years observations, which are close to uniformly distributed over time.

IV.B Data Sources

The final data set merges information from the following six registers.

1. **Annual Reports Archive**

   Data on CEO compensation are hand-collected from annual reports and include the following components; salary, bonus, other cash-based compensation, pension contributions and grant values of equity-based compensation. Firms normally use the grant date closing price and the Black-Scholes formula to value equity-based compensation. However, there are no clear rules on how to compute the value of equity-based compensation, opening up for minor inconsistencies in the reported values. To address this, I collect all relevant contract details, including grant dates, strike prices, share prices, volatilities and expiration dates, and calculate the grant values using the Black-Scholes formula for valuing European call options, as modified by Merton (1973). Whenever the calculated value deviates from the reported value, the calculated value is used. I also use the annual reports to collect data on CEO characteristics.

2. **The Financial Supervisory Authority of Sweden (FI)**

   FI is a governmental agency tasked with monitoring all insider trading in Swedish listed firms. These data are publicly available online and goes back to 1991. The database includes information on all stocks and options traded by insiders, including the date of the transaction, purchase price, strike price, time to maturity and total holdings of a given instrument after each transaction. I use this data to calculate the values of equity grants and holdings and the pay-performance sensitivity measure.

3. **Bisnode**

This database contains all accounting data for Swedish publicly listed firms. I use this data to calculate all accounting-based control variables.

4. **The Swedish Companies Registration Office**

   All limited liability companies in Sweden must register with the Swedish Companies Registration Office. In addition to reporting on corporate actions and submitting financial reports, firms are required to provide information on all key insiders of the firm. The data set includes the name of the manager/director, corporate role(s), date of birth as well as start and end dates of the appointment. I use this data to calculate various board characteristics, CEO tenure and age etc..

5. **FINBAS**

   I collect pricing data for all firms from FINBAS, provided by Stockholm School of Economics, including daily bid and ask prices, trading volumes, and adjusted and unadjusted closing prices.

6. **SIS Ownership Service**

   I collect ownership data from SIS Ownership Service, including the the full name of each shareholder, the number of shares held in each class, the total number of shares outstanding in each class and the total cash flow and voting rights of each shareholder. In addition, the data group individual shareholders into ownership spheres if they have the same ultimate owner or belong to the same family. The sphere feature is important since large shareholders often retain corporate control through the use of pyramid structures. If I treat each individual shareholder as a separate owner, I risk understating the concentration of ownership in the control group. Also, since dual class shares are common in Sweden, cash flow and voting rights can differ substantially. Throughout the paper, I use the control rights rather than cash flow rights to measure ownership concentration.

IV.C Descriptives

IV.C.1 Program Participation

Assignment into treatment and control is based on the concentration of ownership the year before say-on-pay. Since ownership concentration is assumed to facilitate monitoring and reduce information asymmetries, shareholders and managers should be well-aligned over CEO compensation in firms with concentrated ownership.[12] The variable used to assign firms is the control rights of the largest shareholder group. The reason I focus on shareholder groups and not on individual shareholders is that large shareholders often make use of pyramidal structures and dual class share systems to exercise control using only a limited capital investment, and consequently, it is common for multiple shareholders to have the same ultimate owner. As pointed out by La Porta, de Silanes and Schleifer (1999), pyramiding is the one of the most frequently used mechanisms to exercise control over listed firms outside the Anglo-Saxon sphere.[13] By focusing on shareholders groups rather than individual shareholders, I avoid the risk of understating the true ownership stake of the primary owner.

I start by calculating the sample median in the year prior to the event, which is approximately 27%. I then assign firms to the treatment group if the largest shareholder group controls less than 27% of the control rights at the end of the year and to the control group otherwise. The benefit of this approach is that it generates treatment and control groups of equal sizes. A potential concern though is that firms residing marginally to the left of the threshold are likely to be similar in terms of "alignment" to those residing marginally to the right of it. Thus, if the majority of firms are clustered around the threshold so that there is not enough cross-sectional variation in the data, the assignment rule would make little sense. In panels A and B of Figure II, I plot the distribution of the largest ownership around the event. Panel A shows how the ownership of the largest owner change in the four years around the event and panel B shows the exact distribution in the post-treatment period. Evidently, many of the firms in the control

[12] Bereskin and Cicero (2012) uses a similar identification strategy to investigate the effect of a change in regulations regarding staggered boards on compensation.

[13] Holmén and Högfeldt (2009) provides a good description of the ownership structures of listed Swedish firms.

group have primary owners that control more than 50% of the votes whereas many of the firms in the treatment group have primary owners that control less than 20%. Also, the amount of migrating observations (plotted in gray) is small and mainly cluster around the threshold, which I interpret as "normal" variation in the ownership variable. In order to address the fact that the definition of a dispersed firm is somewhat ambiguous and depends on sample characteristics, I run several robustness checks using different assignment rules.

For the assignment rule to work, the concentration of ownership needs to be exogenous with respect to the event. If a large number of firms choose to migrate from one group to another as a result of say-on-pay, the results will suffer from a selection bias. Panel A of Table III reports the first and second moments of the ownership variable. In the treatment (control) group, the average control rights of the primary owner is well below (above) the cut-off point. Also, the ownership stake of the primary owner appears to not change much around the event, suggesting the program-induced variation in ownership is not a major issue.

Panel C of Figure II also plots the distribution of the primary owner's "type" in each year. In the control group, around 80% of the primary owners are families, which is to be expected, whereas in the treatment group, ownership type is more or less equally divided between families and financial firms/institutional investors. Again, the distribution appears stable over time, which adds strength to the claim that ownership concentration is by large historically determined.

IV.C.2 Compensation

Since the say-on-pay initiative is mainly a response to rising CEO compensation levels (Thomas and Elst, 2015), the main dependent variable that I look at is *Total Compensation*, which includes salary, bonus, other cash-based compensation, pension contributions and grants of equity-based compensation. I also examine two measures of incentive strength, namely *%-Incentives* and *Pay-Performance Sensitivity*. *%-Incentives* equals the percentage of equity-based pay and is defined as the grant value of stocks and options divided by total compensation. *Pay-Performance Sensitivity* equals the sum of the number of shares owned and the number of options owned

times their delta, divided by the total number of shares outstanding. If all incentives comes from shares, this measure would equal the share of the firm that is owned by the CEO. The main difference between the two measures is that the latter is based on aggregate holdings of stocks and options whereas the former only accounts for the incentives provided in a given year. Lastly, I look at the average worker's pay and the pay gap between the CEO and the average worker. Average worker's pay is defined as the total compensation expense reported in the income statement minus the compensation of the CEO and the board divided by the total number of employees.

Figure III plots the average and median levels of compensation for each year and group. *Total Compensation* increased steadily in both groups over the sample period. More importantly, the year of the event constitutes a clear cut-off point between the two groups. Looking at panel A, in the period leading up to the event, pay levels do not differ significantly between the two groups, whereas in the period after the event, the treatment group displays a clear decrease relative to the control group. The resulting pay differential is highly persistent. The main effect can be traced to 2006 and 2007, after which the gap stabilizes for the remaining period. This suggests that say-on-pay had long-lasting effects on the pay-setting process. Furthermore, there is a clear parallel trend and overlap in pre-treatment period. This is important as it mitigates concerns about unobserved heterogeneity *ex ante*. The fact the two groups are similar in terms of both levels and growth rates in the pre-treatment period adds strength to the identifying assumption that the treatment effect is independent of group belonging.[14] Also, panel B shows that the median CEO in the treatment group received more compensation in the pre-treatment period than the median CEO in the control group, which is consistent with shareholder dissent in the treatment group at the time of the introduction of say-on-pay.

[14]The extent to which a change in regulation, such as the introduction of say-on-pay, should be interpreted as exogenous is debatable. Two potential concerns are whether say-on-pay was anticipated prior to implementation and whether the timing of the regulation is in itself a function of some pre-existing trend in the outcome variable. On the one hand, if shareholders anticipated say-on-pay, then compensation contracts may have been changing prior to the year of implementation, in which case I would underestimate the impact of say-on-pay. On the other hand, if say-on-pay was not truly exogenous, but rather a reaction to changes in the industry, then the estimated coefficients could not conclusively be interpreted as causal. I find no evidence of any significant pre-existing pay differential between treated and control firms, nor do I find any signs of significant jumps in the outcome variable in the pre-treatment period, which lends credibility to the identifying assumption that say-on-pay was largely unanticipated.

Panel B of Table III reports changes in the various components of *Total Compensation* around say-on-pay. A quick look at sub-panels I and II confirms the results of Figure III. Before the introduction of say-on-pay, there were no major differences between the two groups. In fact, the difference is insignificant for all components of the annual compensation package. After the introduction of say-on-pay, treated firms experienced a significant reduction in all components apart from equity-based compensation. The average difference in the post-treatment period is approximately 1.6 MSEK, which constitutes a relative decrease of 20% compared to the control group.

Looking at the incentive strength, Table III highlights an interesting fact; there is a big difference between annual grants of equity-based compensation and incentives generated through aggregate holdings. Annual grants are very modest in both groups both before and after say-on-pay. On average, less than 4% of CEO pay is equity-based, which is low compared to most countries. On the other hand, the average pay-performance sensitivity is high at approximately 5%, i.e. the average CEO of a listed firm in Sweden owns about 5% of the company. This discrepancy is due to two things; 1) some CEOs are either founders or members of the founding family and thus, tend hold considerable stakes even though they receive modest incentives from their boards, and 2) CEOs who are not members of a founding family often buy their shares in the open market as part of an implicit contract between themselves and the board. Interestingly, the difference in pay-performance sensitivity between the two groups is less pronounced after the introduction of say-on-pay.

The average worker earned approximately MSEK 0.38 over the sample period, which translates into an average pay gap of about 17.21. As expected, the distribution is highly skewed - the highest recorded pay gap is approximately 350 times the average worker's pay. The control group displays higher pay gaps on average, both before and after say-on-pay. However, the difference between the treatment and control groups is larger in the post-treatment period, suggesting that say-on-pay had a mitigating effect on the pay gap in dispersed firms.

IV.C.3 Control Variables

Throughout the paper, I use a number of CEO-, board- and firm-specific variables to control for time-varying heterogeneity. All variables are defined in Table II.

The firm-level variables common to most regressions include *Size* (natural logarithm of total assets), *Leverage* (total liabilities scaled by total assets), *Cash* (cash and short-term investments scaled by total assets), *ROA* (EBITDA scaled by total assets), *Book-to-Market* (book value to equity divided by market value of equity), *Volatility* (annualized standard deviation in stock returns) and *Industry* (GICS 10-industry classification). As discussed by Edmans, Gabaix and Jenter (2017), there is a well-documented positive relationship between firm size and CEO pay. Leverage and stock return volatility are included as proxies for firm risk. The cash ratio is included to control for liquidity constraints and is expected to be positively related to pay levels. I also include ROA to control for the fact that bonus rewards are often linked to accounting performance. Book-to-market is included as a proxy for growth opportunities, which is expected to be correlated with the use of equity-based compensation.

In terms of CEO-specific controls, I include *Age*, *Foreign* (dummy equal to one if the CEO was not born in Sweden), *Urban* (dummy equal to one if the CEO originates from one of the three main cities of Sweden), *Female*, *Tenure*, *Ownership* and *Director* (dummy equal to one if the CEO sits on the board). Age and tenure are standard controls in the literature. Following Keloharju, Knüpfer and Tåg (2018), I include *Urban* to account for the possibility that executives who are born in a large city might have had a career jump start due to the geographical advantage in hiring and networks. *Female* controls for the documented gender gap in compensation. *Director* captures differences between firms where CEOs have no voting power and firms where CEOs can vote, and can be viewed as a proxy of directors' independence from the management. Lastly, I include *CEO Ownership* as a measure of CEO bargaining power.

The board-specific controls include *Size* (number of board members), *Gender Mix* (percentage of female directors), *Age* (average age of directors), *Emp. Directors* (percentage of employee representatives), *Dependence* (percentage of directors that are dependent with respect to man-

agement and large owners), *Busy* (average number of outside directorships) and *Board Ownership* (combined voting rights of directors net of the CEO). Most of these variables are standard in the literature. Similar to German boards, employee representation is statutory in Swedish boards. Since this influences all measures that use board size as a denominator, and since employee representatives might have different objectives compared to regular directors, I include the percentage of directors elected by the employees as a control variable.

Summary statistics are reported in panel C of Table III. Interestingly, treated and control firms do not differ significantly in terms of either firm size or leverage. The control group displays slightly higher book-to-market, lower ROA and is also less volatile. With the exception of information technology, the two groups are also highly comparable in terms of industry belonging. In terms of CEO characteristics, the biggest difference lies in ownership, which is to be expected given that in some firms in the control group, the primary owner is also the CEO. Presumably for the same reason, the control group also have slightly longer-tenured CEOs. The control group also have older and more dependent boards.

V Empirical Tests

V.A Pre-Treatment Pay Levels and Shareholder Dissent

I begin by examining whether shareholder dissent is likely to have been high in treated firms at the time of the introduction of say-on-pay. A natural approach would be to compare voting outcomes between the two groups in the first year of implementation, but since the procedure for casting votes at the annual meeting in Swedish firms is based on acclamation and not ballots, the results are seldom recorded, which makes assessment impossible.

One way to proceed is to examine how CEO pay relates to proxies of CEO power and quality of governance in treated and control firms in the years leading up to say-on-pay. If shareholder dissent was high in treated firms at the time of implementation, I expect pay in this group to be related to at least some governance-related variables in the years leading up to the policy

change.

To test this, I split the sample based on the median value of a certain CEO or board characteristic in the two years leading up to say-on-pay (2004-2005) and test for the difference in unexplained CEO pay for above-median and below-median firms using a simple t-test. As a measure of unexplained CEO pay, I use the residuals from the following cross-sectional model

$$Comp_i = \alpha + \beta_2 Size_i + \beta_3 Leverage_i + \beta_4 Cash_i + \beta_5 ROA_i + \beta_6 BTM_i + \lambda_{Ind} + \varepsilon_i, \quad (1)$$

where *Comp.* is the sum of *Salary*, *Bonus*, *Other Cash*, *Pensions* and *Grants of Equity* and λ_{Ind} is a set of industry-fixed effects. The results are reported in Table IV. CEO and board characteristics that are either significant or insignificant in both groups are not reported. Panel A shows that the only variable that is significantly related to unexplained pay in the treatment group but not in the control group is *CEO Age* (arguably a proxy for managerial entrenchment).

This approach assumes that shareholders care about unexplained CEO pay. However, as pointed out by both Piketty and Saez (2003) and Thomas and Elst (2015), dispersed shareholders may be more concerned with preventing pay disparities from getting out of touch with social values than they are with promoting efficient pay contracts. If so, shareholder are unlikely to factor in firm characteristics when assessing pay.[15] To test this, panel B of Table IV instead reports sample split differences in total CEO pay. *CEO Age* remains significantly and positively related to CEO pay in the treatment group but not in the control group. The same goes for *CEO Director* (a proxy for managerial power) and *Board Age* (arguably a proxy for board lenience). This is consistent with the findings of Cai and Walkling (2011), that shareholder-sponsored say-on-pay proposals are significantly related to total compensation but not to abnormal compensation. Overall, these descriptive results are consistent with shareholder dissent in dispersed firms *ex ante*.

[15] If shareholder dissent is largely driven by public outrage, shareholders may evaluate CEO pay only in terms of whether it is high compared to pay in general.

V.B Main Results

To test whether say-on-pay affected pay levels and incentive strength in treated firms, I fit the following difference-in-differences model

$$Comp_{ist} = \alpha + \beta_1 Treated_s \times After_t + \beta_2 After_t + \beta_3 Treated_s + \beta_4 X_{ist} + \varepsilon_{ist}, \qquad (2)$$

where *Comp* is either a measure of total compensation, incentive strength or individual pay components. The interaction term *Treated*×*After* measures the pay differential in the post-treatment period. X is a matrix consisting of the control variable described in section IV.C.3.

The results are reported in Table V. In columns 1-2, the dependent variable equals *Total Compensation*. The results show a significant negative treatment effect ranging from MSEK 1.06 to MSEK 1.63 (without controls). The average pay level in the post-treatment period is MSEK 7.62 for the control group and only MSEK 6.04 for the treatment group, a difference of MSEK 1.63. On average, this corresponds to a 20% relative drop in pay for treated CEOs. For comparison, the corresponding difference in the pre-treatment period is only MSEK 0.08. The results are robust to the inclusion of various control variables and firm- and year-fixed effects.

In columns 3-4, the dependent variable equals the percentage of equity-based pay ($\%-Incentives$) and the pay-performance sensitivity (PPS) respectively. In both columns, the interaction term comes out insignificant, which suggests that say-on-pay had little effect on incentive strength. At first glance, this result seems puzzling. However, a possible explanation could be that the Swedish tax code makes option grants unfavorable. When options are granted or sold at below market value, social security fees and income tax have to be paid. Also, the sparse use of equity-based compensation before say-on-pay (see panel B of Table III) may imply that shareholders did not fully recognize the incentive effect embedded in equity-based pay and/or that equity grants were not perceived as a problem in the first place.[16]

Panel B examines the effects on the individual pay components. Interestingly, only basic salaries and pensions (which are usually stated as a percentage of the basic salary) are significantly

[16]For comparison, the combined value of bonuses and pension contributions dwarfs the value of equity grants by a factor of 12 in the pre-treatment period.

affected by say-on-pay when controlling for CEO, board and firm characteristics. This is interesting, since these are arguably the easiest components for shareholders to contract on as well as the fastest way for boards to visibly adjust CEO pay. Also, as reported in panel A of Table III, salaries and pensions are by far the largest components of pay in treated firms and therefore the most likely ones to be subject to shareholder dissent.

V.C Robustness Checks

Columns 1-2 in panel A of Table VI report the results of two placebo tests. The first column re-estimates the basic model using pre-treatment observations only and falsely defines treatment to occur in 2004, while the second column instead defines treatment to occur in 2005. The post-treatment dummies are all positive and significant, which confirms that there is a positive trend in compensation in the pre-treatment period (see, Figure II). More importantly, both interaction terms are insignificant, which suggests that there was no treatment in either 2004 or 2005.

In columns 3-4, I analyze the timing of the treatment effect by adding leads and lags to the model. Specifically, I fit the following model

$$Comp_{ist} = \alpha + \lambda_t + \sum_{\tau=0}^{m} \beta_{-\tau} D_{s,t-\tau} + \sum_{\tau=1}^{q} \beta_{+\tau} D_{s,t+\tau} + \delta Treated_s + \gamma X_{ist} + \varepsilon_{ist} \qquad (3)$$

, where D_{st} equals the $Treated_s \times After_t$ switch in the original model, λ_t is a set of time-fixed effects and q and m are the number of lead and lagged interaction terms respectively. Column 3 includes leads only whereas column 4 features the full set of leads and lags. The idea is to establish that causes happen before consequences and not vice versa. By adding lead and lagged treatment switches to the model, I can investigate both whether, in addition to the original policy switch, the effect kicks in before the event and whether the effect subsequently grows or fades over time. If the effect gets stronger after 2006, I expect some of the leading switches to be negative and significant. Alternatively, if the effect is transient, I expect some of the leading switches to be positive and significant. All leading policy switches in column 3 are insignificant, which means that the effect neither increases or decreases, but stays constant. This suggests

that say-on-pay had a persistent effect on pay levels in Swedish firms. Whereas previous studies on say-on-pay have failed to identify such lasting effects on compensation, my results suggests a persistent effect that spans the entire post-treatment period.

In column 4, I add the lagged switches. If the estimated pay differential is really the result of say-on-pay, I expect the effect to be felt after 2005 and thus that the coefficients of the lagged interaction terms to come out insignificant. The results confirm this hypothesis. The coefficient of the 2006 policy switch is comparable to that in Table V and all lagged switches come out insignificant.

One potential concern in Table V is the long post-treatment period. Intuitively, as we move away from the event, the probability that the pay differential is partly caused by factors not related to say-on-pay increases. In particular, if my results are is mainly driven by "later year" observations, I run the risk of falsely attributing changes in compensation to say-on-pay. To address this, I re-estimate the main model using the shorter sample period stretching from 1999 to 2007, that is, I utilize all pre-treatment observations but truncate the post-treatment sample to comprise the first two post-treatment years only. The results are presented in panel B of Table VI. The estimated treatment effect is qualitatively the same as in Table V, which suggests that the main result is not primarily driven by pay differentials generated in periods far away from the event.

Another potential issue is the extent to which the estimated treatment effect is contingent on the assignment rule outlined in section IV.C.1. As discussed, since assignment into treatment is based on the sample characteristics and assumes that ownership concentration is a good proxy for compliance *ex ante*, the results could be sensitive to the chosen threshold. An alternative assignment rule would place firms in the treatment group only if the largest shareholder controls at least 50% of the votes, as this gives absolute control over the company. However, doing so would give me a treatment group that consists of only 48 firms, which reduces the power of my tests considerably. To address this, I instead re-estimate the main model using a truncated sample where I exclude all firms where the ownership stake of the primary owner lies in the 25-35% interval. Doing so mitigates concerns related to both program migration and "ambiguous"

treatment status. If the treatment status is in fact ambiguous around the threshold and if my results are primarily driven by firms belonging to the mid-distribution, I expect the treatment effect to disappear when removing firms residing close to it. Conversely, if the pay differential is the outcome of say-on-pay, I expect the treatment effect to survive exclusion of the mid-distribution. The results are reported in panel B of Table VI. The interaction term is significant and quantitatively the same as in Table V, which adds strength to the main results.

V.D A Second Proxy for Compliance

The specification in (1) measures the effect of policy by removing the effects of time and type. A built-in problem with this approach, which applies to policy evaluations in general, is that it implicitly assumes that say-on-pay is itself not the outcome of contemporaneous shareholder dissent over CEO pay in dispersed firms, or at least not strongly correlated with it. If it is, the estimated effect could simply be capturing boards' responses to shareholder dissent, irrespective of having to hold a say-on-pay vote.

Generally, there is no easy way to get around this problem. However, one way to proceed is to apply a further differencing strategy, using a variable that is independent of both group belonging and shareholder dissent. Arguably, one variable that meets these criteria in the short run is the timing of the firm's annual general meeting in the first year of implementation. As discussed in Section V.B, the estimated treatment effect is unlikely to be the outcome of shareholders actually voting against the board's proposal - anecdotal evidence suggest that approval rates are high in general and that boards are trying to communicate their intended pay policies with shareholders and incorporate their preferences ahead of the vote. If this is the case, and assuming that say-on-pay was unanticipated prior to the March 16, the ability of the board to "react" in the first year of implementation should be related to the timing of the annual general meeting in that year, since firms that held their annual general meetings close to March 16 would have had less time to adjust their pay policies in 2006 compared to firms that held their annual general meetings at a later date.

If say-on-pay was indeed the cause of the treatment effect in Table V, I expect the length of the period between the announcement date and the date of the annual general meeting to be negatively related to changes in pay from 2005 to 2006 in the treatment group. In order to test this, I estimate the following model

$$\Delta Comp_i = \alpha + \beta_1 Treated_i \times AGM_i + \beta_2 AGM_i + \beta_2 \Delta X_i + \varepsilon_i, \tag{4}$$

where Δ denotes the change from 2005 to 2006 and AGM is the distance in days between the announcement date and the annual general meeting in 2006.

For this test to have a causal interpretation, two criteria have to be satisfied, 1) say-on-pay needs to be unanticipated prior to March 16 and 2) the timings of the 2006 annual general meetings have to be exogenous with respect to say-on-pay. The first assumption was tested in section III.B by searching for news articles related to the Swedish say-on-pay legislation dated before March 16, which resulted in zero matches. The second assumption is examined by manually comparing the dates of the 2005 and 2006 meetings to check whether firms are prone to change the timing of their annual general meetings from year to year. It turns out that no sample firms changed their AGM dates in 2006, which suggests that timing of the meeting is largely exogenous.

The results are reported in Table VII. As expected, the coefficient of the distance variable is negative and significant at 5% for the treatment group and insignificant for the control group. In the last column, using the full sample, the coefficient of the interaction term is significant at 5% and is equal to -0.02, which suggests that CEO pay falls by on average MSEK 0.6 (=0.02×30) for each month between the the date of announcement and the AGM. This is consistent with the idea that boards are trying to incorporate shareholder preferences ahead of the vote.

V.E Is Pay Adversely Affected in the Control Group?

As pointed out by Iliev and Vitanova (2017), say-on-pay might lead to more scrutiny of CEO performance and to more media attention and public pressure. If CEOs in the control group enjoy strong bargaining power over pay, which could be the case if social ties between owners

and managers are more developed in tightly held firms, such attention could potentially lead to an increase in pay, which would make the results harder to interpret. Cronqvist et al. (2009) argues that controlling shareholders might be accepting high levels of pay to pursue their own self-interest, such as devoting less effort to contract bargaining, improving social relations or building shareholder-CEO alliances.

If increased scrutiny of CEO performance puts an upward pressure on CEO pay in tightly held firms, I expect pay to increase more in firms where the primary owner enjoys more private benefits from retaining the incumbent CEO. Arguably, this is the case in firms where the ownership stake of the primary owner is levered (so that the control rights exceed the cash flow rights). In such firms, the primary owner can retain the incumbent CEO at a small cost, since the separation of ownership and control gives her cheap access to the firm's internal cash flow.

To test this, I regress pay on ownership leverage, using control firms only. Specifically, I estimate the following model

$$Comp_{it} = \alpha + \beta_1 Lev.Ownership_i \times After_t + \beta_2 After_t + \beta_3 Lev.Ownership_i + \beta_4 X_{it} + \varepsilon_{it}, \quad (5)$$

where *Lev.Ownership* equals the ratio of the control rights and the cash flow rights of the primary owner (columns 1-2), or a dummy that takes the value one if the ratio of the control rights and the cash flow rights of the primary owner exceeds the control group median value in the year before say-on-pay (column 3).

The results are reported in Table VIII. In all columns, the interaction term comes out insignificant, which suggests that there is no positive treatment effect in the control group stemming from the separation of ownership and control. Interestingly, column one suggests an overall effect of ownership leverage on pay levels in tightly held firms.

V.F Does the Treatment Effect Capture a Larger Shift in Pay?

A more general concern in Table V is that the estimated effect might capture some unobservable factor that is both time-varying and group-specific. The observed drop in CEO pay could for example be the result of a larger shift in labor income in the treatment group.

To test this, I examine the effect of say-on-pay on the average worker's pay as well as the pay gap between the CEO and the average worker. *Average Worker's Pay* is defined as the the total remuneration cost stated in the consolidated accounts less the total compensation of the CEO and the board divided by the average number of employees. *Pay Gap* is defined as the total compensation of the CEO divided by *Average Worker's Pay*. If the effect on CEO pay was the result of a more general drop in wages, I expect *Average Worker's Pay* to decrease in the post-treatment period.

The results are reported in Table IX. Column 1 reports the result from re-estimating (1) using the *Average Worker's Pay* pay as the left-hand side variable. The coefficient of the interaction term is both positive and highly insignificant, which suggests that say-on-pay did not affect the average pay level in treated firms. In column 2, I test the effect of say-on-pay on the pay gap between the CEO and the average worker, using *Average Worker's Pay* from column 1 as the denominator. The pay gap coefficient comes out negative and significant, which suggests that the treatment effect was predominantly confined to the CEO. On average, treated firms experienced a relative drop in the firm-level pay gap of approximately 4 times the average worker's pay, which constitutes a 16% drop from pre-treatment levels. Overall, the results reported in Table IX are consistent with the notion that the drop in CEO pay in treated firms is the outcome of say-on-pay.

V.G Sample Splits

As pointed out by Cai and Walkling (2011), firm-specific factors are likely to influence the probability of adjusting the level of compensation in response to say-on-pay. For example, firms with weaker governance structures would intuitively benefit more from say-on-pay and so, if say-on-pay truly enhances governance by discipling managers in poorly-governed firms, I expect the change in compensation following the event to be related to at least some of the CEO and board characteristics of the firm. In Table X, I split the sample on governance characteristics to examine whether the treatment effect is heterogeneous across firms.

In a first step, I examine the relation between pre-treatment levels of compensation and the adjustment in the first post-treatment period. Previous studies have found a significant relationship between the amount of unexplained CEO pay in the periods leading up to the event and the subsequent treatment effect. As a measure of unexplained CEO pay, I use the residuals from the following cross-sectional specification using data from 2005.

$$\begin{aligned} Comp_i = \alpha + \beta_1 CEO.Age_i + \beta_2 Size_i + \\ + \beta_3 Leverage_i + \beta_4 Cash_i + \beta_5 ROA_i + \beta_6 BTM_i + \lambda_{Ind} + \varepsilon_i, \end{aligned} \tag{6}$$

where λ_{Ind} denotes industry-fixed effects. The dependent variable is the level of total compensation. I include *CEO Age* to control for the tendency of older and more experienced CEOs to earn more. *ROA* is included to account for the fact that bonuses are often linked to accounting performance. Similarly, *Book-to-Market* is included to proxy for growth firms. I deliberately avoid including any governance-related variables, which means that any heterogeneity in governance will be captured by the residual. I then split the sample based on the median value of the residual. Above-median firms are compared to below-median firms and the pay differential in 2006 is tested using a simple t-test. In also fit the following model

$$\begin{aligned} \Delta Comp_i = \alpha + \beta_1 D_i + \beta_2 Turnover_i + \beta_3 \Delta Size_i + \beta_4 \Delta Leverage_i + \\ + \beta_5 \Delta Cash_i + \beta_6 \Delta ROA_i + \beta_7 \Delta BTM_i + \lambda_{Ind} + \varepsilon_i, \end{aligned} \tag{7}$$

where D_i is the above-median switch.

Panel A of Table X shows that there is clear distinction in $\Delta Comp$ between treated firms with abnormally high levels of compensation in the year before the event and the rest. For the former group, the average change in compensation is equal to MSEK -0.73 whereas for the latter, the change is equal to MSEK 0.52. The difference is significant at 5% both when using a simple t-test and the regressional approach described above. This suggests that the treatment effects in Table V and Table VI are mainly driven by CEOs with high levels of unexplained pay. The same pattern is not present in the control group. For untreated firms, the change in compensation seems to be independent of previous levels of unexplained CEO pay. In the triple difference-in-differences specification of the last column, the coefficient of the interaction term is equal to -2.56 and significant at 5%. This result is consistent with alignment. Treated firms with

high levels of unexplained CEO pay experience the largest drop in pay whereas control firms experience increases in CEO pay independent of previous pay levels.

Next, in panel B, I test whether the strength of the treatment effect is related to incumbent CEO characteristics. Similar to panel A, I split the sample based on the median value of the following variables; *Age*, *Tenure*, *Ownership* and *Director* and test for differences in the pay differential between the two groups. The results show that long-tenured CEOs (arguably a measure of managerial entrenchment) in the treatment group experience a sharp decrease in pay relative to short-tenured CEOs. This effect is not present in the control group and the triple difference interaction term is significant at 1%. Also, *CEO Ownership* (measured as the voting rights of the CEO) goes in the opposite direction, suggesting that CEOs who have more influence over board decisions receive relatively higher pay.

Lastly, in panel C, I test whether the treatment effect is heterogeneous with respect to board characteristics. The variables that comes out significant in the treatment group are *Board Size* (arguably a measure of board quality and bargaining power) and *Gender Mix* (a proxy for board diversity). Interestingly, having more women on the board result in smaller pay adjustments. One interpretation of this result is that firms with a high proportion of female directors were already perceived as progressive and well-governed before the introduction of say-on-pay, resulting in lower dissent levels.

V.H The Market Reaction to Say-on-Pay

Having established a negative link between the introduction of say-on-pay and the subsequent growth rate in CEO pay, I now turn to the question of whether say-on-pay had any effects on firm value. One of the main criticisms of say-on-pay is that dispersed shareholders are ill-suited to make policy decisions and that potential shareholder dissent therefore is likely to reflect uninformed public opinion rather than a sound understanding of firm operations and the labor market for CEOs. As pointed out by Bainbridge (2008), shareholders might not have the information nor the incentives required to make sound policy decisions. It follows that the

treatment effect in Table V does not necessarily constitute an efficient outcome.

In order to test for the efficiency of the outcome, I use standard event study methodology. The benefit of event studies is that they can quantify the expected net effect of the policy change. A positive market reaction would be a clear sign of efficiency. A negative reaction on the other hand would be an indication that say-on-pay was overall disruptive.

The event dates studied are March 16, the day of announcement, June 1, the day of enactment and July 1, the first day of implementation. When testing for abnormal returns, I adjust the standard errors to account for event-date clustering. Specifically, I conduct two cross-sectional parametric tests, which account for event-induced volatility changes and cross-sectional correlations in abnormal returns (Patell, 1976; Kolari and Pynnönen, 2010). My estimation window spans the 250 trading days prior to the event window and the event window equals the three trading days centered on the event day.

The results are reported in Table XI. In panel A, the cumulative average abnormal returns for the treatment group is positive and significant at 10% on two of the three dates when using *Patell's* test, which adjusts for event-induced volatility. However, when using *Kolari's* test, which in addition accounts for cross-correlations in stocks returns, the adjusted z-value drops by more than 50% and is no longer insignificant. Interestingly, the sign of the CAARs are positive for the treatment group and negative for the control group on the dates of announcement and implementation. Also, the difference in abnormal returns is more pronounced on July 1, the day of implementation.

Another way to deal with cross-correlations in stock returns is to form portfolios of stocks and then test for the significance of the portfolio cumulative abnormal returns around the event. By forming portfolios, the cross-sectional correlation can be diversified and the significance of the CARs can be tested using a simple t-test. In panel B, I form equally-weighted portfolios of stocks belonging to the treatment and control groups. The CARs in panel B are identical to those in panel A by construction, but the corresponding t-values are slightly different. Again, the cumulative abnormal returns come out insignificant when accounting for the cross-sectional

correlations in stock returns. Lastly, the bottom row in panel B reports the CARs of a long-short portfolio that longs the equally-weighted portfolio of all firms in the treatment group and and shorts the equally-weighted portfolio of all firms in the control group. The long-short portfolio has a positive CAR of 0.40% on the first day of implementation, but the coefficient is only significant at the 20% level.

One possible explanation for the weak evidence in Table XI is that individual CARs cancel out at the group-level. To test this, I split the sample the same way as in Table X to examine whether the market reaction is heterogeneous (and offsetting) across firms within the treatment and control groups. The results are reported in Table XII. I find no evidence that the market reaction is related to any of the CEO- or board characteristics of the firm, which confirms the results in Table XI.

Overall, there is little evidence to suggest that say-on-pay had any effect on firm value. The insignificant market reaction suggests that the downward adjustment in pay did not change investors' expectations of the long-term value of the firm in either direction, which can potentially be explained by the modest use of equity-based compensation both before and after say-on-pay. Since say-on-pay mainly worked to reduce the level of CEO pay without strengthening managerial incentives, a modest market reaction is to be expected.

V.I Adverse Effects

Table VI showed that say-on-pay had long-lasting effects on pay levels. As discussed in section V.C though, a potential concern with the long post-treatment period is that say-on-pay may adversely affect turnover rates and/or firm performance. Treated firms could for example experience an increase in CEO turnover after 2006, which in turn could affect firm performance in the long run. Firms experiencing a drop in performance could then be providing their CEOs with lower pay moving forward. If so, the lasting effect on CEO pay would partially be the result of a drop in performance in the post-treatment period and not cleanly attributable to say-on-pay. To test this, I run additional regressions similar to those in Table V using CEO turnover and

various performance measures as left-hand side variables. In all regressions, I include a similar set of control variables as before. The results are presented in Table XIII. In all regressions, the interaction term is insignificant, which suggests that say-on-pay had little effects on either CEO turnover or firm performance.

VI Conclusions

Does binding say-on-pay provide shareholders with an effective tool to curb CEO pay? In this paper, I try to answer this question by studying how the level and structure of pay changed for those CEOs who became more accountable as a result of the introduction of binding say-on-pay in Sweden.

A major contribution of the empirical analysis is to identify a causal treatment effect, using firms with concentrated ownership as the control group. For identification, I rely on the assumption that compensation is well-aligned with shareholder interests, even prior to the introduction of say-on-pay, in firms with concentrated ownership. A careful analysis of the sample shows that ownership vary cross-sectionally but is largely time-invariant, which suggests that program-induced variation in ownership is not a major issue. Also, there is a clear parallel trend and overlap in the outcome variable in the pre-treatment period (7 years), making the difference-in-differences methodology a feasible testing strategy.

A key result of the paper is that binding say-on-pay has a significant effect on the subsequent level of CEO pay, a result that is largely missing in the literature on advisory pay-on-pay. I find that treated CEOs suffered a significant compensation loss amounting to approximately MSEK 1.6 (21%) compared to untreated CEOs. For comparison, the average CEO received an annual compensation packages of approximately MSEK 5.6 in the year prior to the say-on-pay. The effect is mainly attributable to the year of treatment and resulted in a lasting pay differential between the two groups. Sub-sample analysis shows that the treatment effect is stronger for longer-tenured CEOs and CEOs with high levels of unexplained pay in the year before implementation. The effect is also stronger in firms where the board had more time to

revise its compensation guidelines in the first year of implementation (measured as the distance between the announcement date and the date of the AGM in 2006), which suggests that boards are trying to incorporate shareholder preferences ahead of the vote, possibly to avoid the negative publicity associated with explicit shareholder dissent.

The adjustment in CEO pay appears to not have influenced the market value of the firm. Specifically, after adjusting for event-date clustering, the CARs around the dates of announcement and implementation are not significantly different from zero. Also, I find no evidence of say-on-pay adversely effecting either turnover rates or firm performance, which is consistent with efficiency.

Overall, my findings suggest that *binding* say-on-pay mainly worked to adjust pay levels without changing investors' expectations of the long-term value of the firm in either direction. The results supports the hypothesis that say-on-say can help reduce the agency costs between shareholders and CEOs in dispersed firms.

REFERENCES

Alissa, Walid. 2015. "Boards' Response to Shareholders' Dissatisfaction: The Case of Shareholders' Say on Pay in the UK." Working Paper.

Armstrong, Chris, Ian D. Gow and David F. Larcker. 2013. "The Efficacy of Shareholder Voting: Evidence from Equity Compensation Plans." *Journal of Accounting Research* 51:909–950.

Bainbridge, Stephen M. 2008. "Remarks on Say on Pay: An Unjustified Incursion on Director Authority." UCLA School of Law, Law-Econ Research Paper No. 08-06.

Baker, George P. and Brian J. Hall. 2004. "CEO Incentives and Firm Size." *Journal of Labor Economics* 22(4):767–798.

Balsam, Steven, Jeff Boone, Harrison Liu and Jennifer Yin. 2016. "The Impact of Say on Pay on Executive Compensation." *Journal of Accounting and Public Policy* 35:162–191.

Bebchuk, Lucian Arye. 2007. "The Myth of the Shareholder Franchise." *Virginia Law Review* 93(3):675–732.

Bebchuk, Lucian Arye and Jesse M. Fried. 2003. "Executive Compensation as an Agency Problem." *Journal of Economic Perspectives* 17(3):71–92.

Bebchuk, Lucian Arye, Jesse M. Fried and David I. Walker. 2002. "Managerial Power and Rent Extraction in the Design of Executive Compensation." *University of Chicago Law Review* 69:751–846.

Bereskin, Frederick L. and David C. Cicero. 2012. "CEO Compensation Contagion: Evidence From an Exogenous Shock." *Journal of Financial Economics* 107(2).

Bertrand, Marianne and Sendhil Mullainathan. 2001. "Are CEOs Rewarded for Luck? The Ones Without Principals Are." *The Quarterly Journal of Economics* 116(3):901–932.

Burns, Natasha and Kristina Minnick. 2013. "Does Say on Pay Matter? Evidence from Say-on- Pay Proposals in the United States." *Financial Review* 48(2):233–258.

Cai, Jie and Ralph A. Walkling. 2011. "Shareholders' Say on Pay: Does It Create Value?" *Journal of Financial and Quantitative Analysis* 46(2):299–339.

Campbell, John Y., Martin Lettau, Burton G. Malkiel and Yexiao Xu. 2001. "Have Individual Stocks Become More Volatile? An Empirical Exploration of Idiosyncratic Risk." *Journal of Finance* 56(1):1–43.

Conyon, Martin J. and Graham V. Sadler. 2010. "Shareholder Voting and Directors' Remuneration Report Legislation: Say on Pay in The U.K." *Corporate Governance: An International Review* 18:296–312.

Correa, Ricardo and Ugur Lel. 2016. "Say on Pay Laws, Executive Compensation, Pay Slice, and Firm Valuation around the World." *Journal of Financial Economics* 122(3):500–520.

Cronqvist, Henrik, Fredrik Heyman, Mattis Nilsson, Helena Svaleryd and Jonas Vlachos. 2009. "Do Entrenched Managers Pay Their Workers More?" *Journal of Finance* 64(1):309–339.

Cuñat, Vicente, Mireia Gine and Maria Guadalupe. 2016. "Say Pays! Shareholder Voice and Firm Performance." *Review of Finance* 20(5):527–563.

Davis, Stephen. 2007. "Does "Say on Pay" Work? Lessons on Making CEO Compensation Accountable." Policy Briefing.

Eckbo, B. Espen, Giulia Paone and Runa Urheim. 2010. "Efficiency of Share-Voting System: Report on Sweden." Working Paper.

Edmans, Alex, Xavier Gabaix and Dirk Jenter. 2017. "Executive Compensation: A Survey of Theory and Evidence." European Corporate Governance Institute (ECGI) - Finance Working Paper No. 514/2016.

Ferri, Fabrizio and David A. Maber. 2013. "Say on Pay Votes and CEO Compensation: Evidence From The UK." *Review of Finance* 17:527–563.

Ferri, Fabrizio and Robert F. Göx. 2018. "Executive Compensation, Corporate Governance, and Say on Pay." *Foundations and Trends in Accounting* 12(1):1–103.

Frydman, Carola. 2007. "Rising Through the Ranks: The Evolution of the Market for Corporate Executives, 1936-2003." Working Paper.

Gabaix, Xavier and Augustin Landier. 2008. "Why has CEO Pay Increased So Much?" *The Quarterly Journal of Economics* 123(1):49–100.

Gayle, George-Levi and Robert A. Miller. 2009. "Has Moral Hazard Become a More Important Factor in Managerial Compensation?" *The American Economic Review* 99(5):1740–1769.

Gordon, Jeffrey N. 2009. ""Say on Pay": Cautionary Notes on the UK Experience and the Case for Shareholder Opt-In." Columbia Law and Economics Working Paper No. 336.

Grundfest, Joseph A. 1993. "Just Vote No: A Minimalist Strategy for Dealing with Barbarians Inside the Gates." *Stanford Law Review* 45(4):857–937.

Himmelberg, Charles P. and R. Glenn Hubbard. 2000. "Incentive Pay and the Market for CEOs: An Analysis of Pay-for-Performance Sensitivity." Working Paper.

Högfeldt, Peter. 2004. "The History and Politics of Corporate Ownership in Sweden." NBER Working Paper No. 10641.

Holmén, Martin and Peter Högfeldt. 2009. "Pyramidal Discounts: Tunneling or Overinvestment?" *International Review of Finance* 9(1-2):133–175.

Holmström, Bengt. 1979. "Moral Hazard and Observability." *Bell Journal of Economics* 10(1):74–91.

Hubbard, Robert and Darius Palia. 1995. "Executive Pay and Performance Evidence from the U.S. Banking Industry." *Journal of Financial Economics* 39(1):105–130.

Iliev, Peter and Svetla Vitanova. 2017. "The Effect of the Say-on-Pay Vote in the U.S." Working Paper.

Keloharju, Matti, Samuli Knüpfer and Joacim Tåg. 2018. "What Prevents Female Executives from Reaching the Top?" IFN Working Paper No. 1111.

Kolari, J. W. and S. Pynnönen. 2010. "Event Study Testing with Cross-sectional Correlation of Abnormal Returns." *Review of Financial Studies* 23(11):3996–4025.

La Porta, Rafael, Florencio Lopez de Silanes and Andrei Schleifer. 1999. "Corporate Ownership Around the World." *Journal of Finance* 54(2):471–517.

Mason, Stephani A., Ann F. Medinets and Dan Palmon. 2016. "Say-on-Pay: Is Anybody Listening?" *Multinational Finance Journal* 20(4):273–322.

Mason, Stephani A., Dan Palmon and Ephraim Sudit. 2013. "What is the Impact of Say on Pay on Executive Compensation? A Meta-Analysis of the Empirical Evidence." Working Paper.

Merton, Robert C. 1973. "Theory of Rational Option Pricing." *Bell Journal of Economics and Management Science* 4:141–183.

Murphy, Kevin J. and Ján Zábojník. 2004. "CEO Pay and Appointments: A Market-Based Explanation for Recent Trends." *American Economic Review* 94(2):192–196.

Murphy, Kevin J. and Ján Zábojník. 2007. "Managerial Capital and the Market for CEOs." Working Paper.

Patell, J. 1976. "Corporate Forecasts of Earning Per Share and Stock Price Behavior: Emprical Tests." *Journal of Accounting Research* 14(2):246–276.

Piketty, Thomas and Emmanuel Saez. 2003. "Income Inequality in the United States, 1913–1998." *The Quarterly Journal of Economics* 118(1):1–39.

Rapp, Marc, Marco Sperling and Michael Wolff. 2010. "Who is Asking the Shareholders? Voting on Management Compensation in German Listed Firms: Evidence from the Annual Meeting Season 2010." Working Paper.

Rosen, Sherwin. 1982. "Authority, Control and the Distribution of Earnings." *Bell Journal of Economics* 13(2):311–323.

Sheehan, Kym. 2010. "Say on Pay and the Outrage Constraint." Working Paper.

Terviö, Marko. 2008. "The Difference That CEOs Make: An Assignment Model Approach." *American Economic Review* 98(3):642–668.

Thomas, Randall S. and Christoph Van Der Elst. 2015. "Say on Pay Around the World." *Washington University Law Review* 92(3):653–731.

Wagner, Alexander F. and Christoph Wenk. 2017. "Are Shareholders Stupid? On The Surprising Impact of Binding Say on Pay on Stock Prices." Working Paper.

Figure I

Expected Effects of Say-on-Pay on CEO pay and Firm Value

This figure shows how the expected effects of say-on-pay on the level of CEO pay and firm value are related to 1) the efficiency of CEO pay *ex ante* and 2) the efficiency of the voting outcome. "Efficient Pay" denotes a state where CEOs receive exactly their outside options and are well-incentivized. Similarly, "Efficient Vote" denotes a state where the shareholders make at least as informed decisions on CEO pay as the board.

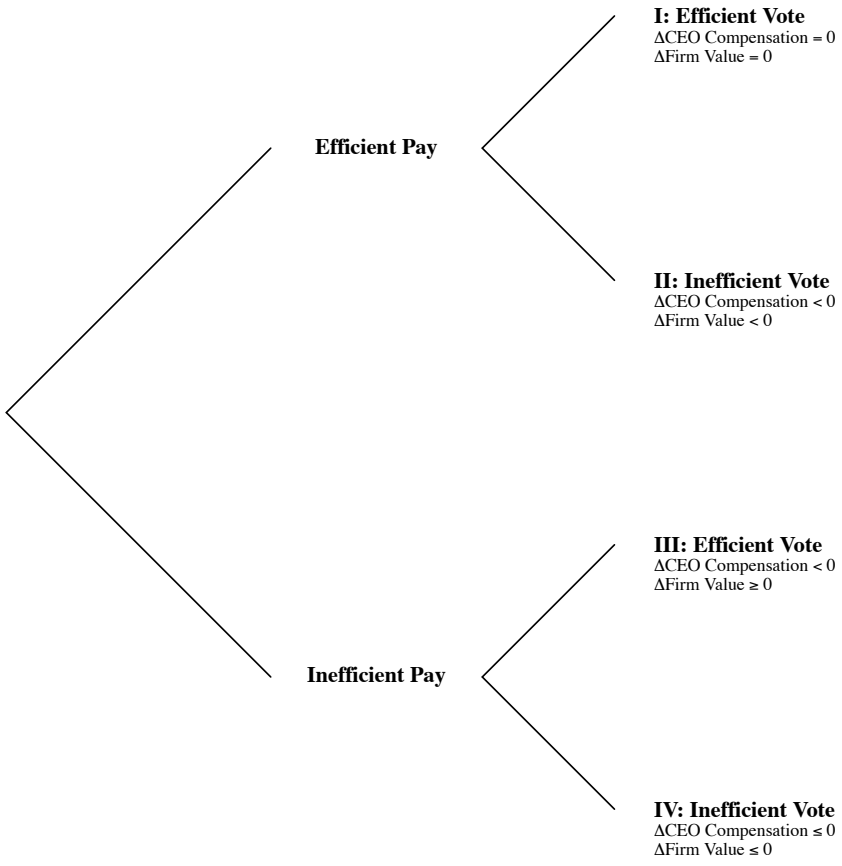

Figure II

Ownership of the Primary Shareholder in Treated and Control Firms

This figure shows how the ownership stake of the primary shareholder changed around the introduction of say-on-pay. Panels A and B plot the average post-treatment ownership stake of the primary shareholder against the average pre-treatment ownership stake. The dotted line marks the 45 degree angle. The x- and y-axes denote pre- and post-treatment observations respectively. Averages are calculated over using a two-year window. Panels C and D show the exact distribution of voting rights in the two-year period after the introduction of say-on-pay. The y-axis counts the number of firms in a given percentile. The dotted line marks the assignment threshold. Lastly, panels E and F show the distribution of primary owners by type for each year. Each primary shareholder is sorted into one of the following four categories; *Individuals and Families*, *Industrial Firms*, *Financial Firms and Institutional Investors* and *Other*, which includes ownership by foundations and the government. The total sample includes 236 firms observed over the period 1999-2013, which corresponds to 3041 firms-year observations.

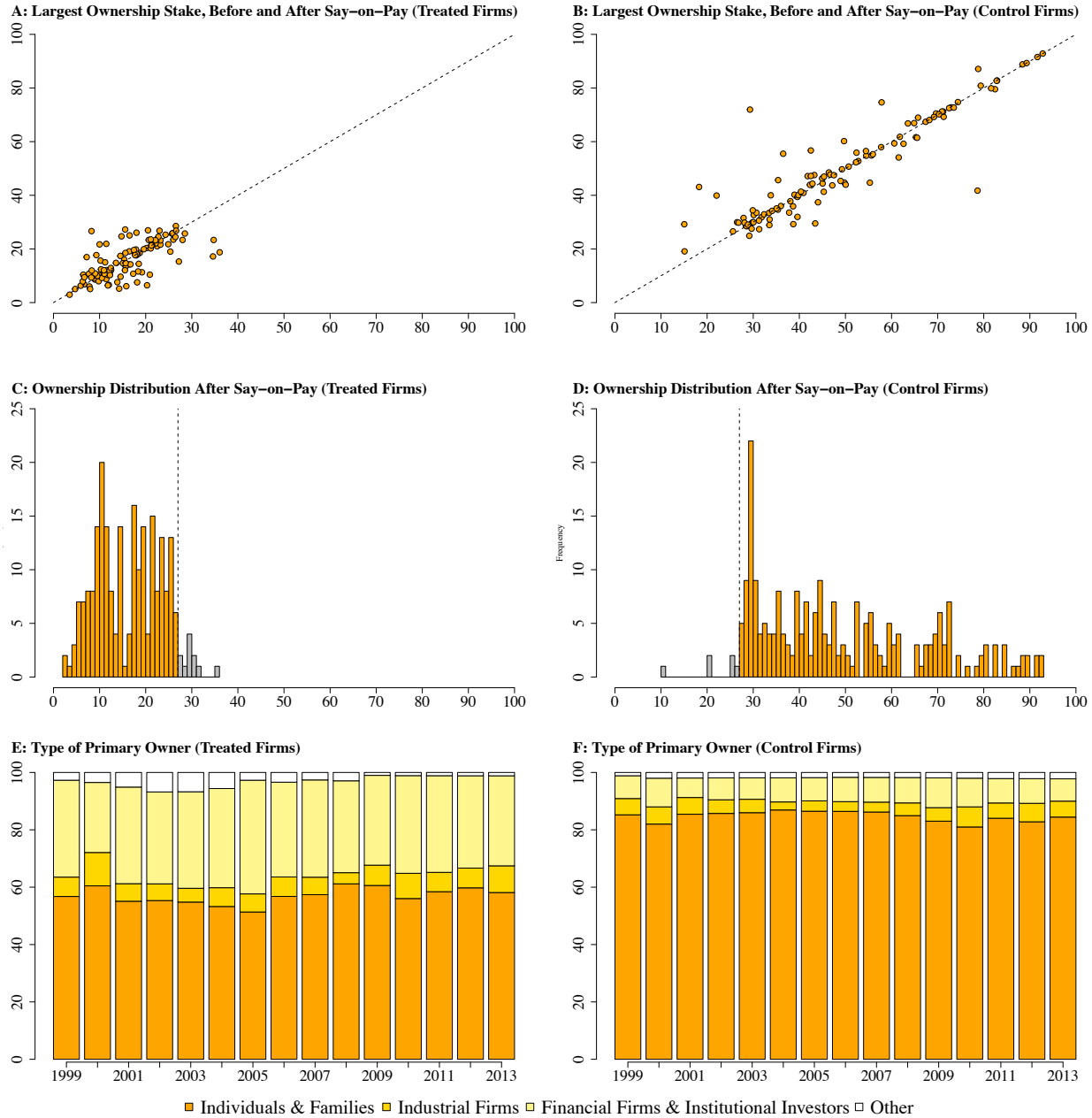

Figure III

Compensation over Time for Treated and Control Firms

This figure shows how the level of compensation changed over time in the treated and control firms. Each dot represents either the average (A) or the median (B) value for a specific group and year. Firms are assigned to treatment if the primary shareholder group controls less than 27% of the voting rights in the year prior to the introduction of say-on-pay, and to control otherwise. *Compensation* equals the sum of the basic salary, bonus, other cash compensation, pension contributions and annual grants of equity-based compensation. For variable definitions, see Table I. The black line marks the last year without say-on-pay. The y-axis measures total compensation in million SEK.

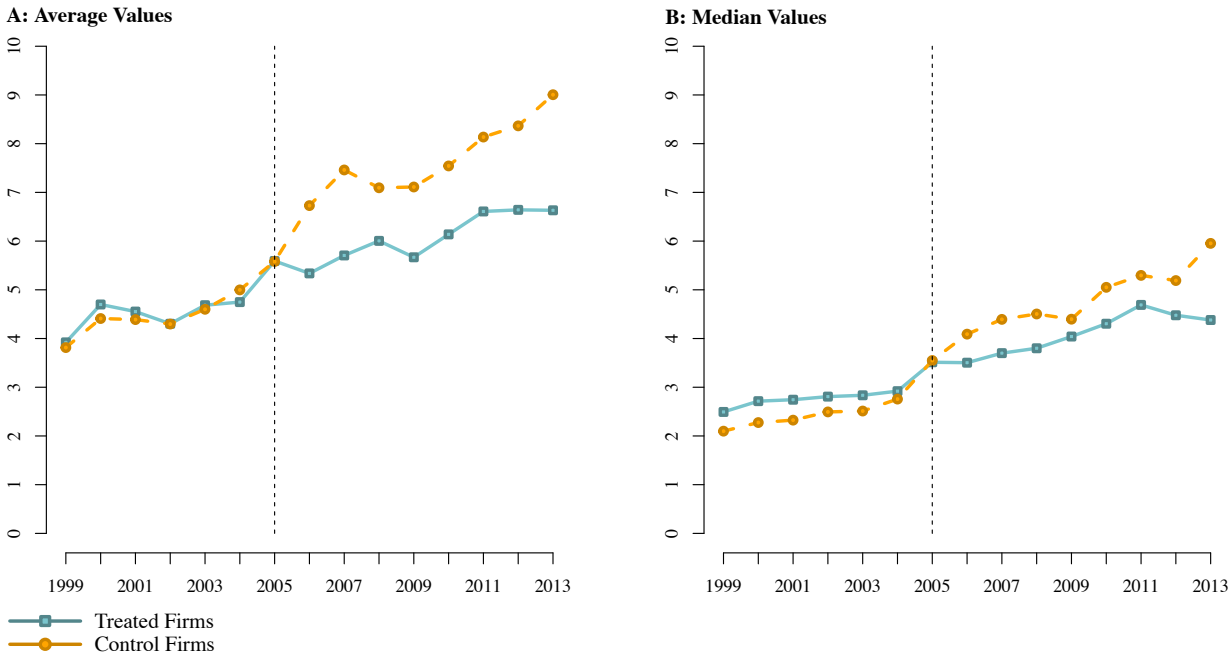

Table I

Comparison of Systems of Corporate Governance and Say-on-Pay Sweden vs. the U.S.

This table summarizes the main differences between the systems of corporate governance and say-on-pay in Sweden and the U.S.. Panel A reports on corporate governance, panel B on say-on-pay and panel C shows the timeline of the Swedish say-on-pay legislation.

	Sweden	U.S.
A: General Governance		
Is the Nomination Committee Independent?	Yes	Yes
Who Controls the Nomination Committee?	Large Shareholders	Board of Directors
Is the Remuneration Committee Independent?	Yes	Yes (Since 2002)
Who Controls the Remuneration Committee?	Large Shareholders	Board of Directors
Do Employees have Board Representation?	Yes	No
Is Shareholder Nomination (Proxy Contest) Available?	Yes	Yes (costly)
B: Features of Say-on-Pay		
Is Say-on-Pay Binding or Advisory?	Binding	Advisory
Does the Vote Concern CEO Pay?	Yes	Yes
Does the Vote Concern Directors' Pay?	Yes	No
What is the Voting Frequency?	Annually	Every Third Year (but this can be amended in the by-laws)
Is the Vote Forward-Looking?	Yes	No
What do Shareholders Vote on?	Individual Components	Whole Package

C: Chronology of the Say-on-Pay Legislation	
March 16, 2006	The government officially proposes say-on-pay and submits the proposition to parliament for voting.
June 1, 2006	The proposition is passed in parliament.
July 1, 2006	Say-on-pay is implemented. As of this date, all publicly listed firms have to facilitate a say-on-pay vote at the annual shareholders' meeting.

Table II
Variable Definitions

This table defines the variables used in the analysis and states their sources. The data combines information from 7 different registers. All corporate insiders were identified by *The Swedish Companies Registration Office*. Compensation data was gathered from annual reports and from *The Financial Supervisory Authority of Sweden*. Ownership data was provided by *SIS Ownership Service*. Accounting and stock price data was provided by *Bisnode* and *FINBAS*.

Name	Definition
A: Compensation	
Salary[1]	Annual cash salary in SEK.
Bonus[1]	Annual cash bonus (earned during the year) in SEK.
Other Cash[1]	All other cash-based compensation received during the year.
Pension Contributions[1]	Annual payments to defined contribution pension plans in SEK.
Grants of Stocks[1,2]	The grant date value of stock awards. Equals the number of Stocks times the share prices at the date of grant. If shares are bought at a discount, the purchase value is subtracted.
Grants of Options[1,2]	The grant date value of option awards. Equals the Black-Scholes value. If options are bought at a discount, the purchase value is subtracted.
Total Compensation[1,2]	Sum of cash compensation and grants of equity-based compensation.
Pay-Performance Sensitivity[1,2]	Equals the sum of the number of shares owned and the number of options owned times their delta, divided by the total number of shares outstanding.
%-Incentives[1,2]	The ratio of equity-based compensation and total compensation.
B: CEO Characteristics	
Age[4]	The age of the CEO in years. Equals the current year minus the year of birth.
Director[4]	Dummy equal to one when the CEO sits on the board.
Female[4]	Dummy equal to one for female CEOs.
Field of Education[1,8]	Categorical variable indicating the main field of education. Can take the values *Business*, *Engineering*, *Law* or *Other*.
Foreign[4]	Dummy equal to one for foreign CEOs.
Level of Education[1,8]	Categorical variable indicating the highest level of education. Can take the values *B.Sc or Lower*, *M.Sc or Higher* or *Other*.
CEO Ownership[2,4]	The voting rights of the CEO.
Tenure[4]	The tenure of the CEO in years. Equals the current year minus the year of appointment.
Urban[4]	Dummy equal to one if the CEO was born in one of the three main cities of Sweden (Stockholm, Gothenburg or Malmö).
C: Board Characteristics	
Age[4]	The average age of directors.
Busy[2]	The average number of outside directorships held by the directors.
Dependence[2]	The percentage of dependent directors. A director is considered dependent if she belongs to the top management team, is a relative to someone who belongs to the top management team or if she owns more than 10% of the firm's shares.
Employee Representatives[4]	The percentage of directors elected by the employees.
Gender[4]	The percentage of female directors.
Board Ownership[2,4]	The combined voting rights of all directors net of the CEO (if she sits on the board).
Size[4]	The total number of directors.
D: Firm Characteristics	
Book-to-Market[1,5]	The ratio of the book value and the market value of equity.
Cash[1,5]	The ratio of cash and total assets.
Industry[6]	GICS 10 industry classification.
Largest Owner[7]	The ownership stake of the largest owner net of the CEO.
Leverage[1,5]	The ratio of total liabilities and total assets.
ROA[1,5]	The ratio of EBITDA and total assets.
Size[1,5]	The natural logarithm of total assets.
Turnover[4]	Dummy equal to one in the year of CEO turnover.

[1]Annual Reports, [2]The Financial Supervisory Authority of Sweden, [3]Statistics Sweden, [4]The Swedish Companies Registration Office, [5]BisNode, [6]FINBAS (Swedish House of Finance), [7]SIS Ownership Service, [8]Google, LinkedIn and Facebook

Table III
Summary Statistics

This table shows summary statistics for the firms analyzed in this paper. The treatment group consists of all firms where the primary owner controlled less than 27% of the votes in the year prior to treatment. Panel A summarizes the compensation variables in the pre- and post-treatment periods. Panel B shows the ownership stake of the primary owner around treatment. Lastly, panel C shows summary statistics for the control variables in the pre-treatment period. The last column tests the difference in means between treated and control firms using a t-test. For variable definitions, see Table I. The full sample comprises 236 listed Swedish firms observed over the period 1999-2013. Say-on-pay was enacted in 2006. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Ownership

	Treatment Group			Control Group			
	Obs.	Mean	Std.	Obs.	Mean	Std.	Diff.
Pre-Treatment (%)	604	18.90	6.60	805	46.70	5.60	−27.80***
Post-Treatment (%)	694	17.20	4.00	924	45.80	3.50	−28.60***

B: Compensation

	Treatment Group			Control Group			
	Obs.	Mean	Std.	Obs.	Mean	Std.	Diff.
I: Pre-Treatment Period							
Salary (MSEK)	665	2.39	1.98	714	2.50	2.04	−0.12
Bonus (MSEK)	665	0.64	1.42	714	0.65	1.51	0.00
Other Cash (MSEK)	664	0.09	0.56	714	0.06	0.23	0.03
Pension (MSEK)	668	1.35	2.98	712	1.16	2.12	0.19
Option Grants (MSEK)	684	0.23	1.11	721	0.24	1.40	−0.01
Stock Grants (MSEK)	684	0.00	0.05	721	0.00	0.04	0.00
Total Compensation (MSEK)	665	4.69	5.88	714	4.62	4.98	0.08
%-Incentives (%)	665	3.03	8.95	713	3.09	10.93	−0.06
Pay-Performance Sensitivity (%)	684	2.36	5.25	721	8.13	16.45	−5.78***
Average Worker's Pay (MSEK)	661	0.37	0.12	713	0.31	0.12	0.06***
Pay Gap (Ratio)	642	13.03	17.27	702	16.64	19.79	−3.61***
II: Post-Treatment Period							
Salary (MSEK)	796	3.33	2.46	838	3.97	3.15	−0.65***
Bonus (MSEK)	796	1.00	1.77	838	1.34	2.47	−0.34***
Other Cash (MSEK)	796	0.19	0.57	838	0.18	0.88	0.01
Pension (MSEK)	795	1.29	1.72	838	1.76	3.19	−0.47***
Option Grants (MSEK)	796	0.18	0.77	840	0.25	2.89	−0.07
Stock Grants (MSEK)	796	0.06	0.40	840	0.11	0.95	−0.06
Total Compensation (MSEK)	796	6.04	5.26	838	7.62	8.51	−1.58***
%-Incentives (%)	796	2.57	8.20	838	2.30	8.45	0.27
Pay-Performance Sensitivity (%)	796	1.39	3.52	840	6.76	15.77	−5.36***
Average Worker's Pay (MSEK)	771	0.43	0.15	831	0.39	0.18	0.04***
Pay Gap (Ratio)	770	15.10	16.71	830	22.88	29.42	−7.78***

C: Controls

	Treatment Group			Control Group			
	Obs.	Mean	Std.	Obs.	Mean	Std.	Diff.
I: CEO Characteristics							
Age (Years)	684	48.45	7.00	721	49.65	7.32	−1.20***
Director (Dummy)	684	70.76	45.52	721	74.48	43.63	−3.72
Female (Dummy)	684	2.19	14.66	721	0.83	9.09	1.36**
Field of Education							
Economics	684	35.67	47.94	721	40.50	49.12	−4.83*
Engineering	684	32.02	46.69	721	23.86	42.65	8.16***
Law	684	1.61	12.59	721	3.33	17.95	−1.72**
Other	684	30.70	46.16	721	32.32	46.8	−1.61
Foreign (Dummy)	684	7.31	26.05	721	5.41	22.64	1.90
Level of Education							
B.Sc or lower	684	16.96	37.55	721	21.36	41.01	−4.40**
M.Sc or higher	684	72.95	44.45	721	71.15	45.34	1.90

Table II Continued from Previous Page

Other	684	10.09	30.14	721	7.49	26.34	2.60*
Ownership (%)	684	3.50	9.22	721	12.74	24.04	−9.24***
Tenure (Years)	684	4.25	3.01	721	5.05	3.34	−0.80***
Urban (Dummy)	684	44.59	49.74	721	36.89	48.29	7.70***
II: Board Characteristics							
Age (Years)	676	52.76	4.16	719	53.88	3.75	−1.11***
Busy (Number)	676	1.80	0.65	719	1.81	0.70	−0.01
Dependence (%)	676	12.78	11.23	719	17.90	14.04	−5.13***
Emp. Rep. (%)	676	10.16	12.24	719	10.97	12.54	−0.82
Gender Mix (%)	676	10.97	11.03	719	9.60	11.24	1.37**
Ownership (%)	676	3.85	7.35	719	8.00	17.25	−4.15***
Size (Number)	676	7.36	2.09	719	7.47	2.24	−0.11
III: Firm Characteristics							
Assets (BSEK)	663	8.78	32.96	714	10.63	25.42	−1.85
Book-to-Market (%)	663	56.56	48.90	714	66.11	47.63	−9.55***
Cash (%)	663	13.36	15.44	714	10.23	13.50	3.13***
Industry							
Consumer Discretionary	684	8.92	28.52	721	14.84	35.57	−5.92***
Consumer Staples	684	1.02	10.07	721	2.36	15.18	−1.33*
Energy	684	0.44	6.61	721	0.83	9.09	−0.39
Financials & Real Estate	684	14.18	34.91	721	19.14	39.37	−4.96**
Health Care	684	8.33	27.66	721	10.26	30.37	−1.93
Industrials	684	24.71	43.16	721	29.54	45.65	−4.83**
Information Technology	684	37.87	48.54	721	14.29	35.02	23.58***
Materials	684	4.53	20.82	721	6.10	23.95	−1.57
Telecommunication Services	684	0.00	0.00	721	2.64	16.03	−2.64***
Leverage (%)	663	44.89	20.59	714	44.77	21.06	0.12
ROA (%)	663	−9.45	42.97	714	1.75	30.60	−11.20***
Turnover (Dummy)	684	15.20	35.93	721	12.07	32.60	3.14*
Volatility (%)	684	57.17	37.15	721	43.96	25.37	13.21***

Table IV

Pre-Treatment Pay Levels and CEO and Board Characteristics

This table shows sample split differences in 1) unexplained CEO pay and 2) total CEO pay in the two-year period prior to the implementation of say-on-pay. Unexplained CEO pay is defined as the residuals from the following cross-sectional model

$$Comp_i = \alpha + \beta_2 Size_i + \beta_3 Leverage_i + \beta_4 Cash_i + \beta_5 ROA_i + \beta_6 BTM_i + \lambda_{Ind} + \varepsilon_i$$

, where *Comp.* is the sum of salary, bonus, other cash compensation, pensions and grants of equity-based compensation and λ_{Ind} is a set of industry-fixed effects. The sample is split based on the median value of a certain CEO or board characteristic in each year. The average difference between above-median and below-median firms is tested using a simple t-test and reported below. Panel A tests for differences in unexplained CEO pay, whereas panel B tests for differences in total CEO pay. CEO and board characteristics that are either significant or insignificant in both groups are not reported. For variable definitions, see Table I. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	Treatment Group		Control Group	
	2005	2004	2005	2004
	(1)	(2)	(3)	(4)
A: Differences in Unexplained Pay				
CEO Age	0.93*	1.35***	−1.75*	−0.32
	(0.10)	(0.00)	(0.06)	(0.66)
B: Differences in Total Pay				
CEO Age	3.38***	3.80***	−0.62	0.41
	(0.00)	(0.00)	(0.57)	(0.66)
CEO Director	3.03***	2.28***	0.53	0.68
	(0.00)	(0.01)	(0.53)	(0.51)
Board Age	2.05*	1.75*	−0.89	0.06
	(0.08)	(0.08)	(0.43)	(0.95)

Table V
The Effect of Say-on-Pay on CEO Compensation

This table shows coefficient estimates from the following difference-in-differences specification

$$Compensation_{ist} = \alpha + \beta_1 Treated_s \times After_t + \beta_2 After_t + \beta_3 Treated_s + \beta_4 X_{ist} + \varepsilon_{ist}$$

, where the dependent variable measures either *Total Compensation* (columns 1-2 of panel A), *%-Incentives* (column 3 of panel A), *PPS* (column 4 of panel A) or individual components (columns 1-5 of panel B). *Total Compensation* equals the sum of *Salary*, *Bonus*, *Other Cash*, *Pensions* and *Grants of Stocks and Options*. *%-Incentives* equals the ratio of the annual grants of equity-based compensation and total compensation. *PPS* equals the sum of the number of shares owned and the number of options owned times their delta, divided by the total number of shares outstanding. X is a vector of control variables that includes *CEO Age*, *CEO Director*, *CEO Female*, *CEO Field of Education*, *CEO Foreign*, *CEO Level of Education*, *CEO Ownership*, *CEO Tenure*, *CEO Urban*, *Board Age*, *Board Busy*, *Board Dependence*, *Board Emp. Rep.*, *Board Gender Mix*, *Board Ownership*, *Board Size*, *Book-to-Market*, *Cash*, *Leverage*, *ROA*, *Size* and *Volatility*. All regressions except the first one include year- and firm-fixed effects. For variable definitions, see Table I. The panel comprises 236 firms observed over 15 years (1999-2013). Say-on-Pay was enacted in 2006. I use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Total Compensation and Incentive Strength

	Total Compensation		Incentive Strength	
			% Inc.	PPS
	(1)	(2)	(3)	(4)
Treated×After	−1.63***	−1.06**	−0.02	0.14
	(0.00)	(0.02)	(0.98)	(0.90)
After	2.85***	-	-	-
	(0.00)	-	-	-
Treated	0.83	-	-	-
	(0.20)	-	-	-
Constant	5.18***	-	-	-
	(0.00)	-	-	-
Additional Controls	No	Yes	Yes	Yes
Year FEs	No	Yes	Yes	Yes
Firm FEs	No	Yes	Yes	Yes
Adj. R^2	7.49%	9.78%	−5.15%	12.78%
Observations	[3013]	[2951]	[2950]	[2978]

B: Individual Components

	Salary	Bonus	Other	Pension	Equity
	(1)	(2)	(3)	(4)	(5)
Treated×After	−0.25**	−0.17	0.00	−0.58**	−0.07
	(0.04)	(0.34)	(0.99)	(0.02)	(0.60)
Additional Controls	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Adj. R^2	29.79%	2.20%	−5.50%	7.29%	−5.14
Observations	[2951]	[2951]	[2950]	[2951]	[2978]

Table VI
Placebo, Causality and Robustness

Panel A shows the results of two placebo regressions and a modified version of the Granger causality test. Columns 1-2 shows the basic regression in Table IV, using pre-treatment observations only. In column 1, treatment is falsely defines to occur in 2004 and in column 2, treatment is set to occur in 2005. In columns 3-4, I add leads and lags to the model according to the following specification

$$Comp_{ist} = \alpha + \lambda_t + \sum_{\tau=0}^{m} \beta_{-\tau} D_{s,t-\tau} + \sum_{\tau=1}^{q} \beta_{+\tau} D_{s,t+\tau} + \delta Treated_s + \gamma X_{ist} + \varepsilon_{ist}$$

, where D_{st} equals the $Treated_s \times After_t$ switch, λ_t is a set of time-fixed effects and q and m are the numbers of lead and lagged interaction terms respectively. Column 3 includes only leads whereas column 4 features a full set of leads and lags. Panel B shows the result of two robustness tests. In the first column, the sample is truncated to include only firms where the ownership stake of the primary owner either exceeds 35% or falls below 25%. The 25-35% interval is disregarded. In the second column, the sample period is restricted to the years 1999-2007. *Total Compensation* equals the sum of *Salary*, *Bonus*, *Other Cash*, *Pensions* and *Grants of Stocks and Options*. Additional controls includes *CEO Age*, *CEO Director*, *CEO Female*, *CEO Field of Education*, *CEO Foreign*, *CEO Level of Education*, *CEO Ownership*, *CEO Tenure*, *CEO Urban*, *Board Age*, *Board Busy*, *Board Dependence*, *Board Emp. Rep.*, *Board Gender Mix*, *Board Ownership*, *Board Size*, *Book-to-Market*, *Cash*, *Leverage*, *ROA*, *Size* and *Volatility*. All regressions include year- and firm-fixed effects. For variable definitions, see Table I. The panel comprises 236 firms observed over the period 1999-2013. I use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Placebo and Causality

	Dep. Variable = Total Compensation			
	(1)	(2)	(3)	(4)
Treated×After	0.06	0.24	−1.22***	−1.19**
	(0.87)	(0.49)	(0.01)	(0.02)
Leads (Post-Treatment Effects)				
Treated×After$_{+6}$	-	-	0.43	0.43
	-	-	(0.51)	(0.51)
Treated×After$_{+5}$	-	-	−0.11	−0.11
	-	-	(0.85)	(0.84)
Treated×After$_{+4}$	-	-	−0.06	−0.06
	-	-	(0.85)	(0.84)
Treated×After$_{+3}$	-	-	0.08	0.08
	-	-	(0.83)	(0.83)
Treated×After$_{+2}$	-	-	−0.29	−0.29
	-	-	(0.52)	(0.52)
Treated×After$_{+1}$	-	-	0.38	0.37
	-	-	(0.22)	(0.22)
Lags (Anticipatory Effects)				
Treated×After$_{-1}$	-	-	-	−0.36
	-	-	-	(0.34)
Treated×After$_{-2}$	-	-	-	0.82
	-	-	-	(0.31)
Treated×After$_{-3}$	-	-	-	−0.31
	-	-	-	(0.59)
Treated×After$_{-4}$	-	-	-	0.00
	-	-	-	(0.99)
Treated×After$_{-5}$	-	-	-	−0.08
	-	-	-	(0.85)
Treated×After$_{-6}$	-	-	-	−0.46
	-	-	-	(0.28)
Additional Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Adj. R^2	−3.99%	−3.95%	9.60%	9.50%
Observations	[1351]	[1351]	[2951]	[2951]

Table VI Continued from Previous Page

B: Robustness

	Dep. Variable = Total Compensation	
	Sample Restricted to Firms Outside the 25-35% Ownership Interval (1)	Sample Restricted to the 1999-2007 Period (2)
Treated×After	−1.24**	−1.07**
	(0.04)	(0.03)
Additional Controls	Yes	Yes
Year FEs	Yes	Yes
Firm FEs	Yes	Yes
Adj. R^2	8.92%	8.12%
Observations	[1813]	[2358]

Table VII

The Effect of the Timing of the Annual General Meeting on Compensation

This table shows coefficient estimates from the following regression

$$\Delta Comp._i = \alpha + \beta_1 Treated_i \times AGM_i + \beta_2 AGM_i + \beta_3 \Delta X_i + \varepsilon_i$$

, where Δ denotes the change from 2005 to 2006 and *AGM* is the distance in days between the announcement date and the annual general meeting in 2006. *Comp.* is regressed on a the distance variable plus the change in the control variables used in Table IV from 2005 to 2006. Panel A shoes summary statistics for the distance variable and panel B shows the regressional results. *Comp.* equals the sum of *Salary*, *Bonus*, *Other Cash*, *Pensions* and *Grants of Stocks and Options*. Additional controls includes *CEO Age*, *CEO Director*, *CEO Female*, *CEO Foreign*, *CEO Ownership*, *CEO Tenure*, *CEO Urban*, *Board Age*, *Board Busy*, *Board Dependence*, *Board Emp. Rep.*, *Board Gender Mix*, *Board Ownership*, *Board Size*, *Book-to-Market*, *Cash*, *Leverage*, *ROA*, *Size* and *Volatility*. For variable definitions, see Table I. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Summary Statistics[1]

	Obs.	Min.	Med.	Mean	Max.
Full Sample	236	−119.00	41.00	39.83	211.00
Treatment Group	118	−119.00	41.00	36.73	211.00
Control Group	118	−94.00	42.00	43.36	193.00

[1]The difference between the two groups in the distance variable is not statistically significant (p-value = 0.25).

B: Regressional Results

	Dep. Variable = ΔTotal Compensation		
	Treatment Group (1)	Control Group (2)	Full Sample (3)
Treated×Distance to AGM	-	-	−0.02**
	-	-	(0.03)
Distance to AGM	−0.01**	0.02	0.02*
	(0.05)	(0.13)	(0.08)
Additional Controls	Yes	Yes	Yes
Adj. R^2	27.99%	13.48%	12.95%
Observations	[118]	[118]	[232]

Table VIII
Private Benefit of Control

This table shows coefficient estimates from the following specification

$$Comp._{it} = \alpha + \beta_1 Levered\ Ownership_i \times After_t + \beta_2 After_t + \beta_3 Levered\ Ownership_i + \beta_4 X_{it} + \varepsilon_{it}$$

, where *Levered Ownership* takes the value one if the ratio of the control rights and the cash flow rights of the primary owner exceeds the within-group median value in the year before say-on-pay. Panel A shows summary statistics for *Levered Ownership* in the pre- and post-treatment periods. Panel B reports the regressional results. *Comp.* equals the sum of *Salary*, *Bonus*, *Other Cash*, *Pensions* and *Grants of Stocks and Options*. Additional controls includes *CEO Age*, *CEO Director*, *CEO Female*, *CEO Field of Education*, *CEO Foreign*, *CEO Level of Education*, *CEO Ownership*, *CEO Tenure*, *CEO Urban*, *Board Age*, *Board Busy*, *Board Dependence*, *Board Emp. Rep.*, *Board Gender Mix*, *Board Ownership*, *Board Size*, *Book-to-Market*, *Cash*, *Leverage*, *ROA*, *Size* and *Volatility*. All regressions except the first one include year- and firm-fixed effects. For variable definitions, see Table I. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Summary Statistics

	Obs.	Min.	Med.	Mean	Max.
Full Period	1551	0.64	1.55	1.79	8.08
Post-Treatment	830	0.64	1.55	1.79	8.08
Pre-Treatment	721	0.64	1.54	1.79	5.70

B: Regressional Results

	Dep. Variable = Total Compensation		
	(1)	(2)	(3)[1]
Levered Ownership×After	0.00	0.01	1.27
	(0.75)	(0.12)	(0.14)
After	2.41*	-	-
	(0.07)	-	-
Levered Ownership	0.02**	-	-
	(0.01)	-	-
Constant	1.92	-	-
	(0.11)	-	-
Additional Controls	No	Yes	Yes
Year FEs	No	Yes	Yes
Firm FEs	No	Yes	Yes
Adj. R^2	14.40%	14.84%	14.34%
Observations	[1544]	[1527]	[1535]

[1]In this column, *Levered Ownership* equals a dummy for whether ownership leverage is above-median in the year prior to the implementation of say-on-pay.

Table IX
Other Measures of Pay

This table shows the coefficient estimates from the following model

$$Comp_{ist} = \alpha + \beta_1 Treated_s \times After_t + \beta_2 After_t + \beta_3 Treated_s + \beta_4 X_{ist} + \varepsilon_{ist}$$

, where, in column 1, *Comp.* denotes the *Average Worker's Pay* in the firm, and in column 2, measures the *Pay Gap* between the CEO and the average worker in the firm. *Average Worker's Pay* is defined as the the total remuneration cost stated in the consolidated accounts less the total compensation of the CEO and the board divided by the average number of employees. *Pay Gap* is defined as the total compensation of the CEO divided by the average worker's pay in the firm. Panel A summarizes the variables. Panel B reports the regressional results. Additional controls includes *CEO Age*, *CEO Director*, *CEO Female*, *CEO Field of Education*, *CEO Foreign*, *CEO Level of Education*, *CEO Ownership*, *CEO Tenure*, *CEO Urban*, *Board Age*, *Board Busy*, *Board Dependence*, *Board Emp. Rep.*, *Board Gender Mix*, *Board Ownership*, *Board Size*, *Book-to-Market*, *Cash*, *Leverage*, *ROA*, *Size* and *Volatility*. For variable definitions, see Table I. The panel comprises 236 firms observed over 15 years (1999-2013). I use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Summary Statistics

	Obs.	Min.	Med.	Mean	Max.
Average Worker's Pay (MSEK)	2970	0.03	0.36	0.38	2.31
Pay Gap (MSEK)	2944	0.00	9.66	17.21	351.75

B: Regressional Results

	Average Worker's Pay	Pay Gap
	(1)	(2)
Treated×After	0.00	−4.02**
	(0.96)	(0.01)
Additional Controls	Yes	Yes
Year FEs	Yes	Yes
Firm FEs	Yes	Yes
Adj. R^2	8.29%	0.39%
Observations	[2974]	[2942]

Table X
Sample Splits

This table shows sample split differences in compensation around the implementation of say-on-pay. The change in compensation between 2005 and 2006 is tested for different sub-samples of the data. The sample is split based on the median values of various CEO and board characteristics in the year prior to the implementation of say-on-pay. The difference between above-median and below-median firms is tested using a t-test. In panel A, the sample is split by abnormal compensation in the year prior to the event. Abnormal compensation is measured as the residuals from following cross-sectional regression using data from 2005.

$$Comp_i = \alpha + \beta_1 CEO.Age_i + \beta_2 Size_i + \beta_3 Leverage_i + \beta_4 Cash_i + \beta_5 ROA_i + \beta_6 BTM_i + \lambda_{Ind} + \varepsilon_i$$

Panels B and C split the sample on CEO and board characteristics respectively. *Diff.* shows the difference in the pay adjustment between above-median and below-median firms. Similarly, the *Coef.* reports the coefficient estimate for D_i from the following specification

$$\Delta Comp_i = \alpha + \beta_1 D_i + \beta_2 Turnover_i + \beta_3 \Delta Size_i + \beta_4 \Delta Leverage_i + \beta_5 \Delta Cash_i + \beta_6 \Delta ROA_i + \beta_7 \Delta BTM_i + \lambda_{Ind} + \varepsilon_i$$

, where D_i denotes the above-median switch for the variable of interest. The last column shows the coefficient estimate of the interaction term $Treat_i \times D_i$ in the following triple difference specification

$$\Delta Comp_i = \alpha + \beta_1 Treat_i \times D_i + \beta_2 Treat_i + \beta_3 D_i + \beta_4 Turnover_i + \beta_5 \Delta Size_i + \\ + \beta_6 \Delta Leverage_i + \beta_7 \Delta Cash_i + \beta_8 \Delta ROA_i + \beta_9 \Delta BTM_i + \lambda_{Ind} + \varepsilon_i$$

For variable definitions, see Table I. P-values are shown in parentheses. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	Treatment Group						Control Group						
	≥ Med.		< Med.				≥ Med.		< Med.				
	N	ΔComp.	N	ΔComp.	Diff.	Coef.	N	ΔComp.	N	ΔComp.	Diff.	Coef.	D-D-D
A: Abnormal Pay													
Abnormal Pay	54	−0.73	54	0.52	−1.25**	−1.31**	55	1.58	55	0.78	0.80	1.41	−2.56**
					(0.02)	(0.02)					(0.36)	(0.14)	(0.02)
B: CEO Characteristics													
Age	58	−0.57	53	0.25	−0.82	−0.68	62	1.03	49	1.36	−0.33	−0.21	−0.17
					(0.14)	(0.29)					(0.72)	(0.82)	(0.87)
Director	64	−0.38	47	0.10	−0.48	−0.52	67	0.83	44	1.70	−0.87	−0.75	0.04
					(0.36)	(0.41)					(0.39)	(0.46)	(0.97)
Ownership	56	0.24	55	−0.60	0.84	0.45	56	0.40	55	1.97	−1.57*	−1.60*	2.24**
					(0.13)	(0.47)					(0.07)	(0.10)	(0.04)
Tenure	60	−0.93	51	0.71	−1.64***	−1.49**	56	1.77	55	0.57	1.21	1.44	−2.74***
					(0.00)	(0.01)					(0.16)	(0.11)	(0.01)
C: Board Characteristics													
Age	57	−0.65	54	0.32	−0.97*	−0.62	57	1.19	54	1.16	0.03	−0.34	−0.19
					(0.08)	(0.36)					(0.97)	(0.71)	(0.86)
Busy	57	−0.41	54	0.06	−0.47	−0.73	57	1.92	54	0.39	1.53*	1.39	−1.88*
					(0.39)	(0.22)					(0.07)	(0.14)	(0.07)
Dependence	61	−0.20	50	−0.15	−0.06	−0.40	57	0.53	54	1.86	−1.33	−0.92	1.00
					(0.91)	(0.53)					(0.13)	(0.34)	(0.34)
Emp. Rep.	46	−0.54	65	0.08	−0.62	−0.38	47	1.24	64	1.13	0.11	−0.35	−1.19
					(0.26)	(0.58)					(0.90)	(0.74)	(0.27)
Gender Mix	62	0.17	49	−0.62	0.79	1.25**	63	0.59	48	1.94	−1.35	−1.29	2.39**
					(0.18)	(0.04)					(0.14)	(0.17)	(0.02)
Ownership	59	−0.04	52	−0.33	0.29	0.35	56	1.04	55	1.31	−0.27	−0.58	0.85
					(0.62)	(0.57)					(0.76)	(0.53)	(0.42)
Size	69	−0.54	42	0.42	−0.97**	−1.04	69	1.61	42	0.45	1.16	0.85	−2.23**
					(0.04)	(0.13)					(0.11)	(0.43)	(0.04)

Table XI
The Market Reaction to Say-on-Pay

This table shows the cumulative average abnormal returns (CAARs) for the three-day windows centered on three event dates. The first event date equals the day when the legislation was first proposed by the government, the second event date equals the day of enactment and the third event date equals the first day of implementation. Panel A tests for cross-sectional abnormal returns, whereas panel B tests for portfolio abnormal returns. Z_P and Z_K refers to the *Patell* and *Kolari* tests statistics. For a detailed description of these test statistics, see Appendix A. Abnormal returns for individual securities are estimated and then aggregated into CAARs using the market model.

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it}$$

, where R_{it} is the return of individual stocks and R_{mt} is the return of the market. Panel B uses the same approach to estimate abnormal returns for portfolios of firms belonging to the treatment and control groups. The bottom row in panel B reports the CARs of a long-short portfolio that longs the equally-weighted portfolio of all firms in the treatment group and and shorts the equally-weighted portfolio of all firms in the control group. The estimation window spans the 250 trading days prior to the event window. Two-sided test statistics are reported in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Cross-Sectional Abnormal Returns

	March 16, 2006				June 1, 2006				July 1, 2006			
	N	CAAR	Z_P	Z_K	N	CAAR	Z_P	Z_K	N	CAAR	Z_P	Z_K
Full Sample	216	0.24%	1.31	0.52	216	−0.19%	−0.82	−0.28	217	0.06%	0.89	0.29
Treatment Group	108	0.34%	1.64*	0.77	108	−0.44%	−1.29	−0.54	109	0.46%	1.81*	0.73
Control Group	108	0.14%	0.21	0.11	108	0.06%	0.13	0.06	108	−0.35%	−0.56	−0.23

B: Portfolio Abnormal Returns

	March 16, 2006			June 1, 2006			July 1, 2006		
	N	CAR	t	N	CAR	t	N	CAR	t
Full Sample	216	0.24%	0.46	216	−0.19%	−0.30	217	0.06%	0.08
Treatment Group	108	0.34%	0.53	108	−0.44%	−0.59	109	0.46%	0.58
Control Group	108	0.14%	0.27	108	0.06%	0.09	108	−0.35%	−0.51
Long-Short Portfolio	216	0.20%	0.39	216	−0.50%	−0.96	216	0.81%	1.53

Table XII

The Market Reaction to Say-on-Pay: Sample Splits

This table shows the same portfolio cumulative abnormal returns (CARs) as in Table XI, but for different sub-samples of the data. The sample is split in the same way as in Table X. The difference in portfolio abnormal returns between above-median and below-median firms is tested using a t-test. Abnormal returns for portfolios of securities are estimated using the market model. The bottom row in each panel reports the CARs of a long-short portfolio that longs the equally-weighted portfolio of all firms in the above-median group and and shorts the equally-weighted portfolio of all firms in the below-median group. The estimation window spans the 250 trading days prior to the event window. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	March 16, 2006								July 1, 2006							
	Treatment Group				Control Group				Treatment Group				Control Group			
	N	Mean	CAR	t	N	Mean	CAR	t	N	Mean	CAR	t	N	Mean	CAR	t
A: Abnormal Pay																
$\geq$ Med.	54	7.13	−0.12	−0.17	55	8.08	0.20	0.34	54	7.13	0.58	0.74	55	8.08	−0.37	−0.49
$<$ Med.	54	3.49	0.78	0.98	55	2.98	0.09	0.14	54	3.49	0.27	0.27	55	2.98	−0.46	−0.58
Long-Short	-	-	−0.90	−1.29	-	-	0.11	0.15	-	-	0.32	0.42	-	-	0.09	0.13
B: CEO Age																
$\geq$ Med.	58	53.93	0.24	0.34	62	55.00	−0.02	−0.03	58	53.93	0.30	0.37	62	55.00	−0.59	−0.85
$<$ Med.	53	42.96	0.44	0.58	49	43.69	0.33	0.51	53	42.96	0.64	0.70	49	43.69	−0.16	−0.20
Long-Short	-	-	−0.20	−0.27	-	-	−0.35	−0.55	-	-	−0.34	−0.48	-	-	−0.42	−0.63
C: CEO Tenure																
$\geq$ Med.	60	6.40	0.15	0.22	56	8.43	0.30	0.52	60	6.40	0.42	0.51	56	8.43	0.12	0.17
$<$ Med.	51	2.17	0.55	0.71	55	2.51	−0.05	−0.08	51	2.17	0.50	0.54	55	2.51	−0.98	−1.26
Long-Short	-	-	−0.40	−0.54	-	-	0.35	0.54	-	-	−0.07	−0.10	-	-	1.10	1.68*
D: CEO Ownership																
$\geq$ Med.	56	3.18	0.26	0.33	56	22.03	0.06	0.10	56	3.18	0.37	0.39	56	22.03	−0.04	−0.05
$<$ Med.	55	0.05	0.41	0.66	55	0.03	0.21	0.40	55	0.05	0.55	0.71	55	0.03	−0.77	−1.15
Long-Short	-	-	−0.16	−0.24	-	-	−0.14	−0.21	-	-	−0.18	−0.24	-	-	0.72	1.01
E: CEO Director																
$\geq$ Med.	64	100.00	0.58	0.89	67	100.00	0.24	0.44	64	100.00	0.54	0.68	67	100.00	0.34	0.48
$<$ Med.	47	0.00	−0.01	−0.81	44	0.00	−0.02	−0.03	47	0.00	0.35	0.35	44	0.00	−1.55	−1.81*
Long-Short	-	-	0.59	0.78	-	-	0.26	0.37	-	-	0.19	0.24	-	-	1.89	2.47***
F: Board Size																
$\geq$ Med.	69	8.72	0.35	0.54	69	8.78	0.13	0.28	69	8.72	0.69	0.90	69	8.78	−0.61	−0.93
$<$ Med.	42	5.17	0.31	0.36	42	5.10	0.15	0.17	42	5.17	0.08	0.08	42	5.10	−0.06	−0.07
Long-Short	-	-	0.04	0.06	-	-	−0.02	−0.03	-	-	0.62	0.75	-	-	−0.55	−0.67
G: Board Age																
$\geq$ Med.	57	56.52	0.48	0.69	57	57.64	−0.20	−0.33	57	56.52	0.82	0.99	57	57.64	−0.08	−0.12
$<$ Med.	54	49.81	0.17	0.23	54	51.24	0.47	0.78	54	49.81	0.06	0.07	54	51.24	−0.73	−0.91
Long-Short	-	-	0.30	0.45	-	-	−0.67	−1.01	-	-	0.75	1.02	-	-	0.65	0.99
H: Board Ownership																
$\geq$ Med.	59	4.93	0.05	0.06	56	12.10	0.37	0.59	59	4.93	0.76	0.81	56	12.10	−0.16	−0.20
$<$ Med.	52	0.03	0.66	0.99	55	0.04	−0.11	−0.18	52	0.03	0.11	0.14	55	0.04	−0.64	−0.95
Long-Short	-	-	−0.61	−0.85	-	-	0.48	0.72	-	-	0.65	0.83	-	-	0.47	0.69
I: Board Dependence																
$\geq$ Med.	61	19.97	0.32	0.43	57	28.75	0.13	0.20	61	19.97	0.95	1.07	57	28.75	0.09	0.12
$<$ Med.	50	2.18	0.35	0.53	54	6.09	0.14	0.24	50	2.18	−0.15	−0.18	54	6.09	−0.96	−1.32
Long-Short	-	-	−0.03	−0.04	-	-	−0.01	−0.02	-	-	1.10	1.53	-	-	1.06	1.50
J: Board Busyness																
$\geq$ Med.	57	2.12	0.69	1.06	57	2.14	−0.11	−0.21	57	2.12	0.79	0.99	57	2.14	−0.55	−0.84
$<$ Med.	54	1.27	−0.05	−0.06	54	1.26	0.39	0.56	54	1.27	0.11	0.12	54	1.26	−0.25	−0.29
Long-Short	-	-	0.74	1.18	-	-	−0.50	−0.73	-	-	0.68	1.01	-	-	−0.30	−0.45

Table XIII

The Effect of Say-on-Pay on Turnover and Performance

This table shows the coefficient estimates from the following specification

$$Outcome_i = \alpha + \beta_1 Treated_s \times After_t + \beta_2 After_t + \beta_3 Treated_s + \beta_4 X_{ist} + \varepsilon_{ist}$$

, where *Outcome* is either *Turnover*, *ROA Book-to-Market* or *Tobin's Q*. Panel A shows summary statistics for the outcome variables and panel B shows the regressional results. Additional controls includes *CEO Age*, *CEO Director*, *CEO Female*, *CEO Field of Education*, *CEO Foreign*, *CEO Level of Education*, *CEO Ownership*, *CEO Tenure*, *CEO Urban*, *Board Age*, *Board Busy*, *Board Dependence*, *Board Emp. Rep.*, *Board Gender Mix*, *Board Ownership*, *Board Size*, *Cash*, *Leverage*, *Size* and *Volatility*. For variable definitions, see Table I. All regressions include year- and firm-fixed effects. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Summary Statistics

	Treatment Group					Control Group				
	Pre		Post			Pre		Post		
	N	Mean	N	Mean	Diff.	N	Mean	N	Mean	Diff.
Turnover	684	15.2	796	11.56	−3.65**	721	12.07	840	6.79	−5.28***
ROA	663	−9.45	772	1.08	10.52***	714	1.75	831	6.46	4.71***
Book-to-Market	663	56.56	772	76.60	20.04***	714	66.11	831	77.91	11.80***
Tobin's Q	655	287.61	767	185.38	−102.23**	711	212.64	827	179.08	−33.56

B: Regressional Results

	Turnover	ROA	Book-to-Market	Tobin's Q
	(1)	(2)	(3)	(4)
Treated×After	−0.03	1.53	9.15	−0.47
	(0.21)	(0.48)	(0.11)	(0.19)
Additional Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Adj. R^2	8.34%	17.91%	8.86%	−6.05%
Observations	[2978]	[2978]	[2978]	[2958]

CEO WEALTH, INCENTIVES AND PRIVATE ASSET ALLOCATION

B. Espen Eckbo[†]

P. Johan E. Mellberg[‡]

Darius Palia[§]

Karin S. Thorburn[¶]

Abstract

Using unique data on the level and composition of private wealth, we analyze the link between risk aversion, the provision of incentives and the ability to hedge the risks embedded in the contract for a sample of CEOs of listed companies in Sweden. We first show that wealthier CEOs make riskier allocation decisions, which is consistent with wealth being a valid proxy for risk aversion. Second, we show that less risk-averse CEOs receive steeper incentive contracts in equilibrium, which is consistent with standard principal-agent theory. Third, we show that CEOs who receive stronger incentives make safer allocation decision for their own wealth, given their level of risk aversion. This result is consistent with risk-averse CEOs responding optimally to the provision of incentives. Lastly, we show that risky allocations are negatively related to the firm's systematic risk component but unrelated to the firm's idiosyncratic risk component, which suggests that CEOs are mainly using their private wealth to adjust the exposure to market risk. Consistent with this result, we also find that the return of the private portfolio is only weakly correlated with the firm and highly correlated with the market index and that its composition is not sensitive to either incentive strength or firm risk. This suggests that CEOs hold relatively well-diversified portfolios. The main channel through which CEOs adjust their exposure to risk seems to be by allocating funds between the market portfolio and the risk-free asset.

Key Words: Corporate Finance, Corporate Governance, Executive Compensation, Incentives, Shareholder Monitoring
JEL Classifications: G32, G34

[†]Dartmouth College, Norwegian School of Economics (NHH)
[‡]Norwegian School of Economics (NHH)
[§]Rutgers University
[¶]Norwegian School of Economics (NHH)

I Introduction

Agency theory advocates the use of equity-based compensation to align the interests of shareholders and managers. By linking pay to performance, shareholders can mitigate the moral hazard problem, which causes the manager to exert too little effort relative to first best. However, forcing the manager to carry firm-specific risk, which is essential for generating the right incentives, also imposes a personal cost, as the manager is forced to hold a less-than-fully-diversified investment portfolio, a problem that is exacerbated by the fact that grants of options and restricted stock are seldom indexed to the market. This cost is of great interest to shareholders because it may incentivize managers to hedge against the risks embedded in their compensation contracts, potentially undoing the incentive effect.

In this paper, we provide new insights on the efficiency of managerial incentives by examining the link between risk aversion, incentives and private allocation decisions. Specifically, we use data on the level of wealth and how that wealth is allocated across different asset classes to test whether wealthier CEOs make riskier investment decisions and receive stronger incentives in equilibrium. We also test whether the level of wealth and incentive strength has any impact on how CEOs allocate their wealth. The main question we ask is whether CEOs systematically adjust their private portfolios in response to the provision of incentives.

Determining whether monetary incentives mitigate conflicts of interest between owners and managers is difficult since the observed contracts depend on personal characteristics that are hard to measure. Moreover, standard models make simplifying assumptions that are unlikely to hold in reality, such as the CEO's inability to privately adjust her exposure to firm risk. Our study addresses both of these issues. The basis of our analysis is the idea that private allocation decisions are predominantly the outcome of two contrasting forces, both attributable to the level of wealth held outside the firm. First, an individual's risk preferences are likely to be important - *ceteris paribus*, less risk-averse CEOs are expected to make riskier investments compared to more risk-averse CEOs. Secondly, incentive strength is expected to have implications for allocation decisions. A risk-averse CEO who receives high-powered incentives has an incentive to allocate

her private wealth in such a way so as to hedge the risks embedded in her incentive contract.

Despite its importance, to date there exist very little evidence on CEOs' attitudes toward risk and in particular the tendency to trade outside the insider portfolio. Becker (2006) proposes to use the level of private wealth as a proxy for absolute risk aversion and shows that wealthier CEOs indeed receive sharper incentive contracts, which is consistent with standard principal-agent theory. Jin (2002) and Garvey and Milbourn (2003) model the agency problem when the CEO can freely trade the market portfolio and show empirically that incentives are sensitive to firm-specific risk but unrelated to market risk, which is consistent with the predictions of the model. However, no study directly tests the assumption behind the model, namely that CEOs can (and do) trade in response to receiving incentives using their private wealth.

We perform our analysis in four steps. First, we examine the validity of the assumption that private wealth proxies for risk aversion. We use cross-sectional variation in the level of private wealth to test whether wealthier CEOs make riskier allocation decisions. If wealthier CEOs are indeed less risk averse, we expect them to invest a larger portion of their outside wealth in risky assets. We find support for this hypothesis. Our results show that wealthier CEOs allocate a larger portion of their outside wealth toward risky assets, such as equity, and away from cash. We also show that the relationship between wealth and risky allocation decisions is not subject to reversed causality. When we regress the level of wealth on lagged allocation decisions and include CEO-fixed effects, the coefficients of the allocation ratios come out insignificant.

Having established a positive link between wealth and private risk-taking, we then test if incentive strength is sensitive to private wealth. Standard principal-agent theory predicts a negative relationship between risk aversion and incentive strength. Our results confirm this hypothesis - wealthier CEOs receive steeper contracts. We also test for other interpretations of wealth, such as power and skill. Our results suggest that wealth is mainly a measure of risk aversion.

Third, we test whether CEOs diversify or hedge firm risk using their private wealth. Our data allows us to analyze how the investment behavior of the CEO changes in response to incentive changes, controlling for the level of wealth and other key inputs. The main question we ask

here is whether the provision of incentives is associated with safer allocation decisions, given the level of risk aversion. We find support for this hypothesis as well. Strong incentives associate with safer allocation decisions, which suggests that CEOs undo some of the risks embedded in the incentive contracts by adjusting their private portfolios. This finding is consistent with risk-averse CEOs responding optimally to the provision of incentives.

Forth, we examine the motives behind the observed allocation decisions. This is important because the results up to this point are consistent with both diversification of systematic risk, which constitutes a form of self-indexing, and hedging of idiosyncratic risk, which undoes part of the incentive effect. First, we show that there is a negative link between the systematic risk component of the firm's stock return and risky allocation decisions, controlling for both wealth and incentive strength, which suggests that CEO are mainly trying to diversify their exposure to systematic risk. Second, using instrument level data, we show that CEOs hold fairly diversified investment portfolios and that the compositions of these portfolios are not significantly affected by the provision of incentives. The main channel through which CEOs adjust their exposure to risk seems to be by allocating funds between the market portfolio and the risk-free asset.

Our study extends the literature on managerial incentives in several ways. First, we show that private wealth is a valid proxy for risk aversion. This is an important and previously unreported result. A person's true risk aversion is inherently difficult to measure and without a valid proxy of risk aversion, it is difficult to test the standard model. Second, we show that CEOs use private wealth to adjust the risks embedded in their incentives contracts. This should have direct implications for the ongoing debate on relative performance evaluation and the effectiveness of the use of equity-based compensation to incentivize managers. Our results provide important insights for the design of the optimal compensation contracts. To the best of our knowledge, this is the first paper to study whether CEOs actually adjust their private portfolios in response to the provision of incentives.

The remainder of this paper is organized as follows. Section II provides a theoretical background. Section III discusses the sample selection and data sources and presents summary statistics. Section IV presents the results and section V concludes.

II Theory and Hypotheses

II.A Wealth and Risk Aversion

Principal-agent theory assumes that agents are under-diversified and therefore risk-averse. In the context of CEO compensation, this assumption seems plausible, since managers often hold disproportionately large amounts of stock and options in their firms. In theory, risk aversion is captured by decreasing marginal utility of wealth, which implies that the agent will require a risk premium to accept equity-based compensation in replacement of cash. This is equivalent to saying that the incentive effect of equity-based compensation is decreasing in wealth. To see this, suppose that a CEO receives incentives which can either increase or decrease by $100 in value with equal probability. If the CEO is wealthy, this gamble will yield close to the same expected utility as receiving a cash reward equal to the grant value since her utility function is close to horizontal at the given level of wealth. Conversely, if the CEO is poor, she will value the gamble below its expected value. Intuitively, winning or loosing $100 when you are relatively wealthy is of little personal concern and provides only a small incentive effect.

II.B Risk Aversion and Incentives

A basic assumption in the standard model is that the manager, if not properly monitored, will indulge in activities that generate private benefits at the expense of shareholder value. Dispersed shareholders are either too unincentivized or too uninformed to perfectly monitor the manager. To mitigate the agency problem, Jensen and Meckling (1976) propose that managers are given an ownership stake in the firm. By aligning the interests of managers and shareholders, the former get an incentive to exert high effort and to minimize perquisite consumption.[1]

If managers are risk-averse, the optimal contract will be a trade-off between providing incentives and insurance to the CEO. On the one hand, a sharper contract will increase the expected value of the firm by inducing the manager to work harder and/or reduce perquisite consumption. On

[1] Monetary incentives is only one of many incentive mechanisms. Others include reputational concerns, competitive labor markets, takeover threats, dismissal and bankruptcy.

the other hand, it will increase the risk premium required by the manager. In fact, since it is always costly for risk-averse managers to carry firm-specific risk, the premium required to exert the desired level of effort is increasing in the risk aversion of the agent (Holmström and Milgrom, 1987; Holmström, 1979). One of the main predictions of the the model is therefore that incentives optimally decrease with the risk aversion of the CEO as it augments the risk premium required to exert the desired level of effort. If wealth proxies for negative risk aversion, we expect wealth to be positively related to the provision of incentives by the firm.

II.C Incentives and Allocation Decisions

One of the limitations of the standard model is that it implicitly assumes that agents are unable to adjust their exposure to firm risk - either CEOs are believed to hold all of their wealth in the firm or, alternatively, they hold private portfolios but are unable to trade. This is clearly an unrealistic assumption - in reality, CEOs can both buy and sell shares in the open market or hedge the embedded risks by allocating private wealth between different asset classes.

Systematic risk can be hedged by taking a short position in the market portfolio (Jenter, 2002; Jin, 2002; Garvey and Milbourn, 2003) and idiosyncratic risk can be hedged by selling a portfolio of highly correlated stocks or by using equity swap agreements and zero-cost collars as described by (Bettis, Bizjak and Lemmon, 2001). By hedging the systematic risk component, CEOs can effectively self-index their incentive contracts, which would explain the modest use of relative performance evaluation (Aggarwal and Samwick, 1999; Murphy, 1999). Both Jin (2002) and Garvey and Milbourn (2003) model the agency problem when the manager is allowed to trade the market portfolio. For incentive reasons, hedging of idiosyncratic risk is prohibited. Under these assumptions, they show that incentives optimally fall with idiosyncratic risk but stay unaffected by systematic risk. As a result, the extent of managerial hedging is increasing in systematic risk but is unaffected by idiosyncratic risk. Intuitively, since the agent can trade the market portfolio, she will adjust her exposure until her marginal cost of bearing systematic risk equals that of the principal. Thus, initial division of systematic risk through the incentive contract does not matter because any party can adjust suboptimal exposure to systematic risk by

trading the market portfolio. Alternatively, by hedging the idiosyncratic risk component, CEOs can partly undo the incentive effect. While CEOs are usually either explicitly or implicitly prohibited from engaging in open-market sales and direct hedging, the techniques mentioned above are harder to detect and not restricted in the same way.

III Sample Selection and Data Description

III.A Constructing the Sample

Our sample comprises all firms listed on the Stockholm Stock Exchange each year between 1999 and 2007. Firms incorporated abroad that were not subject to the same disclosure requirements are excluded due to lack of data. In total, our balanced panel comprises 157 firms. We also include 75 firms (for which we could find the relevant data) that were listed for only part of the sample period. Eleven firm-years had to be excluded since the sitting CEOs had no recorded wealth, most likely due to being registered as living abroad. The final sample comprises 232 firms and 478 CEOs, resulting in 1732 firm-years.

In order to access the necessary data on peoples' wealth, all CEOs in our sample first have to be identified using their personal identification numbers.[2] Under Swedish law, permission to do so can only be granted after filing a special application to the *The Swedish Research Council*. After getting this approval, the relevant personal identification numbers were provided by *The Swedish Companies Registration Office* and in order to protect the integrity of the persons included in the study, the resulting list of names and identification numbers was made anonymous upon data delivery by *Statistics Sweden*, i.e. it is not possible to link wealth to individual CEOs in the final data set.[3]

[2] All individuals registered as having Sweden as their country of residence are assigned an unique personal identification number consisting of the the date of birth (six digits) followed by a four digit control sequence. For Swedish citizens born in Sweden, this number is assigned from birth.

[3] All limited liability companies in Sweden must register with the Swedish Companies Registration Office. In addition to reporting on corporate actions and submitting financial reports, firms are required to provide information on all key insiders of the firm. The data set includes the name of the manager/director, corporate role(s), date of birth as well as start and end dates of the appointment.

III.B Incentive Strength

Throughout the paper, we use the CEO's pay-performance sensitivity, defined by Jensen and Murphy (1990) as the total change in CEO wealth resulting from a $1000 increase in shareholder wealth, as our measure of incentive strength. As pointed out by Jin (2002), this measure is consistent with standard agency models that solves for the agent's optimal percentage stake in the firm. We calculate the pay-performance sensitivity in accordance with Hall and Liebmann (1998) as the sum of the number of shares owned and the number of options owned times their delta, divided by the total number of shares outstanding. If incentives are provided only through shares, this measure equals the fraction of shares owned by the CEO. As such, our measure only captures the change in financial wealth and does not take into account changes in the CEO's capitalized future labor income. However, Hall and Liebmann (1998) conclude that changes in financial wealth accounts for nearly all of the change in pay–performance sensitivity. The benefit of our measure is that it can be measured relatively precisely compared with the more imprecise regression approach required to estimate the pay-performance sensitivity from capitalized future labor income changes. For a full description of how we calculate the pay-performance sensitivity, see Appendix A.

III.C Wealth and Compensation

Data on the total wealth of individual CEOs comes from *Statistics Sweden* and data on the wealth held inside the firm (incentives) is retrieved from *The Financial Supervisor Authority of Sweden*.[4] The raw data from *Statistics Sweden* include all types assets and can be broadly categorized into financial and real assets. Since the original data was collected for tax purposes, we only observe an end-of-the-year snapshot of each asset class/security. Exchange-traded instruments can be identified by their *International Securities Identification Number* (ISIN). There are two

[4] *Statistics Sweden* is a governmental body tasked with gathering and storing the official statistics of Sweden and its citizens. Up to 2008, individuals residing in Sweden were subject to wealth taxation and because of this, *The Swedish Tax Agency* collected annual control statements from banks and land registries on people's wealth. Taxation of wealth was abolished in 2008 and the government subsequently withdrew the mandate to collect information regarding private wealth the same year. Our sample period is therefore limited to the period ending in 2007.

main limitations in the data. First, financial assets held as part of a private pension plan are not included. Second, financial assets held within "capital insurances" are only reported as a total balance at the end of the year, i.e. we cannot observe the composition of the insurance. The reason is that tax rates on those two types of accounts depend merely on the account balances and not on actual capital gains.

Financial assets include cash deposits exceeding 10 KSEK (approximately $1500) and financial instruments held through both Swedish and foreign institutions. The reporting is mainly done automatically by banks and other financial institutions and the data cover fixed income securities, stocks, options, funds and endowment insurances. Exchange-traded instruments are given an end-of-year value using the closing price on the last day of trading. Unlisted shares are valued according to their book value. For other assets, the precision and comprehensiveness of the data will vary by the type of asset. In particular, assets held abroad may be underreported, since these holdings are self-reported and not retrieved directly by third parties. As a rule of thumb, for self-reported assets, such as smaller holdings of unlisted shares and assets held abroad, we cannot dismiss underreporting.

Real assets consist mainly of private houses and apartments, but also include commercial and agricultural real estates as well as summer house and personal chattel, such as art and collectibles. Real estate is valued by the authorities using an elaborate process aimed at approximating the market value. Whenever actual market values are not available, which may be the case if a certain property has been in a person's possession for a long time, that property is assigned a value based on realized selling prices on comparable objects in the same region.

In the analysis, we use private wealth (defined as wealth not invested in the firm) rather than total wealth to proxy for a person's risk aversion. The reason is that private wealth is less likely to be correlated with other CEO and firm characteristics. For example, if strong incentives associate with accumulated wealth over time, some unobserved CEO characteristic, such as power, that may have influenced the provision of incentives in the first place could explain the positive relationship between wealth and incentives. Since our data allow us to observe financial wealth on the instrument level, we can be very precise in our calculation of private wealth.

Rather than subtracting the value of firm wealth from the value of total wealth, which can result in negative values, we can clean the data from any holdings in the firm using ISIN codes (which results in zero cases of negative private wealth).

Compensation data is hand-collected from annual reports and include the following components; salary, bonus, other cash-based compensation (perks), pension contributions and grant values of equity-based compensation. Data on aggregate insider holdings come from both annual reports and *The Financial Supervisory Authority of Sweden*. Firms normally use the grant date closing price and the Black-Scholes formula to value equity-based compensation. However, the fact that there are no clear rules on how to disclose the value equity-based compensation opens up for minor inconsistencies in the reported values. To address this, we collect all relevant contract details from *The Financial Supervisory Authority of Sweden*, including grant dates, strike prices, share prices and expiration dates, and calculate both grant date and end-of-year values using the Black-Scholes formula for valuing European call options, as modified by (Merton, 1973). Whenever the calculated value deviates from the value reported in the annual report, we use the calculated value.

III.D Asset Allocations

In order to examine how allocation decisions relate to private wealth, we divide wealth into three broad asset classes based on their overall riskiness. These are *Cash*, *Real Estate* and *Equity*. In order to get meaningful measures of asset allocation, we create asset class ratios, which we define as the amount of private wealth invested in a certain asset class divided by total private wealth. By construction, this means that the any CEO in our sample can have 0-100% of her private wealth invested in either cash, real estates or equity. These measures should therefore capture heterogeneity in a CEO's personal holdings of risky assets.

III.E Controlling for Co-founding Factors

Throughout the paper, we control for a number of CEO, board and firm characteristics in our regressions. The firm-level variables common to most regressions include size (natural logarithm of sales), leverage, cash ratio (cash and short-term investments scaled by total assets), ROA (EBITDA scaled by total assets), book-to-market (book value to equity divided by market value of equity), volatility (annualized standard deviation in stock returns) and industry (GICS 10-industry classification). Size is included to address the empirical regularity that pay-performance sensitivity tend to decrease with firm size. Leverage and stock return volatility are included as proxies for firm risk. The cash ratio is included to control for liquidity constraints. We include ROA to control for the fact that bonuses are often linked to accounting performance. Book-to-market is included as a proxy for growth opportunities, which is expected to be positively correlated with the use of equity-based compensation.

In terms of CEO-specific controls, I include age, gender, tenure and director (a dummy equal to one if the CEO sits on the board). Age and tenure are standard controls in the literature. Gender controls for the documented gender gap in compensation and potential differences in risk aversion. Director captures differences between firms where the CEO have no voting power and firms where the CEO can vote and can be viewed as a proxy of directors' independence from the management. The board-specific controls include size (number of board members), gender mix (percentage of female directors), age (average age of directors), percentage of employee representatives, dependence (percentage of directors that are dependent with respect to management and large owners) and busy directors (average number of outside directorships). Similar to German boards, employee representation is statutory in Swedish boards. Since this influences all measures that use board size as a denominator, and since employee representatives might have different objectives compared to regular directors, we also include the percentage of directors elected by the employees in our regressions.

III.F Descriptives

Figure I shows the distribution of total wealth across firm wealth, which equals the market value of the aggregate holdings of firm stocks and options at the end of the year, and private wealth for the average CEO in our sample. Variable definitions are provided in Table I and summary statistics are reported in Table II. The figure provides several interesting insights. First, it is evident that CEOs are heavily invested in their firms. Firm wealth constitutes between 40% and 60% of total wealth each year, which confirms one of the main assumptions in the literature, namely that CEOs are under-diversified. Second, investments into real estates (mainly private housing) constitutes a major part of private wealth (50-60% on average). Third, CEOs tend to hold a sizable fraction of their wealth in either cash or equity, which suggests that some degree of diversification using private wealth is possible.

As for the level of private wealth, two things a noteworthy. First, Swedish CEOs must be considered relatively poor compared to U.S. CEOs. In the pooled sample, the 25^{th}, 50^{th} and 75^{th} percentiles of wealth are equal to MSEK 3.04 ($0.37 million), MSEK 5.96 ($0.72 million) and MSEK 12.95 ($1.56 million) respectively. Secondly, we observe considerable cross-sectional variation in our wealth measure. Panel B of Table II shows how the variation in private and firm wealth changes with the level of observation. The coefficient of variation (group level standard deviation divided by the mean) for private wealth drops by a factor of eight when we move from the variation of the pooled sample to within-firm variation. The same is true for firm wealth.

In terms of incentive strength, the CEOs in our sample own a relatively large fraction of the firm's equity compared to their U.S. counterparts. The average CEO has a calculated pay-performance sensitivity of 5.28, which means that private wealth increases by approximately 53 SEK for every 1000 SEK increase in shareholder wealth. Compared to the estimated effective percentage ownership of 0.325% in (Jensen and Murphy, 1990), our CEOs carry incentives that are about sixteen times stronger. The reason for this discrepancy is the large difference in firm sizes across countries. The our sample, the 25th, 50th and 75th percentiles of firm sales are approximately $36M, $133M and $433M respectively, which is small compared to U.S. firms.

Indeed, if we include only the largest firms (A-list firms on SSE), our incentive measure drops by a factor of 10 for the median CEO, which is comparable to the U.S. data.

IV Empirical Tests

IV.A Wealth and Risk Aversion

Our first set of tests examine whether private wealth is a valid proxy for risk aversion. Since the allocation of wealth is observable in the data, we can test this assumption directly. If wealthier individuals are in fact less risk-averse, we conjecture that they will on average invest a larger fraction of their private wealth in risky assets. In panel A of Table III, we run pooled regressions of our allocations ratios on the level of private wealth plus additional co-founding characteristics of the CEO. In column 1, the dependent variable equals the fraction of total private wealth invested in *Cash* at the end of the year, whereas in columns 2 and 3, the dependent variable equals the analogous measures for *Real Estate* and *Equity*. In all regressions, we control for *Age* and *Gender* as well as year fixed effects. The results show that private wealth is both positively correlated with risky allocations (*Equity*) and negatively correlated with safe allocations (*Cash* and *Real Estate*), which is consistent with the idea that private wealth is a valid proxy for risk aversion. All coefficients are significant at a 1% level.

The size of the coefficient of wealth in column 3 is 7.06, which means that a 1% increase in wealth corresponds approximately to an 0.0706 percentage point increase in the fraction of wealth held in equity. The effects on the fractions held in cash and real estate are equal to -2.78 and -7.68. Evaluated at the median, increasing private wealth by 1% (59,600 SEK) has a marginal effect on the fraction of wealth held in equity of approximately 4,200 SEK. Similarly, the marginal effects on *Cash* and *Real Estate* from increasing private wealth by the same amount are approximately -1,700 and -4,600 SEK. In terms of economic significance, a one standard deviation increase in *log(Wealth)* (1.83) increases the fraction private wealth held in equity by approximately half a standard deviation (std. dev. for *Equity* equals 25.33) whereas it decreases the fractions held

in cash and real estate by approximately a quarter and half a standard deviation respectively (std. dev. for *Cash* and *Real Estate* equal 19.61 and 33.53).

One potential issue in panel A is that the estimated relationship may be subject to reversed causality - CEOs that invest more in equity may accumulate more wealth over time due to some unobservable characteristic such as skill. To test this, in panel B of Table III, we regress the level of external wealth on past allocations of external wealth and CEO-fixed effects. If skill is driving the results in panel A, we conjecture that CEOs will accumulate more wealth moving forward whenever they allocate more of their private wealth toward risky assets. The results do not support this idea. All coefficients come out insignificant, suggesting that previous allocation decisions are not significant determinants of current wealth.

IV.B Risk Aversion and Incentive Strength

Next, we examine the link between incentives and risk aversion. Standard agency theory predicts this relationship to be negative and thus, we expect wealth to be positively related to incentive strength. In Table IV, we estimate the following model

$$Incentives_{it} = \alpha + \beta_1 log(Wealth_{it} + 1) + \beta_2 X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it} \quad (1)$$

, where *Incentives* equals the pay-performance sensitivity, *Wealth* equals the CEO's private wealth and X is a matrix of CEO-, board- and firm-specific controls. We do not include firm-fixed effects in our specification. As pointed out by Becker (2006), doing so would disregard all cross-sectional variation and exploit within variation only. Since most of the variation in wealth is cross-sectional, including firm-fixed effects would reduce the power of our tests considerably. Also, exploiting within variation could introduce additional endogeneity issues. A high-performing CEO who gets compensated accordingly in a given year might be wealthier in the next period and simultaneously receive stronger incentives moving forward.

The results are reported in column 1 of Table IV. The coefficient for wealth is positive and significant at a 1% level, which confirms the main result in Becker (2006) that less risk-averse

CEOs receive stronger incentives. The estimated effect is also economically significant. The coefficient for *log(Wealth)* equals 2.16, which suggests that a 1% increase in private wealth corresponds approximately to a 0.0216 percentage points increase in the CEO's effective percentage ownership in the firm. A one standard deviation increase in *log(Wealth)* (1.83) increases the pay-performance sensitivity of the CEO by approximately one third of a standard deviation (std. dev. for *Incentives* equals 12.47).

IV.B.1 Alternative Interpretations

Wealth might capture more than just risk aversion. One possibility is that wealth is a measure of CEO power, which could explain the results in column 1. To address this, we add a control for corporate governance and interact it with wealth in column 2. We use the voting rights of the largest owner net of the CEO to proxy for the monitoring incentives of shareholders. More closely monitored CEOs should be less able to influence their own incentive contracts and thus, if wealth really captures power rather than risk aversion, we expect incentive strength to be more strongly related to wealth in firms with weak monitors. Therefore, the coefficient of the interaction terms should have the opposite sign as the coefficient of wealth. This is indeed the case - the interaction term comes out significant and negative, which means that wealth is less related to the provision of incentives when CEOs are more closely monitored. However, the effect is small compared to the effect of wealth, so even though we cannot rule out the possibility that part of the positive relationship between incentives and wealth is due to CEO power, wealth should not be interpreted as a simple measure of CEO power.

To further test whether wealth captures other CEO characteristics, such as skill, columns 3-4 test the link between wealth and pay levels. The dependent variable equals cash compensation in column 3 and total compensation in column 4. If wealth proxies for either skill or power, we hypothesize that wealth and compensation will be positively related. As pointed out by Edmans and Gabaix (2017), if wealth is primarily a measure of risk aversion, the expected effect on compensation is ambiguous. On the one hand, higher risk aversion increases the required fixed pay as a compensating differential but on the other hand, it reduces the optimal level

of incentives, which lowers the risk premium. For example, a risk-averse CEO that optimally receives strong incentives in firm A would need to be compensated for carrying firm risk, but the same CEO could optimally be given weaker incentives in firm B and receive low compensation. The expected effect of private wealth on compensation if wealth is primarily a measure of risk aversion is thus ambiguous. In both columns, the link between pay levels and wealth is insignificant, which suggests that wealth is not a strong proxy for either skill or power.

The link between wealth and incentive strength could also suffer from selection bias related to CEO-firm matching. Boards might hire managers whose personal characteristics match with those of the firm. The rationale for such matching could be more efficient risk allocation, so that CEOs who are less risk-averse match optimally with firms for which a high level of risk-taking is optimal. Such firms may in turn optimally provide their CEOs with strong incentives. In equilibrium, CEOs with certain personal characteristics match with firms that have a demand for those characteristics. If assignment is based on wealth, we expect CEOs who work for the same firm to have similar levels of wealth. To test this, we study firms that change their CEOs during our sample period to see if the wealth of the new CEO matches with that of the previous CEO. We also study whether changes in wealth around turnover have any effect on incentives.

The results are reported in panel B of Table IV. In column 1, we regress the level of private wealth in the first period under the new CEO on the level of private wealth in the last period of the incumbent CEO. The coefficient of incumbent wealth is highly insignificant, which is inconsistent with the idea that firms and CEOs match based on the wealth. In column 2, we regress the changes in incentive strength on changes in wealth and other explanatory variables around turnover. The results suggest that changes in wealth around turnover lead to changes in incentives. If the succeeding CEO is wealthier than the incumbent CEO, so that the change in wealth is positive, the associated change in incentives is also positive, which is consistent with previous results.

IV.C Incentives and Private Allocation Decisions

Next, we examine whether CEOs use their private wealth to adjust the exposure to risks embedded in their incentive contracts. Hence, we decompose total wealth into our three asset class ratios (*Cash*, *Real Estate* and *Equity*) and test whether the strength of the incentives provided by the firm affect the private allocation decisions of the CEO. Specifically, we estimate the following model

$$Asset.Ratio_{it} = \alpha + \beta_1 Incentives_{it} + \beta_2 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it} \quad (2)$$

In all regressions, we control for the level of wealth as a measure of risk aversion plus other characteristics of the CEO and the firm. We hypothesize that CEOs who receive stronger incentives will invest a larger fraction of their external wealth in safe assets, given their levels of risk aversion.

The results are reported in panel A of Table V. Just as in Table III, wealthier CEOs allocate more of their private wealth toward risky assets. In addition, the coefficient of incentive strength move in the opposite direction, which is consistent with our hypothesis. Given the CEO's level of risk aversion, the stronger the incentives, the safer the asset allocations. The coefficient for cash equals 0.14, which means that a one percentage point increase in incentive strength corresponds to a 0.14 percentage point increase in the amount of wealth held in cash. Likewise, the coefficient for equity equals -0.29, which corresponds to an analogous decrease in the amount of wealth held in equity by 0.29 percentage points. Both of these effects are significant at a 1% level. The effect on real estate is insignificant, which means that CEOs are mainly choosing between how much to hold in cash and in equity. In terms of economic significance, the effects are smaller compared to those estimated for wealth in Table III. For example, a one standard deviation increase in the incentive strength corresponds to a one-tenth standard deviation increase in the fraction held in cash and a one-seventh standard deviation decrease in the fraction held in equity.

IV.C.1 Do Private Allocation Decisions Constitute an Agency Problem?

An interesting question is whether shareholders are aware of (or approve of) the link between incentive strength and private allocations made by the CEO. Since the data on asset allocations used in this paper is not available to the general public, the only way in which shareholders can directly verify how CEOs allocate their wealth is if they willingly disclose that information, which seems unlikely. Even so, shareholders may suspect that CEOs use their private wealth to balance the their total exposure to risk. If shareholders are opposed to the idea of CEOs trading privately in response to receiving equity-based compensation, it could prompt boards and larger shareholders to try to actively discourage CEOs from doing so. In order to test this, we estimate the following model

$$\begin{aligned} Asset.Ratio_{it} = \alpha + \beta_1 Incentives_{it} \times Largest.Owner_{it} + \\ + \beta_2 Largest.Owner_{it} + \beta_3 log(Wealth_{it}) + \beta_2 X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it} \end{aligned} \tag{3}$$

, where *Largest Owner* equals the voting right of the largest shareholder net of the CEO. If shareholders oppose the idea of CEOs allocating wealth strategically, we hypothesize that the pattern documented panel A of Table V will be more pronounced in firms without strong monitors and therefore that the coefficient of the interaction term will have the opposite sign as the coefficient of *Incentives*.

The results are reported in panel B. The presence of a large owner appears to have a small mitigating effect on the baseline allocation effects in panel A, which is consistent with an agency interpretation. However, all coefficients are small, which could potentially be explained by the fact that private allocation decisions by the CEO are not easily observable.

IV.C.2 When Do Incentives Matter More for Allocation Decisions?

Next, we examine if the relationship estimated in Table V is heterogeneous across the wealth distribution. If the reported results actually reflect some form of private hedging, one might expect the degree of hedging to vary with the level of wealth.

In panel A of Table VIII, we present point estimates similar to those in Table V, where we in

subsequent order "slice off" deciles (by wealth) of the data. Moving downward in the table thus corresponds to estimating (2) using sub-samples of the data consisting of less and less wealthy CEOs. The results reveal an interesting pattern. As we condition the results on having lower wealth overall, the effect of increasing wealth on the decision to allocate funds away from cash becomes stronger, that is, among the less wealthy CEOs in our sample, increasing private wealth causes a larger negative effect on the decision to hold cash than it does when we include more wealthy CEOs. The opposite is not true for the decision to hold equity. Instead, less wealthy CEOs appear to substitute housing for cash rather than equity. As overall wealth increases, the effect on real estates switches sign to negative and the effect on equity increases. This is consistent with the idea that less wealthy CEOs are subject to wealth constraints.

The pattern is similar when we look at incentive strength. As we exclude wealthier CEOs, the effect of incentives on the fraction of wealth held in cash diminishes and the effect of the fraction of wealth held in housing increases, which is again consistent with the idea that less wealthy CEOs are more constrained in allocating wealth between cash and equity. The effect on equity also becomes stronger as we move down the table, which suggests that private hedging is present across all levels of wealth.

In panel B of Table VIII we run a similar test by reestimating (2) and instead interacting *Wealth* and *Incentives*. *Wealth* displays similar effects on the allocation ratios as in Table V. The effects of *Incentives* on the other hand switches sign in column 1 and 2 and the interaction terms are all significant and move in the opposite direction as the main effects. In order to better see how wealth and incentives interact, in the bottom part of the panel, we contrast the contributions of each decile of incentive strength on the fractions of wealth held in different asset classes for the distributional ends of wealth, i.e. we evaluate the incentive effects for high and low values of wealth. *High Wealth* corresponds to the 90^{th} percentile cutoff whereas *Low Wealth* corresponds to the 10^{th} percentile. Evaluated at these levels, we see that the marginal effect of incentive strength differs for high and low wealth CEOs. High wealth CEOs divest their equity portfolios less than low wealth CEOs, which is consistent with wealth proxying for risk aversion. The contribution is negative irrespective of wealth levels, which means that CEO always move away

from equity when incentives are imposed on them. High wealth CEOs also allocate more of their wealth toward cash as incentive strength increases, whereas low wealth CEOs allocate more wealth toward housing, which is the same pattern as we saw in panel A.

IV.D Hedging of Idiosyncratic or Systematic Risk?

So far, we have shown that risky allocation decisions are negatively related to incentive strength. CEOs who receive stronger (weaker) incentives invest more (less) of their private wealth in cash and less (more) in equity, thereby reducing their overall risk. Next, we want to examine whether this relationship reflects hedging of idiosyncratic or systematic risk. One the one hand, CEOs can undo their exposure to systematic risk by selling the market portfolio and hold the proceeds in cash. On the other hand, CEOs can undo their exposures to idiosyncratic risk by selling off some highly correlated stock.

One way to approach this is to test whether the allocations are affected by the idiosyncratic and systematic risk components of the the firm's stock return volatility. If the link between risky allocation decisions and incentive strength is mainly driven by diversification, we expect risky allocations to be negatively related to the firm's systematic risk component, when controlling for total risk, incentive strength, wealth and other co-founders. Conversely, if allocation decisions mainly reflect CEOs' tendency to sell off stocks that are highly correlated with the firm, we expect the relationship between idiosyncratic risk and risky allocation decisions to be negative.

To determine whether the asset allocations are firm-specific or systematic risk, in Table VII we estimate the following specification

$$\begin{aligned} Asset.Ratio_{it} = \alpha + \beta_1 Risk.Component_{it} + \beta_2 Tot.Risk_{it} + \\ + \beta_3 Incentives_{it} + \beta_4 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it} \end{aligned} \tag{4}$$

, where *Risk.Component* equals either the idiosyncratic or the systematic component of the firm's stock return volatility. Total risk is decomposed into its systematic and firm-specific components using a market model regression. *Idi.Risk* equals the mean-squared error and *Sys.Risk* equals the beta-squared multiplied by the variance of market return. When fitting (4), we follow

the approach of Aggarwal and Samwick (1999) and normalize our raw risk measures into their empirical cumulative distribution functions using the rank transformation, so that the estimated coefficients represents the effect on asset allocations of moving from the least to the most risky firm in our sample. The results show that systematic risk is negatively related to risky allocations and positively related to safe allocations, which is consistent with the idea that allocation decisions are mainly driven by the desire to adjust the exposure to systematic risk. In contrast, the coefficients of idiosyncratic risk are insignificant in both regressions.

IV.E Allocating Funds Between Asset Classes vs. Changing the Composition of the Investment Portfolio

The previous section showed that risky asset allocations are negatively related to the systematic risk component of the firm's return volatility after controlling for both total risk, incentive strength, private wealth and other key inputs. This is to say that, given the level of incentives and overall firm risk, the fraction of wealth allocated toward safe assets will be bigger when the firm's stock price carries more systematic risk, which suggests that CEOs are predominantly adjusting their exposure to systematic risk. One way this could come about is if CEOs hold diversified portfolios *ex ante* and then adjust the weights for cash and equity in response to incentives.

Below, we examine the composition of the private investment portfolio using instrument level data to determine whether CEOs actually hold diversified portfolios. For each CEO-year in our sample, we identify end-of-year holdings of exchange-traded stocks and funds by their ISIN numbers. We then collect price data from several different sources and calculate various measures of correlation between the private investment portfolio and the firm's stock as well as the market using daily pricing data over the preceding 12 months.[5] For each firm-year, we calculate the following measures; 1) *Portfolio-Firm Beta*, which equals the slope coefficient from regressing portfolio return on firm returns, 2) *Portfolio-Market Beta*, which equals the slope coefficient from regressing portfolio return on the market returns, and 3) *% Portfolio Systematic Variance*,

[5]The price data comes from FINBAS, The Swedish Fund Association and Morningstar Direct.

which equals the goodness-to-fit (R^2) from regressing portfolio return on market returns.

Panel A of Table VIII reports descriptives for the portfolios included in the sample. The median CEO has a portfolio consisting of 4 securities, but the number of securities vary greatly in the sample. Portfolios seem to be only weekly correlated with the firm. The median (average) slope coefficient from regressing portfolio returns on firm returns equals 0.13 (0.17). One the other hand, the slope coefficients from regressing portfolio returns on market returns are hight (β equals 0.82 on average), which suggests that CEOs hold portfolios that are slightly less volatile than the market. Also, the average R^2 is high at 76.28, which means that the majority of the variation in portfolio returns can be attributed to the linear relationship with the market (β).

Having constructed our correlation measures, we then regress them on the same set of explanatory variables as in the previous tables. The idea is as follows; if the asset allocations observed in Table V and Table VII involves changing the composition of the private portfolio, we expect the correlations between the private portfolio and the insider stock and the market portfolio to change as well. For example, if CEOs do not hold well-diversified portfolios *ex ante* and sell off expertise stocks while keeping only the part of the original portfolio that tracks the market, one would expect the remaining portfolio to correlate more with the market.

The results are presented in panel B of Table VIII. In column 1, we test whether the correlation between the portfolio and the firm is affected by incentive strength. We do not find support for this. The coefficient of both *Incentives* and *Wealth* are insignificant and very close to zero, which implies that incentive strength has little bearing on decision to weight the portfolio toward instrument that correlate with the firm.

In the remaining columns we add our risk measures from the previous tables to the specification. In columns 2-3, the dependent variable equals the market beta of the portfolio and in columns 4-5, it equals the percentage of total variation explained by the fitted values. The reason why we drop the portfolio-firm slope coefficient and instead focus on β and R^2 is that the first measure is prone to yield significant results even if CEOs do not change the composition of their portfolios. For example, finding a positive link between the level of systematic risk and the correlation

between the portfolio and the firm could come from changes in the amount of systematic risk across or within firms. Looking at the estimates in columns 2 and 4, we find no evidence that portfolio composition relates to either incentive strength or firm risk. The effects of both risk and incentives are highly insignificant and close to zero. Interestingly, *Wealth* has a significant negative effect on the amount of market risk in the portfolio, which is consistent with our initial results in Table III that wealthier CEOs make riskier allocation decisions.

One possible explanation why the composition of the private portfolio is not affected by either incentives or risk in the pooled regressions could be because of endogenous matching between CEOs and firms. For example, a CEO of a firm with a lot of idiosyncratic risk may be overconfident relative to other CEOs, which could cause him to put more weight on expertise stocks when not given incentives by the firm. When incentives are later imposed on him, he may sell off those stocks, possibly leaving him with more diversified portfolio than he started out with. In contrast, a CEO of a firm with less idiosyncratic risk may lack such overconfidence to begin with and therefore hold a diversified portfolio *ex ante*, whose composition remains unchanged when incentives are imposed on him. This could result in a weak cross-sectional link between risk and portfolio composition. In order to address this, in columns 3 and 5, we reestimate the model using CEO fixed effects, thereby exploiting only within variation in incentives and risk. When we do this, the results are unchanged. Overall, it appears to be no link between the composition of the private portfolio and the provision of incentives and/or the amount of systematic risk embedded in the firm's stock price.

IV.E.1 Insider Portfolio Returns

One possible explanation for the lack of hedging of firm-specific risk found in Table VIII could be that CEOs, in addition to making private allocation decisions, also sell off part of their accumulated holdings in the firm when granted new incentives. Alternatively, the reason why CEOs do not hedge may be because equity-based compensation reflects power rather than governance, which we could not fully discard in Table IV.

In table Table IX, we test these hypotheses by examining how the value of new grants and the return of the CEO's insider holdings relates to the return of the firm's stock. In column 1, we regress the value of new grants over the year on the firm's adjusted stock return over the same period. If the provision of incentives is indeed the result of powerful manager granting themselves equity in the firm, we conjecture that new grants will be positively correlated with the share price over the year.[6] We control for both incentive strength and private wealth in the beginning of the period, since both of these variables are likely to impact the CEO's risk aversion and incentive to hedge. We also control for similar CEO and firm characteristics as in previous tables. The results show that price movements over the year do not explain the provision of incentives, which is consistent with the idea of equity-based compensation being a measure of governance. Also, the coefficient of wealth is positive and significant, which is consistent the result in Table IV that less risk-averse CEOs receive stronger incentives. Lastly, the coefficient of incentive strength is insignificant and slightly negative, which would be surprising if accumulated holdings is the result of CEOs granting themselves more and more equity.

Next, we test how the return of the insider portfolio relates to the return of the firm's stock, keeping in mind that any changes in the value of the insider portfolio over the year could be the result of both accrued capital gains, dividend payouts, increases in holdings due to new grants and decreases in holdings due to decisions to sell off part of the insider portfolio. We calculate the return of the insider portfolio as the sum of the end-of-year value (share price times the number of shares) of aggregate holdings in period t and dividend payouts over the year divided by the sum of the end-of-year value in period t-1, minus one. Dividend payouts are approximated as the number of shares at the beginning of the period times the dividend per share over the year.[7]

In column 2, we regress the return of the insider portfolio on the firm's adjusted stock return

[6]This presupposes that CEOs possess some sort of private information regarding the future prospects of the firm, which they can use to time their own grants of equity in the firm.

[7]This approach does not fully account the actual dividend payouts during the year. In order to calculate returns correctly, one would need to have data on the timing of both new grants and dividend payouts, which we do not have at this point. We address this by multiplying the beginning-of-year holdings with the yearly dividends per share and then add this value to the end-of-year value of the insider portfolio. This yields correct return measures if 1) there are no new grants during the year or 2) new grants are made after the dividend payout.

over the year, again controlling for both lagged incentives strength and lagged private wealth.[8] Since the value of the insider portfolio is in part mechanically related to the share price, we conjecture this relationship to be positive. As expected, the effect of the firm's stock return is both positive and highly significant, which suggests that CEOs are not opportunistically selling off large parts of their accumulated holdings at the end of the year following a positive price return over the year. We also include equity grants and the interaction between equity grants and incentive strength in the beginning of the period in the model. If CEOs actually adjust risk by selling off part of their accumulated holdings, we expect this behavior to be more pronounced among CEOs who receive new equity grants while already carrying strong incentives. The result confirms this hypothesis - whenever incentives are strong in the beginning of the period and the CEO receives additional grants of equity-based incentives during the course of the period, conditional on firm performance, the return of the insider portfolio drops.

V Conclusions

In this paper, we have examined 1) how private wealth relates to CEOs' risk aversion, 2) whether higher risk aversion leads to the provision of stronger incentives by the firm and 3) whether CEOs use their private wealth to partially hedge the risks embedded in their incentive contracts. We document several key results that ought to have direct implications for the design of the optimal contract. First, we show that wealthier CEOs invest a larger fraction of their private wealth in risky assets, which is consistent with the idea that wealth proxies for negative risk aversion. Second, we show that less risk-averse CEOs receive more high-powered incentives in equilibrium. This result is consistent with standard agency models. Third, we show that risky asset allocations are negatively related to the provision of incentives, given the level of private wealth, which suggests that CEOs use their private wealth to adjust their exposure to risk. Incentive strength and private wealth have offsetting effects on the decision to allocate private wealth toward both

[8] Note that the relationship is not trivial, since CEOs could strategically be buying and selling shares in their own firms. For example, following an increase in the adjusted share price over the course of the year, CEOs may strategically sell off shares towards the end of the period, which could results in a negative relationship between the return of the insider portfolio and the underlying share price.

cash and equity. Fourth, we show that risky asset allocations are negatively related to the systematic risk component and insignificantly related to the idiosyncratic risk component of the firm's stock returns after controlling for both total risk, incentive strength, private wealth and other key inputs. This suggests that CEOs are mainly adjusting their exposure to market risk, which can be interpreted as a form of self-indexing. Consistent with this result, the CEOs in our sample appear to hold well-diversified portfolios. Using instrument level data, we show that the private investment portfolio is highly correlated with the market and that the composition of the portfolio is not sensitive to either incentive strength nor firm risk. Lastly, we find some evidence that CEOs adjust their exposure to firm risk by trading in their own firm's stock. Specifically, after controlling for price movements during the year, CEOs who have accumulated strong incentives in the beginning of the period and subsequently receives additional grants of equity over the course of the year, display lower insider portfolio returns. Overall, our results are consistent with traditional principal–agent theory of risk aversion as well as with risk-averse CEOs responding optimally to the provision of incentives. CEOs seem to mainly be adjusting their exposure to market risk. Thus, we do not find evidence that CEOs are using their private wealth to undo the incentive effect of their contracts.

Appendix A

A1 Valuing Stocks and Options

We gather data on stocks and options held by managers from *The Financial Supervisory Authority of Sweden* (FI) as well as from annual reports. FI's database tracks all insider transactions in Swedish listed firms from 1991 onwards and contains information on stocks and options held at the end of the year, including the date of the transaction (grant or purchase date), the number of units acquired, aggregate holdings of each instrument before and after the transaction, the exercise price and the expiration date. A major benefit with our data is that CEOs need to disclose their aggregate holdings (not just new grants) as of the date when they first take office, as well as any subsequent trades, which means that we can easily calculate the appropriate PPS measure proposed in the literature. Compared to for example (Hall and Liebmann, 1998), our PPS measure is therefore not restricted to include only grants and purchases made during the tenure period. Another benefit is that CEOs' are required by law to also disclose holdings of related parties. Our definition of CEO holdings therefore extend to include relatives to the CEO as well as companies in which the CEO personally owns more than 50% of the shares.

Stocks are valued using the closing price of the last trading day in a given year. In case a firm has multiple share classes and only one is listed, we apply the share price of the listed share class (the main traded share) to all holdings. The sensitivity of call options to changes in the underlying share price are calculated using the Black-Scholes formula for valuing European call options, as modified by (Merton, 1973) to account for dividend payouts. We use a 180 trading days treasury bond yield to proxy for the expected risk-free rate. The expected dividend rate is approximated by the average dividend rate in the previous two fiscal years. Whenever the exercise price is missing in our data, we estimate it using the average exercise-to-price ratio of the option contracts where the strike price is known. The exercise price is missing for about 17% of all option. The average exercise-to-price ratio on the day of grant is approximately 1.4, that is, on average, options are granted out of the money.

REFERENCES

Aggarwal, Rajesh K. and Andrew A. Samwick. 1999. "The Other Side of the Trade-Off: The Impact of Risk on Executive Compensation." *Journal of Political Economy* 107(1):65–105.

Becker, Bo. 2006. "Wealth and Executive Compensation." *The Journal of Finance* 61(1):379–397.

Bettis, J. Carr, John M. Bizjak and Michael L. Lemmon. 2001. "Managerial Ownership, Incentive Contracting and the Use of Zero-Cost Collars and Equity Swaps by Corporate Insiders." *Journal of Financial and Quantitative Analysis* 36(3):345–370.

Edmans, Alex and Xavier Gabaix. 2017. "Executive Compensation: A Survey of Theory and Evidence." *Journal of Economic Literature (Forthcoming)* .

Garvey, Gerald and Todd Milbourn. 2003. "Incentive Compensation When Executives Can Hedge the Market: Evidence of Relative Performance Evaluation in the Cross Section." *The Journal of Finance* 58(4):1557–1582.

Hall, Brian J. and Jeffrey B. Liebmann. 1998. "Are CEOs Really Paid Like Bureaucrats?" *The Quarterly Journal of Economics* 113(3):654–691.

Holmström, Bengt. 1979. "Moral Hazard and Observability." *Bell Journal of Economics* 10:74–91.

Holmström, Bengt and Paul R. Milgrom. 1987. "Aggregation and Linearity in the Provision of Intertemporal Incentives." *Econometrica* 55:303–328.

Jensen, Michael C. and Kevin J. Murphy. 1990. "Performance Pay and Top-Management Incentives." *Journal of Political Economy* 98(2):225–264.

Jensen, Michael C. and William H. Meckling. 1976. "Theory of the Firm: Managerial Bahaviour, Agency Costs and Ownership Structure." *Journal of Financial Economics* 3:305–360.

Jenter, Dirk C. 2002. "Executive Compensation, Incentives and Risk." Working Paper.

Jin, Li. 2002. "CEO Compensation, Diversification and Incentives." *Journal of Financial Economics* 66:29–63.

Merton, Robert C. 1973. "Theory of Rational Option Pricing." *Bell Journal of Economics and Management Science* 4:141–183.

Murphy, Kevin J. 1999. *Handbook of Labor Economics: Executive Compensation.* Vol. 3 New York and Oxford: Elsevier/North- Holland.

Figure I

Distribution of Wealth

This figure shows the distribution of *Total Wealth* for the average CEO in our sample. *Firm Wealth* equals the market value of company stocks and options held by the CEO at years end. *Private Wealth* is categorized into four asset classes; *Cash*, *Fixed Income*, *Real Estates* and *Equity*. *Cash* equals the amount held in bank accounts. *Equity* is the market value of non-company stocks and options held directly by the CEO plus equity held in funds and endowment insurances. *Real Estates* is equal to the market value of private housing and commercial real estates. Lastly, *Fixed Income* equals the market value of corporate bonds, bond funds and governmental bonds (including lottery bonds). Asset class ratios are constructed by dividing the value held in a certain asset class by *Total Wealth*. All values are measured at years end and are denominated in million SEK.

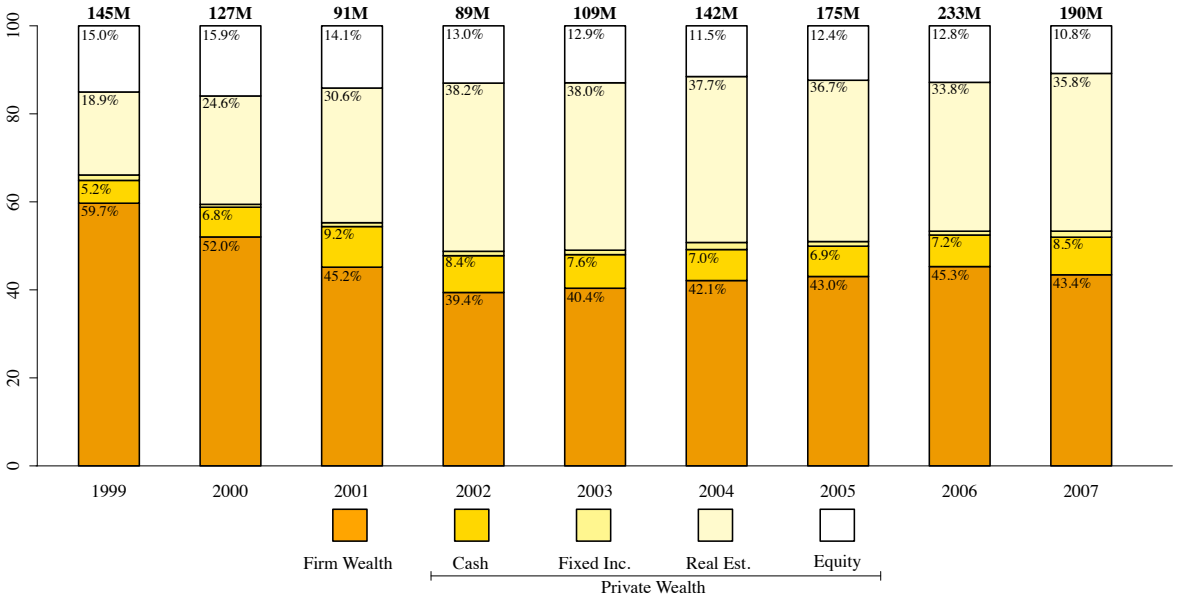

Table I
Variable Definitions

This table defines the variables used in the analysis and states their sources. The sample comprises 478 CEOs of 228 publicly listed Swedish firms observed over the period 1999-2007. The data merges information from 7 different sources. Personal identification numbers for the CEOs in our sample were provided by *The Swedish Companies Registration Office*. Data on private wealth were provided by *Statistics Sweden*. Compensation data was gathered from annual reports and from *The Financial Supervisory Authority of Sweden*. Ownership data was provided by *SIS Ownership Service*. Accounting and stock price data was provided by *Bisnode* and *FINBAS*.

Name	Definition
C: CEO Income	
Cash Compensation	Equals the sum of the annual salary, bonus (earned during the year), other cash-based compensation (perks) and pension contributions.[1]
Total Compensation	Equals the sum of total cash compensation and annual grants of stocks and options. The value of stock grants is calculated as the number of stocks times the share prices at the grant date. Options are valued according to the same principle, using the Black-Scholes formula. If shares and options were bought at a discount, the purchase value is subtracted.[1,2]
B: CEO Wealth and Incentive Strength	
Firm Wealth	Equals the amount of wealth held in the own firm and is calculated as the end-of-year value of stocks and options.[1,2]
Incentives (PPS)	Equals the sum of the number of shares owned and the number of options owned times their delta, divided by the total number of shares outstanding, times 100.[1,2]
Private Wealth	Equals the amount of wealth held outside the own firm.[3]
Allocation Ratios	Equals the end-of-year market value of assets held in a certain asset class (cash, fixed income, real estate or equity) divided by private wealth.[3]
C: CEO and Board Characteristics	
CEO Age	The age of the CEO in years. Equals the current year minus the year of birth.[4]
CEO Director	Dummy equal to one if the CEO sits on the board.[4]
CEO Gender	Dummy equal to one if the CEO is a woman.[4]
CEO Tenure	The tenure of the CEO in years. Equals the current year minus the year of appointment.[4]
Board Age	The average age of directors in years.[4]
Board Busy	The average number of outside directorships held by the directors.[2]
Board Dependence	The percentage of dependent directors. A director is considered dependent if she either belongs to the top management team, is a relative to someone who belongs to the top management team or if she owns more than 10% of the firm's shares.[2]
Board Employee Representatives	The percentage of directors elected by the employees.[4]
Board Gender	The percentage of female directors.[4]
Board Size	The total number of directors.[4]
D: Firm Characteristics	
Book-to-Market	The ratio of the book value and the market value of equity.[1,5]
Cash	The ratio of cash and total assets.[1,5]
Industry	GICS 10 industry classification.[6]
Largest Owner	The ownership stake of the largest owner net of the CEO.[7]
Leverage	The ratio of total liabilities and total assets.[1,5]
ROA	The ratio of EBITDA and total assets.[1,5]
Size	The natural logarithm of total assets.[1,5]
Turnover	Dummy equal to one in the year of CEO turnover.[4]

[1]Annual Reports, [2]The Financial Supervisory Authority of Sweden, [3]Statistics Sweden, [4]The Swedish Companies Registration Office, [5]BisNode, [6]FINBAS (Swedish House of Finance), [7]SIS Ownership Service

Table II
Summary Statistics

This table shows summary statistics for the CEOs and firms in our sample. The sample comprises 475 CEOs of 227 publicly listed Swedish firms observed over the period 1999-2007. Panel A reports the compensation variables, panel B the aggregate holding of stocks and options, panel C the pay-performance sensitivities used to measure incentive strength and panel D our wealth measure, broken down into cash, fixed income, real estate and equity. Share of Company equals the sum of the number of shares owned and the number of options owned times their delta, divided by the total number of shares outstanding, stated in percentages. Money at Stake equals the sum of the number of shares owned times the and the number of options owned times the delta times the share price, stated in millions.

A: Variable Overview

	N	Min.	25^{th}	Med.	Mean	75^{th}	Max.
I: CEO Income							
Cash Comp. (MSEK)	1711	0.00	1.70	2.74	4.51	5.31	57.10
Total Comp. (MSEK)	1711	0.00	1.76	2.84	4.74	5.48	72.19
II: CEO Wealth and Incentive Strength							
Firm Wealth (MSEK)	1732	0.00	0.87	5.00	123.77	26.16	18 261.97
Options (MSEK)	1732	0.00	0.00	0.00	1.64	0.53	124.14
Stocks (MSEK)	1732	0.00	0.43	3.30	122.13	21.89	18 261.97
Incentives (PPS) (%)	1732	0.00	0.06	0.40	5.28	2.44	78.60
Private Wealth (MSEK)	1732	0.00	3.04	5.96	22.73	12.95	2353.60
Cash (MSEK)	1732	0.00	0.08	0.32	2.61	1.08	445.71
Fixed Inc. (MSEK)	1732	0.00	0.00	0.00	0.23	0.00	19.05
Real Est. (MSEK)	1732	0.00	1.29	2.77	4.62	5.21	98.56
Equity (MSEK)	1732	0.00	0.05	0.53	10.63	2.75	1886.79
Cash Ratio (%)	1732	0.00	1.22	5.51	13.67	18.14	100.00
Fixed Inc. Ratio (%)	1732	0.00	0.00	0.00	1.87	0.00	98.80
Real Est. Ratio (%)	1732	0.00	21.67	55.03	52.49	84.23	100.00
Equity Ratio (%)	1732	0.00	1.09	11.83	22.03	35.50	100.00
III: CEO and Board Characteristics							
CEO Age (Y)	1732	27.00	44.00	49.00	49.24	55.00	67.00
CEO Tenure (Y)	1732	1.00	2.00	4.00	4.83	7.00	17.00
CEO Gender (×100)	1732	0.00	0.00	0.00	1.27	0.00	100.00
CEO Director (×100)	1732	0.00	0.00	100.00	67.55	100.00	100.00
Board Size (N)	1732	2.00	6.00	7.00	7.35	9.00	13.00
Board Age (Y)	1732	39.40	51.00	53.83	53.61	56.59	64.20
Board Gender (%)	1732	0.00	0.00	13.00	12.20	20.00	60.00
Board Emp. (%)	1732	0.00	0.00	0.00	10.47	22.00	44.00
Board Dependence (%)	1732	0.00	0.00	14.00	15.25	22.00	75.00
Board Busy (N)	1732	1.00	1.29	1.60	1.78	2.09	5.00
IV: Firm Characteristics							
Sales (BSEK)	1732	0.00	0.27	0.99	8.59	32.44	284.50
Volatility (%)	1732	6.07	28.32	37.63	47.05	50.50	313.87
Leverage (%)	1732	0.44	30.75	47.10	45.54	61.10	111.81
Cash (%)	1732	0.00	2.82	6.09	11.51	14.20	85.08
ROA (%)	1732	−597.27	−1.37	6.62	−0.90	11.78	54.57
BTM (%)	1732	−24.20	28.20	48.08	60.30	79.36	714.07

B: Variation in Wealth

	Full Sample			Firm Level			CEO Level		
	Mean	Std.	Std./Mean	Mean	Std.	Std./Mean	Mean	Std.	Std./Mean
Private Wealth (MSEK)	22.73	115.34	5.08	21.00	13.06	0.62	16.59	6.87	0.41
Firm Wealth (MSEK)	123.77	864.32	6.98	121.65	91.59	0.75	91.73	47.98	0.52

Table III
Private Wealth and Risky Asset Allocation

This table shows the "baseline" relationship between private wealth and asset allocations, not taking into account incentive strength or firm characteristics. Panel A shows how asset allocations varies cross-sectionally with the level of private wealth. Panel B, shows the results of the reversed specification, using lagged right-hand side variables and CEO-fixed effects. Asset classes are stated as ratios with *Private Wealth* as the denominator and are ordered according to their riskiness (from left to right). Year-fixed effects are included in all regression. We use robust standard errors clustered at the individual level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Cross-Sectional Relationship - Private Wealth and Allocation Decisions (Tobit)

	Fraction of Wealth held in Cash	Fraction of Wealth held in Real Estates	Fraction of Wealth held in Equity	Equity - Cash
	(1)	(2)	(3)	(4)
log(Private Wealth)	−2.78***	−7.68***	7.06***	9.07***
	(0.00)	(0.00)	(0.00)	(0.00)
CEO Age	0.04	−0.66***	0.40***	0.35***
	(0.61)	(0.00)	(0.00)	(0.01)
CEO Gender	−11.17***	13.99**	−9.31*	2.28
	(0.00)	(0.02)	(0.10)	(0.65)
Year FEs	Yes	Yes	Yes	Yes
Pseudo R^2	0.54%	1.63%	1.50%	1.43%
Observations	[1732]	[1732]	[1732]	[1732]
Censored	146	144	196	20

B: Within Relationship - Allocation Decisions and Private Wealth (OLS)

	Dep. Variable = log(Private Wealth)		
	(1)	(2)	(3)
Fraction in Cash (*lagged*)	0.00		
	(0.42)		
Fraction in Real Estates (*lagged*)		0.00	
		(0.96)	
Fraction in Equity (*lagged*)			0.00
			(0.21)
CEO Age	0.13**	0.13**	0.13**
	(0.01)	(0.01)	(0.02)
CEO FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Adjusted R^2	−8.06%	−8.20%	−7.73%
Observations	[1255]	[1255]	[1255]

Table IV
Private Wealth and Incentive Strength

Panel A of this table reports coefficient estimates (pooled regressions) of the following model

$$Incentives_{it} = \alpha + \beta_1 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it}$$

Column 1 reports the baseline effects. In column 2, we add a proxy for the quality of governance to the model and interact it with *Wealth*. The quality of governance is captured by the ownership stake of the largest shareholder net of the CEO. In columns 3-4, we run similar regressions, using the level of cash and total compensation as our left-hand side variables.In column 1 of panel B, we examine wealth changes around CEO turnover by regressing the wealth of the succeeding CEO (*Wealth.Suc*) on the wealth of the incumbent CEO (*Wealth.Inc*). Firm-years without turnovers are disregarded. Lastly, in column 2 of panel B, we regress changes in incentive strength on changes in wealth plus additional co-founding factors around CEO turnover. As such, the dependent variable represents the difference in incentive strength between the first year of the succeeding CEO the last year of the incumbent CEO and the control variables are constructed using one-year lagged data. *Wealth* is defined as total wealth minus any insider holdings in the firm, as reported by *Statistics Sweden*. Additional control variables include *CEO Age*, *CEO Tenure*, *CEO Gender*, *CEO Director*, *Board Size*, *Board Age*, *Board Gender*, *Board Emp. Rep.*, *Board Dependence*, *Board Busy*, *lol(Sales + 1)*, *Leverage*, *Cash*, *ROA*, *Market-to-Book* and *Tot.Risk*. For variable definitions, see Table I. All regressions include industry and year fixed effects. The panel comprises 232 firms observed between 1999 and 2007. We use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Cross-Sectional Relationship - Private Wealth and Incentives

	Incentive Strength (Pay-Performance Sensitivity)		log(Cash Comp.)	log(Total Comp.)
	(1)	(2)	(3)	(4)
log(Wealth)	2.16***	3.82**	0.00	0.01
	(0.01)	(0.01)	(0.90)	(0.62)
Largest Owner×log(Wealth)	-	−0.07**	-	-
	-	(0.05)	-	-
Largest Owner	-	0.92*	-	-
	-	(0.08)	-	-
Additional Controls	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Adj. R^2	20.44%	27.84%	49.85%	48.08%
Observations	[1729]	[1725]	[1708]	[1708]

B: Wealth and Incentives Around CEO Turnover

	log(Suc.Wealth)	ΔIncentives
	(1)	(2)
log(Inc.Wealth)	0.02	-
	(0.71)	-
Δlog(Wealth)	-	0.16***
	-	(0.00)
Additional ΔControls	Yes	Yes
Industry FEs	Yes	Yes
Year FEs	Yes	Yes
Adj. R^2	14.12%	21.38%
Observations	[163]	[163]

Table V

The Effect of Incentive Strength on the Allocation of Private Wealth

In panel A of this table, we report coefficient estimates of the following baseline specification

$$Asset.Ratio_{it} = \alpha + \beta_1 Incentives_{it} + \beta_2 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it}$$

In panel B, we reestimate the model and control for the quality of governance and the interaction between the quality of governance and *Incentives*. The quality of governance is captured by the ownership stake of the largest shareholder net of the CEO. *Asset.Ratio* equals either *Cash*, *Real Estate* or *Equity* as defined in Table I. Asset classes are stated as percentages using total private wealth as the denominator and are ordered according to their riskiness (from left to right). *Incentives* equals the pay-performance sensitivity of the CEO. *Wealth* equals private wealth, which is defined as total wealth, as reported by Statistics Sweden, minus the value of any insider holdings. Additional controls include *CEO Age*, *CEO Tenure*, *CEO Gender*, *CEO Director*, *Board Size*, *Board Age*, *Board Gender*, *Board Emp. Rep.*, *Board Dependence*, *Board Busy*, *log(Sales + 1)*, *Leverage*, *Cash*, *ROA*, *Market-to-Book* and *Tot.Risk*. All regressions include industry and year fixed effects. The panel comprises 232 firms observed over nine years (1999-2007). We use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Baseline Results - Asset Allocation vs. Incentives and Wealth

	Fraction of Wealth held in Cash	Fraction of Wealth held in Real Estates	Fraction of Wealth held in Equity	Equity - Cash
	(1)	(2)	(3)	(4)
Incentives	0.14***	−0.09	−0.29***	−0.27***
	(0.00)	(0.19)	(0.00)	(0.00)
log(Wealth)	−4.01***	−6.08***	7.13***	9.95***
	(0.00)	(0.00)	(0.00)	(0.00)
Year FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
Pseudo R^2	2.20%	3.17%	2.90%	2.59%
Observations	[1717]	[1717]	[1717]	[1717]
Censored	146	144	195	20

B: Controlling for the Quality of Governance

	Fraction of Wealth held in Cash	Fraction of Wealth held in Real Estates	Fraction of Wealth held in Equity	Equity - Cash
	(1)	(2)	(3)	(4)
Incentives×Largest Owner	−0.01***	0.01***	0.01***	0.01**
	(0.01)	(0.01)	(0.00)	(0.04)
Largest Owner	−0.03	0.08*	0.03	0.06
	(0.28)	(0.07)	(0.37)	(0.15)
Incentives	0.17***	−0.14*	−0.93***	−0.81***
	(0.00)	(0.08)	(0.00)	(0.00)
log(Wealth)	−4.14***	−6.00***	7.11***	9.98***
	(0.00)	(0.00)	(0.00)	(0.00)
Additional Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
Pseudo R^2	2.53%	3.51%	3.16%	2.92%
Observations	[1713]	[1713]	[1713]	[1713]
Censored	145	144	193	20

Table VI
Heterogeneous Effects

In panel A of this table, we report coefficient estimates of the following model for different subsamples of the data

$$Asset.Ratio_{it} = \alpha + \beta_1 Incentives_{it} + \beta_2 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it}$$

Each sub-panel shows the estimated effects of *Incentives* and *Wealth* for the subsample of firm-years where the wealth of the CEO is below the stated cutoff point (decile values down to the 50^{th} percentile). In panel B, we report coefficient estimates of the following model

$$Asset.Ratio_{it} = \alpha + \beta_1 Incentives_{it} \times log(Wealth_{it}) + \beta_2 Incentives_{it} + \beta_3 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it}$$

, where we in addition to the original specification in Table V also include the interaction term between *Incentives* and *Wealth*. *Asset.Ratio* equals either *Cash*, *Real Estate* or *Equity* as defined in Table I. Asset classes are stated as percentages using total private wealth as the denominator and are ordered according to their riskiness (from left to right). *Incentives* equals the pay-performance sensitivity of the CEO. *Wealth* equals private wealth, which is defined as total wealth, as reported by Statistics Sweden, minus the value of any insider holdings. Additional controls include *CEO Age*, *CEO Tenure*, *CEO Gender*, *CEO Director*, *Board Size*, *Board Age*, *Board Gender*, *Board Emp. Rep.*, *Board Dependence*, *Board Busy*, *log(Sales + 1)*, *Leverage*, *Cash*, *ROA*, *Market-to-Book* and *Tot.Risk*. All regressions include industry and year fixed effects. The panel comprises 232 firms observed over nine years (1999-2007). We use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Sub-Sample Analysis

	Fraction of Wealth held in Cash	Fraction of Wealth held in Real Estates	Fraction of Wealth held in Equity	Equity - Cash
	(1)	(2)	(3)	(4)
Wealth <100th Percentile				
Incentives	0.14***	−0.09	−0.29***	−0.27***
	(0.00)	(0.19)	(0.00)	(0.00)
log(Wealth)	−4.01***	−6.08***	7.13***	9.95***
	(0.00)	(0.00)	(0.00)	(0.00)
Wealth <90th Percentile				
Incentives	0.12***	−0.09	−0.36***	−0.30***
	(0.01)	(0.27)	(0.00)	(0.00)
log(Wealth)	−5.19***	−2.14	5.05***	9.21***
	(0.00)	(0.13)	(0.00)	(0.00)
Wealth <80th Percentile				
Incentives	0.09*	−0.05	−0.35***	−0.25***
	(0.08)	(0.59)	(0.00)	(0.00)
log(Wealth)	−6.65***	1.91	4.27***	9.99***
	(0.00)	(0.19)	(0.00)	(0.00)
Wealth <70th Percentile				
Incentives	0.10*	0.02	−0.33***	−0.26***
	(0.08)	(0.88)	(0.00)	(0.00)
log(Wealth)	−7.93***	4.61***	3.55***	10.44***
	(0.00)	(0.00)	(0.00)	(0.00)
Wealth <60th Percentile				
Incentives	0.08	0.14	−0.41***	−0.27***
	(0.20)	(0.21)	(0.00)	(0.00)
log(Wealth)	−9.72***	8.59***	2.21*	10.85***
	(0.00)	(0.00)	(0.08)	(0.00)
Wealth <50th Percentile				
Incentives	0.05	0.30***	−0.44***	−0.27***
	(0.56)	(0.01)	(0.00)	(0.01)
log(Wealth)	−10.88***	12.45***	1.26	10.67***
	(0.00)	(0.00)	(0.36)	(0.00)

B: Interacting Wealth and Incentives

	Fraction of Wealth held in Cash	Fraction of Wealth held in Real Estates	Fraction of Wealth held in Equity
	(1)	(2)	(3)
log(Wealth + 1)×Incentives	0.06***	−0.07**	0.09***
	(0.00)	(0.03)	(0.00)
Incentives	−0.85***	1.08**	−1.87***

Table VI Continued from Previous Page

	(0.01)	(0.04)	(0.00)
log(Wealth + 1)	−4.58***	−5.41***	6.29***
	(0.00)	(0.00)	(0.00)
Additional Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes
Pseudo R^2	2.26%	3.20%	2.97%
Observations	[1717]	[1717]	[1717]
Censored	146	144	195

	Fraction of Wealth held in Cash			Fraction of Wealth held in Real Estates			Fraction of Wealth held in Equity		
	High W	Low W	Diff.	High W	Low W	Diff.	High W	Low W	Diff.
Marginal Effects	0.18	0.01	0.17	−0.12	0.08	−0.20	−0.32	−0.58	0.26
10^{th} Percentile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	−0.01	0.01
20^{th} Percentile	0.01	0.00	0.01	0.00	0.00	0.00	−0.01	−0.02	0.01
30^{th} Percentile	0.02	0.00	0.02	−0.01	0.01	−0.02	−0.03	−0.06	0.03
40^{th} Percentile	0.04	0.00	0.04	−0.03	0.02	−0.05	−0.07	−0.13	0.06
50^{th} Percentile	0.07	0.00	0.07	−0.05	0.03	−0.08	−0.13	−0.23	0.10
60^{th} Percentile	0.14	0.01	0.13	−0.10	0.06	−0.16	−0.25	−0.45	0.20
70^{th} Percentile	0.28	0.01	0.27	−0.19	0.12	−0.31	−0.49	−0.89	0.40
80^{th} Percentile	0.90	0.05	0.85	−0.61	0.38	−0.99	−1.58	−2.85	1.27
90^{th} Percentile	3.01	0.15	2.86	−2.06	1.28	−3.34	−5.29	−9.58	4.29

Table VII
Firm Risk and Allocation Decisions

This table shows coefficient estimates of the following model

$$Asset.Ratio_{it} = \alpha + \beta_1 Risk.Component_{it} + \beta_2 Tot.Risk_{it} + \beta_3 Incentives_{it} + \beta_4 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it}$$

, where *Asset.Ratio* equals either *Cash* or *Equity* as defined in Table I. Asset classes are stated as percentages using total private wealth as the denominator and are ordered according to their riskiness (from left to right). *Risk.Component* equals the idiosyncratic or the systematic risk component. Total risk is decomposed into its systematic and firm-specific risk components using a market model regression. *Idi.Risk* equals the mean-squared error and *Sys.Risk* equals the beta-squared multiplied by the variance of market return. Both of these variables are then transformed into their empirical cumulative distribution functions. All risk measures are derived using daily returns over the 12 months immediately before the end of the current calendar year. *Incentives* equals the pay-performance sensitivity of the CEO. *Wealth* equals private wealth, which is defined as total wealth, as reported by Statistics Sweden, minus the value of any insider holdings. Additional controls include *CEO Age*, *CEO Tenure*, *CEO Gender*, *CEO Director*, *Board Size*, *Board Age*, *Board Gender*, *Board Emp. Rep.*, *Board Dependence*, *Board Busy*, *log(Sales + 1)*, *Leverage*, *Cash*, *ROA* and *Market-to-Book*. All regressions include industry and year fixed effects. In addition, columns 3 and 5 include CEO fixed effects. The panel comprises 232 firms observed over nine years (1999-2007). We use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	Fraction of Wealth held in Cash		Fraction of Wealth held in Equity		Equity - Cash	
	(1)	(2)	(3)	(4)	(5)	(6)
rank(Idi. Risk)	−15.30	-	18.09	-	30.15	-
	(0.12)	-	(0.17)	-	(0.11)	-
rank(Sys. Risk)	-	6.18***	-	−7.82**	-	−13.09***
	-	(0.01)	-	(0.02)	-	(0.00)
rank(Total Risk)	12.03	−5.69*	−17.88	3.38	−28.04	7.43
	(0.21)	(0.10)	(0.16)	(0.37)	(0.14)	(0.10)
Incentives	0.15***	0.15***	−0.29***	−0.30***	−0.29***	−0.30***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
log(Wealth + 1)	−4.02***	−4.03***	7.09***	7.01***	9.79***	9.81***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	2.19%	2.22%	3.02%	3.05%	2.68%	2.72%
Observations	[1717]	[1717]	[1717]	[1717]	[1717]	[1717]
Censored	146	146	207	207	20	20

Table VIII
Portfolio Compositions, Incentive Strength and the Firm Risk

This table reports descriptive statistics for the private portfolios held by the CEOs in our sample (panel A) as well as coefficient estimates from the following model (panel B)

$$Portfolio.Char_{it} = \alpha + \beta_1 Idi.Risk_{it} + \beta_2 Sys.Risk_{it} + \beta_3 Incentives_{it} + \beta_4 log(Wealth_{it}) + \gamma X_{it} + \theta_t + \lambda_{Ind} + \varepsilon_{it}$$

, where *Portfolio.Char* equals either the *Portfolio-Firm Beta*, defined as the slope coefficient from regressing portfolio return on the returns of the firm, *Portfolio-Market Beta*, defined as the slope coefficient from regressing portfolio return on the returns of the market, or *% Portfolio Systematic Variance*, defined as the goodness-to-fit (R^2) from regressing portfolio return on the returns of the market. Total risk is decomposed into its systematic and firm-specific risk components using a market model regression. *Idi.Risk* equals the mean-squared error and *Sys.Risk* equals the beta-squared multiplied by the variance of market return. Both of these variables are then transformed into their empirical cumulative distribution functions. All risk measures are derived using daily returns over the 12 months immediately before the end of the current calendar year. *Incentives* equals the pay-performance sensitivity of the CEO. *Wealth* equals private wealth, which is defined as total wealth, as reported by Statistics Sweden, minus the value of any insider holdings. Additional controls include *CEO Age*, *CEO Tenure*, *CEO Gender*, *CEO Director*, *Board Size*, *Board Age*, *Board Gender*, *Board Emp. Rep.*, *Board Dependence*, *Board Busy*, *log(Sales + 1)*, *Leverage*, *Cash*, *ROA* and *Market-to-Book*. All regressions include industry and year fixed effects. The panel comprises 232 firms observed over nine years (1999-2007). We use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Portfolio Characteristics

	N	Min.	25^{th}	Med.	Mean	75^{th}	Max.
Number of Securities in Portfolio	1732	0.00	1.00	4.00	7.26	10.00	110.00
Portfolio-Firm Beta	1447	−0.44	0.06	0.13	0.17	0.24	1.15
Portfolio-Market Beta	1447	−0.66	0.53	0.79	0.82	1.04	2.54
% Portfolio Systematic Variance (=R^2×100)	1447	14.50	42.87	69.45	76.28	90.54	96.14

B: Regressional Output

	Portfolio-Firm Beta	Portfolio-Market Beta		% Portfolio Systematic Variance (=R^2×100)	
	(1)	(2)	(3)	(4)	(5)
rank(Idi. Risk)	-	0.04	0.04	−0.09	0.02
	-	(0.65)	(0.70)	(0.14)	(0.75)
rank(Sys. Risk)	-	0.00	0.11	0.03	0.05
	-	(0.98)	(0.11)	(0.58)	(0.17)
Incentives	0.00	0.00	0.00	0.00	0.00
	(0.47)	(0.44)	(0.91)	(0.52)	(0.27)
log(Wealth + 1)	0.00	−0.06***	−0.06**	−0.01	−0.02
	(0.53)	(0.00)	(0.03)	(0.71)	(0.33)
Additional Controls	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
CEO FEs	No	No	Yes	No	Yes
Adj. R^2	26.67%	11.41%	11.34%	11.29%	15.61%
Observations	[1434]	[1434]	[1433]	[1434]	[1433]

Table IX
Equity Grants and Insider Portfolio Returns

This table shows the coefficient estimates from regressing equity grants (1) and insider portfolio returns (2) on the return of the firm's stock over the year plus additional co-founding factors. *Equity Grants* equals the grant date value of stocks and options in the firm. Purchases by the CEO are also included (gross value). *Insider Portfolio Returns* are calculated as as the sum of the end-of-year value (share price times the number of shares) of aggregate holdings in period t and dividend payouts over the year divided by the sum of the end-of-year value in period t-1, minus one. Dividend payouts are approximated as the number of shares at the beginning of the period times the dividend per share over the year. The panel comprises 236 firms observed over nine years (1999-2007). We use robust standard errors clustered at the firm level. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	$\log(\text{Equity Grants}_t + 1)$	$\text{Insider Portfolio Returns}_t$
	(1)	(2)
$\text{Adjusted Returns}_t$	0.00	1.31***
	(0.68)	(0.00)
$\text{Incentive Strength}_{t-1}$	−0.02	−0.68**
	(0.15)	(0.02)
$\log(\text{Private Wealth} + 1)_{t-1}$	0.42***	1.02
	(0.00)	(0.78)
Total Risk_{t-1}	0.26	−1.66
	(0.33)	(0.88)
$\log(\text{Equity Grants}_t + 1)$	-	8.21***
	-	(0.00)
$\log(\text{Equity Grants}_t + 1) \times \text{Incentives}_{t-1}$	-	−0.24***
	-	(0.01)
Additional Controls	Yes	Yes
Industry FEs	Yes	Yes
Year FEs	Yes	Yes
Adj. R^2	7.55%	12.95%
Observations	[1249]	[1249]

CEO DEATH, SUCCESSOR CHARACTERISTICS AND FIRM PERFORMANCE

P. Johan E. Mellberg[†]

Abstract

This paper analyzes how CEO death affects firm performance in *larger firms*. I first show that the detrimental effect reported in the literature survives the exclusion of smaller firms, which reduces the likelihood that the treatment effect is driven by hard-to-overcome frictional costs associated with finding a suitable successor. The passing of the incumbent CEO has a clear negative effect on firm performance, indicating that incumbent CEOs is hard to replace. I then show that the treatment effect is surprisingly homogeneous with respect to the characteristics of both the firm and the incumbent CEO, suggesting that assignment is efficient *ex ante*. To further examine whether the average treatment effect is driven by CEO uniqueness or frictions in the labor market, I test for the effects of successor characteristics on the strength of the treatment effect. It turns out that different successor types play only a minor role in determining the outcome in the post-treatment period, suggesting that the average treatment effect is mainly driven by the uniqueness of the incumbent CEO.

Key Words: Corporate Finance, Corporate Governance, Family Firms, Firm Performance, Human Capital, Ownership Structure, Shareholder Monitoring, Succession
JEL Classifications: G32, G34

[†]Norwegian School of Economics (NHH)

I Introduction

How much do chief executive officers (CEOs) matter for firm performance? Analyzing this question is challenging as CEOs are not randomly assigned, but instead matched to their firms in the labor market, creating a selection bias. For example, skilled CEOs may cumulate in certain types of firms (e.g. more successful and prestigious firms), making it difficult to attribute cross-sectional differences in firm performance to the CEO. Within effects too are hard to assess since regular turnovers are likely to be correlated with pre-treatment characteristics of the firm (e.g. declining performance in the period leading up to succession).

To address this, a strand of literature has focused on examining changes in firm performance around the death of an incumbent CEO, which can be seen as an exogenously timed turnover event, free of the aforementioned endogeneity issues (Johnson et al., 1985; Bennedsen, Pérez-González and Wolfenzon, 2010; Nguyen and Nielsen, 2014; Jenter, Matveyev and Roth, 2016). The main findings of this literature are 1) that exogenously timed turnovers affect firm performance negatively on average and 2) that the effect is highly heterogeneous with respect to the characteristics of both the firm and the incumbent CEO.[1] This suggests that some CEO-firm pairs are not perfectly matched, i.e. assignment is not always efficient *ex ante*. While this identification strategy has the benefit of reducing endogeneity issues, it comes at the cost of focusing predominantly on very small firms (Bennedsen, Pérez-González and Wolfenzon, 2010; Becker and Hvide, 2016). This poses a potential problem since smaller firms are less likely to enter the market for managerial talent following the death of the incumbent CEO, making it more likely

[1] Johnson et al. (1985) examines sudden deaths in 53 U.S. firms and find that returns are abnormally high (low) for founder (non-founder) CEOs and negatively related to past performance, which suggests that founder CEOs destroy firm value whereas professional CEOs enhance it. Jenter, Matveyev and Roth (2016) employ a larger sample consisting of public firms and distinguish between sudden and non-sudden deaths. They show that slow deaths associate with positive value changes and sudden deaths with negative ones. The treatment effect is heterogeneous with respect to CEO age and tenure. The largest effect is for sudden deaths of young or short-tenured CEOs. This suggests that a significant part of the matching surplus benefits shareholders and not just the CEO. Other CEO deaths – non-sudden deaths, and sudden deaths of old and long-tenured CEOs – are on average associated with large value gains. As for private firms, Becker and Hvide (2016) use a sample consisting of smaller firms drawn from population of Norwegian firms and finds significant and negative effects on firm performance. They do not however find a significant treatment effect for the top quintile of the sample, which corresponds to firms with more than eight employees. Similarly, Bennedsen, Pérez-González and Wolfenzon (2010) uses a sample consisting of 1015 CEO events drawn from the population of danish private firms. Their results show that death leads to significant declines in performance as measured by firm profitability, investment, and sales growth.

that the treatment is driven by frictional costs rather than CEO uniqueness.

The aim of this paper is to examine how CEO death affects firm performance in "larger firms" (defined as having 10 or more employees in each of the two years preceding the death of the incumbent CEO). Analyzing whether the negative effect reported in the literature is robust to the exclusion of smaller firms is important because larger firms are presumably better positioned when it comes to recruiting a new CEO. In addition, examining the effect of CEO death in larger firms is interesting in its own right since the contribution of the CEO may differ between large and small firms. On the one hand, the personal traits of incumbent CEOs may be so crucial for operations in smaller firms that most of the firm's value is embedded in the CEO. If this is the case, CEOs of smaller firms may be harder to replace than CEOs of larger firms. On the other hand, if better CEOs get assigned to larger firms and managerial input has a multiplicative effect on the outcome, the opposite may be true.

To further examine whether the treatment effect is the result of CEO uniqueness or frictions in the labor market, I test for the effects of successor characteristics on the strength of the treatment effect. If the treatment effect is mainly driven by frictions, I conjecture that heterogeneity in successor traits will explain part of the treatment effect, after controlling for firm fixed effects. On the other hand, if incumbent CEOs are pivotal to the extent that they are *de facto* irreplaceable at the time of death, I expect heterogeneity in successor traits to have low explanatory power. In the analysis, I focus on four successor traits, namely whether the successor has previously held the position of CEO, whether the successor has previous firm or industry experience and whether the successor has family ties to the incumbent CEO (family succession). In my sample consisting of 381 firms, approximately 39% of the successors have CEO experience, 29% have industry experience, 48% have firm experience and 38% have family ties to the incumbent CEO (family succession). I explore these sources of heterogeneity and test whether they affect the outcome. I recognize that, even though the departure of the incumbent CEO is likely to be exogenous, the choice of successor may not be. Some firms may for example have a succession plan in place whereas others do not and this may be correlated with other characteristics of the firm and the CEO. In order to address this, I carefully compare pre-treatment trends in both the

outcome and firm characteristics for treated and control firms. I also include firm fixed effect in all regressions, which mitigates concerns that the results are driven by simple matching of firms and successors.

The analysis is carried out in four steps. First, I examine the importance of the incumbent CEO relative to the succeeding CEO, using the death of the incumbent CEO as a source of exogenous variation. The benefit of this approach is that death is by large an exogenous event that is uncorrelated with firm performance.[2] Also, the timing of the event is spread over a long period of time, which mitigates concerns about intertemporal heterogeneity.[3] If the operational role of the individual CEO is important, I expect the death of the incumbent CEO to have adverse effects on firm performance. The first set of results confirm those of the extant literature - the treatment effect is significant and negative, which suggests that the incumbent CEO is hard to replace. The results are robust to various model specifications and estimation windows.

Second, I show that the heterogeneity in the treatment effect reported in the literature does not survive the exclusion of smaller firms. The treatment effect is remarkably homogeneous with respect to the pre-treatment characteristics of both the firm and the incumbent CEO, which suggests that the assignment of CEOs is efficient *ex ante*. These results differ from those found in several of the previous studies on CEO deaths (Johnson et al., 1985; Jenter, Matveyev and Roth, 2016; Becker and Hvide, 2016).

Third, I show that the choice of successor is intuitively related to certain characteristics of the firm and the incumbent CEO, i.e. assignment is not random. Larger firms, which are presumably in a better position to hire professional managers, do so more frequently, whereas firms where the incumbent CEO has more power over corporate decisions are more likely to hire a successor from within the firm.

Fourth, I show that successor traits play only a minor role in determining the outcome in the

[2]The event could be correlated with past performance if the firm suffers from the CEO being ill during some period before death. A careful analysis of the pre-treatment trends in the outcome variables suggests that this is not the case.

[3]The staggered nature of the event (CEO death is spread over the period 1998-2014) reduces the likelihood of secular trends in firm performance.

post-treatment period, which suggests that the average treatment effect is mainly driven by CEO uniqueness. After controlling for firm fixed effects, I observe no significant difference in the treatment effect for firms that end up with a certain type of successor compared to those that do not.[4]

The remainder of this paper is organized as follows. Section II provides a brief discussion of the theoretical background. Section III discusses the sample selection and data sources and presents summary statistics. Section IV outlines the empirical strategy. Section V presents the results and section VI concludes.

II Theoretical Background

II.A Why Should Turnover Matter?

Jenter, Matveyev and Roth (2016) outline the conditions under which CEO turnover may have an effect on the firm. Assuming that managerial input is part of the production process, the effect hinges on the extent to which CEOs differ from one another, the scarcity of managerial talent in the labor market and whether there are frictions in the assignment process. If managerial input is not important, or if there is a large supply of homogeneous managerial talent in the market and if the assignment of managers to firms is sufficiently free of frictions, then the death of the incumbent CEO should have little effect on firm performance. On the other hand, if managerial input is important and talented CEOs are in scarce supply, or alternatively if there are hard-to-overcome frictions in the matching process, then exogenously forced turnover can have severe consequences for the firm.

[4]Since the choice of successor is not exogenous in the same sense as the death of the incumbent CEO, the results in this part of the analysis is more prone to suffer from selection bias. Previous studies have for example shown that firm performance, size, and board characteristics affect firms' hiring and firing decisions (Weisbach, 1988; Denis and Denis, 1995; Parrino, 1997), as well as as the selection of internal relative to external successors (Parrino, 1997). To deal with this, I carefully analyze the pre-treatment trends in both the outcome and other characteristics relating to the firm and the CEO. Overall, the results appear not to be caused by differences in observable characteristics in the period leading up to the event.

II.B The Effect of Firm Size

In theory, the importance of the CEO may vary with firm size. In large corporations, the CEO typically deals with high-level strategic decisions, whereas in smaller firms, the CEO is typically more hands-on and involved in the day-to-day functions and therefore sets the tone, vision and sometimes the whole culture of the organization. Thus, it is intuitive to think of CEOs in small firms as possessing some sort of unique firm-specific human capital, which makes them crucial for the growth and survival of their firms.[5] Several models support this line of thinking and explicitly assume the role of the manager to be pivotal in the formative stages of a firm's life (Hart, 1995; Rajan and Zingales, 1998, 2001). An implicit assumption is therefore that the importance of the manager may weaken as the firm matures and grows larger.

Other models assume managerial input to have a multiplicative effect on the outcomes and therefore predict that larger firms will hire the most talented CEOs (Rosen, 1982; Gabaix and Landier, 2008; Terviö, 2008). If more talented CEOs get assigned to larger firms and if matching talent in the high end of the distribution is sufficiently scarce in supply, CEOs in larger firms could turn out to be more important for the firm than CEOs in smaller firms.

II.C Scarcity of Managerial Talent vs. Market Frictions

Market frictions associated with abrupt turnover, such as screening and transition costs, may prove hard to overcome for some firms and could lead to inefficient matching outcomes. Due to such frictions, the firm may fail to recruit the best available successor. There are several potential reasons why this might happen. Family-led firms could for example be biased toward family succession and smaller firms may lack the muscles needed to screen the market for a professional manager (Bennedsen et al., 2007; Jenter, Matveyev and Roth, 2016). To help distinguish between the supply side of managerial talent and market frictions, I examine the impact

[5] Early models view managers as an homogeneous group, where individual managers form close substitutes (Coase, 1937; Alchian and Demsetz, 1972; Jensen and Meckling, 1976; Williamson, 1979). Much like capital or labor, CEO effort is considered an important input, but individual traits do not enter the production process. More recent models view human capital as an important source of value creation (Rosen, 1981; Hart and Moore, 1994; Rajan and Zingales, 1998, 2001; Murphy and Zabojnik, 2004; Gabaix and Landier, 2008). Knowledge and skills are seen as important resources and managers are allowed to differ in terms of their abilities and preferences.

of successor traits on the strength of the treatment effect. If the average effect is predominantly the result of incumbent CEOs being so pivotal for the outcome that they are *de facto* irreplaceable, I conjecture that successor types will have only a small effect of the treatment effect, after controlling for firm fixed effects. Conversely, if the the average effect is predominantly the result of market frictions, I conjecture that firms that actually do manage to hire successors with desirable qualities (presumably those with CEO or industry experience) will outperform those that do not.

III Sample and Data Description

III.A Sample Construction

I start with the entire population of Norwegian firms from 1998 to 2014, a total of approximately half a million unique firms. Of these, 11,174 experience the death of an incumbent CEO at some point during the sample period. Most of these firms are small and are discontinued when the incumbent CEO dies. I first eliminate 7,502 firms that do not survive after the incumbent CEO dies, which reduces the sample to 3,672 firms. While such elimination risks creating a selection bias, the only way to include these firms in the sample is to set their post-treatment performance to zero. Since this may create an even greater bias, I limit the sample to surviving firms.[6] Survival rates are analyzed separately using the full sample.

I require firms to have 10 or more employees at the end of the years t=-1 and t=-2, where t=0 is the year of treatment.[7] This reduces the sample size dramatically to 412 firms. As of January 1, 2016, there were 558,959 firms incorporated in Norway and 498,876 (89.3%) of them had less than 10 employees. 56,507 (10.1%) had between 10 and 100 employees and 3,576 (0.6%) had more than 100 employees. As such, the final sample firms belong to the top decile of the

[6] I relax this condition when examining survival rates.

[7] Measuring pre-treatment characteristics close to the event constitutes a trade-off between preserving the sample size and mitigating concerns about selection bias. Treated firms are potentially exposed to treatment prior to the actual event. This concern might be particularly relevant in private firms, where the CEO arguably have more power to stay on despite poor health.

Norwegian economy in terms of the number of employees.

There are two reasons for excluding micro firms from the analysis. First, micro firms are often single-person businesses, making them less likely to enter the market for managerial talent following treatment. Second, micro firms have volatile accounting figures, which makes interpretation of treatment effects difficult. For example, the fraction of zeros (not NAs) for *Sales* in the first two periods following treatment is 56% and 62% respectively in the sample consisting of micro firms only, which is more than ten times the frequency observed in my sample. This raises questions about the post-treatment status of many micro firms.

Lastly, I exclude firms that do not report a primary industry classification. The final sample consists of 381 unique firms and 5519 firm-year observations.

III.B Data

The data set includes financial statements, individual-level data on top managers and directors, including names and dates of birth and death as well as ownership stakes. The final data set merges data from three different sources.

1. Samfunns- og Næringslivsforskning at NHH (SNF): The database "Norwegian Corporate Accounts" contains financial statements for the entire population of Norwegian limited liability firms and stretches from 1992 to 2014. In addition, it also provides additional firm-specific information, such as the year of incorporation, address, industry classification etc..

2. Brønnøysundregistrene: Includes data on key individuals and covers the period from 1998 to 2014. For each individual, the data set identifies corporate role, year of birth, sex, address and year of death.

3. Bisnode: This database contains complete ownership data for Norwegian firms. For each shareholder and intermediate owner, the database tracks the name and share of the ultimate owner.

All variables used in the analysis are defined in Table I.

III.C Successor Types and Founder Status

In order to test the effects of successor types on firm performance, I focus on four successor traits, namely whether the successor has previously been employed as a CEO, whether the successor has industry experience, whether the successor is recruited from within the firm and whether the successor has family ties to the incumbent CEO.

CEO experience is defined as having held the position of CEO in any other Norwegian firm with more than 10 employees in the 5 years leading up to the succession. An obvious limitation with this measure is that I cannot identify CEO experience received in firms domiciled outside Norway. Industry and firm experience is defined as having held a top position in any Norwegian firm with more than 10 employees that has the same industry classification as the treated firm or in the treated firm itself in the 5 years leading up to the death of the incumbent CEO. In order to avoid overlap, industry experience excludes the CEO position. Lastly, family ties is identified by matching the surname of the incumbent CEO and the successor. This measure too is limited by the fact that I am only able to identify family members that have the same surname as the incumbent CEO. There is certain degree of overlap in the dependent variables. In particular, CEO experience and industry experience may capture similar characteristics as they both represent external hires whereas firm experience and family succession both represent recruitment from within the firm.

I also construct a dummy for whether the incumbent CEO is the founder of the firm. Incumbent CEOs are classified as founders whenever in the five-year period leading up to the event 1) they own more than 50 percent of the shares on average and 2) no other CEOs are present. This approach is similar to that used in Nguyen and Nielsen (2014).

III.D Outcome Variables

In order to evaluate the impact of CEO death, I test for effects on firm profitability, growth and survival. Profitability is measured as operating return on assets (OROA). OROA is defined as the ratio of earnings before interest and taxes (EBIT) to the total asset base used to generate them. This measure is widely used in the literature (Bennedsen, Pérez-González and Wolfenzon, 2010; Becker and Hvide, 2016). To assess growth, I examine the effect on sales and on human assets as measured by the number of employees. Lastly, survival is measured by whether a firm is included in the source files in a given year.

III.E Descriptives

Table II describes the firm's age at the time of treatment (t=0) as well the year of treatment. Column three shows that death occurs in all stages of the firm's life, with a close to uniform distribution. Similarly, column six shows that treatment is spread across the whole period. This mitigates concerns that intertemporal heterogeneity could be driving the results.

Table III shows summary statistics for the full sample in the four-year window around the event (from t=-2 to t=2). Year zero is excluded since it is not clearly attributable to either the pre- or post-period. Indented entries contrast pre- and post-treatment statistics. Incumbent CEOs are on average in their late 50s at the time of death and successors are about ten years younger when they assume the position. Board representation is high and in 35% of all firms, the CEO also serves as the chairman of the board. The fraction of female CEOs is 11%. CEO ownership changes considerably around the event, with stakes dropping on average from around 40% in the pre-treatment period to around 20% in the post-treatment period. This drop is persistent over time and even though there is a certain degree of recovery up to t=3, ownership never recovers more than 50% of its pre-treatment level. Even if the data does not allow for complete identification of family members, the drop in ownership suggests that CEOs who die are often succeeded by professional managers rather than by other family members. Similarly, the fact that pre-treatment ownership levels are high suggests that the incumbent CEOs are

often founders or members of the founding family. Untabulated results show that the change in ownership is more pronounced in my sample compared to the population of firms that experience CEO death (i.e., including micro firms). This is expected, since larger firms presumably have more resources to recruit a professional manager than do smaller firms. About 30% of the incumbent CEOs are classified as founders. In terms of successor characteristics, 39% of the incoming CEOs have CEO experience, 48% have firm experience, 29% have industry experience and about 38% have identifiable family ties to the incumbent CEO. This shows that the sample contains considerable heterogeneity in terms of successor characteristics.

In terms of firm characteristics, the sample display skewness in variables related to firm size. The number of shareholders, the number of employees as well as total assets and sales are all highly skewed to the right. To mitigate the effects of outliers in the empirical test, I log these variables.

An important question is whether firms that subsequently select into different successor categories are comparable. For example, if all firms that choose to hire professional managers following the death of the incumbent CEO are experiencing a downward trend in the outcome compared to firms that do not, attributing differences in post-treatment performance to successor traits is problematic. In order to address this, Figure II plots the trends in the outcome variables in the four-year period leading up to the event. The blue lines represent firms that select into a given category (treated) and the yellow lines shows the trend for the firms that do not (control). There are two interesting takeaways from this table. First, treated and control firms seem to follow similar trends but sometimes differ in levels. In terms of internal validity, differences in levels are acceptable, since they are canceled out in the analysis. Second, there are no sharp drops in the outcome variables before the event, which suggests that firms were not greatly affected by the upcoming departure of the incumbent CEO before it actually happened. Arguably, this can be interpreted as a sign that most deaths were relatively sudden.

IV Testing Strategy

IV.A Evaluating the Effects of CEO Death

The simplest way to test for a treatment effect is to assess the difference in firm performance around the event. If incumbent CEOs are important to firms, one would expect firm performance to fall following the death of the incumbent CEO. The primary threat to identification is of course that there might be other time-varying factors affecting the outcome as well, such as changes in firm characteristics or operating environment that are not attributable to the event. However, the staggered nature of the event studied in this paper arguably makes it well-suited for time-series comparisons. As shown in Table II, firms are not treated at the same point in time, but instead assigned to treatment over a long period. Because of this, secular trends are unlikely to be driving the results. Estimating CEO effects at the firm level is attractive as it allows me to filter out time-invariant characteristics of the firm. To further mitigate concerns that the potential treatment effect is driven by secular trends, I also evaluate the effect against a control group of firms where the founder does not die (matched sample). Appendix A describes the matching procedure for constructing the control group. I then estimate the treatment effect of CEO death using difference-in-differences.

IV.B Evaluating the Effects of Successor Types

In order to examine the effects of successor characteristics on firm performance, I again employ a difference-in-differences testing strategy, where the treatment group consists of firms with a certain successor trait. Using this additional difference-in-differences approach is attractive because it highlights whether the net effect of CEO death is sensitive to different successor types. For example, if CEO experience is an important trait, I expect the negative effect of CEO death to be significantly smaller in firms that manage to recruit successors with CEO experience. Even though the death of the incumbent CEO is likely to provide exogenous variation in terms of the

timing of succession, it is less clear that the choice of successor is random.[8] For example, firm prospects at the time of death may influence the decision to employ a professional manager and at the same time affect the outcome moving forward. A natural way to proceed would be to employ an instrument for successor types, in the spirit of Bennedsen et al. (2007). However, finding suitable instruments for CEO traits and experiences is difficult. In the absence of an exogenous source of variation in the succession choice, I use the following approaches.

1. I include firm fixed effects in all specifications, mitigating concerns that the results are driven by endogenous matching of firms and successor types. An advantage of using within-firm variation in performance is that it allows me to control for time-invariant characteristics that might jointly affect a firm's prospects and its decision to appoint a certain type of CEO.

2. I carefully check for pre-treatment differences in performance across groups of firms that subsequently select into different successor categories. It turns out that there are no major differences in outcome trends prior to the death of incumbent CEO.

V Empirical Tests

V.A Average Effects

The first part of the analysis examines the overall effect of CEO death on firm outcomes. If incumbent CEOs are important for the firms that they manage in a way that makes them hard to replace, I expect passing of the incumbent CEO to have a negative impact on firm outcomes. To test this, I estimate the following model

$$Outcome_{it} = \alpha + \beta_1 After_t + \beta_2 X_{it} + \lambda_i + \varepsilon_{it} \quad (1)$$

[8] On the other hand, it could be argued that CEO deaths cause a certain degree of randomness in the selection of the successor. It seems plausible that at least a fraction of the firms in the sample will have to make quicker and "less planned" succession decision compared to what they would have done under an endogenously timed transition.

, where *Outcome* is either *ROA*, *log(Sales + 1)*, *log(Employees + 1)* or *Survival*. *After* is the treatment switch and takes the value one in the years following the event. X is a matrix of control variables that includes *log(Assets + 1)*, *Leverage*, *CEO Age*, *CEO Gender*, *CEO Director*, *Board Size* and *Board Ownership* and λ is a vector of firm dummies.

The results are reported in panel A of Table IV. The sample is restricted to the period -4/+2 years around treatment and year zero is excluded as it is not clearly attributable to either the pre- or post-treatment period. Also, in all columns except the last, I condition the results on survival until $t = +2$. In all columns, the *After* switch is negative and highly significant, which suggests that incumbent CEOs are both important for firm performance and hard to replace. The effects on profitability and growth are large. *OROA* drops by 5 percentage points on average in the two-year period following the event, and *Sales* and *Employees* drop by 46% and 17% respectively.[9] These drops are similar in magnitude to those reported by Bennedsen, Pérez-González and Wolfenzon (2010) and Becker and Hvide (2016). Bennedsen, Pérez-González and Wolfenzon (2010) report a 2.2% drop in OROA in the two-year period following CEO death and Becker and Hvide (2016) report a 2.4% drop in OROA, a 67% drop in sales and a 24% drop in employees when studying a sample of smaller entrepreneurial firms. In the last two columns, I test the effect on firm survival.[10] On average, *Survival* drops by 13% in the post-treatment period.

As a robustness check, panel B of Table IV re-estimates the basic model using different estimation windows and sample cuts. The results are both qualitatively and quantitatively very similar to those in panel A.

In panel C, I analyze the timing/duration of the treatment effect by adding leads to the model. Specifically, I fit the following model

$$Outcome_{it} = \alpha + \sum_{\tau=1}^{q} \beta_{+\tau} After_{t+\tau} + \delta After_t + \gamma X_{it} + \lambda_i + \varepsilon_{it} \quad (2)$$

, where $After_t$ equals the switch in the original model and q is the number of leading terms.

[9] The coefficients are transformed into percentage effects using exp(coefficient)-1.

[10] This specification does not include any continuous control variables. The reason for this is that all continuous variables will have missing values whenever *Survival* is equal to 0, which makes estimation impossible.

By adding lead treatment switches to the model, I can investigate whether, in addition to the original switch, the effect subsequently grows or fades over time. If the effect gets stronger after the passing of the incumbent CEO, I expect some of the leading switches to be negative and significant. Alternatively, if the effect is transient, I expect some of the leading switches to be positive and significant. Most of the leading switches are either insignificant or negatively significant, which suggests that the effect increases slightly over time. The death of the incumbent CEO seems to have a persistent effect on firm outcomes that are felt well beyond the first couple of years. Looking at the last leading switch for sales, we also get some indication that the negative effect may be reversing after the fifth post-treatment period. Figure I paints a similar picture.

V.B Checking for Pre-Treatment Effects

Whether or not the results in panel A of Table IV can be given a causal interpretation depends on pre-treatment trends in the output. A potential cause for concern is of course that I cannot distinguish between sudden and slow deaths, which calls into question whether the event can be thought of as exogenous. If most deaths are slow, selection into treatment might be a severe problem. One mitigating factor here is that CEOs of larger firms may have less power to continue in their roles as CEOs while being terminally ill. If so, it seems plausible that these CEOs would step down and be replaced before the time of death, which would exclude them from the sample. Figure I shows the pre-treatment trends in the outcome, starting six years before the event and ending six years after the event. If the event is truly exogenous, there should be no major movements or downward-sloping trends in the outcome before the event. On the other hand, if pre-shock trends can explain the results, movements in the output should be observable before the event. Figure I shows no signs of anticipating movements in the outcome - both sales and the number of employees increases in the period leading up to the event whereas ROA decreases slightly, but the drop in not significant. Even so, the point estimates in Table IV may confound two types of effects; the post-treatment effect of abruptly loosing the incumbent CEO and the pre-treatment effect associated with continuing operations under a slowly dying CEO. Although

these two effects are different, they are similar in both being expected to have adverse effects on firm performance.

Panel D of Table IV tests for differences in pre-treatment trends more formally by running a placebo test, using only observations from the pre-treatment period. I use observations from t=-8 to t=-1 and define treatment to occur at t=-4. Under this specification, all coefficients come out insignificant, which suggests that the results in panel A should be given a causal interpretation.

V.C Alternative Approach: Matching

Another way to isolate the treatment effect is to use a comparable control group consisting of firms where the incumbent CEO does not pass away but with otherwise similar pre-treatment characteristics. This type of approach further alleviates concerns that the main treatment effect in Table IV is the results of contemporaneous shifts in some omitted endogenous variables or the outcome. The matching approach in described in detail in Appendix A. Each treated firm is matched to one control firm, based on year of incorporation, firm age, industry, the outcome variable at t=-1 and the change in the outcome at t=-1 and t=-2. The year of death in the treated firm is used to impute the counterfactual year of death in the corresponding control form. The results are presented in Table A2. In all columns, the treatment effect is both quantitatively and qualitatively comparable to those in Table IV. The death of the incumbent CEO has both a significant and negative effect on the outcome.

V.D Heterogeneous Effects: Incumbent Traits and Firm Characteristics

The effect of CEO death may be heterogeneous with respect to the incumbent CEO and the firm. In the following subsections, I explore whether the pre-treatment characteristics of the firm and the CEO has any effects on the strength of the treatment effect.

V.D.1 Founders vs Non-Founders

Founders not only manage their firms, they also provide the initial business concept, which arguably makes them unique compared to other CEOs. If entrepreneurial skills are correlated with managerial skills once the firm has been established, the two groups could experience different treatment effects. Also, founders may have more power over decision-making compared to non-founders, which could be used to stay on as CEO even though it is no longer optimal for the firm. To test this, I add a dummy to (1) that takes the value one if the incumbent CEO is also the founder and interact it with the post-treatment switch. If founders are unique, I expect founder deaths to have a stronger detrimental effect on the firm than non-founder deaths. If instead, founders use their power to stay on as CEOs, I expect the coefficient of the interaction term to come out positive.

The results are presented in panel I of Table V. Contrary to the results in Johnson et al. (1985) and Becker and Hvide (2016), founder deaths do not lead to stronger detrimental effects compared to non-founder deaths. Overall, founders do not appear to be more valuable to the firm than non-founder CEOs.

One possible explanation for this lack of effect could be measurement error. As outlined in section III.C, the data does not allow for direct identification of founders. Instead, I classify CEOs as founders whenever in the five-year period leading up to the event 1) they own more than 50 percent of the shares on average and 2) no other CEOs are present. Obviously, the combination of CEO ownership and tenure is by no means a perfect proxy for founder status. However, when restricting the sample to firms that were incorporated after 1997, for which I can observe the identity of the CEO for all years of the firm's life, I observe that more than 90 percent of the CEOs that had a majority stake in the firm at the time of death were also CEOs at the time of foundation. Even though these firms account for only 25 percent of the full sample of firms, it suggests that the group of CEOs that I classify as founders predominantly consist of actual founders. Also, unreported results show that the treatment effects for founders and non-founders are unaffected by the exclusion of firms for which I cannot directly establish

the identity of the incumbent CEO.

V.D.2 Old vs Young Incumbents

Similar to the argument made in the previous section, the treatment effect may be heterogeneous across the age distribution. Older CEOs may for example be less dynamic than younger CEOs and therefore easier to replace. Also, the likelihood of death being anticipated (which could cause firms to have a succession plan installed prior to the event) is likely to be higher when the incumbent CEO is old. In panel II of Table V, I test for the impact of CEO age on the treatment effect. Again, all interaction term coefficients come out insignificant.

V.D.3 Firm Characteristics

Another possibility is that the treatment effect is heterogeneous with respect to pre-existing firm characteristics, such as past performance or firm size. The importance of the incumbent CEO could for example diminish as the firm matures and grows bigger. Also, the quality of the incumbent CEO could be correlated with past performance. To analyze this, panels III-V in Table V tests for heterogeneity with respect to firm characteristics. Panels III.A-III.B tests for the impact of firm size, panel IV of firm age and panels V.A-V.B of previous performance. The baseline effects come out highly significant and with the exception of previous performance in panels V.A and V.B, all interaction terms come out insignificant. As for previous performance, the death of the incumbent CEO seems to be associated with a larger drop in OROA when the firm performs well in the pre-treatment period, as measured by the level of OROA in t=-1. If we instead measure performance by the change in OROA from t=-4 to t=-1, the effect disappears. Also, strong performance in the pre-treatment period is associated with an approximate 10 percentage points higher survival rate. Overall, the results in panels V.A and V.B give some support to the idea that incumbent CEOs are hard to replace when firms are doing well at the time of treatment.

V.E Who Takes Up the Mantle?

Next, I examine whether pre-treatment characteristics of the firm and the incumbent CEO explains the choice of successor. Given the departure of the incumbent CEO, if boards and shareholders believe that the incoming CEO is going to be important for the future performance of the firm, I expect at least some characteristics relating to the firm to matter.

Table VI reports point estimates from logit regressions of successor type (dummies) on firm and incumbent CEO characteristics at t=-1. The only variable that consistently influences the choice of successor is firm size, as measured by *log(Assets + 1)*. As expected, size has a positive effect on the likelihood of appointing a successor with CEO or industry experience and a negative effect on the likelihood of appointing a successor with previous firm experience or family ties to the incumbent CEO. A consistent interpretation is that larger firms with more financial muscles are more successful in recruiting professional managers, whereas smaller firms have to rely on internal recruitment. In addition, the change in ROA is positively correlated with industry experience and the level of ROA is negatively correlated with family succession. Most of the other firm characteristics come out insignificant.

Several variables relating to the incumbent CEO and the board are also significant. *Director* is for example positively related to the probability of family succession and the probability of hiring a successor with firm experience, which is consistent with the notion that firms where the incumbent CEO has more power are more likely to recruit internally. Family succession is also positively related to *Chair* and *Age*, which can be given a similar interpretation. Interestingly, *Founder* is only significantly related to the decision to appoint a successor with industry experience. A potential explanation for this could be that founder status is correlated with many of the other CEO characteristics. Lastly, both *Board Ownership* and *Board Size* are positively related to internal succession.

In summary, the results in Table VI gives some support to the idea that firm characteristics around the time of death should matter for the choice to appoint a certain type of successor. In particular, larger firms seem to rely more on external recruitment while smaller firms where the

incumbent CEO has more power tend to rely more on internal recruitment.

V.F Does Successor Type Matter?

I now turn to the question of whether the negative treatment effect documented in Table IV is affected by successor characteristics. Using difference-in-differences methodology, I fit the following model.

$$Outcome_{it} = \alpha + \beta_1 Treated_i \times After_{it} + \beta_2 After_{it} + \beta_3 Treated_i + \lambda_i + \delta X_{it} + \varepsilon_{it} \quad (3)$$

, where *Treated* indicates whether the succeeding CEO possesses a certain trait. If the average treatment effect is predominantly the result of scarcity of matching managerial talent, I hypothesize that successor type will matter little for the strength of the treatment effect once I control for firm fixed effects. Conversely, if the average effect is predominantly the result of friction in the assignment process, I conjecture that firms that do manage to recruit better successors (presumably those with CEO or industry experience) will do better than firms that do not and vice versa, and therefore that successor type will matter for the outcome.

The results are reported in Table VII. Each of the panels A-D report the effects of a separate trait. In each panel, sub-panel (I) tests for pre-treatment differences in the outcome using a simple t-test whereas sub-panel (II) reports the results from the difference-in-differences regression.

The results in (I) suggest that there are no statistically significant differences in the outcome trends before the event and with the exception of *log(Sales + 1)* and *log(Employees + 1)* in panel D, there are also no individual trends. This suggests that the parallel trend assumption is satisfied and that the difference-in-differences approach in (II) is feasible.

The main results in (II) show that successor characteristics have little effects on the outcome, both when it comes to ROA, sales and the number of employees. There is a slightly negative effect on survival when the successor has CEO experience and a slightly positive effect when the successor has family ties to the incumbent CEO. In the former case, the effect is only significant at 10%, whereas in the latter, the effect is significant at 1%. The negative effect on survival in panel D is also associated with significant drop in ROA. One interpretation of this result is that

family succession gives incentive to keep the firm alive irrespective of performance.

Overall, the results Table VII suggest that successor traits are not very important in determining the post-treatment performance of the firm, which is consistent with the idea that incumbent CEOs are unique. At the same time, it raises the question of why firms choose to recruit professional managers in the first place? One explanation could be that firms optimally select into different successor categories, i.e. successors matter per se, but since the incumbent CEOs are unique, successors will always under-perform relative to their predecessors.

V.G Are Firms that Select into Different Successor Categories Comparable?

To make sure that firms that choose a certain successor do not experience different trends in observable characteristic in the periods leading up to the event, Table VIII tests for differences in both the outcome and other observable characteristics in the four-year period leading up to the death of the incumbent CEO. In columns 1-3, I spit the sample based on whether the successor has CEO experience, in columns 4-6 on whether the successor has firm experience and so on. Each row represents the difference in means between the treatment and control group at a specific time. As shown, with few exceptions, most of the trends in the observable characteristics (highlighted in gray) are not significantly different across groups, which again suggests that the difference-in-differences approach is feasible. Importantly, whenever groups differ in levels, they follow similar trends.

V.H Higher Order Interactions

The results so far are consistent with the idea that incumbent CEOs are unique and personally embed a major part of the value of the firm. One possible explanation for the lack of heterogeneity in the treatment effect documented in Table V and Table VII could be that the effects cancel out at the group level. For example, it is possible that a certain successor trait, such as *CEO Experience*, has a positive effect on the outcome for only a subsample of the firms that under-perform during the last years under the incumbent CEO, but a negative effect on

firms that over-perform. In order to test this, Table IX interacts successor type with firm and incumbent CEO characteristics and the post-treatment dummy.

I begin in panel A by showing how successor traits are distributed across different sub-samples. If firms optimally select into different successor categories based on observable characteristics, there could be an overlap issue in the data. It turns out that this is not the case. Irrespective of how I split the sample, there seems to be ample variation in terms of successor experience. Of the firms that are sorted into any of the specific pre-treatment categories, the percentage of firms that subsequently select into any of the four successor categories ranges from approximately 15% to 50%, which suggests that the choice of successor type is not highly correlated with pre-treatment characteristics of the firm.

The regressional results are reported in Panel B. Each sub-panel shows the triple interaction term from the following specification

$$\begin{aligned} Outcome_{it} = \alpha + \beta_1 D_i \times Treated_i \times After_t + \beta_2 Treated_i \times After_t + \\ + \beta_3 D_i \times After_t + \beta_4 After_t + \lambda_i + \delta X_{it} + \varepsilon_{it} \end{aligned} \tag{4}$$

, where D is a dummy that takes the value one if the firm is either above-median in terms of a certain observable pre-treatment characteristic, such as size, or constitutes a certain firm type, such as being a founder firm. *Treated* is a dummy that takes the value one if the firm selects into a certain successor category and *After* takes the value one in the two post-treatment periods and zero otherwise.

In most cases, I do not find any clear evidence of more granular treatment effects. There is some evidence that *Family Succession* is particularly hurtful in firms where the incumbent CEO is also the founder. The coefficients for OROA, sales and employees are all negative but only sales is significant. *Firm Experience* in founder firms is also associated with slight increase in the number of employees and survival. Firm performance in the period leading up to the event seems to not matter much the effect of successor type. Looking at firm age, *Firm Experience* has a significant negative effect on OROA in more mature firms, which is expected. When it comes to CEO age, *CEO Experience* is associated with a slight drop in OROA, which is surprising. Also, *Industry Experience* seems to have a negative impact on the outcome when the departing CEO

is older. Apart from that, there are no strong patterns in the data and overall, the results in Table IX do not reveal any clear differences in the treatment effects across different combinations of pre-treatment characteristics and successor traits. In particular, irrespective of how I define D, there is no combination of D and successor type that generates significant treatment effects across all outcome variables.

VI Conclusions

In this paper, I have studied the relationship between exogenously timed successions and firm profitability, growth and survival rates. In doing so, I have tried to answer a central questions in the literature, namely, how important are individual CEOs to the firms that they manage? By excluding micro-firms from the sample, I focus the analysis on firms that are more likely to enter the market for managerial talent. Using the death of an incumbent CEO as a proxy for exogenous turnover, I document significant changes in firm outcomes around the year of succession for approximately 380 "larger firms" over the period 1998-2014. The departure of the incumbent CEO has a significant negative effect on the outcome, suggesting that the incumbent CEO is indeed hard to replace. The effect is remarkably homogeneous with respect to the characteristics of both the firm and the incumbent CEO, suggesting that assignment is efficient *ex ante*. Also, successor characteristics are mostly insignificant in determining the strength of the treatment effect, consistent with the initial result being driven by the uniqueness of the incumbent CEO and not by frictions relating to the assignment process. Overall, CEOs of more established firms seem to play a unique role not previously documented. The results support the hypothesis that CEOs possess unique qualities which make them hard to replace.

REFERENCES

Alchian, Armen A. and Harold Demsetz. 1972. "Production, Information Costs and Economic Organization." *The American Economic Review* 62(5):777–795.

Azoulay, Pierre, Joshua S. Graff Zivin and Jialan Wang. 2010. "Superstar Extinction." *Quarterly Journal of Economics* 125(2):549–589.

Becker, Sascha O. and Hans Hvide. 2016. "Do Entrepreneurs Matter?" Working Paper.

Bennedsen, Morten, Francisco Pérez-González and Daniel Wolfenzon. 2010. "Do CEOs Matter?" Working Paper.

Bennedsen, Morten, Kasper Meisner Nielsen, Francisco Pérez-González and Daniel Wolfenzon. 2007. "Inside the Family Firm: The Role of Families in Succession Decisions and Performance." *Quarterly Journal of Economics* 122:647–691.

Coase, Ronald H. 1937. "The Nature of the Firm." *Economica* 4(16):386–405.

Denis, David J. and Diane K. Denis. 1995. "Performance Changes Following Top Management Dismissals." *Journal of Finance* 50(4):1029–1057.

Gabaix, Xavier and Augustin Landier. 2008. "Why has CEO Pay Increased so Much?" *The Quarterly Journal of Economics* pp. 49–100.

Hart, Oliver and John Moore. 1994. "A Theory of Debt Based on the Inalienability of Human Capital." *The Quarterly Journal of Economics* 109(4):841–879.

Hart, Stuart L. 1995. "A Natural-Resource-Based View of the Firm." *The Academy of Management Review* 20(4):986–1014.

Jensen, Michael C. and William H. Meckling. 1976. "Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure." *Journal of Financial Economics* 3(4):305–360.

Jenter, Dirk, Egor Matveyev and Lukas Roth. 2016. "Good and Bad CEOs." Working Paper.

Johnson, Bruce W., Robert Magee, Nandu Nagarajan and Herry Newman. 1985. "An Analysis of the Stock Price Reaction to Sudden Executive Death: Implications for the Management Labor Market." *Journal of Accounting and Economics* 7:151–174.

Murphy, Kevin J. and Jan Zabojnik. 2004. "CEO Pay and Turnover: A Market-Based Explanation for Recent Trends." *American Economic Review Papers and Proceedings* 94:192–196.

Nguyen, Bang Dang and Kasper Meisner Nielsen. 2014. "What Death can Tell: Are Executives Paid for their Contributions to Firm Value?" *Management Science* 60(12):2994–3010.

Parrino, Robert. 1997. "CEO Turnover and Outside Succession: A Cross-sectional Analysis." *Journal of Financial Economics* 46(2):165–197.

Rajan, Raghuram G. and Luigi Zingales. 1998. "Power in a Theory of the Firm." *The Quarterly Journal of Economics* 113(2):387–432.

Rajan, Raghuram G. and Luigi Zingales. 2001. "The Firm as a Dedicated Hierarchy: A Theory of the Origins and Growth of the Firm." *The Quarterly Journal of Economics* 116(3):805–851.

Rosen, Sherwin. 1981. "The Economics of Superstars." *American Economic Review* 71(5):845–858.

Rosen, Sherwin. 1982. "Authority, Control and the Distribution of Earnings." *Bell Journal of Economics* 13:311–323.

Terviö, Marko. 2008. "The Difference That CEOs Make: An Assignment Model Approach." *American Economic Review* 98:642–668.

Weisbach, Michael S. 1988. "Outside Directors and CEO Turnover." *Journal of Financial Economics* 20:431–460.

Williamson, Oliver E. 1979. "Transaction-Cost Economics: The Governance of Contractual Relations." *Journal of Law and Economics* 22(2):233–261.

Figure I

Outcome Variables over Time for Firms Experiencing CEO Death

This figure shows end-of-year average values of *ROA*, *log(Sales + 1)*, *log(Employees + 1)* and *Survival* for those firms that experienced the death of the incumbent CEO sometime during the sample period. Death occurs at $t = 0$ and is spread in real time over 16 years. All outcome variables are normalized to 0 at $t = -1$. For readability, *log(Sales + 1)* and *log(Employees + 1)* have been scaled by a factor of 10. *Survival* is dummy that takes the value 1 if the firm has available accounting data in a given year. The vertical line marks the last full year of the incumbent CEO. The total sample includes 381 firms observed over the period 1999-2014, resulting in 3675 firms-year observations when restricting the period to $-6/+6$ years around the event.

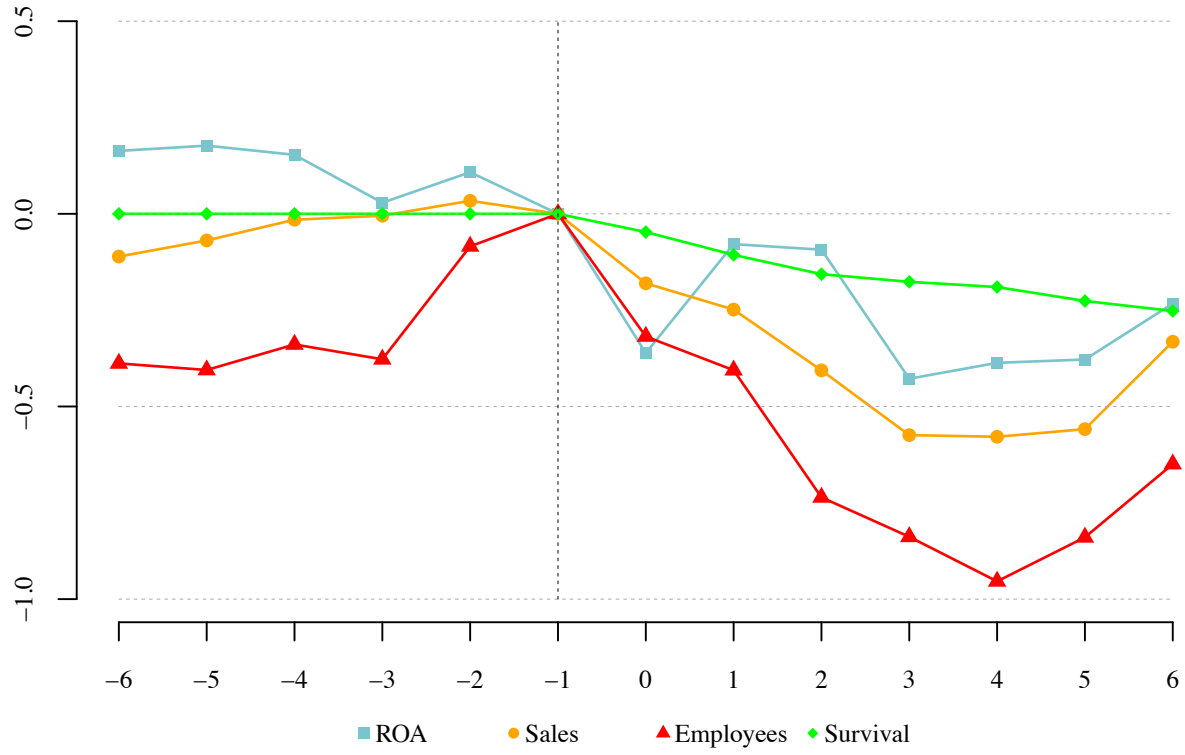

Figure II

Outcome Variables over Time for Firms Experiencing CEO Death

This figure shows the trend in the outcome variables (*ROA*, *Sales* and *Employees*) in the four-year period leading up to the death of the incumbent CEO. The blue lines represent firms that select into a certain successor category and the yellow lines those that do not. There are four successor categories, namely *CEO Experience*, *Firm Experience*, *Industry Experience* and *Family Succession*. *CEO Experience* takes the value one if the succeeding CEO has served as CEO of any other firm with more than 10 employees incorporated in Norway in the five-year period prior to the death of the incumbent CEO. *Firm Experience* takes the value one if the succeeding CEO has held a top position in the same firm in the five-year period prior to the death of the incumbent CEO. *Industry Experience* takes the value one if the succeeding CEO has held a top position in another firm with more than 10 employees and with the same industry classification in the five-year period prior to the death of the incumbent CEO. Lastly, *Family Succession* takes the value one if the succeeding CEO has the same surname as the incumbent CEO.

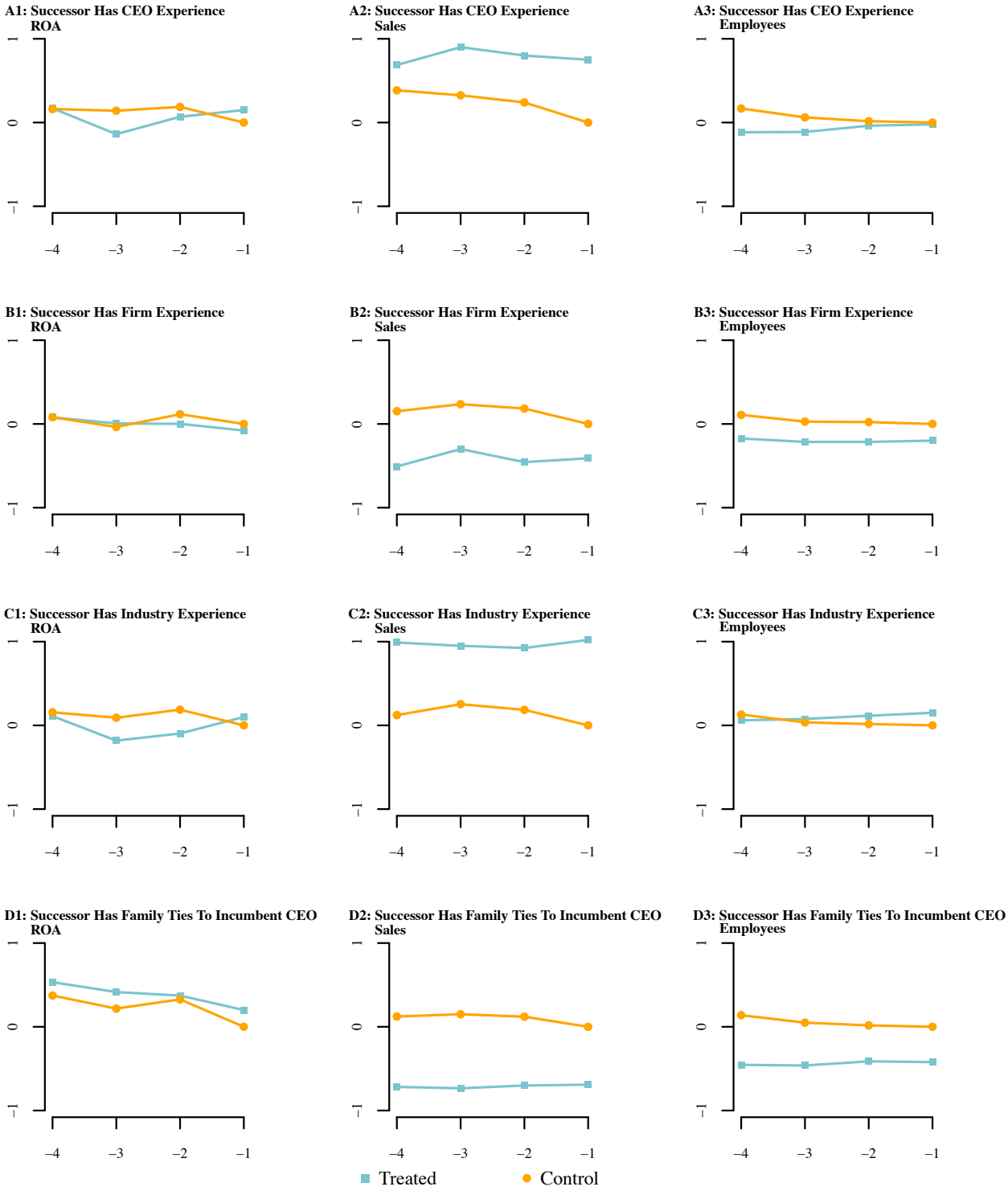

Table I
Variable Definitions

This table defines the variables used in the analysis and states their sources. The data combines information from 3 different registers. All information on corporate insiders as well as the accounting data comes from the *SNF: Norwegian Corporate Accounts* database. The ownership data comes from *Bisnode*. Lastly, information on CEO deaths come from *Brønnøysundregistrene*. The final sample comprises 381 firms observed over the period 1998-2014.

Name	Definition
A: Incumbent CEO Characteristics	
Age	The age of the CEO in years. Equals the current year minus the year of birth.
Director	Dummy that takes the value one if the CEO sits on the board.
Female	Dummy that takes the value one if the CEO is a woman.
Founder[1,2]	Dummy that takes the value one if the incumbent CEO is also the founder of the firm. A CEO is considered to be the founder if 1) her tenure stretches the entire pre-treatment period and 2) her average ownership stake in the period leading up to the event exceeds 50%. are present in the pre-treatment period.
Ownership[1]	The number of shares owned by the CEO divided by the total number of shares outstanding multiplied by 100. This measure takes into account both direct and indirect ownership.
CEO Experience[3]	Dummy that takes the value one if the succeeding CEO has served as CEO in any firm incorporated in Norway in the five-year period prior to the death of the incumbent CEO.
Industry Experience[3]	Dummy that takes the value one if the succeeding CEO has held a top position in another firm with the same industry classification in the five-year period prior to the death of the incumbent CEO. Industry classification equals *GICS 10 industry classification.*.
Firm Experience[3]	Dummy that takes the value one if the succeeding CEO has held a top position in the same firm in the five-year period prior to the death of the incumbent CEO.
Family Ties to the Incumbent CEO[3]	Dummy that takes the value one if the succeeding CEO has the same surname as the incumbent CEO.
B: Board Characteristics	
Board Ownership[1]	The number of shares owned collectively by the firm's director net of the CEO divided by the total number of shares outstanding multiplied by 100. This measure takes into account both direct and indirect ownership.
Board Busy	The average number of outside directorships held by the directors.
Board Size	The total number of directors.
D: Firm Characteristics	
Number of Employees	The number of employees in the end of the year.
Number of Shareholders	The total number of shareholder, including corporate insiders.
Industry	GICS 10 industry classification.
Largest Owner[1]	The ownership stake of the largest owner net of the CEO.
Leverage	The ratio of total liabilities and total assets.
Net Income	Net income as stated in the corporate accounts.
OROA	The ratio of earnings before interest and taxes to the book value of assets.
Sales	Total sales as stated in the corporate accounts.
Size	The natural logarithm of total assets.

[1]Source: *Bisnode*, all other variables were constructed using SNFs database *Norwegian Corporate Accounts*
[2]Only defined for incumbent CEOs
[3]Only defined for succeeding CEOs

Table II
Timing of CEO Death

This table shows the timing of the death of the incumbent CEO in terms of firm age, year of incorporation and year of treatment. t=0 equals the year of treatment. The sample comprises 381 firms observed over the period 1998-2014.

Firm Age (at t=0)			Year of Treatment		
Firm Age	Frequency	Percent	Year	Frequency	Percent
1	1	0.26	-	-	-
2	4	1.05	-	-	-
3	12	3.15	-	-	-
4	10	2.62	-	-	-
5	12	3.15	-	-	-
6	5	1.31	-	-	-
7	16	4.20	-	-	-
8	10	2.62	-	-	-
9	11	2.89	-	-	-
10	17	4.46	-	-	-
11	35	9.19	1999	6	1.57
12	33	8.66	2000	25	6.56
13	21	5.51	2001	25	6.56
14	20	5.25	2002	31	8.14
15	20	5.25	2003	17	4.46
16	27	7.09	2004	27	7.09
17	21	5.51	2005	19	4.99
18	14	3.67	2006	29	7.61
19	18	4.72	2007	18	4.72
20	10	2.62	2008	29	7.61
21	13	3.41	2009	26	6.82
22	17	4.46	2010	27	7.09
23	10	2.62	2011	27	7.09
24	10	2.62	2012	26	6.82
25	13	3.41	2013	25	6.56
26	1	0.26	2014	24	6.30
Total	381	100.00		381	100.00

Table III
Summary Statistics

This table shows summary statistics for the sample in the four-year window around the death of the incumbent CEO (from $t-2$ to $t+2$). The sample consists of firms that experienced CEO death sometime during the sample period. The timing of death spans a 16-year period (1999-2014). All variables are defined in Table I. The full sample comprises 381 private firms with more than 10 employees in the each of the two year preceding the death of the incumbent CEO. *CEO Age* and *Firm Age* are denominated in years. *Board Size*, *Number of Shareholders* and *Number of Employees* are denominated in numbers. *Assets*, *Sales* and *Net Income* are denominated in million NOK. All other variables are stated as either percentages or dummies.

	Obs.	Min.	25^{th}	Med.	Mean	75^{th}	Max.
I: CEO Characteristics							
Age (Y)	1331	20.00	44.00	53.00	52.50	60.00	90.00
Incumbent Age (Y)	755	27.00	51.00	58.00	57.46	63.00	90.00
Successor Age (Y)	576	20.00	40.00	45.00	45.98	52.00	69.00
Chairman (Dummy)	1351	0.00	0.00	0.00	0.35	1.00	1.00
Directorship (Dummy)	1351	0.00	0.00	1.00	0.69	1.00	1.00
Female (Dummy)	1331	0.00	0.00	0.00	0.11	0.00	1.00
Ownership (%)	1199	0.00	0.00	15.00	28.28	50.00	100.00
Incumbent Ownership	648	0.00	0.00	33.33	36.64	60.00	100.00
Successor Ownership	551	0.00	0.00	0.00	18.44	32.65	100.00
Incumbent CEO Founder	755	0.00	0.00	0.00	0.30	1.00	1.00
Successor has CEO Experience	578	0.00	0.00	0.00	0.39	1.00	1.00
Successor has Firm Experience	578	0.00	0.00	0.00	0.48	1.00	1.00
Successor has Industry Experience	578	0.00	0.00	0.00	0.29	0.00	1.00
Successor has Family Ties to the Incumbent CEO	578	0.00	0.00	0.00	0.38	1.00	1.00
II: Board and Shareholder Characteristics							
Number of Shareholders	927	1.00	1.00	2.00	15.69	5.00	1715.00
Largest Owner	927	1.21	34.00	54.71	63.20	100.00	100.00
HHI	927	0.23	33.04	50.00	59.44	100.00	100.00
Board Ownership	1354	0.00	0.00	0.00	26.66	50.00	100.00
Board Size	1331	0.00	2.00	3.00	2.96	4.00	9.00
Board Busy	1354	0.00	0.00	0.00	0.77	0.75	32.33
III: Firm Variables							
Firm Age	1354	0.00	9.00	13.00	13.37	18.00	26.00
Number of Employees	1354	0.00	12.00	17.00	33.49	30.00	1197.00
Assets	1327	0.04	5.41	10.91	72.88	25.43	7977.83
Sales	1327	0.00	11.20	20.72	72.83	46.29	3701.46
Net Income	1327	−71.59	0.05	0.60	3.80	1.92	1058.30
Leverage	1327	0.63	55.71	72.76	71.27	85.78	354.51
ROA	1327	−211.49	0.71	6.04	6.57	13.29	409.54

Table IV
The Effect of CEO Death on Firm Performance

This table shows point estimates from the following model

$$Outcome_{it} = \alpha + \beta_1 After_t + \lambda_i + \delta X_{it} + \lambda_i + \varepsilon_{it}$$

The outcome variable is either *OROA*, *log(Sales + 1)*, *log(Employees + 1)* or *Survival*. *After* is an indicator that is equal to one for post-treatment observations, zero otherwise. All regressions control for *log(Assets + 1)* and *Leverage*, *CEO Age*, *CEO Gender*, *CEO Director*, *Board Size* and *Board Ownership*. All regressions also include firm fixed effects. Standard errors are clustered at the firm level. For the main results in panel A, the sample period is restricted to period -4:2. Year zero is excluded since it is not clearly attributable to either the pre- or post-period. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Sample Restricted to t=-4/+2. Results Conditional on Survival Until t=+2.

	ROA	log(Sales + 1)	log(Employees + 1)	Survival
	(1)	(2)	(3)	(4)
After	−5.04***	−0.61**	−0.19***	−0.13***
	(0.00)	(0.05)	(0.00)	(0.00)
Additional Controls	Yes	Yes	Yes	No
Firm FEs	Yes	Yes	Yes	Yes
Adj. R^2	8.93%	7.83%	19.88%	12.31%
Observations	[1537]	[1536]	[1537]	[2083]

B: Alternative Estimation Windows & Conditional Outcome Distributions

	ROA	log(Sales + 1)	log(Employees + 1)	Survival
	(1)	(2)	(3)	(4)
I: t=-4 to t=+4. Results Conditional on Survival Until t=+2.				
After	−7.13***	−0.90***	−0.29***	−0.15***
	(0.00)	(0.00)	(0.00)	(0.00)
II: t=-4 to t=+4. Results Conditional on Survival Until t=+4.				
After	−7.49***	−1.03***	−0.32***	-
	(0.00)	(0.00)	(0.00)	-
III: t=-6 to t=+6. Results Conditional on Survival Until t=+2.				
After	−7.87***	−1.07***	−0.34***	−0.17***
	(0.00)	(0.00)	(0.00)	(0.00)
IV: t=-6 to t=+6. Results Conditional on Survival Until t=+6.				
After	−5.88***	−1.60***	−0.40***	-
	(0.00)	(0.00)	(0.00)	-

C: Duration of the Effect. (t=-6 to t=+6. Results Conditional on Survival Until t=+6.)

	ROA	log(Sales + 1)	log(Employees + 1)	Survival
	(1)	(2)	(3)	(4)
$After_{+5}$	0.04	0.28*	0.00	−0.02**
	(0.97)	(0.10)	(0.94)	(0.02)
$After_{+4}$	1.51	−0.02	−0.05**	−0.03***
	(0.26)	(0.85)	(0.04)	(0.00)
$After_{+3}$	−2.15**	0.17	−0.03	−0.02
	(0.04)	(0.42)	(0.25)	(0.14)
$After_{+2}$	−2.40**	−0.33**	−0.06*	−0.02**
	(0.01)	(0.04)	(0.08)	(0.04)
$After_{+1}$	−0.62	−0.27	−0.09**	−0.05***
	(0.62)	(0.24)	(0.02)	(0.00)
After	−3.18**	−1.29***	−0.25***	−0.10***
	(0.02)	(0.00)	(0.01)	(0.00)
Additional Controls	Yes	Yes	Yes	No
Firm FEs	Yes	Yes	Yes	Yes
Adj. R^2	6.86%	19.13%	27.62%	16.61%
Observations	[1715]	[1714]	[1715]	[3641]

Table IV Continued from Previous Page

D: Pre-Treatment Effects (t=-8 to t=-1. "After" takes the value 1 in the period t=-4 to t=-1)

	ROA	log(Sales + 1)	log(Employees + 1)	Survival
	(1)	(2)	(3)	(4)
After	−0.83	0.07	0.00	-
	(0.43)	(0.59)	(0.94)	-
Additional Controls	Yes	Yes	Yes	-
Firm FEs	Yes	Yes	Yes	-
Adj. R^2	8.41%	9.57%	22.55%	-
Observations	[2241]	[2241]	[2241]	-

Table V
Interactions: Incumbent CEO Traits and Firm Characteristics

This table shows coefficient estimates from the following model

$$Outcome_{it} = \alpha + \beta_1 Treated_i \times After_t + \beta_2 After_t + \beta_3 Treated_i + \lambda_i + \delta X_{it} + \varepsilon_{it}$$

, where *Outcome* is either *ROA*, *log(Sales + 1)*, *log(Employees + 1)* or *Survival*. *Treated* is a dummy that equals one if the firm is characterized in t=-1 by I) the incumbent CEO is also the founder, II) the incumbent CEO is above median in terms of age, III.A-III.B) the firm is above median in terms of size (measured either by total assets or by the number of employees), IV) the firm is above median in terms of firm age and V.A-V.B) the firm performs better than the median firm (measured either by the level of OROA in t=-1 or as the change in OROA from t=-4 to t=-1). *After* is a dummy that equals one in the two periods following CEO death and zero otherwise, λ is a set of firm fixed effects and X is a matrix of control variables. The estimation window is from t=-4 to t=+2. The year of death (t=0) has been excluded since it is not clearly attributable to either the pre- or post-period. For variable definitions, see Table I. P-values are shown in parentheses. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	ROA (1)	log(Sales + 1) (2)	log(Employees + 1) (3)	Survival (4)
I: Founder vs. Non-Founder Firms				
Founder × After	0.38	−0.70	−0.10	0.05
	(0.87)	(0.11)	(0.28)	(0.14)
After	−3.95***	−0.48**	−0.16***	−0.14***
	(0.00)	(0.04)	(0.00)	(0.00)
Adj. R^2	10.93%	8.45%	19.04%	12.71%
Observations	1557	1556	1557	2093
II: Old vs. Young Incumbents				
Old Incumbent × After	2.90	−0.43	−0.13	0.03
	(0.11)	(0.25)	(0.12)	(0.39)
After	−5.31***	−0.46*	−0.12*	−0.14***
	(0.00)	(0.06)	(0.05)	(0.00)
Adj. R^2	11.23%	8.19%	19.32%	12.46%
Observations	1557	1556	1557	2093
III.A: Large vs. Small Firms (measured by total assets)				
Large Firm × After	−1.85	0.15	0.05	0.01
	(0.33)	(0.69)	(0.56)	(0.86)
After	−2.93*	−0.74***	−0.21***	−0.13***
	(0.07)	(0.00)	(0.00)	(0.00)
Adj. R^2	11.07%	8.00%	18.91%	11.92%
Observations	1546	1545	1546	2051
III.B: Large vs. Small Firms (measured by the number of employees)				
Large Firm × After	1.49	0.04	−0.03	0.02
	(0.42)	(0.92)	(0.75)	(0.54)
After	−4.59***	−0.70***	−0.17***	−0.14***
	(0.00)	(0.01)	(0.00)	(0.00)
Adj. R^2	11.00%	7.97%	18.84%	12.38%
Observations	1557	1556	1557	2093
IV: Old vs. Young Firms				
Old Firm × After	−0.40	−0.22	−0.06	0.02
	(0.83)	(0.56)	(0.45)	(0.63)
After	−3.64**	−0.57**	−0.15**	−0.14***
	(0.01)	(0.05)	(0.02)	(0.00)
Adj. R^2	10.93%	8.02%	18.94%	12.35%
Observations	1557	1556	1557	2093

Table V Continued from Previous Page

V.A: Strong vs. Weak Performance (defined as being above median in the change in ROA from t=-4 to t=-1)				
Strong Performers × After	0.13	0.41	0.12	0.11***
	(0.94)	(0.28)	(0.14)	(0.00)
After	−3.74**	−0.89***	−0.25***	−0.18***
	(0.02)	(0.00)	(0.00)	(0.00)
Adj. R^2	10.00%	8.30%	20.04%	14.62%
Observations	1533	1532	1533	2062
V.B: Strong vs. Weak Performance (measured by the level of ROA)				
Strong Performers × After	−8.76***	0.07	−0.03	0.09***
	(0.00)	(0.86)	(0.73)	(0.01)
After	0.55	−0.70***	−0.18***	−0.17***
	(0.65)	(0.01)	(0.01)	(0.00)
Adj. R^2	12.87%	8.08%	19.32%	13.51%
Observations	1540	1539	1540	2041

Table VI
Determinants of Successor Type

This table shows coefficient estimates from logit regressions of successor characteristics on CEO, board and firm characteristics at $t = -1$. The dependent variable is one of four dummy variables; *CEO Experience*, *Insider*, *Family Ties* or *Industry Experience*. *CEO Experience* takes the value one if the succeeding CEO has served as CEO of any other firm with more than 10 employees incorporated in Norway in the five-year period prior to the death of the incumbent CEO. *Firm Experience* takes the value one if the succeeding CEO has held a top position in the same firm in the five-year period prior to the death of the incumbent CEO. *Industry Experience* takes the value one if the succeeding CEO has held a top position in another firm with more than 10 employees and with the same industry classification in the five-year period prior to the death of the incumbent CEO. Lastly, *Family Succession* takes the value one if the succeeding CEO has the same surname as the incumbent CEO. For other variable definitions, see Table I. All regressions include industry- and year of treatment-fixed effects. The timing of death spans a 16-year period (1999-2014). Δ denotes changes from $t = -2$ to $t = -1$. "% Change" denotes the change from $t = -2$ to $t = -1$, divided by the value at $t = -2$. P-values are shown in parentheses. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	Successor has CEO Experience	Successor has Firm Experience	Successor has Industry Experience	Successor has Family Ties to the Incumbent CEO
Incumbent CEO and Board Characteristics				
CEO Age	0.00	0.03	0.01	0.06***
	(0.95)	(0.13)	(0.80)	(0.00)
CEO Chair	−0.25	−0.22	−0.22	0.92**
	(0.51)	(0.61)	(0.65)	(0.03)
CEO Director	−0.23	1.56***	−1.12	1.34**
	(0.60)	(0.00)	(0.82)	(0.03)
CEO Founder	−0.35	0.51	−1.57***	−0.36
	(0.43)	(0.31)	(0.01)	(0.44)
CEO Female	1.03*	−0.47	0.01	−0.73
	(0.09)	(0.53)	(0.99)	(0.33)
CEO Ownership	0.01	0.00	0.01	0.02**
	(0.19)	(0.92)	(0.15)	(0.03)
Busy Board	0.06	−0.14	0.03	−0.25
	(0.30)	(0.19)	(0.60)	(0.35)
Board Ownership	0.01	0.03***	0.00	0.02**
	(0.21)	(0.00)	(0.78)	(0.02)
Board Size	−0.10	0.52***	−0.10	−0.15
	(0.42)	(0.00)	(0.53)	(0.36)
Firm Characteristics				
ROA	0.00	0.00	0.01	−0.03*
	(0.77)	(0.89)	(0.58)	(0.06)
ΔROA	0.01	0.02	0.04**	0.02
	(0.50)	(0.20)	(0.02)	(0.13)
log(Sales + 1)	−0.04	−0.06	0.00	−0.06
	(0.48)	(0.38)	(0.95)	(0.48)
% Change in Sales	0.01	0.02*	−0.01	0.01*
	(0.26)	(0.06)	(0.34)	(0.08)
log(Assets + 1)	0.36***	−0.32**	0.34**	−0.34*
	(0.01)	(0.05)	(0.03)	(0.10)
% Change in Assets	−0.01*	−0.02***	−0.01	0.01
	(0.06)	(0.01)	(0.39)	(0.25)
Leverage	0.00	0.00	0.02*	−0.01
	(0.92)	(0.94)	(0.06)	(0.14)
ΔLeverage	−0.01	0.00	0.01	−0.03**
	(0.69)	(0.84)	(0.60)	(0.04)
Firm Age	−0.52	0.03	−0.60	0.28
	(0.10)	(0.93)	(0.12)	(0.46)
Industry FEs	Yes	Yes	Yes	Yes
Year of Treatment FEs	Yes	Yes	Yes	Yes
Pseudo-R^2	10.34%	30.57%	18.94%	30.96%
Observations	[278]	[278]	[278]	[278]

Table VII

The Effects of Successor Types Around Transition

This table shows coefficient estimates of the following model

$$Outcome_{it} = \alpha + \beta_1 Treated_i \times After_t + \beta_2 After_t + \beta_3 Treated_i + \lambda_i + \delta X_{it} + \varepsilon_{it}$$

, where *Outcome* is either *ROA*, *log(Sales + 1)*, *log(Employees + 1)* or *Survival*. *Treated* is a dummy that equals one if the successor has A) *CEO Experience* B) *Firm Experience* C) *Industry Experience* or D) *Family Ties to the Incumbent CEO (Family Succession)*. *After* is a dummy that equals one in the two periods following CEO death and zero otherwise, λ is a set of firm fixed effects and X is a matrix of control variables. *CEO Experience* takes the value one if the succeeding CEO has served as CEO of any other firm with more than 10 employees incorporated in Norway in the five-year period prior to the death of the incumbent CEO. *Firm Experience* takes the value one if the succeeding CEO has held a top position in the same firm in the five-year period prior to the death of the incumbent CEO. *Industry Experience* takes the value one if the succeeding CEO has held a top position in another firm with more than 10 employees and with the same industry classification in the five-year period prior to the death of the incumbent CEO. Lastly, *Family Succession* takes the value one if the succeeding CEO has the same surname as the incumbent CEO. Each panel (A-D) includes two sub-panels. The first one (I) reports the difference in the outcome variable in the pre-treatment period, whereas the second (II) shows the main results around CEO transition. The estimation window is from t=-4 to t=+2. The year of death (t=0) has been excluded since it is not clearly attributable to either the pre- or post-period. For other variable definitions, see Table I. P-values are shown in parentheses. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Successor Has CEO Experience

	ROA (1)	log(Sales + 1) (2)	log(Employees + 1) (3)	Survival (4)
I: Pre-Treatment Trends in the Outcome				
Outcome.Treat$_{t=-1}$ - Outcome.Treat$_{t=-4}$	0.06	−35.16	1.20	-
	(0.98)	(0.62)	(0.42)	-
Outcome.Cont$_{t=-1}$ - Outcome.Cont$_{t=-4}$	−1.39	−11.78	1.32	-
	(0.32)	(0.54)	(0.17)	-
Difference in Differences	1.46	−23.38	−0.12	-
	(0.57)	(0.75)	(0.95)	-
II: Main Effects				
CEO Experience × After	−1.31	−0.26	−0.02	−0.06*
	(0.53)	(0.63)	(0.88)	(0.06)
After	−2.70**	−0.59***	−0.17***	−0.04***
	(0.01)	(0.00)	(0.00)	(0.00)
Adj. R^2	6.16%	7.72%	17.53%	6.04%
N	272	272	272	335

B: Successor Has Firm Experience

	ROA (1)	log(Sales + 1) (2)	log(Employees + 1) (3)	Survival (4)
I: Pre-Treatment Trends in the Outcome				
Outcome.Treat$_{t=-1}$ - Outcome.Treat$_{t=-4}$	−0.56	−29.51	1.25	-
	(0.71)	(0.49)	(0.23)	-
Outcome.Cont$_{t=-1}$ - Outcome.Cont$_{t=-4}$	−1.29	−10.35	1.31	-
	(0.44)	(0.71)	(0.26)	-
Difference in Differences	0.73	−19.15	−0.06	-
	(0.75)	(0.70)	(0.97)	-
II: Main Effects				
Firm Experience × After	−0.39	−0.19	0.00	−0.01
	(0.52)	(0.41)	(0.94)	(0.32)
After	−2.83***	−0.55**	−0.18***	−0.05***
	(0.01)	(0.01)	(0.00)	(0.00)
Adj. R^2	6.15%	7.98%	17.53%	5.20%
N	272	272	272	332

Table VII Continued from Previous Page

C: Successor Has Industry Experience

	ROA	log(Sales + 1)	log(Employees + 1)	Survival
	(1)	(2)	(3)	(4)
I: Pre-Treatment Trends in the Outcome				
Outcome.Treat$_{t=-1}$ - Outcome.Treat$_{t=-4}$	0.73	−95.02	1.65	-
	(0.81)	(0.32)	(0.38)	-
Outcome.Cont$_{t=-1}$ - Outcome.Cont$_{t=-4}$	−1.39	−0.76	1.21	-
	(0.27)	(0.97)	(0.18)	-
Difference in Differences	2.11	−94.26	0.44	-
	(0.52)	(0.34)	(0.83)	-
II: Main Effects				
Industry Experience × After	−0.33	−0.88	0.00	0.02
	(0.87)	(0.19)	(0.97)	(0.45)
After	−3.00***	−0.51**	−0.18***	−0.06***
	(0.01)	(0.02)	(0.00)	(0.00)
Adj. R^2	6.11%	8.21%	17.53%	5.13%
N	272	272	272	332

D: Successor Has Family Ties to the Incumbent CEO

	ROA	log(Sales + 1)	log(Employees + 1)	Survival
	(1)	(2)	(3)	(4)
I: Pre-Treatment Trends in the Outcome				
Outcome.Treat$_{t=-1}$ - Outcome.Treat$_{t=-4}$	−1.10	3.23*	1.42*	-
	(0.59)	(0.06)	(0.08)	-
Outcome.Cont$_{t=-1}$ - Outcome.Cont$_{t=-4}$	−0.94	−30.27	1.21	-
	(0.51)	(0.42)	(0.30)	-
Difference in Differences	−0.16	33.50	0.21	-
	(0.95)	(0.37)	(0.88)	-
II: Main Effects				
Family Ties × After	−4.06**	−0.36	−0.06	0.09***
	(0.04)	(0.36)	(0.51)	(0.00)
After	−1.62	−0.54**	−0.16***	−0.13***
	(0.15)	(0.03)	(0.01)	(0.00)
Adj. R^2	6.70%	7.80%	17.61%	11.51%
N	272	272	272	350

Table VIII

Comparing Characteristics in the Period Before Treatment

This table describes the trends in the observable characteristics of the firm in the four-year period leading up the the death of the incumbent CEO. Treated firm are those that subsequently select into a certain successor category. Death occurs at t=0. For variable definitions, see Table I. P-values are shown in parentheses. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	CEO Experience			Firm Experience			Industry Experience			Family Experience		
	t_{-1} (1)	t_{-4} (2)	$[t_{-1}-t_{-4}]$ (3)	t_{-1} (4)	t_{-4} (5)	$[t_{-1}-t_{-4}]$ (6)	t_{-1} (7)	t_{-4} (8)	$[t_{-1}-t_{-4}]$ (9)	t_{-1} (10)	t_{-4} (11)	$[t_{-1}-t_{-4}]$ (12)
I: Ownership Characteristics												
Number of Shareholders$_{Treat.-Cont.}$	−19.26*	−32.02*	8.68	−4.60	−4.12	0.60	−16.13*	−23.98	2.79	−18.87*	−31.43*	7.59
	(0.08)	(0.06)	(0.34)	(0.75)	(0.86)	(0.95)	(0.08)	(0.10)	(0.73)	(0.08)	(0.06)	(0.39)
HHI$_{Treat.-Cont.}$	0.09	0.13*	−0.08	−0.07	0.02	−0.06	0.06	0.10	0.01	−0.02	0.09	−0.09*
	(0.17)	(0.08)	(0.37)	(0.13)	(0.75)	(0.15)	(0.34)	(0.16)	(0.87)	(0.72)	(0.11)	(0.06)
II: CEO Characteristics												
CEO Ownership$_{Treat.-Cont.}$	−0.41	0.86	−0.09	−2.73	2.64	−5.63*	−1.75	−6.70	5.73	18.14***	22.88***	−5.20
	(0.94)	(0.89)	(0.98)	(0.50)	(0.52)	(0.06)	(0.74)	(0.19)	(0.22)	(0.00)	(0.00)	(0.11)
CEO Age$_{Treat.-Cont.}$	−1.20	−0.93	−0.27	3.34***	3.26***	0.08	−0.14	0.23	−0.38	5.87***	5.77***	0.10
	(0.37)	(0.50)	(0.63)	(0.00)	(0.00)	(0.83)	(0.90)	(0.85)	(0.42)	(0.00)	(0.00)	(0.77)
CEO Director$_{Treat.-Cont.}$	−0.05	−0.03	−0.03	0.15***	0.13***	0.02	0.02	−0.01	0.03	0.28***	0.29***	−0.01
	(0.46)	(0.66)	(0.25)	(0.00)	(0.01)	(0.38)	(0.74)	(0.88)	(0.42)	(0.00)	(0.00)	(0.59)
CEO Chair$_{Treat.-Cont.}$	−0.10	−0.09	0.00	−0.02	0.01	−0.03	−0.06	−0.07	0.00	0.36***	0.35***	0.02
	(0.22)	(0.27)	(0.94)	(0.66)	(0.85)	(0.31)	(0.36)	(0.29)	(0.92)	(0.00)	(0.00)	(0.57)
CEO Founder$_{Treat.-Cont.}$												
III: Board Characteristics												
Board Ownership$_{Treat.-Cont.}$	4.51	−0.81	5.33	22.61***	20.19***	2.42	0.71	0.46	0.25	3.74	1.57	2.16
	(0.40)	(0.87)	(0.21)	(0.00)	(0.00)	(0.34)	(0.88)	(0.92)	(0.94)	(0.30)	(0.64)	(0.32)
Board Size$_{Treat.-Cont.}$	−0.14	−0.20	−0.02	0.38**	0.25	0.06	0.25	0.40*	−0.20	−0.85***	−0.84***	−0.03
	(0.47)	(0.38)	(0.88)	(0.02)	(0.17)	(0.50)	(0.27)	(0.09)	(0.12)	(0.00)	(0.00)	(0.78)
IV: Firm Characteristics												
Credit Rating$_{Treat.-Cont.}$	−0.11	−0.30	0.21	−0.24	−0.11	−0.10	−0.31*	−0.45*	0.10	−0.16	0.10	−0.33
	(0.59)	(0.16)	(0.46)	(0.15)	(0.51)	(0.65)	(0.09)	(0.05)	(0.64)	(0.41)	(0.54)	(0.21)
Employees$_{Treat.-Cont.}$	−8.80*	−10.76	1.96	−7.49	−10.10	2.61	5.19	2.07	3.12	−16.39***	−19.69***	3.30
	(0.09)	(0.13)	(0.57)	(0.22)	(0.23)	(0.47)	(0.52)	(0.83)	(0.29)	(0.00)	(0.00)	(0.28)
Firm Age$_{Treat.-Cont.}$	−0.51	−0.64	0.13	−0.30	−0.22	−0.08	−0.79	−0.71	−0.08	0.82	0.85	−0.03
	(0.56)	(0.45)	(0.18)	(0.65)	(0.73)	(0.41)	(0.38)	(0.41)	(0.44)	(0.21)	(0.18)	(0.78)
Assets$_{Treat.-Cont.}$	1.68	6.38	−5.05	−62.68	−53.90	−9.42	164.43	157.54	6.43	−91.30***	−87.04***	−3.79
	(0.92)	(0.73)	(0.37)	(0.13)	(0.16)	(0.37)	(0.18)	(0.15)	(0.78)	(0.01)	(0.00)	(0.62)
Sales$_{Treat.-Cont.}$	20.20	25.70	−5.39	−34.71*	−18.19	−16.92	58.98	129.06	−70.18	−62.35***	−85.55***	24.44
	(0.46)	(0.58)	(0.83)	(0.09)	(0.66)	(0.68)	(0.14)	(0.13)	(0.37)	(0.00)	(0.00)	(0.39)
Net Income$_{Treat.-Cont.}$	4.41	1.20	3.19	−6.84**	−1.96	−4.95*	8.26	5.41	2.81	−4.93*	−3.63***	−1.30

Table VIII Continued from Previous Page

	(0.18)	(0.40)	(0.16)	(0.04)	(0.24)	(0.08)	(0.39)	(0.14)	(0.72)	(0.07)	(0.00)	(0.55)
$\text{Leverage}_{Treat.-Cont.}$	1.09	−3.31	4.63	0.11	−2.45	2.96	5.50*	1.12	4.70	−2.32	0.03	−2.54
	(0.73)	(0.27)	(0.15)	(0.96)	(0.33)	(0.22)	(0.06)	(0.72)	(0.14)	(0.38)	(0.99)	(0.35)
$\text{ROA}_{Treat.-Cont.}$	−0.76	−0.07	−0.70	−1.22	−0.96	−0.28	1.11	−1.51	2.59	3.28	1.46	1.74
	(0.76)	(0.97)	(0.78)	(0.54)	(0.52)	(0.89)	(0.69)	(0.45)	(0.36)	(0.15)	(0.36)	(0.48)

Table IX
Higher Order Interactions

This table shows coefficient estimates of the following specification

$$Outcome_{it} = \alpha + \beta_1 D_i \times Treated_i \times After_t + \beta_2 Treated_i \times After_t + \beta_3 D_i \times After_t + \beta_4 After_t + \lambda_i + \delta X_{it} + \varepsilon_{it}$$

, where $Outcome_{it}$ is a measure of either profitability, growth or survival ,D_i represents a set of pre-existing firm characteristics and incumbent CEO traits, $Treated_i$ denotes a set of successor characteristics (*CEO Experience*, *Firm Experience*, *Industry Experience* and *Family Successioon*), *After* is a dummy that equals one in the two periods following CEO death and zero otherwise, λ_i is a vector of firm fixed effects and X is a matrix of control variables. *Outcome* is either *OROA*, *log(Sales + 1)*, *log(Employees + 1)* or *Survival* (dummy that takes the value zero when the firm is discontinued and no longer appears in the source data). Firms are categorized as *Large Firms* when their assets in period t=-1 is above the sample median. Firms are classified as *Founder Firms* if the incumbent CEO is also the founder. *Old Incumbent* and *Old Firm* are defined as above sample median age at t=-1. Lastly, *Strong Performance* is defined as having above median change in OROA between t=-4 and t=-1. Standard errors are clustered at the firm level. The sample period is restricted to period -4:2. Year zero is excluded since it is not clearly attributable to either the pre- or post-period. P-values are shown in parentheses. The number of observations are shown in brackets. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

A: Descriptives

	Firms in Category		Successor has CEO Experience		Successor has Firm Experience		Successor has Industry Experience		Successor has Family Ties to the Incumbent CEO	
	N	%	N	%	N	%	N	%	N	%
Large Firms	138	49.27%	45	32.61%	48	34.78%	31	22.46%	39	28.26%
Founder Firms	79	28.21%	21	26.58%	31	39.24%	12	15.19%	37	46.84%
Old Incumbent	137	48.93%	42	30.66%	64	46.72%	26	18.98%	67	48.91%
Old Firm	130	46.43%	38	29.23%	49	37.69%	20	15.38%	50	38.46%
Weak Performers	134	48.91%	39	29.10%	51	38.06%	24	17.91%	45	33.58%

B: Regressional Output

Treatment Var. =	Successor has CEO Experience				Successor has Firm Experience				Successor has Industry Experience				Successor has Family Ties to the Incumbent CEO			
	ROA (1)	Sal. (2)	Emp. (3)	Sur. (4)	ROA (5)	Sal. (6)	Emp. (7)	Sur. (8)	ROA (9)	Sal. (10)	Emp. (11)	Sur. (12)	ROA (13)	Sal. (14)	Emp. (15)	Sur. (16)
I: Large Firms																
D × Treated × After	4.72	0.53	0.05	−0.02	0.49	−0.62	−0.30*	−0.09*	3.45	0.57	0.01	0.01	7.81*	−0.34	−0.17	0.00
	(0.31)	(0.60)	(0.83)	(0.75)	(0.89)	(0.42)	(0.08)	(0.08)	(0.43)	(0.67)	(0.98)	(0.85)	(0.06)	(0.68)	(0.35)	(0.98)
Adj. R^2	5.76%	7.96%	18.10%	6.16%	5.56%	8.00%	18.68%	5.94%	5.62%	8.47%	18.08%	5.12%	6.88%	7.94%	18.31%	11.49%
Obs.	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]
II: Founder Firms																
D × Treated × After	−7.89	−0.27	−0.13	−0.06	−4.16	0.98	0.36**	0.08*	−3.68	0.29	0.32	0.05	−1.86	−1.90**	−0.10	0.05
	(0.17)	(0.83)	(0.58)	(0.42)	(0.34)	(0.27)	(0.03)	(0.06)	(0.49)	(0.86)	(0.18)	(0.28)	(0.70)	(0.03)	(0.57)	(0.48)
Adj. R^2	5.86%	8.13%	18.15%	6.30%	5.55%	8.39%	18.81%	5.77%	5.48%	8.67%	18.38%	5.21%	6.16%	8.99%	18.17%	11.57%
Obs.	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]

Table IX Continued from Previous Page

III: Strong Performance (defined as being above median in the change in OROA from t=-4 to t=-1)																
D × Treated × After	6.28	1.12	−0.18	−0.09	−2.31	−0.25	−0.05	−0.03	−4.39	−1.30	−0.53	0.00	−1.26	−0.22	0.10	−0.13
	(0.12)	(0.31)	(0.42)	(0.15)	(0.52)	(0.76)	(0.77)	(0.55)	(0.27)	(0.34)	(0.11)	(0.94)	(0.76)	(0.79)	(0.58)	(0.11)
Adj. R^2	5.55%	8.36%	19.00%	6.82%	5.26%	8.16%	18.86%	5.36%	5.31%	8.88%	19.97%	5.21%	5.85%	8.19%	19.01%	13.97%
Obs.	[1495]	[1494]	[1495]	[1815]	[1495]	[1494]	[1495]	[1815]	[1495]	[1494]	[1495]	[1815]	[1495]	[1494]	[1495]	[1815]
IV: Firm Age																
D × Treated × After	1.98	0.14	0.11	−0.01	−9.59***	0.29	−0.06	−0.06	1.08	1.47	0.15	−0.06	1.57	−2.04***	−0.27	0.06
	(0.64)	(0.90)	(0.62)	(0.87)	(0.01)	(0.70)	(0.71)	(0.17)	(0.78)	(0.25)	(0.56)	(0.34)	(0.70)	(0.01)	(0.11)	(0.30)
Adj. R^2	5.49%	7.80%	18.06%	6.51%	6.26%	7.87%	18.04%	6.01%	5.41%	8.67%	18.08%	5.74%	6.10%	9.01%	18.53%	0.12%
Obs.	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]
V: Old Incumbent																
D × Treated × After	−6.97*	−0.49	−0.13	−0.01	−3.67	0.50	0.09	−0.03	−8.12**	−0.58	−0.45*	−0.01	4.32	−1.66**	−0.18	−0.02
	(0.08)	(0.64)	(0.55)	(0.86)	(0.31)	(0.52)	(0.59)	(0.57)	(0.04)	(0.66)	(0.08)	(0.90)	(0.31)	(0.02)	(0.32)	(0.77)
Adj. R^2	6.06%	8.12%	18.56%	6.19%	5.74%	8.16%	18.52%	5.29%	6.00%	8.61%	19.33%	5.17%	6.74%	8.73%	18.66%	11.69%
Obs.	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]	[1513]	[1512]	[1513]	[1833]

Appendix A: Matching

The matching procedure described below aims to form a control group that is similar to the treatment group in terms of the trend in the outcome in the period leading up to the event. This approach broadly follows that of Azoulay, Graff Zivin and Wang (2010) and Jenter, Matveyev and Roth (2016) and serves two purposes. First, it enables me to impute the year of treatment for the control group and second, it helps minimize *ex ante* differences between the two groups. From the point of view of identification, a substantial advantage of panel data is that matching based on conditioning on pre-treatment outcomes is feasible. This is an important issue because it appears to be a natural requirement for a "good" control group to have similar pre-treatment means of the outcome variables (since it is likely that pre-treatment outcomes are correlated with post-treatment outcomes as well as selection, either directly, or because the unobservables that influence those three quantities are correlated). This approach is not possible with repeated cross-sections, since pre- and post-treatment observations for the same individuals is not observed.

The matching procedure outlined below puts emphasis on ensuring that treated firms exhibit no differential outcome trends relative to control firms up to the passing of the incumbent CEO. Matching on the pre-shock trend should implicitly account for the determinants of the outcome as well. In order for matching to be effective, the pool of control firms from which to draw the matches needs to be considerably larger than the sample of treated firms. Since my treatment group constitutes only about 0.1% of all active firms in Norway, matching is feasible. I use propensity score matching to identify firms in the control group that are similar to treated firms in terms of pre-treatment levels and trends in the outcome. I use nearest neighbor matching with a matching ratio of one without replacement, which generates treatment and control groups of equal sizes. Treatment and control firms are matched one year prior to the event. Ideally, matching should be performed well in advance of treatment, so as to minimize the risk of program anticipation, or alternatively use variables that are time-invariant, if applicable. There are two reason for matching one year prior to the event. First, having already established that the treatment group does not exhibit any significant pre-treatment movements in the outcome, program anticipation is unlikely to be a problem. Second, matching close to the event has the effect of preserving the sample size as treatment and control firms need only to have been active three periods prior to the event. Going further back in time would require exclusion of "young" firms. As such, the timing of the matching represents a trade-off between minimizing the probability of program anticipation and maximizing the sample size.

I start by identifying the year of treatment as well as the year of incorporation for all firms that experience the passing of an incumbent CEO sometime during the sample period. I then run a logit regression with the treatment dummy as the dependent variable on a matching vector X, where X contains the following variables; *Year of Incorporation*, *Firm Age*, *Industry* (10 industry classification), $Outcome_{t-1}$, $\Delta Outcome_{t-1}$ and $\Delta Outcome_{t-2}$. I require exact matching on both *Year of Incorporation*, *Firm Age* and *Industry*, which enables me to impute the year of treatment for the matched sample. For example, if a firm was incorporated in 2000 and experienced a CEO death in 2004, the matched firm would have to have output data available for 2001, 2002 and 2003. In 2003 the matched firm would have to be of the same age as the treated firm, i.e. *Firm Age = 3*, and belong to the same industry. The algorithm then searches for the best match

in terms of the trend in the output variable of interest, i.e. the outcome level in $t = -1$ and the first difference in $t = -1$ and $t = -2$. For each matched firm, I then impute the year of treatment as the year of CEO death in the corresponding treated firm. Table A1 reports the results of the matching.

Table A1
Matching Outcomes

This table reports sample descriptives for the treated and matched control firms. Each matched firm is drawn from the population of firms incorporated in Norway (approximately 500,000 firms). Panel I shows sample descriptives when matching on the trend in OROA (at t=-1. t=-2 and t=-3), whereas panels II and III shows the corresponding descriptives when matching on sales and employees. *Difference* shows the result of a simple t-test of differences in means. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	Control Firms		Treated Firms		Difference
	Mean	Std. Dev.	Mean	Std. Dev.	
I: Matched Samples: ROA					
Firm Age$_{t-1}$	13.32	5.80	13.32	5.80	0.00
ROA$_{t-1}$	7.86	22.24	8.96	21.52	1.10
ΔROA$_{t-1}$	−0.71	22.24	−1.88	20.12	−1.16
ΔROA$_{t-2}$	−1.46	20.96	0.63	14.15	2.09
II: Matched Samples: Sales					
Firm Age$_{t-1}$	13.31	5.78	13.31	5.77	0.00
Sales$_{t-1}$	68.58	197.68	70.66	197.21	2.08
ΔSales$_{t-1}$	8.34	48.17	4.13	40.62	−4.21
ΔSales$_{t-2}$	5.24	74.34	2.31	45.89	−2.93
III: Matched Samples: Employees					
Firm Age$_{t-1}$	12.75	5.87	12.75	5.87	0.00
Table A1 Continued from Previous Page					
Employees$_{t-1}$	21.91	34.67	32.10	55.31	10.20***
ΔEmployees$_{t-1}$	0.61	6.78	−0.27	12.05	−0.88
ΔEmployees$_{t-2}$	−0.68	8.78	0.44	15.24	1.12

Having formed my matched samples, I then proceed with the following basic difference-in-differences regression with firm fixed effects

$$Outcome_{it} = \alpha + \beta_1 Treated_i \times After_{it} + \beta_2 After_{it} + \beta_3 Treated_i + \lambda_i + \delta X_{it} + \varepsilon_{it} \quad (5)$$

, where β_1 is the coefficient of interest. X is a vector controlling for firm size and leverage. The basic idea is to compare firm outcomes for treated and control firms before and after the event. A significant β_1 would indicate a treatment effect. The matching procedure ensures that there are no pre-shock differences in the outcome between the two groups. The results are presented below.

Table A2

The Effect of CEO Death on Firm Performance: Matched Sample

This table shows coefficient estimates of the following model

$$Outcome_{it} = \alpha + \beta_1 Treated_i \times After_{it} + \beta_2 After_{it} + \beta_3 Treated_i + \lambda_i + \delta X_{it} + \varepsilon_{it}$$

, where *Outcome* is either *ROA*, *log(Sales + 1)*, *log(Employees + 1)* or *Survival*. *Treated* is a dummy that equals one if the firm experience CEO death sometime during the sample period, *After* is a dummy that equals one in the two periods following CEO death (or imputed year of death if the firm belongs to the control group) and zero otherwise, λ is a set of firm fixed effects and X is a matrix of control variables. When fitting the data, β_3 is subsumed in the firm fixed effects and is therefore not reported. The estimation window is from t=-4 to t=+2. The year of death (t=0) has been excluded since it is not clearly attributable to either the pre- or post-treatment period. For variable definitions, see Table I. P-values are shown in parentheses. Superscripts ***, ** and * correspond to significance at 1, 5 and 10 percent levels respectively.

	ROA	log(Sales + 1)	log(Employees + 1)	Survival
	(1)	(2)	(3)	(4)
Treated × After	−7.43**	−0.33*	−0.11**	−0.03*
	(0.04)	(0.09)	(0.04)	(0.10)
After	−0.27	−0.35***	−0.09**	−0.08***
	(0.94)	(0.00)	(0.03)	(0.00)
Additional Controls	Yes	Yes	Yes	No
Firm Fixed Effects	Yes	Yes	Yes	Yes
Adj. R^2	19.71%	19.51%	19.09%	9.72%
Observations	2847	2748	3129	4020