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Selected Topics in Corporate Finance

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Summary

Structured into three chapters, the present thesis comprises three stand-alone research articles on selected topics in Corporate Finance. The first chapter investigates how changes in status affect CEOs and their firms. The second chapter studies the effect of managerial entrenchment on focal firms and their competitors. The third chapter analyzes the influence of pessimistic managerial and media tone on CEO dismissals. The chapters' main research questions, methodological approaches, and results are outlined below.

The Paradox of Awards: How Status Spillover and Status Ripples Affect Who Benefits from CEO Awards

The first chapter presents a joint project with Professor Michael Jensen from the University of Michigan and Torsten Twardawski from the University of Konstanz. The underlying study contributes theoretically and empirically to the understanding of status shifts. In particular, we examine the consequences of sudden positive status shifts induced by CEO media awards for the awarded CEOs and CEOs who are affiliated with the award winners at the time of the awarding. Thereby, the study theorizes status ripples and distinguishes them from traditional status spillovers. A traditional status spillover implies that the status effect is immediately stronger for the awarded than for the affiliated CEOs and perpetuates to be relatively stronger. Status ripples, on the contrary, imply that awarded CEOs are subject to relatively stronger immediate but weaker subsequent status effects compared to the affiliated CEOs. Sudden positive status shifts are expected to create status ripples when the awarded CEOs are more constrained from fully exploiting their new status advantages than their affiliates. In this paper, we hypothesize that award-winning CEOs experience stronger immediate but weaker subsequent increases in compensation and the number of directorships compared to affiliated CEOs because the awarded CEOs are particularly constrained by an increased public attention. To underline the distinction between status ripples and status spillovers, we additionally investigate the post-award performance of firms managed by affiliated and awarded CEOs because the firms' performance depends on less visible and controllable award-induced factors.

To test our predictions empirically, we exploit a set of hand-collected CEO awards that are associated with sudden positive status shifts. Using LexisNexis, we collect announcements of media awards granted by the Business Week, Financial World, Chief Executive, Forbes, Industry Week, Morningstar.com, Time, Time/CNN, Electronic Business Magazine, and Ernst and Young for CEOs of S&P 1500 firms between 1997 and 2012. Using ISS Riskmetrics, we obtain a sample of affiliated CEOs by identifying all CEOs who sit on the boards of the award-winning CEOs' firms at the time of the awarding. The final sample contains 208 awarded CEOs and 182 affiliated CEOs.

We investigate the personal and organizational consequences of positive status shifts for awarded and affiliated CEOs by using different methodological approaches. We run Feasible Generalized Least Squares (FGLS) regressions to analyze shifts in the managers' compensation packages and

fixed-effects Poisson regressions to examine changes in the number of board directorships. Differences in compensation and in the number of directorships between awarded and affiliated CEOs are examined using Wald-tests. In addition, we employ an event-study design that investigates the cumulative abnormal returns of awarded and affiliated CEOs around the announcement date of a CEO award. Finally, we apply a matching procedure to investigate changes in the post-award firm performance of affiliated CEOs and CEOs who have the same likelihood of being an award-winner's affiliate. To compare performance differences between awarded and affiliated CEOs, we run pooled Ordinary Least Squares regressions with fixed effects.

The FGLS regression results exhibit a higher increase in compensation in the award year for awarded compared to affiliated CEOs. Considering a four-year post-award period, affiliated CEOs receive a significantly higher increase in compensation compared to awarded CEOs. Further, we document no difference in the immediate change of directorships for affiliated CEOs and awarded CEOs. In contrast, our results show that compared to award-winning CEOs, affiliated CEOs accept significantly more additional directorships in the post-award period. Finally, results from short- and long-term event studies as well as from the matching approach evidence that firms of affiliated CEOs are subject to decreases in value and performance at the time and in the aftermath of the status shift. However, the decline in long-term performance for firms managed by affiliated CEOs is not as strong as the drop in performance of firms managed by the award-winners. The results do not change when using alternative regression analyses, accounting for auto-correlation, and altering the matching approach.

In supplemental analyses, we provide empirical evidence for the assumption that awarded CEOs are subject to increased attention by analysts and the public media, which likely hinders them from fully exploiting their increased status. We also find that subsequent to a status shift, affiliated CEOs do not receive higher attention from the public but from the corporate elite which may explain the stronger subsequent rise in compensation and the number of directorships.

Overall, this study suggests that a CEO's shift in status also affects affiliates. Thereby, the concept of simple status spillovers accounts best for changes in the performance of firms managed by awarded- and affiliated CEOs. Status ripples, in contrast, are more suitable to explain changes in compensation and directorships.

The Effect of Managerial Entrenchment on Competitors: Evidence from Exogenous Shocks

The second chapter contains a joint project with my doctoral advisor Prof. Dr. Axel Kind and Torsten Twardawski from the University of Konstanz. The study contributes to the understanding of the ramifications of managerial entrenchment by switching the focus from focal firms to their competitors. Managerial entrenchment is prevalent when managers are powerful enough to follow their own interests rather than the interests of the shareholders (Weisbach, 1988). In this study, we argue that the reduction of managerial entrenchment is not only advantageous for focal firms but has a likewise beneficial effect on competing firms by increasing the competitive pressure.

To test this argument empirically, we make use of a natural experiment that exogenously reduces managerial entrenchment in focal firms: the sudden death of entrenched CEOs. Our paper relies on a hand-collected set of suddenly-deceased entrenched CEOs of S&P 1500 firms (focal firms) between the years 1993 and 2014. By applying an event-study methodology, we investigate the value effects associated with these events for both, focal and competing firms. By running additional t-tests and Difference-in-Differences (DiD) regressions, we examine changes in the focal and competing firms' adjusted operating return on assets. To identify underlying channels which may cause potential changes in the performance and the value of competing firms, we continue to use DiD regressions

and study changes in the risk-taking behavior measured by the volatility of the firms' stock returns, the systematic risk, the idiosyncratic risk, as well as the R&D intensity. We additionally focus on changes in the firms' sales, costs of goods sold, and sales margin. Finally, we investigate value effects for customer and supplier firms by running event studies and cross-sectional regressions.

Following the sudden death of an entrenched CEO, we document positive and significant announcement returns over a five-day event window, amounting to 4.6% for focal firms and 4.7% for competing firms. The market expectations for competing firms mirror subsequent increases in the firms' adjusted operating performance, risk-taking behavior, and sales margins which implies that the competing firms' managers put higher effort into their firms. We further document decreases in the value of customer and supplier companies which suggests that competitors aim at increasing their profitability by renegotiating existing contracts.

Our results are robust to a large battery of robustness checks. Besides conducting a falsification analysis which approves the validity of the applied DiD approach, we aim at mitigating concerns regarding differences among firms and industries by adding a number of control variables and either industry-fixed or firm-fixed effects. We further apply a matching procedure to identify control firms for our competing firms, use two additional classification schemes to assign competitors, and test a narrower definition of managerial entrenchment.

In a final step, we use an alternative and more prevalent exogenous shock to managerial entrenchment that allows us to estimate the associated value effects for a larger sample of focal and competing firms: close votes on entrenchment-related shareholder-sponsored proposals. Applying a regression discontinuity design that exploits the quasi-random assignment of companies into groups with passed and failed shareholder-sponsored proposals, our findings remain unchanged.

Overall, this study suggests that managerial entrenchment does not only harm focal firms but spills over to competitors. By reducing competitive pressure, the prevalence of managerial entrenchment in one firm destroys more shareholder wealth than yet assumed.

Uncovering the Role of Tone: Evidence from CEO Dismissals

The third chapter presents a joint project with Patrick Hauf from the University of Konstanz. The study contributes to the understanding of the influence of pessimistic managerial and media tone on forced turnover decisions.[1] Pessimistic media tone can serve as a governance mechanism by putting pressure on the board of directors to pursue the interests of shareholders (Bednar, 2012) and may alter the investors' sentiment (see e.g., Tetlock, 2007; Tetlock, Saar-Tsechansky, and Macskassy, 2008; Ahmad, Han, Hutson, Kearney, and Liu, 2016). Recent findings evidence that also managerial tone is able to actively elicit positive or negative investor perceptions about the manager's abilities and the future company performance (Jiang, Lee, Martin, and Zhou, 2017). In this study, we investigate whether pessimistic managerial and media tone influence the decision of the board of directors to fire the CEO. We further analyze whether managerial and media pessimism affect the shareholders' reactions to the dismissal announcement.

We empirically examine the influence of media and managerial pessimism on the probability of a CEO dismissal and the corresponding stock-price reactions for S&P 1500 firms between 1996 and 2012. We obtain a sample of 675 hand-collected CEO dismissals by screening news articles from LexisNexis. Following Jiang, Lee, Martin, and Zhou (2017), we measure managerial pessimism as the negative tone used in annually and quarterly disclosed company filings. In addition, we measure media pessimism as the firm-related negative tone used in over 800,000 newspaper articles published by the USA Today, the New York Times, the Washington Post, and the Wall Street

[1] Tone refers to the choice of words used to describe a certain content.

Journal. Specifically, we capture the level of pessimism by computing the fraction of negative words to total words in newspaper articles and filings. Negative words are identified using the financial word list of Loughran and McDonald (2011).

Probit regressions reveal that managerial pessimism is positively associated with the likelihood of a CEO dismissal, whereas a board's firing decision does not seem to be governed by pessimistic media tone. In contrast, using an event-study methodology and cross-sectional regressions, we document that firings of managers with higher media pessimism are associated with more positive stock-price reactions, whereas pessimistic managerial tone does not seem to affect the market reactions. The effect of media pessimism on stock returns, however, reverses in the days subsequent to the dismissal announcement, indicating a sentiment-driven overreaction.

We check the robustness of our results in many regards. With respect to the examination of announcement returns, our results remain stable by using alternative return models, by computing abnormal returns over different event windows, and by adding several control variables as well as industry- and year-fixed effects to our cross-sectional regression analyses. Likewise, probit regression results remain stable by adding various controls and industry- as well as year-fixed effects, by applying a relevance filter for newspaper articles, and by using abnormal managerial tone that already accounts for company performance and other characteristics. Finally, we do not find any significant changes in our results when using semi-parametric Generalized Estimating Equations which account for auto-correlation or applying a matching approach that deals with heterogeneity.

Overall, our study suggests that managers who use a pessimistic tone, are more likely to get fired. However, there is no evidence for a comprehensive governance mechanism through pessimistic media tone. In addition, we document that only pessimistic media tone but not pessimistic managerial tone explains market reactions to CEO replacements, suggesting that in case of a dismissal, shareholders rather rely on the media than on the management when evaluating the board's firing decision.

Summary in German

Die vorliegende Dissertation besteht aus drei eigenständigen Forschungsartikeln, welche sich über drei Kapitel erstrecken und ausgewählte Sachverhalte zu Themenstellungen aus dem Bereich der Corporate Finance darlegen. Das erste Kapitel untersucht den Einfluss von Statusänderungen auf Vorstandsvorsitzende und deren Unternehmen. Das zweite Kapitel beschäftigt sich mit den Auswirkungen von Vorstandsverwurzelungen auf betroffene Unternehmen und deren Wettbewerber. Das dritte Kapitel analysiert in wie weit sich der durch das Management und die Medien angeschlagene Ton auf Entlassungen von Vorstandsvorsitzenden auswirkt. Die in den Kapiteln untersuchten Forschungsfragen, verwendeten methodischen Ansätze und dargelegten Hauptergebnisse werden im Folgenden zusammengefasst.

The Paradox of Awards: How Status Spillover and Status Ripples Affect Who Benefits from CEO Awards

Das erste Kapitel präsentiert ein gemeinsames Forschungsprojekt mit Professor Michael Jensen von der Universität Michigan und Torsten Twardawski von der Universität Konstanz. Durch die Untersuchung der Konsequenzen positiver Statusveränderungen für Vorstandsvorsitzende, die eine Auszeichnung für ihre Leistungen erhalten (Preisträger) und der ihnen zum Zeitpunkt der Auszeichnung nahestehenden Vorstandsvorsitzenden anderer Unternehmen, trägt diese Studie auf theoretische und empirische Weise zum besseren Verständnis von Statusveränderungen bei. In diesem Zusammenhang stellt diese Studie die Theorie der Statuswellen auf und grenzt diese von traditionellen Statusübertragungen ab. Eine traditionelle Statusübertragung ruft einen sofortigen Statuseffekt hervor, der stärker für den Preisträger als für einen dem Preisträger nahestehenden Vorstandskollegen ist und der auch nachfolgend verhältnismäßig stärker bleibt. Im Gegensatz dazu implizieren Statuswellen, dass die Preisträger im Vergleich zu ihnen nahestehenden Vorstandskollegen einen relativ starken sofortigen aber schwächeren nachfolgenden Statuseffekt erfahren. Plötzliche eintretende positive Statusveränderungen erzeugen genau dann Statuswellen, wenn die Preisträger bei der Ausschöpfung ihres neuen Statusvorteils vergleichsweise gehemmt sind und sich diesen nicht allumfassend zunutze machen können. In diesem Papier theoretisieren wir insbesondere, dass, gehemmt durch zunehmende öffentliche Aufmerksamkeit, die Preisträger einen stärkeren unmittelbaren jedoch schwächeren langfristigen Anstieg in der Vergütung sowie der Annahme von Aufsichtsratsmandaten erfahren, wohingegen die den Preisträgern nahestehenden Vorstandsvorsitzenden einen schwächeren sofortigen jedoch mit der Zeit stärkeren Anstieg in Hinblick auf ihre Vergütung und zusätzliche Aufsichtsratsmandate aufweisen. Um die Unterscheidung zwischen Statuswellen und Statusübertragen hervorzuheben, untersuchen wir zusätzlich den Unternehmenserfolg von Preisträgern und ihren Kollegen, welcher von durch den Preis hervorgerufenen, jedoch weniger sichtbaren und kontrollierbaren Faktoren abhängt.

Um unsere theoretischen Darlegungen empirisch zu validieren, machen wir uns eine von Hand gesammelte Stichprobe positiver Statusveränderungen bei Vorstandsvorsitzenden, hervorgerufen durch mediale Auszeichnungen, zunutze. Mit Hilfe von LexisNexis identifizieren wir alle preistragenden Vorstandvorsitzenden von S&P 1500 Unternehmen in den Jahren 1997 bis 2002, welche durch die Zeitschriften Business Week, Financial World, Chief Executive, Forbes, Industry Week, Morningstar.com, Time, Time/CNN, Electronic Business Magazine und Ernst and Young ausgezeichnet wurden. Wir definieren die den Preisträgern nahestehenden Vorstandskollegen als jene Vorstandsvorsitzenden, die im Aufsichtsrat des ausgezeichneten Managers zum Zeitpunkt der Preisvergabe ein Mandat halten und identifizieren sie mittels der Datenbank ISS Riskmetrics. Es ergibt sich eine finale Stichprobe von 208 preistragenden Vorstandsvorsitzenden und 182 den Preisträgern nahestehenden Vorstandsvorsitzenden.

Wir untersuchen die persönlichen und unternehmensspezifischen Konsequenzen positiver Statusveränderungen für die Preisträger und ihnen nahestehende Vorstandskollegen mittels verschiedener methodischer Ansätze. Wir bedienen uns der Methode der verallgemeinerten kleinsten Quadrate (VKQ) um Veränderungen in den Vergütungsstrukturen zu beziffern und Poisson Regressionen unter Berücksichtigung fixer Effekte um Veränderungen in Aufsichtsratsmandaten zu analysieren. Differenzen in der Vergütung und den Aufsichtsratsmandaten von Preisträgern und ihren Vorstandskollegen untersuchen wir mit Hilfe von Wald-Tests. Des Weiteren analysieren wir mittels Ereignisstudien die Überschussrenditen, die zum Zeitpunkt der Bekanntgabe der Preisträger in Unternehmen mit einem Preisträger als Vorstandsvorsitzenden sowie in Unternehmen, in denen die dem Preisträger nahestehenden Personen den Vorsitz haben, auftreten. Mittels eines Matchingverfahrens vergleichen wir noch den Unternehmenserfolg von Vorstandsvorsitzenden, die einem Preisträger nahestehen, mit dem Unternehmenserfolg von Vorstandsvorsitzenden, die die gleiche Wahrscheinlichkeit aufweisen einem Preisträger nahe zu stehen. Im Hinblick darauf werden die Differenzen mit einem gebündelten Schätzmodell unter Berücksichtigung fixer Effekte und auf Grundlage der Methode der kleinsten Quadrate untersucht.

Die Durchführung der VKQ Regressionsanalysen zeigt, dass die Preisträger im Jahr der Preisvergabe einen höheren Anstieg in ihrer Vergütung verzeichnen. Die Betrachtung eines auf die Preisvergabe folgenden vierjährigen Zeitfensters zeigt allerdings, dass die dem Preisträger nahestehenden Vorstände einen signifikant stärkeren Anstieg in ihrer Vergütung verzeichnen als die Preisträger selbst. Darüber hinaus finden wir keine signifikant unterschiedliche, sofortige Zunahme in der Anzahl der Aufsichtsratsmandate zwischen Preisträgern und ihnen nahestehenden Vorstandskollegen. Im Gegensatz dazu zeigen unsere Ergebnisse, dass in den Jahren nach der Preisverleihung die den Preisträgern nahestehenden Vorstände eine relativ höhere Anzahl an zusätzlichen Aufsichtsratsmandaten annehmen. Zusätzlich dazu dokumentieren kurz- und langfristig ausgelegte Ereignisstudien und das Matching, dass Unternehmen mit Vorstandsvorsitzenden, die einem Preisträger nahestehen, einen andauernden Rückgang im Aktienkurs aufweisen. Dieser ist jedoch nicht so dramatisch wie jener in Unternehmen mit einem direkten Preisträger.

Unsere Ergebnisse halten alternativen Regressionsanalysen, der Berücksichtigung von Autokorrelationen sowie Veränderungen des Matchings stand. Weitere, ergänzende Untersuchungen zeigen, dass die Preisträger einer stark ansteigenden öffentlichen Aufmerksamkeit ausgesetzt sind, die sie vermutlich daran hindert den neuen Statusvorteil vollkommen für sich auszuschöpfen. Darüber hinaus kommt zum Vorschein, dass die dem Preisträger nahestehenden Vorstände keine stärkere Aufmerksamkeit durch die Öffentlichkeit erfahren, jedoch mehr im Fokus der unternehmerischen Elite stehen. Dieser Umstand könnte den stärkeren Anstieg in der Vergütung als auch in der Anzahl

von Aufsichtsratsmandaten erklären.

Zusammenfassend macht diese Studie deutlich, dass die Statusveränderung eines Vorstandsvorsitzenden auch ihm nahestehende Vorstände beeinflusst. Dabei ist festzustellen, dass das Konzept einer einfachen Statusübertragung die Veränderung im Unternehmenserfolg beider Parteien am besten begründet, wohingegen Statuswellen die Veränderungen in der Vergütung als auch in der Anzahl der Aufsichtsratsmandate besser erklären.

The Effect of Managerial Entrenchment on Competitors: Evidence from Exogenous Shocks

Das zweite Kapitel basiert auf einer gemeinsamen Forschungsarbeit mit meinem Doktorvater Prof. Dr. Axel Kind und Torsten Twardawski von der Universität Konstanz. Die Studie trägt zum besseren Verständnis der Bedeutung von Vorstandsverwurzelungen[2] bei und legt die Auswirkungen auf betroffene Unternehmen und deren Wettbewerber dar. Eine Vorstandsverwurzelung entsteht durch eine zu große Machtposition des Vorstandsvorsitzenden im Unternehmen und führt dazu, dass diese Manager in erster Linie ihre eigenen Interessen verfolgen anstatt im Sinne der Aktionäre zu handeln (Weisbach, 1988). Dieses Forschungspapier stellt die These auf, dass eine Verringerung von Verstandsverwurzelungen, die den kompetitiven Druck erhöht, nicht nur den primär betroffenen Unternehmen sondern auch den Wettbewerbern zugutekommt.

Um diese These empirisch zu testen, machen wir uns ein natürliches Experiment zunutze, das Vorstandsverwurzelungen in den betroffenen Unternehmen auf exogene Weise verringert: den unerwarteten Tod verwurzelter Vorstandsvorsitzender. Dem Papier liegt ein von Hand gesammelter Datensatz plötzlich verstorbener Vorstände von S&P 1500 Unternehmen (betroffene Unternehmen) aus den Jahren 1993 bis 2014 zugrunde. Mit Hilfe von Ereignisstudien erforschen wir die Auswirkungen dieser Vorfälle auf den Wert betroffener Unternehmen und ihrer Wettbewerber. Durch zusätzliche t-tests und Differenz-von-Differenzen (DiD) Regressionsanalysen untersuchen wir in einem zweiten Schritt Veränderungen in der operativen Rentabilität betroffener Unternehmen und ihrer Wettbewerber. Zur Erklärung potentieller Wert- und Rentabilitätsveränderungen bei Wettbewerbern behalten wir die DiD Methode bei und erforschen Anpassungen im Risikoverhalten gemessen an der Volatilität des Aktienkurses, dem systematischen und idiosynkratischen Risiko sowie der Forschungs- und Entwicklungsintensität. Des Weiteren untersuchen wir Veränderungen des Umsatzes, der Fertigungskosten und der Verkaufsspannen. Abschließend ermitteln wir unter Zuhilfenahme weiterer Ereignis- und Querschnittsstudien auch die Wertveränderungen bei Auftraggeber- und Zulieferfirmen.

Betroffene Unternehmen als auch ihre Wettbewerber reagieren auf den unerwarteten Tod eines verwurzelten Vorstandsvorsitzenden unter Anbetracht eines fünftägigen Eventfensters mit signifikant positiven Überschussrenditen von 4.6% bzw. 4.7%. Darüber hinaus zeigt sich, dass die Überschussrendite von Wettbewerbern einen Anstieg der bereinigten operativen Rentabilität, des Risikoverhaltens und der Verkaufsspannen wiederspiegelt und somit eine erhöhte Anstrengung des Managements suggeriert. Zudem deutet ein Wertverfall bei Auftraggeber- und Zulieferfirmen von Wettbewerbern auf erwartete Neuverhandlungen zur Steigerung der Profitabilität hin.

Die Ergebnisse dieses Forschungspapiers erweisen sich im Hinblick auf eine Vielzahl zusätzlicher

[2]Der Begriff “Vorstandsverwurzelung” ist die deutsche Übersetzung des englischen Fachbegriffs “Managerial Entrenchment”.

Validitätsprüfungen als robust. Neben einer Falsifizierungsanalyse zur Validierung der angewandten DiD Methode, wurden durch die Hinzunahme diverser Kontrollvariablen und industrie- bzw. firmenspezifischer fixer Effekte potentielle Bedenken bezüglich des Einflusses unterschiedlicher Unternehmenscharakteristika sowie industriespezifischer Trends adressiert. Darüber hinaus bestätigen wir die Robustheit der Ergebnisse mit Hilfe eines Matchings, zwei weiterer Klassifizierungsschemata für Wettbewerber sowie einer alternativen Definition der Vorstandsverwurzelung.

In einem finalen Schritt machen wir uns ein zweites, häufiger auftretendes, natürliches Experiment zu eigen, um die Wertveränderungen von Vorstandsverwurzelungen für eine größere Stichprobe betroffener Unternehmen und ihrer Wettbewerber nachzuweisen: Knappe Abstimmungsergebnisse zu Aktionärsanträgen die Vorstandsverwurzelungen reduzieren sollen. Die Durchführung einer Regressions-Diskontinuitäts-Analyse, die sich dem Umstand bedient, dass Unternehmen zufällig in Gruppen mit erfolgreichen und gescheiterten Aktionärsanträgen allokiert werden, bestätigt die Übertragbarkeit unserer Primärergebnisse.

Zusammenfassend legt diese Studie dar, dass Vorstandsverwurzelungen nicht nur den betroffenen Unternehmen, sondern auch dem kompetitiven Umfeld schaden. Durch die Reduzierung des Wettbewerbsdrucks führen Vorstandsverwurzelungen daher zu einem größeren Wertverfall als bisher angenommen.

Uncovering the Role of Tone: Evidence from CEO Dismissals

Das dritte Kapitel basiert auf einem Kooperationsprojekt mit Patrick Hauf von der Universität Konstanz. Die Studie trägt zum Verständnis der Auswirkungen pessimistischen Tons, angeschlagen durch das Management (Managerton) sowie die Medien (Medienton), im Hinblick auf Entlassungsentscheidungen des Vorstandsvorsitzendens durch den Aufsichtsrat bei.[3] Der Ton der Medien fungiert dabei zum einen als eine Art Überwachungsmechanismus indem er indirekt Druck auf den Aufsichtsrat ausübt im Sinne der Aktionäre zu handeln (Bednar, 2012). Zum anderen ist er in der Lage die Wahrnehmung von Investoren zu beeinflussen (see e.g., Tetlock, 2007; Tetlock, Saar-Tsechansky, and Macskassy, 2008; Ahmad, Han, Hutson, Kearney, and Liu, 2016). Wie aus aktuellen Forschungserkenntnissen hervorgeht, können jedoch auch Manager ihren Ton dazu nutzen, um bei Investoren aktiv positives als auch negatives Empfinden über die eigenen Fähigkeiten sowie den zukünftigen Unternehmenserfolg hervorzurufen (Jiang, Lee, Martin, and Zhou, 2017). Dieses Forschungspapier eruiert, ob ein pessimistischer Manager- als auch Medienton die Entlassungsentscheidungen des Aufsichtsrats beeinflusst. Des Weiteren wird untersucht, ob ein pessimistischer Manager- als auch Medienton in Zusammenhang mit den auf die Entlassungsentscheidungen folgenden Reaktionen von Aktionären steht.

Im Rahmen einer empirischen Analyse untersuchen wir die Kohärenz zwischen pessimistischem Medien- als auch Managerton und der Wahrscheinlichkeit einer Vorstandsentlassung sowie den damit verbundenen Aktienkursreaktionen für S&P 1500 Unternehmen in den Jahren 1996 bis 2012. Basierend auf einer von Hand mit LexisNexis durchgeführten Zeitungsartikelrecherche zu Vorstandsentlassungen bedienen wir uns einer Stichprobe von 675 Beobachtungen. Wie von Jiang, Lee, Martin, and Zhou (2017) vorgeschlagen, definieren wir pessimistischen Managerton als den negativen Ton in Jahres- und Quartalsberichten. Im Gegensatz dazu messen wir pessimistischen

[3] Ton beschreibt in diesem Zusammenhang die zur Darstellung eines bestimmten Sachverhaltes herangezogene Wortwahl.

Medienton als den auf eine Firma gerichteten negativen Ton, der in über 800.000 Zeitungsartikeln der USA Today, der New York Times, der Washington Post, und des Wall Street Journals vorherrscht. Zur Festlegung des Grades an Pessimismus in Zeitungsartikeln sowie Jahres- und Quartalsberichten nutzen wir Loughran and McDonald (2011)'s Wörterliste und berechnen den Anteil der negativen Worte in Relation zur Gesamtanzahl an Worten.

Die angewendeten Probitmodelle zeigen, dass vorherrschender Pessimismus im Management zu einer deutlich höheren Entlassungswahrscheinlichkeit führt. Im Gegensatz dazu scheint die Entlassungsentscheidung des Aufsichtsrats nicht durch einen pessimistischen Medienton beeinflusst zu werden. Die Implementierungen von Ereignis- und Querschnittsstudien zeigen dagegen, dass die Bekanntgaben von Vorstandsentlassungen positivere Marktreaktionen auslösen, wenn der Medienton vor der Entlassung pessimistischer war. Dieser Effekt verschwindet allerdings im Laufe der darauffolgenden Tage wieder und impliziert eine tongetriebene Überreaktion der Aktionäre. Im Gegensatz dazu scheint ein pessimistischer Managerton keinen Einfluss auf die Marktreaktionen zu haben.

Die Ergebnisse halten einer Vielzahl von Validitätsprüfungen stand. In Anbetracht der Untersuchung der Überschussrenditen finden wir keine grundlegende Veränderung unserer Ergebnisse unter Verwendung alternativer Renditemodelle, verschiedener Eventfenster oder der Aufnahme diverser Kontrollvariablen sowie industrie- und jahresspezifischer Effekte in unseren Querschnittsstudien. Im Hinblick auf die Probitmodelle testen wir die Gültigkeit unserer Ergebnisse durch die Berücksichtigung diverser Kontrollvariablen, der Relevanz der verwendeten Zeitungsartikel sowie eines zusätzlichen Maßes, welches den klassisch gemessenen Managerton um potentielle Einflussfaktoren wie beispielsweise den früheren Unternehmenserfolg bereinigt. In einem letzten Schritt führen wir noch semi-parametrische generalisierte lineare Modelschätzungen sowie ein Matching durch. Die Verwendung dieser methodischen Ansätze erlaubt die Berücksichtigung von Autokorrelationen und Heterogenität.

Zusammenfassend macht diese Studie deutlich, dass Vorstandsvorsitzende, die einen pessimistischen Ton anschlagen, eine deutliche höhere Wahrscheinlichkeit aufweisen entlassen zu werden. Im Gegensatz dazu scheint pessimistischer Medienton den Aufsichtsrat nicht zu beeinflussen. Zudem impliziert die Studie, dass lediglich ein pessimistischer Medienton die Wahrnehmung von Investoren im Entlassungsfall beeinflusst und zu kurzfristigen Überreaktionen am Tag der Bekanntgabe führt.

Chapter 1

The Paradox of Awards: How Status Spillover and Status Ripples Affect Who Benefits From CEO Awards

with Michael Jensen (University of Michigan) and Torsten Twardawski (University of Konstanz)

Abstract: Distinguishing between status spillovers and status ripples, we argue that sudden positive status shifts create status ripples when the social actors experiencing the status shifts are more constrained from fully exploiting their new status advantages than the social actors to whom they are affiliated. We emphasize specifically the status ripple paradox that the indirect status effects experienced by the affiliated actors can be as important and sometimes more enduring than the direct status effects experienced by the upwardly mobile actors themselves. Focusing empirically on prestigious CEO awards from U.S. news magazines, we examine the consequences of status shifts for the awarded CEOs and the CEOs who are directors on the awarded CEOs' boards at the time of the award. We find evidence of status ripples in CEO compensation by showing that awarded CEOs have relatively greater immediate but smaller subsequent increases in compensation and partial evidence of status ripples in directorships by showing that they have similar immediate but smaller subsequent increases in the number of directorships.

1 Introduction

Winning a prestigious award such as a Nobel Prize, Fields Medal, or Academy Award represents a particularly poignant sudden positive status shift – a salient entry into a group of elite peers (Jensen and Kim, 2015). Despite gaining more resources and opportunities, winning awards and moving up the status hierarchy can, however, have both positive and negative consequences for the upwardly mobile social actors. Mathematicians awarded a Fields Medal tend to publish less frequently afterwards than their close contenders, a drop only half explained by the winners using their higher status to explore new research areas (Borjas and Doran, 2015). Similarly, Academy Award winners and nominees appear on average in more movies than other actors after the awards, but they are also more likely, in particular the male actors, to experience divorce (Jensen and Kim, 2015). The mixed consequences of winning awards, and sudden positive status shifts more generally, are not limited to the social actors winning the awards, but affect other social actors around them. When life scientists are appointed to a prestigious research institution, a sudden positive status shift, citations to comparable academic articles by non-appointed scientists drop significantly (Reschke, Azoulay, and Stuart, 2017). And when CEOs win prestigious awards, the status shift affects the other executives in their firms positively in the form of a higher likelihood of becoming CEOs themselves (Graffin, Wade, Porac, and McNamee, 2008) but their firms negatively in the form of decreased firm performance (Malmendier and Tate, 2009).

Moving beyond identifying the positive or negative consequences of status shifts, we focus on when status shifts take the form of a status spillover or a status ripple to theorize the implications of social actors winning awards and suddenly moving up the status hierarchy. A status spillover implies that the immediate status effects are stronger for the social actors that won the awards and moved up the status hierarchy compared to the actors affiliated with the upwardly mobile actors and that the subsequent status effects continue to be relatively stronger for the award winner even if the status effects eventually dissipate. A status ripple implies, in contrast, that the social actors that win awards and move to higher status positions experience relatively stronger immediate but weaker subsequent status effects and that the social actors affiliated with the upwardly mobile social actors experience relatively weaker immediate but stronger subsequent status effects. A status shift is likely to take the form of a status ripple when the social actors moving to higher status positions are constrained from fully exploiting the extra resources and opportunities that come with their higher status but the social actors surrounding them are less constrained from exploiting their affiliations with the upwardly mobile actors. When politicians, bureaucrats, and executives move into higher office, for example, they are typically constrained from taking personal advantage of their new office, whereas it is easier for actors affiliated with the office holders to exploit their connections for personal gains.

We study the importance of status ripples and their distinction from status spillovers in the context of CEOs winning prestigious awards from U.S. public media such as Business Week, Forbes, and Times. Winning a CEO award represents the entry into a highly elite group of corporate executives that provides a strong signal of quality and raises the status of the awarded CEO (Malmendier and Tate, 2009; Gallus and Frey, 2017). Awarded CEOs are perceived positively by

investors (Wade, Porac, Pollock, and Graffin, 2006), competitors (Ammann, Horsch, and Oesch, 2016), and other firms (Stern, Dukerich, and Zajac, 2014) and gain access to valuable resources and opportunities such as higher compensation and corporate directorships for themselves and their executives (Wade, Porac, Pollock, and Graffin, 2006; Graffin, Wade, Porac, and McNamee, 2008; Malmendier and Tate, 2009). We focus on status ripples to explain how awards affect director CEOs, defined as the CEOs who affiliate with the awarded CEOs through being on their firms' boards of directors. We hypothesize specifically that awarded CEOs experience stronger immediate increases in compensation and directorships but weaker subsequent increases, whereas director CEOs experience weaker immediate increases but stronger subsequent increases. We highlight the differences between status ripples and status spillovers by examining the post-award performance of director CEOs. Whereas status ripples explain compensation and the number of directorships, status spillover explains firm performance: The post-award performance of director CEOs decreases compared to their pre-award performance but it decreases less than the performance of awarded CEOs.

Our study makes several contributions to research on status and CEOs. Most status theory and research on sudden positive status shifts focuses on the social actors moving to higher status positions and simple status spillovers to affiliated actors (Azoulay, Stuart, and Wang, 2013; Reschke, Azoulay, and Stuart, 2017). We focus instead on the status ripple paradox that the status effects experienced by the affiliated actors can be as important and sometimes more enduring than the direct status effects experienced by the upwardly mobile actors themselves. Although sudden positive status shifts certainly are advantageous to the social actors moving up the status hierarchy (Kovács and Sharkey, 2014; Jensen and Kim, 2015), status ripples imply that sudden positive status shifts are not necessarily a "winner-take-all" phenomenon (Frank and Cook, 2010) because the upwardly mobile social actors can be constrained from fully exploiting their status advantages. Introducing, theorizing, and testing status ripples in the context of CEOs winning prestigious awards, we contribute also to research on CEOs and corporate governance by showing the potential downside of CEO awards. Specifically, by making it easier for director CEOs to pursue personal perquisites that distract them from their internal duties, the downsides of CEO awards are not limited to the awarded CEOs and their firms (Wade, Porac, Pollock, and Graffin, 2006; Graffin, Wade, Porac, and McNamee, 2008; Malmendier and Tate, 2009) but represent a broader and more diffuse corporate governance problem.

2 Theory and Hypotheses

2.1 Positive Status Shifts and CEO Awards

Status refers to the hierarchical position a social actor occupies in a social system (Gould, 2002). The general advantages of status, such as signaling quality, providing agency, and granting legitimacy, are well established (see Jensen, Kim, and Kim (2010) and Piazza and Castellucci (2014), for reviews). Status is also important for CEOs for several reasons. First, status functions as a signal of quality (Podolny, 1993), which means that high-status CEOs tend to be perceived more

favorably and competent than low-status CEOs (Hayward, Rindova, and Pollock, 2004; Wade, Porac, Pollock, and Graffin, 2006). Second, status reduces accountability pressures (Jensen, 2006). It is easier for corporate directors to justify not monitoring high-status CEOs closely than not monitoring low-status CEOs, thus granting high-status CEOs relatively more managerial discretion (Hambrick and Finkelstein, 1987). Third, status is a source of power and legitimacy (Salancik and Pfeffer, 1974). It is therefore relatively easier for high-status CEOs to overcome resistance inside the firm, including the board of directors, thus ensuring high-status CEOs a greater impact on firm performance (Adams, Almeida, and Ferreira, 2005). Because of the advantages of status, it is not surprising that high-status CEOs are associated with higher compensation packages, better outside options in the director labor market, and higher rent extraction (Belliveau, O'Reilly, and Wade, 1996; Wade, Porac, Pollock, and Graffin, 2006; Graffin, Wade, Porac, and McNamee, 2008; Malmendier and Tate, 2009).

Media awards provide CEOs with a sudden positive status shift. Although CEO awards are of relatively low economic value, innate desires for social recognition and comparison incentivize CEOs to accept awards that move them up the status hierarchy (Wood, 1989; Frey, 2006; Rossman and Schilke, 2014; Gallus and Frey, 2017). The award itself may not be of significant economic value but moving up the status hierarchy is economically and socially valuable, even for CEOs. Resources and opportunities typically accumulate at the top of the status hierarchy (Merton, 1968; Sørensen, 1996), which implies that high status ultimately provides access to disproportionally more resources and opportunities. Because CEO awards are personal awards given to the CEO, not to the firm the CEO manages, they are particularly likely to increase access to personal resources and opportunities such as higher compensation, more directorships, authoring books, and giving television and print-media interviews (Wade, Porac, Pollock, and Graffin, 2006; Malmendier and Tate, 2009). When awarded CEOs use their higher status to pursue personal interests that are unrelated to their firms, the increase in outside activities reduces the amount of time and energy left to manage their firms, which has been associated with subsequent decreases in firm performance (Malmendier and Tate, 2009).

Becoming a corporate director is an outside activity that can divert managerial attention (Conyon and Read, 2006; Pandey, Vithessonthi, and Mansi, 2015). The increase in demand for awarded CEOs in the director labor market results in some awarded CEOs dedicating a significant portion of their scarce time to other firms (Malmendier and Tate, 2009). Accepting a directorship requires a heavy investment of time and energy, which makes it increasingly difficult for CEOs to justify outside directorships to their boards and shareholders (Booth and Deli, 1996; Davis and Mizruchi, 1999; Geletkanycz and Boyd, 2011). Time is a particularly valuable resource for CEOs because they cannot easily delegate their responsibility for the strategic direction and operational effectiveness of their firms to others (Mintzberg, 1973; Norburn, 1989; Daily and Johnson, 1997). Finding it difficult to delegate, time constraint CEOs are therefore more likely to shift their focus from complex long-term strategic goals to simple short-term operational goals (Simon, 1955; Byrne, Symonds, and Silver, April 1, 1991). Anthony Burns, former CEO of Ryder System, a U.S. transportation and logistics firm, is a vivid example. After receiving public recognition in 1988, he joined several boards, witnessed a subsequent decline in Ryder's performance, before resigning from

two corporate directorships to focus on Ryder's recovery. Burns admitted later that "the demands on your time for external activities are incredible" and that he turned "from being a genius to braindead" (Byrne, Symonds, and Silver, April 1, 1991, p. 267).

When CEOs move up the status hierarchy, they not only encounter more opportunities to divert their time and effort to activities outside their firms, their power inside their firms increases as well (D'Aveni, 1990; Malmendier and Tate, 2009). Powerful CEOs face less opposition and are less likely to be dismissed (Boeker, 1992; Daily and Johnson, 1997), which makes it easier for them to put their personal interests ahead of the interests of their firms. Powerful CEOs are therefore more likely to be able to pursue personal goals such as maximizing their compensation and decoupling their compensation from firm performance (Westphal and Zajac, 1993; Zajac and Westphal, 1994; Bebchuk, Fried, and Walker, 2002). To increase the compensation of an award-winning CEO is not necessarily problematic because winning a CEO award could result in CEOs being granted more responsibilities within their firms and raise external expectations about future firm performance (Khurana, 2002; Hayward, Rindova, and Pollock, 2004; Wade, Porac, Pollock, and Graffin, 2006). Research shows, however, that the increase in status allows these powerful CEOs to increase their compensation independent of the future performance of their firms (Malmendier and Tate, 2009). It is not surprising, therefore, that the positive status shifts experienced by awarded CEOs have been shown to have positive personal consequences for the awarded CEOs such as more corporate directorships and increased compensation but negative performance consequences for their firms (Wade, Porac, Pollock, and Graffin, 2006; Malmendier and Tate, 2009).

We argue next that sudden positive status shifts not only have positive personal and negative firm consequences for the awarded CEOs but create status spillovers and status ripples that extend beyond the awarded CEOs.

2.2 Positive Status Shifts, Status Spillovers, and Status Ripples

Status spillovers and status ripples are both different from "status leakage" (Podolny, 2005, p. 13). First, whereas status leakage implies that status is transferred from the high-status actor to the low-status actor, thereby decreasing the status of the high-status actor and increasing the status of the low-status actor (Podolny, 1994; Jensen, 2003), status spillovers and status ripples do not necessarily imply a transfer of status from the high-status actor to the low-status actor. Second, status leakage has negative consequences for the high-status actors losing status and positive consequences for the low-status actors gaining status. Status spillovers and status ripples, in contrast, do not necessarily affect the high-status actors and can have both positive and negative consequences for the low-status actors including basking in reflected glory and decreased attention (Reschke, Azoulay, and Stuart, 2017).[1] Status spillovers and status ripples are particularly relevant when sudden status shifts occur because sudden status shifts are unlikely to have been factored into the relationship and

[1]Without over-interpretation, the difference between status leakage, spillover, and ripple can be expressed using the following imagery: Status leakage is a bucket leaking water (status), leaving less water in the bucket and more water around the bucket. Status spillover is an overflowing bucket, leaving the same amount of water in the bucket and more water around the bucket. Status ripple is a drop of water in the bucket that first increases the water level at the drop point (splash) and then increases the level in waves around the drop point.

are therefore more likely to affect the actors surrounding the focal actor. When the status of a firm is threatened by scandal, a negative status shift, for example, the firms to whom the scandalized firm is connected or simply share industry category may become anxious about how the unforeseen scandal affects their own status (Jensen, 2006) and experience legitimacy loss (Jonsson, Greve, and Fujiwara-Greve, 2009).

Prior research shows that the status shifts that follow CEO awards result in two types of status spillovers. First, when CEOs win an award, their positive status shift has consequences for the other senior executives inside the firm. Graffin, Wade, Porac, and McNamee (2008) showed that although winning an award allowed CEOs to capture a higher share of the executive compensation, it also increased the compensation to the other executives and the likelihood that they become CEOs themselves. Second, when CEOs win an award, their positive status shift has consequences for senior executives outside the firm. Ammann, Horsch, and Oesch (2016) reported that CEO awards have an incentivizing effect on the CEOs in the firms that compete with the awarded CEOs' firms. The incentivizing effect manifests itself in the competing firms having a positive stock market performance due to increased risk taking, operating performance, and patenting activities. And Shi, Zhang, and Hoskisson (2017) showed that the competitors of awarded CEOs undertake more expensive acquisitions, presumably to obtain the same amount of social recognition. These studies show, in other words, that the effects of status shifts extend beyond the CEOs that experience the status shifts to the senior executives around them inside and outside their firms. We add that the effects of CEO awards may also extend to the CEOs who are on the board of directors of the award-winning CEOs' firms in the specific form of status ripples and, therefore, paradoxically have more enduring effects on the director CEOs than the awarded CEOs.

Being on the board of a firm whose CEO wins an award is beneficial for several reasons. First, CEO awards are closely related to the recent performance of the awarded CEOs' firms (Malmendier and Tate, 2009). It is difficult, however, to determine the unique contributions of the CEO, the other senior executives, and the corporate directors to firm performance (Hambrick and Mason, 1984; Judge and Zeithaml, 1992; Zahra and Pearce, 1989). Director CEOs can therefore justifiably claim that they, like the awarded CEO and other senior executives (Graffin, Wade, Porac, and McNamee, 2008), contributed to firm performance. And the corporate elite as well as the board of directors monitoring the director CEOs can justifiably give the director CEOs credit for contributing to the performance of the awarded CEOs or for having hired the awarded CEO (Rindova, Williamson, Petkova, and Sever, 2005). Second, if credit cannot be claimed or given to a director CEO for the performance of the awarded CEO, the director CEO may still be credited for future opportunities to learn from the awarded CEO and therefore become more valuable (Geletkanycz and Hambrick, 1997; Geletkanycz, Boyd, and Finkelstein, 2001; Carpenter and Westphal, 2001; Westphal, Seidel, and Stewart, 2001). Third, even if a director CEO did not contribute significantly to the awarded CEOs' performance and has few opportunities to learn from the awarded CEO going forward, simply being on the awarded CEOs' board represents a strong endorsement of the director CEO (Podolny, 1994; Boivie, Graffin, and Pollock, 2012) that is likely to increase the visibility and the perceived value of the director CEO (Fich and Shivdasani, 2007; Jensen and Roy, 2008; Chandler, Haunschild, Rhee, and Beckman, 2013).

In sum, CEO awards create positive status shifts for the awarded CEOs that affect director CEOs in the form of a status spillover or a status ripple such that director CEOs affiliated with awarded CEOs are associated with increases in compensation and directorships:

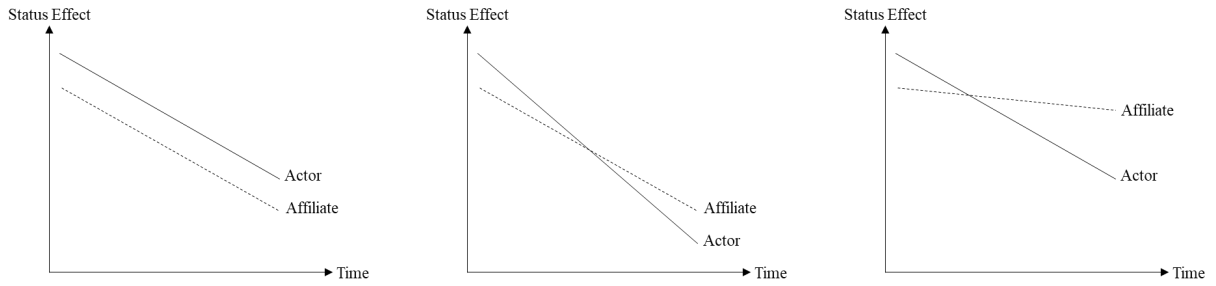
Hypothesis 1: Director CEOs affiliated with awarded CEOs are associated with increases in compensation.

Hypothesis 2: Director CEOs affiliated with awarded CEOs are associated with increases in the number of corporate directorships.

2.3 Status Spillover versus Status Ripple

The arguments behind hypothesis 1 and hypothesis 2 are consistent with the positive status shift creating either a status spillover or a status ripple. The main difference between a status spillover and a status ripple is the relative sizes of the immediate effects and the subsequent effects of the status shift for the awarded CEOs and the director CEOs. Figure 1.1 illustrates the difference between status spillovers and status ripples. Figure 1.1 (a) is a traditional status spillover: The status shift has a stronger immediate and subsequent effect on the awarded CEO than the director CEO. Figure 1.1 (b) and figure 1.1 (c) are status ripples: The immediate effect of the status shift is stronger for the awarded CEO than the director CEO, but the subsequent effect of the status shift is stronger for the director CEO than the awarded CEO. Figure 1.1 (b) implies that the status ripple occurs because the effect of the status shifts decreases more rapidly for the awarded CEO than the director CEO (compared to the traditional status spillover case), whereas figure 1.1 (c) implies that the status ripple occurs because the effect of the status shift decreases less rapidly for the director CEO than the awarded CEO (again compared to the traditional status spillover case). We argue next that the difference between status spillover and status ripple in our context hinges on the awarded CEOs being more constrained than the director CEOs from fully exploiting their new status in ways that could harm their firms after being awarded (figure 1.1 (b)) and not simply on whether director CEOs benefit from being affiliated with awarded CEOs (figure 1.1 (a)).

Figure 1.1: Status Spillover and Status Ripple



(a) Spillover: Similar Decrease (b) Ripple: Actor Fast Decrease (c) Ripple: Affiliate Slow Decrease

Whereas the director CEOs are likely to share some of the benefits of the positive status shift, they are less likely to be constrained by the award in the same way the awarded CEOs are constrained. An important constraint that follows the positive status shift is the so-called

"burden of celebrity". The burden of celebrity refers to the negative consequences of the extra attention awarded CEOs receive from the media, security analysts, and other important audiences: "Because they perform in the limelight, their every move is subject to close scrutiny, interpreted and reinterpreted; their every action - like Midas's every touch - has significance" (Fombrun, 1996, p. 387). Although a CEO may have won an award based on superior performance, the expectations to continue delivering superior performance and surpass themselves can be hard to meet going forward. Awarded CEOs may simply not be able to keep pace with ever increasing expectations (Jensen, Kim, and Kim, 2012) and therefore end up disappointing shareholders and other important stakeholders (Khurana, 2002; Malmendier and Tate, 2009). Wade, Porac, Pollock, and Graffin (2006) and Graffin, Wade, Porac, and McNamee (2008) both reported that awarded CEOs experience the burden of celebrity in the form of a tighter coupling of CEO compensation and firm performance. When their firms perform well, their compensation increases more than that of non-awarded CEOs (Wade, Porac, Pollock, and Graffin, 2006) and other senior executives (Graffin, Wade, Porac, and McNamee, 2008), but their compensation increases less when they perform poorly as a result of the intense scrutiny that follows them and their firms.

Director CEOs are less likely to experience the burden of celebrity. Because CEO awards are personal awards, most public media coverage focuses on the awarded CEOs and not on the other executives and directors surrounding the awarded CEOs. Even if director CEOs get more attention because of their affiliations with awarded CEOs, the extra attention is likely to come less from the public media and security analysts, the main drivers of the burden of celebrity, and more from other executives and directors affiliated with the director CEOs (we show in the results section that awarded CEOs receive more attention from media and analysts than director CEOs after their award). The increased attention from public media and security analysts, however, makes it difficult for the awarded CEOs to continue demanding higher compensation and accepting more directorships because it is likely to be portrayed negatively as self-serving behavior (Dyck and Zingales, 2002; Core, Guay, and Larcker, 2008). Indeed, research shows that media attention often acts as a watchdog and information intermediary that reduces self-serving CEO behavior such as value-reducing acquisitions (Liu and McConnell, 2013), corporate governance violations (Dyck, Volchkova, and Zingales, 2008), costly strategic changes (Bednar, Boivie, and Prince, 2013), and lowering the performance-pay sensitivity (Bednar, 2012). The media simultaneously strengthens corporate governance by increasing the threat of dismissal and stigmatization of CEOs (Wiesenfeld, Wurthmann, and Hambrick, 2008; Bednar, 2012).

In sum, the awarded CEOs may benefit from the positive status shift in the form of increased compensation and corporate directorships at the time of the award but higher expectations combined with the increased media scrutiny makes it harder for them to increase their compensation and accept corporate directorships in the years after the award. Although the director CEOs are likely to benefit less from CEO awards than the awarded CEOs at the time of the award, the director CEOs are more likely to continue to benefit from the positive status shift in the form of status ripples in the years after the award because they are burdened less by celebrity. We hypothesize therefore that:

Hypothesis 3: Director CEOs affiliated with awarded CEOs are associated with lower immediate

increases in compensation and higher subsequent increases than the awarded CEOs.

Hypothesis 4: Director CEOs affiliated with awarded CEOs are associated with lower immediate increases in the number of corporate directorships and higher subsequent increases than the awarded CEOs.

The distinction between status spillovers and status ripples also helps to understand why the firms of awarded CEOs tend to perform worse after winning the award compared to before the award (Wade, Porac, Pollock, and Graffin, 2006; Malmendier and Tate, 2009). The difference between status spillovers and status ripples hinges, as noted above, on the ability to constrain the awarded CEOs from exploiting their higher status in ways that could damage their firms. A status ripple is therefore more likely to account for status effects in more visible and controllable areas such as CEO compensation and outside directorships, whereas a status spillover is more likely to account for status effects in less visible and controllable areas such as CEO motivation and effort.

Boards of directors formally control CEO compensation and can, to a great extent, request awarded CEOs to limit their outside directorships (Perry and Peyer, 2005). One reason that awarded CEOs perform poorly, however, is that they can more easily increase other distracting leisure activities outside their core responsibilities including writing books, giving media interviews, and playing golf - awarded CEOs have significantly lower golf handicaps than other CEOs (Malmendier and Tate, 2009). To the extent that awarded CEOs enjoy their new celebrity status, it becomes more difficult for the board to determine how these CEOs spend their time and, of course, even more difficult to control their cognitive focus and effort. Awarded CEOs may therefore gradually and without negative intent start living a costly celebrity life rather than being solely focused on maximizing shareholder wealth. Another reason may be performance-destroying CEO hubris in the form of exaggerated self-confidence or inflated pride fostered by the award and the resulting media attention that ultimately may bias CEO decision-making negatively (Hayward, Rindova, and Pollock, 2004; Wade, Porac, Pollock, and Graffin, 2006; Malmendier and Tate, 2009). Although director CEOs may leverage their affiliations with awarded CEOs professionally and exploit the increased power within their firms, they are unlikely to experience the distraction of becoming a celebrity and succumb to hubris because of the personal nature of CEO awards (Shi, Zhang, and Hoskisson, 2017).

In sum, we argue that the status shifts that follow CEO awards are likely to have negative effects on the firms of the awarded CEOs and the firms of the director CEOs in the form of a negative status spillover because the immediate and subsequent negative effects are likely stronger for the awarded CEOs than the director CEOs. We hypothesize therefore that:

Hypothesis 5: Director CEOs affiliated with awarded CEOs are associated with decreases in firm performance.

Hypothesis 6: Director CEOs affiliated with awarded CEOs are associated with lower immediate and subsequent decreases in firm performance than awarded CEOs.

3 Methods

3.1 Sample

Following prior research on CEO awards (Malmendier and Tate, 2009; Ammann, Horsch, and Oesch, 2016; Shi, Zhang, and Hoskisson, 2017), our sample is comprised of all S&P 1500 firms covered by ExecuComp between 1997 and 2012, and we focus on CEO media awards granted by the Business Week, Financial World, Chief Executive, Forbes, Industry Week, Morningstar.com, Time, Time/CNN, Electronic Business Magazine, and Ernst and Young. All magazines provide nationwide coverage and their awards are not restricted to a specific industry affiliation or certain CEO demographics such as male or female. The awards can be won by CEOs of U.S. companies and the awards are prestigious enough to create a status shift. Winning an award entails that CEOs are publicly presented as superstar managers by the respective magazines, who salute their winners with a hymn of praise in an individual paragraph or even publish a cover story about the awarded CEO. In addition, some of the magazines provide access to supplemental online material such as videos, interviews, or extended in-depth articles about the winners. Celebratory dinners hosted by some of the magazines in compliment to the awarded managers reflect the peak of the external recognition. Awarded CEOs include well known CEOs such as Philip Condit (Boeing, 1997) and Steven Ballmer (Microsoft, 2002) and lesser known CEOs such as Roger Ackerman (Corning, 2000) and Richard Sands (Constellation Brands, 2005).

To identify CEOs that are affected by a status spillover and status ripples, we identify all the board members of firms with an award-winning CEO at the time of the award using ISS Riskmetrics. Riskmetrics covers information on board directors of S&P 1500 firms. From this set of affected directors, we gather the subset of board directors who are simultaneously CEOs of S&P 1500 companies (Director CEOs), as reported by ExecuComp. We focus on these CEOs because the direct affiliation ensures proper recognition by the corporate elite and their focal board of directors and at least allows the CEOs to have contributed to the firm's success.[2] We do not include firms in our analyses that have an awarded CEO on their boards because we cannot distinguish whether the observed outcomes are caused by the status shift or the influence of the awarded CEO. The awarded CEO may, for example, have gained enough power to shape the corporate strategy of the affiliated CEO's firm and to influence the decision on outside directorships or the compensation structure of the affiliated CEO. We also do not include CEOs who are tied to an awarded CEO via a directorship in a third-party board since enough recognition by the corporate elite may not be given in this constellation. These CEOs also cannot contribute to the awarded CEOs success and are less likely to learn from awarded CEOs. In unreported analyses, we found, however, that adding

[2]Graffin, Wade, Porac, and McNamee (2008) suggest that a strong positive signal like a CEO media award does not only confirm a CEO's managerial ability but also the directors' skills in personnel selection. For director CEOs appointed in the awarded CEOs' tenure, we measured an expected contribution to the awarded CEOs' success by analyzing the abnormal announcement returns of the director CEOs' appointments to the awarded CEOs' boards. After excluding all appointments that were subject to confounding events such as earnings announcements, our total sample consisted of 80 director CEOs who joined the boards of future award-winners. Using the market model with the CRSP value-weighted index as a benchmark index to estimate expected returns, we found a positive and significant cumulative abnormal return (observed return minus expected return) over the event windows [-5 +5] (+1.034 percent, $p < 0.05$) and [-3 +3] (+0.862 percent, $p < 0.05$), indicating that the market at least expected the director CEOs to significantly contribute to firm success.

this constellation to our sample of affiliated CEOs confirms the findings of the paper suggesting that simple affiliation may be enough to get affected by status spillovers and ripples.

Our final sample consists of 15,348 firm-year observations with available accounting data (obtained from Compustat), stock returns (obtained from Center for Research in Security Prices/CRSP), CEO data (obtained from ExecuComp), as well as corporate governance data (obtained from ISS Riskmetrics). The final sample contains 1909 unique firms (S&P 1500 constituents change from year to year), 208 awarded CEOs, and 182 CEOs who were directors on the boards of the awarded CEOs' firms at the time of the award.

3.2 Dependent Variables

Compensation: We obtain a CEO's total direct compensation from ExecuComp. Total direct compensation covers the total amount of compensation paid to the CEO in a given year, including salary, bonus, other annual compensation, the total value of restricted stock grants, the total value of stock options granted (using Black-Scholes), long-term incentive payouts, and all other miscellaneous forms of compensation. Following prior research, we take the natural logarithm of the total amount of compensation (*Total Compensation*) to reduce skewness and to avoid biases due to extreme outliers (Graffin, Wade, Porac, and McNamee, 2008; Boivie, Bednar, Aguilera, and Andrus, 2016).

Directorships: We count the number of outside directorships (*Directorship Count*) a CEO fills in S&P 1500 firms in a particular year using ISS Riskmetrics. Although CEOs may have additional directorships in smaller (non-S&P) firms, prior research highlights that board mandates in S&P 1500 firms are the most reputable and more time-consuming than less prestigious directorships (Davis, 1993; Palmer and Barber, 2001). We assign a value of zero directorships to CEOs who are not listed in ISS Riskmetrics in the respective year. Applying this procedure, our average amount of directorships is in line with recent related studies (see, e.g., Geletkanycz and Boyd, 2011). In addition, we verified the accuracy of our measure by randomly selecting 100 directors of our sample. Specifically, we hand-collected the directors' total numbers of directorships and found that 96 percent of all zeros were accurate. The correlation between S&P1500 and overall directorships (average value of 1.04) was 0.9.

Firm performance: We investigate both the immediate and long-term effects of status shifts on firm performance using cumulative abnormal stock market returns (*CAR*) (Malmendier and Tate, 2009; Ammann, Horsch, and Oesch, 2016). A firm's CAR over a certain time period (event window) is the cumulated difference between the actual return and the predicted return over that period. The difference reflects the extent to which an event affects the value of the firm (Brown and Warner, 1985). For instance, if the announcement of a CEO award conveys additional information about the CEO's expected higher level of distraction, which may negatively affect future cash flows, then a negative abnormal stock price return should be observed around the event date (and beyond if additional news related to the event is made public).

We use Lexis Nexis to search for the exact announcement dates of the awards. We apply

a market model estimation to calculate expected returns for each event and firm. Specifically, we regress daily firm returns on a benchmark index (CRSP value-weighted index) and use the estimated parameters to predict the normal return of firms around and after the event date (Brown and Warner, 1985). Following prior research (Malmendier and Tate, 2009; Ammann, Horsch, and Oesch, 2016), we estimate the parameters of the market model based on an estimation period of 755 trading days, starting 778 days before the event date. To make sure that our results are not affected by an inappropriately long estimation period, we verified the robustness of our results with parameters estimated over 255 trading days. If an award is announced on a weekend or a holiday, we consider the next available trading day as the event date.

We use short- and long-term event windows as suggested by related studies (Malmendier and Tate, 2009; Ammann, Horsch, and Oesch, 2016). Specifically, to measure immediate market responses, we investigate an 11-day event window, [-5 +5], which is centered around the announcement date, starting 5 days prior to the announcement and ending 5 days after the announcement. Malmendier and Tate (2009) argue that choosing a rather long event window might be most appropriate to study reactions to award announcements because an identification of the exact time the subscribers of the magazines receive the award notification is not possible. To mitigate concerns about distortions, we re-estimated the immediate announcement effects over a shorter time period of 7 days, [-3 +3]. Results are similar in magnitude and significance. With respect to the long-term performance, we investigate periods over one year ([+6 +255]), two years ([+6 +510]), and three years ([+6 +765]) following the announcement date. We repeated all analyses using Fama and French (1993) and Carhart (1997) return models and found results similar to the reported findings.

3.3 Independent Variables

Status shifts: Following prior research on CEO awards (Wade, Porac, Pollock, and Graffin, 2006; Malmendier and Tate, 2009; Shi, Zhang, and Hoskisson, 2017), we created binary variables that indicate the time passed since a status shift. *Awarded CEO (award year)* is equal to one if a CEO received an award in the current year. *Awarded CEO (post-award period)* is equal to one if a CEO won at least one award in the past four years (Shi, Zhang, and Hoskisson, 2017). Binary variables that capture the average effects over several years are appropriate for studying award induced changes in directorships and compensation because these changes are rare, happen at different times relative to an award, and the effect of an award likely extends beyond the award year (Wade, Porac, Pollock, and Graffin, 2006; Malmendier and Tate, 2009). We follow the same approach to capture the effect of CEO awards on director CEOs by creating binary variables that indicate the time passed since a director CEO experienced an affiliated CEO's status shift by winning an award. *Director CEO (award year*) is equal to one if a status shift happened in the current year to a CEO who is on the awarded CEO's board of directors and *Director CEO (post-award period)* is equal to one if a status shift happened in the past four years. In supplementary analyses, we measured our independent variables using post-award periods of five years (Wade, Porac, Pollock, and Graffin, 2006) and the entire sample period (Malmendier and Tate, 2009) subsequent to winning an award. The results were unchanged.

In the firm performance analyses used to test hypothesis 5 and hypothesis 6, *Director CEO* is a binary variable equal to one if a CEO experienced a status spillover and, depending on the analysis, zero if a CEO is either a matched CEO with the same probability of being director of an awarded CEO (hypothesis 5) or an awarded CEO (hypothesis 6).

3.4 Control Variables

To isolate the effect of a CEO status ripple and spillover on dependent variables, we add several firm- and CEO-level control variables to our regressions. We control for firm size (*Size*) by taking the natural logarithm of total assets. We include firm performance using the last year's *ROA* because CEOs who work for better performing firms get higher compensation packages and are also more likely to get appointed as board members by other firms (Kaplan and Reishus, 1990). Further, we add *Tobin's Q* which is calculated as a firm's market value divided by book value of equity as a measure of a firm's growth potential (Wright, Ferris, Sarin, and Awasthi, 1996) and control for the CEO's tenure (*Tenure*), age (*Age*), and gender (*Female*) as these attributes are related to managerial compensation (Westphal and Zajac, 1993; Zajac and Westphal, 1994; Hambrick and Finkelstein, 1995) and the number of directorships (Bilimoria and Piderit, 1994b,a; Ocasio, 1994; Westphal and Stern, 2007; Adams and Ferreira, 2009; Hillman, Shropshire, Certo, Dalton, and Dalton, 2011). We also include the square of tenure (*Tenure squared*) when running regressions on directorship count and compensation (Wade, Porac, Pollock, and Graffin, 2006). Controlling for tenure and age also addresses the concern that awarded CEOs have reached the peak of their careers, whereas director CEOs are still ascending. On the board level, we control for *Board independence* by including a dummy variable equal to one if a board has 75 percent independent directors (75 percent quantile).

We also control for CEO entrenchment by using the entrenchment index (*E-Index*), developed by Bebchuk, Cohen, and Ferrell (2008) and recently investigated by management scholars (Kang and Kroll, 2013). The E-Index comprises six corporate governance and bylaw provisions which are argued to weaken shareholder rights and protect executives from being removed: (1) staggered boards, (2) poison pills, (3) golden parachutes, (4) supermajority voting requirements, and restrictions on shareholders' ability to carry (5) charter as well as (6) bylaw amendments. For each provision ISS Riskmetrics provides a binary variable that is equal to one if the firm has implemented the respective provision and zero otherwise. We assign each CEO the sum of all binary variables. A higher E-Index value reveals weaker shareholder rights, whereas a lower value is associated with stronger shareholder rights. Finally, we include lagged (one year) values of our dependent variables directorship count (*lagged directorship count*) and total compensation (*lagged total compensation*).

3.5 Analysis

To analyze CEO compensation, we used Feasible Generalized Least Squares (FGLS) regressions with firm-level heteroscedasticity that allow observations to be influenced by common firm-level factors (Greene, 2011). For robustness purposes, we adopted the FGLS models with an AR1

autocorrelation structure and ran fixed-effects OLS regression models with clustered standard errors at the firm level. Clustering standard errors accounts for any bias due to residuals being correlated across observations (Petersen, 2009). To analyze board appointments, we use a Poisson regression with fixed effects, which is ideally suited to deal with our discrete count-based outcome variable (*Directorship Count*), and clustered standard errors at the firm level. The results are robust to clustering at the CEO level. We apply both a deviation- and a Pearson goodness-of-fit test to verify the appropriateness of a Poisson regression model. We chose to employ a fixed-effects model rather than a random-effects model after running Hausman (1978) specification tests ($p < 0.001$). For robustness purposes, we used Negative Binomial regressions and FGLS regressions with firm-wise heteroscedasticity and an independent as well as an AR1 autocorrelation structure. In the FGLS regressions, we used the natural logarithm of directorship count instead of the absolute numbers because we cannot directly account for the Poisson distributed outcome variable. We also used generalized estimating equations (GEEs) with robust standard errors and the Poisson link family with and without AR1 structures (Liang and Zeger, 1986; Ballinger, 2004). All robustness approaches confirmed the results of this study. We conduct Wald-tests to test for significant differences in the coefficient estimates of awarded CEOs and director CEOs.

To illustrate the impact of a status spillover on performance (hypothesis 5), we employ an event study design that investigates the cumulative abnormal returns of awarded CEOs and director CEOs around the announcement date of a CEO award (MacKinlay, 1997; McWilliams and Siegel, 1997). Specifically, we run regressions of abnormal returns without regressors and present the results in supplementary figures. We additionally employ a matching procedure that compares the changes in the performance of firms whose CEOs are treated by a status spillover to changes in the performance of firms whose CEOs have the same likelihood of being treated but were not treated. As suggested by prior related literature (Malmendier and Tate, 2009; Ammann, Horsch, and Oesch, 2016; Shi, Zhang, and Hoskisson, 2017), this procedure reinforces a causal relationship between status spillover and subsequent firm performance if the estimates are distorted by unobserved heterogeneity, omitted time trends, or a selection bias (Roberts and Whited, 2012). A firm's performance may, for example, change over time due to a mean reversion process that would lead to a spurious relationship between an award and performance. In our setting, treated firms are companies with CEOs who sit on the board of an award-winning CEO's firm at the time of the awarding. We identify a treatment firm's control firm by applying a nearest neighbor propensity score matching (Rosenbaum and Rubin, 1983).

Following Shi, Zhang, and Hoskisson (2017), we identify control firms in a two-step procedure. First, we run a logit regression where the dependent variable is a dummy variable equal to one if a CEO holds directorship in a firm whose CEO received an award in the respective year and zero otherwise. We include pre-event information on firm size, firm performance (ROA), Tobin's q, a CEO's age, gender, tenure, and the number of directorships as predictors. We additionally control for industry-fixed and year-fixed effects. Second, we estimate the probability of being treated, i.e., the propensity score, for all ExecuComp firms with available matching variables. To ensure least biased propensity scores, we did not exclude firms with CEOs who have no outside directorships although their theoretical likelihood of being affected is zero. However, we only considered a firm

to be a possible match if their CEO had at least one outside directorship. To ensure a proper identification and least biased estimates, we select a single but best matching firm for each treated firm in the respective event year based on the closest propensity score and allow firms to be a control firm for more than one treated firm (Roberts and Whited, 2012). We run pooled OLS regressions with treated and control firms. We include control variables as well as industry- and year-fixed effects and cluster standard errors at the firm level.

Finally, we run pooled OLS regressions with industry-fixed effects, year-fixed effects, and clustered standard errors at the firm level for the comparison between the performance of an awarded CEO and a director CEO who was affected by status spillover (hypothesis 6). It is important to note that we examined the variance inflation factor in all the above-mentioned regressions, but did not find any evidence that multicollinearity would cause problems. As a precaution, we followed Zhu and Westphal (2014) and dropped variables from each pair of highly correlated variables (e.g., Tenure and Tenure squared). The findings remained unchanged suggesting that multicollinearity is not an issue in our analyses.

Table 1.1 reports descriptive statistics and bivariate correlations for the variables used to test hypotheses 1 to 4. Table 1.2 presents descriptive statistics for firms of affected CEOs and a matched sample used to test hypothesis 5. Simple pairwise t-tests of the treatment indicator on observables prove a high matching quality because the groups do not differ along the matching variables. Table 1.3 reports descriptive statistics and bivariate correlations for the variables used to test hypothesis 6.

Table 1.1: Descriptive Statistics for Data Used to Test Hypotheses 1 to 4

	Mean	**S.D.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**
1 Director CEO (award year)	0.01	0.11							
2 Awarded CEO (award year)	0.01	0.12	0.05						
3 Director CEO (post-award period)	0.02	0.15	0.09	0.04					
4 Awarded CEO (post-award period)	0.03	0.16	0.03	0.13	0.17				
5 Directorship count	1.17	0.83	0.16	0.03	0.15	0.06			
6 Lagged directorship count	1.21	0.86	0.15	0.03	0.12	0.05	0.65		
7 Total compensation	8.09	1.11	0.09	0.07	0.15	0.13	0.17	0.19	
8 Lagged total compensation	7.11	2.78	0.03	0.06	0.04	0.05	0.28	0.11	0.29
9 Size	7.82	1.73	0.11	0.11	0.18	0.22	0.21	0.20	0.55
10 Board independence	0.30	0.46	-0.02	-0.01	0.03	0.02	0.04	0.04	0.15
11 Tenure	7.22	7.45	-0.01	0.02	-0.03	-0.01	0.09	-0.03	-0.08
12 Tenure squared	107.63	237.33	-0.01	0.01	-0.03	-0.01	0.01	-0.06	-0.10
13 ROA	0.05	0.11	0.01	0.04	0.01	0.04	0.04	0.06	0.08
14 E-Index	2.54	1.32	-0.03	-0.04	-0.02	-0.06	0.01	0.02	0.04
15 Tobin's Q	3.48	47.97	0.00	0.01	0.00	0.01	-0.00	0.00	0.00
16 Female	0.02	0.15	0.04	0.02	0.06	0.04	0.03	0.02	0.00
17 Age	55.83	7.10	0.03	0.00	0.02	0.00	0.19	0.06	0.01
	8.	**9.**	**10.**	**11.**	**12.**	**13.**	**14.**	**15.**	**16.**
9 Size	0.20								
10 Board independence	0.10	0.14							
11 Tenure	0.25	-0.10	-0.08						
12 Tenure squared	0.10	-0.09	-0.10	0.91					
13 ROA	0.08	-0.02	-0.00	0.05	0.03				
14 E-Index	0.02	-0.01	0.13	-0.09	-0.09	-0.02			
15 Tobin's Q	0.00	-0.01	0.01	0.02	0.02	-0.00	-0.01		
16 Female	-0.02	-0.03	0.03	-0.04	-0.03	-0.01	-0.00	-0.01	
17 Age	0.16	0.10	-0.03	0.41	0.39	0.03	-0.02	-0.01	-0.07

Note. n = 15,384.

Table 1.2: Descriptive Statistics for Data Used to Test Hypotheses 5

	Director CEOs (DC) (n = 182)			Director CEOs (DC) (n = 182)			Difference in means (n = 364)
	Mean	Median	S.D.	Mean	Median	S.D.	p(DC-CC)
Tobin's Q	4.973	3.769	13.624	4.079	2.936	4.381	0.400
ROA	0.052	0.053	0.090	0.055	0.047	0.057	0.710
Size	9.496	9.484	1.723	9.391	9.618	1.661	0.551
Age	57.604	59.000	5.522	58.028	59.000	5.942	0.482
Female	0.071	0.000	0.258	0.050	0.000	0.217	0.380
Tenure	6.700	6.000	6.205	7.231	6.000	6.222	0.413
Directorship count	2.368	2.000	1.152	2.473	2.500	1.260	0.410
Board independence	0.165	0.000	0.372	0.203	0.000	0.404	0.345
E-Index	2.165	2.000	1.233	2.291	2.000	1.394	0.360

Table 1.3: Descriptive Statistics for Data Used to Test Hypothesis 6

	Mean	S.D.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
1 CAR [-5; +5]	-0.01	0.07											
2 CAR [+6; +255]	-0.13	0.44	0.20										
3 CAR [+6; +510]	-0.26	0.67	0.18	0.84									
4 CAR [+6; +765]	-0.41	0.84	0.17	0.75	0.92								
5 Director CEO	0.47	0.50	-0.05	0.14	0.14	0.21							
6 ROA	0.07	0.09	0.01	-0.11	0.00	-0.05	-0.17						
7 Tenure	7.66	6.79	0.00	-0.03	-0.01	-0.01	-0.13	0.08					
8 Female	0.06	0.23	0.00	0.07	0.09	0.09	0.06	-0.04	-0.09				
9 Age	56.68	6.97	0.02	-0.01	0.02	0.07	0.13	-0.01	0.30	-0.25			
10 Size	9.50	1.72	-0.01	0.04	0.11	0.14	0.00	-0.12	-0.13	-0.19	0.27		
11 Board independence	0.17	0.37	0.02	0.07	0.10	0.11	-0.10	0.00	-0.03	0.01	-0.10	0.01	
12 E-Index	2.09	1.36	0.00	-0.01	-0.04	-0.04	0.05	-0.02	-0.03	0.04	-0.03	-0.21	0.11

Note. n = 390.

4 Results

Table 1.4 presents results from FGLS regressions for total compensation. Model 1 is the control model. Model 2 and model 3 show the effects of CEO awards on compensation for awarded CEOs and director CEOs in the award year and the post-award period, respectively. We discuss the findings based on model 4, which combines model 2 and model 3. Model 4 shows that awarded CEOs receive an increase in compensation in the award year ($\beta = 0.24$, $p < 0.001$), whereas director CEOs affiliated with the awarded CEOs do not receive an increase in compensation in the year of the award ($\beta = 0.05$, $p > 0.1$). The difference in the coefficient estimates between awarded CEOs and director CEOs in the year of the award is statistically significant ($p < 0.001$) and suggests that awarded CEOs are associated with a 22.4 percent higher increase in compensation. In the post-award period director CEOs ($\beta = 0.11$, $p < 0.001$), but not awarded CEOs ($\beta = -0.01$, $p > 0.1$), receive a statistically significant increase in compensation. The increase for director CEOs is significantly higher (12.1 percent, $p < 0.01$) than the insignificant aggregate increase received by the awarded CEOs. Hypothesis 1, which suggested that director CEOs affiliated with awarded CEOs are associated with increases in compensation, and hypothesis 3, which suggested that director CEOs are associated with lower immediate and higher subsequent increases in compensation than awarded CEOs, are therefore supported.

Table 1.5 presents fixed-effects Poisson regression results for directorship count. Model 5 is the control model. Model 6 and model 7 show the effects of CEO awards on the number of directorships for awarded CEOs and director CEOs in the award year and the post-award period,

Table 1.4: CEO Total Compensation

	Model 1	Model 2	Model 3	Model 4
Lagged total compensation	0.057***	0.055***	0.055***	0.055***
	(0.001)	(0.001)	(0.001)	(0.001)
Size	0.400***	0.397***	0.398***	0.396***
	(0.002)	(0.002)	(0.002)	(0.002)
Board independence	0.090***	0.093***	0.091***	0.092***
	(0.007)	(0.007)	(0.007)	(0.007)
Tenure	-0.007***	-0.006***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Tenure squared	-0.000†	-0.000*	-0.000*	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
ROA	0.722***	0.732***	0.729***	0.729***
	(0.041)	(0.041)	(0.041)	(0.041)
E-Index	0.051***	0.051***	0.051***	0.051***
	(0.003)	(0.003)	(0.003)	(0.003)
Tobin's Q	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.024	0.033*	0.031†	0.032*
	(0.017)	(0.015)	(0.016)	(0.014)
Age	-0.006***	-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Awarded CEO (award year)		0.251***		0.241***
		(0.026)		(0.028)
Director CEO (award year)		0.054		0.047
		(0.038)		(0.038)
Awarded CEO (post-award period)			-0.006	-0.008
			(0.028)	(0.028)
Director CEO (post-award period)			0.107***	0.107***
			(0.030)	(0.030)
Constant	4.747***	4.766***	4.767***	4.781***
	(0.039)	(0.039)	(0.039)	(0.039)
Industry- and year dummies	Yes	Yes	Yes	Yes
△ Awarded CEO/Director CEO (award year)		4.27***		4.11***
△ Awarded CEO/Director CEO (post-award period)			2.67**	2.68**
Wald χ^2	66779.16	58981.94	68412.83	93721.70

Note. n = 15,348; standard errors in parentheses; † $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; two-tailed tests; z-statistics from Wald-tests are reported for △ Awarded CEO/Director CEO (award year) and △ Awarded CEO/Director CEO (post-award period).

respectively. We discuss the findings based on model 8, which combines the estimations of model 6 and model 7. Awarded CEOs do not assume additional directorships in the year of the award ($\beta = -0.01$, $p > 0.1$), whereas director CEOs increase the number of directorships slightly in the award year ($\beta = 0.08$, $p < 0.1$). The difference in coefficient estimates between awarded CEOs and director CEOs is, however, not statistically significant (9.4 percent, $p > 0.1$). In the post-award period, awarded CEOs still do not assume additional directorships whereas director CEOs display a statistically significant increase ($\beta = 0.13$, $p < 0.001$).[3] The post-award increase for director CEOs is significantly higher (14.9 percent, $p < 0.01$) than the insignificant increase experienced by the awarded CEO. Hypothesis 2, which suggested that director CEOs affiliated with awarded CEOs are associated with increases in the number of directorships, is therefore supported. Hypothesis 4,

[3]The results seem to stay in contrast to the findings of Malmendier and Tate (2009) who reported a positive relationship between awarded CEOs and directorships. In unreported analyses, we replicated the findings of Malmendier and Tate (2009) using a dummy variable as dependent variable (unlike our count variable) which was equal to one if a CEO occupied at least five directorships and zero otherwise and defined the post-award period as the entire remaining sample period (unlike our four years). We found that awarded CEOs were indeed positively associated with having more than five directorships but also that the significance disappeared when we included the post-award variable that indicated if a CEO was affected by a status spillover. The results showed that director CEOs had a significantly higher probability of having at least five directorships in the post-award period compared to non-affected- and awarded CEOs.

which suggested that director CEOs are associated with lower immediate and higher subsequent increases in the number of directorships than awarded CEOs, is, however, only partially supported: The subsequent increase in directorships is higher for director CEOs than awarded CEOs but the immediate effect is not different for director CEOs and awarded CEOs.

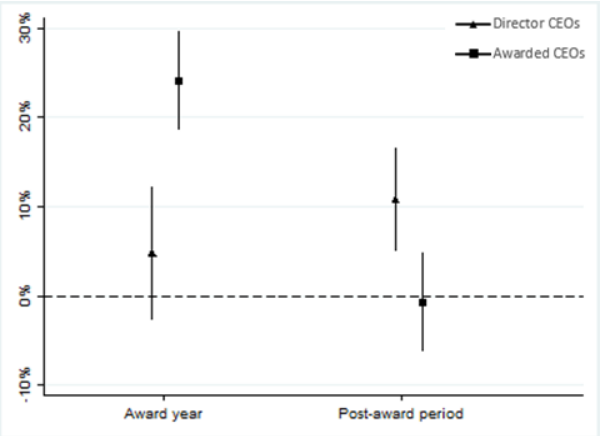
Table 1.5: CEO Directorship Count

	Model 5	**Model 6**	**Model 7**	**Model 8**
Lagged directorship count	0.380***	0.378***	0.378***	0.376***
	(0.009)	(0.009)	(0.009)	(0.009)
Size	0.052***	0.051***	0.049***	0.049***
	(0.005)	(0.005)	(0.005)	(0.005)
Board independence	0.037**	0.038**	0.038**	0.038**
	(0.013)	(0.013)	(0.013)	(0.013)
Tenure	0.039***	0.039***	0.039***	0.039***
	(0.003)	(0.003)	(0.003)	(0.003)
Tenure squared	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
ROA	-0.001	-0.001	-0.005	-0.005
	(0.057)	(0.057)	(0.056)	(0.056)
E-Index	0.019***	0.019***	0.019***	0.020***
	(0.005)	(0.005)	(0.005)	(0.005)
Tobin's Q	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.152***	0.147***	0.138***	0.134***
	(0.041)	(0.041)	(0.040)	(0.040)
Age	0.012***	0.012***	0.012***	0.012***
	(0.001)	(0.001)	(0.001)	(0.001)
Awarded CEO (award year)		-0.011		-0.014
		(0.042)		(0.041)
Director CEO (award year)		0.083*		0.078†
		(0.041)		(0.040)
Awarded CEO (post-award period)			-0.010	-0.008
			(0.031)	(0.030)
Director CEO (post-award period)			0.133***	0.131***
			(0.034)	(0.034)
Constant	-1.669***	-1.664***	-1.638***	-1.634***
	(0.078)	(0.078)	(0.077)	(0.077)
Industry- and year dummies	Yes	Yes	Yes	Yes
△ Awarded CEO/Director CEO (award year)		1.51		1.51
△ Awarded CEO/Director CEO (post-award period)			2.85**	2.79**
Wald χ^2	4890.94	4876.31	5054.86	5030.77

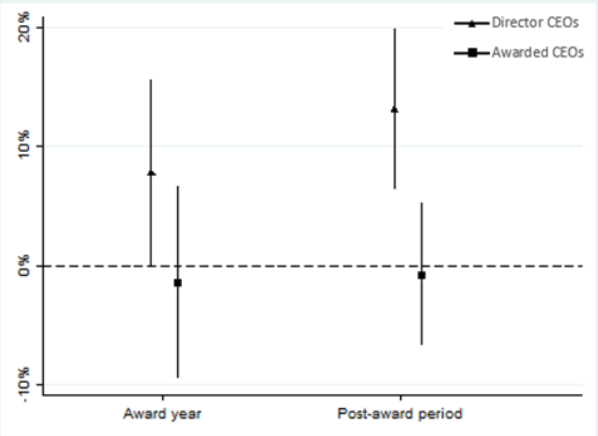
Note. n = 15,348; standard errors in parentheses; † $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; two-tailed tests; z-statistics from Wald-tests are reported for △ Awarded CEO/Director CEO (award year) and △ Awarded CEO/Director CEO (post-award period).

Figure 1.2 illustrates the increase in total compensation (a) and board appointments (b) for director CEOs and awarded CEOs. Figure 1.2 (a) shows that the effects of CEO awards on compensation fully resemble a status ripple: The initial higher increase in compensation for awarded CEOs compared to director CEOs is followed by a higher increase for director CEOs compared to awarded CEOs. Figure 1.2 (b) shows that the effects of CEO awards on directorship count partly resemble a status ripple: The initial higher increase in directorships for awarded CEOs compared to director CEOs is not found (the effects on awarded CEOs and director CEOs are not statistically different), only the subsequent higher increase for director CEOs compared to awarded CEOs. A potential reason for the weaker effects of CEO awards on both the awarded CEOs and director CEOs on directorship count compared to compensation is that acquiring a directorship requires available directorships and typically happens only once a year, thus ensuring immediate effects are less likely.

Figure 1.2: Status Effects on Compensation and Directorships



(a) Percent Increase in Compensation



(b) Percent Increase in Directorships

Table 1.6 reports results of pooled OLS regressions for performance. Model 9 shows negative returns for firms of director CEOs compared to matching firms (control CEOs) around the announcement dates of the award publications ($\beta = -1.4$ percent, $p < 0.1$). Model 10 reports post award performance over a period of one year for director CEOs compared to the matched sample and shows that their firms suffer a performance decrease of 9.6 percent ($p < 0.05$). Model 11 and model 12 show that performance decreases even more over 2 years ($\beta = -19.4$ percent, $p < 0.01$) and 3 years ($\beta = -25.2$ percent, $p < 0.01$). Hypothesis 5, which suggested that director CEOs experiencing status spillovers from awarded CEOs are associated with decreases in firm performance, is therefore supported. In unreported analyses, we found that compared to pre-event levels and the matched sample, the status spillover leads to a decrease in accounting performance (as measured by ROA) by 1.3 percent ($p > 0.1$) over 1 year, 3.0 percent ($p < 0.05$) over 2 years, and 3.2 percent ($p < 0.01$) over 3 years for firms of director CEOs. Thus, the drop in performance may be explained by lower effort that director CEOs put into their firms (cf. Malmendier and Tate, 2009; Ammann, Horsch, and Oesch, 2016).

Table 1.7 presents the comparison between the performance of awarded CEOs and director CEOs. Model 13 shows no significantly different stock market reaction for firms of awarded CEOs and firms of director CEOs following the award announcement ($\beta = -1.0$ percent, $p > 0.1$). Model 14 shows that director CEOs perform not as poorly as awarded CEOs over a period of one year ($\beta = 12.0$ percent, $p < 0.05$). Model 15 and model 16 show that the significant difference in the decreasing performance is more pronounced over 2 years ($\beta = 22.8$ percent, $p < 0.001$) and 3 years ($\beta = 40.6$ percent, $p < 0.001$). Specifically, investigating the cumulative abnormal returns in the second (model 17) and the third year (model 18) following the award, we find that awarded CEOs perform significantly worse by 10.8 percent ($p < 0.01$) and 17.8 percent ($p < 0.001$), respectively, compared to director CEOs. Thus, consistent with hypothesis 6, the negative consequences of CEO awards for firm performance are immediately and subsequently stronger for firms of awarded CEOs than for firms of director CEOs.

Figure 1.3 illustrates the performance effects of awards based on event studies for the subsamples

Table 1.6: Stock Performance of Director CEOs vs. Control CEOs

	Model 9 CAR [-5; +5]	**Model 10 CAR [+6; +255]**	**Model 11 CAR [+6; +510]**	**Model 12 CAR [+6; +765]**
Director CEO	-0.014†	-0.096*	-0.194**	-0.252**
	(0.008)	(0.044)	(0.072)	(0.081)
Tobin's Q	-0.000	-0.002	-0.007	-0.013†
	(0.001)	(0.002)	(0.004)	(0.007)
ROA	-0.068	-1.048*	-1.053	-1.246
	(0.064)	(0.517)	(0.802)	(0.850)
Tenure	-0.000	0.002	0.008	0.008
	(0.001)	(0.004)	(0.006)	(0.007)
Female	0.013	0.189*	0.282†	0.552*
	(0.018)	(0.086)	(0.156)	(0.260)
Age	0.001	-0.000	-0.001	0.006
	(0.001)	(0.005)	(0.007)	(0.009)
Size	0.003	0.009	0.005	0.023
	(0.003)	(0.020)	(0.029)	(0.040)
Board independence	0.016	0.100	0.170†	0.224†
	(0.010)	(0.065)	(0.095)	(0.119)
E-Index	-0.001	0.012	-0.019	-0.003
	(0.003)	(0.019)	(0.028)	(0.038)
Constant	-0.048	0.074	0.106	-0.578
	(0.054)	(0.312)	(0.526)	(0.654)
Industry- and year dummies	Yes	Yes	Yes	Yes
R^2	0.089	0.147	0.162	0.206

Note. n = 364; standard errors in parentheses; † $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; two-tailed tests.

Table 1.7: Stock Performance of Director CEOs vs. Awarded CEOs

	Model 13 CAR [-5; +5]	**Model 14 CAR [+6; +255]**	**Model 15 CAR [+6; +510]**	**Model 16 CAR [+6; +765]**	**Model 17 CAR [+256; +510]**	**Model 18 CAR [+511; +765]**
Director CEO	-0.010	0.120*	0.228***	0.406***	0.108**	0.178***
	(0.008)	(0.048)	(0.069)	(0.084)	(0.041)	(0.036)
Tobin's Q	-0.001	-0.006†	-0.011*	-0.014**	-0.006***	-0.003†
	(0.001)	(0.003)	(0.004)	(0.005)	(0.002)	(0.002)
ROA	-0.001	-0.462	0.166	-0.150	0.628†	-0.316
	(0.070)	(0.486)	(0.737)	(0.757)	(0.333)	(0.270)
Tenure	-0.000	0.004	0.010†	0.012†	0.006†	0.001
	(0.001)	(0.004)	(0.006)	(0.006)	(0.003)	(0.003)
Female	-0.007	0.033	0.131	0.208	0.097	0.077
	(0.014)	(0.122)	(0.222)	(0.320)	(0.122)	(0.112)
Age	0.000	-0.003	-0.006	-0.000	-0.003	0.006†
	(0.001)	(0.004)	(0.006)	(0.008)	(0.003)	(0.003)
Size	-0.002	0.007	0.026	0.054	0.019	0.028†
	(0.003)	(0.021)	(0.031)	(0.039)	(0.016)	(0.015)
Board independence	0.008	0.105†	0.234**	0.299**	0.130**	0.064
	(0.010)	(0.057)	(0.076)	(0.098)	(0.046)	(0.045)
E-Index	0.001	-0.009	-0.039	-0.048	-0.030†	-0.009
	(0.003)	(0.020)	(0.030)	(0.038)	(0.017)	(0.015)
Constant	0.000	0.116	-0.156	-0.955†	-0.272	-0.800***
	(0.045)	(0.294)	(0.508)	(0.572)	(0.268)	(0.219)
Industry- and year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.073	0.137	0.185	0.241	0.209	0.214

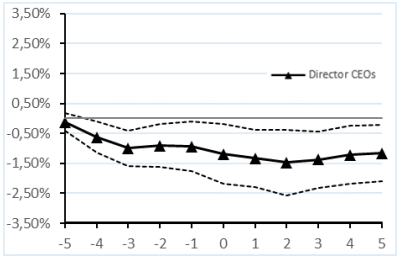
Note. n = 390; standard errors in parentheses; † $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; two-tailed tests.

of firms of awarded CEOs, director CEOs, and control CEOs.[4] Figure 1.3 (a) shows cumulative abnormal returns for firms of director CEOs from 5 days prior to the award announcement to

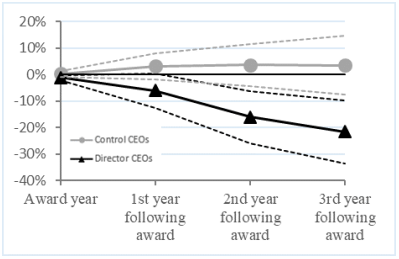
[4]Figure 1.3 is based on event studies that investigate cumulative abnormal returns (CARs) around the announcement date of a CEO award (MacKinlay, 1997; McWilliams and Siegel, 1997). Specifically, we present mean CARs for each subsample of awarded CEOs, director CEOs, and control CEOs. We run regressions of CARs without regressors for each subsample to test for significance and to obtain confidence intervals.

5 days after the award announcement. Figure 1.3 (b) compares the post-award performance of these firms to control firms. Figure 1.3 (c) compares post-award performance of firms of director CEOs to the performance of firms of awarded CEOs. All event windows show that matching firms performed as expected and are not affected by a spillover or an award effect, suggesting that our return models are reliable. In addition, firms of director CEOs suffer a significant decrease in firm value by approximately 1.2 percent ($p < 0.05$), 6.1 percent ($p < 0.1$), 16.1 percent ($p < 0.01$), and 21.7 percent ($p < 0.001$) over a period of 11 days, 1 year, 2 years, and 3 years, respectively. In line with the findings of Malmendier and Tate (2009), awarded CEOs' firms suffer a decrease in value by 0.5 percent ($p > 0.1$), 18.6 percent ($p < 0.001$), 35.1 percent ($p < 0.001$), and 57.7 percent ($p < 0.001$).

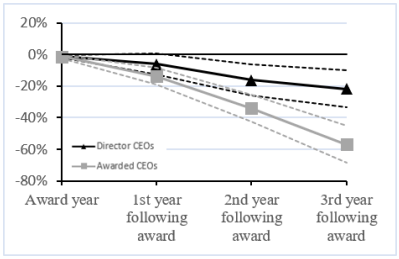
Figure 1.3: Status Effects on Stock Price Performance



(a) CARs Around Award Announcement



(b) Post-Award CARs – Director vs. Control CEOs



(c) Post-Award CARs – Director vs. Awarded CEOs

5 Robustness Checks and Additional Analyses

To mitigate concerns about the matching procedure, used to test hypothesis 5, we ran additional robustness checks. First, we performed out-of-sample tests for significant differences in variables that are not included in the matching procedure. Comparing market capitalization (price x shares outstanding), sales, return on equity (net income divided by book equity), and net operating assets, we found no significant differences across our sample of director CEOs and their predicted counterfactuals. Thus, since our treatment and control samples do not even differ along dimensions that are not explicitly considered in the matching procedure, it is unlikely that they differ along unobservable dimensions (Malmendier and Tate, 2009; Roberts and Whited, 2012). Second, we repeated the matching using two and three neighbors, respectively. While using a best match may lead to the least biased estimates, additional matching firms can produce more precise estimates (Dehejia and Wahba, 2002; Roberts and Whited, 2012). Table 1.8 shows that both specifications confirmed our previous findings: Model 19 to model 22 confirm our main results using two matches and model 23 to model 26 using three matches.

In our hypotheses development, we argue that the personal and organizational outcomes for awarded CEOs differ from the outcomes for director CEOs because awarded CEOs are more likely to experience the burden of celebrity in the form of increased public attention. To demonstrate the difference in external attention, we counted the number of newspaper articles for our sample firms published by the Wall Street Journal between 1997 and 2012. Model 27 in table 1.9 shows that an

Table 1.8: Robustness: Stock Performance of Director CEOs vs. Control CEOs

	Model 19 CAR [-5; +5]	**Model 20 CAR [+6; +255]**	**Model 21 CAR [+6; +510]**	**Model 22 CAR [+6; +765]**	**Model 23 CAR [-5; +5]**	**Model 24 CAR [+6; +255]**	**Model 25 CAR [+6; +510]**	**Model 26 CAR [+6; +765]**
Director CEO	-0.014*	-0.093*	-0.172*	-0.206*	-0.016*	-0.103*	-0.191*	-0.233*
	(0.007)	(0.046)	(0.079)	(0.090)	(0.007)	(0.044)	(0.077)	(0.091)
Tobin's Q	-0.000	-0.002	-0.008†	-0.015*	-0.000†	-0.000***	-0.000***	-0.001***
	(0.001)	(0.003)	(0.005)	(0.007)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	-0.135†	-1.256**	-1.498*	-1.651*	-0.114†	-1.154***	-1.547**	-1.887**
	(0.075)	(0.402)	(0.654)	(0.696)	(0.060)	(0.344)	(0.540)	(0.596)
Tenure	-0.000	-0.000	0.002	-0.001	-0.000	-0.002	-0.002	-0.006
	(0.001)	(0.004)	(0.005)	(0.006)	(0.000)	(0.003)	(0.004)	(0.005)
Female	0.005	0.068	0.130	0.297	0.007	0.042	0.128	0.318†
	(0.015)	(0.065)	(0.125)	(0.197)	(0.011)	(0.051)	(0.102)	(0.170)
Age	0.001	-0.001	-0.001	0.006	0.000	0.001	0.004	0.011†
	(0.001)	(0.004)	(0.006)	(0.008)	(0.001)	(0.003)	(0.005)	(0.007)
Size	-0.001	-0.004	-0.003	0.001	-0.000	0.007	0.006	0.008
	(0.003)	(0.015)	(0.023)	(0.031)	(0.002)	(0.013)	(0.020)	(0.027)
Board independence	0.023*	0.119†	0.160†	0.225*	0.015†	0.083†	0.089	0.131
	(0.010)	(0.061)	(0.084)	(0.102)	(0.008)	(0.046)	(0.071)	(0.089)
E-Index	-0.001	0.004	-0.021	-0.008	0.000	0.007	-0.005	0.014
	(0.003)	(0.015)	(0.023)	(0.031)	(0.002)	(0.012)	(0.018)	(0.025)
Constant	-0.016	0.143	0.104	-0.527	-0.018	-0.066	-0.280	-0.944*
	(0.046)	(0.227)	(0.387)	(0.488)	(0.041)	(0.196)	(0.340)	(0.453)
Industry- and year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.073	0.143	0.153	0.185	0.060	0.149	0.151	0.178
Number of control firms	2	2	2	2	3	3	3	3

Note. n = 546 for models 19 to 22; n = 728 for models 23 to 26; standard errors in parentheses; † $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; two-tailed tests.

award increased average media coverage (average number of articles) by 29.69 percent ($p < 0.01$) in the post award period, whereas media coverage for director CEOs did not change significantly (-10.77 percent, $p > 0.1$). Using data on security analyst coverage from I/B/E/S in model 28, we also found that the firms of awarded CEOs attracted 6.72 percent ($p < 0.05$) more security analysts, which amounted to an increase in the number of analysts for an average firm by one, whereas security analyst coverage for the firms of director CEOs did not increase significantly (2.63 percent, $p > 0.1$).

Table 1.9: Additional Analyses

	Model 27 Number of articles in WSJ	**Model 28 Number of analysts**	**Model 29 Highly Admired (Top 30%)**	**Model 30 Highly Admired (Above median score)**
Size	0.731***	0.236***	1.240***	1.209***
	(0.021)	(0.008)	(0.027)	(0.028)
Board independence	-0.088	-0.038†	-0.094	-0.156*
	(0.060)	(0.020)	(0.063)	(0.069)
Tenure	0.009	0.017***	0.003	0.016
	(0.011)	(0.003)	(0.009)	(0.010)
Tenure squared	-0.000	-0.000***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
ROA	0.885	1.052***	4.088***	8.086***
	(0.704)	(0.150)	(0.480)	(0.596)
E-Index	-0.020	-0.001	0.032	0.066*
	(0.020)	(0.009)	(0.023)	(0.026)
Tobin's Q	0.001***	0.000**	0.000	0.001**
	(0.000)	(0.000)	(0.001)	(0.000)
Female	-0.145	-0.077	-0.309	-0.455*
	(0.135)	(0.077)	(0.195)	(0.219)
Age	-0.011†	-0.009***	0.011**	0.008†
	(0.006)	(0.002)	(0.004)	(0.005)
Awarded CEO (award year)	0.405***	0.129**	0.280	0.551**
	(0.120)	(0.040)	(0.199)	(0.195)
Director CEO (award year)	0.004	0.053	0.566**	0.564**
	(0.107)	(0.036)	(0.212)	(0.216)
Awarded CEO (post-award period)	0.260*	0.065*	0.796***	0.880***
	(0.101)	(0.032)	(0.153)	(0.138)
Director CEO (post-award period)	-0.114	0.026	0.567***	0.411**
	(0.086)	(0.036)	(0.152)	(0.151)
Constant	-2.316***	1.156***	-11.784***	-12.249***
	(0.383)	(0.119)	(0.358)	(0.401)
Industry- and year dummies	Yes	Yes	Yes	Yes
Regression Type	Poisson	Poisson	Logit	Logit

Note. n = 15,348; standard errors in parentheses; † p < .10; * p < .05; ** p < .01; *** p < .001; two-tailed tests.

Finally, we tested whether the firms managed by director CEOs rose in the esteem of the corporate elite, as indicated by the *Fortune Most Admired Companies* survey, following the awards.[5] Fortune surveys approximately 15,000 top executives, directors, and security analysts and ranks firms on a number of different attributes including financial soundness, quality of management, and long-term investment value. We defined a firm as a highly admired firm if the firm was among the top 30 percent most admired firms (the top 300 of the Fortune 1000 firms surveyed). Model 29 in table 1.9 shows that firms of director CEOs showed a significant increase in the likelihood of becoming a highly admired firm (odds ratio: 1.76, $p < 0.001$) following the CEO award (same

[5]Using a similar approach, Pfarrer, Pollock, and Rindova (2010) called their measure "high reputation". We prefer to call our measure "highly admired" because we use the Fortune Most Admired Companies survey only, not also the Wall Street Journal/Harris Interactive Corporate Reputation list. We thank Michael Pfarrer for sharing the Fortune Most Admired Companies data.

results in model 30 (odds ratio: 1.51, $p < 0.01$) when highly admired firm is defined by the firms with an above median admiration rank). Despite not attracting more public attention, the firms of director CEOs are more likely to be highly admired by the corporate elite in the post award period, thus providing some justification for their higher compensation and directorships.

6 Discussion and Conclusion

Moving beyond research on status and status spillovers, we argued in this study that sudden positive status shifts create status ripples, not only status spillovers, when social actors are more constrained from fully exploiting their status advantage than their affiliates. Using prestigious CEO awards from U.S. news magazines for our empirical investigation, we examined the consequences of status shifts for awarded CEOs and affiliated director CEOs. Our results provide evidence of a status ripple paradox: The director CEOs experience longer enduring increases in compensation and the number of directorships after the award, whereas the awarded CEOs themselves only experience an immediate but non-durable rise in compensation and no increase in the number of directorships. The consequences for the awarded- and director CEOs firm performance are different. We found that the firm performance of director CEOs decreases in the aftermath of an award but the decrease is less pronounced than for firms of awarded CEOs. We conclude that simple status spillovers best explain changes in the performance of firms of awarded- and director CEOs, whereas status ripples are more suitable to account for changes in compensation and directorships. Our supplementary analyses confirmed that awarded CEOs are constrained by increased external attention from public media and security analysts. Director CEOs do not experience increased external attention, however, even if their firms are more likely to be highly admired following the spillover.

In addition to the contributions mentioned in the introduction, our study contributes to status and CEO research in other ways. By theorizing the difference between status spillovers and status ripples, we broaden and deepen research on status and status spillover by arguing that the effects of positive status shifts differ depending on the extent to which social actors are constrained from exploiting the status shift. Status ripples represent an important limit to the cumulative advantage of status by emphasizing that the indirect status effects experienced by the affiliated actors can be more important and enduring than the direct status effects experienced by the upwardly mobile actors themselves – the paradox of status ripples. Other research shows that status homophily (Podolny, 1994), status discrimination (Jensen, 2008), status complacency and distraction (Bothner, Kim, and Smith, 2012), status deprivation and status disruption (Jensen and Kim, 2015), and status inconsistency (Jensen and Wang, 2017) limit the self-perpetuating advantage of status. We add that status ripples not only imply that status effects are experienced more broadly than the social actors experiencing status shift, as implied by status spillovers, but also that there can be limits to the cumulative advantage of status because not all social actors can fully exploit their status advantages. Our approach to status ripples and status spillovers provides, in other words, a complement to current status research by broadening and limiting the effects of status.

Second, we extend prior research on the importance of the media in corporate governance by

theorizing status spillovers and status ripples in the context of CEO media awards. Prior research focused on two types of status spillovers: Spillovers to senior executives inside the firm and spillovers to other senior executives outside the firm (Graffin, Wade, Porac, and McNamee, 2008; Ammann, Horsch, and Oesch, 2016). Our study adds that media awards create firm-level status spillovers in the form of negative firm performance and CEO-level status ripples in the form of different compensation and directorship effects for awarded CEOs and director CEOs. By showing that negative implications of CEO media awards are not limited to the awarded CEOs and their firms (Wade, Porac, Pollock, and Graffin, 2006; Graffin, Wade, Porac, and McNamee, 2008; Malmendier and Tate, 2009), we also contribute to research on corporate governance by indicating a broader and more diffuse governance problem. Specifically, our study suggests that the burden of celebrity indeed creates extra media attention that serves as an additional corporate governance mechanism by preventing awarded CEOs from fully exploiting their status advantages to add directorships and increasing compensation. Unfortunately, this governance mechanism seems less effective in counteracting less visible but nevertheless potentially value destroying forms of celebrity behavior. And because our results suggest that director CEOs are less likely to be subject of additional media attention, the burden of celebrity does not prevent them from adding directorships and securing higher compensation for a longer time than awarded CEOs.

Third, our emphasis on status ripples highlights the interdependence of status, director selection, compensation, and firm performance. Prior research highlights that direct positive status shifts and status spillovers serve as a signal of quality for firms, investors, and the corporate elite, equipping managers with increases in compensation and creating a higher demand for these managers in the executive and director labor market (Graffin, Wade, Porac, and McNamee, 2008; Malmendier and Tate, 2009). Our study adds that the corporate elite recognizes status ripples when determining their employment and compensation strategies and highlights that status ripples from positive status shifts cause positive personal consequences. By examining a director CEO's focal firm's performance, our study suggests, however, that the positive personal consequences from status ripples limit the manager's time and effort spent at the focal firm. Consequently, status ripples can also have negative consequences when they are exploited by the affected actors. In this respect, we note that board appointments are not the only firm-unrelated and distracting outside activity that managers could commence following positive status shifts as illustrated by the lower golf handicaps of awarded CEOs (cf. Malmendier and Tate, 2009).

Our study is not without limitations. Our study implies that status spillovers and ripples affect all the affiliates of awarded CEOs but we focus only on corporate directors who are simultaneously CEOs of other companies. In unreported analyses, however, we found that all corporate directors who were affected by status spillovers, accepted additional board appointments in the post-award period. Similar to the findings of Graffin, Wade, Porac, and McNamee (2008), our analyses also revealed that board directors who were not yet CEOs, showed a higher probability of being promoted to CEO positions subsequent to a status spillover. The consequences of a positive status shift, therefore, do not seem to be restricted to a certain group of executives and directors, thus opening up for further research to explore status implications for other affiliates of the awarded CEOs. We also encourage future research to apply our status ripple framework outside the context

of CEOs, executives, and directors. A first step would be to examine whether the effects of CEO awards spill over to firm-level relationships such as buyer-supplier relations and strategic alliances. A second step would then be to move beyond CEO awards and examine spillover effects of firm-level awards and certifications to examine if the status spillover and status ripple generalize beyond the individuals to other types of social actors.

Despite these limitations, our study takes an important first step in theorizing status ripples, distinguishing them from status spillovers, and providing empirical evidence for the status ripple paradox that awarded social actors may benefit less from their positive status shift than the social actors with whom they affiliate. Like a stone skipped across water, bouncing several times until eventually sinking, our study suggests that positive status shifts reverberate throughout social systems, sometimes as status spillovers, other times as status ripples, thus suggesting that positive status shifts are unlikely to always be zero-sum events that only have consequences for the social actors experiencing the status shifts.

Chapter 2

The Effect of Managerial Entrenchment on Competitors: Evidence from Exogenous Shocks

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Abstract: This paper studies the existence of spillover effects of managerial entrenchment on competing firms. To this aim, we exploit an exogenous reduction of managerial entrenchment triggered by sudden deaths of entrenched CEOs. We document significantly positive abnormal announcement returns both for the focal firms and their competitors. The market expectations for competing firms mirror subsequent increases in operating performance, risk taking, and sales margins. We confirm the positive value effects using close votes on entrenchment-related shareholder proposals as an alternative entrenchment shock. The study suggests that competitors react on an expected increase in competitive pressure, which amplifies the positive shareholder value effect of a reduction in managerial entrenchment.

1 Introduction

Managerial entrenchment is a widespread issue in modern corporations and occurs when managerial power outweighs prevailing governance structures, allowing managers to pursue their own interests rather than those of shareholders (Weisbach, 1988). Managerial entrenchment is detrimental to shareholders because it allows managers to extract higher rents and lower their fraction of effort-based compensation (Shleifer and Vishny, 1989; Borokhovich, Brunarski, and Parrino, 1997; Faleye, 2007), reduce the sensitivity of their turnover probability to firm performance (Faleye, 2007), hamper hostile takeovers (Borokhovich, Brunarski, and Parrino, 1997; Bebchuk and Cohen, 2005; Salas, 2010), acquire poor targets (Morck, Shleifer, and Vishny, 1990), and decrease corporate risk for the sole purpose of higher job security (Shleifer and Vishny, 1989; Berger, Ofek, and Yermack, 1997). For these reasons, shareholders respond positively to the reduction of entrenchment, e.g., through sudden deaths of entrenched CEOs (Salas, 2010), and tend to strengthen corporate-governance policies by voting in favor of shareholder proposals that counteract entrenchment (Gillan and Starks, 2000; Ertimur, Ferri, and Stubben, 2010; Renneboog and Szilagyi, 2011; Cuñat, Gine, and Guadalupe, 2012).

Despite the vast strand of literature on managerial entrenchment, an important but unexplored question is whether managerial entrenchment in the focal firm affects competitors. Extant research has shown that competitors serve as a firm's natural information source. By comparing to competitors, firms can reduce complexity and uncertainty in various corporate policy decisions, such as setting up their capital structure (Leary and Roberts, 2014), determining the amount of executive compensation (Bizjak, Lemmon, and Naveen, 2008), and deciding on stock splits (Kaustia and Rantala, 2015) or acquisition strategies (Shue, 2013). However, the ongoing comparison also creates competitive pressure (Ehrenberg and Bognanno, 1990; Ammann, Horsch, and Oesch, 2016). Recent research shows that managers keep an eye on competitors, in particular on their valuation (Foucault and Fresard, 2014), because they are incentivized to respond to their strategic decisions (Ammann, Horsch, and Oesch, 2016) in order to maintain market shares and a good relative performance which, in turn, is of supreme importance for both managerial compensation and the threat of dismissal (DeFond and Park, 1999; Jenter and Kanaan, 2015). If managers face intensifying competition, they are shown to adopt more conservative financial policies (Hoberg, Phillips, and Prabhala, 2014), increase capital expenditures (Foucault and Fresard, 2014; Dessaint, Foucault, Frésard, and Matray, 2016), focus on business areas in which they have a competitive advantage (Li, Winkelman, and D'Amico, 2014; Flammer, 2015b), and increase their effort, efficiency, and risk taking behavior (Ammann, Horsch, and Oesch, 2016).

In this paper, we argue that the positive (value) effect of a reduction in managerial entrenchment on the focal firm serves as an incentive mechanism for competing firms. Providing a causal link between managerial entrenchment and responses of competing firms is empirically challenging. For instance, lower competitive pressure may facilitate managerial entrenchment, but the presence of managerial entrenchment may, in turn, reduce the competitive pressure on other firms. To establish causality, we exploit sudden deaths of entrenched CEOs as an exogenous downward shock to managerial entrenchment. Investigating unexpected deaths is particularly interesting be-

cause, in contrast to both voluntary and forced managerial turnover, they occur randomly and are exogenous to current company characteristics and market conditions (Nguyen and Nielsen, 2010; Salas, 2010; Nguyen and Nielsen, 2014). In accordance with Salas (2010), we consider suddenly-deceased CEOs to be entrenched if the announcement of death is followed by a significant positive abnormal stock-price reaction. Salas (2010) argues that positive stock-price reactions reveal that shareholders would have preferred an earlier removal of the entrenched CEO which was, however, impossible under the prevalent governance structure of the firm. Consequently, a sudden death enables the board to finally choose a better successor (Borokhovich, Brunarski, Donahue, and Harman, 2006) who is, at least in the beginning, less powerful and thus less likely to extract private benefits but more likely to focus on the firm's success (Hambrick and Fukutomi, 1991; Hill and Phan, 1991; Shen, 2003). To measure the influence of an entrenched CEO's sudden death on competitors, we study stock-market reactions around the death announcement by running event studies and investigate subsequent changes in the competitors' operating performance and strategy using difference-in-differences (DiD) regressions. Within the DiD design, we compare pre- and post-period characteristics of the treatment group, i.e., competitors of firms that were subject to the sudden death of an entrenched CEO, to those of a control group. We consider all deaths of entrenched CEOs in S&P1500 firms between 1993 and 2014. Competitors are S&P1500 firms that belong to the same 4-digit Standard Industrial Classification (SIC) code. Our control group consists of all remaining S&P1500 firms.

Our results show that the removal of an entrenched manager has important wealth implications for both the focal firms and their competitors. We find that competing firms exhibit significant cumulative abnormal returns (CARs) of 4.7% over a five-day event window, which is similar to the wealth effect experienced by the focal firm with CARs of 4.6%. Additionally, competitors experience a rise in operating performance, which suggests that the sudden death of an entrenched CEO has an incentive effect on them. By studying potential channels through which a boost in value and performance is achieved, we find that competitors experience a significant rise in total equity risk as measured by stock-return volatility. In addition, we document increases in idiosyncratic risk and R&D intensity, which suggest that the rise in total equity risk likely reflects investments in riskier projects (Coles, Daniel, and Naveen, 2006; Bargeron, Lehn, and Zutter, 2010; Li, Griffin, Yue, and Zhao, 2013; Roussanov and Savor, 2014; Ammann, Horsch, and Oesch, 2016). We also find an improvement in the competitors' sales margins and a reduction of costs of goods sold. Combined with negative stock-market reactions of supplier and customer industries upon the announcement of the entrenched CEOs' deaths, these findings suggest that competitors put effort in becoming more profitable, e.g., by renegotiating contract conditions. We trace back our empirical findings to the attempt to react on and to catch up with a competitor who is expected to perform better after the exogenous removal of an entrenched CEO. The results withstand a large battery of robustness checks, such as using alternative measures of entrenchment and competitor classification schemes, different event windows, a placebo test with shifted event dates, a characteristic-based matching procedure for the control firms, and the inclusion of observable firm and industry characteristics.

To confirm the positive value effects of a reduction in managerial entrenchment for competitors, we use an alternative and more frequent exogenous shock triggered by close votes on shareholder-

sponsored proposals. Recent studies show that close votes, i.e., proposals that fail or pass by a small margin (e.g., 49% or 51%), on various shareholder-sponsored governance proposals are exogenous to a firm's governance structure because the outcome can hardly be anticipated and is also uncorrelated with firm characteristics (Cuñat, Gine, and Guadalupe, 2012; Flammer, 2015a). We focus on a large set of proposals related to the entrenchment index (E-Index) of Bebchuk, Cohen, and Ferrell (2008) and examine the impact of their vote outcomes on the stock prices of focal and competing firms. Our results show that, on the day on which an E-Index related governance proposal passes by a small margin of votes (falling within 10 percentage points of the majority threshold), the focal firms' abnormal returns are, on average, 0.79 percentage points (p.p.) higher compared to firms with failed proposals. Similar as for the sudden death of an entrenched CEO, the passing creates a positive spillover effect on competitors which simultaneously experience an increase in stock prices by 0.43 p.p. compared to competitors of firms with failed proposals. The results are robust to a large set of robustness checks, such as restricting the sample to narrower vote margins, applying alternative return models, using different degrees of polynomials in the estimation of the treatment effect, and applying alternative definitions of competitors.

This paper contributes to the literature on managerial entrenchment, CEO turnover, shareholder activism, and product-market competition. First, whereas existing empirical work examines wealth effects of CEO deaths (e.g., Johnson, Magee, Nagarajan, and Newman, 1985; Worrell, Davidson, Chandy, and Garrison, 1986; Etebari, Horrigan, and Landwehr, 1987; Salas, 2010) and the consequences of managerial entrenchment for the focal firm (Shleifer and Vishny, 1989; Berger, Ofek, and Yermack, 1997; De Jong and Veld, 2001; Almazan and Suarez, 2003; Faleye, 2007; Salas, 2010), our paper studies the impact of entrenchment shocks through CEO deaths on the value, performance, and strategic behavior of competing firms. By switching the focus from the focal firms to their competitors, our study shows that the reduction of managerial entrenchment is not only positive for the focal firm but creates spillover effects to other firms in the same competitive environment. By showing that the direct effect measured for the focal firms and the spillover effects to competitors are of similar magnitude and influence multiple firms, our paper evidences that managerial entrenchment in one firm destroys a significant share of overall shareholder wealth. Second, our findings complement the literature on managerial risk taking and effort by identifying competitors as an additional potential driver. By investigating potential channels that competitors use to withstand an expected increase in competitive pressure, we also add to recent work on supplier and customer relationships which documents that a customer's CEO turnover represents a disruptive event that is typically followed by a decline in the suppliers' firm values (Intintoli, Serfling, and Shaikh, 2016). Using an alternative identification scheme of customer and supplier firms (Shahrur, 2005) along with a clear exogenous shock to managerial entrenchment, we confirm that the turnover of an entrenched customer's CEO leads to a decrease in the suppliers' firm values. We add to this strand of research that the turnover of an entrenched supplier's CEO leads to a similar decrease in the customers' firm values, suggesting that renegotiations in customer-supplier relationships are bidirectional. Third, our study extends the understanding of the importance and value of shareholder activism (see, e.g., Cuñat, Gine, and Guadalupe, 2012; Flammer, 2015a; Gillan and Starks, 2015). Using vote outcomes of entrenchment-related shareholder proposals as an additional exogenous shock, we provide causal evidence of positive externalities on competitors' shareholder

wealth triggered by shareholder activism in focal firms. Fourth, from a methodological perspective, we contribute the use of shareholder proposals as a natural experiment for investigating causal effects of managerial entrenchment on competitors.

2 Empirical Strategy and Data

2.1 Identification of an Entrenchment Shock

To identify changes in managerial entrenchment, we exploit sudden deaths of entrenched CEOs. By definition entrenched CEOs can hardly be removed by the board of directors (Borokhovich, Brunarski, Donahue, and Harman, 2006; Salas, 2010) and their voluntary turnovers are endogenous decisions. In contrast, turnovers through sudden deaths are not endogenously chosen by the firm or the CEO and their timing cannot be anticipated, making them exogenous to the focal firms and especially their competitors (Worrell, Davidson, Chandy, and Garrison, 1986; Fee, Hadlock, and Pierce, 2013). Following Salas (2010), we consider suddenly-deceased CEOs as entrenched if the stock-market reaction around the announcement of death is positive. First, Salas (2010) argues that positive stock-price reactions reveal that shareholders would have preferred an earlier removal of the entrenched CEO. This argument complies with the empirical finding that positive stock-price reactions to CEO deaths mainly arise if the deceased CEO exhibited a persistent underperformance combined with a long tenure (Salas, 2010). In addition, many of these CEOs were the firm's founders who are powerful enough to entrench themselves (Johnson, Magee, Nagarajan, and Newman, 1985; Worrell, Davidson, Chandy, and Garrison, 1986; Hayes and Schaefer, 1999; Adams, Almeida, and Ferreira, 2005). Second, in contrast to other previously used managerial entrenchment proxies, such as age and tenure (Intintoli, Serfling, and Shaikh, 2016), stock-price changes account for valuable experience and inter-organizational networks of the CEO (Davies and Easterby-Smith, 1984; Guthrie and Datta, 1997). Indeed, Salas (2010) shows that young CEOs and CEOs with a short tenure perform worse than more matured CEOs and CEOs with a long tenure, which indicates that older and tenured CEOs may not necessarily be entrenched. Finally, positive market reactions reflect increased expectations about the future performance of the firm, which should arguably be better after managerial entrenchment is reduced.

To identify positive stock-price reactions, we compute CARs over a five-day event window [-2 +2], centered around the CEO death announcement which we consider as the event date. If it lies on a weekend or a holiday, we use the next available trading day. The window allows us to capture possible information leakage and delays of news articles around the day of death. We find similar results by requiring the stock-market reaction of the decease-announcing firms to be positive over a three- and a seven-day event window centered around the announcement date. CARs are the sum of daily abnormal returns over the respective event window. Daily abnormal returns are computed as the difference between daily realized returns and daily expected returns. We compute expected returns using the market model with parameters estimated over 250 trading days ending 10 days prior to the announcement of the CEO death and CRSP value-weighted index as benchmark. Our sample of entrenched CEOs does not change when using the Carhart (1997) four-factor model

instead of the market model.

2.2 Sample of Entrenched CEOs

We focus on S&P1500 firms between 1993 and 2014 and collect a complete list of CEO changes together with relevant information, such as CEO tenure and age. We manually check in LexisNexis whether each CEO change is due to a regular (forced or voluntary) turnover or death. In particular, we hand-collect all events of deaths for which we find news articles indicating that the market was surprised and that the death was unexpected. Following prior research on CEO deaths (Nguyen and Nielsen, 2010; Salas, 2010; Nguyen and Nielsen, 2014), we search for keywords including 'heart attack', 'stroke', 'accident', 'sudden', 'unexpected', and 'suddenly' along with the words 'chief executive' or 'CEO', as well as the CEOs' full or last name. We also include events for which the specific reason of death is unreported, but the death is described as unexpected, unanticipated, or sudden. Based on this procedure, we identify 34 instances of sudden CEO deaths. This number is similar to previous studies that make use of the same exogenous shock (e.g., Johnson, Magee, Nagarajan, and Newman, 1985; Worrell, Davidson, Chandy, and Garrison, 1986; Hayes and Schaefer, 1999; Latif, Strickland, and Yang, 2011). By applying our measure of entrenchment, we classify 21 events as sudden deaths of entrenched CEOs. According to Compustat's 4-digit (and the narrower 3-digit) SIC codes, these firms operate in 20 different industries: 14 in the manufacturing sector, one in wholesale trading, three in the field of financial services and insurance, and three in the health- and business-services segment. The CEO deaths do not coincide with other firm-specific events or idiosyncratic industry shocks and is evenly distributed over our sample period (with a maximum of two instances in one year).

Table 2.1: Summary Statistics of Sudden CEO Deaths

This table reports descriptive statistics of sudden CEO deaths. Panel A presents average characteristics of the deceased CEOs. Panel B presents characteristics of the deceased CEOs by entrenchment. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. *CEO age* and *CEO tenure* provide a managers' age and tenure at the time of death. *Founder* is a dummy that equals one if the CEO was also the company's founder and zero otherwise. *3Y-Alpha* is the intercept of a market model regression for a three-year period prior to the death. *Negative 3Y-Alpha* is a dummy variable equal to one if *3Y-Alpha* is negative and zero otherwise. *Abnormal returns* are measured over a five-day event window [-2 +2] using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. *Alternative entrenchment* is an alternative entrenchment proxy, which equals one if the suddenly-died manager had a tenure of more than 10 years and a negative three-year alpha (*3Y-Alpha*).

Panel A: CEO Characteristics	**N**	**Mean**	**Median**	**Q1**	**Q3**
CEO age	33	64.27	63.00	58.00	68.00
CEO tenure	33	14.39	11.00	4.00	17.00
Founder	34	0.26	0.00	0.00	1.00
3Y-Alpha (in %)	31	0.02	0.02	-0.02	0.05
Abnormal returns (in %)	31	2.85	2.01	-0.64	5.25
Panel B: CEO Characteristics by Entrenchment		**Mean of Non-Entrenched CEOs (N=10)**	**p-value**	**Mean of Entrenched CEOs (N=21)**	
CEO age		60.40	0.034	67.30	
CEO tenure		12.20	0.217	16.85	
Founder		0.20	0.231	0.33	
Negative 3Y-Alpha		0.00	0.002	0.52	
Alternative entrenchment		0.00	0.049	0.24	

Table 2.1 reports summary statistics. Panel A summarizes descriptive statistics of all deceased CEOs and their firms. Their mean age and tenure are 64.3 and 14.4 years, respectively, with a correlation of approximately 0.76. In slightly more than 25% of the cases, the CEOs are also the firms' founders. The average three-year (daily) alpha computed by the market model is 0.02% and the average five-day ([-2 +2]) abnormal return upon a CEO's sudden death amounts to 2.9%. Panel B compares firm and CEO characteristics of entrenched and non-entrenched CEOs. In line with prior literature (see, e.g., Salas, 2010, for a summary), entrenched CEOs are on average older (67.3 vs. 60.4 years), have longer tenures (17 vs. 12 years), are more likely to be the founders (33% vs. 20%), and deliver more often a negative performance as measured by the (daily) three-year alpha of the market model regression (52% vs. 0%). Finally, we follow Salas (2010) and compute an alternative entrenchment measure that we will use in our robustness section. The measure classifies a CEO as entrenched if the manager worked in a firm for more than ten years and delivered a negative three-year alpha prior to death. We find a positive relationship between abnormal stock-price reactions and Salas (2010)' alternative entrenchment measure.

2.3 Identification of Competing Firms

To identify the competitors of firms that experience the sudden death of an entrenched CEO, we follow previous literature and consider all S&P1500 companies with the same Compustat 4-digit SIC codes (see, e.g., Sundaram, John, and John, 1999; Grullon, Kanatas, and Kumar, 2006; Flammer, 2015b; Qui and Wan, 2015; Helwege and Zhang, 2016). Compustat assigns SIC codes based on the business segment breakout provided by the firm. For instance, if one specific SIC applies to 50% or more of the firm's total sales, that specific SIC is reported. Otherwise, based on an algorithm used by Compustat, a more general code is used.[1] Thus, by using Compustat 4-digit SIC codes, we are able to select firms that compete for the same customers and market shares in accordance with an established and widely-used procedure. In the robustness section, we also identify competitors based on their 3-digit SIC codes or their product similarity according to Hoberg and Phillips (2016).

2.4 Methodology and Sample of Competing Firms

To test the impact of an entrenched CEO's sudden death on the value of competing firms, we run short-term event studies and compute abnormal stock-market returns. We obtain abnormal returns using the market model and, for robustness purposes, the Carhart (1997) four-factor model. The parameters for both models are estimated over 250 trading days ending 10 days prior to the event date with the CRSP value-weighted index as benchmark. We consider the announcement of an entrenched CEO's death as event date. In all event studies for competitors, we cluster standard errors at the event-date level because the returns of each firm's competitors may not be independent around the days of death.

[1] Consider the example of a firm with 10% sales in a business segment with SIC code 2812, 15% sales in SIC code 2819, 35% sales in SIC code 2821, and 40% sales in SIC code 3079. Although most sales are associated with SIC code 3079, the business segments in SIC code area 2800 amount to 60 percent of total sales. As 2821 amounts to 58 percent of the 60 percent, i.e., more than 50% of sales in 2800s, 2821 is the major industry code for that respective firm. For further details about the procedure used in Compustat, see Guenther and Rosman (1994).

In addition, we run DiD-regressions that compare changes in our outcome variables of interest (e.g., accounting performance, risk taking, and sales margin) for treated firms to changes in the same variables for a control group. By considering also control firms, the DiD-methodology automatically controls for possible trends that influence all firms. For example, enacted in July 2002, the Sarbanese-Oxley (SOX) Act dramatically affected corporate boards and their monitoring role by increasing the demand for independent directors (see, e.g., Linck, Netter, and Yang, 2009). The DiD-regressions would filter out the SOX effect and avoid to attribute changes in firm performance resulting from the new legislation to the entrenchment shock triggered by a sudden CEO death. In our study, treated firms are competitors of companies that experienced the sudden death of an entrenched CEO and control firms are all remaining S&P1500 firms. We exclude all focal firms from the sets of treated and control firms. The DiD-regressions used throughout the paper are of the following form:

$$Dep_i = \alpha + \beta_1 Post_i + \beta_2 Treated_i + \beta_3 Post_i \times Treated_i + \beta_4 X_i + \epsilon_i, \tag{2.1}$$

where Dep_i is the dependent variable for firm i, $Post_i$ denotes an indicator variable that is set to one in the post-event period and zero otherwise, $Treated_i$ is an indicator variable that equals one if the firm experiences the sudden death of a competitor's CEO and zero otherwise, and X_i is a set of control variables that capture the effect of observable differences among firms in the treatment and control sample. ϵ_i is an error term. The coefficient of interest is β_3 which measures the causal treatment effect of an entrenched CEO's sudden death on competitors. In all regressions we include either industry- or firm-fixed effects that account for time-invariant factors related to an industry or firm, and cluster standard errors at the firm level.[2]

Table 2.2: Summary Statistics of Treated and Control Firms

This table reports summary statistics of the dependent and control variables for our treatment and control samples one year prior to death. The treatment sample consists of all stocks in the competitive environment of the decease announcing firm (based on the same 4-digit SIC code). The control sample is the remainder of all S&P1500 firms. The number of observations may change due to missing values for some accounting variables. All dependent and control variables are defined in Appendix A.

	Treated firms					**Control firms**				
	N	**Mean**	**Median**	**Q1**	**Q3**	**N**	**Mean**	**Median**	**Q1**	**Q3**
$OROA_{adj}$	225	0.00	0.00	-0.01	0.01	22,021	0.00	0.00	-0.02	0.02
Total risk	291	0.09	0.08	0.06	0.12	22,719	0.10	0.09	0.06	0.13
Syst. risk	291	1.11	0.76	0.32	1.45	22,719	1.21	1.05	0.44	1.78
Idios. risk	291	0.09	0.07	0.05	0.11	22,719	0.10	0.08	0.06	0.12
R&D	116	0.05	0.03	0.02	0.06	12,007	0.04	0.02	0.00	0.05
Sales	259	6.81	6.67	5.74	7.93	23,005	7.06	7.00	5.99	8.14
Cogs	259	6.10	5.93	5.01	7.28	22,867	6.52	6.50	5.37	7.72
Sales margin	259	1.25	1.12	0.48	1.65	22,867	1.16	0.54	0.30	1.09
HHI	265	0.09	0.06	0.03	0.12	23,003	0.21	0.15	0.08	0.27
ROA	265	0.04	0.02	0.01	0.08	23,003	0.04	0.05	0.01	0.09
Leverage	265	0.11	0.08	0.03	0.15	23,003	0.19	0.16	0.03	0.29
Market value	265	7.35	7.10	6.34	8.33	23,003	7.24	7.12	6.15	8.25
Sales growth	265	0.24	0.11	0.05	0.25	23,003	0.36	0.10	0.02	0.22

Our final sample contains 291 competitors with available stock returns. Table 2.2 provides summary statistics of dependent and control variables for treated and control firms used in the

[2]It is not possible to include both industry-fixed and firm-fixed effects simultaneously because each CEO-death event is related to one industry and thus firm-fixed effects already capture industry-fixed effects. For this reason, we control for industry- and firm-fixed effects in succession.

DiD-regressions. The variables are taken from the fiscal year prior to the event date and may vary because of missing data. All variables are described in Appendix A. The treatment and control samples differ along few dimensions.[3] However, in various robustness checks, we demonstrate that our results are not affected by these ex-ante differences in observables.

3 Results

3.1 Stock-Price Reactions

To investigate the value effects of a reduction in managerial entrenchment for focal firms and competitors, we run an event study on their stock-market reactions upon the announcement of an entrenched CEO's sudden death.

Table 2.3: Abnormal Returns around the Sudden Deaths of Entrenched CEOs

This table reports cumulative abnormal returns (CARs) of the focal firms (Panel A) and their competitors (Panel B) following the sudden death of an entrenched CEO. Entrenched CEOs are managers whose announcement of death is followed by positive CARs over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. The dependent variables are CARs computed over three- ([-1 +1]) and five- ([-2 +2]) days around the CEO deaths using either the market model or the Carhart (1997) four-factor model. Both models are estimated over 250 trading days, which end 10 days prior to the sudden death. Standard errors in Panel B are clustered on the event level. *t*-statistics are in parentheses. *** and ** denote 1% and 5% statistical significance.

Panel A: CARs of Focal Firms				
	Market Model		**Carhart (1997)**	
	[-1 +1]	**[-2 +2]**	**[-1 +1]**	**[-2 +2]**
Mean	2.836**	4.594***	2.513**	4.361***
t-stat.	(2.62)	(4.58)	(2.52)	(5.24)
Panel B: CARs of Competing Firms				
	Market Model		**Carhart (1997)**	
	[-1 +1]	**[-2 +2]**	**[-1 +1]**	**[-2 +2]**
Mean	3.595**	4.733**	2.760**	3.636**
t-stat.	(2.63)	(2.77)	(2.47)	(2.32)

Table 2.3 presents stock-market reactions for focal firms (Panel A) and their competitors (Panel B) over a three-day ([-1 +1]) and a five-day ([-2 +2]) event window centered around the announcement day. Columns I and II show results using the market model to compute CARs. The announcement of an entrenched CEOs death (Panel A) is followed by a significant increase in firm value by 2.8% over a three-day window and 4.6% over a five-day window. This result is in line with Salas (2010) who documents a significant increase of almost 6.76% for the deaths of entrenched CEOs, and Johnson, Magee, Nagarajan, and Newman (1985), who evidence an average abnormal return of 3.5% for the death of a founder. The returns of the competing firms display a similar pattern with abnormal returns of 3.6% over a three-day window and 4.7% over a five-day window. Columns III and IV show that the results are similar when using the Carhart (1997) four-factor

[3]Two-tailed difference in means tests (unreported) show significant differences for total risk, idiosyncratic risk, cogs, HHI, leverage at the 1% level but no significant differences for OROA_{adj}, R&D, sales, sales margin, systematic risk, ROA, market value, sales growth. The mean values of OROA_{adj} for treated and control firms are zero because OROA is lagged and industry-adjusted. Thus, OROA_{adj} measures an abnormal operating return.

model. Overall, the results provide clear evidence that market participants perceive the reduction in managerial entrenchment as a positive signal for the economic prospects of both the focal firm and all other firms in the same competitive environment.

3.2 Operating Performance

To investigate whether the positive stock-price reactions of focal and competing firms triggered by the reduction of managerial entrenchment materializes in a higher future accounting performance, we study changes in the operating performance for our sample of suddenly-died CEOs using t-tests and run DiD-regressions of competing firms' operating performances as outlined in equation 2.1. To measure operating performance, we compute adjusted operating return on assets (OROA_{adj}). Following Barber and Lyon (1996), we calculate OROA as a company's earnings before interest and taxes divided by its total assets. To obtain OROA_{adj}, we adjust the firms' OROA for firm- and industry-trends by computing the difference between OROA in year t and the sum of OROA in year $t-1$ and the change in the median industry OROA from year $t-1$ to year t.[4]

Table 2.4: Changes in Operating Performance following the Sudden Deaths of Entrenched CEOs

This table reports the results for changes in the operating performance of the focal firms (Panel A) and their competitors (Panel B) following the sudden death of an entrenched CEO. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. Panel A shows regression results of changes in the operating performance as measured by OROA_{adj} for the focal firms without predictor. Panel B shows changes in the operating performance as measured by OROA_{adj} for competitors using a difference-in-difference approach. The dependent variable is winsorized at the 5% level and the regressions include industry-fixed effects. *Post* is a dummy variable equal to one in the post-death period and zero in the pre-death period. *Treated* is a dummy variable equal to one if a firm is affected by the sudden death of an entrenched CEO in a competing firm and zero otherwise. Standard errors are clustered at the firm level. t-statistics are in parentheses. *** and ** denote 1% and 5% statistical significance. Dependent variables are defined in Appendix A.

Panel A: Changes in Operating Performance of Focal Firms		
	[-1 +1]	**[-1 +2]**
Mean	-0.000	-0.028
t-stat.	-0.00	-1.74
Panel B: Changes in Operating Performance of Competitors		
	[-1 +1]	**[-1 +2]**
PostxTreated	0.003	0.009**
	(0.84)	(2.37)
Treated	-0.003	-0.003
	(-1.00)	(-1.08)
Post	-0.004***	-0.005***
	(-11.54)	(-15.78)
Fixed Effects	YES	YES
Obs.	44,492	42,144
R^2	0.004	0.006

As reported in Panel A of Table 2.4, in spite of positive announcement returns, firms that experience the sudden death of an entrenched CEO display no change in operating performance after the decease. One possible explanation is that at the beginning of their tenure, CEOs manage

[4] We find similar results if we only adjust OROA for industry-trends by computing the difference between the firm's OROA in year t and the median industry OROA in year t.

earnings downwards to offer themselves a fresh start (Latif, Strickland, and Yang, 2011). As shown in Panel B, the average treatment effect for competing firms (the estimated coefficient of Post x Treated) is positive and significant in the second year following the entrenchment shock. Industry-fixed effects mitigate the concern that time-invariant industry-specific factors could drive the observed relationship between sudden deaths of entrenched CEOs and performance. Overall, the results of the DiD-analysis support the findings of the short-term event study based on forward-looking stock-returns. In line with Foucault and Fresard (2014), our results suggest that the positive (value) effect experienced by the focal firms as a consequence of the reduction in managerial entrenchment has an incentive effect on competing firms.

3.3 Management Strategies to Improve Performance

3.3.1 Risk Taking

Scholars have shown that managers tend to implement riskier strategies when they aim at outperforming competitors (Aron and Lazear, 1990; Roussanov and Savor, 2014; Ammann, Horsch, and Oesch, 2016). In this section, we investigate changes in corporate risk caused by managerial actions taken in response to a competitor's entrenchment shock. Following Ammann, Horsch, and Oesch (2016), we measure total equity risk as the volatility of monthly returns in a given firm-year. The change in total equity risk can either arise through adjustments in systematic risk, e.g., when CEOs decide to reach new markets, or from changes in idiosyncratic risk, e.g., when riskier projects are realized, or a combination of both. We examine the idiosyncratic risk component as measured by the standard deviation of the residuals regressing monthly stock returns against the CRSP value-weighted index returns over one firm-year. The systematic risk is measured by the beta of this regression. Finally, we investigate the firms' R&D intensity, measured by the logarithm of one plus the annual R&D expenses divided by year-end market equity (Chan, Lakonishok, and Sougiannis, 2001; Hirshleifer, Hsu, and Li, 2013). A firm's R&D intensity captures the riskiness of the corporates policies as a consequence of more distant and uncertain cash-flows (Coles, Daniel, and Naveen, 2006; Bargeron, Lehn, and Zutter, 2010; Li, Griffin, Yue, and Zhao, 2013). As our measures of equity risk (total risk, idiosyncratic risk, and systematic risk) are obtained from changes in stock prices – which are forward-looking and reflect market expectations or announcements of investments in riskier projects – they adjust more quickly than accounting measures. In order to capture their immediate responses, we substitute the three-year window [-1 +2] with a one-year event window [-1 0] and compare the equity risk measured over the year prior to the event to the equity risk in the event year.

The results of the DiD-regressions are shown in Table 2.5. The sudden death of an entrenched CEO leads to a significant increase in the stock-return volatility of competing firms, starting in the event year. More precisely, the change in total risk of the treated firms is 2.9 percentage points larger than that of the control firms. By splitting total risk into its systematic and its idiosyncratic component, we observe that the increase in total risk is driven by the idiosyncratic component. In the subsequent year, all equity-risk measures are significantly higher compared to their pre-event levels (see window [-1 +1]). In addition, treated firms significantly increase their R&D intensity

Table 2.5: Changes in Risk Taking following the Sudden Deaths of Entrenched CEOs

This table reports the results of difference-in-differences regressions for *Total risk*, *Syst. risk*, *Idios. risk*, and *R&D*. *Post* is a dummy variable equal to one in the post-death period and zero in the pre-death period. *Treated* is a dummy variable equal to one if a firm is affected by the sudden death of an entrenched CEO in a competing firm and zero otherwise. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. All regressions include industry-fixed effects. Dependent variables are winsorized at the 5% level. Standard errors are clustered at the firm level. *t*-statistics are in parentheses. ***, **, and * denote 1%, 5%, and 10% statistical significance. Dependent variables are defined in Appendix A.

	Total risk		**Syst. risk**		**Idios. risk**		**R&D**	
	[-1 0]	**[-1 +1]**	**[-1 0]**	**[-1 +1]**	**[-1 0]**	**[-1 +1]**	**[-1 +1]**	**[-1 +2]**
PostxTreated	0.029***	0.022***	-0.135**	0.177**	0.031***	0.015***	0.005*	0.008**
	(9.17)	(6.22)	(-2.19)	(2.56)	(9.62)	(4.67)	(1.67)	(2.08)
Treated	-0.001	-0.002	-0.051	-0.049	0.003	0.002	-0.003	-0.002
	(-0.53)	(-1.07)	(-1.01)	(-0.97)	(1.40)	(0.71)	(-0.74)	(-0.54)
Post	0.005***	0.006***	-0.010**	-0.027***	0.003***	0.003***	0.002***	0.003***
	(25.31)	(23.71)	(-2.20)	(-4.44)	(16.86)	(12.44)	(9.03)	(10.15)
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	46,020	44,156	46,020	44,156	46,020	44,156	24,246	22,780
R^2	0.133	0.132	0.090	0.087	0.122	0.117	0.279	0.280

over the first two years following an entrenched CEO's death. Overall, our findings suggest that firms pursue riskier strategies to catch up with competitors who are expected to perform better in the aftermath of an entrenchment shock.

3.3.2 Sales, Costs of Goods Sold, and Sales Margin

The finding that firms experience a rise in market values and operating performance raises the question whether these achievements are obtained via higher growth, cost cutting, or a combination of both. To answer this question, we run DiD-regressions with sales, cost of goods sold (cogs), and the sales margin as dependent variables. Table 2.6 presents the regression results. While we find no evidence of a significant change in sales for the competing firms on top of a general upward trend that influences all firms (significant coefficient of Post), competing firms are able to significantly reduce their costs of goods sold by almost 15% two years after the entrenchment shock. The stable amount of sales and the lower production costs lead to a significant improvement of the sales margin by 35.9% over the same time frame. The results provide strong evidence that the performance improvements of competing firms arise due to higher profitability that likely originates in successful cost cutting rather than growth strategies.

Table 2.6: Changes in Sales, Cogs, and Sales Margin following the Sudden Deaths of Entrenched CEOs

This table reports the results of difference-in-differences regressions for *Sales*, *Cogs*, and *Sales margin*. *Post* is a dummy variable equal to one in the post-death period and zero in the pre-death period. *Treated* is a dummy variable equal to one if a firm is affected by the sudden death of an entrenched CEO in a competing firm and zero otherwise. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. All regressions include industry-fixed effects. *Sales margin* is winsorized at the 5% level. Standard errors are clustered at the firm level. *t*-statistics are in parentheses. *** and ** denote 1% and 5% statistical significance. Dependent variables are defined in Appendix A.

	Sales		**Cogs**		**Sales margin**	
	[-1 +1]	**[-1 +2]**	**[-1 +1]**	**[-1 +2]**	**[-1 +1]**	**[-1 +2]**
PostxTreated	0.004	-0.018	0.028	-0.149***	-0.0198	0.359***
	(0.16)	(-0.63)	(1.11)	(-4.12)	(-0.623)	(6.926)
Treated	-0.079	0.007	-0.020	0.079	-0.102**	-0.129**
	(-0.88)	(0.08)	(-0.23)	(0.91)	(-2.129)	(-2.557)
Post	0.151***	0.209***	0.149***	0.209***	-0.00219	0.00108
	(39.33)	(38.28)	(38.95)	(38.52)	(-0.785)	(0.297)
Fixed Effects	YES	YES	YES	YES	YES	YES
Obs.	46,527	43,865	46,256	43,601	46,256	43,601
R^2	0.150	0.152	0.201	0.204	0.271	0.274

3.3.3 Impact on Suppliers and Customers

In this section, we investigate whether the significant increase in profitability of competing firms is partially attributable to advantageous renegotiations of contract conditions with existing customers and suppliers. Specifically, we compute the announcement returns for suppliers and corporate costumers of competing firms upon the deaths of entrenched CEOs. If competing firms are expected to cut costs or increase margins by successfully renegotiating contractual conditions with corporate

partners, the latter should get hurt and exhibit negative stock returns (Intintoli, Serfling, and Shaikh, 2016).

Since the identification of exact pairs of supplier-competitor and competitor-customer relationships is not feasible, we follow Shahrur (2005) and identify suppliers and customers at the industry level.[5] As competing firms in one industry are affected by a common entrenchment shock, we match supplier and customer industries on the industry level of the decease-announcing firm, i.e., the decease-announcing industry. We identify supplier and customer industries of the decease-announcing industries utilizing *Use Tables* from annual industry input-output accounts as published by the Bureau of Economic Analysis at the U.S. Department of Commerce. For any pair of supplier and customer industries in the years 1997 to 2013, the tables provide estimates of the dollar value of the supplier industry's output that is applied as input in the production of the customer industry's output.

We examine suppliers that belong either to a main supplier industry or a dependent supplier industry. The main supplier industry delivers the primal input of the decease-announcing industry and the dependent supplier industry sells the highest percentage of its output to the decease-announcing industry. To identify main and dependent industries, we follow Shahrur (2005) and compute a *Decease input coefficient* and a *Supplier percentage sold* value for each pair of supplier-decease-announcing industries. The former measures the importance of the supplier industry's output in the production process of the decease-announcing industry and is calculated as the dollar value of the supplier industry's output that was sold to the decease-announcing industry divided by the decease-announcing industry's total output. The latter measures the importance of the decease-announcing industry as a purchaser of the supplier industry's output and is calculated as the percentage of the supplier industry's output sold to the decease-announcing industry. Consequently, the main supplier industry is the industry with the highest *Decease input coefficient*. The dependent supplier industry is the industry with the highest *Supplier percentage sold*. In line with Shahrur (2005), we require that the identified supplier industries sell a significant fraction of their output (at least 1%) to the decease-announcing industry.

Analogously, we examine customers that belong either to the main or the dependent customer industry. We identify the main customer industry as the industry that purchases the highest percentage of the decease-announcing industry's output and the dependent customer industry as the industry that relies most on the decease-announcing industry's output. We compute a *Customer input coefficient* as the dollar value of the decease-announcing industry's output vended to the customer industry divided by the customer industry's total output. *Decease percentage sold* is the percentage of the decease-announcing industry's output vended to the customer industry. Whereas

[5] Financial Accounting Standards No. 14 (FAS 14) of the Financial Accounting Standard Board requires firms to disclose the identity of customers. Unfortunately, customers only need to be disclosed if they generate more than 10% of the firm's total sales and the disclosure of their names is rather fuzzy, which makes it difficult to match customer names to available databases (Fee and Thomas, 2004). In addition, firms are not required to and typically do not disclose a list of their suppliers. However, Shahrur (2005) shows that using entire supplier and customer industries leads to results that are comparable to those obtained with potentially exact relationships. Applying Shahrur (2005)'s approach is also a rather conservative choice because if some suppliers or customers are wrongly assigned (due to the use of entire industries instead of exact relationships), their computed abnormal returns would be zero and thus weaken our results.

the former reflects the importance of the decease-announcing industry's output as an input in the production process of the customer industry's output, the latter defines the importance of the customer industry as a purchaser of the decease-announcing industry's output. The main customer industry is the industry with the highest value of *Decease percentage sold* and the dependent customer industry is the industry with the highest *Customer input coefficient*. Again, we only consider customer industries that purchase at least 1% of the decease-announcing industry's output.

We obtain 1,339 (1,348) S&P 1500 firms that belong to main (dependent) supplier industries and 1,263 (1,101) S&P 1500 firms that belong to main (dependent) customer industries of the decease-announcing industries. Event-study results based on the market model (not reported) show that main and dependent suppliers experience negative and significant abnormal returns in a five-day ([-2 +2]) event window (-0.70%, *t*-value: -2.94, and -0.70%, *t*-value: -2.84, respectively) upon the announcement of an entrenched CEOs' sudden death. Similar results are obtained for main and dependent customers (-0.76%, *t*-value: -2.99, and -0.74%, *t*-value: -2.83, respectively).

Table 2.7 presents results of cross-sectional regressions of customers' and suppliers' CARs on the CEO entrenchment measure and a set of control variables. In these regressions, we include all customers and suppliers that are affected by a CEO's death (not only those affected by an entrenched CEO's death). Our results show that the abnormal returns of suppliers and customers that already performed poorly, i.e., exhibit a negative one-year alpha prior to the death (*Negative alpha* (*supplier/customer*)), are significantly higher than the abnormal returns for well-performing suppliers and customers. This indicates that a CEO death offers unsuccessful customers and suppliers an opportunity to reposition. However, if the sudden death is related to a reduction in managerial entrenchment, announcement returns of firms from the main and dependent supplier industries are significantly lower (-2.6% and −2.4%, respectively) compared to the announcement of non entrenched CEO deaths. Similarly, also the announcement returns of firms from the main and dependent customer industries are significantly lower (-3.7% and −3.9%, respectively) if the deceased CEO was entrenched. Unreported results show that these findings remain unchanged for alternative event windows ([-1 +1], [-3 +3], and [-5 +5]), by including additional controls (book-to-market ratio, market value, ROA, fraction of large blockholders, and leverage), or by using either all firms covered by Compustat or only single-segment firms in the S&P1500.

In line with Intintoli, Serfling, and Shaikh (2016), our findings suggest that the death of an entrenched CEO leads firms within an industry to beneficially renegotiate existing contract terms with both suppliers and customers. Thus, the previously measured increases in operating performance and stock returns may reflect, at least to some extent, a redistribution of wealth from suppliers and customers to the firms directly (focal firm) and indirectly (competitors) affected by an entrenchment shock.

3.4 Robustness

In this section, we provide evidence for the validity of our natural experiment and the robustness of our findings. First, we conduct a falsification analysis to test the DiD-assumption of parallel trends in the dependent variables between treated and control firms. Second, to mitigate concerns

Table 2.7: Regressions of Abnormal Returns to Main and Dependent Suppliers and Customers

This table reports regression results of cumulative abnormal returns (CARs) over a five-day event window [-2 +2] for S&P1500 firms that are either main or dependent suppliers, or main or dependent customers of industries affected by an entrenchment shock. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. *Entrenchment* is a dummy variable indication whether a CEO is entrenched or not. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period of one year ranging from 260 to 10 trading days [-260 -10] prior to the death. *Negative 3Y-Alpha* is a dummy variable equal to one if the intercept of a market model regressions for the deceased CEO over three years prior to the sudden death is negative and zero otherwise. *Long tenure* is a dummy variable equal to one if the deceased CEO has a tenure of more than 10 years and zero otherwise. *Negative alpha (supplier/customer)* is a dummy variable equal to one if the intercept of a market model regressions for the main/dependent suppliers/customers over one year ([-260 -10]) prior to the sudden death is negative and zero otherwise. *HHI (supplier/customer)* reflects the Herfindahl-Hirschman Index, measured by the sum of the squared fraction of industry sales by all firms in the supplier/customer industry. *HHI* is measured by the sum of the squared fraction of industry sales by all firms in the decease-announcing industry. *Supplier percentage sold* is the percentage of the supplier industry's output sold to the decease-announcing industry. *Customer input coefficient* is the dollar amount of the decease-announcing industry's output sold to the customer industry divided by the customer industry's total output. *Size (supplier/customer)* is the firm size of the main or dependent supplier or customer. *Size* is the firm size of the decease-announcing firm. Size is measured as the logarithm of total assets. Standard errors are clustered at the event level. *t*-statistics are in parentheses. ***, **, and * denotes 1%, 5%, and 10% significance levels, respectively.

	Main suppliers	**Dependent suppliers**	**Main customers**	**Dependent customers**
Entrenchment	-0.026***	-0.024***	-0.037***	-0.039***
	(-3.59)	(-3.17)	(-4.55)	(-7.11)
Negative 3Y-Alpha	-0.008	-0.011	-0.005	0.003
	(-1.10)	(-1.30)	(-0.84)	(0.45)
Long tenure	-0.036**	-0.030	-0.033***	-0.056***
	(-2.46)	(-1.73)	(-3.00)	(-4.41)
Negative alpha (supplier/customer)	0.017***	0.017***	0.019***	0.020***
	(4.22)	(4.13)	(4.34)	(5.23)
HHI (supplier/customer)	0.008	0.012	0.014	0.014
	(1.01)	(1.04)	(1.20)	(1.14)
HHI	-0.234**	-0.187*	-0.054	-0.244**
	(-2.72)	(-1.83)	(-0.50)	(-2.88)
Supplier percentage sold/	0.119	0.106	0.036	-0.140**
Customer input coefficient	(1.55)	(1.23)	(0.70)	(-3.02)
Size (supplier/customer)	-0.001	-0.001	-0.001	-0.001
	(-0.89)	(-1.04)	(-0.87)	(-1.58)
Size	-0.011**	-0.010**	-0.010**	-0.017***
	(-2.78)	(-2.37)	(-2.86)	(-7.72)
Constant	0.130**	0.119*	0.115**	0.244***
	(2.55)	(1.97)	(2.91)	(6.31)
Obs.	1,339	1,348	1,263	1,101
R^2	0.090	0.088	0.099	0.112

regarding differences among treated and control firms that may bias our results, we add a number of control variables to the specification of our main analysis and exchange industry-fixed with firm-fixed effects. Third, we rerun our DiD-regressions based on a prior characteristic-based matching of control firms to treated firms instead of using all remaining non-treated S&P1500 firms as control group. Finally, we assign competitors according to two alternative classification schemes and test an additional entrenchment proxy.

3.4.1 Placebo Tests

The DiD-approach relies on the assumption that the dependent variables of the treated and control firms exhibit, on average, a parallel trend in the hypothetical case of no treatment. To test the validity of this assumption, we conduct a placebo test by mechanically shifting the announcement

dates backwards and considering the change of any dependent variable from the year $t-2$ to the year $t-1$. This period should be unaffected by the real CEO death which happens in $t=0$. Thus, if the model is well-specified and the parallel-trend assumption is satisfied, we should not find many significant treatment effects.

The results of the placebo DiD-regressions presented in Table 2.8 suggest no dramatic violation of the parallel-trend assumption. In seven of the eight regressions, the average treatment effect is not statistically different from zero. The only exception is column IV, showing a significant different trend for idiosyncratic risk. However, the alternative measure for corporate-specific risk, R&D intensity, does not change significantly.[6] The results confirm that the increases in operating performance, risk taking, and sales margin for the treated companies can only be observed after an actual entrenchment shock and not around artificially constructed placebo dates.

3.4.2 Control Variables and Firm-Fixed Effects

In this section, we address the concern that our average treatment effects may reflect systematic differences in characteristics between treated and control firms by adding a set of control variables to the regressions of our main analysis. We control for size as measured by the market value of equity, sales growth, return on assets, industry concentration as measured by the Herfindal Hirschman Index (HHI), R&D intensity, as well as financial leverage.[7] Additionally, we include firm-fixed effects instead of industry-fixed effects to account for time-invariant unobservable heterogeneity among firms.

The results of Table 2.9 indicate that the average treatment effects estimated in the main analysis are robust to the inclusion of time-varying observables and time-invariant unobservable firm characteristics. In fact, the impact, magnitude, and statistical significance of the treatment effects are comparable to those in the DiD-regressions without controls and with industry-fixed instead of firm-fixed effects. The results are similar if we substitute firm-fixed effects with industry-fixed effects. In this respect, the findings on performance improvements and changes in management strategies of the treated compared to control firms do not seem to be ascribable to systematic differences between the two groups of firms.

3.4.3 Difference-in-Differences Matching Estimator

The inclusion of control variables in our linear regression framework might not appropriately account for differences between treated and control firms if the companies differ along unobservable dimensions or if there are non-linearities in the data (Roberts and Whited, 2012). Following related studies (see, e.g., Irani and Oesch, 2013; Ammann, Horsch, and Oesch, 2016), we apply the nearest-neighbor propensity score matching scheme of Rosenbaum and Rubin (1983). We run a logit

[6] Given the high number of tested variables some of them were expected to differ even if the two groups of firms are drawn from the same distribution.

[7] The variables are described in Appendix A. In each regression, we include all controls except the dependent variable itself. Further, in order to avoid a large loss of observations, we follow Hirschey, Skiba, and Wintoki (2012) and Hirshleifer, Hsu, and Li (2013) and set missing R&D values to zero if R&D is used as a control variable.

Table 2.8: Robustness: Difference-in-differences Placebo Regressions

This table reports the results of difference-in-differences placebo regressions. *Post* is a dummy variable equal to one in the post-death period and zero in the pre-death period. *Treated* is a dummy variable equal to one if a firm is affected by the sudden death of an entrenched CEO in a competing firm and zero otherwise. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. All regressions include industry-fixed effects. All dependent variables except of *Sales* and *Cogs* are winsorized at the 5% level. Standard errors are clustered at the firm level. *t*-statistics are in parentheses. ***, **, and * denote 1%, 5%, and 10% statistical significance. Dependent variables are defined in Appendix A.

	OROA$_{adj}$	**Total risk**	**Syst. risk**	**Idios. risk**	**R&D**	**Sales**	**Cogs**	**Sales margin**
PostxTreated	0.001	0.000	0.049	0.008**	0.005	0.031	0.056	-0.056
	(0.27)	(0.01)	(0.74)	(2.38)	(1.09)	(0.23)	(0.40)	(-0.81)
Treated	-0.002	-0.001	-0.086*	-0.006**	-0.006*	-0.109	-0.073	-0.070
	(-0.90)	(-0.37)	(-1.74)	(-2.17)	(-1.87)	(-1.11)	(-0.72)	(-1.42)
Post	0.001*	0.002***	-0.056***	-0.003***	0.002***	0.125***	0.121***	0.004
	(1.80)	(12.30)	(-12.73)	(8.36)	(3.85)	(8.76)	(8.19)	(0.54)
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	44,516	45,100	45,100	45,100	24,870	48,201	47,952	47,952
R^2	0.004	0.155	0.085	0.142	0.286	0.147	0.199	0.272

Table 2.9: Robustness: Difference-in-differences Regressions with Control Variables

This table reports the results of difference-in-differences regressions with control variables. *Post* is a dummy variable equal to one in the post-death period and zero in the pre-death period. *Treated* is a dummy variable equal to one if a firm is affected by the sudden death of an entrenched CEO in a competing firm and zero otherwise. Entrenched CEOs are managers whose announcement of death is followed by a positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. All regressions include industry-fixed effects. Controls added are *Treated, Post, HHI, ROA, R&D, Leverage, Market value, and Sales growth.* All dependent variables except of *Sales* and *Cogs* are winsorized at the 5% level. Standard errors are clustered at the firm level. t-statistics are in parentheses. *** and ** denote 1% and 5% statistical significance. Dependent and control variables are defined in Appendix A.

	OROA$_{adj}$	**Total risk**	**Syst. risk**	**Idios. risk**	**R&D**	**Sales**	**Cogs**	**Sales margin**
	[-1 +1]	**[-1 0]**	**[-1 0]**	**[-1 0]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**
PostxTreated	0.003	0.023***	-0.161**	0.025***	0.005**	0.025	0.045	-0.014
	(0.65)	(6.79)	(-2.27)	(7.29)	(2.10)	(1.08)	(1.44)	(-0.44)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	42,991	43,386	43,386	43,386	23,792	44,632	44,583	44,583
R^2	0.063	0.484	0.271	0.493	0.781	0.931	0.928	0.859
	[-1 +2]	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**
PostxTreated	0.008**	0.014***	0.067	0.009***	0.008***	-0.009	-0.145***	0.363***
	(2.23)	(3.73)	(0.86)	(2.71)	(2.77)	(-0.33)	(-3.81)	(6.82)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	40,647	41,624	41,624	41,624	22,350	41,984	41,936	41,936
R^2	0.066	0.486	0.255	0.496	0.778	0.928	0.926	0.854

regression of an indicator variable that equals one if a particular firm-year is classified as treated (i.e., an entrenched competitor dies in the respective year) and zero otherwise, on selected risk and performance measures in the year before the sudden CEO death.[8] The estimated coefficients are used to predict the probability of a firm being treated. With these probabilities, a nearest-neighbor matching with replacement is conducted using a 0.001 caliper (tolerance level) and allowing for multiple matches per treated firm to enhance the accuracy of the estimated treatment effects.

Table 2.10: Robustness: Difference-in-differences Regressions with Matched Control Firms

This table reports summary statistics (Panel A) and results (Panel B) of using a difference-in-differences matching estimator. Panel A shows summary statistics of the treated and matched control samples one year prior to the event. The treatment sample consists of firms that are affected by the sudden death of an entrenched CEO in a competing company. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. Up to 12 control firms (from S&P1500) are matched on cogs, systematic risk, idiosyncratic risk, and operating performance using a nearest-neighbor logit propensity score match under a 0.001 caliper. Panel B estimates the average treatment effect of a sudden competitor's death for a sample of matching firms over the time periods [-1 0], [-1 +1], and [-1 +2]. All dependent variables except of *Sales* and *Cogs* are winsorized at the 5% level. Missing *R&D* values are set to zero. *t*-statistics are in parentheses are robust to clustering at the firm level. *, **, and *** denotes significance at the 10%, 5% and 1% level, respectively. Dependent variables are defined in Appendix A.

Panel A: Summary Statistics for Matched Samples

	Mean treated firms	**Mean control firms**	**Difference in means**	***t*-stat.**
$OROA_{adj}$	-0.000	0.000	-0.001	-0.07
Total risk	0.097	0.100	-0.003	-0.67
Syst. risk	1.311	1.292	0.009	0.14
Idios. risk	0.090	0.090	-0.000	-0.04
R&D	0.024	0.021	0.003	0.83
Sales	6.827	6.775	0.052	0.33
Cogs	6.157	6.143	0.014	0.08
Sales margin	1.162	1.284	-0.122	-0.66
Market value	7.360	7.133	0.227	1.51

Panel B: Difference-in-differences Matching Estimator

	[-1 +1]	**[-1 +2]**
$OROA_{adj}$	0.002	0.009**
	(0.51)	(2.20)
R&D	0.002*	0.002*
	(1.72)	(1.68)
Sales	-0.004	-0.025
	(-0.16)	(-0.77)
Cogs	-0.007	-0.164***
	(-0.27)	(-3.96)
Sales margin	0.010	0.356***
	(0.27)	(6.06)
	[-1 0]	**[-1 +1]**
Total risk	0.016***	0.006
	(4.38)	(1.52)
Syst. risk	-0.134*	0.044
	(-1.714)	(0.51)
Idios. risk	0.017***	0.002
	(4.77)	(0.65)

Table 2.10 presents the results for up to 12 matches per treated firm, which corresponds to 221 treated and 2,497 control firms.[9] Panel A reports summary statistics from the year prior to the entrenched CEOs sudden deaths for treated and control firms. Our treated and control firms seem

[8] In the reported setting, we use the systematic risk, idiosyncratic risk, adjusted operating return on assets, and cost of goods sold as predictors. We run several additional regressions with alternative combinations of predicting variables that confirmed our results and were even more significant. However, alternative combinations identified control firms that were significantly different from the treated firms which presumably emerge from industry differences.

[9] The results are robust to using 5, 8, or 10 matching firms.

to be equivalent (even in unobservable characteristics) because they do not differ along matched characteristics and characteristics that are not explicitly considered in the matching procedure. Panel B displays the effect of the entrenchment shock on the behavior of competitors. We investigate again the accounting figures (OROA_{adj}, R&D, Sales, Cogs, and Sales margin) in the periods [-1 +1] and [-1 +2], and the equity risk measures (Syst. risk, Idios. risk, and Total risk) in the event windows that capture immediate responses ([-1 0] and [-1 +1]). The DiD-matching estimator generates average treatment effects that are similar in magnitude and significance to those of the main analysis. Only the significance of the equity risk measures vanishes in the long run. Overall, we find that treated firms increase their risk taking and decrease their cost of goods sold by simultaneously increasing sales margins and OROA_{adj}. This result reinforces the previous findings and suggests that our results are neither driven by cross-sectional heterogeneity between treated and control firms nor by a non-random treatment.

3.4.4 Alternative Assignments of Competitors

The definition of competing firms in our main analysis is based on 4-digit SIC codes and thus very narrow and accurate. However, it also raises concerns that the observed effects could be driven by very specific cross-industry differences, which cannot be filtered out by including industry- or firm-fixed effects. To mitigate these concerns, we repeat the previous analysis using two alternative sets of competing firms. First, we assign competitors according to a broader 3-digit SIC classification scheme. Second, we apply a recent approach developed by Hoberg and Phillips (2016) that identifies competing firms by comparing the product descriptions in the firms' 10-K filings. For this additional analysis, we use the Hoberg-Phillips Text-based Network Industry Classifications (TNIC-3) data and consider all S&P1500 companies.

Our samples contain 377 competitors with available stock returns using the 3-digit SIC codes and 363 competitors using the TNIC-3 data. We run our DiD-regressions with control variables to capture remaining systematic differences between the treated and the control firms. We also include firm-fixed effects and cluster standard errors at the firm level.

Using the market model, we find that abnormal returns over a five-day event window (not reported) amount to 4.3% (t-value: 2.18) for 3-digit SIC competitors and 4.4% (t-value: 2.23) for Hoberg and Phillips (2016) competitors. Table 2.11 presents the results of the DiD regressions. The impact, magnitude, and statistical significance of the estimated average treatment effects in the DiD-regressions are similar to those measured with competitors defined on a 4-digit SIC classification scheme. The results suggest that our findings are not affected by the specific choice of the competitive environment.

3.4.5 Alternative Entrenchment Measure

Although scholars suggest that positive stock-price reactions to sudden CEO deaths are superior to other proxies of managerial entrenchment (Johnson, Magee, Nagarajan, and Newman, 1985; Worrell, Davidson, Chandy, and Garrison, 1986; Hayes and Schaefer, 1999; Salas, 2010), Salas

Table 2.11: Robustness: Difference-in-differences Regressions with Control Variables for Alternative Competitor Assignments

This table reports the results of difference-in-differences regressions with control variables for two alternative sets of competing firms. Competitors are assigned according to 3-digit SIC codes (Panel A) and the Hoberg and Phillips (2016)' product similarity classification scheme (Panel B). Competitors are only chosen from companies listed in the S&P1500. *Post* is a dummy variable equal to one in the post-death period and zero in the pre-death period. *Treated* is a dummy variable equal to one if a firm is affected by a sudden death of an entrenched CEO in a competing firm and zero otherwise. Entrenched CEOs are managers whose announcement of death is followed by positive cumulative abnormal returns (CARs) over a five-day event window [-2 +2]. CARs are computed using the market model with CRSP value-weighted index as benchmark and an estimation period ranging from 260 to 10 trading days prior to the death. Controls added are *Treated, Post, HHI, ROA, R&D, Leverage, Market value, and Sales growth.* All regressions include firm-fixed effects. All dependent variables except of *Sales* and *Cogs* are winsorized at the 5% level. Standard errors are clustered at the firm level. *t*-statistics are in parentheses. ***, **, and * denote 1%, 5%, and 10% statistical significance. Dependent and control variables are defined in Appendix A.

Panel A: Competitors by 3-digit SIC

	OROA$_{adj}$	**Total risk**	**Syst. risk**	**Idios. risk**	**R&D**	**Sales**	**Cogs**	**Sales margin**
	[-1 +1]	**[-1 0]**	**[-1 0]**	**[-1 0]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**
PostxTreated	0.005	0.012***	-0.132**	0.013***	0.005**	0.031	0.051*	-0.016
	(1.40)	(3.54)	(-2.06)	(4.41)	(2.48)	(1.61)	(1.93)	(-0.64)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	42,991	43,386	43,386	43,386	23,792	44,632	44,583	44,583
R^2	0.063	0.484	0.271	0.493	0.781	0.931	0.928	0.859
	[-1 +2]	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**
PostxTreated	0.009***	0.002	-0.044	0.001	0.008***	0.010	-0.080**	0.246***
	(2.88)	(0.53)	(-0.66)	(0.43)	(3.48)	(0.47)	(-2.57)	(6.39)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	40,647	41,624	41,624	41,624	22,350	41,984	41,936	41,936
R^2	0.066	0.485	0.255	0.496	0.778	0.928	0.926	0.854

Panel B: Competitors by Product Similarity (Hoberg and Phillips, 2016)

	OROA$_{adj}$	**Total risk**	**Syst. risk**	**Idios. risk**	**R&D**	**Sales**	**Cogs**	**Sales margin**
	[-1 +1]	**[-1 0]**	**[-1 0]**	**[-1 0]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**
PostxTreated	0.003	0.015***	-0.118**	0.019***	-0.002	-0.050***	-0.003	-0.037
	(1.08)	(4.86)	(-2.20)	(6.30)	(-0.99)	(-2.69)	(-0.11)	(-1.42)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	42,991	43,386	43,386	43,386	23,792	44,632	44,583	44,583
R^2	0.063	0.484	0.272	0.494	0.780	0.931	0.928	0.859
	[-1 +2]	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**
PostxTreated	0.006**	0.009**	0.202***	0.005*	-0.000	-0.074***	-0.158***	0.286***
	(2.17)	(2.55)	(3.37)	(1.65)	(-0.16)	(-3.06)	(-5.00)	(6.78)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	40,647	41,624	41,624	41,624	22,350	41,984	41,936	41,936
R^2	0.066	0.486	0.255	0.496	0.778	0.928	0.926	0.854

(2010) evidences that a long tenure in combination with poor prior performance is also a viable entrenchment proxy. We follow Salas (2010) and use a tenure longer than 10 years in combination with a negative three-year alpha (estimated from a market model regression) prior to death in order to identify entrenched CEOs. We obtain 114 affected competitors using this managerial entrenchment measure.

Repeating the main analysis with the alternative entrenchment measure confirms our previous findings. The cumulative abnormal return of the average competitor over a five-day event window (unreported) is statistically different from zero and amounts to 2.40% (t-value: 3.46) using the market model. Table 2.12 presents the DiD-regressions with controls. In line with the forward-looking stock prices, the average operating performance of the treated firms increases significantly two years after the event date. We also document increases in equity risk, corporate policy risk, and sales margins and a decrease in cost of goods sold.

4 Shareholder Proposals

The death of an entrenched CEO is a major and irreversible corporate event and exogenous to both the focal firm and its competitors. However, this natural experiment has a rather low number of occurrences. In this section, we use an alternative and more frequent exogenous shock to managerial entrenchment that allows us to estimate the consequences of entrenchment for a larger set of focal firms and their competitors: close votes on entrenchment-related shareholder-sponsored proposals.

Recent studies show that shareholder-sponsored governance proposals that fail or pass by a small vote margin are exogenous to a firm's corporate governance structure because the vote outcome can hardly be anticipated and is also uncorrelated with firm characteristics (see, e.g., Cuñat, Gine, and Guadalupe, 2012; Flammer, 2015a). Consequently, the quasi-random assignment of firms into groups with positive and negative corporate governance shocks allows one to estimate the causal impact of corporate governance on firm value, e.g., by investigating abnormal announcement returns upon the day of the vote (Cuñat, Gine, and Guadalupe, 2012). In our setting, the close passing of shareholder proposals related to a reduction of managerial entrenchment should be exogenous to the focal firms (the firms whose shareholders voted on a proposal) and followed by positive announcement returns. If the shareholders of competing firms expect the positive effect of the reduction in entrenchment to have an incentive effect on their own firm through increased competitive pressure, competing firms should simultaneously experience positive stock-price reactions around the announcement of the passing. In contrast, the close failing of such a proposal should be accompanied by negative announcement returns for both the focal firms and competitors due to disappointed shareholders.

To measure the effect of close vote outcomes on focal firms' and competitor' stock-market returns, we follow Lee and Lemieux (2010) and employ a regression discontinuity design by estimating the following regression:

$$Dep_{ij} = \alpha + \theta Pass_j + \eta_j + \epsilon_{ij}, \tag{2.2}$$

where Dep_{ij} is the dependent variable in the firm of interest i on the day of the vote on proposal

Table 2.12: Robustness: Difference-in-differences Regressions with Control Variables for an Alternative CEO Entrenchment Measure

This table reports the results of difference-in-differences regressions with control variables for an alternative entrenchment measure. *Post* is a dummy variable equal to one in the post-death period and zero in the pre-death period. *Treated* is a dummy variable equal to one if a firm is affected by a sudden death of an entrenched CEO in a competing firm and zero otherwise. Entrenched CEOs are managers with a tenure larger than 10 years and a negative three-year alpha computed by a market model regression with CRSP value-weighted index as benchmark. Competitors are only chosen from companies listed in the S&P1500. Competitors are defined using 4-digit SIC codes. Controls added are *Treated, Post, HHI, ROA, R&D, Leverage, Market value, and Sales growth.* All regressions include firm-fixed effects. All dependent variables except of *Sales* and *Cogs* are winsorized at the 5% level. Standard errors are clustered at the firm level. *t*-statistics are in parentheses. *** and ** denote 1% and 5% statistical significance. Dependent and control variables are defined in Appendix A.

	OROA$_{adj}$	**Total risk**	**Syst. risk**	**Idios. risk**	**R&D**	**Sales**	**Cogs**	**Sales margin**
	[-1 +1]	**[-1 0]**	**[-1 0]**	**[-1 0]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**
PostxTreated	0.003	0.022***	0.082	0.019***	0.010***	0.040	0.032	0.037
	(0.42)	(5.38)	(0.81)	(4.30)	(2.79)	(1.21)	(0.83)	(1.03)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	11,414	11,666	11,666	11,666	6,226	11,818	11,806	11,806
R^2	0.261	0.605	0.392	0.595	0.821	0.943	0.942	0.903
	[-1 +2]	**[-1 +1]**	**[-1 +1]**	**[-1 +1]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**	**[-1 +2]**
PostxTreated	0.013**	0.004	-0.058	-0.002	0.017***	-0.029	-0.216***	0.543***
	(2.00)	(0.70)	(-0.53)	(-0.47)	(2.91)	(-0.65)	(-3.52)	(6.62)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	10,912	10,916	10,916	10,916	5,930	11,217	11,203	11,203
R^2	0.203	0.635	0.416	0.645	0.807	0.938	0.941	0.896

j. $Pass_j$ is a dummy equal to one if the proposal j passes, i.e., the votes in favor (v_j) are larger than a required threshold (v^*), and zero otherwise. α is the average impact of the vote on the dependent variable. η_{ij} is a year dummy and ϵ_{ij} is an error term. θ is the unbiased estimate of the vote outcome on the dependent variable and can be interpreted as causal if a narrow vote margin is used. Specifically, the regression discontinuity design exploits the fact that in an arbitrarily small interval around the discontinuity (the threshold v^*), the passing or failing of a proposal is akin a random assignment, which allows a causal interpretation of the treatment effect θ. To improve the efficiency of the estimation and to provide robustness of our results, we also use polynomials on the vote share to approximate the continuous underlying relationship between the dependent and independent variables:

$$Dep_{ij} = \alpha + \theta Pass_j + P_r(v_j, \gamma^r) + P_l(v_j, \gamma^l) + \eta_j + \epsilon_{ij} \tag{2.3}$$

$P_r(v_j, \gamma^r)$ is the polynomial for observations on the right-hand side of the threshold v^* and $P_l(v_j, \gamma^l)$ is the polynomial on its left-hand side. As Lee and Lemieux (2010) argue that high-order polynomials may overestimate the true effect if only votes within a narrow margin are considered, we follow their suggestion and use polynomials of order zero and order one in the main analysis. In the robustness section, we present results for higher-order polynomials of degree two and three. If the shareholders of focal firms vote closely on more than one proposal in a given shareholder meeting, we follow the method of Cuñat, Gine, and Guadalupe (2012) and aggregate the proposals and their vote shares linearly.

Our dependent variable, Dep_{ij}, is the abnormal stock return of either the focal firm or one of its competitors i on the day of the shareholder meeting. Following prior literature, we use the Carhart (1997) four-factor model to compute abnormal returns in the main analysis and report results based on market-model estimations for robustness purposes (Cuñat, Gine, and Guadalupe, 2012; Flammer, 2015a). We follow Flammer (2015a) and estimate the parameters for both return models over a 200-day period ending 20 days before the event day using the CRSP value-weighted index as benchmark.[10] In all regressions that examine the focal firms' market reactions, we cluster standard errors at the firm level. We cluster standard errors at the meeting level for regressions investigating the competitors market reactions because the returns of each focal firm's competitors are unlikely independent around the meeting day.

We use shareholder proposals for the period 1997-2013. The data includes information on voted proposals for firms within the S&P1500 universe. From the entire set of proposals, we only consider those related to the entrenchment-index (E-Index) of Bebchuk, Cohen, and Ferrell (2008). The E-Index comprises six corporate governance and bylaw provisions that weaken shareholder rights and protect the management from being removed: (1) classified boards, (2) poison pills, (3) golden parachutes, (4) supermajority voting requirements, as well as (5) restrictions on shareholders' ability to carry charter and (6) bylaw amendments.[11] Following previous studies (Lee and Lemieux, 2010; Cuñat, Gine, and Guadalupe, 2012; Flammer, 2015a), we only consider close votes, i.e., votes within 10, 5, or 2.5 percentage points to each side of the majority threshold because the outcomes

[10] We find similar results using the estimation window of our analysis of entrenched CEO deaths, starting 260 trading days before and ending 10 days prior to the event.

[11] For a detailed description of each type of provision see Bebchuk, Cohen, and Ferrell (2008).

Table 2.13: Summary Statistics for E-Index Proposals

This table shows summary statistics for E-Index proposals (Panel A), the firms whose shareholders voted on these proposals (Panel B), and the competitors of these firms (Panel C). Panel A displays the frequency of each E-Index proposal type within a 10%, 5%, and 2.5% vote margin. Panel B and C present statistics on abnormal returns computed for the meeting day and accounting variables taken from the fiscal year prior to the meeting day. Abnormal returns are calculated using the Carhart (1997) four-factor model and an estimation period ranging from 220 to 20 trading days prior to the event. Competitors are assigned according to 4-digit SIC codes. Competitors are only chosen from companies listed in the S&P1500. All accounting variables are defined in Appendix A. The number of observations may change due to missing values for some accounting variables.

Panel A: Distribution of E-Index Proposals

	±10		±5		±2.5	
	N	**%**	**N**	**%**	**N**	**%**
Repeal classified board	185	56.23%	94	54.97%	44	50.57%
Repeal poison pill	43	13.07%	31	18.13%	18	20.69%
Golden parachute	38	11.55%	16	9.36%	7	8.05%
Eliminate supermajority provision	41	12.46%	17	9.94%	7	8.05%
Restrictions on bylaws & charter amendments	22	6.69%	13	7.60%	11	12.64%
Total	329	100%	171	100%	87	100%

Panel B: Focal Firm Characteristics

	N	**Mean**	**Median**	**Std. dev.**	**Q1**	**Q3**
Abnormal returns (%)	329	0.10	0.00	2.47	-0.96	0.86
Market value (ln)	292	8.66	8.96	2.02	7.34	10.05
ROA	294	0.03	0.03	0.10	0.01	0.06
Sales growth	279	0.07	0.06	0.23	-0.03	0.15
Leverage	305	0.21	0.19	0.15	0.10	0.28

Panel C: Competitor Characteristics

	N	**Mean**	**Median**	**Std. dev.**	**Q1**	**Q3**
Abnormal returns (%)	5,851	0.06	0.04	2.68	-0.92	1.06
Market value (ln)	5,271	7.86	7.62	1.53	6.77	8.85
ROA	5,275	0.02	0.01	0.07	0.01	0.04
Sales growth	4,746	0.13	0.07	0.43	-0.03	0.20
Leverage	5,841	0.15	0.11	0.14	0.05	0.24

of votes that are further away from the cutoff point are likely anticipated by market participants and do not trigger stock-price reactions. We exclude meetings of firms that voted closely on more than one proposal in the same shareholder meeting if the outcomes are mixed, i.e., some proposals passed while others failed. However, our results remain unchanged if we refrain from excluding these votes. We also exclude shareholder meetings whenever there is a close vote on a proposal unrelated to the E-Index. Finally, we require the availability of at least one competitor per firm. Our final sample consists of 329 close votes on entrenchment-related proposals (within 10 p.p. in each direction of the majority threshold) in 203 unique firms that operate in 93 unique 4-digit SIC industries. The votes affect 5,851 (868 unique) competitors with available stock-return data. Panel A of Table 2.13 shows the distribution of proposals by type within the three vote margins of 10, 5, and 2.5 percentage points. Panel B and Panel C present statistics for the focal firms and competitors (based on a 10 p.p. margin), respectively.

Following Flammer (2015a), we test the assumption that the assignment of firms into pass and fail is random. By applying the McCrary (2008) test for smoothness of the density function around the threshold (within a 10 p.p. margin), we find no evidence for a discontinuous jump, which suggests that the assignment is random. In addition, we compare ex-ante characteristics of firms whose vote shares are below the majority threshold to those of firms whose shares are above

the majority threshold (again within a 10 p.p. margin). If the outcome of close-vote proposals is akin random, the characteristics of the two groups of firms should not differ significantly. We also test for pre-existing differences between competitors of firms with vote shares below the majority threshold and competitors of firms with vote shares above the majority threshold to show that the competitor assignment into pass and fail is also akin random.

Table 2.14: Pre-existing Differences in Firm Characteristics as a Function of Vote Outcome

This table tests whether the passing or failing of an E-Index proposal is systematically related to focal firm characteristics prior to the meeting (Panel A) and whether competitors are randomly assigned into passing or failing groups (Panel B). Competitors are assigned according to 4-digit SIC codes and belong to the S&P1500 index. Accounting variables are taken from the last fiscal year prior to the meeting day. Changes in accounting variables are computed by taking the difference from one fiscal year to two fiscal years prior to the meeting. Abnormal returns are computed for one day prior to the meeting using the Carhart (1997) four-factor model with an estimation period ranging from 220 to 20 trading days prior to the event. Changes in abnormal returns are the difference in abnormal returns from one day to two days prior to the meeting day. Each row corresponds to a separate regression of the dependent variable on a *Pass* dummy equal to one if the proposal was passed and zero otherwise. Columns I and II solely include year-fixed effects. Columns III and IV include year-fixed effects and polynomials of order one for each side of the majority threshold. Standard errors are clustered at the firm level in Panel A and at the meeting level in Panel B. *t*-statistics are in parentheses. All accounting variables are defined in Appendix A.

Panel A: Pre-existing Differences in Focal Firm Characteristics				
	Before Meeting: (t-1)	**Change: (t-2) to (t-1)**	**Before Meeting: (t-1)**	**Change (t-2) to (t-1)**
Abnormal returns (%)	0.0136	0.100	-0.523	0.154
	(0.05)	(0.25)	(-1.00)	(0.23)
Market value (ln)	-0.265	-0.070	-0.628	-0.179
	(-1.08)	(-1.11)	(-1.30)	(-1.56)
ROA	-0.010	0.002	0.023	0.011
	(-0.84)	(0.21)	(0.68)	(0.80)
Sales growth	-0.040	-0.028	-0.058	-0.062
	(-1.64)	(-0.81)	(-0.99)	(-0.74)
Leverage	0.008	0.006	0.013	0.012
	(0.44)	(0.89)	(0.36)	(0.70)
Polynomials	0	0	1	1
Panel B: Pre-existing Differences in Competitor Characteristics				
	Before Meeting: (t-1)	**Change: (t-2) to (t-1)**	**Before Meeting: (t-1)**	**Change (t-2) to (t-1)**
Abnormal returns (%)	-0.049	-0.005	0.163	0.637
	(-0.24)	(-0.02)	(0.30)	(0.98)
Market value (ln)	0.046	0.011	0.129	-0.048
	(0.76)	(0.43)	(1.02)	(-0.85)
ROA	-0.003	-0.002	-0.006	-0.006
	(-0.71)	(-0.76)	(-0.70)	(-1.01)
Sales growth	0.029	-0.012	-0.034	-0.086
	(1.31)	(-0.38)	(-0.62)	(-1.26)
Leverage	-0.021	0.004	-0.054	0.006
	(-1.45)	(1.64)	(-1.60)	(1.09)
Polynomials	0	0	1	1

Table 2.14, columns I and III present regression results for the differences of characteristics in the year (or day) prior to the shareholder meeting. In columns II and IV, we report the results for differences in the changes of these characteristics from two years (two days) prior to the meeting day to one year (one day) prior to the meeting day. Each regression contains year dummies and either clusters standard errors at the firm level (Panel A) or at the meeting level (Panel B). The regressions presented in columns I and II are estimated without polynomials, whereas the regressions presented in columns III and IV control for first-order polynomials on both sides of the threshold. As shown in Panel A, firms that pass E-Index proposals do not significantly differ from firms that reject these proposals in terms of prior (changes in) daily abnormal returns, market values, returns on assets, sales growths, or leverage ratios. In addition, we do not find any significant difference between

competitors of firms whose shareholders voted in favor of governance proposals and competitors of firms that rejected them. This result indicates that both focal firms and their competitors are not selected into either side of the discontinuity due to different pre-existing characteristics, which supports our identification strategy.

Table 2.15: Abnormal Returns around the Majority Threshold

This table reports regression results on abnormal returns for the voting day (t) based on a *Pass* dummy which is equal to one if the E-Index proposal passed and zero otherwise. Abnormal returns are computed using the Carhart (1997) four-factor model with an estimation period ranging from 220 to 20 trading days prior to the event. Columns I and II, report results for proposals whose vote shares are within 10% of the majority threshold. Columns III and IV (columns V and VI) restrict the sample to proposals whose vote shares are within 5% (2.5%) of the majority threshold. Competitors are assigned according to 4-digit SIC codes and belong to the S&P1500 index. Columns I, III, and V are estimated using equation 2.2 that controls for year-fixed effects and clusters standard errors either at the firm level (Panel A) or at the meeting level (Panel B), respectively. Columns II, IV, and VI include polynomials of order one for each side of the majority threshold and year-fixed effects. Standard errors are clustered at the firm level (Panel A) or at the meeting level (Panel B), respectively. t-statistics are in parentheses. ***, **, and * denote 1%, 5%, and 10% statistical significance.

Panel A: Abnormal Returns of Focal Firms at the Day of Vote (t)						
	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
Pass	0.788***	2.041***	1.327***	2.393**	2.210**	1.823
	(3.00)	(3.19)	(3.09)	(2.36)	(2.34)	(1.11)
Obs.	329	329	171	171	87	87
R^2	0.084	0.114	0.148	0.165	0.190	0.244
Polynomials	0	1	0	1	0	1
Vote margin	±10	±10	±5	±5	±2.5	±2.5
Panel B: Abnormal Returns of Competitors at the Day of Vote (t)						
	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
Pass	0.428***	0.715**	0.589***	0.857*	0.761***	1.401**
	(2.59)	(2.18)	(2.86)	(1.83)	(2.65)	(2.11)
Obs.	5,851	5,851	2,622	2,622	1,344	1,344
R^2	0.016	0.017	0.023	0.025	0.076	0.084
Polynomials	0	1	0	1	0	1
Vote margin	±10	±10	±5	±5	±2.5	±2.5

Table 2.15 shows estimates of the differences in abnormal returns on the voting day between E-Index proposals that pass and E-Index proposals that fail by increasingly small intervals around the majority threshold. Panel A reports results for focal firms. Column I contains the sample of proposals whose votes in favor are within 10 p.p. of the majority threshold. The difference in abnormal returns amounts to 0.79 percentage points and is significant at the 1% level. This difference remains highly significant and becomes even larger in magnitude (up to 2.39 percentage points) in column III and column IV, where we restrict our sample to proposals whose vote shares are within 5 p.p. and 2.5 p.p., respectively. Column II, IV, and VI replicate the findings by including two polynomials of order one. In column VI, the difference remains economically relevant but becomes statistically insignificant, which is likely attributable to the smaller sample size. Panel B reports the results for competing firms. The difference in abnormal returns is significantly positive in all regressions ranging from 0.43 percentage points (column I) to 1.40 percentage points (column VI). This finding supports our argument that a reduction of managerial entrenchment in the focal firm is expected to incentivize the management of competing firms to work harder, improve performance, and increases shareholder value. Although the lower magnitudes in abnormal returns for competing firms indicate that the entrenchment shock is stronger for the focal firm, one needs to take into account that the outcome affects almost 18 competitors per focal firm. In addition, the value effect is smaller for shareholder proposals than for sudden deaths because the entrenchment

shock is less disruptive and the proposals are non-binding, which leads to an underestimation of the true value of an actual implementation (Cuñat, Gine, and Guadalupe, 2012).

Table 2.16: Robustness: Shareholder Proposals

This table reports various robustness checks. In Panel A, abnormal returns are computed using the market model instead of the Carhart (1997) four-factor model. In Panel B, regressions of abnormal returns (estimated with the Carhart (1997) four-factor model) include two polynomials for each side of the majority threshold, either of order two or three. In Panel C, competitors are assigned according to their 3-digit SIC instead of their 4-digit SIC codes. In Panel D, competitors are assigned according to their product similarity provided by Hoberg and Phillips (2016) instead of their 4-digit SIC codes. *Pass* is a dummy equal to one if the proposal was passed and zero otherwise. All regressions control for year-fixed effects and cluster standard errors at the meeting level. *t*-statistics are in parentheses. ***, **, and * denote 1%, 5%, and 10% statistical significance.

Panel A: Market Model						
	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
Pass	0.434**	0.815*	0.696***	-0.312	0.819***	1.448*
	(2.07)	(1.93)	(2.60)	(-0.48)	(2.70)	(1.88)
Obs.	5,851	5,851	2,622	2,622	1,344	1,344
R^2	0.0246	0.0269	0.0257	0.0369	0.0971	0.1026
Polynomials	0	1	0	1	0	1
Vote margin	±10	±10	±5	±5	±2.5	±2.5
Panel B: High-Order Polynomials						
	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
Pass	1.047**	2.322***	2.596***	3.121***	1.882*	2.945**
	(2.07)	(3.14)	(2.88)	(3.09)	(1.66)	(2.54)
Obs.	5,851	5,851	2,622	2,622	1,344	1,344
R^2	0.0202	0.0277	0.0378	0.0391	0.0909	0.0926
Polynomials	2	3	2	3	2	3
Vote margin	±10	±10	±5	±5	±2.5	±2.5
Panel C: Competitors by 3-digit SIC Codes						
	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
Pass	0.411***	0.790***	0.712***	0.710*	0.761***	1.294**
	(3.07)	(2.83)	(4.23)	(1.75)	(3.12)	(2.10)
Obs.	7,666	7,666	3,579	3,579	1,775	1,775
R^2	0.0129	0.0146	0.0227	0.0230	0.0686	0.0778
Polynomials	0	1	0	1	0	1
Vote margin	±10	±10	±5	±5	±2.5	±2.5
Panel D: Competitors by Product Similarity (Hoberg and Phillips, 2016)						
	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
Pass	0.277***	0.673***	0.582***	0.660*	0.835***	1.221*
	(2.60)	(2.76)	(4.02)	(1.67)	(3.23)	(1.77)
Obs.	9,576	9,576	4,374	4,374	2,167	2,167
R^2	0.0151	0.0173	0.0325	0.0328	0.0458	0.0483
Polynomials	0	1	0	1	0	1
Vote margin	±10	±10	±5	±5	±2.5	±2.5

Table 2.16 confirms the robustness of the results. We find that the results are robust to using a market model instead of a Carhart (1997) four-factor model (Panel A), polynomials of degree two and three (Panel B), and competitors assigned according to 3-digit SIC codes (Panel C) or the product similarity approach of Hoberg and Phillips (2016) (Panel D).

5 Conclusion

This paper studies the impact of managerial entrenchment on competitors. The exogenous termination of managerial entrenchment triggered by the sudden death of an entrenched CEO allows a causal inference on this question. Our results provide strong evidence that competitors experience a significant increase in firm value after the sudden death of an entrenched CEO in a competing

firm. We confirm the positive value implications for competing firms by examining close-votes on E-Index related shareholder proposals as an additional and more frequent entrenchment shock. Furthermore, by exploiting the irreversible and pervasive nature of sudden CEO deaths, we examine potential channels through which the rise in value may be explained. We show that subsequent to the entrenchment shock, competitors significantly increase their operating performance, total equity risk, idiosyncratic risk, R&D intensity, and sales margin, but not their sales. These findings suggest that competitors raise effort and initiate investments in riskier projects. The improvement in sales margins along with stable decreases in costs of goods sold combined with the observed negative stock-price reactions of supplier and customer industries suggest that competing firms may renegotiate contracts with existing customers and suppliers. We interpret these findings as an attempt to react on and to catch up with a competitor who is expected to perform better after a positive entrenchment shock.

By switching the focus from companies with an entrenched management to their competitors and by exploiting two natural experiments – the death of entrenched CEOs and the vote outcomes of shareholder-sponsored proposals – our paper provides causal evidence that managerial entrenchment spills over to a large number of firms. In this respect, our paper suggests that managerial entrenchment in one firm destroys a significant amount of shareholder wealth by reducing competitive pressure.

Chapter 3

Uncovering the Role of Tone: Evidence from CEO Dismissals

with Patrick Hauf (University of Konstanz)

Abstract: This paper uncovers the role of pessimistic media and managerial tone in forced CEO turnovers. By analyzing pessimistic tone in firm-specific newspaper articles to measure media tone and quarterly and annually disclosed company filings to measure managerial tone, we provide a differentiated view on how both information sources relate to CEO replacement decisions and the associated announcement returns. Using a large set of CEO dismissals over the years 1996 and 2012, we find that pessimistic managerial tone significantly increases the likelihood of CEO dismissals but does not influence the investor reactions upon the dismissal announcement. In contrast, pessimistic media tone does not affect the board's firing decision but the corresponding investor reactions. This effect, however, reverses in the days following the announcement, indicating a sentiment-driven mispricing. Overall, our results show that pessimistic media tone does not serve as a comprehensive governance mechanism. Further, managers have to raise consciousness regarding their tone of voice. The findings additionally suggest that investors seem to rely more on the media's than on the managers' tone when evaluating the board's firing decision.

1 Introduction

Tone has been shown to affect the decisions made by firms, the board of directors and investors. Pessimistic media tone can serve as a corporate governance mechanism by putting pressure on the board to act in the best interest of shareholders (Bednar, 2012). To avoid a loss of reputation and stigmatization caused by negative press coverage (Wiesenfeld, Wurthmann, and Hambrick, 2008), firms increase the independence of their boards (Bednar, 2012) and board directors adjust the CEO's compensation in a way that is less likely to upset investors (Bednar, 2012; Kuhnen and Niessen, 2012). Previous studies also provide weak evidence that CEOs are more likely to get dismissed following negative press coverage (Core, Guay, and Larcker, 2008; Bednar, 2012).

Besides providing a governance function, media tone is capable of shaping investor sentiment by being overly optimistic or pessimistic about the firm (Tetlock, 2007; Tetlock, Saar-Tsechansky, and Macskassy, 2008; Dougal, Engelberg, Garcia, and Parsons, 2012; Garcia, 2013; Ferguson, Philip, Lam, and Guo, 2015; Heston and Sinha, 2015; Ahmad, Han, Hutson, Kearney, and Liu, 2016). As a result, investors get biased and asset prices can temporarily drive away from their fundamental values (Tetlock, 2007). Recent findings evidence, however, that pessimistic newspaper coverage is not the only driver of investor perception. Specifically, managers can actively evoke optimistic but also pessimistic investor perceptions about their ability and the future firm performance through an overly optimistic or pessimistic tone in company filings (Jiang, Lee, Martin, and Zhou, 2017).

In this paper, we analyze the effect of pessimistic managerial and media tone on a board's decision to fire the CEO. CEO dismissals belong to the board's major responsibilities and result from a consensus of the board of directors that the benefit of replacing the incumbent CEO exceeds the expected cost (Huson, Malatesta, and Parrino, 2004). Hence, the firing decision is driven by the board's perception of the company's standing and the quality of the CEO. Intuitively, both pessimistic media and pessimistic managerial tone should reduce trust in the CEO's ability and the future success of the firm which negatively affects the board's sentiment while increasing the pressure on directors to fire the manager.[1] In addition, both pessimistic managerial and media tone should drive investor sentiment which increases the propensity for a sentiment-driven overreaction upon the announcement of a dismissal. However, it is an open empirical question whether or not these hypothesized effects are observable in CEO dismissals.

We aim at answering this question by analyzing the effect of pessimistic media and managerial tone on the likelihood of a CEO firing and the corresponding stock market reactions for S&P1500 firms between 1996 and 2012. Our sample contains 675 forced turnovers. Following Jiang, Lee, Martin, and Zhou (2017), we measure managerial pessimism based on the negative tone in 10-Q and 10-K filings. To measure pessimistic media tone, we collect over 800,000 newspaper articles, covering the four major U.S. newspapers with weekday circulation: USA Today (USAT), New York Times (NYT), Washington Post(WP), and the Wall Street Journal (WSJ). Using Loughran and

[1] Composed by the management, annual reports (10-K) and the corresponding quarterly (10-Q) filings are of major interest for the board because they play a crucial role with respect to their monitoring and advisory functions. In particular, the content presented in 10-K filings requires approval by at least 50% of the board members ($https://www.sec.gov/about/forms$).

McDonald (2011)'s word list, we compute the pessimistic tone of newspaper articles and filings as the fraction of negative words to total words. We focus on pessimistic tone for two reasons. First, in various contexts, negative information is processed more rigorously than positive information and is therefore more influential (Baumeister, Bratslavsky, Finkenauer, and Vohs, 2001; Rozin and Royzman, 2001). Second, analyzing positive words to obtain optimistic tone may create biased results due to potential negations. The negation of negative words such as "no terrible earnings", however, is not expected to appear frequently in a financial context (Loughran and McDonald, 2011).

The results obtained from probit regressions reveal that managerial pessimism is positively linked to the propensity to dismiss the CEO. A calculation of the marginal effects at means indicates that an increase in the proportion of managerial pessimism by 1%, i.e., from 2% to 3%, yields an increase of 1.5% in the turnover probability. In contrast, pessimistic media tone does not seem to influence the board's firing decision. By analyzing the corresponding announcement returns, we observe an opposite picture. Event-study results show that firings of CEOs with above-average pessimistically-toned news articles exhibit positive stock-market reactions of 1.61% on the announcement day. These abnormal stock returns are on average 1.68% higher than for companies with less pessimistic press releases. The effect of media tone, however, dissipates in the post-dismissal period. We do not find that pessimistic managerial tone influences the investors' reactions. Cross-sectional analyses confirm these event-study results. Overall, our findings suggest that pessimistic managers are more likely to get dismissed by the board of directors. However, there is no evidence for a comprehensive governance mechanism of pessimistic media coverage. In addition, only media but not managerial tone seem to affect investor sentiment, leading to short-term overreactions as a response to the board's firing decision.

Our results are robust to a large battery of alternative tests that address estimation biases and endogeneity concerns. First, we use various return models and event windows in the event study and in the corresponding cross-sectional regressions to reduce concerns that our results are artifacts of specific modeling choices. Second, we consider only newspaper articles that are identified as highly relevant for the firm as suggested by a relevance score from LexisNexis. Thereby, we increase the confidence that the newspaper articles we evaluate have a strong focus on the investigated company. Third, we perform semi-parametric Generalized Estimating Equations (GEE) instead of our probit analysis to account for different possible (auto-)correlation structures among the yearly observations of one firm. Fourth, we run a matching approach to deal with heterogeneity between the sample of firms where a forced CEO turnover occurred compared to those without a dismissal. Finally, we repeat the probit estimation using a measure of abnormal tone instead of absolute tone to filter out company performance and other characteristics as potential driver of our results.

With this paper, we primarily contribute to the literature on CEO turnovers (see, e.g., Warner, Watts, and Wruck, 1988; Kang and Shivdasani, 1996; Ertugrul and Krishnan, 2011; Eisfeldt and Kuhnen, 2013; Jenter and Lewellen, 2015; Jenter and Kanaan, 2015, among others) as well as to research on tone and sentiment (see e.g., Tetlock, 2007; Tetlock, Saar-Tsechansky, and Macskassy, 2008; Groß-Klußmann and Hautsch, 2011; Dougal, Engelberg, Garcia, and Parsons, 2012; Boudoukh, Feldman, Kogan, and Richardson, 2013; Garcia, 2013; Hillert, Jacobs, and Müller, 2014;

Chen, De, Hu, and Hwang, 2014; Ferguson, Philip, Lam, and Guo, 2015; Heston and Sinha, 2015; Ahmad, Han, Hutson, Kearney, and Liu, 2016, among others). To the best of our knowledge, we are the first to show that pessimistic managerial tone, measured by the tone in 10-Ks and 10-Qs, is a significant predictor of CEO dismissals. This finding underscores the relevance of public disclosures for a company's management and suggests that CEOs have to raise consciousness regarding their tone of voice because it can negatively affect the board's perception. Second, our study adds to previous research focusing on the interplay of media and CEO turnover (Farrell and Whidbee, 2002; Core, Guay, and Larcker, 2008; Bednar, 2012). Whereas Core, Guay, and Larcker (2008) cannot identify a relationship between pessimistic media tone and CEO dismissals, the more recent work of Bednar (2012) evidences a weak relationship. However, since Loughran and McDonald (2011) suggest that a non-financial word list, as used by Bednar (2012), might not be the optimal choice for investigating the media tone about corporations, the use of a financial word list, as applied in this study, contributes an additional view on the topic. In addition, Core, Guay, and Larcker (2008) do not distinguish between voluntary and forced turnovers and Bednar (2012)'s mechanical identification of dismissals is based on a resignation variable in the Execucomp database which may contain voluntary turnovers or neglect certain forced turnovers. Our study applies an algorithm that is based on an in-depth analysis of each CEO turnover and is thus able to separate forced turnovers from voluntary departures. An exact determination of dismissals is particularly necessary because voluntary turnovers are based on the decision of the CEO and do not reflect the decision made by the board of directors. Finally, by contributing a joint investigation of the influence of two tone measures on firing decisions and the corresponding investor reactions, our study suggests that managerial and media tone are not one and the same but have to be considered separately. In the context of CEO dismissals, managerial pessimism tends to influence the board's decision whereas media pessimism seems to shape investor decisions. In this respect, the findings of this paper set the stage for future research that may investigate when one type of tone is more important than the other.

The paper is structured as follows: In section 2, we describe the data and the methodology used to determine document tone and to identify forced CEO turnovers. In section 3, the empirical approach and the results are presented. Section 4 concludes.

2 Methodology and Data

2.1 Identification of Media and Managerial Tone

To identify pessimistic media and managerial tone, we employ a textual analysis approach that analyzes the tone of a document which is provided by the media or the management, respectively. Specifically, we follow Jiang, Lee, Martin, and Zhou (2017) and measure managerial tone based on the tone used in 10-Q and 10-K disclosed company filings. Although these filings satisfy a regulatory need, have a standardized format, and present the results in form of numbers, there is a substantial amount of text that is authored at the discretion of the management and may at least partially reflect the managers' beliefs and expectations about the firm's future performance (Jiang,

Lee, Martin, and Zhou, 2017). To identify media tone, we follow extant literature and measure the tone used in newspaper articles (see e.g., Hillert, Jacobs, and Müller, 2014; Ferguson, Philip, Lam, and Guo, 2015; Ahmad, Han, Hutson, Kearney, and Liu, 2016).

We determine a document's tone by distinguishing negative from positive or neutral words and by computing their percentage share. Standardizing by all words is particularly important because of the heterogeneity among authors and document sources. We focus only on negative words because positive words are likelier negated which may lead to biased estimations (Loughran and McDonald, 2011). In this respect, our measures of media and managerial tone particularly reflect the pessimism of the respective source. We compute the average firm-specific pessimistic media or managerial tone for each company i in calendar year t as follows:

$$MediaPessimism_{i,t}(ManagerialPessimism_{i_t}) = \frac{1}{|\Omega_{i,t}|} \sum_{\Omega i,t} \frac{\#\text{ Negative Words in Article (Filing)}}{\#\text{ All Words in Article (Filing)}}, \quad (3.1)$$

where $\Omega_{i,t}$ denotes the set of all firm-specific articles (filings) for company i in year t. To ensure a high measurement quality, we follow the related literature (Garcia, 2013; Hillert, Jacobs, and Müller, 2014; Ahmad, Han, Hutson, Kearney, and Liu, 2016) and identify negative words by using the word list of Loughran and McDonald (2011). In contrast to other word lists such as the Inquirer's Harvard IV-4 psychosocial dictionary (see Kearney and Liu, 2014, for a review), Loughran and McDonald (2011)'s word list is particularly calibrated to fit a financial context. For example, while words such as *board*, *liability*, or *cost* are classified as negative according to the Inquirer's Harvard IV-4 psychosocial dictionary, they are considered as neutral using the word list of Loughran and McDonald (2011).

Further, a common misconception related to text-based tone classification in a financial context is to assume that pessimistic tone and poor company performance are two sides of the same coin. Since an understanding of the distinct nature of tone and performance is important to follow our argument throughout this paper, we want to briefly exemplify our measurement of tone and its relationship to performance by the following two statements:

I. The CEO of Company A considers the increase in earnings as the result of central cost reductions. In spite of a demanding market environment, she sees the firm to be well-positioned for the future.

II. The CEO of Company B considers the increase in earnings as the result of **crucial** cost **cutbacks**. In spite of a **challenging** market environment, she sees the firm to be well-positioned for the future.

Negative words, as indicated by Loughran and McDonald (2011)'s word list, are printed in **bold**. We see that not a single word in statement I is printed in bold. Since we define pessimistic tone as the fraction of negative words to total words, the rate of pessimism in statement I is 0. For statement II, we count three negative words: crucial, cutbacks, and challenging. Hence, the rate of

pessimism is $\frac{3}{35} = 0.086$. Consequently, although statement II presents the same content in terms of fundamentals, it has a more pessimistic tone attached to it.

2.2 Data and Sample Construction

To analyze CEO dismissals, we use panel data of firms that are covered by ISS Riskmetrics over the period from 1996 to 2012. Riskmetrics comprises firm-year observations of S&P1500 firms. In order to identify firm-years in which a forced turnover occurs, we manually collect announcements of CEO turnovers by screening newspaper articles gathered from LexisNexis. Following Parrino (1997), we classify a CEO turnover as forced if the press either directly reports that the CEO is *fired*, *forced from position* or *dismissed* by the board or if internal conflicts or policy differences were present. A turnover is also declared as forced if (i) the CEO has not exceeded his or her 60th year of age and (ii) the press did not report death, poor health, or the acceptance of a new (comparable) position or (iii) the media reports that the CEO is retiring but did not make the announcement at least 6 months in advance. All remaining turnovers are viewed as voluntary departures. We create a dummy variable equal to one if the firm-year is affected by a forced turnover and zero otherwise.

The basis to proxy media pessimism is the tone of newspaper articles, obtained from LexisNexis. Following Hillert, Jacobs, and Müller (2014) and Bajo and Raimondo (2016), we consider newspaper articles published by one of the four most reputable and widespread U.S. newspapers with weekday circulation: USA Today (USAT), New York Times (NYT), Washington Post (WP), and the Wall Street Journal (WSJ). To proxy pessimistic managerial tone, we obtain information on the number of negative to total words for disclosed 10-K and 10-Q filings in each sample firm and year from Bill McDonald's Word List Page.[2]

Within our empirical analysis, we include a large set of controls related to board, industry, company, and CEO characteristics that are motivated by findings from the CEO turnover literature. We add prior performance which has been repeatedly shown to be a major factor related to CEO replacements (see e.g., Warner, Watts, and Wruck, 1988; Kang and Shivdasani, 1996; Ertugrul and Krishnan, 2011; Eisfeldt and Kuhnen, 2013; Jenter and Lewellen, 2015; Jenter and Kanaan, 2015, among others). Specifically, CEOs who do not turn out to be capable in bringing financial success face an increased chance of being replaced. Gregory-Smith, Thompson, and Wright (2009) find entrenchment as measured by the CEO's tenure pivotal to the firing probability and further control for age. Gibbons and Murphy (1990) control for market value, and DeFond and Park (1999) for the book-to-market ratio. Goyal and Park (2002) underline the significance of CEO duality whereas Yermack (1996) and Hermalin and Weisbach (1998) suggest considering board size and the fraction of independent directors, respectively. Denis, Denis, and Sarin (1997) additionally emphasize the role of block holders with respect to CEO turnovers.

We use stock market data and calculate a CEO's prior performance (CAR) as the cumulative return in the calendar year prior to the turnover, adjusted by the value-weighted S&P 1500 return. We calculate a firm's market value (MarketValue) as the natural logarithm of the product between

[2] $http://www3.nd.edu/\ mcdonald/Word\_Lists.html$

stock price and common shares outstanding at the year's end. The book-to-market ratio is the natural logarithm of the product between the number of shares outstanding and the book value per share over market capitalization. Return on asset (ROA) is the net income divided by total assets. Following Eisfeldt and Kuhnen (2013), we define IndustryROABelowTrend as a dummy variable that equals one if the average profitability during the three preceding years is below its value during the prior ten years. Firms' institutional block owner data is hand-collected. All board characteristics are obtained from ISS Riskmetrics. In this respect, we calculate board size (BoardSize) as the number of directors in a board. Following Fich and Shivdasani (2006), we define busy boards (BusyBoard) as boards in which the majority of outside directors holds three or more directorships. The portion of independent directors (IndependencePortion) is measured as the fraction of independent outside directors in a board whereas we determine CEOs who simultaneously hold the title of the chairman as dual CEOs (DualCEOs).

2.3 Final Sample and Summary Statistics

Our final sample consists of 30,117 firm-years with 675 forced turnovers between 1996 and 2012 in S&P1500 firms. Table 3.1 reports summary statistics for the full sample. A description of all variables is provided in Table B1 of the appendix.

Table 3.1: Summary Statistics

This table reports summary statistics of control and explanatory variables of our S&P 1500 universe. One *observation* corresponds to a year-company observation. The sample period includes the years 1996 to 2012. *Mean* corresponds to the arithmetic mean, *Median* to the median, *Q1* and *Q3* to the first and third quartile, respectively. *MediaPessimism* (*ManagerialPessimism*) is calculated as the average over the ratio of negative words to total words in all newspaper articles (10-K and 10-Q company filings) per company per year using the word lists by Loughran and McDonald (2011). See Table B1 in the appendix for the definition of the other variables.

	Observations	**Mean**	**Median**	**Q1**	**Q3**
MediaPessimism (in %)	19,601	1.83	1.65	1.05	2.31
ManagerialPessimism (in %)	30,117	1.62	1.57	1.29	1.91
CEOAge	29,231	55.68	56.00	51.00	60.00
CEOTenure	28,485	7.25	5.00	2.00	10.00
MarketValue	23,650	7.33	7.22	6.27	8.34
BookToMarket	22,946	-0.84	-0.77	-1.25	-0.36
ROA	30,096	0.03	0.04	0.01	0.08
CAR	28,736	-0.02	0.00	-0.22	0.21
BoardSize	19,993	9.50	9.00	8.00	11.00
BusyBoard	19,993	0.11	0.00	0.00	0.00
IndependencePortion	19,993	0.70	0.73	0.60	0.83
DualCEO	19,993	0.70	1.00	0.00	1.00
Block	23,778	2.27	2.00	1.00	3.25
IndustryROABelowTrend	30,096	0.43	0.00	0.50	1.00

The age of a CEO (CeoAge) in our sample is on average 56 years with a mean tenure (CEOTenure) of around 7 years. The average company in our sample has a market value of around USD 1.5 billion, and the book-to-market value amounts to 0.43.[3] With respect to the firms' governance structures, we observe that 11% of all boards are busy (BusyBoard), the majority of directors is independent (IndependencePortion), and 70% of our sample CEOs simultaneously hold the position of CEO and chairman of the board (DualCeo). The average firm has 9.5 board members (BoardSize) and

[3]The presented numbers are the transformed log numbers of market value and book-to-market value given in the Table 3.1.

a prior stock (CAR) and operating (ROA) performance of -2% and 3%, respectively. The mean value of 0.43 in IndustryROABelowTrend indicates that in close to half of the cases, the recent profitibality deceeded the long-term firm performance. Furthermore, the firms in our sample have on average two institutional block owners.

Our measure for pessimistic media tone (MediaPessimism) shows a mean of 1.83% and a median of 1.65%. Pessimistic managerial tone (ManagerialPessimism) displays a mean of 1.62% and a median of 1.57%, indicating that managers are on average less pessimistic than the media. Although not reported in this table, in our sample, company tone in a calendar year as measured by MediaPessimism and ManagerialPessimism can get as high as 21% and 7%, respectively. In addition, we measure a low correlation ($\rho = 0.1452$) between the tone measures which implies that the attitude of the managers towards their future prospects differs from the view of the media. The correlations between media or managerial tone and the prior performance variables, CAR and ROA, are also low, indicating that both, media and managerial tone only partially emerge from performance.[4]

Table 3.2 reports additional information on the underlying data used to measure media and managerial tone. Panel A provides the fragmentation of newspaper articles, the number of covered firms, and the average rate of pessimistic tone by newspapers. The New York Times has published most newspaper articles (368,461), covering 2,700 sample firms, whereas USA Today contributes the least number of articles (99,188), covering 1,271 sample firms. Newspaper articles in the USA Today and Washington Post show to be the least pessimistic, with average ratios of 1.76% and 1.78%, respectively, while the Wall Street Journal shows the most pessimistic tone with a score of 2.55%. Panel B provides information on the newspaper articles and filing distribution across time. The number of firm-specific newspaper articles has its peak shortly before the year 2000, which might be attributed to the hype associated with the new economy. After the dot.com bubble, one could conjecture that the press switched to other topics instead. Considering media pessimism, we observe high values in the aftermath of the dot.com bubble (with 2.43% in the year 2002) and the financial crisis in 2007 and 2008 (with 2.36% in the year 2008). Since every company has to report four financial statements within a fiscal year, the number of company filings is rather stable. The yearly variation in the fraction of negative words in filings, ranging from 1.24% to 1.8%, is similar to newspaper articles but on a lower level.

[4]Managerial tone has a stronger link to operating performance ($\rho = -0.1454$) than media tone ($\rho = -0.0769$), whereas the latter has a closer relationship to CAR ($\rho = -0.1055$) than the former ($\rho = -0.0407$).

Table 3.2: Newspaper Descriptives

This table reports summary statistics on all newspaper articles and filings collected for S&P 1500 companies in the years 1996 to 2012 by source (Panel A) and year (Panel B). Newspaper articles are taken from LexisNexis. Company filing data is obtained from Bill McDonald's webpage (*http* : *//www*3*.nd.edu/ mcdonald/Word_Lists.html*). *Source* indicates the source of the article (NYT= New York Times, USAT = USA Today, WP = Washington Post, WSJ= Wall Street Journal), *Number of Articles* the number of articles over the observation period and *Covered Firms* the number of firms, covered by the respective newspaper. *MediaPessimism* is calculated as the average over the ratio of negative words to total words in all newspaper articles per company per year using the word lists by Loughran and McDonald (2011). *Year* is the respective year, *Number of Filings* indicates the number of filings per year, and *ManagerialPessimism* is calculated as the average over the ratio of negative words to total words in all filings using the word lists by Loughran and McDonald (2011).

Panel A: Descriptives by Source

Source	Number of Articles	Covered Firms	Media Pessimism
NYT	368,461	2,700	0.0214
USAT	99,188	1,271	0.0178
WP	208,729	1,771	0.0176
WSJ	213,163	2,590	0.0255

Panel B: Descriptives by Year

Year	Number of Filings	Managerial Pessimism	Number of Articles	Media Pessimism
1996	7,143	0.0124	69,211	0.0199
1997	7,978	0.0124	67,796	0.0202
1998	8,050	0.0135	72,493	0.0201
1999	7,944	0.0153	68,674	0.0187
2000	7,828	0.0145	69,031	0.0198
2001	7,689	0.0156	62,752	0.0220
2002	7,947	0.0171	62,115	0.0243
2003	8,423	0.0171	58,163	0.0224
2004	8,328	0.0169	39,369	0.0223
2005	8,119	0.0170	39,168	0.0212
2006	7,866	0.0167	48,080	0.0193
2007	7,712	0.0168	43,964	0.0205
2008	7,600	0.0171	45,303	0.0236
2009	7,466	0.0180	36,357	0.0225
2010	7,317	0.0176	33,016	0.0224
2011	7,111	0.0174	35,867	0.0216
2012	6,921	0.0174	38,182	0.0207

3 Empirical Approach and Results

3.1 The Effect of Pessimistic Tone on CEO Dismissals

In this part, we investigate the effect of pessimistic managerial and media tone on a CEO's firing probability by estimating binary probit models. Table 3.3 presents results obtained from probit models that estimate the probability that the CEO of a firm i is fired in year t, using

$$Pr(Forced_{i,t}) = \Phi(\beta_0 + \beta_1 \text{Managerial tone}_{i,t-1} + \beta_2 \text{Media tone}_{i,t-1} + \gamma X_{i,t-1} + \epsilon_{i,t}), \quad (3.2)$$

where β_1 (β_2) estimates the effect of prior managerial (media) tone on the firing probability. A large set of controls, X, related to board, industry, company, and CEO characteristics disseminates the concern that pessimistic media and managerial tone derives its significance from other influencing factors. In all regressions, we account for heteroscedasticity by clustering standard errors on firm level.

Table 3.3: Probit: Out-of-Sample View

This table presents (predictive) probit regressions on a binary dependent variable which equals one if a CEO got fired in the respective year, say t, and zero otherwise. Our analysis is based on the S&P 1500 universe after merging all data sources with the newspaper data extracted from LexisNexis and company filing data from Bill McDonald's webpage ($http://www3.nd.edu/\ mcdonald/Word\_Lists.html$) for each regression. One observation corresponds to a year-company observation. The sample period includes the years 1996 to 2012. Industry-fixed effects are based on the first 2 digits of the corresponding SIC industry classifier. All independent variables in the regression are lagged by one year, i.e. are based on data of the year $t-1$. *MediaPessimism* (*ManagerialPessimism*) is calculated as the average over the ratio of negative words to total words (in the calendar year prior to the turnover) in all newspaper articles (10-K and 10-Q company filings) per company per year using the word lists by Loughran and McDonald (2011). See Table B1 in the appendix for the definition of control variables. The notations ***, **, and * denote significance at the 1%, 5%, and 10% confidence level, respectively. The t-statistics are in parentheses. Standard errors are clustered on firm level.

	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
MediaPessimism	0.039***		0.026**	0.004	0.008	0.001
	(3.38)		(2.21)	(0.32)	(0.41)	(0.03)
ManagerialPessimism		0.216***	0.204***	0.179***	0.150***	0.267***
		(8.13)	(6.51)	(5.47)	(2.68)	(4.04)
MarketValue					0.006	0.059*
					(0.24)	(1.91)
CEOAge					-0.012***	-0.013***
					(-2.82)	(-2.77)
CEOTenure					-0.019***	-0.014**
					(-3.32)	(-2.24)
BookToMarket					-0.054	-0.001
					(-1.34)	(-0.02)
ROA					-0.075	-0.017
					(-0.62)	(0.14)
Block					0.001	0.03
					(0.06)	(0.97)
DualCEO					-0.072	-0.152**
					(-1.14)	(-2.19)
CAR				-0.458***	-0.510***	-0.512***
				(-11.51)	(-6.69)	(-6.14)
BusyBoard					-0.041	-0.095
					(-0.50)	(-1.05)
BoardSize					0.002	-0.003
					(0.19)	(-0.20)
IndependencePortion					0.118	0.626**
					(0.60)	(2.50)
IndustryROABelowTrend					0.062	0.025
					(1.05)	(0.33)
Constant	-2.065***	-2.371***	-2.383***	-2.370***	-1.698***	-2.658***
	(-68.18)	(-49.05)	(-40.69)	(-38.78)	(-5.398)	(-4.738)
Industry & Year FE	NO	NO	NO	NO	NO	YES
R^2	0.002	0.009	0.010	0.040	0.050	0.089
Observations	19,601	30,117	19,601	19,135	9,491	9,491

Regressions I and II of Table 3.3 show that in an univariate setting, pessimistic media (MediaPessimism) and managerial tone (ManagerialPessimism) significantly increase a manager's firing probability. The coefficients remain significant when including both variables into one regression (Regression III), confirming that both measures independently influence the board's firing decision. In regression IV, the inclusion of prior abnormal stock market performance (CAR), as a main driver of CEO dismissals, eludes the magnitude and significance of pessimistic media tone but does not change the significantly positive influence of pessimistic managerial tone on the firing probability. The rather stable coefficient estimate of pessimistic managerial tone confirms the independence of the manager's tone from prior performance and suggests that managers can significantly influence the board's firing decision by avoiding a pessimistic tone. In line with Core, Guay, and Larcker (2008), the fact that the inclusion of prior performance disperses the effect of pessimistic media

tone indicates that pessimistic media tone does not serve as an additional governance mechanism. In regression V and VI, we show that these findings are robust to the inclusion of control variables, especially performance, and year- as well as industry-fixed effects.[5]

Using probit regression VI of Table 3.3 we calculate marginal effects at means to provide an economic interpretation. The estimates show that an increase in the proportion of negative words by 1%, from a level of around 2% to 3%, yields an increase of 1.5% in the turnover probability. For the sake of comparison, a 1% decline in the previous year's abnormal stock performance increases the likelihood of a turnover by 2.9%, holding all other variables constant at their means values.

Finally, the effects of our control variables are in line with prior literature which supports the reliability of our results. Specifically, poor prior performance increases significantly and tremendously the likelihood of a firing in the subsequent year. Moreover, the older the CEO (CEOAge) and the longer the tenure (CEOTenure), the lower the probability of a firing. This finding stands in accordance to previous literature stating that an advanced age and a long tenure might proxy managerial entrenchment (Berger, Ofek, and Yermack, 1997; Combs, Ketchen, Perryman, and Donahue, 2007), which is associated with a lower threat of dismissal (Goyal and Park, 2002; Gregory-Smith, Thompson, and Wright, 2009; Bebchuk, Cohen, and Ferrell, 2008). With respect to CEO power, we find that CEOs who are less governed by the board of directors, as measured by the independence of the board directors and by a dummy equal to one if the CEO simultaneously holds the chairman position, are harder to replace.

3.2 The Effect of Pessimistic Tone on Investor Reactions

In this section, we investigate whether pessimistic managerial and media tone are able to shape the investors' evaluation of the firing decision as well as whether one type of tone dominates the other. To this aim, we apply an event-study methodology that examines the magnitude and significance of cumulative abnormal returns around the announcement of a CEO dismissal in the subsamples of firms with high and low pessimistic media and managerial tone, respectively.

We measure the cumulative abnormal return of a firm as the sum of its daily abnormal returns over a certain event window. A daily abnormal return is the difference between the actual firm return and a benchmark return on the respective day. The benchmark return is the expected firm return which is usually estimated over a pre-event period using a return model that exploits information on the firm's prior stock returns (see, e.g., Brown and Warner, 1985). However, a performance based estimation may bias the results because of the positive correlation between poor prior performance and pessimistic tone. Thus, to mitigate the concern that we wrongly attribute the effect of prior performance to tone, we use a model free approach with a value-weighted index as benchmark which is unrelated to the firm's prior performance. In the robustness section, we use a market model to predict benchmark returns in order to show that our results are not affected by

[5] In this analysis, CAR is computed by using a value-weighted index as a benchmark. Results remain qualitatively equal using the average 2-digit SIC industry return as benchmark. Further, we proxied an insider's future expectations on firm profitablity by the change in operating performance from the year before to the year after the firing and added it as another performance control in unreported results. However, our main results remain stable.

this specific approach. We use the announcement of the dismissal as event day. If the announcement lies on a holiday or weekend, we define the next available trading day as event day. Finally, in order to avoid that the announcement returns are contaminated by confounding events, such as earnings or merger announcements, we exclude turnover events with relevant news in the period from 10 days before until 10 days after the turnover. This additional restriction reduces our sample to 446 forced turnovers.

Table 3.4: Event-Study Results

This table reports the mean abnormal stock returns obtained from an event study of 446 forced CEO turnover announcements during the years 1996 to 2012. As indicated, abnormal returns refer to different event windows (measured in number of days before and after the announcement date) and are calculated using a model-free approach with a value-weighted index as market return. Newspaper data is extracted from LexisNexis and company filing data from Bill McDonald's webpage ($http://www3.nd.edu/\ mcdonald/Word\_Lists.html$). Column I refers to the total sample of 446 forced turnovers. Column II a and b show the total sample divided into CEOs with overproportionally (HIGH) and underproportionally negative tone (LOW) in 10-K and 10-Q filings with its respective difference in means (DIFF.) in column II c. Analogously, column III a and b report abnormal returns for overproportionally (HIGH) and underproportionally negatively-toned (LOW) news articles with its respective difference in means (DIFF.) in column III c. HIGH is a binary variable that is one, if the company's average tone in the exact year prior to the turnover (over day 370 to day 10 before the turnover event) is higher than the average filing or news tone over the whole sample (including non-turnover firms) up to that point (day 10 before the turnover) and zero otherwise (then being classified as LOW). A lack of any newspaper article or filing in the exact year prior to the turnover results in a reduced sample size. The notations ***, **, and * denote significance of the corresponding t-statistics at the 1%, 5%, and 10% confidence level, respectively.

		Managerial Tone			Media Tone		
	(I) ALL	(II a) HIGH	(II b) LOW	(II c) DIFF.	(III a) HIGH	(III b) LOW	(III c) DIFF.
[-1 1]	-0.37%	-0.28%	-0.65%	0.37%	0.82%	-1.20%**	2.02%**
t-stat.	(-1.27)	(-0.74)	(-1.33)	(0.40)	(1.58)	(-2.44)	(2.15)
[0 0]	0.39%**	0.38%*	0.58%**	-0.20%	1.61%***	-0.07%	1.68%*
t-stat.	(2.31)	(1.75)	(2.03)	(-0.28)	(5.38)	(-0.25)	(2.23)
[0 1]	0.02%	0.27%	-0.55%	0.82%	1.23%***	-0.77%*	2.01%**
t-stat.	(0.07)	(0.86)	(-1.39)	(0.97)	(2.91)	(-1.93)	(2.28)
[0 2]	-0.13%	0.24%	-1.16%**	1.40%	0.83%	-0.35%	1.18%
t-stat.	(-0.45)	(0.63)	(-2.37)	(1.41)	(1.60)	(-0.72)	(1.23)
[0 3]	-0.59%*	-0.28%	-1.5%***	1.22%	0.04%	-0.65%	0.69%
t-stat.	(-1.73)	(-0.65)	(-2.65)	(1.17)	(0.07)	(-1.14)	(0.68)
Observations	446	313	104		163	125	

Table 3.4 presents event-study results for the full sample and different subsamples. In Column I, we present cumulative abnormal returns for the full sample of forced CEO turnovers with respect to different event windows. In line with the improved management theory by Huson, Malatesta, and Parrino (2004) who argue that the board is only expected to replace the current CEO if there is a superior alternative, we find positive and significant abnormal returns for the announcement day ([0 0]). However, due to the absence of stable positive and significant abnormal returns for other event windows, the evidence is only weak.

In Columns IIa and IIb, we split the sample based on high and low managerial pessimism and in Columns IIIa and IIIb based on high and low media pessimism. If a company's average managerial or media tone in the time span from 370 days to 10 days prior to the turnover is higher than the average tone in the full sample (including non-turnover firms), it is allocated to the HIGH subsets, otherwise the firm belongs to the LOW subsamples. Our results show that pessimistic managerial tone does not influence the estimated announcement returns. Although we find sporadic significant effects, particularly for the LOW sample, the effect is never different between firms with less pessimistic and highly pessimistic managerial tone. In contrast, we document that a pessimistic media tone significantly determines the investor reactions to the announcement of the

dismissal over the event windows [-1 1], [0 0], and [0 1]. For instance, the firing of a CEO in a company with a highly pessimistic media tone yields a 1.61% increase in firm value on the day of the announcement ([0 0]) and 1.23% when considering the cumulative effect by including the day following the announcement ([0 1]). Compared to dismissals that happen in firms with less pessimistic media coverage, the increase in firm value even amounts to 1.68% on the day of the announcement ([0 0]) and to 2.02% in a three-day event window ([-1 1]) – a window that accounts for possible information leakage or delays.

With respect to these results, we observe a reversal pattern on the days following the dismissal announcement. Specifically, the average return one day after the event is -0.38% (1.61% - 1.23%) and the day-two return is -0.4% (1.23% - 0.83%), leading to insignificant average cumulative abnormal returns of 0.83% for firms with highly pessimistic media coverage over the three-day event window [0 2]. Compared to firms with less pessimistic media coverage, the difference ceases to be significant over the three-day ([0 2]) and four-day event windows ([0 3]). Thus, in line with prior literature (Baker and Wurgler, 2006; Lily and Ivo, 2006; Mian and Sankaraguruswamy, 2012; Da, Engelberg, and Gao, 2015), our findings suggest a sentiment-driven mispricing. Specifically, a highly pessimistic tone triggers an immediate overreaction among investors upon the dismissal announcement. The overreaction, however, is followed by an adjustment to the fundamental value subsequent to the event day.

To isolate the effect of tone on the investors' reactions from other influencing factors such as prior performance (Ertugrul and Krishnan, 2011), we run cross-sectional regressions of abnormal returns over different event periods on pessimistic tone and a set of control variables as well as industry- and year-dummies.

Results from multivariate regressions are reported in Table 3.5. Column I shows the regression results for the announcement day ([0 0]). Column II, III, and IV show the results for a two-day ([0 1]), three-day ([0 2]), and four-day ([0 3]) event window, respectively. After controlling for alternative explanatory variables, the estimated effects still support the event-study findings. Our results show that pessimistic media tone significantly increases the abnormal returns by 3.5% on the announcement day of dismissal. However, the value effect is only temporary. For the event windows [0 2] and [0 3], the significance ceases which supports the assumption of sentiment-driven mispricing. Managerial tone, in contrast, is not able to predict a significant portion of the abnormal announcement returns, which suggests that investors primarily rely on media rather than managerial tone when evaluating the dismissal decision.

With respect to our controls, we follow Ertugrul and Krishnan (2011) and construct a prior performance indicator. CEOs are considered as underperformers (Underperformer) if their stock-price performance over the year prior to the event year is lower than the value-weighted market index over the same time period. Dependent on the event dates, we document significant average firing premiums of 2.8% to 3.8% associated with the replacement of an underperforming CEO. In line with Ertugrul and Krishnan (2011), we find that investors adjust their evaluation of the firm's fundamental value by the reassessment of the outgoing CEO's ability. Most notably, our result suggests that on the day of the dismissal announcement, the sentiment-driven mispricing effect is

of the same magnitude as the well-established fundamental performance effect and thus at least temporarily of major economic relevance.

Table 3.5: Cross-Sectional Regression

This table reports results of the cross-sectional regressions of cumulative abnormal returns, calculated using a model-free approach with a value-weighted index as market return, for the event windows [0 0], [0 1], [0 2], and [0 3]. The variables *MediaPessimismDummy* (*ManagerialPessimismDummy*) correspond to the *HIGH* variables for news articles and filings from Table 3.4, respectively. Industry-fixed effects are based on the first 2 digits of the corresponding SIC industry classifier. See Table B1 in the appendix for the control variable definitions. The notations ***, **, and * denote significance at the 1%, 5%, and 10% confidence level, respectively. The t-statistics are in parentheses. Standard errors are clustered on firm level.

	(I) **[0 0]**	**(II)** **[0 1]**	**(III)** **[0 2]**	**(IV)** **[0 3]**
Underperformer	0.038***	0.033**	0.028*	0.036**
	(3.33)	(2.18)	(1.67)	(2.21)
MediaPessimismDummy	0.035***	0.033**	0.023	0.026
	(3.00)	(2.22)	(1.41)	(1.51)
ManagerialPessimismDummy	0.006	-0.001	-0.002	-0.004
	(0.49)	(-0.04)	(-0.10)	(-0.25)
MarketValue	0.004	0.007**	0.007**	0.006
	(1.50)	(2.19)	(2.23)	(1.63)
CEOAge	-0.001	-0.001	-0.001	-0.001
	(-0.85)	(-0.70)	(-0.78)	(-0.32)
CEOTenure	0.000	0.001	0.000	-0.000
	(0.10)	(0.73)	(0.10)	(-0.19)
ROA	-0.032	-0.068	-0.069	-0.068
	(-0.44)	(-0.93)	(-0.96)	(-1.10)
OutsideSuccessor	0.001	0.015	0.022	0.008
	(0.07)	(0.90)	(1.28)	(0.43)
Constant	-0.052	-0.103	-0.079	-0.055
	(-0.71)	(-1.20)	(-0.85)	(-0.49)
Industry & Year FE	YES	YES	YES	YES
R^2	0.406	0.391	0.356	0.339
Observations	198	198	198	198

3.3 Robustness Checks

This section provides robustness by addressing potential concerns such as measurement errors in the tone measures, endogeneity, and unobserved heterogeneity.

3.3.1 Tone as a Driver of CEO Dismissals

Following Hillert, Jacobs, and Müller (2014), we re-run our probit regressions of Table 3.3 by considering different LexisNexis "relevance scores". The higher a company's relevance score, the more likely an article can be explicitly attributed to a specific firm. By performing this robustness check, we account for the concern that we may measure non-company-specific tone or noise in our analysis.

Table 3.6 presents results of the probit regressions when applying a 70% relevance score filter to focus on articles with a strong focus on a given firm. The relevance score is provided by LexisNexis and serves as a measure for the match between article and firm accounting for factors such as the frequency of the firm being mentioned or the location of the company name within the article.[6]

[6]Other authors set similar filters (see, e.g., Fang and Peress, 2009; Hillert, Jacobs, and Müller, 2014; Ferguson,

Table 3.6: Probit with Relevance Score Filter

This table presents (predictive) probit regressions on a binary dependent variable which equals one if a CEO got fired in the respective year, say t, and zero otherwise. Our analysis is based on the S&P 1500 universe after merging all data sources with the newspaper data extracted from LexisNexis and company filing data from Bill McDonald's webpage ($http://www3.nd.edu/\ mcdonald/Word\_Lists.html$) for each regression. For newspaper articles, we only include firm-specific articles reaching at least 70% in the Nexis relevance score. One observation corresponds to a year-company observation. The sample period includes the years 1996 to 2012. Industry-fixed effects are based on the first 2 digits of the corresponding SIC industry classifier. All independent variables in the regression are lagged by one year, i.e. are based on data of the year $t-1$. *MediaPessimism* (*ManagerialPessimism*) is calculated as the average over the ratio of negative words to total words (in the calendar year prior to the turnover) in all newspaper articles (10-K and 10-Q company filings) per company per year using the word lists by Loughran and McDonald (2011). See Table B1 in the appendix for the definition of control variables. The notations ***, **, and * denote significance at the 1%, 5%, and 10% confidence level, respectively. The t-statistics are in parentheses. Standard errors are clustered on firm level.

	(I)	**(II)**	**(III)**	**(IV)**	**(V)**	**(VI)**
MediaPessimism	0.036***		0.025**	0.006	0.005	-0.005
	(3.20)		(2.16)	(0.43)	(0.28)	(-0.27)
ManagerialPessimism		0.216***	0.206***	0.188***	0.143**	0.263***
		(8.13)	(6.06)	(5.32)	(2.34)	(3.61)
MarketValue					-0.005	0.0344
					(-0.17)	(0.99)
CEOAge					-0.009*	-0.010**
					(-1.95)	(-1.96)
CEOTenure					-0.023***	-0.019**
					(-3.35)	(-2.54)
BookToMarket					-0.023	0.031
					(-0.48)	(0.49)
ROA					-0.095	0.003
					(-0.74)	(0.02)
Block					-0.008	0.007
					(-0.31)	(0.22)
DualCEO					-0.152**	-0.207***
					(-2.14)	(-2.67)
CAR				-0.456***	-0.473***	-0.484***
				(-9.73)	(-5.57)	(-5.12)
BusyBoard					-0.054	-0.085
					(-0.604)	(-0.867)
BoardSize					-0.005	-0.016
					(-0.336)	(-0.987)
IndependencePortion					0.111	0.507*
					(0.51)	(1.82)
IndustryROABelowTrend					0.050	0.000
					(0.76)	(0.01)
Constant	-2.057***	-2.371***	-2.376***	-2.376***	-1.520***	-2.136***
	(-61.76)	(-49.05)	(-37.57)	(-36.17)	(-4.31)	(-3.53)
Industry & Year FE	NO	NO	NO	NO	NO	YES
R^2	0.002	0.009	0.011	0.039	0.052	0.095
Observations	15,480	30,117	15,480	15,082	7,263	7,263

By examining tone, endogeneity concerns are rooted in the nature of such an analysis. First, tone can be seen as a reaction to the current state of a company but tone may also influence the future state of the company by shaping the opinion of investors and affiliates of the firm. Second, there is always the chance that omitted variables drive both, the dependent and the explanatory variable of interest. Third, auto-correlation with lagged endogenous variables may exist among observations of one firm. To address these concerns, we already added a broad set of controls variables including market and accounting performance as well as fixed effects to our regressions. The inclusion of performance controls should particularly be able to capture various other factors such as managerial ability that might be omitted but simultaneously influence our dependent

Philip, Lam, and Guo, 2015). Results remain qualitatively equal for different cutoff values.

variable and tone (Bednar, 2012). We further lagged all our independent variables, in order to make sure that we measure pessimistic managerial and media tone before and not around the firing, which mitigates the reverse causality concern. In this subsection, we apply three additional approaches to mitigate endogeneity concerns.

Table 3.7: GEE Results

This table presents the results of the GEE regressions. We follow Halekoh, Højsgaard, Yan, et al. (2006), running GEE with a probit link function and a working correlation of type exchangeable (I), with logit link function and a working correlation of type AR(1) as in Bednar (2012) (II) and a GEE with probit link function and an unstructured working correlation (III). Our sample data comprises the S&P 1500 universe after merging all data sources with the newspaper data extracted from LexisNexis and company filing data from Bill McDonald's webpage ($http://www3.nd.edu/\ mcdonald/Word\_Lists.html$). The sample period includes the years 1996 to 2012. *MediaPessimismews* (*ManagerialPessimism*) is calculated as the average over the ratio of negative words to total words (in the calendar year prior to the turnover) in all newspaper articles (10-K and 10-Q company filings) per company per year using the word lists by Loughran and McDonald (2011). See Table B1 in the appendix for the definition of control variables. The notations ***, **, and * denote significance at the 1%, 5%, and 10% confidence level, respectively. Significance is based on a Wald test statistic (in parenthesis).

	(I)	**(II)**	**(III)**
MediaPessimism	0.008	0.007	0.011
	(0.17)	(0.00)	(0.35)
ManagerialPessimism	0.151***	0.330***	0.011*
	(7.15)	(6.74)	(2.77)
MarketValue	0.006	-0.138	-0.039
	(0.05)	(2.11)	(0.82)
CEOAge	-0.012***	-0.029***	-0.011***
	(8.14)	(8.46)	(6.92)
CEOTenure	-0.019***	-0.046***	-0.024***
	(11.42)	(11.44)	(13.70)
BookToMarket	-0.054	-0.138	-0.039
	(1.72)	(2.11)	(0.82)
ROA	-0.075	-0.058	-0.079
	(0.46)	(0.08)	(0.50)
Block	-0.001	-0.010	0.007
	(0.00)	(0.03)	(0.07)
DualCEO	-0.074	-0.163	-0.084
	(1.31)	(1.21)	(1.85)
CAR	-0.511***	-1.157***	-0.514***
	(42.69)	(50.47)	(45.22)
BusyBoard	-0.042	-0.108	-0.050
	(0.26)	(0.31)	(0.37)
BoardSize	0.002	0.008	-0.003
	(0.04)	(0.07)	(0.04)
IndependencePortion	0.117	0.346	0.011
	(0.34)	(0.56)	(0.00)
IndustryROABelowTrend	0.061	0.149	0.046
	(1.06)	(1.15)	(0.65)
Constant	-1.697***	-3.060***	-1.662***
	(29.28)	(17.34)	(29.31)
Number of clusters	1,715	1,715	1,715
Max. Cluster Size	16	16	16
Observations	9,491	9,491	9,491

In the first part of the result section, we run standard probit regressions following prior literature (see, e.g., Goyal and Park, 2002; Bushman, Dai, and Wang, 2010; Jenter and Lewellen, 2014). Although we cluster standard errors and control for various firm characteristics and year- and industry-fixed effects, we lack to exploit all available information by pooling observations. For this reason, we follow Halekoh, Højsgaard, Yan, et al. (2006) and Bednar (2012) and use Generalized Estimating Equations (GEE) to account for the panel structure of the data. A GEE estimation is a semi-parametric approach which is tailored to deal with correlated and clustered observations

(Liang and Zeger, 1986). We estimate our probit regressions with GEE assuming independent, exchangeable and AR-1 working correlations. As displayed by Table 3.7 filing tone remains significant, even when allowing for an unstructured working correlation.

We further run a matching approach to deal with heterogeneity among firms and in order to mitigate concerns about omitted variables that may drive the results (Roberts and Whited, 2012). Specifically, we construct a sample of control firms (firms that have the same probability to be subject to a forced CEO turnover but were not, i.e., predicted forced) that we compare to firms that were subject to a forced turnover. We apply the nearest-neighbor propensity score matching scheme of Rosenbaum and Rubin (1983) by running a logit regression of our indicator variable that equals one if a particular firm has a forced CEO turnover and zero otherwise, on all available firm, governance, board, and CEO characteristic.[7] We do not match on media and managerial tone. The estimated coefficients are used to predict the probability that a firm is subject to a firing. With these predicted probabilities we conduct a one-to-one matching.

Table 3.8: Propensity-Score Matching

This table reports descriptive statistics of forced turnovers and predicted forced turnovers in the calendar year prior to the turnover. We use a nearest neighbor propensity score matching on all variables except *MediaPessimism* and *ManagerialPessimism*, including industry- and year-fixed effects. *Obs.* denotes the number of observations entering the matching, *Std.Dev.* denotes the standard deviation of our variable, the p-value refers to the p-value of the t-test for the difference in means (Diff.) between forced turnovers and predicted forced turnovers. See Table B1 in the appendix for the definition of all variables. The notation * represents significance at the 10% level.

	Forced				**Predicted Forced**					
	Obs.	**Mean**	**Median**	**Std. Dev.**	**Obs.**	**Mean**	**Median**	**Std. Dev.**	**Diff.**	**p-value**
MediaPessimism	203	1.88	1.70	1.29	203	1.96	1.95	1.08	-0.08	0.24
ManagerialPessimism	203	1.79	1.73	0.54	203	1.71	1.65	0.48	0.08*	0.06
MarketValue	203	8.06	7.96	1.81	203	8.04	8.11	1.73	0.02	0.45
CEOAge	203	53.61	55.00	5.86	203	53.89	54.00	7.31	-0.28	0.33
CEOTenure	203	4.87	4.00	4.51	203	5.49	3.00	6.22	-0.62	0.11
BookToMarket	203	-0.92	-0.78	0.87	203	-0.89	-0.87	0.74	-0.03	0.37
ROA	203	0.08	0.08	0.09	203	0.09	0.08	0.09	-0.01	0.28
Block	203	2.18	2.00	1.61	203	2.20	2.00	1.50	-0.02	0.46
DualCEO	203	0.68	1.00	0.47	203	0.67	1.00	0.47	0.02	0.37
CAR	203	-0.21	-0.14	0.45	203	-0.18	-0.11	0.52	-0.03	0.20
BusyBoard	203	0.15	0.00	0.36	203	0.11	0.00	0.31	0.04	0.11
BoardSize	203	9.79	9.00	2.66	203	9.89	10.00	2.77	-0.10	0.35
IndependencePortion	203	0.73	0.63	0.15	203	0.74	0.78	0.14	-0.00	0.39
IndustryROABelowTrend	203	0.47	0.00	0.50	203	0.48	0.00	0.50	-0.01	0.42

As expected and indicated by Table 3.8, matching firms show no difference compared to our control firms regarding the characteristics used in the matching procedure. In this respect, the large amount of matching variables suggests that the firms may also be equivalent along unobservable dimensions. In line with our previous findings, the insignificance of pessimistic media tone does not change with respect to the occurrence of a dismissal. However, pessimistic managerial tone is still significant for firms that initiated a forced turnover compared to those solely having the same probability of facing a dismissal. This result supports the assumption that, independent of poor prior performance and other observable and unobservable characteristics, pessimistic managerial tone is significantly related to the probability of a firing.

[7] The number of observations is slightly higher than the 198 observations in the cross-sectional regressions because it is not possible to match on the variable outside-successor.

As an additional robustness check to account for endogeneity and to lower the concern that the dynamic interrelation between tone, performance - and other related firm characteristics are central drivers of our results, we follow Hillert, Jacobs, and Müller (2014) and orthogonalize the managerial tone measure by performing a two-stage regression approach. In a first step, we run a contemporaneous regression with pessimistic managerial tone as dependent variable and all firm- and performance-related characteristics as independent variables. The residuals of the regression can be interpreted as abnormal or residual tone. This abnormal tone measure only incorporates the informational content which is unique to tone and is, for example, clean of performance-induced tone.

Table 3.9: Abnormal Filing Tone

This table presents a two-step regression approach. In the first step, we regress *ManagerialPessimism* on the standard (contemporaneous) controls including year and industry-fixed effects. In step 2, we repeat our (predictive) probit regression (VI) from Table 3.3 where the dependent variable is the binary variable which equals one if a CEO got fired in the coming year. However, we replace the variable *ManagerialPessimism* with the residuals of our Step 1 regression which are interpreted as abnormal managerial pessimism (*AbnManagerialPessimism*). Our analysis is based on the S&P 1500 universe after merging all data sources with the newspaper data extracted from LexisNexis and company filing data from Bill McDonald's webpage (*http* : *//www3.nd.edu/ mcdonald/Word_Lists.html*) for each regression. One observation corresponds to a year-company observation. The sample period includes the years 1996 to 2012. Industry-fixed effects are based on the first 2 digits of the corresponding SIC industry classifier. All other independent variables in the regression are lagged by one year, i.e. are based on data of the year $t-1$. *MediaPessimism* (*ManagerialPessimism*) is calculated as the average over the ratio of negative words to total words (in the calendar year prior to the turnover) in all newspaper articles (10-K and 10-Q company filings) per company per year using the word lists by Loughran and McDonald (2011). See Table B1 in the appendix for the definition of control variables. The notations ***, **, and * denote significance at the 1%, 5%, and 10% confidence level, respectively. The t-statistics are in parentheses. Standard errors are clustered on firm level.

	(I) **Step 1: ManagerialPessimism**	**(II)** **Step 2: Forced**
AbnManagerialPessimism		0.267***
		(3.84)
MediaPessimism		0.001
		(0.03)
MarketValue	0.038***	0.016
	(4.47)	(0.27)
CEOAge	-0.002*	-0.014***
	(-1.73)	(-2.81)
CEOTenure	-0.003**	-0.014**
	(-2.51)	(-2.32)
BookToMarket	0.055***	0.016
	(3.61)	(0.27)
ROA	-0.583***	-0.138
	(-4.83)	(-1.14)
Block	0.010*	0.029
	(1.68)	(1.01)
DualCEO	-0.044**	-0.164**
	(-2.47)	(-2.28)
CAR	-0.008	-0.514***
	(-0.58)	(-5.74)
BusyBoard	0.034	-0.086
	(1.55)	(-0.93)
BoardSize	-0.015***	-0.007
	(-3.70)	(-0.49)
IndependencePortion	0.204***	0.680**
	(3.40)	(2.57)
IndustryROABelowTrend	-0.012	0.021
	(-1.08)	(0.27)
Constant	1.428***	8.910***
	(6.93)	(12.99)
Industry & Year FE	YES	YES
R^2	0.256	0.098
Observations	9,491	9,491

The results are shown in Table 3.9. Regression I indicates that, indeed, many factors are linked to pessimistic managerial tone. For instance, good performance (CAR) is associated with lower managerial pessimism whereas having an independent board hinders the management to choose too flowery phrases despite significant challenges. However, accounting for these factors, we still find (abnormal) pessimistic managerial tone to remain a highly significant predictor for the

firing decision (Regression II), indicating that independent from other factors such as performance, pessimistic managerial tone can be considered as a potential driver of forced managerial turnovers.

3.3.2 Tone as a Driver of Investor Reactions

We run several robustness checks with respect to our event-study and cross-sectional analysis. First, we adjust our model-free estimation approach to obtain the abnormal returns by exchanging the value-weighted market index with the median industry return based on the 2-digit SIC code of the firms that are affected by a CEO dismissal. Second, we measure abnormal returns applying a market model estimation. Specifically, we regress actual returns on value-weighted benchmark returns for a period starting 370 days and ending 10 days prior to the event date (dismissal announcement). Using the parameter estimates, we compute expected returns for the day of and the days around the dismissal and use them as benchmark returns.

Table 3.10: Robustness: Event-Study Results

This table reports the mean abnormal stock returns obtained from an event study of 446 forced CEO turnover announcements during the years 1996 to 2012. Variables are defined as in Table 3.4 except that, in Panel A, abnormal returns are calculated with respect to the median industry return (based on 2-digit SIC code). In Panel B, abnormal returns are calculated with the market model using a value-weighted index as market return. The notations ***, **, and * denote significance of the corresponding t-statistics at the 1%, 5%, and 10% confidence level, respectively.

Panel A: Industry-Adjusted CAR							
		Managerial Tone			**Media Tone**		
	(I) ALL	**(II a) HIGH**	**(II b) LOW**	**(II c) DIFF.**	**(III a) HIGH**	**(III b) LOW**	**(III c) DIFF.**
[-1 1]	0.01%	0.14%	-0.29%	0.43%	1.29%**	-0.67%	1.96%**
t-stat.	(0.02)	(0.37)	(-0.59)	(0.47)	(2.50)	(-1.41)	(2.13)
[0 0]	0.54%***	0.55%**	0.68%**	-0.13%	1.75%***	0.16%	1.59%**
t-stat.	(3.19)	(2.55)	(2.43)	(-0.19)	(5.88)	(0.59)	(2.10)
[0 1]	0.024%	0.51%*	-0.31%	0.82%	1.50%***	-0.42%	1.92%**
t-stat.	(1.02)	(1.68)	(-0.79)	(0.98)	(3.57)	(-1.07)	(2.21)
[0 2]	0.24%	0.61%	-0.72%	1.33%	1.17%**	0.09%	1.08%
t-stat.	(0.82)	(1.64)	(-1.50)	(1.37)	(2.27)	(0.18)	(1.15)
[0 3]	-0.10%	0.19%	-0.87%	1.06%	0.46%	-0.13%	0.59%
t-stat.	(-0.30)	(0.44)	(-1.55)	(1.10)	(0.78)	(-0.24)	(0.59)
Observations	446	313	104		163	125	
Panel B: Market Model CAR							
		Managerial Tone			**Media Tone**		
	(I) ALL	**(II a) HIGH**	**(II b) LOW**	**(II c) DIFF.**	**(III a) HIGH**	**(III b) LOW**	**(III c) DIFF.**
[-1 1]	-0.18%	-0.02%	-0.57%	0.55	1.11%**	-0.88%*	1.99%**
t-stat.	(-0.61)	(-0.06)	(-1.18)	(0.58)	(2.19)	(-1.84)	(2.15)
[0 0]	0.47%***	0.47%**	0.61%**	-0.14%	1.66%***	0.07%	1.59%**
t-stat.	(2.84)	(2.24)	(2.20)	(-0.19)	(5.68)	(0.26)	(2.11)
[0 1]	0.14%	0.45%	-0.47%	0.92%	1.41%***	-0.52%	1.93%**
t-stat.	(0.59)	(1.49)	(-1.20)	(1.08)	(3.41)	(1.33)	(2.19)
[0 2]	0.08%	0.49%	-0.98%**	1.47%	1.11%**	0.03%	1.08%
t-stat.	(0.27)	(1.33)	(-2.05)	(1.47)	(2.19)	(0.07)	(1.13)
[0 3]	-0.29%	0.07%	-1.17%**	1.24%	0.46%	-0.20%	0.66%
t-stat.	(-0.87)	(0.16)	(-2.11)	(1.17)	(0.79)	(-0.36)	(0.71)
Observations	446	313	104		163	125	

Table 3.10 reports the event-study results using industry-adjusted (Panel A) and market model-implied (Panel B) abnormal returns. The results confirm the previous findings. Compared to the main analysis, we find a slightly more persistent effect of media tone on the market reactions as suggested by event windows [0 2] in column HIGH of the media tone subsamples in Panel A and B. Again, this effect eventually dissipates. There is still no evidence for an influence of pessimistic managerial tone on investor reactions.

Table 3.11 presents the corresponding cross-sectional regressions which also confirm the validity of our main analysis. In particular, all longer-term event windows suggest a reversal pattern with respect to the influence of media pessimism.

Table 3.11: Robustness: Cross-Sectional Regressions

This table reports results of the cross-sectional regressions of cumulative abnormal returns, calculated using a market-model approach with a value-weighted index as market return for the event windows [0 0], [0 1], [0 2], and [0 3] (Columns I - IV). Columns V - VIII reports cross-sectional regressions results of cumulative abnormal returns, calculated using a model-free approach and the median industry return (based on 2-digit SIC code) as market return. The variables *MediaPessimismDummy* (*ManagerialPessimismDummy*) correspond to the *HIGH* variables for filings and news articles from Table 3.4. Industry-fixed effects are based on the first 2 digits of the corresponding SIC industry classifier. See Table B1 in the appendix for the control variable definitions. The notations ***, **, and * denote significance at the 1%, 5%, and 10% confidence level, respectively. The t-statistics are in parentheses. Standard errors are clustered on firm level.

	(I) **[0 0]**	**(II)** **[0 1]**	**(III)** **[0 2]**	**(IV)** **[0 3]**	**(V)** **[0 0]**	**(VI)** **[0 1]**	**(VII)** **[0 2]**	**(VIII)** **[0 3]**
Underperformer	0.041***	0.037**	0.035**	0.046***	0.038***	0.03**	0.03	0.04**
	(3.59)	(2.50)	(2.10)	(2.76)	(3.25)	(2.26)	(1.61)	(2.30)
MediaPessimismDummy	0.033***	0.033**	0.019	0.022	0.035***	0.036**	0.025*	0.027*
	(2.90)	(2.23)	(1.16)	(1.35)	(3.16)	(2.49)	(1.66)	(1.67)
ManagerialPessimismDummy	0.007	0.001	0.000	-0.006	0.005	0.000	-0.001	-0.004
	(0.52)	(0.09)	(0.01)	(-0.34)	(0.42)	(0.02)	(-0.07)	(-0.24)
MarketValue	0.004	0.007**	0.008**	0.008**	0.005	0.007**	0.008**	0.007*
	(1.56)	(2.26)	(2.46)	(2.07)	(1.59)	(2.35)	(2.40)	(1.96)
CEOAge	-0.001	-0.001	-0.001	-0.000	-0.001	-0.001	-0.001	-0.001
	(-0.84)	(-0.55)	(-0.60)	(-0.26)	(-0.73)	(-0.65)	(-0.77)	(-0.32)
CEOTenure	-0.000	0.001	-0.000	-0.001	0.000	0.001	0.000	0.000
	(-0.11)	(0.51)	(-0.21)	(-0.75)	(0.14)	(0.71)	(0.30)	(0.13)
ROA	-0.022	-0.060	-0.058	-0.061	-0.031	-0.068	-0.074	-0.082
	(-0.32)	(-0.87)	(-0.85)	(-1.03)	(-0.42)	(-0.93)	(-1.03)	(-1.36)
OutsideSuccessor	0.002	0.017	0.025	0.013	-0.000	0.014	0.023	0.009
	(0.12)	(1.07)	(1.50)	(0.70)	(-0.02)	(0.88)	(1.41)	(0.50)
Constant	-0.050	-0.115	-0.088	-0.070	-0.062	-0.110	-0.082	-0.077
	(-0.70)	(-1.36)	(-0.95)	(-0.65)	(-0.85)	(-1.30)	(-0.87)	(-0.71)
Industry & Year FE	YES	YES	YES	YES	YES	YES	YES	YES
R^2	0.413	0.398	0.361	0.361	0.407	0.408	0.366	0.352
Observations	198	198	198	198	198	198	198	198

4 Conclusion

This paper uncovers the role of pessimistic media and managerial tone in forced CEO turnovers. By analyzing pessimistic tone in firm-specific newspaper articles to measure media tone and 10-Q and 10-K company filings to measure managerial tone, we provide a differentiated view on how both information sources relate to CEO replacement decisions and the associated announcement returns. Using panel data for firms between 1996-2012, we are the first to evidence that pessimistic managerial tone increases the likelihood of forced managerial turnovers. In line with Core, Guay, and Larcker (2008) but in contrast to Bednar (2012), we also show that pessimistic media coverage does not significantly influence the board's firing decision, suggesting that media tone does not serve as a comprehensive governance mechanism. Our results hold after controlling for various factors, including prior stock and accounting performance which are considered to be main drivers of forced CEO turnovers. Further, several additional checks are performed to disseminate endogeneity concerns and to confirm the robustness of our results.

An examination of abnormal stock price reactions around the turnover announcement suggests

that media tone induces sentiment-driven investor reactions. Specifically, we find that pessimistic media tone helps to explain abnormal returns on the announcement day. The immediate positive market reactions, however, reverse within some days after the announcement. We additionally document that pessimistic managerial tone does not explain market reactions to CEO replacements which suggests that in case of a dismissal, shareholders rather rely on the media than on the management when evaluating the board's decision.

In spite of these results, this study refrains from proposing strict policy implications but points out that CEOs have to raise consciousness regarding their tone of voice because it may actively influence the board's firing decision. The question whether CEOs choose their words unconsciously or based on a pessimistic outlook remains unanswered. In this respect, it would be interesting to investigate the effect of external pressure, litigation concerns, and managerial experience on a CEO's tone. We leave it up to future research to shed light on the drivers behind managerial tone with the help of a more qualitative or survey-based approach.

Abgrenzung

Die vorliegende Version der Studie "The Paradox of Awards: How Status Spillover and Status Ripples affect who benefits from CEO Awards" habe ich als Co-Author zusammen mit Professor Michael Jensen und Torsten Twardawski verfasst. Die ursprüngliche Forschungsfrage entstand aus einer Zusammenarbeit mit Torsten Twardawski. Dabei wurden die Entwicklung der Methodik sowie die Datenaufbereitung und deren Analysen gemeinschaftlich durchgeführt. Durch die Kooperation mit Professor Jensen hat sich der Fokus der Studie von der primären Analyse Vorstandsvorsitzender, die einem mit einem Preis ausgezeichneten Vorstandsvorsitzenden nahestehen, zu einer Vergleichsstudie zwischen diesen beiden Parteien entwickelt. Die darauf aufbauenden Analysen sowie die theoretische Ausarbeitung der Hypothesen wurden gemeinschaftlich durchgeführt.

Die vorliegende Version der Studie "The Effect of Managerial Entrenchment on Competitors: Evidence from Exogenous Shocks" habe ich als Co-Author zusammen mit Prof. Dr. Axel Kind und Torsten Twardawski verfasst. Die ursprüngliche Version des Artikels wurde von mir ausgearbeitet. Dementsprechend habe ich die Forschungsfrage und Methodik erarbeitet sowie die Datenaufbereitung und deren Analysen durchgeführt. In späterer Zusammenarbeit wurde die Studie dann um die Aktionärsanträge sowie weitere Robustnesschecks ergänzt. Darüber hinaus wurden Restrukturierungen durchgeführt.

Die vorliegende Version der Studie "Uncovering the Role of Tone: Evidence from CEO Dismissals" habe ich als Co-Author zusammen mit Patrick Hauf verfasst. Alle Bestandteile der Studie wurden gemeinschaftlich erarbeitet.

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Appendix

A

Table A1: Variable Definitions

This table presents the definitions for the dependent and control variables used throughout the main analysis of chapter 2.

Variable	**Definition**
OROA_{adj}	Lagged and industry adjusted operating return on assets. OROA is calculated as the company's earnings before interest divided by the firm's total assets. OROA_{adj} is computed as the difference between the OROA in year t and the sum of OROA in year $t-1$ and the change in the median industry OROA from year $t-1$ to year t.
Total risk	The volatility of monthly returns for a given firm-year.
Syst. risk	The beta of the market model regression of monthly returns.
Idios. risk	The standard deviation of the residual of the regression of the monthly firm returns on the CRSP value-weighted index.
R&D	Natural logarithm of 1 plus R&D expenditures divided by a firm's total assets. If R&D serves as control variable, missing values are set to zero.
Sales	Natural logarithm of a firm's sales.
Cogs	Natural logarithm of a firm's cost of goods sold.
Sales margin	The difference of sales and cost of goods sold divided by cost of goods sold.
ROA	Net income divided by total assets.
Leverage	Total long term debt divided by the firm's total assets.
Market value	Natural logarithm of common shares outstanding times the share price.
Sales growth	Yearly growth in sales as measured by $(sales_t - sales_{t-1})/sales_{t-1}$.

B

Table B1: Definitions of Control Variables

This table reports the definitions of the control variables used throughout the main analysis of chapter 3.

Underperformer	A dummy variable that equals one if the holding-period return of the company adjusted by the daily value-weighted holding period return of the market is smaller than zero one year prior to the turnover, and zero otherwise.
CAR	Cumulative abnormal return in the calendar year prior to the turnover announcement date using the stock return and a monthly value-weighted holding period return.
BookToMarket	The log of the number of shares outstanding times book value per share divided by the market capitalization of the stock in a given calendar year.
MarketValue	The log of the number of shares outstanding times the price of the stock (in million US Dollar) in the calendar year prior to the turnover.
ROA (Return on Asset)	Net income divided by total assets in a given calendar year.
IndustryROABelowTrend	Defined in accordance with Eisfeldt and Kuhnen (2013): Equals one if the average profitability in the industry during the 3 preceding years is below its value during the preceding 10 year, and 0 elsewhere. In this context industry profitability is measured by the fraction of net income over total asset across all firms in the industry (measured by the first 2 digit SIC).
IndependencePortion	The number of independent outside directors per board divided by the boardsize in a given calendar year.
BoardSize	The number of directors per board in a given calendar year.
BusyBoard	Dummy that equals one if the majority of independent directors sits on three or more boards in a given calendar year, zero elsewhere.
DualCEO	Equals one if the CEO is simultaneously chairman of the board in a given calendar year, and zero otherwise.
Block	Average number of institutional blockowners in a given calendar year (capped at 10).
OutsideSuccessor	Taken from Eisfeldt and Kuhnen (2013).
CEOTenure	Number of years as CEO at the time of dismissal.
CEOAge	Age of the CEO at the time of dismissal.