Essays in Corporate Finance: Research on the Corporate Governance of Cartel Participants, Coordinated Shareholder Activism, and the Lead Director

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Tanja Artiga González, July 2015

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Summary

This thesis empirically investigates three research questions in the area of corporate finance.

In Chapter 1 we study financial reporting and corporate governance in U.S. companies accused of price fixing. These firms engage in evasive financial reporting strategies, including earnings smoothing and frequent restatements. In corporate governance, cartel firms favor outside directors likely to monitor inattentively due to low attendance, other board commitments, and overseas residence. New auditors are engaged at below-average rates. Cartel firms have unusually low CEO turnover and exhibit unusual reliance on internal promotions. Their CEOs exercise stock options faster than CEOs of other firms and receive abnormally large shares of their compensation in cash bonuses. Cartel firms are large donors to political candidates and experience high rates of securities fraud lawsuits, generally for actions unconnected to the price fixing allegations. While our results are based only upon firms engaged in price fixing, they may apply generally to companies in which managers seek to conceal poor performance or wrongdoing.

Chapter 2 studies coordinated shareholder activism campaigns, i.e., multiple activists simultaneously targeting the same firm. Using a unique database of activism events we document that activists prefer to participate in coordinated campaigns and that these campaigns produce significantly higher returns than non-coordinated campaigns. Consistent with activists working together, we find that this phenomenon is more prevalent among geographically proximate activists where frictions to coordination are lower. We posit that coordinated campaigns are a mechanism through which activists can mitigate free-rider problems. Consistent with this hypothesis, we find that coordinated campaigns are more prevalent where the free-rider problems faced by activists are greater - at large firms and among small activists.

In Chapter 3, using a sample of lead and presiding directors of S&P 500 firms we examine the effectiveness of this board position. The average lead director is male and has more board experience in comparison to his fellow directors. He is older and receives on average less votes than his colleagues on the board. This paper confirms prior research that firm value is not affected by the deci-

sion to combine or separate the CEO and chairman positions and shows that firm value is also not correlated with the leadership structure in place (i.e., independent chairman, lead director, or presiding director). We find evidence that firm value is associated with the choice of the lead director. Lead directors who receive a high retainer are associated with lower firm value. Paid lead directors are also associated with higher discretionary accruals whereas a lead director who is appointed to all three mandatory board committees reduces the probability of restating the financial statement. Total CEO compensation does not depend on the leadership structure. However, performance based compensation is higher for firms with paid lead directorships.

Zusammenfassung

Die vorliegende Dissertation besteht aus drei voneinander unabhängigen Forschungsarbeiten, die sich mit unterschiedlichen Fragestellungen aus dem Bereich der Corporate Finance beschäftigen.

Das erste Kapitel befasst sich mit der Finanzberichterstattung und Unternehmensführung von US-Firmen, die wegen illegaler Preisabsprachen angeklagt wurden. Typisch für diese Firmen sind unverhältnismäßig hohe Cashflows. Unsere Ergebnisse zeigen, dass diese Firmen vermehrt auf verschleiernde Strategien in ihrer Finanzberichterstattung zurückgreifen, um damit entsprechende Regulierungsbehörden zu täuschen. Sie stellen vermehrt Aufsichtsräte ein, die ihre Aufgaben nur eingeschränkt wahrnehmen können. Neue Auditoren werden seltener engagiert. Außerdem wechseln Kartellfirmen weniger oft den Geschäftsführer, zeichnen sich aber durch überdurchschnittlich viele interne Beförderungen aus. Kartellfirmen spenden im Vergleich zu der Kontrollgruppe signifikant mehr Geld für politische Kampagnen und verstoßen besonders oft gegen die gültigen Aktiengesetze. Wir glauben, dass unsere Resultate auch auf andere Firmen übertragen werden können, in denen Manager versuchen schlechte Performanz oder Fehlverhalten zu verschleiern.

Das zweite Kapitel untersucht koordinierte Investitionsaktivitäten von Anteilseignern, die durch ihr gemeinsames Vorgehen Einfluss auf die Unternehmensführung auszuüben versuchen. Unsere Ausgangshypothese lautet, dass koordinierte Investitionsaktivitäten eine Antwort auf ein allgemeines Trittbrettfahrerproblem darstellen: Anteilseigner, die versuchen durch ihre Investitionsaktivitäten die Firmenstrategie zu ändern, tragen die gesamten Kosten und Risiken ihrer Kampagne, wohingegen die Gewinne einer erfolgreichen Steigerung des Aktionärswerts allen Anteilseignern zu Gute kommen. Anhand eines bisher unveröffentlichten Datensatzes zeigen wir im ersten Schritt, dass koordinierte Investitionsaktivitäten signifikant höhere abnormale Renditen erzielen. Wir zeigen außerdem, dass koordiniertes Verhalten der Anteilseigner eher in den Firmen anzutreffen ist, in denen das Problem des Trittbrettfahrertums am größten ist: in größeren Firmen und unter kleineren Aktionären. Im zweiten Schritt unserer Analyse zeigen wir, dass Aktionäre öfter koordinieren, wenn ihre Unternehmenssitze näher beieinander liegen, d.h. in Fällen, in

denen soziale Beziehungen zwischen den Anteilseignern wahrscheinlich sind.

In Kapitel 3 wird die Position des Lead- und Presidingdirektors in US-Verwaltungsräten untersucht. Die Untersuchungen basieren auf einem Datensatz von US-Firmen, die von 2005 bis 2011 Mitglieder des S&P 500 waren. Wir zeigen auf, dass der Leaddirektor - im Mittel - männlich und älter als seine Kollegen ist und auch mehr Verwaltungsraterfahrung besitzt. Auffällig ist, dass Leaddirektoren nach ihrer Amtszeit durchschnittlich weniger Stimmen bei der Wiederwahl bekommen als ihre Kollegen im Verwaltungsrat. Unsere Untersuchungen bestätigen das Ergebnis früherer Forschungen, dass die Trennung der Positionen des Geschäftsführers und des Vorsitzenden des Aufsichtsrats, in keinem direkten Zusammenhang zum Firmenwert steht. Wir legen dar, dass Firmen, deren Leaddirektoren Mitglieder in allen drei obligatorischen Verwaltungsratskomitees sind, seltener ihren Finanzbericht korrigieren müssen. Die Wahl des Leaddirektors übt keinen direkten Einfluss auf die Höhe der Gesamtvergütung der Geschäftsführer aus. Es zeigt sich aber, dass Firmen mit überdurchschnittlich gut bezahlten Leaddirektoren ein höheres Niveau an leistungsabhängiger Vergütung aufweisen.

Chapter 1

Smokescreen: How managers behave when they have something to hide

Smokescreen: How managers behave when they have something to hide

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Abstract. We study financial reporting and corporate governance in U.S. companies accused of price fixing. These firms engage in evasive financial reporting strategies, including earnings smoothing and frequent restatements. In corporate governance, cartel firms favor outside directors likely to monitor inattentively due to low attendance, other board commitments, and overseas residence. New auditors are engaged at below-average rates. Cartel firms have unusually low CEO turnover and exhibit unusual reliance on internal promotions. Their CEOs exercise stock options faster than CEOs of other firms and receive abnormally large shares of their compensation in cash bonuses. Cartel firms are large donors to political candidates and experience high rates of securities fraud lawsuits, generally for actions unconnected to the price fixing allegations. While our results are based only upon firms engaged in price fixing, they may apply generally to companies in which managers seek to conceal poor performance or wrongdoing.

JEL: D43, G34, K42, L40, M43

Keywords: Cartels, price fixing, accounting fraud, boards of directors, corporate governance.

1.1 Introduction

In most financial frauds, a company tries to make its performance appear better than it really is, hoping to achieve a valuation that wouldn't be supported by its true cash flows. This paper looks at firms that mislead external audiences for more complex reasons. We investigate the disclosure and governance practices of more than 200 U.S. companies accused by government authorities of participating in price fixing cartels. These firms earn strong cash flows, and continuation of their schemes requires obfuscation of their windfalls from regulators, analysts, customers, and even their own auditors and boards of directors.

Connor and Helmers (2007) define a cartel as "an association of legally independent firms that aims to raise their joint profits through explicit agreements. Hard-core cartels aim to control prices or restrict supply (or both)." Decisions to join cartels are typically taken by a firm's very top managers and then implemented by the intermediate management (Harrington, 2006). The role of top management suggests that corporate governance may affect formation and continuation of a cartel. For example, cartels may occur more readily in firms with a high concentration of power at the top level, a weak or inattentive board of directors, or strong pay-for-performance incentives (Spagnolo, 2005).[1] In addition, financial reporting strategies that cause signal-jamming,

[1]Theoretical research shows that cartel formation may be motivated not only by the potential profits from price fixing, but also by management incentives (e.g., Levenstein and Suslow, 2006). Spagnolo (2005) adds managerial incentives schemes to a supergame-theoretic model of dynamic competition and shows that when managers have a preference for smooth time paths, collusion with other firms becomes more likely. This could be caused by management bonus contracts that have capped incentive provisions. His model shows that even though income smoothing is costly, shareholders tolerate the cost in return for the higher collusive profits. Buccirossi and Spagnolo (2008) show in a classical model of repeated oligopoly that the stability of tacit collusive agreements is positively correlated with performance-based incentives provided to top management.

such as earnings smoothing and suspicious accrual patterns, might occur frequently with cartels. Cartelists have an ongoing need to deter both cheating and the entry into the industry by new firms (Levenstein and Suslow, 2006). If a cartel member deviates from a collusive agreement, its sudden change in earnings might be detected by co-conspirators, who could then start a price war that could destroy the cartel. Therefore, we would expect cartels to use financial reporting strategies that obscure year-to-year swings in profitability.

This paper investigates how cartel firms attempt to cover up their conspiracies. We study a sample 224 U.S. companies participating in hard-core cartels between 1986 and 2010. We define a cartel firm-year as year in which the given cartelist has been involved in price fixing. The starting point and duration of the cartels in our sample are identified by enforcement actions brought by government antitrust authorities. We compare these cartel firm-year observations to observations from a set of control firms matched on size and industry. As in other empirical studies on cartels, our sample is subject to a selection bias because we are only able to consider discovered cartels. These firms are believed to represent only a minority of the price fixing conspiracies that occur worldwide (see Connor, 2010).

We document a range of accounting and governance strategies that cartel firms adopt systematically, apparently with an eye toward prolonging their conspiracies and evading liability. To mislead readers of financial statements, companies engage in earnings smoothing and file abnormally large numbers of financial restatements. These accounting patterns appear to be part of a strategy to confuse readers of financial statements by making time series

comparisons of performance difficult. In corporate governance, cartel firms favor outside directors who are likely to be inattentive monitors due to their status as foreign or "busy" (belonging to a large number of boards) and their low rates of meeting attendance. Since stability of the management team probably represents a necessary condition for continuing a conspiracy, cartel firms tend to replace CEOs slowly (controlling for performance) and promote CEOs from within rather than hiring them from outside. Cartel firm CEOs exercise their stock options faster than managers of other firms, and they receive larger shares of their compensation from cash bonuses that usually are connected to accounting earnings. We also find a pronounced pattern of political donations by cartel firms, as their political action committees (PACs) contribute more frequently to political candidates than firms in a matched control sample. Cartel firms are also sued for securities fraud more often than other companies, although few of these suits are connected to price fixing allegations. While our results are based only upon firms engaged in price fixing, they might apply generally to all companies in which the managers seek to conceal poor performance or personal wrongdoing.

We identify several channels through which CEOs profit by taking their firms into cartels, including longer job tenures, changes in executive compensation, and less intrusive oversight by their boards. Despite these personal benefits for top managers, cartels do not necessarily represent agency problems since shareholders will also profit if a firm's higher earnings lead to increased stock prices. In untabulated results we find that cartel firms' stock prices fall significantly after government authorities uncover a scheme, but we cannot say whether these losses exceed the expected gains from joining a cartel. It is

widely believed that government authorities discover only a minority of price fixing schemes, with the large majority of cartel firms evading prosecution and collecting economic rents that shareholders keep permanently. For those cartels that are identified, we cannot reliably estimate shareholders' gains during the cartel period since the inception of a conspiracy is often ambiguous even for the participants. Even those cartel firms that are caught tend to have high rates of recidivism, as we discuss below.

The prominent pharmaceutical maker Bristol-Myers Squibb Co. provides a useful example to illustrate the behavior of the cartel firms in our sample. Authorities charged Bristol-Myers with participating in cartels in three different countries between 1999 and 2004. During this cartel period, Bristol-Myers engaged in many of the practices described in our analysis below. Five years of earnings results were restated, including two years that were restated twice. The company was sued twice during this period for securities fraud. It retained the same auditing firm for the entire cartel period despite the outward signs of financial reporting problems. Two new outside directors joined the Bristol-Myers board between 1998-2004. One fell into the "busy" category, with three or more board memberships, and the other was based in a foreign country. Five of the incumbent outside directors from the start of the cartel period also had busy status. Both CEOs who served during this period had been promoted internally. At the end of its cartel period in 2006, soon before it agreed to plead guilty to federal criminal charges arising from an antitrust investigation, Bristol-Myers appears to have undergone a governance and financial reporting overhaul. The CEO was replaced by an outside board member who had not previously worked for the company.

The firm changed auditors, replacing PricewaterhouseCoopers with Deloitte & Touche, and added a law-and-order independent outsider to its board, the former FBI Director and federal judge Louis Freeh.

The remainder of the paper is organized as follows. Section 2 outlines our hypotheses. Section 3 describes the sample and variables. Section 4 reports the results from the empirical analysis. Section 5 concludes.

1.2 Hypotheses

We study aspects of corporate governance, financial reporting, and interactions with regulators by firms engaged in illegal cartels. We expect the managers of these companies to work actively to conceal price fixing from shareholders, prosecutors, and other audience, while at the same time using financial statements to signal stability to other companies in the cartel. Additionally, managers should be expected to seek opportunities to profit personally from the cartel.

In corporate governance, the board of directors plays a critical oversight role, and we believe that very few boards are informed about or co-opted into active participation in the operational details of cartels. Rather, we expect managers to attempt to influence the form of their boards in ways that should make cartel detection more difficult. It would not be surprising to see above-average rates of outside director turnover in cartels, since other research indicates that directors sometimes resign to protest objectionable behavior without necessarily being candid about their reasons for leaving (Fahlenbrach, Low, and Stulz, 2012). When directors leave, we would expect boards to leave their seats unfilled in order to avoid appointing new monitors who may question manage-

ment practices from a fresh perspective. If new directors are appointed, we expect them to have attributes of poor monitors, including poor attendance records, foreign residence, and "business" due to commitments to serve on other boards.

We expect cartel companies to exhibit different patterns of CEO selection and replacement than other firms. Stability of the management team is probably a necessary condition for continuation of a price fixing conspiracy. Therefore, we would expect CEOs to leave their positions more slowly than usual, and when CEO vacancies occur, their successors should be more likely to be promoted internally than hired from outside.

Auditors and regulators represent two external groups who could threaten the success and continuity of a cartel. We expect cartel firms to manage their relationships with these potential monitors. In particular, cartelists should be reluctant to replace auditing firms who have not succeeded in detecting price fixing behavior, so we predict that auditors will be replaced less quickly by these companies compared to those in the control sample. To gain influence with regulators, we expect cartel members to make larger and more frequent political donations compared to other companies.

Financial reporting strategies should provide a crucial opportunity for cartel firms to reduce the possibility that price fixing may be detected, and we investigate a number of hypotheses in this area. We expect cartel firms to engage in earnings smoothing to obscure sharp changes in profitability. A complimentary strategy would involve financial restatements, which make intertemporal comparisons of financial performance more difficult, and we ex-

pect more frequent restatements from cartel firms as a signal jamming tactic. Abnormally large accruals and manipulation of deferred revenue provide two common channels for firms to smooth earnings, and also provide rationales for restatements. We therefore expect more accruals and deferred revenue from cartel firms. The accounting adjustments should likely have negative impacts on reported income, if price fixing contributes to rapid increases in actual profits that cartel firms wish to conceal or only publicize slowly. However, we cannot make an unambiguous prediction about the signs of accruals and deferred revenue, since firms may sometimes wish to manipulate profits higher as part of an earnings smoothing strategy or as a signal to other cartel participants. Managerial overconfidence might also cause positive accounting adjustments if executives believe that a cartel can be prolonged indefinitely.

Executive compensation provides several channels for corporate managers to profit from cartel membership. Many managers hold stock options and have discretion over when to exercise them. Since a firm's performance should be enhanced by price fixing, we expect the frequency of stock option exercises to increase during the cartel period. Additionally, CEOs typically participate in both short- and long-term cash bonus plans tied to accounting earnings. We expect CEOs of cartel firms to receive higher bonuses since cartel membership increases their firms' profits.

We also investigate shareholder litigation against cartel firms for securities fraud. We expect high levels of private lawsuits against cartel firms, but these could occur for two distinct reasons. Shareholders may sue because of misleading financial reporting connected to the price fixing activity itself.

Alternatively, companies that do not comply with laws related to price fixing may also be predisposed to ignore regulations related to securities filings and other laws as well.

1.3 Sample selection

1.3.1 Cartel firms

We use the U.S. firms included in an extended version of Connor's (2010) hand collected Private International Cartel dataset, which covers private cartels discovered, disclosed and sanctioned by regulators around the world between January 1986 and December 2010.[2] The dataset omits cartels protected by sovereignty or multilateral treaties, as well as those for which no sanctions were imposed within five years of the authorities' discovery. A total of 648 cartels involving 2,115 companies appear in the dataset, although in certain cases many more firms are sanctioned anonymously. Many companies are repeat offenders and participate in multiple cartels. The median cartel involves eight companies and lasts five years before discovery by regulators; the maximum values are considerably higher, with some cartels lasting for decades and involving dozens of companies or more.[3]

The dataset includes each firm's name, country of incorporation, the market(s) and continent(s) where collusion took place, the duration of the collusive agreement, and if known, the fines imposed, leniency granted by regulators, and estimated overcharges to consumers. Information is collected mainly from filings, documents, reports, and press releases from the antitrust

[2]The dataset in Connor (2010) covers the time period from January 1990 to December 2009.

[3]Connor (2010) reports one case of more than 2,000 unnamed construction companies accused of price fixing by authorities in the Netherlands.

authorities in different countries, as well as newspaper and magazine articles retrieved through search engines like Factiva or Lexis-Nexis. The sample generally includes more observations in recent years, with between 300 and 400 companies in each of the years 2005-2009. We do not know whether this pattern occurs due to better enforcement, more disclosure by regulators, more coverage by the press, or a greater tendency by companies to collude in price fixing or bid rigging, though Connor and Helmers (2007) estimate that only 10% to 30% of all price fixing conspiracies are ever discovered. European companies comprise the majority of observations in the dataset, although many cartels are global in nature and involve multinational firms operating on several continents. Connor and Helmers (2007) estimate that by the early 2000s, worldwide corporate penalties for firms participating in cartels stabilized at or above $2 billion per year, with approximately 60% due to government fines (mainly from U.S. and European regulators) and 40% paid to settle private litigation.

To select our sample, we begin with 819 U.S. companies included in the international dataset. We exclude all cartels which started before 1986 and all firms not covered by Compustat, which substantially reduces the sample size to 224 firms that are involved in a total of 188 conspiracies (numerous conspiracies concern more than one sample company). We obtain data for 1,592 cartel company-year observations (including part-years) for these 224 firms over the 1986-2010 period, with 70 of the 224 companies participating in more than one cartel. Of the 224 cartel firms, 14 represent cases of spinoffs that continue participating in cartels begun when they were wholly owned subsidiaries of their parent firms. In these 14 cases a single cartel firm is

represented by a sequence of two companies' data from Compustat and other sources, with us dropping the parent and following the subsidiary once it becomes separated via spinoff. Although these will appear as two unique cartel firms in our sample, we do not change the original control sample midstream.

The mean (5.91) and median (6) cartel period is six years for our sample, and the maximum value is 22 years for a marine hose cartel involving the rubber manufacturers Goodyear and Parker Hannifin. ExxonMobil is involved in 13 individual cartels, the most of any company in the sample, followed by Johnson & Johnson with nine.

We have only limited information about the economic harm caused by each cartel and the sanctions against firms and their managers. Connor's dataset provides information at the cartel level about consumer overcharges, and this data covers about 40% of the events in our sample. The consumer overcharges have a mean value of $5.7 billion, median of $0.4 billion, and maximum of $22.1 billion, indicating that some of the cartels extract significant economic rents from the retail market. We cannot track the individuals who are found culpable in individual cartels, and sometimes regulators will sanction only a firm and not its executives. However, a review of recent news stories indicates that often these cases implicate CEOs, division presidents, and other members of top management.[4] For this reason, some of our analysis below focuses upon aspects of the careers of company CEOs and directors, including their

[4]There are abundant examples, including the high-profile 2013 conviction (currently under appeal) of Apple Inc. for price fixing in the eBook market, which a U.S. federal court concluded involved very top officers of the firm. See J. Edwards, "How Steve Jobs, Rupert Murdoch, and Stephen King Worked to Fix eBook Prices," July 11, 2013, http://www.businessinsider.com.au/how-steve-jobs-and-apple-fixed-ebook-prices-2013-7.

selection, compensation, and removal.

We augment the financial statement data from Compustat for our 224 firms with information from other financial and governance sources, including the Center for Research in Securities Prices (CRSP) stock price database, the RiskMetrics Governance and Directors databases, Standard and Poor's ExecuComp database, the AuditAnalytics database, the U.S. Federal Election Commission database of political contributions, and the Stanford Law School Securities Class Action database. Missing values in these datasets reduce the observations available for some of our analysis below.

1.3.2 Control sample of matched firms

We construct a control sample of comparable companies matched on size and industry. For every cartelist, we identify the Compustat firms with the same two-digit primary SIC code in the year before the first collusive agreement starts. We then take the five firms with firm size, measured by total assets, closest to the size of the cartel firm during the year before the start of the cartel. No cartel firm is permitted also to enter the control sample.

In many cases cartel firms rank among the largest in their industries, and we refine our matching procedure to reduce the potential size disparity between our cartel firms and the control sample. While we choose the five closest firms to each cartel firm as measured by total assets, we discard those matching firms for which the size difference is greater or less than 50%. Some control firms are delisted prior to the end of a cartel, and in these cases we do not replace them. In 28 cases there are no matching firms in the same two-digit industry within 50% of the size of the cartel firm, and we drop these from our

sample, leaving us with 196 cartel firms with 1,362 annual observations. Our final control sample has 3,460 observations for 529 Compustat firms matched on size and industry.

We experiment with a number of alternative control samples and find that other matching procedures make little difference in the size and significance of most regression estimates reported in the paper. These investigations are discussed in a section on robustness tests near the end of the paper.

Table 1.1 presents summary statistics including the means, medians and standard deviations across these two subsamples for all the variables used in our analysis below, and Table 1.2 presents a correlation matrix of the key dependent variables along with firm size and return on assets. The summary statistics in Table 1.1 show that cartel firms are larger, faster growing, and more profitable than their counterparts in the control sample. Due to this pattern, we include a measure of firm size in virtually all regressions, generally using an estimate of total enterprise value equal to the log of market value of equity plus book value of debt. To be sure that our cartel firms do not deviate materially in size from the matching firms, we calculate the distribution of relative sizes between each cartel company and the mean value of all of its matching firm observations. This size ratio has a mean value of 1.12, median of 1.03, and standard deviation and 0.23, with a range between 0.54 and 1.98 (the minimum and maximum ratios are limited to 0.50 and 2.00 by construction). These statistics indicate that the large majority of cartels have firm size reasonably close to the sizes of the relevant control sample observations.

In general we cannot include all of our cartel and control sample observa-

tions in our regression analysis below, because some databases do not cover all of our firms (for example, ExecuComp tracks only 1,500 companies from Compustat), and nearly all databases except Compustat have a later coverage start date than 1986, which is our first year of cartel data. Beginning with Table 1.3 and for the rest of the paper, we indicate in each table the number of firms and observations used in the analysis for both the cartel sample and the control sample.

1.4 Empirical results

In this section, we study the behavior of cartel firms in two broad areas: corporate governance and financial reporting, and we also investigate the gains to cartel CEOs from exercising stock options and obtaining unexpectedly high cash compensation. We further examine the frequency with which cartel firms become targets of class action shareholder litigation for securities fraud.

1.4.1 Director turnover and replacement

Evading detection by monitors from inside and outside the firm must represent a primary goal of the managers of cartel firms. Regardless of whether current members of the board of directors have knowledge of a cartel, we can make a straightforward prediction about changes in the board. Companies should be reluctant to replace directors who resign or retire, because recruiting a new monitor from outside the company creates a risk of the cartel being halted and/or exposed. When new directors are appointed, we would expect them to have poor monitoring characteristics.

Table 1.3 presents an analysis of board turnover, changes in board size, and new director appointments for our cartel firms compared to companies in the

control sample. We collect data on individual directors from the RiskMetrics database, and the limited coverage of this source causes a large reduction in our sample size. In addition to the indicator variable for cartel firms, our regression models in the first two columns include control variables for changes in firm size and annual abnormal stock performance estimated by the market model (following Yermack's (1996) study of board size changes), indicators for CEOs near retirement age and for recently appointed CEOs, and industry fixed effects. In the models for director appointments, we include two other variables from the study of boards by Coles, Daniel, and Naveen (2008): intangible assets/total assets, and an indicator that equals one if research & development/total assets is in the 75^{th} percentile or higher in a given year on the Compustat database. Rather than including year fixed effects, we use an indicator variable that equals one beginning in 2002, the first-year in which the Sarbanes-Oxley Act and other regulations required companies to begin implementing changes in board structure. However, our other results in Table 1.3 are robust to replacing the Sarbanes-Oxley indicator with year indicators. The inclusion of industry fixed effects costs us some observations, because industries with no variation in the dependent variable are deleted by the maximum likelihood estimation procedures.

In the first two columns of Table 1.3, we do not find support for the hypotheses that cartel firm directors resign at abnormally high rates and that cartel firms shrink their boards by not replacing departed directors. Although the cartel indicator variable has a positive estimate as expected in each of the first two columns, the statistical significance of each estimate does not quite reach the 10% level. We do find in robustness tests below that these estimates do acquire

statistical significance when we use a variety of alternative control samples. Reasons for director resignations are not always clear, but one possibility is that board members who become aware of wrongdoing leave quietly to evade future legal liability or to signal disagreement with management's actions (see Fahlenbrach, Low, and Stulz, 2012). The motive for cartel firms not to replace exiting directors seems plain: by leaving a board seat vacant, the company avoids the possibility of being monitored by a new individual from outside the firm.

The subsequent four columns of Table 1.3 analyze the monitoring capability of new outside directors appointed to the boards of cartel firms. We study directors in three categories: those who are busy, those who have poor attendance records, and those who reside in foreign countries. These types of directors have been shown in recent papers to perform poorly as monitors, due to such factors as distraction, distance, fatigue, and their unfamiliarity with U.S. accounting rules. Busy directors, defined as those serving on three or more boards simultaneously, are studied by Fich and Shivdasani (2006). Directors with attendance problems are those who miss more than 25% of the board and committee meetings in a given year. Foreign independent directors are the subject of a recent paper by Masulis, Wang, and Xie (2012), who kindly shared their sample with us for use in this study.

We estimate a Poisson maximum likelihood model of busy director appointments in the third column of Table 1.3. In column 4, we study the appointment of directors with attendance problems, but we use a binary probit dependent variable instead of a Poisson count data model since there are almost

no cases in which companies appoint more than one director with attendance problems in the same year. For the same reason, we continue using the probit framework in models of foreign director appointments in columns 5 and 6 of Table 1.3.

Estimates for these models indicate that when appointing new directors, cartel firms are more likely to select outsiders with each of the attributes of business, attendance problems, and foreign residence, consistent with a conjecture that management nominates new board members who are unlikely to monitor aggressively. The result for foreign directors continues to hold when we augment the regression with a control variable for the fraction of the firm's sales that occur outside the U.S., since foreign directors are expected to be more valuable in multinational firms. These effects appear to be economically largest for busy and foreign directors, for which the cartel variable in columns 3 and 5 has marginal effects of 0.110 and 0.033, respectively. These effects should be contrasted with the mean values for the frequency of these director appointments by non-cartel firms, which are shown in Table 1.1 as 0.337 and 0.026, respectively.

1.4.2 CEO turnover and replacement

Controlling for performance and other relevant factors, we expect cartel firms to exhibit less CEO turnover than other companies, since it should be risky for the firm to recruit a successor and entrust that person with the continuation of a conspiracy. For similar reasons, when a CEO replacement occurs, we would expect the new manager to come from within the company rather than from outside.

Table 1.4 presents estimates from a probit model in which we study these relationships. In the first column, we model CEO replacement as a function of standard control variables such as age, tenure, stock performance, and industry and year fixed effects. Other control variables for this model come from the study of forced CEO turnover by Parrino, Sias, and Starks (2003). Estimates for this model have the expected signs and significance - for instance, CEO replacement occurs less often when a firm is performing well and when the CEO is younger. The model also includes an indicator variable for cartel membership. As expected, it has a negative estimate and is statistically significant at the 1 percent level. The marginal effect of this estimate is -0.040, which is economically meaningful compared to the non-cartel firms' turnover frequency of 14.5 percent.

In the second and third columns we study whether a CEO is selected internally or externally. The model in the second column is restricted to those company-years (about 11.4% of the observations) in which a CEO turnover occurs. The model in the third column is a cross sectional study of whether the firm's current CEO was selected from inside the firm, regardless of his tenure in office. In both columns we obtain positive and significant estimates for the cartel firm indicator variable, consistent with these firms choosing their leaders from inside the company. According to Table 1.1, the ordinary rate of internal CEO promotions is about 80 percent, and the marginal effects of the two estimates in the second and third columns are 0.085 and 0.076, respectively, implying that the rate of internal CEO promotions is roughly 10 percent higher than usual in cartel firms. Finally, in the fourth and fifth columns we replicate the previous analysis and include a control variable for

the number of business segments within a firm. This augmentation leaves the previous estimates nearly unchanged, but the coefficient in column 4 does not quite reach the 10 percent significance level.

We conclude that after controlling for age, tenure and performance, cartel firms replace their CEOs less frequently than other companies, and when a cartelist's CEO job does become open, it is more likely to be filled by an internal promotion. Unlike most of our other regression models, the estimations in Table 1.4 do not include a control for firm size, because one does not appear in the analysis of Parrino, Sias, and Starks (2003) which serves as the source for our control variables. If we include the log of enterprise value (market value of equity plus book value of debt), the estimates for the cartel variable weaken somewhat and are not always significant, as the z-statistics for the top row of estimates in Table 1.4 are reduced to values between 1.40 and 2.74.

1.4.3 Changes in auditors

We investigate cartel firms' changes in auditors to explore whether a pattern exists similar to that for boards of directors, with management exhibiting reluctance to bring in new outsiders who might monitor aggressively and become aware of the firm's illegal conduct. We use Compustat to identify changes in auditors and create an indicator variable that equals one for years in which the database reports a different auditor than the previous year. We restrict the sample to observations in which firms have a Big 4 auditor and change from one Big 4 auditing firm to another. Only 22 observations, or around 1%, for the cartel firm sample are associated with smaller auditing firms, and

the fraction is significantly higher for firms in the control sample. A limited number of auditing firms exit the industry due to mergers or liquidation (including, most famously, Arthur Andersen). In these cases, when an auditor change is mandatory and beyond the control of the firm, we set the auditor change indicator equal to missing.

Summary statistics in Table 1.1 indicate that auditor changes occur far less frequently for cartel firms than for companies in the control sample, with annual frequencies of 2.4% vs. 3.6%, respectively. In Table 1.5 we report estimates for probit regressions in which the auditor change indicator is regressed against the cartel indicator as well as a wide range of control variables used by Landsman, Nelson, and Rountree (2009) in their study of auditor replacement and year and industry fixed effects.

Estimates for the cartel firm indicator confirm the results found in the simple comparison of sample means, but only weakly. Cartel firms change auditors less often than other companies, controlling for company size, performance, industry membership, and time period, but the effect is significant only for the model in the third column, which includes an indicator variable for years with financial restatements. In the first two columns, the restatement indicator is replaced by alternative measures of the use of accruals, and these variables influence the cartel indicator variable's coefficient toward zero. While the results in Table 1.5 are not robust, they are consistent with those found above documenting unusually slow replacement of directors and CEOs for cartel companies, and they suggest that management attempts to reduce external scrutiny by restricting access by new monitors. The estimated marginal effects

for the cartel variable are consistent with the negative but modest coefficient estimates, with reduced replacement frequencies of -0.008 estimated in the first two columns, both insignificant, and a significantly negative effect of -0.013 estimated for the cartel variable in the third column.

1.4.4 Contributions to political candidates

We study contributions by the sample of cartel companies to U.S. federal political candidates, using Federal Election Commission data for annual donations by Political Action Committees (PACs). We conjecture that the cartel firms will be unusually heavy donors, since political contributions may be viewed as a strategy for pre-empting future enforcement activity and deterring monitoring by regulators. Some basic descriptive statistics seem to bear out this hypothesis. Of the ten largest overall donors in the Federal Election Commission dataset, four are cartel firms in our sample, and eight of the ten largest one-year contribution totals come from cartel firms. As shown in the descriptive statistics in Table 1.1, cartel firms are much more like to be PAC donors than firms in the control sample (49% vs. 33%) and donate on average nearly three times as much per year as firms in the control sample ($67 million vs. $25 million).

In Table 1.6, we examine three dependent variables: the total dollar value of donations, if any, by each company's PAC each year; a binary (0,1) indicator variable for whether a firm's PAC reports any political activity, and the "political index" of Cooper, Gulen, and Ovtchinnikov (2010) based upon the number of candidates receiving donations from a company's PAC over the

previous five years.[5] As control variables, we include a number of financial variables used in studies by Cooper, Gulen, and Ovtchinnikov (2010) as well as Aggarwal, Meschke, and Wang (2012), including the frequency of donations by firms in the industry overall, and industry and year fixed effects.

Results in the table indicate that cartels are significantly more likely to donate than firms in the control sample. In the model measuring the size of donations and the number of candidates donated to, the cartel estimate is also positive but it misses statistical significance with t-statistics of 1.30 and 1.54, respectively. Collectively these findings are consistent with the hypothesis that cartel firms actively support political candidates as part of a strategy of discouraging regulatory scrutiny and seeking future forbearance should the firm's behavior become a target of government regulators. However, the marginal impact of cartel participation on political donations appears to be economically weak. Although Table 1.1 indicates that the propensity to donate is much higher for cartel vs. non-cartel firms (48.8% vs. 32.5%), the estimated marginal effect for the probit model in the middle column of Table 1.6 is only 0.036. This suggests that much of the difference in propensity to donate is explained by differences in other factors, especially firm size.

1.4.5 Accounting restatements

We investigate the propensity of cartel firms to engage in misleading financial reporting by analyzing their patterns of financial restatements. Our hypothesis is that cartel firms use restatements intentionally as part of a signal

[5]Matching PAC donations with the sample companies is difficult, since some PACs use the names of corporate subsidiaries, and the Federal Election Commission does not provide an index number that can be matched with Compustat and other research databases. We are grateful to Jongsub Lee for providing a link table and assisting us with the data merge. A few companies report negative values for total political contributions in certain years as a result of donations that are returned; our results are robust to deleting these observations.

jamming strategy that makes the firm's historical financials harder to understand.

We use the Audit Analytics database to download information about restatements filed by all of our sample firms between 2000 and 2010. The database covers restatements filed electronically with the SEC since January 1, 2001, including restatements for past years filed since that date. We exclude years earlier than 2000, because the database's coverage of their entire restatement history is likely to be incomplete. We do not find any significant differences in the types of restatements made by cartel firms and the companies in the control sample. Audit Analytics has four broad categories of restatements: accounting related (the large majority), fraud related, clerical errors, and other. The frequency of restatements in the two samples is not significantly different across any of these four classifications. Fraud related restatements account for more than twice the frequency of events for the cartel firms compared to the control firms (8.3% vs. 3.8%), but the difference is not statistically significant, and the sample size is quite small - only five restatements by the cartel firms are characterized by Audit Analytics as fraud related.

Table 1.7 presents our regression analysis of the frequency of restatements by cartel firms and the matched sample. In the left column, the dependent variable is a binary indicator for whether a fiscal year's results are eventually restated. This model is estimated in a probit framework including industry and year fixed effects. In the right column, the dependent variable is the number of times that a given year's results are restated, with the model estimated in a Poisson maximum likelihood framework. Although the entire sample

includes only 90 firm-years with more than one restatement, the analysis indicates a strong association between the frequency of these cases and cartel participation. The control variables in all models follow those used by Larcker, Richardson, and Tuna (2007). They include the book-to-market ratio of common equity lagged one year, the log of the market value of equity lagged one year, a measure of external financing equal to net equity plus net debt issued deflated by the lagged market value of equity, acquisition spending over the lagged market value of equity, and a measure of free cash flow calculated as the difference of operating cash flow and average capital expenditure over the three prior years, deflated by lagged market value of equity.

Table 1.7's estimates for the cartel firm indicator are positive and strongly significant in both columns. This evidence indicates that cartel firms are more likely to file restatements than firms in the control sample, consistent with a strategy of using misleading accounting in order to conceal the firm's true operating performance. The estimated marginal effect for the cartel variable in the left column is 0.056, which implies more than a one-third higher frequency for cartel firms than the baseline restatement frequency of 15.4 percent shown in Table 1.1 for companies in the control sample.

1.4.6 Earnings management

We expect cartel firms to engage in abnormally high levels of accounting earnings management. This strategy might serve either of two purposes: it could conceal the firm's rising profits from regulators and analysts, and it may signal to competing firms a desire to promote stable profits in the industry. We note that the direction of earnings management could be either positive or

negative, as necessary to distract outsiders from sharp changes in profitability. We begin by investigating whether cartels are unusually active in smoothing earnings. Our measure for earnings smoothing is based on the variability of the change in net income scaled by total assets (e.g., Leuz, Nanda, and Wysocki, 2003; Barth, Landsman, and Lang, 2008). A smaller variance of the change in net income is considered evidence of earnings smoothing.

As the change in net income is likely to be sensitive to various other factors, we use the variance of the residuals from a regression of the change in net income scaled by total assets on explanatory variables identified in previous research (Lang, Raedy, and Wilson, 2006; Barth, Landsman, and Lang, 2008). Our controls include firm size (the natural log of the market value of equity), sales growth rate, common equity growth rate, total liabilities growth rate, leverage, net cash flow scaled by assets, cash flow growth rate, sales scaled by assets, and year and industry fixed effects. Analysis in the left column of Table 1.8 is based on the residuals obtained from this regression. In the right column of Table 1.8, we repeat the analysis using a regression model augmented with interaction terms between all independent variables and an indicator variable that equals one for cartel firm-years. These interaction terms allow for different regression slopes for cartel firms and matched control firms, as the relation between the change in net income and the control variables may be affected by the cartel agreement itself.

To investigate earnings smoothing, we save the regression residuals from both models and compare the standard deviation of the residuals for cartel firm observations with the standard deviation of residuals for the control observa-

tions (although the test is commonly referred to as a "variance ratio test," the calculations actually use standard deviations). This test design is valid provided that the mean level of the residuals does not significantly differ between cartel firms and control firms (Barth, Landsman, and Lang, 2008). As shown in Table 1.8, the residuals' means are not significantly different between the subsamples. The variance ratio test statistics for both models indicate significantly less variability of residuals for the cartel firms compared to the control firms, consistent with more earnings smoothing by cartel members.

To understand firms' earnings smoothing more fully, we investigate two strategies for earnings management, the manipulation of discretionary accruals and deferred revenue. We estimate a firm's discretionary accruals with a version of the Jones (1991) model and modified Jones model (Dechow, Sloan, and Sweeney, 1995). We closely follow the empirical approach of Klein (2002) and Bergstresser and Philippon (2006), and we refer the reader to these papers for the exact specifications.

In Table 1.9 we present least-squares estimates of discretionary accruals for our cartel firms and the control sample. Other explanatory variables, following Klein (2002) and Bergstresser and Philippon (2006), include firm size (the log of enterprise value), leverage, the book-to-market ratio, the absolute value of the change in earnings before interest and taxes, and an indicator variable for firms that report two subsequent years of negative net income. All models include year and industry fixed effects. Estimates for the cartel firm indicator in the left two columns of Table 1.9 are both positive but not quite statistically significant.

In the third and fourth columns of Table 1.9, we change the dependent variable to equal the absolute value of discretionary accruals. This allows us to obtain a measure of cartel firms' accounting aggressiveness without reference to whether the accruals tend to increase or reduce reported earnings. We obtain estimates similar to those in the first two columns, but now they become statistically significant, providing evidence that cartel firms are more likely to avail themselves of accruals, whatever the direction of the impact on earnings.

Table 1.9 continues with two regressions estimating cartel firms' propensity to record deferred revenue, an accounting entry that essentially pushes profits into a future period by balancing an increase in cash received with an entry on the liability side of the balance sheet; the liability is then converted into shareholders' equity, an act that increases the firm's profits, during the future period when the income is deemed to be earned. We calculate deferred revenue as the sum of the Compustat variables DRC (revenue that has not been earned but is expected to be recognized in the current year) and DRLT (revenue that has not been earned and will be recognized in more than one year).

We present a Tobit analysis in column 5 of Table 1.9, with the dependent variable equal to deferred revenue scaled by net sales. In column 6, we estimate a binary probit model in which the dependent variable equals one if the firm has positive deferred revenue on its balance sheet in that year. We use the same explanatory variables that appear in our models of discretionary accruals. For either model, the estimate for the cartel firm indicator is negative and significant. The result is quite similar to that found for discretionary accruals, implying that cartel firms tend to report higher earnings in current

periods.

Interpreting the results in Table 1.9 seems challenging. Our main thesis in this paper is that cartel firms actively try to conceal the extent of their success in order to prolong the benefits from collusion. This might suggest less aggressive earnings management and slower revenue recognition, but we find the opposite. This may occur for a number of reasons. Cartel firms may be confident that profits will rise in the future and see little need for establishing accounting reserves and delaying revenue recognition. Alternatively, cartelists may feel vulnerable to outside scrutiny if revenue deferrals and slow accruals lead to an overwhelming delay of profit reporting, and they may make no effort to delay favorable accounting news in order to keep the pipeline clear of future positive news that could raise a red flag for regulators. Finally, cartel managers may be inherently unethical. They may inflate current-period earnings for reasons that are similar to, but not dependent upon, their tendency to engage in collusive behavior with rival firms. We pursue this interpretation further below when we investigate the incidence of litigation for securities fraud and also the propensity of cartel CEOs to exercise stock options early and inflate their cash bonus compensation.

1.4.7 CEO compensation

In this subsection we examine financial gains to CEOs of cartel firms. We do not find that CEOs of cartel firms earn significantly more compensation than their counterparts in the control sample, when compensation is valued as of the award date. However, we do find two significant patterns that are consistent with rent extraction by these CEOs. First, cartel firm CEOs time

the exercise of their stock options to coincide with the cartel period, during which their firms' stock prices are likely inflated. We document this tendency using the analysis in Table 1.10, and it probably allows these managers to increase the value of realized ex-post compensation. We also find that CEOs of cartel firms obtain a significantly larger fraction of total compensation in the form of cash bonuses, which is shown in Table 1.11. This pattern is consistent with the upward manipulation of accounting earnings that we document above, since cash bonuses are often tied to financial statement benchmarks, and it also implies that cartel firm CEOs receive compensation in a less risky form, since cash pay is not subject to the ex-post fluctuations in value that characterize awards of restricted stock and stock options.

In studying the timing of option exercises by cartel firm CEOs, we follow the format of Kedia and Philippon's (2009) study of companies that commit financial fraud. In that paper, the authors document an abnormally rapid exercise of in-the-money stock options by those firms' managers during the fraud period. The obvious interpretation of their result is that managers attempt to withdraw equity compensation from the company when the per-share price is inflated above its likely long-term value. We investigate stock option exercises by cartel firms. If these managers also exercise stock options rapidly, the motivation may be more subtle than for executives in companies that commit fraud. A successful cartel may expect its stock price to rise over time, suggesting that the managers would be patient about withdrawing equity compensation. However, they may have concerns about whether the cartel can be sustained, and they may wish to withdraw their compensation before regulators discover the scheme. In addition, rapid stock option exer-

cises may play a diversionary role, by communicating to outsiders that the managers do not expect future abnormal increases in the stock. It may also serve as a communication device by sending signals to managers of other firms in the cartel.

In Table 1.10 we present an analysis of stock option exercises by the CEOs in our cartel firms and our control sample. Our data source for option exercises is the S&P ExecuComp database, and relying on this database greatly reduces our sample size since it covers only about one-quarter of the companies on Compustat. Our regressions follow those reported by Kedia and Philippon (2009). We use two dependent variables: (i) the dollar value of option profits realized by managers, divided by the total amount by which vested options are in-the-money (their intrinsic value) at the start of the year, and (ii) a more simple calculation of the ratio between number of options exercised and the number that are vested and could theoretically have been exercised, whether in-the-money or not. Control variables include the size of the firm's total inventory of outstanding employee stock options, the exercise rate for all firms in the two-digit SIC industry, Tobin's Q, and the firm's stock return in the past year, along with fixed effects for year and industry.

In both models reported in Table 1.10, estimates indicate that CEOs from cartel firms exercise their stock options more rapidly than managers from firms in the control sample. As noted above, these patterns of early option exercise could occur for a number of reasons, but they are consistent with an interpretation that managers wish to withdraw their equity compensation before some future date at which the cartel might be exposed and the firm's

stock price could drop. The estimates appear to be economically large. For example, the mean of the dependent variable for non-cartel firms is 0.163, as shown in Table 1.1. The coefficient estimate of 0.080 in the top left cell of Table 1.10 indicates that the propensity to exercise options is nearly 50 percent stronger for CEOs in cartel firms compared to their counterparts in the control sample.

We examine CEOs' annual bonuses as a fraction of total compensation in Table 1.11. If cartel firms are able to increase profits while also smoothing cash flows, as suggested by the analysis above, their top managers may be able to obtain higher bonuses. This could occur not only because the firm earns higher overall returns, but also because the managers could more easily deliver quarterly and annual results tied to the accounting performance thresholds commonly found in cash bonus plans (Healy 1985).

We estimate Tobit models in Table 1.11 with the dependent variable equal to the CEO's bonus divided by total compensation, all as reported by ExecuComp. Control variables in the first column follow those used by Leone, Wu, and Zimmerman (2006) in their study of cash compensation. In columns 2 and 3, we augment the model with CEO-specific variables for age, tenure in office, and percentage ownership. As shown in the top line of the table the cartel indicator variable has a significant estimate in the range of +0.025, implying that cartel firm CEOs obtain about 2.5% higher compensation due to increased bonuses. As a robustness test, we follow the approach of Shaw and Zhang (2010) and multiply the control variables in Table 1.11 by interaction terms for firms with high and low ROA and high and low shareholder

returns. These additional controls make the estimate for the cartel variable slightly smaller, as it ranges between 0.020 and 0.022, and it remains significant in all three models. The mean CEO bonus as a fraction of total compensation is 0.165 for non-cartel firms, as shown in Table 1.1, indicating that the OLS coefficient estimates for the cartel variable in the top row of Table 1.11 are economically meaningful.

1.4.8 Securities fraud litigation

We use the Stanford Law School Securities Class Action database to identify companies accused of financial fraud in class action civil litigation brought by shareholders. After using stock trading symbols to match our sample firms with those in the database, we find that 44 of our 224 cartel firms are sued for securities fraud one or more times (for a total of 61 fraud firm-years) during the sample period, which does not begin until 1996, the starting date of the Stanford database. When scaled by the number of observations in the sample, the apparent lawsuit frequency for the cartel firms exceeds 5 percent per year, more than twice as high as the frequency for companies in the control sample, as shown by the sample means reported in Table 1.1.

We review the cases against the cartel firms in the Stanford database and find that concealment of price fixing schemes factor into only five of the 61 shareholders' complaints (including three against the co-conspirator companies in a single scheme). Cartel membership is not by itself an act of fraud, and shareholders may have difficulty arguing that they sustain damages when a firm successfully conspires to fix prices. Instead, many of the securities fraud suits cluster around well publicized events such as misleading equity research

by securities firms, deceptive client sales practices by brokerages, and illegal market timing by mutual fund managers. Numerous cases are also brought against medical companies for concealing adverse clinical trials, companies involved in cancelled mergers, and financially declining firms alleged to have inflated their forecasts.

In Table 1.12 we present regression analysis of the incidence of securities fraud lawsuits. A binary probit model appears in the left column, with the dependent variable equal to one if the firm is sued in a given year. A Poisson maximum likelihood model in the right column is based on the number of lawsuits filed per year. Our control variables follow studies of securities fraud lawsuits by Fich and Shivdasani (2007) and Bereskin, Campbell, and Kedia (2014), including firm size, profitability, leverage, and cash on the balance sheet. We obtain strongly positive and significant estimates for the cartel indicator variable, confirming the result that these firms attract shareholder litigation for fraud at unusually high frequencies. Again the results appear to be economically large. The annual lawsuit frequency is about 0.021 for non-cartel firms, as shown in Table 1.1, and the marginal effect for the cartel variable in the probit model is estimated at 0.014, implying that the lawsuit frequency at cartel firms is approximately two-thirds higher than at other comparable firms, controlling for other relevant factors.

The results about securities fraud lawsuits in Table 1.12 may fit a pattern consistent with those presented earlier indicating more irregular accounting and opportunistic compensation practices by the cartel firms. These companies' management teams may be predisposed to engage in unethical behavior,

and these tendencies may manifest themselves in a range of opportunistic and illegal conduct including both price fixing and financial statement fraud.

1.4.9 Alternative control samples as a robustness test

We examine a number of alternative procedures to constructing our control samples to ensure that our results are not sensitive to variations in our sampling procedure. Table 1.13 lists the sampling rule for the base case that is used throughout the paper along with six alternatives. Recall that in the base case, we identify the set of Compustat firms in the same two-digit SIC industry as the cartel firm and then choose the five closest based on size and industry, but retaining only those observations that satisfy a size criteria of total assets within +/- 50%. The first alternative listed in Table 1.13 relaxes the final exclusion, keeping all five control firms so long as at least one of them lies within the +/- 50% size criteria. The second alternative retains all five matched firms regardless of any size disparity, permitting all 210 cartel firms to enter the sample as opposed to the 185 cartel firms (prior to spinoffs) represented in the first two alternatives. The subsequent three alternatives are variations on propensity score matching. In the fourth line of Table 1.13, for the procedure labeled PS1, we retain the five closest observations based on fitted values from a logit regression with the cartel indicator as dependent variable and the log of total assets, log of firm age, industry concentration ratio, ROA, and year fixed effects as explanatory variables. In the next alternative, PS2, we restrict the set of candidate matching observations so that they come from the same set of industries as the cartel firms, although we do not require direct industry matching for each individual cartel. The final propensity score procedure, PS3, is similar to PS1 but excludes four cartels

for which no potential matching observations fall within a caliper of 0.01. Finally, the final alternative matching procedure takes the entire Compustat universe, excluding the cartel firms, as the control sample.

Panel A of Table 1.13 shows descriptive statistics about the size ratios of the cartel firms compared to the mean values of the matching firms for each matching procedure (no statistics appear for the final alternative, since no matching takes place between the cartel sample and the entire Compustat universe). The table illustrates that our base case exhibits mean and median size ratios closer to 1.00 than all of the alternatives, a lower standard deviation of the size ratio than any of them, and a tighter range of minimum and maximum values as well. The propensity score procedure picks control firms that are often larger than the matching cartel firms, while the other alternatives tend to do the opposite. Panel B of Table 1.13 uses asterisks to illustrate whether the cartel indicator variable has a statistically significant coefficient estimate in the various regression models studied above, with the outcome of the base case sample shown in the top row for comparison to the results of estimations using each of the six alternative control samples. As shown in the table, for most models the choice of control sample has little effect on the outcome of the estimation, and the estimates for our base case often achieve less statistical significance than would be the case if alternative control samples were used instead.

1.5 Conclusions

We study the behavior of U.S. public companies that are accused by governments of illegal cartel activity. We find that the sample firms engage in a

range of practices designed to obscure their behavior from both internal and external audiences.

Boards of directors of cartel firms appoint directors with poor monitoring capabilities, as new outside directors are likely to be busy with other board service, to have poor attendance records, and to live overseas. However, we find only limited evidence in support of two additional hypotheses related to boards. We expect high rates of director turnover and many board vacancies left unfilled, and while regression estimates tend in these directions they are not statistically significant in our base case model.

Cartel firms' CEOs turn over less quickly than CEOs of other companies, after controlling for size, performance, and other variables. When CEOs retire, their successors in cartel firms tend to be promoted from within. This appears to be a consequence of the need for management continuity in order to prolong price fixing conspiracies with other firms.

We find evidence that cartel firms attempt to manage their relationships with auditors and regulators. Auditing firms that do not blow the whistle on price fixing, either because of neglect or to protect their mandates, tend to be replaced significantly more slowly by cartel companies compared to firms in the control sample. Cartels appear to attempt to co-opt political regulators by making abnormally large and frequent political donations through their political action committees.

In financial reporting, cartel firms engage in unusually high levels of earnings smoothing and file high numbers of financial restatements for the cartel years. Additional analysis shows that these firms have high frequencies of abnormal

accruals and deferred revenue. Surprisingly, the signs of these accounting adjustments are positive, indicating that cartel managers tend to inflate their reported earnings. We conjecture that this may occur as a byproduct of managerial overconfidence, which could lead managers to enter into an illegal conspiracy and perhaps believe it can be prolonged indefinitely.

We find evidence that CEOs of cartel firms benefit from higher executive compensation in two ways. These managers exercise stock options earlier than usually, apparently to obtain higher ex-post compensation during a period in which the firm's stock price is likely inflated by successful price fixing.

Finally, we find that cartel firms are sued at high rates for securities fraud, although this litigation is rarely connected to the price fixing activity itself. Instead, this litigation occurring alongside cartel behavior may indicate a predisposition of certain companies not to comply with the law in a variety of situations that aren't necessarily connected.

Our results may provide a template for understanding how companies behave when they wish to conceal aspects of their financial performance. The firms in our sample engage in a range of signal-jamming strategies when preparing their annual financial statements, and they appear to avoid inviting scrutiny from new directors, new auditors, or new CEOs hired from outside. Managers try to cash out their performance-based compensation earlier than would be expected. Multiple explanations may apply to some of our results. For example, earnings smoothing or a certain timing of the exercise of stock options may be a communication device to other firms in a cartel.

Our paper's findings may extend to other situations, both benign and malign,

in which companies actively try to conceal information from their own monitors and from outside audiences. Firms may not want to give clear pictures of their capital investment or new product development spending, for instance, and may seek to obscure unusual spending patterns through strategies such as earnings smoothing or financial restatements. More concerning would be cases in which managers scheme to embezzle funds or mislead creditors about the firm's financial health. By understanding the playbook of strategies outlined in this paper, these and other problems might become apparent earlier to shareholders, analysts, and regulators.

Table 1.1: Summary statistics of cartel sample and control sample

The table shows descriptive statistics for a sample of 1,362 annual observations for 196 cartel participant firms and a control sample of 3,460 observations for 529 Compustat firms matched on size and industry. Cartel firms are identified from the dataset of Connor (2010). Board of directors data is tabulated from the RiskMetrics Directors database. Foreign independent directors are identified from the sample of Masulis, Wang, and Xie (2012). The political index is calculated according to Cooper, Gulen, and Ovtchinnikov (2010), and PAC contribution data is obtained from the Federal Election Commission database. Financial statement data is obtained from Compustat, which is also used to identify auditor changes. CEOs' stock option exercise and bonus data are obtained from ExecuComp, securities fraud lawsuits are tabulated from the Stanford Law School Securities Class Action website, and financial restatements are reported on the Audit Analytics database. Abnormal stock performance is the intercept from a market model regression calculated for each firm each year. Other variables follow definitions given in the tables below. Sample sizes below the maximum number of observations are the result of the limited coverage of these databases.

	Cartel firms				Control firms				Difference		
	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Obs.	in Means		p-value
Board becomes smaller (indicator)	0.315	0.000	0.465	609	0.286	0.000	0.452	1245	0.029		(0.194)
Fraction of directors leaving the board	0.102	0.091	0.105	650	0.097	0.091	0.105	1273	0.005		(0.339)
Newly appointed directors that are busy	0.454	0.000	0.752	348	0.337	0.000	0.607	691	0.117	***	(0.007)
Newly appointed directors with attend. problems (ind.)	0.038	0.000	0.193	156	0.010	0.000	0.099	307	0.029	**	(0.035)
Newly appointed directors that are foreign (ind.)	0.047	0.000	0.212	318	0.026	0.000	0.158	623	0.022	*	(0.081)
CEO change (indicator)	0.114	0.000	0.318	923	0.145	0.000	0.352	1970	-0.031	**	(0.021)
New CEO promoted from inside the firm (ind.)	0.858	1.000	0.350	106	0.788	1.000	0.409	288	0.070		(0.117)
Current CEO promoted from inside the firm (ind.)	0.911	1.000	0.284	553	0.816	1.000	0.388	1269	0.096	***	(0.000)
Auditor changes (indicator)	0.024	0.000	0.154	1243	0.036	0.000	0.186	3122	-0.012	**	(0.049)
Annual spending by PACs ($ millions)	67188.2	0.000	162547.3	1362	24636.1	0.000	85032.4	3460	42552.1	***	(0.000)
Indicator for nonzero spending by PACs	0.488	0.000	0.500	1362	0.325	0.000	0.469	3460	0.163	***	(0.000)
Political index	107.036	0.000	163.362	1362	42.922	0.000	91.763	3460	64.110	***	(0.000)
Restatement filed for the current year (indicator)	0.161	0.000	0.367	760	0.154	0.000	0.361	1869	0.006		(0.680)
Number of restatements filed for the current year	0.189	0.000	0.466	760	0.195	0.000	0.504	1869	-0.005		(0.804)
Discretionary accruals (Jones)	0.011	0.004	0.135	1165	0.003	0.003	0.133	2880	0.008	*	(0.092)
Discretionary accruals (Modified Jones)	0.003	-0.001	0.159	1165	-0.004	-0.002	0.140	2876	0.006		(0.210)
Absolute discretionary accruals (Jones)	0.076	0.043	0.113	1165	0.076	0.043	0.109	2880	0.000		(0.971)
Absolute discretionary accruals (Modified Jones)	0.083	0.043	0.135	1165	0.079	0.043	0.116	2876	0.004		(0.290)
Deferred revenues	0.016	0.000	0.058	587	0.042	0.000	0.220	1402	-0.026	***	(0.004)
Nonzero deferred revenue (indicator)	0.261	0.000	0.439	587	0.335	0.000	0.472	1402	-0.074	***	(0.001)
CEO value realized/intrinsic value of vested options	0.203	0.066	0.293	647	0.163	0.002	0.266	1364	0.040	***	(0.003)
CEO options excercised/vested options	0.111	0.014	0.201	724	0.094	0.000	0.190	1561	0.018	**	(0.045)
CEO bonus/total compensation	0.198	0.171	0.179	971	0.165	0.142	0.161	2032	0.032	***	(0.000)
Securities fraud lawsuits for current year (indicator)	0.050	0.000	0.217	988	0.021	0.000	0.142	2526	0.029	***	(0.000)
Number of securities fraud lawsuits for current year	0.057	0.000	0.264	988	0.021	0.000	0.142	2526	0.036	***	(0.000)

	Cartel firms				Control firms				Difference		
	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Obs.	in Means		p-value
Firm size (ln (MV of equity + BV of debt))	9.261	9.225	2.041	1265	8.341	8.325	1.852	3084	0.920	***	(0.000)
Return on assets	0.099	0.092	0.083	1353	0.070	0.070	0.100	3379	0.029	***	(0.000)
Book-to-market ratio	0.265	0.413	4.403	1264	-1.396	0.447	32.294	3081	1.661	*	(0.069)
Tobin's Q	1.928	1.463	1.377	1265	1.766	1.383	1.210	3084	0.161	***	(0.000)
Leverage	0.661	0.643	0.218	1348	0.669	0.649	0.305	3445	-0.007		(0.421)
Sales growth log(sales (t)/sales (t-1))	0.073	0.067	0.180	1327	0.051	0.055	0.233	3418	0.023	***	(0.001)
Sales Impact	0.045	0.019	0.066	1355	0.023	0.008	0.039	3436	0.022	***	(0.000)
Intangible assets/total assets	0.666	0.685	0.219	1335	0.689	0.736	0.236	3345	-0.022	***	(0.003)
Cash/total assets	0.058	0.032	0.072	1330	0.062	0.032	0.081	3320	-0.004		(0.106)
Free Cash Flow	-0.403	0.046	14.284	1124	-7.381	0.044	1015.298	2797	6.978		(0.818)
External financing	0.000	-0.004	0.092	1136	0.003	-0.001	0.110	2951	-0.003		(0.480)
Acquisitions/market capitalization	0.033	0.001	0.192	1095	2.843	0.000	148.194	2794	-2.810		(0.530)
Long term debt/total assets	0.225	0.186	0.212	1141	0.252	0.206	0.249	2924	-0.027	***	(0.001)
Average net debt issued/total assets, past 3 years	0.007	0.003	0.041	1135	0.010	0.002	0.060	2906	-0.003		(0.120)
Average net equity issued/total assets, past 3 years	-0.008	-0.002	0.038	1074	-0.004	0.000	0.052	2850	-0.004	**	(0.024)
Average cash shortfall, past three years	0.022	0.010	0.091	1076	0.049	0.009	1.099	2872	-0.028		(0.412)
Ind. for (R&D/total assets) above 75th percentile	0.266	0.000	0.442	1362	0.261	0.000	0.439	3460	0.005		(0.717)
Indicator for dividend reduction	0.143	0.000	0.351	1242	0.127	0.000	0.333	3214	0.016		(0.155)
Number of business segments	2.624	2.000	1.573	1096	2.217	2.000	1.493	2878	0.407	***	(0.000)
Foreign sales percentage	0.298	0.305	0.242	1203	0.252	0.190	0.262	2933	0.046	***	(0.000)
Options outstanding	48.996	11.270	97.791	729	26.066	7.113	84.310	1592	22.930	***	(0.000)
Abnormal stock performance	0.066	0.053	0.320	815	0.050	0.044	0.375	1821	0.016		(0.299)
Return to shareholders	1.865	0.044	60.322	1241	0.487	0.027	11.497	3066	1.379		(0.225)
Industry concentration	0.083	0.052	0.079	1362	0.072	0.047	0.070	3460	0.011	***	(0.000)
Industry contribution frequency	0.101	0.070	0.077	1362	0.095	0.068	0.075	3460	0.006	**	(0.020)
(Inventory + receivables)/total assets	0.307	0.288	0.171	1305	0.282	0.249	0.196	3395	0.025	***	(0.000)
Audior tenure	13.055	11.000	9.308	1297	12.266	10.000	8.963	3342	0.789	***	(0.008)
Indicator for CEO age $>$ 59	0.337	0.000	0.473	981	0.324	0.000	0.468	2071	0.013		(0.478)
Indicator for CEO tenure $<$ 4	0.398	0.000	0.490	981	0.477	0.000	0.500	2071	-0.080	***	(0.000)
CEO tenure	7.495	5.000	6.618	946	6.589	4.000	6.582	1977	0.906	***	(0.001)
CEO share ownership (%)	0.012	0.001	0.045	934	0.012	0.002	0.038	1924	0.001		(0.711)
Fraction of independent directors	0.703	0.750	0.160	680	0.688	0.714	0.176	1382	0.016	*	(0.053)

Table 1.2: Correlation matrix

The table shows simple correlations between the cartel indicator variable, the dependent variables, and variables for firm size and return on assets. The sample includes 1,362 annual observations for 196 cartel participant firms and a control sample of 3,460 observations for 529 Compustat firms matched on size and industry.

	Cartel firm		Board becomes smaller		Fraction of directors leaving the board		New busy directors		New directors with attendance problems	New foreign director		CEO change		New CEO promoted inside the firm	
Cartel firm (indicator)	1.000														
Board becomes smaller (indicator)	0.030		1.000												
Fraction of directors leaving the board	0.022		0.002		1.000										
New busy directors	0.083	***	-0.042		-0.003		1.000								
New directors with attend. problems (ind.)	0.098	**	-0.037		-0.009		0.022		1.000						
New foreign directors (indicator)	0.057	*	0.036		-0.003		0.015		-0.029	1.000					
CEO change (indicator)	-0.043	**	0.058	**	0.106	***	0.008		0.024	-0.018		1.000			
New CEO promoted inside the firm (ind.)	0.122	***	-0.008		-0.068	**	0.053		-0.011	-0.018		-0.048	**	1.000	
Auditor change (indicator)	-0.030	**	-0.033		-0.017		-0.012		0.034	-0.005		0.000		-0.038	
Annual spending by PACs	0.168	***	0.043	*	0.012		0.124	***	-0.022	0.024		-0.009		0.073	***
Restatement filed for the current year (ind.)	0.008		0.019		0.058	**	0.101	***	-0.063	-0.020		-0.005		0.096	***
Discretionary accruals (Mod. Jones)	0.020		-0.001		-0.010		0.011		-0.028	0.060	*	-0.011		-0.003	
CEO value realized/intrinsic value of vested options	0.067	***	0.016		0.013		0.040		0.019	0.058		-0.088	***	0.093	***
CEO bonus/total compensation	0.090	***	0.005		-0.034		-0.011		-0.042	-0.008		-0.035	*	0.065	***
Securities fraud lawsuit (indicator)	0.078	***	0.035		0.062	***	0.101	***	-0.027	-0.012		0.036	*	0.046	*
Firm size (enterprise value)	0.175	***	0.045	*	0.102	***	0.131	***	-0.037	-0.009		0.019		0.093	***
Return on assets	0.135	***	-0.082	***	-0.105	***	0.043		-0.034	0.080	**	-0.049	***	0.047	**

	Auditor change		Annual spending by PAC's		Restatement filed for current year		Disc. accruals (Mod. Jones)		CEO value realized/ intrinsic value of vested options		CEO bonus/total comp.		Securities fraud lawsuit		Firm size		Return on assets
Auditor change (indicator)	1.000																
Annual spending by PACs	-0.031	**	1.000														
Restatement filed for current year	0.041	**	-0.030		1.000												
Discretionary accruals (Mod. Jones)	-0.003		-0.007		-0.023		1.000										
CEO value realized/intrinsic value of vested options	0.049	**	0.043	*	-0.024		-0.015		1.000								
CEO bonus/total compensation	-0.016		-0.050	***	-0.017		0.015		-0.020		1.000						
Securities fraud lawsuit (indicator)	-0.020		0.118	***	0.042	**	-0.046	**	0.006		-0.013		1.000				
Firm size (enterprise value)	-0.019		0.362	***	0.075	***	-0.025		0.039	*	0.086	***	0.187	***	1.000		
Return on assets	-0.017		0.042	***	-0.066	***	0.020		0.020		0.124	***	-0.014		-0.026	*	1.000

Table 1.3: Changes in boards of directors

The table shows regression estimates of changes in firms' boards of directors. The first column reports results from a Tobit regression of the percentage of directors leaving the board in a given year. The second column reports results from a probit regression in which the dependent variable equals one if board size decreases in a given year. The next four columns report results for models of the probability that new outside board members have certain characteristics. While the dependent variable for these regressions is a count variable, it has too little variation to justify the estimation of Poisson models in columns 4 through 6 (in only two cases more than one director with attendance problems is appointed and in four cases more than one foreign independent director). The sample includes all new outside board appointments by 196 U.S. firms identified by regulators between 1986-2010 as cartel participants, as well as a control sample of 529 Compustat firms matched on size and industry. Observations are restricted to the firms and period covered by the RiskMetrics Directors database (1996-present). Busy directors are defined as those holding three or more board seats. Directors with attendance problems are those who attend fewer than 75% of all board and committee meetings. The observations for attendance problems are stopped after 2001, when the Sarbanes-Oxley Act took effect and director attendance improved markedly. Data on foreign outside directors is obtained from Masulis, Wang, and Xie (2012). In addition to the control variables in Table 3, we include controls for research & development and intangible assets following Coles, Daniel, and Naveen (2008). All regressions include industry fixed-effects. t-statistics and z-statistics appear in parentheses.

Dependent variable:	% of directors leaving (Tobit)		Board becomes smaller (Probit)		New busy directors (Poisson)		Attendance problem (Probit)		New foreign director (Probit)		New foreign director (Probit)	
Cartel firm indicator	0.012		0.102		0.282	***	1.281	**	0.447	**	0.514	**
	(1.534)		(1.502)		(2.583)		(2.461)		(2.405)		(2.486)	
log(firm size(t)/firm size(t-1))	-0.043	**	-0.063		0.063		0.369		-0.540		-0.605	
	(-2.530)		(-0.432)		(0.256)		(0.469)		(-1.495)		(-1.638)	
Abnormal stock performance (t)	-0.025	*	-0.023		-0.085		-1.013		0.208		0.322	
	(-1.786)		(-0.196)		(-0.432)		(-1.489)		(0.644)		(0.939)	
Abnormal stock performance (t-1)	-0.031	***	-0.240	**	-0.011		1.780	**	0.038		0.130	
	(-2.787)		(-2.435)		(-0.068)		(2.432)		(0.144)		(0.478)	
CEO age > 59 (indicator)	0.004		0.017		-0.092		-1.076	*	0.532	**	0.521	**
	(0.473)		(0.230)		(-0.722)		(-1.897)		(2.548)		(2.384)	
CEO tenure < 4 (indicator)	0.020	**	0.176	**	-0.018		-0.574		0.210		0.185	
	(2.484)		(2.557)		(-0.160)		(-1.260)		(1.024)		(0.858)	
(R&D/total assets) above 75th					0.188		-1.130	*	0.139		-0.063	
percentile (indicator)					(1.338)		(-1.941)		(0.684)		(-0.286)	
Intangible assets/total assets					0.570		0.516		0.897		1.123	
					(1.521)		(0.330)		(1.204)		(1.369)	
SOX Dummy	-0.008		-0.128	*	-0.158				-0.180		-0.249	
	(-1.062)		(-1.916)		(-1.421)				(-0.984)		(-1.252)	
Foreign sales percentage											1.455	***
											(3.267)	
Firm-year observations	1837		1749		958		351		790		667	
Pseudo r-squared			0.015		0.019		0.321		0.092		0.122	
LR chi-squared	55.636											
Prob. > chi-squared	0.000											
Cartel firm-years	627		579		321		127		266		231	
Control firm-years	1210		1170		637		224		524		436	
Cartel firms	119		113		100		51		87		77	
Control firms	225		231		190		95		164		138	
Marginal cartel effect (at means)	0.012		0.035		0.110		0.016		0.033		0.039	
z	1.53		1.49		2.47		1.27		2.16		2.26	
P>|z|	0.125		0.136		0.013		0.205		0.031		0.024	

Table 1.4: Retention and replacement of CEOs

The table shows probit regression estimates for models of CEOs' turnover and the characteristics of their replacements. The sample includes 196 U.S. firms identified by regulators as cartel participants, as well as a control sample of 529 Compustat firms matched on size and industry. The first column presents a model of CEO replacement with the dependent variable equal to 1 if a new CEO takes office. The second and fourth columns show estimates for models in which the dependent variable equals 1 if the new CEO is promoted from within the firm. This estimation is restricted only to those firm-years in which a CEO replacement occurs. The third and fifth columns present models for whether the incumbent CEO, regardless of length of tenure, was originally promoted from within the firm. Most control variables are based on Parrino, Sias, and Starks (2003). All regressions include year and industry fixed-effects. z-statistics appear in parentheses.

Dependent variable:	CEO replaced		New CEO promoted inside		Current CEO promoted inside		New CEO promoted inside		Current CEO promoted inside	
Cartel firm indicator	-0.233	***	0.527	*	0.435	***	0.498		0.326	**
	(-2.627)		(1.849)		(3.928)		(1.453)		(2.502)	
CEO Tenure, as of prior year	0.010	*	-0.008		0.012		0.022		0.029	*
	(1.748)		(-0.473)		(0.961)		(0.871)		(1.745)	
Dividend reduction in prior year (indicator)	-0.056		0.240		-0.226	*	0.435		-0.007	
	(-0.472)		(0.642)		(-1.705)		(1.027)		(-0.042)	
Board composition (% independent)	0.145		-3.139	***	-1.580	***	-2.325	**	-1.230	***
	(0.563)		(-3.518)		(-4.384)		(-2.330)		(-3.066)	
Return on asset, industry adjusted, (t-1)	-0.237		0.022		0.425		-0.039		1.310	**
	(-0.609)		(0.019)		(0.944)		(-0.029)		(2.476)	
Abnormal stock performance (t-1)	-0.229	**	0.474		0.014		0.476		0.102	
	(-2.112)		(1.294)		(0.096)		(1.212)		(0.666)	
CEO age, as of prior year	0.816	***	0.814	***	0.152		0.618	**	0.023	
	(9.295)		(3.122)		(1.299)		(2.078)		(0.174)	
Number of business segments							0.001		0.080	**
							(0.009)		(2.126)	
Firm-year observations	1851		241		1268		183		969	
Pseudo r-squared	0.110		0.194		0.082		0.176		0.083	
Cartel firm-years	633		67		420		46		302	
Control firm-years	1218		174		848		137		667	
Cartel firms	116		60		90		41		69	
Control firms	233		133		184		107		157	
Marginal cartel effect (at means)	-0.040		0.085		0.076		0.087		0.060	
z	-2.76		2.15		4.34		1.72		2.71	
P>|z|	0.006		0.031		0.000		0.085		0.007	

Table 1.5: Changes in auditors
The table reports the results from probit regressions of an indicator variable that equals 1 when the firm changes auditors in a given year. The sample includes 196 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of 529 Compustat firms matched on size and industry. Auditor changes are identified from Compustat. The sample excludes a small number of observations for firms that do not have Big 4 auditors. All income statement and balance sheet variables are measured with respect to the prior fiscal year, and the definitions of control variables follow those used by Landsman, Nelson, and Rountree (2009). All regressions include year and industry fixed-effects. z-statistics appear in parentheses.

Dependent Variable:	Company changes auditor (0,1)					
	(1)		(2)		(3)	
Cartel firm indicator	-0.181		-0.182		-0.348	*
	(-1.469)		(-1.475)		(-1.725)	
Growth in total assets	-0.074		-0.045		-0.002	
	(-0.502)		(-0.311)		(-0.014)	
Return on assets < 0 (indicator)	0.137		0.140		0.392	
	(0.619)		(0.635)		(1.345)	
(Invetory + receivables)/total assets	0.338		0.370		1.068	*
	(0.718)		(0.788)		(1.869)	
Auditor Tenure (*0.001)	-0.004		-0.187		-5.945	
	(-0.001)		(-0.030)		(-0.747)	
Cash/total assets	0.938	**	0.937	**	0.600	
	(2.009)		(2.010)		(0.999)	
Total liabilities/total assets	0.188		0.193		-0.036	
	(0.932)		(0.961)		(-0.124)	
Return on assets	0.785		0.766		0.053	
	(0.957)		(0.933)		(0.045)	
Firm size (log of enterprise value)	-0.097	**	-0.098	**	-0.005	
	(-2.399)		(-2.410)		(-0.094)	
Acquisition (indicator)	-0.204		-0.201		-0.511	
	(-0.878)		(-0.869)		(-1.193)	
Absolute discretionrary accruals (Jones)	0.308					
	(0.689)					
Absolute discretionrary accruals (Modified Jones)			-0.023			
			(-0.049)			
Restatement (indicator)					0.176	
					(0.540)	
Firm-year observations	2749		2749		1487	
Pseudo r-squared	0.165		0.165		0.123	
Cartel firm-years	783		783		394	
Control firm-years	1966		1966		1093	
Cartel firms	139		139		98	
Control firms	348		348		235	
Marginal cartel effect (at means)	-0.008		-0.008		-0.013	
z	-1.58		-1.59		-2.01	
P>|z|	0.113		0.112		0.045	

Table 1.6: Contributions to political candidates
The table reports regression estimates of models of contributions to political candidates by corporate political action committees (PACs). In the left column, the dependent variable equals the log of the total dollar value contributed per year by each company. In the center column, the dependent variable is a binary indicator for nonzero political contributions by the company. In the right column, the dependent variable is the log of the political index, defined by Cooper et al. (2010) to equal the total number of supported candidates over the previous five years. The sample includes 196 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of 529 Compustat firms matched on size and industry. Control variables are based on definitions used in Cooper, Gulen, and Ovtchinnikov (2010) and Aggarwal, Meschke, and Wang (2012). All regressions include year and industry fixed-effects. t-statistics based on standard errors clustered at the firm level and z-statistics appear in parentheses.

Dependent variable:	Log(annual spending) (OLS)		Contribution indicator (probit)		log(political index) (OLS)	
Cartel firm indicator	0.569		0.099	*	0.312	
	(1.302)		(1.836)		(1.541)	
Firm size (log of enterprise value)	1.370	***	0.480	***	0.622	***
	(9.955)		(18.814)		(9.700)	
Book-to-Market	0.005	*	0.002	**	0.002	**
	(1.821)		(2.096)		(2.132)	
Leverage	0.415		0.298	***	0.216	
	(0.809)		(2.851)		(0.926)	
Return on asstes	-2.113		-0.546		-0.875	
	(-1.258)		(-1.592)		(-1.167)	
log(sales(t)/sales (t-1))	-0.771	**	-0.237	**	-0.432	***
	(-2.197)		(-2.006)		(-2.784)	
Sales Impact	5.774		-1.143		3.563	
	(1.080)		(-1.303)		(1.431)	
Free Cash Flow	0.010	*	0.022		0.004	*
	(1.699)		(1.470)		(1.847)	
Industry concentration	1.192		1.092		-0.431	
	(0.219)		(0.771)		(-0.179)	
Industry contribution frequency	14.744	***	5.840	***	6.715	***
	(3.074)		(4.864)		(2.976)	
Firm-year observations	3883		3825		3883	
R-squared	0.341				0.369	
Pseudo r-squared			0.245			
Cartel firm-years	1121		1102		1330	
Control firm-years	2762		2723		3398	
Cartel firms	176		172		204	
Control firms	438		426		521	
Marginal cartel effect (at means)	0.569		0.036		0.312	
z	1.30		1.82		1.54	
P>|z|	0.193		0.068		0.123	

Table 1.7: Restatements
The table reports probit regression estimates for whether a firm restates its audited financial statements for a given year and Poisson regression estimates of the number of restatements per year. The sample includes 196 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of 529 Compustat firms matched on size and industry. Regressions include control variables following the definitions in Larcker, Richardson, and Tuna (2007). The External Financing variable equals net equity plus net debt issued deflated by the lagged market value of equity. Free Cash Flow is calculated as the difference between operating cash flow and average capital expenditure over the three prior years, deflated by lagged market value of equity. Data for restatements is obtained from the Audit Analytics database, which covers restatements disclosed since January 1, 2001. All regressions include year and industry fixed-effects. z-statistics appear in parentheses.

Dependent Variable:	Restatement indicator (Probit)		Number of times restated (Poisson)	
Cartel firm indicator	0.268	***	0.336	**
	(2.873)		(2.471)	
Book-to-market ratio	0.018		0.025	*
	(1.561)		(1.817)	
Firm size (log of enterprise value)	-0.103	***	-0.171	***
	(-3.539)		(-3.826)	
External financing	0.650		1.037	
	(1.513)		(1.629)	
Acquisitions/market capitalization	0.518		0.403	
	(1.596)		(0.844)	
Free Cash Flow	0.064	**	0.065	**
	(2.024)		(2.099)	
Firm-year observations	1686		1686	
pseudo r-squared	0.086		0.078	
Cartel firm-years	463		463	
Control firm-years	1223		1223	
Cartel firms	116		116	
Control firms	304		304	
Marginal cartel effect (at means)	0.056		0.048	
z	2.70		2.29	
P>|z|	0.007		0.022	

Table 1.8: Earnings smoothing
The table reports the results from a variance ratio test that compares the standard deviations of regression residuals for subsamples of observations used to estimate earnings smoothing models. In the left column, statistics are based on residuals from a regression model of the annual change in net income using control variables described in the text, with observations for cartel firms pooled together with observations from a control sample of Compustat firms matched on size and industry. In the right column, statistics are based on residuals from a similar regression that also includes interaction terms between all control variables and an indicator that equals one for cartel firms. Results in the fifth line of the table show the outcome of F-tests for the null hypothesis that the variance ratios equal one. Rejection of the null hypothesis in both columns is robust to the use of alternative Levene and Brown-Forsythe test statistics.

	Pooled regression	Pooled regression with interaction terms
t-test equality means (p-value for H_0: difference=0)	0.848	1.000
Control firm residuals' standard deviation	0.132	0.132
Cartel firm residuals' standard deviation	0.093	0.091
Ratio of residuals' standard deviation	1.426	1.454
F-test (p-value for H_0: Ratio=1)	0.000	0.000
Firm-year observations	4132	4132
Levene and Brown-Forsythe test statistics		
W0	0.000	0.000
W50	0.000	0.000
W10	0.000	0.000
Cartel firm-years	1210	1210
Control firm-years	2922	2922
Cartel firms	184	184
Control firms	455	455

Table 1.9: Earnings management

The table reports regression estimates of discretionary accruals and revenue recognition. Following Bergstresser and Philippon (2006) and Klein (2002), the discretionary accruals are constructed according to the cross-sectional Jones and the cross-sectional modified Jones models. The measure of deferred revenue is the ratio of Compustat items DRC+DRLT divided by net sales. DRC is revenue which has not yet been earned, but is expected to be classified as earned during the current year, while DRLT is revenue which has not yet been earned. The indicator in the sixth column equals 1 if DRC+DRLT is greater than zero. All regressions include year and industry fixed-effects. *t*-statistics based on standard errors clustered at the firm level, *t*-statistics, and *z*-statistics appear in parentheses.

Dependent Variable:	Discretionary accruals		Absolute discretionary accruals		Deferred revenue	
	(Jones)	(Mod. Jones)	(Jones)	(Mod. Jones)	(Amount)	(Indicator)
	(OLS)	(OLS)	(Tobit)	(Tobit)	(Tobit)	(Probit)
Cartel firm indicator	0.008	0.008	0.008 *	0.013 ***	-0.117 ***	-0.339 ***
	(1.503)	(1.588)	(1.878)	(2.822)	(-3.961)	(-3.880)
Book-to-market ratio (t-1) (*0.001)	-0.010 ***	-0.004	-0.005	-0.005	1.531	6.044 *
	(-2.840)	(-1.180)	(-0.249)	(-0.203)	(1.449)	(1.797)
Absolut value of change in EBIT (*0.01)	-0.022	-0.020	0.016	0.016	0.051	0.397
	(-1.102)	(-1.032)	(1.640)	(1.466)	(0.430)	(0.892)
Firm size (log of enterprise value)	-0.001	0.000	-0.007 ***	-0.006 ***	0.021 **	0.110 ***
	(-0.684)	(0.331)	(-5.123)	(-4.330)	(2.424)	(4.084)
Long term debt/total assets	0.084 ***	0.084 ***	0.051 ***	0.033 ***	0.182 ***	0.041
	(3.538)	(4.253)	(6.126)	(3.463)	(3.330)	(0.240)
Two years of subsequent negative	-0.025 ***	-0.013	-0.001	0.008	0.154 ***	0.423 ***
income (indicator)	(-2.709)	(-1.302)	(-0.160)	(1.080)	(4.430)	(3.767)
Firm-year observations	3171	3169	3171	3169	1583	1494
r-squared	0.066	0.048				
Pseudo r-squared						0.168
LR chi-squared			519.305	600.812	305.302	
Prob. > chi-squared			0.000	0.000	0.000	
Cartel firm-years	934	934	934	934	460	404
Control firm-years	2237	2235	2237	2235	1123	1090
Cartel firms	145	145	145	145	126	113
Control firms	343	343	343	343	277	272
Marginal cartel effect (at means)	0.008	0.008	0.008	0.013	-0.117	-0.112
z	1.50	1.59	1.88	2.82	-3.96	-4.10
P>|z|	0.133	0.112	0.060	0.005	0.000	0.000

Table 1.10: Stock option exercises by CEOs
The table reports Tobit regression estimates for the value realized from options exercised over the intrinsic value of vested options (Column 1) and the number of options exercised over total vested options (Column 2). The sample includes 196 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of 529 Compustat firms matched on size and industry. Option exercise data is obtained from ExecuComp. Regressions include control variables for options outstanding, past stock performance, Tobin's Q and average industry exercises, which are calculated as the average value of the dependent variable for all firms in the same two-digit SIC industry with data on ExecuComp in that year. All control variables are lagged by one year. All regressions include year and industry fixed-effects. *t*-statistics appear in parentheses.

Dependent variable:	Value realized/intrinsic value of exercisable options (1)		Options exercised/ vested options held (2)	
Cartel firm indicator	0.080	***	0.050	**
	(2.755)		(2.481)	
Average industry exercises	0.236		0.133	
	(0.982)		(0.549)	
Options outstanding (*0.01)	-0.001		-0.019	*
	(-0.040)		(-1.708)	
Past year returns	-0.001		0.001	
	(-0.255)		(0.418)	
Tobin's Q	0.031	***	0.038	***
	(3.538)		(6.361)	
Firm-year observations	1642		1849	
LR chi-squared	99.039		176.300	
Prob. > chi-squared	0.000		0.000	
Cartel firm-years	554		619	
Control firm-years	1088		1230	
Cartel firms	120		121	
Control firms	215		224	
Marginal cartel effect (at means)	0.080		0.050	
z	2.75		2.48	
P>|z|	0.006		0.013	

Table 1.11: Bonus compensation for CEOs
The table reports Tobit regression estimates for the value of the CEO's annual bonus as a fraction of total compensation. The sample includes 196 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of 529 Compustat firms matched on size and industry. Compensation data is obtained from the ExecuComp database. Control variables in the first column follow Leone, Wu, and Zimmerman (2006). Cash shortfall equals dividends plus financial cash flow, minus operating cash flow, all divided by total assets. All regressions include year and industry fixed-effects. *t*-statistics appear in parentheses.

Dependent Variable:	CEO bonus/total compensation					
	(1)		(2)		(3)	
Cartel firm indicator	0.024	**	0.027	***	0.025	**
	(2.576)		(2.777)		(2.504)	
Firm size (log of enterprise value)	-0.007	*	-0.007	*	-0.007	*
	(-1.828)		(-1.792)		(-1.916)	
Return on asste	0.753	***	0.771	***	0.796	***
	(10.632)		(10.488)		(10.665)	
Book-to-market ratio (*0.01)	-0.033	*	-0.037	*	-0.035	*
	(-1.773)		(-1.953)		(-1.890)	
Leverage	0.015		0.011		0.003	
	(0.730)		(0.496)		(0.134)	
Shareholder return (*0.01)	0.047		0.047		0.042	
	(0.491)		(0.489)		(0.442)	
Average cash shortfall	0.022		0.020		0.017	
(past three years)	(0.509)		(0.440)		(0.375)	
Average net debt issued/total assets	-0.102		-0.122		-0.108	
(past three years)	(-1.019)		(-1.153)		(-1.007)	
Average net equity issued/total assets	0.322	***	0.293	***	0.279	**
(past three years)	(3.047)		(2.650)		(2.498)	
CEO tenure			-0.001		-0.001	
			(-1.258)		(-1.390)	
log(CEO age)			0.061		0.033	
			(1.426)		(0.756)	
CEO share ownership (%)					0.066	
					(0.520)	
Firm-year observations	1996		1902		1807	
LR chi-squared	737.279		703.594		704.130	
Prob. > chi-squared	0.000		0.000		0.000	
Cartel firm-years	614		592		571	
Control firm-years	1382		1310		1236	
Cartel firms	132		127		124	
Control firms	267		256		250	
Marginal cartel effect (at means)	0.024		0.027		0.025	
z	2.58		2.78		2.50	
P>|z|	0.01		0.005		0.012	

Table 1.12: Securities fraud litigation
The table reports the results from regressions for the incidence of securities fraud lawsuits filed during the period 1996-2010, based on the Stanford Law School Securities Class Action database. The sample includes 196 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of 529 Compustat firms matched on size and industry. Control variables follow definitions in Fich and Shivdasani (2007) and Bereskin, Campbell, and Kedia (2014). All regressions include year and industry fixed-effects. z-statistics appear in parentheses.

Dependent Variable:	Fraud lawsuit indicator (Probit) (1)		Number of fraud lawsuits (Poisson) (2)	
Cartel firm indicator	0.248	**	0.711	***
	(2.184)		(3.204)	
Book-to-market ratio	0.020		0.052	
	(0.877)		(1.245)	
Return on assets	-2.546	***	-5.710	***
	(-3.690)		(-4.346)	
Leverage	-0.068		-0.341	
	(-0.262)		(-0.547)	
Firm size (log of enterprise value)	0.284	***	0.564	***
	(6.382)		(6.499)	
Cash/total assets	0.660		1.924	
	(0.952)		(1.384)	
log(sales(t)/sales(t-1))	-0.301		-0.683	*
	(-1.387)		(-1.646)	
Firm-year observations	2505		2505	
pseudo r-squared	0.138		0.161	
Cartel firm-years	729		729	
Control firm-years	1779		1779	
Cartel firms	132		132	
Control firms	326		326	
Marginal cartel effect (at means)	0.014		0.016	
z	1.90		2.56	
P>|z|	0.057		0.011	

Table 1.13: Robustness tests
The table shows effects of alternative procedures for drawing a control sample for the cartel firms in the paper. The matching excludes 14 observations that enter the sample as spinoffs and inherit the control observations of the parent firm, so the potential number of cartel firms to be matched is 210. As shown in the first row of the table, we ultimately find successful matches for 185 of these cartel firms in our base case. These firms spin off 11 additional cartel companies during the sample period, for a total of 196 cartel firms in our main sample.

Panel A describes the method used to construct the control sample for the main sample, along with the methods for six alternative matching procedures that could also be used to construct control samples. In each row, the table shows how many of the 210 cartel firms remain in the analysis contingent on satisfying the matching criteria. The table also shows descriptive statistics about the size ratios between the cartel firms and their matched firms, using total assets as the measure of size. In each case, the size ratio compares the total assets of the cartel firm with the mean value of total assets for the matching firms, calculated as of the matching year. The base case requires matching observations to come from the same two-digit SIC industry, and it then selects the five closest firms based on size but excludes 25 cartels for which no size match is within +/- 50%. The control sample includes only the subset of five matching observations that also meet the +/- 50% size restriction. The alternative size restriction method is similar to the base case, but it uses all five matching observations so long as at least one of them meets the +/- 50% size restriction. The method with no size restriction simply draws the five closest firms based on total assets without excluding any cartels based on size difference of the matching firms. PS1 is a propensity score matching method that selects the five closest observations based on fitted values from a logit regression with the cartel indicator as dependent variable and the log of total assets, log of firm age, industry concentration ratio, return on assets, and year fixed effects as explanatory variables. PS2 is similar to PS1, but it restricts the potential control observations to the same set of two-digit SIC industries as the cartel firm, though it does not require all matching to be done within industries. PS3 is also similar to PS1, but it excludes four cartels for which no potential matching observations fall within a caliper of 0.01. The final alternative uses the entire Compustat universe, excluding the cartel firms, as the control sample. No size comparisons are shown for the final case since no control firms are matched to individual cartel firms.

Panel B shows the results of re-estimating the paper's regressions using the samples generated by the alternative matching procedures. The table shows whether the coefficient estimate for the cartel indicator in each column of every table has statistical significance at conventional levels. For all significant estimates the signs of the coefficients are identical across the different samples.

***, **, and * indicate statistical significance of regression estimates at the 1%, 5%, and 10% levels, respectively.

Panel A: matching method

	Type of match	Description of method	Cartel firms	Size ratio based on total assets Mean	Median	Std.	Min.	Max.
(1)	Base case	Match on industry and use the five closest firms based on size. Drop any cartel firm for which no match has a size difference less than 50%. Use only those matched firms with size difference less than 50% for those cartel firms that are kept.	185	1.12	1.03	0.23	0.54	1.98
(2)	Alternative size restriction	Match on industry and use the five closest firms based on size. Drop any firm for which no match has a size difference less than 50%. Use all five matched firms for those cartel firms that are kept.	185	1.31	1.08	0.52	0.86	4.36
(3)	No size restriction	Match on industry and use the five closest firms based on size without restriction.	210	2.52	1.13	8.16	0.86	114.55
(4)	PS1	Use the five closest firms based on propensity score.	209	1.07	0.84	0.81	0.08	5.48
(5)	PS2	Use the five closest firms based on propensity score, with all matching observations drawn from same industry set as cartel firms.	209	1.00	0.79	0.77	0.03	4.90
(6)	PS3	Use the five closest firms based on propensity score, but drop any firm for which no match is within caliper of 0.01.	205	0.99	0.78	0.79	0.03	4.90
(7)	All of Compustat	Use the entire Compustat universe, excluding the cartel firms, as the matching sample.	210	n.a.	n.a.	n.a.	n.a.	n.a.

Panel B: statistical significance of coefficient estimates for cartel indicator variable

	Table 3						Table 4					Table 5		Table 6			Table 7	
	c1	c2	c3	c4	c5	c6	c1	c2	c3	c4	c5	c1	c2	c3	c1	c2	c3	c1	c2
(1)			***	**	**	**	***	*	***		**			*		*		***	**
(2)	*		*	**	**	**	**	*	***	*	***			*		*		***	**
(3)	*	**	***	***	**	**	*	**	***	**	***			*		*	*	**	*
(4)	**		**	**	*				***		**					***	*	*	
(5)	**		**	**	**		*		***		***			*	**	***	**	*	
(6)	**		*	**	*				***		***			*	*	***	**	*	
(7)	***	***	***	***	***	**		**	***	**	***			**	***	***	***	*	**

	Table 9						Table 10		Table 11			Table 12	
	c1	c2	c3	c4	c5	c6	c1	c2	c1	c2	c3	c1	c2
(1)			*	***	***	***	***	**	**	***	**	**	***
(2)		*	*	***	***	***	***	**	**	***	**	**	***
(3)	*	*	**	***	***	***	***	***	**	***	***	**	***
(4)					**	***	*		*	**	**	***	***
(5)	*			***	***		***	***		*		*	**
(6)	*			***	***		***	***				*	**
(7)						***	***	**	**	**		**	***

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Chapter 2

Coordinated shareholder activism: A potential remedy for the firm free-rider problem

Coordinated shareholder activism: A potential remedy for the firm free-rider problem

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Abstract. We study coordinated shareholder activism campaigns, i.e., multiple activists simultaneously targeting the same firm. Using a unique and comprehensive database of activism events we document that activists prefer to participate in coordinated campaigns and that these campaigns produce significantly higher returns than non-coordinated campaigns. Consistent with activists working together, we find that this phenomenon is more prevalent among geographically proximate activists where frictions to coordination are lower. We posit that coordinated campaigns are a mechanism through which activists can mitigate free-rider problems. Consistent with this hypothesis, we find that coordinated campaigns are more prevalent where the free-rider problems faced by activists are greater - at large firms and among small activists.

JEL: G11, G14, G32
Keywords: Shareholder activism, large blockholder, coordination, free-rider.

"The more numbers [of activists] you have, the more influence you have."

– Charles Elson, Director of the John L. Weinberg Center for Corporate Governance

2.1 Introduction

Shareholder activists are central to mitigating the agency costs associated with the separation of firm ownership and management. Shleifer and Vishny (1986) describe a world where activists discipline poor management through merger threats, proxy fights, and internal management shake-ups. Yet, despite the theoretical importance of activists, there is limited and mixed empirical support for their ability to create shareholder value.

One explanation for the limited success of shareholder activists is the free-rider problem these shareholders face: a shareholder attempting to invoke change must bear the full cost of the activism, while the benefit of the activism is divided among all the shareholders at the firm (Grossman and Hart, 1980). This paper examines the potential for activists to overcome this free-rider problem by coordinating their activities with other activist shareholders.

We posit two possible channels through which coordinated activism can mitigate firm-free rider problems - cost sharing and an increase in the probability of a successful campaign. Gantchev (2013) estimates that the cost of activism accounts for two thirds of the abnormal returns earned by the activist, and that the mean net activist return is close to zero.[1] Through coordination,

[1]The cost of activism includes time and effort to identify firms whose corporate strategy is conflicting share-

activists can potentially share these costs and thus increase their net activism returns.

Coordination among activists can also increase the probability their campaigns are successful. Consider the recent activist campaign at Sotheby's. In June 2013, Marcato Capital Management announced a 6.61 percent activist stake in the company. Two month later Daniel Loeb's Third Point LLC announced a 9.6 percent stake in the company. Loeb asked Sotheby's CEO and chairman William F. Ruprecht to step down and pressed for board seat representation.[2,3] In his SC-13D filings Loeb indicated his intention to coordinate with other activists, stating that he "may engage in a dialogue and other communications regarding the Issuer with other stockholders of the Issuer [...]" and that he "support[s] the Company placing a designee from another large shareholder on the Board." Sotheby's responded by instituting a poison pill with a 10 percent threshold, however the activists were able to accumulate a 19 percent ownership stake in Sotheby's stock, a level that gave them effective veto power over major corporate decisions without triggering the pill, because the stake was split among three different activists, none of whom exceeded the 10 percent ownership threshold. Through coordination, Loeb was able to negotiate an agreement with Sotheby's management to place three directors on its board.

holder value. Further costs include expenses which are connected to activists attempting to change actions at the firm, e.g., through negotiations with management and the Board of Directors, seeking board seat representation, and proxy fights (Admati, Pfleiderer, and Zechner, 1994). Another cost associated with exercising influence is the cost of foregoing diversification benefits which is available to passive, risk spreading investors (Cheffins and Armour, 2011).

[2]http://www.sec.gov/Archives/edgar/data/823094/000119312513388165/d605390dex993.htm

[3]At this point, Third Point was Sotheby's second largest shareholder after BlackRock Fund Advisors. Trian Fund Management LP also announced a 3 percent stake during the second quarter of 2013.

Consistent with coordination adding value at the target firm, past research suggests that coordination among non-activist shareholders can also improve merger outcomes (Huang, 2013) and can reduce the odds that a company defaults (Chakraborty and Gantchev (2013)).

The media, as well as previous research,[4] has provided anecdotal evidence that shareholder activists form "wolf packs" and target firms in mass. In survey evidence McCahery, Sautner, and Starks (2011) report that 59 percent of activists would consider coordinating their actions. In light of this evidence, the U.S. Security Exchange Commission (SEC) has acknowledged the importance of shareholder coordination in effective corporate governance, initiating reforms in 1992 and 2008 aimed at reducing frictions to coordination among shareholders.[5]

Coordination among activists may take direct or indirect forms. As observed in the Sotheby's campaign, shareholders may coordinate through direct communication with one another. Shareholder coordination may also take an indirect form. In a court ruling on the Sotheby's case, Vice Chancellor Donald Parsons highlighted the potential for activists to coordinate through conscious parallelism – defined as imitative activity between competitors that occurs without an actual agreement between the parties.[6] Consistent with indirect coordination among investors, Matvos and Ostrovsky (2010) provide

[4]See Opler and Sokobin (1995), Gillian and Starks (2007), Greenwood and Schor (2009), Briggs (2007), McCahery, Sautner, and Starks (2011).

[5]See SEC. 2008. Electronic Shareholder Forums: Amendment to Exchange Act Rule 14a-2 and New Exchange Act Rule 14a-17.
Retrieved from http://www.sec.gov/info/smallbus/secg/rules14a-2-17-secg.htm

[6]http://www.cov.com/files/Publication/1d9f001e-d341-42a6-96ea-16fd16f71480/Presentation/PublicationAttachment/358a2b1b-8754-44af-ab35-1dd3983abcd1/Conscious_Parallelism_May_Justify_A_Wolf_Pack_Pill.pdf

evidence that peer effects drive coordination among mutual funds in the proxy voting process. Alternately, if it is difficult for activists to scale up their position even though the campaign is worth undertaking, indirect coordination could take a certification form. Within this framework the second activist could provide a signal that the campaign is worth waging.[7]

Despite the rich anecdotal evidence of coordinated activism and its potential to add firm value, there has been little research that empirically addresses the phenomenon. Our paper seeks to fill this void by empirically examining coordinated activism using a unique and comprehensive dataset of shareholder activism events.

We collect our dataset using a web crawler algorithm which transforms shareholder activism events, described by SC-13D filings to the SEC, into database form.[8] Our dataset spans from 2000 to 2011 and contains over 4900 unique shareholder activist events. We define coordinated activism campaigns as instances where two or more activists simultaneously target the same firm.

Our first set of tests look broadly at coordinated activism to test if, in aggregate, it improves the success of activism campaigns. If so, we posit that the probability that an activist targets a firm will increase if another activist

[7]Shleifer and Vishny (1992) suggest a role for certification when the supply of arbitrage capital is inelastic.

[8]The current ownership disclosure rule by the SEC is divided in 5 sections. First, Schedule 13D has to be filed by anyone who acquires beneficial ownership of more than 5 percent. This Schedule has to be filed within 10 days of the acquisition. A more passive version of Schedule 13D is Schedule 13G, which has fewer requirements than Schedule 13D. A Schedule 13G may be filed if the person does not intend on changing or influencing the control of the issuer. If a 13G filer wants to change the purpose of his investment he has to refile a 13D. In addition to the 13D and 13G Schedules, Form 13F covers quarterly equity holdings of institutional investors with more than $100 million under management (13F filings are collected in Thompson Reuters Ownership Database (formerly the CDA/Spectrum database)). Further, insider ownership (e.g., directors, executives) is regulated in the Section 16 Ownership reports. Form N-CSR requires investment companies and trusts to file semiannual reports with the SEC disclosing security holdings and proxy voting policies. http://www.sec.gov/answers/sched13.htm

is already present at that firm, and that the announcement of coordinated activism will generate abnormal returns. Consistent with this hypothesis, we find that the probability that an activist targets a firm in a given month is approximately fifty percent higher if another activists is already targeting the firm and that this result is robust to controlling for firm and time specific factors. With respect to returns, we find that the announcement of coordinated activism generates a 5.43 percent abnormal return in the 21 day window around the SC-13D filing, and that the entire coordinated activism campaign produces approximately twice the return, 10 percent versus 5 percent, as non-coordinated activism campaigns.

Despite the rich anecdotal evidence of "wolf pack" activism, since we cannot directly observe coordination among activists, we are concerned that activists may simultaneously target the same firms for reasons other than coordination.[9] Specifically, we may be observing shareholder activists independently finding and pursuing profitable activism opportunities at the same firm.[10] To distinguish coordination from other potential explanations of simultaneous firm activism, we examine if the prevalence of coordinated activism campaigns increases as frictions to coordination among activists decreases. Marwell, Oliver, and Prahl (2002) demonstrate that the density and frequency of social ties within a group decreases frictions to coordination and increases its likelihood. We use the geographic proximity of activists to proxy for the social ties between them. Consistent with coordination increasing as the frictions to

[9]An alternate explanation for why activists may be targeting the same firm is that the second activist is free-riding on the first activist's success. We interpret the finding that the announcement of the second activist's arrival produces abnormal positive returns as evidence against this explanation.

[10]Our tests do address this concern by including firm specific control variables. However, there is an omitted variable concern that our controls are unable to completely control for the potential profitability of an activist campaign at the firm.

coordination decrease, we find geography proximate activists are more likely to coordinate. This result is robust to controlling for the location of the target and other firm specific factors.

Our last set of tests attempts to address why activists coordinate. Specifically, we focus on the heterogeneity of targets and activists to explore if the observed coordinated behavior of activists is a response to the free-rider problems faced by activists. If so, we should observe that coordinated activism is more prevalent where the free-rider problems activists face are larger. The framework developed by Grossman and Hart (1980) suggests that as the size of the target increases and as the size of the activist decreases it becomes more difficult for activists to overcome free-rider problems. Consistent with activist coordination being a response to a free-rider problem, we find that, although activists in general prefer to target small firms, coordinating activists prefer to target relatively larger firms. Next, we use Form 13F filings to measure the size of each activist and find that smaller activists are more likely to coordinate than larger activists.

The main contribution of this paper is to document the coordinated shareholder activism phenomenon. To our knowledge, this is the first paper to directly examine the prevalence and outcomes of coordinated shareholder activism. Opler and Sokobin (1995) analyze coordinated activism indirectly by examining outcomes at proposed targets of the Council of Institutional Investors (a group of public and private pension funds). They conclude that coordinated activism creates shareholder value, and suggest that the reason for the success is the coordinated behavior. Additionally, the potential for

activists to coordinate has been highlighted anecdotally (via the media and past research, see e.g., Cheffins and Armour (2011)), and with survey evidence (McCahery, Sautner, and Starks, 2011). Our paper also contributes to the broader investor coordination literature which has documented the potential for coordination among passive shareholders to improve merger outcomes (Huang, 2013) and reduce default probabilities (Chakraborty and Gantchev, 2013).

The results of our paper also complement the literature that has examined factors relating to the success of shareholder activism. Brav, Jiang, Partnoy, and Thomas (2008), Klein and Zur (2009) and Kim, Kim, and Kwon (2009) find evidence that shareholder activism can reduce agency costs and increase the target firm's wealth. In contrast to previous research that suggests that activists can be seen as new monitors, Greenwood and Schor (2009) find that target firm gains are limited to firms that are acquired after the activism event. They see the role of the activist as a merger and acquisition broker who matches target and acquiring firms, rather than as a monitor. Our paper contributes to this literature by exploring the impact of a new activism characteristic, coordination, on the gains of activism at the target firm. We find that coordinated activism campaigns are associated with larger returns than those earned by non-coordinated activism campaigns.

We also contribute to the literature which studies the impact of information networks on investor decisions. Past research suggests that information is transferred between investors through social networks (Shiller and Pound, 1989, Coval and Moskowitz (1999, 2001), Hong, Kubik, and Stein (2005),

Cohen, Frazzini, and Malloy (2008), Cohen and Schmidt (2009) and Calluzzo (2014)). Our finding that coordination is more likely among geographically proximate activists suggest that information networks impact activist positions as well. We also find that activists prefer to target geographically proximate firms. Although this result is not the focus of our paper, to our knowledge it is a new result which complements the finding that passive investors also prefer to invest in geographically proximate firms (Coval and Moskowitz, 1999; 2001, Huberman, 2001).

The remainder of this paper is organized as follows. Section 2 summarizes the data collection process used to generate the unique dataset utilized in this study, and provides descriptive statistics of the sample. In Section 3 we present the main results of the paper, analyses that examine the prevalence, mechanisms, reasons for and shareholder value added by coordinated activism campaigns. We conclude in Section 4.

2.2 Data and variables

2.2.1 Sample selection

Investigating coordinated shareholder activism requires collecting data which describes activist activity. In the United States, disclosure rules imposed by the SEC create transparency in the shareholder activist process.[11] Shareholder activists are required to disclose their activity to the SEC through a Schedule 13D (SC-13D) filing, referred to as the "beneficial ownership report." Except for special cases where an investor petitions to be considered passives and file a Schedule 13G in lieu of the Schedule 13D, an investor must file a

[11] See Williams (1999).

Schedule 13D when he owns more than 5 percent of a stock, and he must file the Schedule 13D within ten days of purchasing the stock.[12]

In this form, the beneficial owner, herein referred to as the "filer" or the "activist," is required to disclose their identity, the identity of the activism target, source of its funds, location, size of holdings, type of investor, and the purpose of the transaction. Any material changes in the facts contained in the schedule require a prompt amendment, filed in Schedule 13D/A.

Previous research on shareholder activism has partly been limited by the lack of a publically available and comprehensive dataset of SC-13D filings. This paper uses an automated technique known as web crawling[13] to create a unique database of all SC-13D and SC-13D/A filings. Two factors make this possible. First, through the EDGAR database, the SEC makes available to the public all SEC filings from 1994 to present. Second, the SC-13D filings adhere to a uniform format that, through the use of regular expression coding techniques, can be read electronically and converted into database form. The web crawling algorithm "reads through" 222,009 SC-13D and SC-13D/A forms filed with the SEC from 1994 to 2011. Specifically, for each filing the algorithm records the date of the filing, the filer's identity, source of funds, type of reporting person, holdings stake, purpose of transaction, the identity of the activism target, and the ZIP Codes of the activist's and the target's headquarters in database form. We remove duplicates and filings where an insider at the firm files a SC-13D to disclose an ownership stake, i.e., non-activism intentions. In some instances, a firm (or its employees) files under

[12]For more information see "Schedule 13D," http://www.sec.gov/answers/sched13.htm.
[13]See Engelberg and Sankaraguruswamy (2007) for detail.

different CIKs.[14] We manually check the database, and consolidate affiliated filers into a single CIK. This insures that we are not confusing coordinated activism with instances where one activist, through several affiliates, files multiple SC-13Ds at a target firm under different CIK numbers. We merge the activism database with Compustat and the Center for Research in Security Prices (CRSP) and delete all target firms which could not be matched at any point during our sample period.[15] We also collect the assets under management for each activist from the Form 13F filings. Form 13F is a quarterly report of equity holdings by institutional investors with investment discretion of at least $100 million. Form 13F filings are available from 2000 onwards at EDGAR.[16] Our final sample spans the years 2000 to 2011 and consists of 18,962 filings. In total we have 4,914 SC-13D filings and 14,048 SC-13D/A filings. To identify the purpose of the transaction we use a text mining algorithm to search Item 4 in the SC-13D form. The details of this algorithm are explained in Appendix A. Of the 4,914 SC13-D filings, we identify the purpose of 874 as being merger related and 1,620 as being governance (activism) related.

2.2.2 Variable definition

Our definition of coordinated shareholder activism is derived from the binary variable "Activist present." The entry date of an activist is the first SC-13D

[14]The Central Index Key (CIK) is used on the SEC's computer systems to identify corporations and individuals who have filed with the SEC.

[15]This means that we keep some observations in our sample even though they could not be matched to return or accounting data. This is necessary since we rely on the time series information of activism events of each firm to classify coordinator filings. The observations with missing data drop out when doing the regression analysis.

[16]We prefer collecting the assets under management from Form 13F directly instead of using the Thomson Reuter's database. In the SC-13D filings and in Form 13F, activists are classified through their CIK codes and names, the Thomson Reuter's database uses a different identifier which requires using a noisier fuzzy-logic name matching algorithm.

filing an activist files for a target company. The exit date is proxied by one year after the last SC-13D/A filing of an activist for that target company. After 12 months of no new filing, the activist is dropped from the sample. A filing is classified as a "Sole filing" if the activist present dummy is zero, meaning that no activist is currently targeting the firm. If an activist is already targeting the firm, a filing is classified as "Follower filing." A filing is defined as a "Coordinator filing" if the previous or the next filing for the target is within one year. A "Leader filing" is a filing which is followed by the SC-13D filing of another activist within the next year. We extract the asset base of the activist from the 13F Forms and introduce two size categories: 13F Activist and Non-13F Activist.

Stock price and return data stems from CRSP. The SC-13D filing announcement returns are calculated over -10 to 10, 30, 60, and 90 days windows using the Daniel, Grinblatt, Titman, and Wermers (1997)(DGTW) benchmarks.[17] If a firm drops out of the sample (e.g., is acquired or goes bankrupt), its abnormal returns until it exits the sample are included. All announcement returns are calculated from daily return data. Accounting data about the target firm is obtained from Compustat.

2.2.3 Descriptive statistics

Table 2.1 reports sample statistics for our database and our two subsamples. The first column of Panel A describes the database from which our final sample is drawn. Over the course of the sample there are 4,914 SC-13D filings,

[17] The DGTW benchmarks are available via http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm. See also Daniel, Grinblatt, Titman, and Wermers (1997).

2,959 unique activists, and 2,816 target firms. The second and third column report overviews of the sample after matching it to return and accounting data. The second column includes merger filings, the third column excludes those. Panel B of Table 2.1 reports the average number of filings per year. Each year, there is an average of 1,580 SC-13D and SC-13D/A filings, ranging from 986 to 2,217. The trend has been an increasing number of filings over time. Panel C of Table 2.1 reports the announcement CARs for all first time pairings of activist and targets.[18] The filings are divided into the three subsets; merger, governance (activism), and investment only related filings. The mean short term announcement returns for merger related filings are highest at 15 percent (median 11 percent), followed by the governance (activism) related filings which have a mean short term abnormal return of 6 percent (median 2 percent). Investment related filings are associated with short term announcement returns of 4 percent (median 2 percent).

The summary statistics presented in Table 2.1 show that filings related to merger activities are different from governance activism filings. In untabulated results we also confirm that merger filings are mostly followed by other merger related filings. For most of our analyses we remove the merger filings from our sample to avoid confusing toe holds and bidding fights with governance activism and coordinated activism. Our results are robust to including these filings.

Table 2.2 presents summary statistics of activism activity by activist. On

[18]The magnitude of abnormal returns we find is consistent with the previous literature. Analyzing 1059 SC-13D filings by 236 hedge funds, Brav, Jiang, Partnoy, and Thomas (2008), estimate a large positive significant announcement return of the window [-20,+20] of 7 percent to 8 percent. Comparing hedge fund targets to other entrepreneurial targets, Klein and Zur (2009) find a [-30,+30] SC-13D announcement returns of 10.2 percent for hedge fund targets and 5.2 percent for other entrepreneurial investors.

average, each activist files 1.60 SC-13D forms. In addition to each filer perusing activism at multiple target firms, and each activist filing multiple SC-13D's for each target firm, multiple individual entities often file their SC-13D in the same form. An example of this would be Strome Investment Management L.P.'s SC-13D filing in Bank Plus Corporation on June 4, 2001. This filing lists seven different reporting entities as activists: Strome Investment Management L.P., SSCO, Inc., Mark E. Strome, Strome Partners L.P., Strome Offshore Limited, Strome Hedgecap Fund, L.P., Strome Hedgecap Limited. In this case, the different activists filed under the same SEC CIK code, 0000936711, which is associated with Strome Investment Management L.P. Our analysis identifies filers by SEC codes and thus treats the different entities associated with Strome Investment Management L.P. as one activist. As described earlier, in some instances affiliates file under different CIK codes. To avoid confusing affiliated filings with coordinated activism we hand check the sample and assign affiliated activists the same CIK code.

Panel B of Table 2.2 reports summary statistics for the publically traded firms that are targets of shareholder activism. All statistics are reported for the quarter prior to the activist's initial SC-13D filing. The Market value deciles and the market-to-book ratio quintiles displayed are calculated with respect to the whole Compustat universe in the quarter before the filing. The data suggests that the target firms are similar to the average Compustat firm in size, measured by the market value of their equity, and firm value (market-to-book ratio). Panel C reports on our set of control variables which are based on Klein and Zur (2009). Return on asset is calculated as EBIT over total assets and Z-score denotes Altman's (1968) Z-score.

In Panel D of Table 2.2 we report summary statistics about Coordinators and Non-Coordinators. There is an average of 140 Coordinator filings per year where a Coordinator is defined as filing where the previous or the next filing is within 365 days. There are on average 142 filings from sole activist and 135 Follower filings per year. 13F activist file on average 68 filings per year. Panel E tabulates the geographic location of activists and target firms by their first digit ZIP code region. The states corresponding to each first digit ZIP code region is listed in Appendix B.

2.3 Results

2.3.1 Does coordination improve activist success?

In this section we broadly examine the success of coordinating activists. We first focus on the probability an activist targets a firm. If coordination improves the success of activism campaigns - either through cost sharing (Gantchev, 2013) or greater probabilities of success (Chakraborty and Gantchev, 2013, Huang, 2013), we expect to observe that, all else equal, activists will prefer to target firms where other activists are already present (with whom they can coordinate) compared to firms where no other activists are present. Next, we focus on the abnormal returns associated with activism announcements. If coordination improves the success of activism campaigns, we expect to observe that the announcement of a coordinating activist will produce abnormal returns at the target firm.

2.3.1.1 Do activists prefer to coordinate?

The presence of an activist at a firm may attract, repel or have no effect on the choice of other activists to target the firm. Although anecdotal ev-

idence points to instances where multiple activists coordinate, it is unclear how widespread this behavior is. Furthermore, the presence of an activist deterring another activist from targeting a firm is difficult to observe, so a bias in the anecdotal evidence towards activist coordinating with, rather than repelling, other activists is unsurprising and may not accurately describe the phenomenon.

In this section we quantify the tendency of activists to coordinate by examining the effect the presence of an activists at a firm has on the probability that an additional activist targets the firm. If coordination improves the success of activism campaigns, we expect to observe that the presence of an activist at a firm increases the probability another activist targets the firm. To test this, we use a panel dataset that includes an observation for every firm in the Compustat database each month. The dependent variable, New activist, is a dummy variable that identifies if a new activist targets the firm in month $t + 1$ and the independent variable of interest is a dummy variable, Activist present, which indicates if at least one activist is targeting the firm in month t.

The univariate results presented in Panel A of Table 2.3 indicate that the probability of an activist targeting a firm in a given month is approximately fifty percent higher, 1.24 percent compared to 0.79 percent, when another activist is already targeting the firm.

Although this result is consistent with coordination, there is concern that other factors drive the reported univariate results. After all the goal of a shareholder activist is to maximize investment returns. Some firms may be

more attractive targets for activism than others, and our result may be driven by activists independently deciding to target the same firm based upon firm specific characteristics which make it a profitable target of activism. To isolate the effect of the activist's presence from confounding factors that may make a firm a more attractive target, we employ a logistic regression that controls for various firm characteristics. Our set of control variables is based on Klein and Zur (2009). We control for return on assets, Altman's Z-score, capital expenditure, dividends per share, total debt, cash, the prior year return to shareholders, and firm size as measured by the logarithm of market value. Additionally, we include year and industry fixed-effects as specified. If the tendency for activists to target the same firm is driven by firm specific factors then the addition of these firm specific factors should make the coefficient on the Activist present variable statistically insignificant.

The results presented in Panel B of Table 2.3 suggest that firm-specific factors do play a role in the tendency of activists to target a firm. Activists prefer to target small firms with negative return momentum. In addition, activists target firms with lower Altman's Z-score, indicating they prefer to target firms with lower financial health. However, the fact that the coefficient on the activist present variable remains positive and statistically significant suggests that activists do prefer to target firms where other activists are present, even after controlling for firm specific factors.

2.3.1.2 Do coordinating activists generate abnormal returns?

Next, we examine the abnormal returns associated with the announcement of coordinating activists. If activist coordination improves the success of ac-

tivism campaigns, then their announcement should be associated with positive abnormal returns.

Table 2.4 presents results which compare the abnormal returns of sole activists who target firms where no other activists are present to those earned by coordinating activists who target firms where other activists are present. The dependent variable, CAR(-10, *),[19] measures the activism CAR around the specified window.

The results suggest that the announcement of coordinating activists generates significantly positive abnormal returns at the firm. The magnitude of these returns range from 4.6 percent to 5.6 percent, and the lack of a price reversal over longer event windows suggests that the returns are not driven by the short term buying pressure of the activist. The coordinated activism announcement return is significantly larger than the sole activism announcement return over the (-10, +10) event window, however, over longer event windows the difference is statistically insignificant.

This result implies that shareholders at the target firm receive a "double benefit" from coordinating activists, as they capture abnormal returns from both the first and second activist. To test this implication more directly, Table 2.5 presents OLS regressions where the dependent variable is the activist's announcement CAR(-10,*), and the independent variable is a dummy variable Leader(0,*) which indicates if another activist targets the firm from (0,*).

[19]In untabulated robustness tests we change the benchmark for the abnormal returns from the DGTW benchmarks to the CRSP value weighted index. Our results remain robust to this change. Given the long period of the abnormal return windows, we find the DGTW benchmark more appropriate. To control for extreme observation we run robustness test with winsorized return variables. Our results are robust for winsorizing the abnormal returns at the 99th percentile.

Consistent with a double benefit from coordinated activism, we find the coefficient on the Leader(0,*) variable is positive and statistically significant in all specifications. The results suggest that having two activists coordinate at a firm produces abnormal returns of between 9 and 13 percent.

2.3.2 Detecting coordination among activists

Despite anecdotal evidence that activists coordinate their actions and our empirical evidence presented thus far, we do acknowledge that the data contained in the SC-13D filings is not granular enough to provide direct evidence that activists are in fact working together, and that alternate, non-coordination, explanations may drive our observed results.

For example, one potential explanation is that activists target the same firm because of a herding phenomenon similar to that observed in passive investment, where follower activists free-ride off of the first activist who targets a firm (Badrinath and Wahal, 2002, Nofsinger and Sias, 2002, Sias, 2004). However, this explanation is inconsistent with the result presented that the announcement of a second activist at a firm generates economically and statistically significant abnormal returns at the target firm. If follower activists are free-riding off of the work of the original activist, we should not see positive long-term abnormal returns associated with their activism.

Another alternate explanation for our findings is that shareholder activists independently find and pursue profitable activism opportunities at the same firm. Although we do take steps to control for firm specific factors that drive shareholder activism, we may not be capturing all of the relevant variables and our analysis may be subject to omitted variable bias.

To address these concerns, in this section we present tests aimed at more directly detecting coordination among activists. Specifically, if shareholder activists who simultaneously target a firm, do so with the intention of coordinating with the other activists, we should observe that the probability of two activists targeting the same firm is directly related to the ease with which the activists can coordinate. Sociology literature (Marwell, Oliver, and Prahl, 1985; 2002, Oliver, Marvell, and Teixeira, 1985, Hardin, 1982) provides evidence that social ties among parties improve the probability and success of coordination among those parties, i.e., it is easier to work with whom you know. We expect this finding to translate to coordination among shareholder activists, as socially tied activists will face few frictions to coordination. Consistent with past finance research that suggests ties among geographically proximate investors (Coval and Moskowitz, 1999; 2001, Hong, Kubik, and Stein, 2005), we use geographic proximity to proxy for social ties among shareholder activists.[20]

Activists are required to disclose their business address in their SC-13D filing. We use the first digit of an activist's ZIP code to classify the activist's location into one of ten geographic regions.[21] We then use a logistic regression model to examine if activists are more likely to coordinate with activists from the same geographic region. The dependent variable, ZIP *, is a dummy variable indicating the geographic region of the follower activist, and the independent

[20]This approach is consistent with existing literature. Huang (2013) uses geographic proximity as to proxy for the ability of passive shareholders to coordinate.

[21]The first digit of a ZIP code correspond to a geographic region. For example, 0 corresponds to New England, and 9 responds to the West Cost, Alaska and Hawaii. There are a few exceptions to this. New Jersey has a zip code beginning with zero but is not in New England. We manually reassign it to region 1 which includes Delaware, New York and Pennsylvania. The other exception is U.S. territories which are not states such as Puerto Rico and Guam. We remove these observations from the analysis. For more information on the first digit ZIP Code region see Appendix B.

variable in each regression is the activist's first digit ZIP code *, which is a dummy variable indicating the geographic region of the initial activist.

Panel A of Table 2.6 presents ten specifications of the model, one for each geographic region. The independent variable ZIP * can be interpreted as the probability that a leader activist from the specified region has a follower from that region, compared to activists not from that region. A consistent result emerges, the coefficient on the ZIP * variable is positive and statistically significant across all specifications. Consistent with social ties lowering the friction to coordination, activists are more likely to coordinate with activists from the same geographic region.

There is some concern that variables not included in the previous analysis may drive the observed result. For example, activists may tend to target firms in their geographic region and this, not the location of the coordinating activist, may drive the observed result. To address this concern, we introduce a variable, target firm first digit ZIP code *, which identifies if the target firm is in the specified geographic region. We also include the control variables used in our previous models to address concerns that activists from the same region prefer to target firms with certain characteristics.

In Panel B of Table 2.6 we present results after adding our firm specific controls. The coefficient on the ZIP * variable remains positive and statistically significant across all specifications. Furthermore, the coefficient on the Target ZIP * is also positive and statistically significant across almost all specification with exception to regions three and four. To our knowledge this is the first paper which has looked at the role geography plays in shareholder activism.

Consistent with research on passive investments (Huberman, 2001, Coval and Moskowitz, 1999; 2001) this result suggests that activist investors prefer to target geographically proximate firms.

2.3.3 Is coordinated activism a response to free-rider problems faced by activists?

In this section we address why shareholder activists coordinate. Specifically, we investigate if coordination is a response to free-rider problems faced by the activists. If so, we expect to observe that coordinated activism campaigns are more likely to occur where the free-rider problems they face are the greatest. Grossman and Hart (1980) posit that monitoring of management is effective only when a party becomes large enough to internalize the externalities of collective action. Within this framework, free-rider problems will be greatest where it is most difficult for the activist to accumulate large stakes: when the target firm is large and when the activist's resources are small. Thus, if activists coordinate as a response to free-rider problems, we expect that coordination will be more prevalent in these situations.

2.3.3.1 Do activists tend to coordinate at large firms?

To test if coordinated activists are more likely to target large firms we build on the regression model presented in Table 2.3 which examined how the presence of an activist at a firm influences the probability an additional activist targets that firm in a given month. The unit of observation is again firm-month and the dependent variable, New activist, is a dummy variable which indicates if a new activist targets the firm in month $t + 1$. The independent variables of interest are Activist present, Ln(Market value) and Activist

present * Ln(Market value) which is the interaction of the two variables. If coordination is more prevalent at larger firms, as the free-rider explanation for activism suggests, we expect to observe that the coefficient on the Activist present * Ln(Market value) interaction variable will be positive.

Column 1 of Table 2.7 presents results of the model when no other control variables are included. Consistent with coordinated activism as a response to free-rider problems, we observe the coefficient on the Activists present * Ln(Market value) variable is positive and statistically significant. While activists in general prefer to target small firms, as seen in the significantly negative coefficient on the Ln(Market value) variable, coordinating activists prefer relatively larger firms. For robustness, Column 2 adds our earlier set of control variables to the model, and Column 3 includes the interaction of these control variables with the Activist present variable. Across both specifications, the coefficient on the Activists present * Ln(Market value) variable remains positive and statistically significant. As this is the first paper to empirically examine coordination activism, the coefficient on the other interactive variables are of some interest. The lack of significance on the other interactive variables suggests that other than firm size, there is little difference in the firm specific factors that determine coordinated activism targets versus non-coordinated targets.

2.3.3.2 Are small activists more likely to coordinate?

Next we focus on the impact activist assets have on the tendency of activists to coordinate. We use SEC Form 13F to measure the size of the asset base the activist manages. The form, which contains holdings information, must

be filed by all investment managers with over $100 million in assets, and the total value of assets held by the manager is included. If an investment manager has under $100 million in assets they are not required to file Form 13F. Using this information, we measure investor wealth in two ways. First we introduce a dummy variable, 13F Activist, which indicates if an investor files a 13F and thus has over $100 million in assets.[22] Next, we focus on the subset of activists who file a Form 13F, and measure their size using the continuous variable ln(AUM) which is the log of their assets under management as reported in their most recent 13F filing. If activists coordinate as a response to free-rider problems, we should observe that activists with more wealth, i.e., 13F Activist and those with high assets are less likely to pursue coordinated activism campaigns compared to smaller activists.

Table 2.8 presents the results of logistic regressions where the dependent variable is Coordinator, a dummy variable which indicates if the activist was part of a coordinated activism campaign. In the first column of Table 2.7 the independent variable of interest is the 13F Activist dummy. Consistent with our hypothesis that activists with more assets will have less incentives to pursue coordinated activism campaigns, we find that the coefficient on the 13F Activist dummy is negative and statistically significant. In the second column of Table 2.8, we restrict our sample to 13F firms for whom we can observe asset size using the ln(AUM) variable. Consistent with the first specification and with the hypothesis that activist coordination is a response to free rider problems, we find that the coefficient on the ln(AUM) variable is significantly

[22]Non-investment manager activists are not required to file a 13F, even if they possess over $100 million in assets. For this reason we remove non-investment managers from this analysis.

negative. Large activists, who face smaller free-rider problems, are less likely to coordinate than smaller activists.

There are two ways through which activists coordinate, as a leading activist, defined as another activist targeting the same firm within a year of the initial activist's arrival, or as a follower activist, defined as when an activist targets a firm that another activist is already targeting.

In Table 2.9 we introduce dependent variables which identify if an activist is a leader or a follower, we show that this result is driven both by the fact that large activists are less likely to target firms where other activists are present, and are also less likely to attract other activists once their activism campaign is underway.

2.4 Conclusions

The media, courts and regulators have given much attention to "wolf pack" activism. However, with only anecdotal evidence, these parties have a limited view of the interaction between shareholder activists. The results of this paper shed light on how investors interact by empirically addressing the prevalence, mechanisms, reasons for and shareholder value added by the phenomenon.

Using a unique and comprehensive dataset of activism events, we present evidence that suggests shareholder activists coordinate, that coordination improves the success of activism campaigns, and that this behavior can help mitigate the free-rider problems activists face. Our results show that the probability of an activist targeting a firm increases if another activist is already targeting the firm and that the announcement return of coordinated

activism is economically and statistically significant. The entire coordinated activism campaign yields on average an abnormal return of approximately 10 percent.

There are concerns that alternate, non-coordination, explanations drive activists to target the same firm. To distinguish coordination from these competing explanations, we present tests which show that activists are more likely to work with geographically proximate activists, with whom frictions of coordination are lower.

Last, we address why activists coordinate. We find that coordinated campaigns are more likely to occur where the free-rider problems faced by activists are larger, i.e., in large firms and among small activists, suggesting that coordinated activism is a response by activists to free-rider problems associated with their campaigns.

Appendix A

Text mining algorithm
To identify the intent of each activism event we use a text mining algorithm to search Item 4 in the SC-13D filing. We divide our sample into three subgroups; merger intentions, governance activism intentions and pure investment intention. If a filing is classified into two categories, the order of classification is merger beats activism beats investment. If a filing for an activist-target pair is put into the activism category, all subsequent filings of this pair are defined to be in the same subset. The same is done for the merger category overwriting the activism definition if necessary. By defining intent this way, a change from activism to merger is possible; the other way around is not. Hand checking the classifications indicates that the false positive rate for merger classification is about 6 percent.

Text mining - example of dictionary

Merger	**Governance Activism**
tender agreement	discussions with management
proposed merger of the issuer	discussions with senior
to effect a change in control of the issuer	may discuss such matters with management
merger transaction	will routinely monitor the Issuer
become a wholly owned subsidiary	influence the management or strategic direction
all of the shares of common stock of issuer	recommending certain actions
partnership agreement	discussions with the issuer
100% ownership interest	seeking board representation
the proposed merger	meet with management
became a wholly-owned subsidiary	meet with the board
acquiring all of the	seek to influence management
acquire all outstanding	discussions with the management
reverse merger	consulting and advisory services
purpose of the merger	discuss these matters with
becoming a wholly- owned subsidiary	voting agreement
effectuate the merger	be appointed as members of the board
as a tender offeror	meet with
complete the merger	has sought representation on the
to effect a change in control of the company	commercialization agreement
acquire control of the issuer	delivered a letter to the company
to obtain 100% ownership of	discussions with the company's management
tender their common shares in favour of the offer	restructure the board of directors
the offeror currently intends to pursue	to elect a director
controlling interest in the company	changes to the company's board
in a merger involving the issuer	to remove
obtain a controlling equity interest	sent a letter to
as a result of the merger of	one nominee to the board of directors
to effect a change of control of the company	will change the business of the issuer
with the intent to merge it	directing the business and affairs of the issuer
to effect a change of control of the company	by letter to the issuer
with the intent to merge it	seek to increase their influence
change in control	...
expectation of a proposed merger	
expectation of a merger	
obtain a controlling interest	
acquired substantially all of the	
tender offer made	
vote to merge into	
are in favor of the acquisition	
...	

Appendix B

ZIP code classification
We use the first digit of the activists' and the targets' Headquarters ZIP code to identify their geographic region. We change the classification from New Jersey from the first digit ZIP code 0 to 1 in order to group it together with the Northeast region rather than the New England region.

First digit ZIP code	States
0	Connecticut, Massachusetts, Maine, New Hampshire, Rhode Island, Vermont
1	Delaware, New York, Pennsylvania, New Jersey
2	District of Columbia, Maryland, North Carolina, South Carolina, Virginia, West Virginia
3	Alabama, Florida, Georgia, Mississippi, Tennessee
4	Indiana, Kentucky, Michigan, Ohio
5	Iowa, Minnesota, Montana, North Dakota, South Dakota, Wisconsin
6	Illinois, Kansas, Missouri, Nebraska
7	Arkansas, Louisiana, Oklahoma, Texas
8	Arizona, Colorado, Idaho, New Mexico, Nevada, Utah, Wyoming
9	Alaska, California, Hawaii, Oregon, Washington

Table 2.1: Sample overview
The table presents summary statistics for our sample of SC13-D filings. Panel A describes the database and our final sample. In Panel B the average number of filings per year is tabulated. More information about the purpose of transaction can be found in Appendix A. Cumulative abnormal returns as presented in Panel C are calculated with respect to the Daniel, Grinblatt, Titman, and Wermers (1997) benchmarks.

Panel A:		Data with return & accounting information	
Overview	Sample	with merger	without merger
Data range	2000-2011	2000-2011	2000-2011
SC-13D:	4914	3271	2608
SC-13D/A:	14048	-	-
Number of activist	2959	2167	1629
Number of targets	2816	2317	1928

Panel B:						
Filings per year	Mean	Median	St. Dev	Min	Max	N
All Filings	1580.17	1530	414.90	986	2217	12
SC-13D	409.50	406	141.38	222	683	12
SC-13D/A	1170.67	1068	304.54	764	1728	12
Merger (M)	55.25	53	21.63	26	105	12
Governance activism (G)	78.50	73	25.98	45	126	12
Investment (P)	133.67	130	53.2	73	236	12

Panel C:						
CARs (first activist-target filings)	Mean	Median	St. Dev	Min	Max	N
Merger (M)						
CAR(-10,10)	0.15	0.11	0.27	-1.03	1.70	663
CAR(-10,30)	0.15	0.12	0.29	-1.20	1.82	663
CAR(-10,60)	0.15	0.12	0.32	-1.36	2.20	663
CAR(-10,90)	0.14	0.12	0.35	-1.63	1.82	663
Activism (G)						
CAR(-10,10)	0.06	0.02	0.31	-0.93	5.48	942
CAR(-10,30)	0.06	0.04	0.38	-1.71	5.42	942
CAR(-10,60)	0.07	0.04	0.43	-3.09	5.41	942
CAR(-10,90)	0.06	0.03	0.47	-2.45	5.23	942
Investment (P)						
CAR(-10,10)	0.04	0.02	0.22	-1.60	2.73	1604
CAR(-10,30)	0.05	0.02	0.29	-1.60	2.57	1604
CAR(-10,60)	0.05	0.02	0.36	-1.60	4.22	1604
CAR(-10,90)	0.05	0.01	0.41	-2.01	4.36	1604

Table 2.2: Summary statistics
The table presents summary statistics for our sample of SC13-D filings. Panel A reports statistics on the filer level. Panel B and C report descriptive statistics for all financial variables for the target firms used in this study. The firm size (measured in terms of market value) deciles and the market-to-book quintiles are relative to the whole Compustat universe in the quarter before the filing. Return on assets is calculated as EBIT over total assets. Z-score denotes Altman's (1968) Z-score. Panel D reports the number of filings from coordinating, sole, follower, and 13F activists per year. All variables are defined in Section 2.2 of the main text. Panel E tabulates the filings by first digit ZIP Code on the activist and target level.

Panel A: Filer level data	Mean	Median	St. Dev	Min	Max	N
SC-13D filings per activist	1.60	1	5.52	1	211	1629
Activist per target	1.35	1	0.66	1	7	1928
ln(AUM)	14.55	14.55	1.82	8.97	18.55	1111

Panel B: Target overview	Mean	Median	St. Dev	Min	Max	N
Size decile (Market value)	5.25	5.00	2.39	1	10	2560
Market-to-book quintile	2.78	3.00	1.32	1	5	2447
Prior 3 month return	0.02	-0.01	0.55	-0.90	19.62	2601
Prior year return to shareholders	0.08	-0.10	1.64	-0.98	44.62	2589

Panel C: Target overview	Mean	Median	St. Dev	Min	Max	N
Return on assets	-0.01	0.06	0.36	-7.93	1.51	2466
Z-score	2.94	2.52	11.51	-145.61	308.40	2118
Capital expenditures/assets	0.05	0.03	0.07	-0.15	0.62	2364
Dividends/share	0.14	0.00	0.56	0	17	2466
Total debt/assets	0.22	0.17	0.23	0	4.23	2466
Cash + short-term investment/assets	0.20	0.10	0.23	0.00	1	2475
ln(Market value)	4.76	4.64	1.82	-1.46	11.86	2560

Panel D: Filings per year	Mean	Median	St. Dev	Min	Max	N
Coordinating activist	140.18	126	42.66	95	211	11
Sole activist	142.09	139	69.13	72	317	11
Follower	135.73	124	41.24	92	204	11
13F Activist	68.73	53	30.03	38	121	11

Panel E:

Activist		Target firm	
First-digit ZIP	Number of filings	First-digit ZIP	Number of filings
0	121	0	152
1	616	1	267
2	73	2	94
3	87	3	132
4	52	4	99
5	33	5	73
6	115	6	109
7	96	7	137
8	61	8	120
9	288	9	359

Table 2.3: Do activists prefer to target firms where they can coordinate?
Panel A presents the mean value of a new activist targeting the firm conditional on whether an activist is already targeting the firm or not. Panel B reports results from logistic regressions of an indicator variable taking the value of one if a new activist files a SC-13D in a given month. Activist present is a binary variable which indicates the presence of another activist in the target firm in the month of the filing. The set of control variables is based on Klein and Zur (2009). Column 1 includes year fixed-effects, Column 2 includes year and industry (of target firm) fixed-effects. p-values are shown in parenthesis with ***, ** and * indicating a statistical significance level of 1%, 5% and 10% respectively.

Panel A: New activist	Mean		Obs.	Std. Err.	[95% Conf.	Interval]
0	0.008		233825	0.000	0.008	0.008
1	0.012		20814	0.001	0.011	0.014
Difference	-0.005	***				
p-value t-test	0.000					

Panel B: Dependent Variable:	New Activist (1)		New Activist (2)	
Constant	-3.698	***	-3.785	***
	(0.000)		(0.000)	
Activist present	0.275	***	0.262	***
	(0.000)		(0.000)	
Return on assets	0.022		0.002	
	(0.750)		(0.982)	
Z-score	-0.008	***	-0.008	***
	(0.002)		(0.002)	
Capital expenditures/assets	-0.083		-0.314	
	(0.798)		(0.391)	
Dividends/share	0.019		0.023	
	(0.415)		(0.323)	
Total debt/assets	0.000		-0.010	
	(0.999)		(0.837)	
Cash + short-term investment/assets	-0.052		0.014	
	(0.628)		(0.901)	
Prior year return to shareholders	-0.133	***	-0.130	***
	(0.000)		(0.000)	
ln(Market value)	-0.160	***	-0.159	***
	(0.000)		(0.000)	
Year fixed-effects	Yes		Yes	
Industry fixed-effects	No		Yes	
Obs.	254639		254639	
Pseudo r-squared	0.021		0.022	

Table 2.4: The announcement returns of sole activist vs. coordinating activist
The table reports mean abnormal returns surrounding the initial SC-13D filing of sole activists and coordinating activists. Cumulative abnormal returns are calculated with respect to the Daniel, Grinblatt, Titman, and Wermers (1997) benchmarks over the -10 to 10, 30, 60 and 90 days intervals surrounding the SC-13D filing. p-values are shown in parenthesis with ***, ** and * indicating a statistical significance level of 1%, 5% and 10% respectively.

	Mean abnormal return surrounding the initial Schedule 13D filing			
	CAR(-10,10)	CAR(-10,30)	CAR(-10,60)	CAR(-10,90)
	(1)	(2)	(3)	(4)
(1) Sole activist	0.036 ***	0.049 ***	0.050 ***	0.058 ***
	(0.000)	(0.000)	(0.000)	(0.000)
(2) Coordinating activist	0.054 ***	0.056 ***	0.054 ***	0.046 ***
	(0.000)	(0.000)	(0.000)	(0.000)
Difference (1)-(2)	-0.018 *	-0.006	-0.004	0.012
	(0.078)	(0.613)	(0.783)	(0.473)
Obs.	2608	2608	2608	2608

Table 2.5: The double return of coordinated activism campaigns
The table reports results from OLS regressions comparing the abnormal announcement returns of Leader(0,*) filings. A Leader(0,*) filing is a filing which generates at least one follower filing in the * days after the initial filing. Cumulative abnormal returns are calculated with respect to the Daniel, Grinblatt, Titman, and Wermers (1997) benchmarks over the -10 to 30, 60 and 90 days intervals surrounding the SC-13D filing. p-values are shown in parenthesis with ***, ** and * indicating a statistical significance level of 1%, 5% and 10% respectively.

Dependent variable:	Abnormal return surrounding the initial Schedule 13D filing		
	CAR(-10,30)	CAR(-10,60)	CAR(-10,90)
	(1)	(2)	(3)
Constant	0.049 ***	0.044 ***	0.047 ***
	(0.000)	(0.000)	(0.000)
Leader (0,30)	0.064 **		
	(0.016)		
Leader (0,60)		0.083 ***	
		(0.002)	
Leader (0,90)			0.046 *
			(0.097)
Obs.	2608	2608	2608
R-squared	0.002	0.004	0.001

Table 2.6: Detecting Coordination: Is coordinated activism more prevalent when the frictions to coordination are lower?
The table reports results of logistic regressions of an indicator variable which takes the value of one if a given filing generates a follower filing in the indicated first digit ZIP code region. The independent variable of interest is the activist first digit ZIP code. The * denotes the same first digit ZIP code as the dependent variable in the respective column. The regressions are restricted to filings which generate at least one follower filing in the year after the initial filing. Panel B adds the set of control variables and an indicator variable for the target firm's first digit ZIP code. Column 5 of Panel B omits the dummy for the target firm's headquarters because all 10 filings with target firms located in ZIP code area 5 do not generate a follower filling in this area and hence predict perfectly the dependent variable. All regressions include year fixed-effects. p-values are shown in parenthesis with ***, ** and * indicating a statistical significance level of 1%, 5% and 10% respectively.

Panel A:										
Dependent variable:	Indicator variable if filing generates a follower filing in the indicated first digit of the ZIP-code									
	ZIP 0	ZIP 1	ZIP 2	ZIP 3	ZIP 4	ZIP 5	ZIP 6	ZIP 7	ZIP 8	ZIP 9
Constant	-2.560 ***	-0.709 ***	-2.540 ***	-2.493 ***	-2.709 ***	-3.642 ***	-3.206 ***	-2.118 ***	-3.055 ***	-1.520 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Activist first digit ZIP code *	1.114 ***	0.756 ***	2.423 ***	1.290 ***	2.857 ***	2.090 *	2.512 ***	1.756 ***	2.120 ***	1.318 ***
	(0.003)	(0.000)	(0.000)	(0.005)	(0.000)	(0.068)	(0.000)	(0.000)	(0.003)	(0.000)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1050	1050	1050	1008	956	791	1050	1050	1008	1050
Pseudo r-squared	0.028	0.019	0.082	0.036	0.061	0.053	0.095	0.048	0.056	0.033

Panel B:										
Dependent variable:	Indicator variable if filing generates a follower filing in the indicated first digit of the ZIP-code									
	ZIP 0	ZIP 1	ZIP 2	ZIP 3	ZIP 4	ZIP 5	ZIP 6	ZIP 7	ZIP 8	ZIP 9
Constant	-2.443 ***	-0.729 **	-1.410 *	-1.611 ***	-1.617	-2.340 *	-2.826 ***	-2.105 ***	-3.521 ***	-3.015 ***
	(0.000)	(0.035)	(0.084)	(0.005)	(0.108)	(0.050)	(0.000)	(0.000)	(0.000)	(0.000)
Activist first digit ZIP code *	1.127 **	0.531 **	2.258 ***	1.531 **	3.274 **	4.105 **	2.633 ***	1.409 **	3.183 **	1.078 ***
	(0.015)	(0.037)	(0.007)	(0.021)	(0.034)	(0.015)	(0.000)	(0.014)	(0.021)	(0.002)
Target firm first digit ZIP code *	0.713	1.108 ***	2.048 **	0.545	1.553		3.292 ***	1.556 ***	1.790 **	0.942 ***
	(0.154)	(0.002)	(0.043)	(0.444)	(0.126)		(0.000)	(0.005)	(0.010)	(0.006)
Return on assets	0.330	-0.114	1.286	-0.072	4.898 **	1.049	0.838	2.178 ***	1.547	-0.606 ***
	(0.446)	(0.576)	(0.209)	(0.851)	(0.016)	(0.466)	(0.202)	(0.008)	(0.152)	(0.006)
Z-score	-0.030	0.002	-0.055	-0.001	0.024	-0.005	0.009	-0.019	0.005	0.007
	(0.132)	(0.717)	(0.174)	(0.938)	(0.172)	(0.924)	(0.297)	(0.576)	(0.709)	(0.262)
Capital expenditures/assets	-6.008 **	-2.836 **	2.338	1.625	-23.658 **	9.209 ***	0.518	3.014 **	1.803	1.609
	(0.036)	(0.028)	(0.322)	(0.301)	(0.015)	(0.008)	(0.834)	(0.040)	(0.380)	(0.204)
Dividends/share	-0.047	0.041	-1.072	-0.020	-1.494	-1.114	-1.037	-1.195	-0.648	-1.009
	(0.743)	(0.576)	(0.487)	(0.871)	(0.455)	(0.733)	(0.464)	(0.264)	(0.674)	(0.144)
Total debt / assets	0.757 *	0.336	-2.631 **	-1.589 **	-1.265	-1.691	0.254	1.236 **	1.778 ***	0.237
	(0.064)	(0.270)	(0.036)	(0.022)	(0.214)	(0.352)	(0.696)	(0.014)	(0.004)	(0.542)
Cash + short-term investment/	0.384	-0.195	-1.647	-2.166 **	-1.608	2.653	0.483	0.998	-0.937	0.330
assets	(0.527)	(0.627)	(0.216)	(0.013)	(0.276)	(0.163)	(0.578)	(0.234)	(0.523)	(0.502)
Prior year return to shareholders	0.046	-0.403 ***	0.119	-0.023	-0.332	-0.206	0.173	0.277	0.219	0.058
	(0.809)	(0.004)	(0.690)	(0.910)	(0.469)	(0.711)	(0.524)	(0.158)	(0.479)	(0.718)
ln(Market value)	0.039	0.041	-0.249	-0.056	-0.325 *	-0.426	-0.252 *	-0.299 ***	-0.167	0.165 **
	(0.637)	(0.461)	(0.113)	(0.567)	(0.084)	(0.143)	(0.056)	(0.003)	(0.256)	(0.031)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	687	687	641	660	631	457	603	687	586	687
Pseudo r-squared	0.070	0.056	0.126	0.078	0.201	0.172	0.183	0.157	0.201	0.089

Table 2.7: Is coordinated activism more prevalent where free rider problems are greater?
The table reports results from logistic regressions of an indicator variable taking the value of one if a new activist files a SC-13D in a given month. Activist present is a binary variable which indicates the presence of other activist in the target firm in the month of the filing. The set of control variables is based on Klein and Zur (2009). All specifications include year and industry (of target firm) fixed-effects. p-values are shown in parenthesis with ***, ** and * indicating a statistical significance level of 1%, 5% and 10% respectively.

Dependent variable:	New activist		
	(1)	(2)	(3)
Constant	-4.025 ***	-3.750 ***	-3.741 ***
	(0.000)	(0.000)	(0.000)
Activist present	0.049	-0.035	-0.191
	(0.796)	(0.854)	(0.399)
ln (Market value)	-0.158 ***	-0.165 ***	-0.165 ***
	(0.000)	(0.000)	(0.000)
Activist present * ln (Market value)	0.073 *	0.065 *	0.073 *
	(0.059)	(0.093)	(0.079)
Return on assets		0.002	-0.014
		(0.981)	(0.849)
Z-score		-0.008 ***	-0.008 ***
		(0.002)	(0.002)
Capital expenditures/assets		-0.321	-0.483
		(0.381)	(0.225)
Dividends/share		0.023	0.031
		(0.326)	(0.143)
Total debt/assets		-0.011	-0.027
		(0.829)	(0.605)
Cash + short-term investment/assets		0.015	0.019
		(0.900)	(0.878)
Prior year return to shareholders		-0.131 ***	-0.121 ***
		(0.000)	(0.000)
Activist present * Return on assets			0.118
			(0.615)
Activist present * Z-score			0.003
			(0.744)
Activist present * (Capital expenditures/assets)			1.105
			(0.221)
Activist present * (Dividends/shares)			-0.272
			(0.196)
Activist present * (Total debt/assets)			0.294
			(0.256)
Activist present * (Cash + short-term investment/assets)			0.022
			(0.947)
Activist present * Prior year return to shareholders			-0.080
			(0.426)
Year fixed-effects	Yes	Yes	Yes
Industry fixed-effects	Yes	Yes	Yes
Obs.	254639	254639	254639
Pseudo r-squared	0.009	0.022	0.022

Table 2.8: Coordinated activism and activist size
The table reports results from logistic regressions of an indicator variable taking the value of one if a filing is from a coordinating activist. In Column 1 the independent variable of interest is 13F Activist which indicates if an activist has over $100 million in assets and files Form 13F. In Column 2 the independent variable of interest is ln(AUM) which is the natural logarithm of the asset base of the activist. To address concerns that activists classified as non-Form 13F filers may have more than $100 million in assets but are not required to file the form, we include only activists whom we can identify as institutional investment managers. The set of control variables is based on Klein and Zur (2009). p-values are shown in parenthesis with ***, ** and * indicating a statistical significance level of 1%, 5% and 10% respectively.

Dependent variable:	Coordinator	
	(1)	(2)
Constant	1.525 ***	2.705 ***
	(0.000)	(0.000)
13F Activist	-0.297 ***	
	(0.008)	
ln(AUM)		-0.101 **
		(0.022)
Return on assets	0.172	-0.065
	(0.230)	(0.881)
Z-score	-0.006	-0.019
	(0.120)	(0.104)
Capital expenditures/assets	-0.312	-0.262
	(0.695)	(0.817)
Dividends/share	0.024	0.050
	(0.679)	(0.613)
Total debt/assets	0.188	0.397
	(0.386)	(0.300)
Cash + short-term investment/assets	-0.220	-0.252
	(0.368)	(0.547)
Prior year return to shareholders	0.031	0.062
	(0.678)	(0.573)
ln(Market value)	-0.074 **	-0.073
	(0.042)	(0.154)
Obs.	1916	855
Pseudo r-squared	0.011	0.020

Table 2.9: Activist size and the probability of being a leader or follower?
The table reports results from logistic regressions of an indicator variable taking the value of one if a filing generates a follower filing in the next year (Columns 1 and 2) and an indicator variable taking the value of one if the filing is a follower filing (Columns 3 and 4). The independent variable of interest is in Columns 1 and 3 is 13F Activist which is an indicator variable if an activist files Form 13F, and in Columns 2 and 4 is ln(AUM) which is the natural logarithm of the asset base of the activist. To address concerns that activists classified as non-Form 13F filers may have more than $100 million in assets but are not required to file the form, we include only activists whom we can identify as institutional investment managers. The set of control variables is based on Klein and Zur (2009). p-values are shown in parenthesis with ***, ** and * indicating its statistical significance level of 1%, 5% and 10% respectively.

Dependent variable:	Leader		Follower	
	(1)	(2)	(3)	(4)
Constant	-1.315 ***	1.069	1.079 ***	1.491 **
	(0.000)	(0.168)	(0.000)	(0.021)
13F Activist	-0.270 **		-0.289 ***	
	(0.026)		(0.006)	
ln(AUM)		-0.239 ***		-0.033
		(0.000)		(0.428)
Return on assets	-0.024	-0.041	0.140	-0.085
	(0.868)	(0.923)	(0.295)	(0.831)
Z-score	0.000	-0.007	-0.004	-0.017
	(0.954)	(0.495)	(0.260)	(0.124)
Capital expenditures/assets	0.947	1.311	-0.069	-0.241
	(0.241)	(0.264)	(0.927)	(0.822)
Dividends/share	0.005	-0.459	0.038	0.057
	(0.919)	(0.128)	(0.525)	(0.545)
Total debt/assets	0.405 *	0.865 **	0.183	0.572
	(0.050)	(0.026)	(0.356)	(0.111)
Cash + short-term investment/assets	0.261	0.272	-0.285	-0.227
	(0.311)	(0.565)	(0.212)	(0.564)
Prior year return to shareholders	-0.115	-0.210	0.069	0.055
	(0.178)	(0.145)	(0.331)	(0.595)
ln(Market value)	-0.005	0.125 **	-0.060 *	-0.108 **
	(0.898)	(0.043)	(0.079)	(0.026)
Obs.	1916	855	1916	855
Pseudo r-squared	0.008	0.047	0.009	0.018

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Chapter 3

Lead and presiding directors in S&P 500 companies

Lead and presiding directors in S&P 500 companies

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Abstract. Using a sample of lead and presiding directors of S&P 500 firms we examine the effectiveness of this board position. The average lead director is male and has more board experience in comparison to his fellow S&P 500 directors. He is older and receives on average less votes than his colleagues on the board. This paper confirms prior research that firm value is not affected by the decision to combine or separate the CEO and chairman positions and shows that firm value is also not affected by the leadership structure with regard to the independent board leader (i.e., independent chairman, lead director, or presiding director). We find evidence that firm value is affected by the choice of the lead director. Lead directors who receive a high retainer are associated with lower firm value. Paid lead directors are also associated with higher discretionary accruals whereas a lead director who is appointed to all three mandatory board committees reduces the probability of restating the financial statement. Total CEO compensation does not depend on the leadership structure of the firm. However, performance based compensation is higher for firms with paid lead directorships.

JEL: G30, G34
Keywords: Board of Directors, Lead Director, Committee Structure.

3.1 Introduction

In their seminal work, Fama and Jensen (1983) stress that one of the main duties of the board of directors is to monitor the firm's corporate strategy. Since then, a large body of academic literature has focused on directors' responsibilities and has tried to characterize attributes which influence a board's effectiveness and its involvement in the firm.[1] In the aftermath of last decade's accounting scandals (e.g., Enron and Worldcom), the board of directors and the central flaws in one of their main task, financial oversight, became the focus of attention of political discussions. In 2002, the Sarbanes-Oxley Act (SOX) established new standards for U.S. public companies which led to new listing requirements for companies on the major U.S. stock exchanges (NYSE, NASDAQ). One key aspect with respect to the board of directors were the new rules for the so-called outside or independent directors. Section 303A of the NYSE manual defines that all listed companies must have a majority of independent directors in order to "increase the quality of board oversight and lessen the possibility of damaging conflicts of interest." It further defines an independent director and determines that the three mandatory board committees, the audit committee, the compensation committee, and the nominating/corporate governance committee must be entirely composed of independent directors. Furthermore, the independent and non-management directors of public firms listed on the major U.S. stock exchanges are obliged to regularly meet in executive sessions, i.e., in sessions where the management is not present and a non-management director presides. In a response to this requirement, many companies established the position of the lead (or presiding)

[1] E.g., board size (Yermack, 1996) or independence of directors (Rosenstein and Wyatt, 1990).

director: a director serving as a leader of the independent directors and as liaison between management and the independent directors on the board. This regulation can be seen as a reasonable compromise between the demand of shareholder activists calling for the separation of the position of the CEO and the chairman (and consequently, the nomination of an independent chairman) and the status quo in most U.S. firms, where these two positions are combined. Since then, the financial press is regularly announcing appointments of lead directors in corporate America. Warren Buffet (The Washington Post Co.), John E. Pepper Jr. (The Walt Disney Company), and William H. T. Bush (WellPoint Inc.) are just a few of well-known U.S. business men who served as lead directors in S&P 500 companies.

In this paper we take a closer look at this new position and analyze its effectiveness. We study a sample of lead and presiding directors from S&P 500 companies from 2005 to 2011. We report that the lead or presiding director is male, older, and more tenured than his colleagues. We further show that the firm's leadership structure does, on average, not influence firm value. However, we find evidence that well-paid lead and presiding directors are associated with lower firm value. Two of the most important tasks of the board of directors are financial oversight as it is done by the audit committee and establishing incentives for the top executives through compensation contracts. Therefore, we also analyze the position of the lead director with respect to audit quality and executive compensation. Audit quality can be improved if the lead director is appointed to all three mandatory board committees whereas his appointment to the compensation committee may reduce equity-based compensation. We conclude that there is no superior leadership structure for

all firms and that further regulation with regard to an independent chairman or the lead director position should be carefully considered.

The main contribution of this paper is to describe and analyze the lead director's position. To the best of our knowledge, there is still no academic evidence if this position is improving board oversight or the company's operating performance. While politics and regulators often highlight the importance of the independence of the board leader, critics argue that it is unknown to what extend the CEO can influence the nomination of the lead (or presiding) director and thus potentially weaken his position.

This paper adds to the body of literature which analyzes the leadership structures in corporations and the role of independent board directors. Independent outside directors are widely considered as better monitors (e.g., Yermack, 2005; Rosenstein and Wyatt, 1990). However, recent articles claim that the cost of information to monitor the firm conscientiously is higher for outside directors than for directors who are affiliated with the firm. Furthermore, Fahlenbrach, Low, and Stulz (2012) argue that outside directors have incentives to leave the firm if their reputation is in danger. They state, among other things, that after surprise director departures, affected firms have worse stock performance and are more likely to suffer from an extreme negative return event (e.g., named in federal class action lawsuits, delisting from major stock exchanges).

Starting with Brickley, Coles, and Jarell (1997) a large body of literature analyzes the leadership structure of the board. However, the empirical evidence is twofold. Brickley, Coles, and Jarell (1997) find that the cost of separating

the positions of the CEO and chairman are larger than the benefits for most of the large firms. More than ten years later Dey, Engel, and Liu (2011) confirm that the decision to split the role of the CEO and the role of the board chair should be considered more carefully. They show that firms that split the positions due to investors' pressure have a significantly lower performance and a significantly lower contribution to shareholder wealth. Based on a sample of UK companies, Carapeto, Lasfer, and Machera (2005) show that the decision to split the roles is greeted with positive abnormal returns. However, no strong over-performance was measured in the post event period for those firms. Goyal and Park (2002) report that the sensitivity of CEO turnover to firm performance is significantly higher in firms in which the positions are not combined.

Besides the mixed evidence, shareholder activists in the U.S. are still pressuring for separation of these two positions. The number of shareholder proposals asking for an independent chairman per year has more than doubled from 19 in 2003 to 42 in 2010 in the Russel 3000 universe. The Institutional Shareholder Services Inc. (ISS) provides advice for mutual funds and other large shareholders how to vote on shareholder votes and proxy voting. ISS policy places significant weight on the duties of the lead director and its voting recommendations depended on the leadership structure in place. During our sample period, 187 shareholder proposals asking to establish an independent chairman have been submitted. ISS advised to vote against the proposal in 41% of the cases in which a lead director already served on the board, and on 39% of the cases in which a presiding director has been leading the independent directors. If the company did not have an explicitly named lead or

presiding director, ISS recommended to vote against the shareholder proposals in just 6% of all cases.[2]

This paper also provides new insights on the committee structure of board of directors. There is a body of work discussing specific committees, their composition, and their influence on the firm. Hayes, Mehran, and Schaefer (2004) analyze the committee structure and committee functions of S&P 500 firms in 1997 and 1998. They discover a negative relationship between CEO ownership and committee functions. Klein (1998) focuses on the number of independent outsiders and insiders in the nominating, investment, and finance committees and shows that inside directors on these committees lead to higher stock market returns. With respect to the audit committee, Klein (2002) documents a negative relation between the fraction of independent outsiders and various measures of earnings management. Xie, Davidson, and DaDalt (2003) shows that the composition of the audit committee, specifically the financial background of its members, is related to the likelihood of a firm engaging in earnings management. Horstmeyer (2011) connects the size of the nominating committee, and cross-memberships between committees, to outside director turnover.

The remainder of the paper is organized as follows. Section 2 summarize the data and provides descriptive statistics of the sample. Section 3 reports the results from the empirical analysis. Section 4 concludes.

[2]Choi, Fisch, and Kahan (2010) analyze the importance of ISS and other proxy advisers and report that, for example, ISS recommendation shifts 6% to 13% of shareholder votes.

3.2 Regulation, data, and variables

3.2.1 Regulation

Public firms listed on the major U.S. stock exchanges (NYSE, NASDAQ) have to fulfill certain corporate governance criteria. Among other things, the stock exchanges require that the independent or non-management directors meet in regularly scheduled executive sessions without management present. In addition, a non-management director is required to preside over those sessions and the company has to disclose how interested persons may communicate with the non-management directors. If one director is chosen to preside at all of the executive sessions, his name must be disclosed either on or through the company's website or in its annual proxy statement. In cases in which the company does not file an annual proxy statement, it has to disclose the information in its annual report (Form 10-K) filed with the SEC.[3] If the same individual is not presiding at every executive session, the company must disclose the procedure by which a presiding director is selected.[4]

Most companies call this independent board leader, lead director or presiding director. On average, the lead director position is associated with a larger number of tasks and more responsibilities than the position of the presiding director. This is also mirrored in the enumeration of both positions. In addition, companies which exhibit a rotating system or which associate the presiding director position with a committee chairmanship seldom use the expression "lead director." However, there are exceptions. Some companies

[3]If the company makes the disclosure through its website, the company must disclose this information in the proxy statement with the website's address.

[4]More information about current corporate governance disclosure requirements can be found at http://nysemanual.nyse.com in Section 3.

explicitly state that they do not want to use the term "lead director" since all directors should be equally important and should anytime, for example, engage in discussions with the CEO.

There is no "one size fits all approach" when it comes to the responsibilities of the independent board leader. In most cases, the responsibilities are described in the proxy statements or in the firm's corporate governance guidelines.[5] For example, Google Inc. assigns the following responsibilities to its lead director:[6]

- *Coordinating and moderating executive sessions of the board of directors' independent directors.*
- *Advising the executive chairman of the board of directors as to the quality, quantity, and timeliness of the flow of information from management that is necessary for the independent directors to perform their duties effectively and responsibly.*
- *Confirming the agenda with the Chief Executive Officer for meetings of the board of directors.*
- *Holding regular update sessions with the executive chairman of the board of directors.*
- *Acting as the principal liaison between the independent directors and the executive chairman of the board of directors on sensitive issues.*
- *Performing such other duties as the board of directors may from time to time delegate to the Lead Independent Director to assist the board of directors in the fulfillment of its responsibilities.*

Firms listed on U.S. stock exchanges also have to appoint certain committees: the audit committee, the compensation, and the nominating/corporate governance committee.[7] All three committees have to be composed entirely of independent directors. Companies are also required to identify members of

[5]The California public employees' retirement system (CalPERS) provides in their *global principles of accountable corporate governance* a list of lead director duties which can be accessed through the following link: www.calpers.ca.gov/eip-docs/about/board-cal-agenda/agendas/invest/201111/item03b.pdf.

[6]www.sec.gov/Archives/edgar/data/1288776/000130817914000114/lgoogle2014_def14a.htm

[7]For ease of notation, the nominating/corporate governance committee will be abbreviated to nominating committee.

those three mandatory board committees in their proxy statements.

3.2.2 Data

The empirical analysis is based on a sample of S&P 500 firms. The sample period is from 2005 to 2011.[8] Every company which once during the sample period has been a member of the S&P 500 enters the sample for the whole period, regardless of inclusion or exit of the S&P 500. For those companies data on the lead director position, committee membership, and retainers for the lead director and the committee chairs are collected from the annual proxy statements. In some cases the proxy statement does not include board information. This is for example the case if a firm is acquired shortly after the meeting or is part of some other restructuring plans. Two companies classify themselves as controlled company and are removed from the sample.[9] The resulting lead director sample is merged to the risk metrics director database to combine the lead director and his fellow directors. The final sample consists of 604 firms and 3,764 firm-years. We further use accounting data from Compustat and compensation data from Compustat's ExecuComp. Restatement data stems from the Audit Analytics database. Voting data and ISS recommendation information are obtained from Voting Analytics.

3.2.3 Overview of the independent board leaders

Our definition of an independent chairman follows the NYSE definition of an independent director with one exception. Whereas the NYSE sets the threshold for a former employee to become an independent director to three

[8] 2005 is the first year in which the above outlined regulations had to be implemented.

[9] Controlled companies do not have to comply to the stock exchange requirements to the same extent as public companies.

years, we think that e.g., a former CEO who serves four years as a chairman after his retirement is still not independent. Therefore, in our sample it it not possible to change the status from dependent to independent. The sample consist of 565 firm-years with an independent chairman. We observe that 82 companies change from a non-independent chairman (e.g., the CEO, a former executive officer, a immediate family member) to an independent chairman. In 2,518 firm-years a lead (1,471 firm-years) or presiding director (1,047 firm-years) is explicitly named. Apple Inc. is the only company which names two of its director as co-lead directors (7 firm-years). In 1,061 firm-years the lead or presiding director position is determined by another position. The most common case (486 firm-years) is the association of the lead or presiding director position and the chairmanship of the nominating committee. Due to data limitations it is not possible to distinguish the cases in which the lead director is chosen/elected and then given the nominating committee chairmanship or vice versa. In 510 firm-years the companies exhibit a rotating system. There are several rotating systems in use. The most common systems are the ones in which rotation takes place among all directors (196 firm-years) or among committee chairs (281 firm-years). In most cases the position is changed after one year (annual meeting to annual meeting), however, at some firms the presiding director changes at every executive session. Several companies have their own rotation procedure e.g., rotation among the non-committee chairs, rotation among the most senior directors, rotation among the members of the nominating committee etc. In our sample, a total of 739 directors hold the position of a lead (464) or presiding (343) director. In 674 firm-years the presiding or lead director is not specified explicitly. These are e.g., the

cases in which the presiding or lead director changes from executive session to executive session or if the company only discloses the form of the rotating system, but not the actual person (e.g., annual rotation among all committee chairs). The mean tenure for lead (presiding) directors in companies which do not exhibit rotating systems is 2.8 (2.7) years.[10]

Table 3.1 compares the lead/presiding director to his fellow S&P 500 directors. The first part of Panel A in Table 3.1 displays summary statistics for all independent board leaders. The second part of Panel A reports summary statistics for all other non-executive directors. On average, the lead/presiding director is more tenured and older than the average S&P 500 director. This holds also true if we compare the age and the tenure of the lead/presiding director to the age and tenure of his colleagues on the board he is serving. The tenure ratio is calculated as the tenure of the lead/presiding director divided by the mean tenure of the board. The age ratio is calculated respectively. The mean tenure of the lead/presiding director is 1.5 times greater than the mean tenure of his colleagues. Further, the lead/presiding director is more often male and holds more additional board seats. The lead/presiding director holds on average 1.4 additional board seats. Further the attendance rate of lead/presiding directors is higher than those of the other directors. The differences of the means is statistically significant at the 1% level for all variables in Panel A except for the dummy indicating directors' attendance rate. Fort the variable the difference of the means is statistically significant at the 5% level.

[10]The tenure is calculated with respect to the sample, i.e., minimum is one year, maximum is 7 years.

Panel C of Table 3.1 displays committee membership and chairmanship of lead/presiding directors in comparison to all other independent directors. The sample used in the table is restricted to those lead/presiding directors whose position is not determined by another position or a rotating system. Lead/presiding directors are more often appointed to the compensation committee and the nominating committee. Since an appointment to the audit committee is more time consuming and requires special qualifications, the lead and presiding directors are less likely to be appointed to those committees. Again, the difference in means is statistically significant at the 1% level.[11]

Most non-management directors receive a fixed annual cash retainer. Some companies pay part of the retainer in restricted stocks or options. Directors often receive additional compensation in the form of annual retainers for the chairmanship or membership of board committees, meeting fees, insurance fees, usage of firm vehicles, or other non-pecuniary benefits.[12] To obtain a measure for the relative importance of the lead/presiding director position within the firm, the annual retainer for the lead/presiding director is collected. In addition, the annual chairmanship retainer for the audit, compensation, and nominating committee is collected. Table 3.2 displays the annual committee fees and lead/presiding director retainer. The annual mean lead director retainer is \$24,559 and the annual presiding director retainer is \$19,680, for the combined position the mean retainer amounts to \$23,514 with a minimum of \$1,000 and a maximum of \$200,000. This is slightly above the mean audit

[11] A chairman of a committee also counts as a member of the committee.

[12] More information about directors compensation is outlined by Yermack (2005). Perry (2000) estimates that the cash fees increase the annual retainer by about one third.

committee chair retainer of $17,723 followed by the mean compensation chair retainer ($12,148) and the mean nominating chair retainer ($10,720). Those numbers do not provide information about the level of total compensation of a director and can only be used for a within firm comparison and as an indicator of how much additional time and effort the director is expected to dedicate to this position in comparison to the committee chairs.[13]

Table 3.3 displays voting results of the independent board leader (i.e., independent chair, lead or presiding director) at the annual meeting one year after his appointment. *Min. Vote* is an indicator variable which takes the value of one if the corresponding director receives the lowest voting result of the voted directors on the board. *25 perc. Vote* is an indicator variable which takes the value of one if the director's voting results are in the 25 percentile of all directors voted on within the company in a given year. *50 perc. Vote* takes the value of one if the voting results are below the median within the company within the same year.[14] The results indicate that within each board, the independent board leaders receive significantly less approval than their colleagues. A possible explanation is that the independent board leaders are more likely to be held responsible for poor performance in comparison to the other directors on the board. This is also mirrored in the ISS voting recommendations. ISS recommended to vote against 2.32% of the independent board leaders and to vote against 1.97% of the other independent directors during our sample period.[15]

[13]The means are taken over the subset of firms with a non-zero value in the respective category.

[14]Some firms in the sample have a classified board which means that just one third of all director positions are voted for each year.

[15]Voting results are displayed for the years 2005-2010.

For our analysis, we use two measures for a strong lead or presiding director. Since it is widely assumed that most of the work is done in the committees of the board (Laux and Laux, 2009), we introduce, *Com. Lead*, a dummy variable which takes the value of one if the lead or presiding director is a member of all three mandatory board committees. A director who is a member of all three committees could potentially be more informed, and thus be a better independent board leader and monitor. Alternatively, three committee memberships could also imply more work and thus ineffectiveness. Our second measure of lead and presiding directorship is based on the additional retainer the lead or presiding director is eligible to receive. Given the additional responsibilities of the position the lead or presiding director is expected to devote a greater amount of time on board duties than his colleagues and thus might obtain an additional retainer. *$ Lead* is a dummy variable indicating if an additional retainer is paid to the lead or presiding director and if this retainer is at least as high as any of the committee chair retainers. In unreported robustness test we also use an indicator whether the lead or presiding director receives a higher retainer than all of the three committee chairs. Our results remain robust, however, the coefficients are slightly lower than for *$ Lead*. This can be taken as evidence that the extreme observations with respect to the lead director retainer are not driving our results. In 186 firm-years the lead or presiding director is a member of all three committees. The lead or presiding director receives an additional retainer in 1,197 firm-years. The correlation between *Com. Lead* and *$ Lead* is 0.14.

3.2.4 Summary statistics financial controls

Table 3.4 presents summary statistics for all variables used in our analysis. As common in the corporate governance literature (see e.g., Bebchuk, Cohen, and Ferrell (2009), Gompers, Ishii, and Metrick (2003) or Fracassi and Tate (2012)), we use Tobin's Q as a proxy for firm value and analyze empirically the association of firm value and characteristics of the independent board leader. Tobin's Q is defined as the market value of assets divided by the book value of assets. As an additional measure for firm performance we use return on assets (ROA) which is calculated as EBIT over total assets. We industry-adjust Tobin's Q (ROA) by deducting the median Q (ROA) for that year of all Compustat firms within the same industry and the same size tercile. Industries are defined based on the Fama-French 12 industry classifications. For the firm value regressions we use the following control variables which are based on Cremers and Ferrell (2014): firm size (measured as the natural logarithm of total assets), capital expenditure over lagged total assets (CAPX/total assets), research and development spending scaled by total assets (R&D/total assets), property, plant, and equipment scaled by total assets (PPE/total assets) and leverage (defined as book value of total debt over book value of total assets). The set of financial control variables is augmented by a set of Corporate Governance control variables which are based on Faleye, Hoitash, and Hoitash (2014). We control for board size (Yermack, 1996), the friction of independent outsiders (Rosenstein and Wyatt, 1990), directors' shareholdings, board business (Fich and Shivdasani, 2006),[16] and

[16] A busy director is defined as one who is serving on three or more boards simultaneously (Fich and Shivdasani, 2006). A busy board is a board on which the majority of independent directors hold more than three board seats.

Bebchuk, Cohen, and Ferrell's (2009) Entrenchment-Index (E-Index). The E-Index consists of the six most important provisions of Gompers, Ishii, and Metrick's (2003) G-Index.[17] A higher E-Index indicates stronger takeover protection and lower shareholder rights. All estimations include year fixed-effect and the regressions on the non-industry adjusted dependent variables also include industry fixed-effects.

3.3 Lead director choice and firm value

3.3.1 Independent board leader and firm value

Table 3.5 and Table 3.6 confirm prior research that firm value and return on assets does not depend on the leadership structure a firm chooses. The dependent variables in both tables are Tobin's Q, industry-adjusted Tobin's Q, ROA, and industry-adjusted ROA. In Table 3.5 the independent variable of interest is an indicator whether the board of director is led by an independent chairman. In Table 3.6 the independent variables of interest are an indicator whether the board of director has nominated a named lead director and an indicator whether the board has nominated a named presiding director. The financial control variables are based on Cremers and Ferrell (2014) and augmented with Corporate Governance control variables from Faleye, Hoitash, and Hoitash (2014). Neither the measures of Q, nor the measures of ROA seem to be correlated to the board's leadership form. A t-test on the equality of the coefficients of the lead director dummy variable and the presiding director dummy variable confirms that there is no difference between this two

[17] The E-Index is calculated as the sum of dummy variables for a staggered board, limitations on amending bylaws, limitations on amending the charter, a super-majority requirement to approve of a merger, golden parachutes, and poison pills.

positions with regard to their correlation with firm value and ROA. Both tables display a positive, consistent relationship between the overall governance as measured by Bebchuk, Cohen, and Ferrell's (2009) E-Index and firm value and return on asset.

Since the leadership structure is not correlated with firm value or ROA, in a next step, we investigate if certain characteristics of the lead or presiding drive differences in firm value. Table 3.7 shows regression results from various lead director characteristics on Q and ROA. We add the following variables to our set of explanatory variables: *Tenure 50 perc.* indicates whether the lead or presiding director has served longer on the board than at least half of his colleagues. *Age 75 perc.* indicates whether the lead or presiding directors' age is in the 75th percentile of the board he is serving on. We also include variables indicating the lead or presiding directors busyness and his committee memberships. The table provides support for our hypothesis that more tenured directors make better leaders as they have a better knowledge of the company. Furthermore, consistent with the literature about directors, the table reports negative correlation between firm value and the age of the lead director. Surprisingly, it seems that a paid lead or presiding director position is correlated with lower firm value and lower return on assets whereas committee membership, especially in the audit committee, improves those values. Business of the lead or presiding director seems not to be associated with firm value.

In Table 3.8 and Table 3.9 we regress our two measures of lead/presiding directorship on Q and ROA. In Columns 1 and 2, the independent variable of

interest is a dummy whether the lead or presiding director is member of all three mandatory board committees. The independent variable of interest in Columns 2 and 3 is *$ Lead*, a dummy variable indicating whether the holder of the lead or presiding receives an additional annual retainer which is as least as high as one of the committee chair retainers. The results of both tables indicate a negative association between firm value (or ROA) and the firms which have a paid lead or presiding director. The coefficients of *Com. Lead* are positive but insignificant in both columns in both tables.[18]

3.4 Audit quality

Table 3.7 indicates that committee membership of the independent board leader is associated with higher firm value. In this section, we investigate whether audit committee membership of the lead or presiding director improves the audit quality. We use financial restatements and absolute abnormal accruals as a measure of audit quality.

3.4.1 Financial Restatements

In Table 3.10 we analyze the propensity that a firm has to restate its financial statement with respect to our two measures of strong lead directorships and committee memberships of the lead or presiding director. The dependent variable of the probit regressions is an indicator whether the firm restated its fiscal year's results eventually. The control variables are based on Larcker, Richardson, and Tuna (2007). We control for last years book-to-market ratio, firm size, external financing (calculated as net equity plus net debt issued de-

[18]In unreported robustness test, we include the dummy variables for within board tenure and within board age, defined as in Table 3.7, in our regression analysis in Table 3.8 and Table 3.9. The results remain robust. However, the interpretation of the results is slightly different. When including lead or presiding director characteristics, firm-years with no named lead or presiding director are excluded from the analysis.

flated by the lagged market value of equity), acquisition spending over lagged total assets, and free cash flow. The measure of free cash flow is calculated as the difference of operating cash flow and average capital expenditure over the three prior years, deflated by lagged market value of equity. The estimations include year and industry fixed-effects. We further add a dummy variable for audit committee membership and the interactions of audit committee membership and our measure for the paid lead director. Column 1 and 2 are estimated for the whole sample. In firm-years in which no independent board leader is explicitly named (and thus committee membership is unknown), the committee dummy and the interaction term is set to zero. In addition, we include an indicator for those firm-years without a named lead or presiding director. In Columns 3, the sample is restricted to firm-years with named lead or presiding directors. Table 3.10 provides evidence that a lead or presiding director who is a member of the audit, the compensation, and the nominating committee can reduce the propensity of a financial restatement. Whereas it makes no difference on the propensity of a financial restatement if the director receives an additional annual retainer or if the lead director is members of the audit committee.

3.4.2 Discretionary accruals

Following Klein (2002), our second measure of audit quality is the absolute value of discretionary accruals. We estimate the modified Jones discretionary accruals (Dechow, Sloan, and Sweeney, 1995).[19] Discretionary accruals are

[19] All result remain robust when we exchange the modified Jones discretionary accruals with the Jones (1991) discretionary accruals.

the difference between total accruals and non-discretionary accruals (NDA):

$$DA_{i,t} = Total\ Accruals_{i,t} - NDA_{i,t}.$$

Total accruals are calculated as

$$Total\ Accruals_{i,t} = \frac{(\Delta CA_{i,t} + \Delta CL_{i,t} + \Delta Cash_{i,t} + \Delta STD_{i,t} + Dep_{i,t})}{A_{i,t-1}}$$

where ΔCA is the change in current assets, ΔCL is the change in current liabilities, $\Delta Cash$ is the change in cash holdings, ΔSTD is the change in long term debt in current liabilities, and Dep are the depreciation and amortization expenses. A denotes the total assets. The non-discretionary accruals are calculated based on the cross-sectional modified Jones model. They are the fitted values of the following regression:

$$Total\ Accruals_{i,t} = \alpha_0 + \alpha_1 \cdot \frac{1}{A_{i,t-1}} + \alpha_2 \cdot (\Delta Rev_{i,t} - \Delta Rec_{i,t}) + \alpha_3 \cdot PPE_{i,t} + \varepsilon_{i,t}.$$

The coefficients are estimated for every 2-digit SIC industry-year. ΔRev is the change in sales scaled by lagged assets, ΔRec is the change in receivables scaled by lagged assets, and PPE is gross property, plant and equipment deflated by lagged assets. We use the same set up as in Table 3.10 with a new set of control variables which follows Klein (2002). We control for the lagged book-to-market ratio, firm size, the absolute value of the change in EBIT, long term debt scaled by total assets, and a binary variable which takes the value of one for two subsequent years of negative income. We also include the CEO's pay-for-performance sensitivity as a control variable taking Laux and Laux's

(2009) argument into account that a lower magnitude of earnings management can be the result of better monitoring or the use of compensation systems that are less sensitive to performance. Pay-for-performance sensitivity is calculated as stock-related compensation (i.e., the value of stock and option awards) over salary and bonuses.

Table 3.11 shows the results of Tobit regressions on the absolute value of non-discretionary accruals. Column 2 and 3 provide evidence for a positive correlation between paid lead or presiding directorships and the magnitude of earnings management. The interaction term in Columns 2 and 3 are insignificant. This points to the idea that the absolute amount of non-discretionary accruals does not depend on audit committee membership of the independent board leader.

3.5 Compensation structure

In this subsection we analyze whether compensation committee membership of the lead or presiding director is associated with the CEO's payment structure. The payment structure consists of the level of pay and the design of contracts (i.e., fixed salary, bonus, stock option, restricted stock, other equity-based compensation). It is not as straight forward as it seems to be to determine what "good" or "bad" compensation designs are. It is often argued that equity compensation diminishes the agency problems of shareholders and executives (see e.g., Mehran, 1995) and aligns the interest of shareholders and management. However, in the aftermath of the great accounting fraud cases (e.g., Enron in 2002), researchers and regulators have argued that the risk shifting might potentially lead top executives to optimize the short term

share price instead of long-term firm value.

For our analysis, we use three measures of CEO compensation: the natural logarithm of total CEO compensation, CEO's pay-for-performance sensitivity (Jensen and Murphy, 1990), and CEO Centrality (Bebchuk, Cremers, and Peyer, 2011). CEO Centrality is the CEO's pay slice and calculated as the percentage of the total compensation of the top five executives that is captured by the CEO. We use CEO Centrality as a measure of the hierarchy level of the firm and the relative importance of the CEO. It is also argued that the CEO's pay slice measures to which extend the CEO is able to extract rent from the company. Bebchuk, Cremers, and Peyer (2011) find a positive association of the pay slice and various agency problems.

3.5.1 Total compensation

In Table 3.12 the independent variable is the natural logarithm of overall CEO compensation. The independent variables of interest are our two measures of the independent board leader: *Com. Lead* and *$ Lead*. In addition, we include a dummy variable indicating compensation committee membership and the interaction between compensation committee membership and our lead director measures. All independent variables are lagged by one year. Our set of control variables is based on Drobertz, von Meyerinck, Oesch, and Schmid (2014) and augmented with two CEO characteristics, the natural logarithm of CEO age and CEO tenure. The set of control variables is lagged for one year. Column 1 and 2 include all firm-years. Column 3 is restricted to firm-years in which a lead or presiding director is explicitly named. The regressions include year and industry fixed-effects and standard errors are

clustered at firm level.

Table 3.12 provides evidence that there is no overall relationship between the level of CEO enumeration and the compensation committee membership of the lead director. Further, the compensation of the lead or presiding director position is also not correlated with the total enumeration of the CEO. This confirms that potential enumeration of the lead director is not associated with overall level of compensation in a company. Not surprisingly, larger firms with more takeover protection pay on average more compensation to their CEOs.

3.5.2 Pay-for-performance sensitivity

Table 3.13 shows the result of Tobit regressions on CEO's pay-for-performance sensitivity. We use the same setup as in Table 3.12. In Column 1 the variable of interest is an indicator variable, *Com. Lead*, whether the lead or presiding director is a member of all three mandatory board committees. In Column 2 and 3 the independent variables of interest are, *$ Lead*, an indicator of compensation committee membership, and the interaction of those two variables. All independent variables are lagged by one year. As above, the set of financial and governance control variables is based on Drobertz, von Meyerinck, Oesch, and Schmid (2014).

Column 1 shows a negative association between *Com. Lead* and our measure of pay-for-performance sensitivity. This is in line with the model of Laux and Laux (2009) which predicts that an increase in task separation leads to higher pay-for-performance sensitivity. In Column 2 and 3, we see a positive relationship between the dependent variable and the indicator variables for paid lead or presiding director positions. The positive and significant coef-

ficient of the interaction term indicates that this relationship is weaker and almost if the lead or presiding director is also a member of the compensation committee.

3.5.3 The CEO's pay slice

In this subsection we study the association of CEO's Centrality and our two lead director characteristics. We use the setup of the OLS and Tobit regressions of Table 3.12 and Table 3.13. Our dependent variable, the centrality measure, is calculated as the percentage of total compensation of the top five executives captured by the CEO. Our results remain robust if we calculate the CEO's pay slice with respect to the top three executives. Table 3.14 shows that firms in which the lead or presiding director is a member of the three mandatory committees are negatively associated with CEO centrality. This results point towards the idea that firms with a well-informed lead director exhibit flatter hierarchies. In unreported robustness test, we find that the results hold, but are slightly weaker, if the lead or presiding director is just a member of the nominating and compensation committee. The coefficient of interest increases from -0.02 to -0.01. Columns 2 and 3 provide evidence that a paid lead or presiding director position is associated with a stronger CEO who is able to extract more rent. This effect weakened if the paid CEO is also a member of the compensation committee. Also of importance is the high and statistically significantly positive correlation between CEO Centrality and the indicator for firm-years without a named lead or presiding director. This points towards the idea that a strong independent board leader might help mitigate agency conflicts.

3.6 Conclusions

We study the effectiveness of the lead and presiding director position in a sample of S&P 500 firms from 2005 to 2011. We show that the average lead director is male, older and has more board experience in comparison to his fellow S&P 500 directors. The shareholder votes after a year of service are lower for lead and presiding directors in comparison to their colleagues indicating that shareholders tend to "punish" the lead or presiding director after a year of poor firm performance. We find that the overall choice of the independent board leader, i.e., an independent board chairman, a lead director or a presiding director does not influence firm value and return on assets. Analyzing the lead or presiding director position in more detail, we find a negative relation between lead or presiding directors which receive a relatively high additional retainer for their service and operating performance.

We provide evidence that appointing the lead or presiding director to the audit committee does not improve audit quality as measured by restatements and discretionary accruals. The probability of restating the financial statement is lower if the lead or presiding director is appointed to all three board committees and thus has a better overview of the decisions of the board. Overall the level of CEO compensation is not associated with the leadership structure. However, we show that the compensation structure varies with different lead or presiding director measures. CEOs receive more equity based compensation in firms with paid lead director positions. We find a negative correlation between CEO centrality, a measure of the power of the CEO, and director who is a member of the three mandatory board committees.

Our results indicate that there is no "one size fits all" solution when it comes to the leadership structure. From a theoretical point of view, a strong lead director or independent chairman should be improving board oversight. However, separating the role of the CEO and the chairman or establishing a strong lead director position is not automatically associated with higher operating performance. Politics and regulators should consider carefully if further regulation with regard to the independent board leader is needed.

Table 3.1: Independent board leader characteristics

The table reports summary statistics on characteristics of the independent board leaders and their fellow directors of S&P 500 companies from fiscal year 2005 to 2011. An independent board leader is defined as the independent chairman, the lead director, or the presiding director. A director is classified as a director with attendance problems if he attends less than 75% of all meetings in a year. The Tenure ratio is calculated as the tenure of the lead or presiding director divided by the mean tenure of the board. The age ratio is calculated respectively. Panel C displays summary statistics on committee chairmanship and membership. All variables in Panel C are indicator variables. A chairman is also a member of the committee. The table also reports *p*-values from a *t*-test on the difference in means. ***, **, and * indicate a statistical significance level of 1%, 5% and 10% respectively.

Panel A:	Independent board leader				Other non-executive directors				Diff.		
	Mean	Median	St. Dev.	Obs.	Mean	Median	St. Dev.	Obs.	Mean		p-value
Tenure	12.281	11	6.942	2985	8.647	7	6.903	26745	3.633	***	(0.000)
Age	65.892	66	6.496	2981	62.318	63	7.452	26743	3.574	***	(0.000)
Female (dummy)	0.077	0	0.267	3000	0.179	0	0.383	26749	-0.102	***	(0.000)
# of addional board seats	1.382	1	1.246	2648	1.141	1	1.168	26722	0.241	***	(0.000)
Attendance problems (dummy)	0.005	0	0.067	2648	0.009	0	0.094	26749	-0.004	**	(0.017)

Panel B:	Mean	Median	Min	Max	Std. Dev.	Obs
Tenure ratio	1.575	1.391	0.118	11.308	0.915	2219
Age ratio	1.057	1.060	0.693	1.490	0.110	2218

Panel C:	Independent board leader				Other non-executive directors				Diff.		
	Mean	Median	St. Dev.	Obs.	Mean	Median	St. Dev.	Obs.	Mean		p-value
Audit committee chair	0.077	0	0.267	2498	0.107	0	0.309	26749	-0.030	***	(0.000)
Audit committee member	0.353	0	0.478	2498	0.424	0	0.494	26749	-0.071	***	(0.000)
Comp. committee chair	0.179	0	0.384	2498	0.096	0	0.295	26749	0.083	***	(0.000)
Comp. committee member	0.554	1	0.497	2498	0.387	0	0.487	26749	0.168	***	(0.000)
Nom. committee chair	0.327	0	0.469	2495	0.070	0	0.256	19016	0.256	***	(0.000)
Nom. committee member	0.683	1	0.466	2498	0.414	0	0.493	26749	0.269	***	(0.000)

Table 3.2: Lead and presiding director retainer
The table reports summary statistics of the annual retainer for the lead or presiding director, and the chairmen of the audit, compensation, and nominating committee. Firm-years in which no retainer is paid are excluded from the calculations.

Retainer	Mean	Median	Min	Max	Std. Dev.	Obs
Lead/presiding director	23514.38	20000	1000	200000	16624.58	1370
Audit committee chair	17737.21	15000	1200	100000	9302.78	3418
Comp. committee chair	12150.90	10000	1200	150000	7075.61	3288
Nom. committee chair	10737.66	10000	500	130000	6196.91	3116

Table 3.3: Voting results
The table reports the voting results at the annual meeting after a year of service on the board. *Min. Vote* is an indicator variable whether a director received fewest votes on the board. *25 perc. Vote* is an indicator variable whether the voting results of a director are in the lower 25th percentile, and *50 perc. Vote* indicates whether a director's results are smaller than the median. The group of comparison are all non-executive directors in the same company and in the same year. The drop in observations is caused through companies in which not all directors are elected every year. The table also reports p-values from a t-test on the difference in means. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

	Independent board leader			Other non-executive directors			Diff.		
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Mean		p-value
Min. Vote	0.302	0.459	1156	0.172	0.377	10248	0.130	***	(0.000)
25 perc. Vote	0.478	0.005	1156	0.306	0.461	10248	0.172	***	(0.000)
50 perc. Vote	0.704	0.457	1156	0.532	0.499	10248	0.172	***	(0.000)

Table 3.4: Summary statistics of the financial control variables
The table reports summary statistics of the financial and accounting variables used in this study. The variables are defined in the main text.

	Mean	Median	St. Dev.	Obs.
log(total assets)	9.365	9.196	1.413	3762
CAPX/total assets	0.050	0.034	0.068	3751
Leverage	0.615	0.612	0.220	3762
R&D/total assets	0.026	0.000	0.053	3761
PPE/total assets	0.260	0.174	0.235	3610
Book-to-market value	0.472	0.393	0.627	3759
Tobin's Q	1.932	1.550	1.225	3759
Industry-adjusted Tobin's Q	0.499	0.128	1.145	3759
Return-on-assets (ROA)	0.105	0.094	0.086	3750
Industry-adjusted ROA	0.033	0.021	0.080	3750
External Financing	-0.019	-0.014	0.090	3370
Acquisition/total assets	0.030	0.001	0.105	3436
Free cash flow	0.055	0.062	0.525	3660
Absolute change in EBIT	0.598	0.159	5.275	3745
Long term debt/total assets	0.223	0.188	0.194	3749
Dummy for 2 years of subsequent negative income	0.046	0.000	0.210	3758
E-Index	2.554	2.000	1.425	3474
log(Board size)	2.337	2.303	0.221	3275
% of independent outsiders	0.792	0.818	0.119	3275
% of directors' shareholdings	1.723	0.068	6.578	3271
Busy board	0.265	0.000	0.441	3275
Restatement dummy	0.064	0.000	0.244	3764
Absolute discretionary accruals (Modified Jones)	0.347	0.070	1.086	3084
CEO tenure	7.280	6.000	5.718	3666
log(CEO age)	4.018	4.025	0.115	3640
log(total compensation CEO)	8.815	8.904	1.175	3716
Pay-for-performance sensitivity CEO	0.690	0.777	0.251	3646
Centrality 5	0.391	0.406	0.127	3594

Table 3.5: Independent chairman and firm value
The table reports results from regressions on Tobin's Q, industry-adjusted Tobin's Q, return-on-assets, and industry-adjusted return-on-assets. The independent variable of interest is *Independent chair*, an indicator variable whether the firm has appointed an independent chairman. Industry adjustment is made with respect to the median value of all companies in the same Fama-French 12 industry and the same size tercile. Control variables are based on Cremers and Ferrell (2014) and Faleye, Hoitash, and Hoitash (2014). All variables are defined in the main text. The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	Q		Ind.-adj. Q		ROA		Ind.-adj. ROA	
	(1)		(2)		(3)		(4)	
Constant	6.117	***	3.972	***	0.341	***	0.215	***
	(0.000)		(0.000)		(0.000)		(0.000)	
Independent chair	-0.116		-0.126		-0.008		-0.009	
	(0.217)		(0.190)		(0.281)		(0.188)	
log(total assets)	-0.225	***	-0.228	***	-0.012	***	-0.012	***
	(0.000)		(0.000)		(0.000)		(0.000)	
CAPX/total assets	3.412	***	3.116	***	0.201	***	0.158	***
	(0.000)		(0.000)		(0.000)		(0.003)	
Leverage	-0.383		-0.127		-0.043		-0.039	
	(0.229)		(0.678)		(0.125)		(0.143)	
R&D/total assets	3.820	***	1.689		-0.016		-0.036	
	(0.000)		(0.118)		(0.828)		(0.642)	
PPE/total assets	-0.954	***	-0.918	***	-0.056	**	-0.058	***
	(0.001)		(0.000)		(0.017)		(0.000)	
log(board size)	-0.403	**	-0.244		-0.010		-0.005	
	(0.022)		(0.162)		(0.468)		(0.720)	
% of independent outsiders	-0.287		-0.511	*	-0.011		-0.021	
	(0.332)		(0.076)		(0.608)		(0.305)	
% of directors' shareholdings	-0.005		-0.002		-0.000		-0.000	
	(0.197)		(0.572)		(0.158)		(0.335)	
Busy board	0.067		0.037		0.010	*	0.009	*
	(0.320)		(0.581)		(0.081)		(0.086)	
E-Index	-0.097	***	-0.110	***	-0.006	***	-0.006	***
	(0.000)		(0.000)		(0.003)		(0.003)	
Obs.	3132		3132		3122		3122	
R-squared	0.333		0.195		0.239		0.102	

Table 3.6: Lead and presiding director and firm value
The table reports results from regressions on Tobin's Q, industry-adjusted Tobin's Q, return-on-assets, and industry-adjusted return-on-assets. The independent variable of interest are *Lead director*, an indicator variable whether the firm has appointed a lead director and *presiding director*, an indicator whether the firm has appointed a presiding director. Industry adjustment is made with respect to the median value of all companies in the same Fama-French 12 industry and the same size tercile. Control variables are based on Cremers and Ferrell (2014) and Faleye, Hoitash, and Hoitash (2014). All variables are defined in the main text. The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. The table also reports on *p*-values of a *t*-test on the equality of the coefficients of lead and presiding director. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	Q		Ind.-adj. Q		ROA		Ind.-adj. ROA	
	(1)		(2)		(3)		(4)	
Constant	6.096	***	4.077	***	0.350	***	0.237	***
	(0.000)		(0.000)		(0.000)		(0.000)	
Lead director	-0.136		-0.133		-0.011		-0.010	
	(0.189)		(0.196)		(0.164)		(0.203)	
Presiding director	-0.137		-0.087		-0.008		-0.005	
	(0.158)		(0.368)		(0.272)		(0.491)	
log(total assets)	-0.231	***	-0.243	***	-0.014	***	-0.014	***
	(0.000)		(0.000)		(0.000)		(0.000)	
CAPX/total assets	3.609	***	3.473	***	0.192	***	0.159	***
	(0.000)		(0.000)		(0.001)		(0.004)	
Leverage	-0.266		-0.038		-0.033		-0.034	
	(0.440)		(0.909)		(0.283)		(0.242)	
R&D/total assets	4.213	***	1.878	*	-0.030		-0.029	
	(0.000)		(0.089)		(0.675)		(0.703)	
PPE/total assets	-1.057	***	-0.973	***	-0.062	***	-0.059	***
	(0.000)		(0.000)		(0.009)		(0.000)	
log(board size)	-0.355	*	-0.213		-0.009		-0.006	
	(0.053)		(0.233)		(0.553)		(0.672)	
% of independent outsiders	-0.323		-0.519	*	-0.010		-0.020	
	(0.281)		(0.076)		(0.656)		(0.335)	
% of directors' shareholdings	-0.008		-0.005		-0.001		-0.000	
	(0.142)		(0.338)		(0.127)		(0.197)	
Busy board	0.126		0.080		0.015	**	0.014	**
	(0.103)		(0.288)		(0.017)		(0.024)	
E-Index	-0.111	***	-0.128	***	-0.006	***	-0.006	***
	(0.000)		(0.000)		(0.003)		(0.002)	
Obs.	2675		2675		2665		2665	
R-squared	0.347		0.210		0.252		0.118	
p-value *t*-test: presiding=lead	0.996		0.554		0.681		0.446	

Table 3.7: Lead and presiding director characteristics and firm value
The table reports results from regressions on Tobin's Q, industry-adjusted Tobin's Q, return-on-assets, and industry-adjusted return-on-assets. Industry adjustment is made with respect to the median value of all companies in the same Fama-French 12 industry and the same size tercile. The independent variables include an indicator *Tenure 50 perc.*, whether the lead or presiding director has served longer on the board as the median of his colleagues and an indicator variable *Age 75 perc.* whether the lead or presiding directors' age is in the 75th percentile on his board. We further include indicator variables for committee memberships and an indicator variable whether the lead or presiding director is paid extra for his position. Control variables are based on Cremers and Ferrell (2014) and Faleye, Hoitash, and Hoitash (2014). All variables are defined in the main text. The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	Q		Ind.-adj. Q		ROA		Ind.-adj. ROA	
	(1)		(2)		(3)		(4)	
Constant	4.676	***	2.704	***	0.268	***	0.151	***
	(0.000)		(0.000)		(0.000)		(0.001)	
Tenure 50 perc.	0.146	**	0.151	**	0.008		0.011	*
	(0.028)		(0.025)		(0.158)		(0.059)	
Age 75 perc.	-0.117	*	-0.107	*	-0.010	**	-0.012	**
	(0.053)		(0.077)		(0.047)		(0.029)	
Busy lead or presiding director	-0.029		-0.005		-0.001		-0.000	
	(0.680)		(0.947)		(0.845)		(0.969)	
Audit committee membership	0.162	**	0.162	**	0.009		0.012	*
	(0.028)		(0.035)		(0.142)		(0.086)	
Comp. committee membership	0.114		0.107		0.010		0.012	*
	(0.106)		(0.138)		(0.101)		(0.062)	
Nom. committee membership	0.099		0.088		0.010	*	0.010	*
	(0.145)		(0.219)		(0.085)		(0.086)	
Paid lead or presiding director	-0.126	*	-0.173	**	-0.010	*	-0.011	*
	(0.091)		(0.023)		(0.082)		(0.058)	
log(total assets)	-0.209	***	-0.222	***	-0.012	***	-0.013	***
	(0.000)		(0.000)		(0.001)		(0.001)	
CAPX/total assets	3.855	***	3.381	***	0.190	***	0.162	***
	(0.000)		(0.000)		(0.001)		(0.007)	
Leverage	-0.318		-0.114		-0.054	*	-0.043	
	(0.355)		(0.727)		(0.053)		(0.170)	
R&D/total assets	3.614	***	1.442		-0.082		-0.067	
	(0.001)		(0.209)		(0.268)		(0.399)	
PPE/total assets	-0.955	***	-0.936	***	-0.056	***	-0.059	***
	(0.000)		(0.000)		(0.009)		(0.000)	
log(board size)	-0.106		0.008		0.008		0.010	
	(0.537)		(0.962)		(0.606)		(0.490)	
% of independent outsiders	0.165		-0.028		0.019		0.012	
	(0.579)		(0.923)		(0.368)		(0.615)	
% of directors' shareholdings	-0.002		-0.000		-0.000		-0.000	
	(0.625)		(0.916)		(0.625)		(0.876)	
Busy board	0.094		0.065		0.010	*	0.011	*
	(0.208)		(0.369)		(0.075)		(0.067)	
E-Index	-0.099	***	-0.119	***	-0.007	***	-0.007	***
	(0.000)		(0.000)		(0.002)		(0.003)	
Obs.	2088		2088		2080		2080	
R-squared	0.355		0.203		0.291		0.122	

Table 3.8: *Com. Lead*, *$ Lead* and firm value

The table reports results from regressions on Tobin's Q and industry-adjusted Tobin's Q. Industry adjustment is made with respect to the median value of all companies in the same Fama-French 12 industry and the same size tercile. *Com. Lead* is an indicator variable whether the lead or presiding director is member of all three mandatory board committees. *$ Lead* is an indicator variable whether the lead or presiding director is paid as least as much as one of the committee chairs. Control variables are based on Cremers and Ferrell (2014) and Faleye, Hoitash, and Hoitash (2014). All variables are defined in the main text. The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	Q		Ind.-adj. Q		Q		Ind.-adj. Q	
	(1)		(2)		(3)		(4)	
Constant	6.055	***	4.060	***	6.027	***	3.996	***
	(0.000)		(0.000)		(0.000)		(0.000)	
Com. Lead	0.172		0.074					
	(0.285)		(0.657)					
$ Lead					-0.208	***	-0.239	***
					(0.001)		(0.000)	
log(total assets)	-0.232	***	-0.244	***	-0.237	***	-0.246	***
	(0.000)		(0.000)		(0.000)		(0.000)	
CAPX/total assets	3.441	***	3.408	***	3.567	***	3.463	***
	(0.000)		(0.000)		(0.000)		(0.000)	
Leverage	-0.296		-0.045		-0.245		-0.011	
	(0.383)		(0.889)		(0.471)		(0.972)	
R&D/total assets	4.308	***	1.864	*	4.200	***	1.940	*
	(0.000)		(0.090)		(0.000)		(0.074)	
PPE/total assets	-1.012	***	-0.973	***	-1.024	***	-0.949	***
	(0.000)		(0.000)		(0.000)		(0.000)	
log(board size)	-0.359	*	-0.224		-0.365	**	-0.222	
	(0.053)		(0.218)		(0.044)		(0.210)	
% of independent outsiders	-0.345		-0.553	*	-0.230		-0.400	
	(0.275)		(0.069)		(0.459)		(0.186)	
% of directors' shareholdings	-0.007		-0.004		-0.008	*	-0.006	
	(0.153)		(0.412)		(0.089)		(0.232)	
Busy board	0.125	*	0.083		0.122		0.078	
	(0.094)		(0.252)		(0.108)		(0.284)	
E-Index	-0.111	***	-0.127	***	-0.115	***	-0.130	***
	(0.000)		(0.000)		(0.000)		(0.000)	
Obs.	2675		2675		2675		2675	
R-squared	0.346		0.208		0.351		0.218	

Table 3.9: *Com. Lead*, *$ Lead*, and ROA
The table reports results from regressions on ROA and industry-adjusted ROA. Industry adjustment is made with respect to the median value of all companies in the same Fama-French 12 industry and the same size tercile. *Com. Lead* is an indicator variable whether the lead or presiding director is member of all three mandatory board committees. *$ Lead* is an indicator variable whether the lead or presiding director is paid as least as much as one of the committee chairs. Control variables are based on Cremers and Ferrell (2014) and Faleye, Hoitash, and Hoitash (2014). All variables are defined in the main text. The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	ROA		Ind.-adj. ROA		ROA		Ind.-adj. ROA	
	(1)		(2)		(3)		(4)	
Constant	0.325	***	0.232	***	0.325	***	0.232	***
	(0.000)		(0.000)		(0.000)		(0.000)	
Com. Lead	0.017		0.018					
	(0.153)		(0.226)					
$ Lead					-0.013	**	-0.014	***
					(0.013)		(0.007)	
log(total assets)	-0.012	***	-0.014	***	-0.013	***	-0.014	***
	(0.000)		(0.000)		(0.000)		(0.000)	
CAPX/total assets	0.169	***	0.145	***	0.183	***	0.158	***
	(0.001)		(0.007)		(0.000)		(0.004)	
Leverage	-0.047	*	-0.035		-0.043		-0.032	
	(0.078)		(0.211)		(0.108)		(0.262)	
R&D/total assets	-0.017		-0.028		-0.028		-0.027	
	(0.808)		(0.716)		(0.694)		(0.726)	
PPE/total assets	-0.054	***	-0.057	***	-0.056	***	-0.058	***
	(0.009)		(0.000)		(0.008)		(0.000)	
log(board size)	-0.005		-0.005		-0.006		-0.007	
	(0.751)		(0.728)		(0.691)		(0.644)	
% of independent outsiders	-0.015		-0.022		-0.008		-0.014	
	(0.483)		(0.311)		(0.691)		(0.533)	
% of directors' shareholdings	-0.001		-0.000		-0.001	*	-0.001	
	(0.143)		(0.264)		(0.091)		(0.157)	
Busy board	0.013	**	0.014	**	0.013	**	0.013	**
	(0.010)		(0.017)		(0.012)		(0.020)	
E-Index	-0.006	***	-0.006	***	-0.006	***	-0.006	***
	(0.003)		(0.002)		(0.002)		(0.002)	
Obs.	2665		2665		2665		2665	
R-squared	0.269		0.118		0.272		0.122	

Table 3.10: Financial restatements
The table reports probit regression estimates for whether a firm restates its audited financial statements for a given year. *Com. Lead* is an indicator variable whether the lead or presiding director is member of all three mandatory board committees. *$ Lead* is an indicator variable whether the lead or presiding director is paid as least as much as one of the committee chairs. We further include an indicator variable for audit committee membership and an interaction term for committee membership and *$ Lead*. For firm-years in which no lead or presiding director is appointed, audit committee membership is set to zero. Control variables are based on Larcker, Richardson, and Tuna (2007). All variables are defined in the main text. The regressions include year and industry fixed-effects. *p*-values appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	Restatement Dummy					
	(1)		(2)		(3)	
Constant	-0.502		-0.597		-0.832	*
	(0.230)		(0.162)		(0.083)	
Com. Lead	-0.620	**				
	(0.013)					
$ Lead			0.013		-0.018	
			(0.921)		(0.896)	
Audit committee ms			0.035		0.028	
			(0.795)		(0.838)	
$ Lead * Audit committee ms			-0.115		-0.078	
			(0.581)		(0.712)	
No lead or pres. director	-0.353	***	-0.307	**		
	(0.004)		(0.032)			
Book-to-market value (t-1)	0.380	**	0.384	***	0.492	***
	(0.010)		(0.009)		(0.008)	
log(total assets)	-0.050		-0.045		-0.021	
	(0.220)		(0.266)		(0.648)	
External financing	2.114	***	2.057	***	1.841	***
	(0.000)		(0.001)		(0.007)	
Acquisition/total assets	0.117		0.141		0.492	
	(0.785)		(0.745)		(0.282)	
Free cash flow	-0.102		-0.068		-0.094	
	(0.784)		(0.849)		(0.804)	
E-Index	-0.026		-0.027		-0.032	
	(0.511)		(0.482)		(0.471)	
Obs.	2393		2393		1887	
Pseudo r-squared	0.140		0.133		0.139	

Table 3.11: Absolute discretionary accruals
The table reports Tobit regression estimates of absolute discretionary accruals. Following Bergstresser and Philippon (2006) and Klein (2002), the discretionary accruals are constructed according to the cross-sectional modified Jones models. *Com. Lead* is an indicator variable whether the lead or presiding director is member of all three mandatory board committees. *$ Lead* is an indicator variable whether the lead or presiding director is paid as least as much as one of the committee chairs. We further include an indicator variable for audit committee membership and an interaction term for committee membership and *$ Lead*. For firm-years in which no lead or presiding director is appointed, audit committee membership is set to zero. Control variables are based on Klein (2002) and defined in the main text. The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	Absolute discretionary accruals		
	(1)	(2)	(3)
Constant	-0.310	-0.352	-0.532 **
	(0.159)	(0.117)	(0.048)
Com. Lead	0.061		
	(0.509)		
$ Lead		0.130 **	0.127 *
		(0.040)	(0.056)
Audit committee ms		0.056	0.061
		(0.422)	(0.403)
$ Lead * Audit committee ms		-0.091	-0.089
		(0.369)	(0.397)
No lead or pres. director named	0.050	0.109 *	
	(0.369)	(0.094)	
Book-to-market value (t-1)	0.029	0.015	-0.016
	(0.733)	(0.855)	(0.882)
Abs. change in EBIT	-0.001	-0.001	0.001
	(0.936)	(0.902)	(0.970)
log(total assets)	0.027	0.029	0.040
	(0.206)	(0.180)	(0.116)
Long term debt total assets	-0.062	-0.076	-0.024
	(0.632)	(0.558)	(0.882)
2 years subs. negative income (dummy)	-0.100	-0.106	-0.153
	(0.469)	(0.445)	(0.355)
Pay-for-performance sensitivity	0.106	0.089	0.121
	(0.268)	(0.347)	(0.281)
E-Index	-0.000	0.002	0.019
	(0.993)	(0.918)	(0.399)
Obs.	2347	2347	1858
LR chi-squared	626.146	630.221	503.565
Prob. > chi-squared	0.000	0.000	0.000

Table 3.12: Total compensation

The table reports regression estimates of the natural logarithm of total compensation. *Com. Lead* is an indicator variable whether the lead director is member of all three mandatory board committees. *$ Lead* is an indicator variable whether the lead director is paid as least as much as one of the committee chairs. We also include an indicator variable for compensation committee membership. For firm-years in which no lead or presiding director is appointed, comp. committee membership is set to zero. Control variables are based on Drobertz, von Meyerinck, Oesch, and Schmid (2014). The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	log(total compensation)					
	(1)		(2)		(3)	
Constant	5.589	***	5.305	***	5.950	***
	(0.000)		(0.000)		(0.000)	
Com. Lead	0.103					
	(0.501)					
$ Lead			0.331		0.315	
			(0.105)		(0.114)	
Comp. com. ms			0.193		0.184	
			(0.339)		(0.349)	
$ Lead * Comp. com. ms			-0.302		-0.278	
			(0.162)		(0.191)	
No lead or pres. director	0.187	*	0.348			
	(0.078)		(0.129)			
log(total assets)	0.200	***	0.209	***	0.169	*
	(0.009)		(0.003)		(0.051)	
ROA	1.010	*	1.010	*	0.792	
	(0.077)		(0.071)		(0.271)	
Q	-0.086		-0.077		-0.085	
	(0.402)		(0.428)		(0.544)	
CAPX/total assets	0.991	*	1.070	*	0.968	
	(0.061)		(0.053)		(0.215)	
Leverage	0.145		0.141		0.089	
	(0.420)		(0.421)		(0.639)	
R&D/total assets	1.068		1.017		1.387	
	(0.456)		(0.464)		(0.444)	
PPE/total assets	-0.151		-0.147		-0.233	
	(0.449)		(0.467)		(0.300)	
2 years subs. negative income (dummy)	-0.069		-0.095		-0.115	
	(0.456)		(0.308)		(0.244)	
log(board size)	0.320		0.325		0.362	
	(0.226)		(0.219)		(0.286)	
% of independent outsiders	0.555	**	0.503	*	0.722	**
	(0.031)		(0.053)		(0.020)	
% of directors' shareholdings	-0.000		0.002		0.005	
	(0.983)		(0.618)		(0.229)	
Busy board	0.154	**	0.149	**	0.149	**
	(0.016)		(0.013)		(0.044)	
E-Index	0.066	**	0.073	**	0.056	
	(0.018)		(0.014)		(0.118)	
Tenure CEO	-0.008		-0.007		-0.013	
	(0.308)		(0.361)		(0.133)	
log(CEO age)	0.051		0.058		-0.009	
	(0.866)		(0.848)		(0.979)	
Obs.	2081		2081		1645	
R-squared	0.132		0.138		0.122	

Table 3.13: Pay-for-performance sensitivity
The table reports Tobit regression estimates of the CEO's pay-for-performance sensitivity. *Com. Lead* is an indicator variable whether the lead director is member of all three mandatory board committees. *$ Lead* is an indicator variable whether the lead director is paid as least as much as one of the committee chairs. We further include an indicator variable for compensation committee membership. For firm-years in which no lead or presiding director is appointed, comp. committee membership is set to zero. Control variables are based on Drobertz, von Meyerinck, Oesch, and Schmid (2014). The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	Pay-for-performance sensitivity		
	(1)	(2)	(3)
Constant	0.641 ***	0.605 ***	0.948 ***
	(0.004)	(0.007)	(0.000)
Com. Lead	-0.049 **		
	(0.041)		
$ Lead		0.074 ***	0.072 ***
		(0.000)	(0.000)
Comp. com. ms		0.023	0.024
		(0.148)	(0.138)
$ Lead * Comp. com. ms		-0.069 ***	-0.065 ***
		(0.005)	(0.009)
No lead or pres. director	-0.004	0.026	
	(0.763)	(0.129)	
log(total assets)	0.026 ***	0.028 ***	0.025 ***
	(0.000)	(0.000)	(0.000)
ROA	0.057	0.045	-0.066
	(0.526)	(0.622)	(0.519)
Q	0.000	0.002	0.017 *
	(0.974)	(0.765)	(0.055)
CAPX/total assets	0.025	-0.012	-0.114
	(0.849)	(0.927)	(0.454)
Leverage	-0.053 *	-0.060 *	-0.073 **
	(0.086)	(0.051)	(0.038)
R&D total assets	-0.011	0.011	-0.080
	(0.936)	(0.934)	(0.593)
PPE/total assets	0.054	0.064	0.050
	(0.201)	(0.125)	(0.279)
2 years subs. negative income (dummy)	-0.002	-0.006	-0.006
	(0.949)	(0.854)	(0.869)
log(board size)	0.025	0.034	0.052
	(0.386)	(0.233)	(0.107)
% of independent outsiders	0.304 ***	0.297 ***	0.333 ***
	(0.000)	(0.000)	(0.000)
% of directors' shareholdings	-0.001 *	-0.001	-0.001
	(0.077)	(0.191)	(0.235)
Busy board	0.011	0.011	0.008
	(0.353)	(0.383)	(0.541)
E-Index	0.025 ***	0.027 ***	0.019 ***
	(0.000)	(0.000)	(0.000)
Tenure CEO	-0.002 *	-0.002 *	-0.002 *
	(0.055)	(0.072)	(0.060)
log(CEO age)	-0.105 **	-0.113 **	-0.203 ***
	(0.050)	(0.035)	(0.001)
Obs.	2028	2028	1611
LR chi-squared	187.197	197.545	151.438
Prob. > chi-squared	0.000	0.000	0.000

Table 3.14: CEO centrality
The table reports Tobit regression estimates of the CEO's pay slice (Bebchuk, Cremers, and Peyer, 2011). *Com. Lead* is an indicator variable whether the lead director is member of all three mandatory board committees. *$ Lead* is an indicator variable whether the lead director is paid as least as much as one of the committee chairs. We also include an indicator variable for comp. committee membership. For firm-years in which no lead or presiding director is appointed, comp. committee membership is set to zero. Control variables are based on Drobertz, von Meyerinck, Oesch, and Schmid (2014). The regressions include year and industry fixed-effects. *p*-values based on standard errors clustered at the firm level appear in parentheses. ***, **, and * indicate a statistical significance level of 1%, 5%, and 10% respectively.

Dependent variable:	CEO Centrality 5					
	(1)		(2)		(3)	
Constant	0.540	***	0.525	***	0.589	***
	(0.000)		(0.000)		(0.000)	
Com. Lead	-0.014					
	(0.239)					
$ Lead			0.019	*	0.017	*
			(0.050)		(0.080)	
Comp. com. ms			0.017	**	0.016	*
			(0.041)		(0.051)	
$ Lead * Comp. com. ms			-0.025	**	-0.022	*
			(0.048)		(0.065)	
No lead or pres. director	0.023	***	0.035	***		
	(0.001)		(0.000)			
log(total assets)	-0.009	***	-0.008	***	-0.009	***
	(0.002)		(0.005)		(0.003)	
ROA	0.094	**	0.087	*	0.015	
	(0.045)		(0.064)		(0.770)	
Q	-0.016	***	-0.016	***	-0.006	
	(0.000)		(0.000)		(0.147)	
CAPX / total assets	-0.087		-0.099		-0.181	**
	(0.184)		(0.127)		(0.017)	
Leverage	0.020		0.017		0.032	*
	(0.210)		(0.280)		(0.064)	
R&D/total assets	-0.118	*	-0.108		-0.143	*
	(0.086)		(0.112)		(0.052)	
PPE/total assets	0.035		0.039	*	0.044	*
	(0.100)		(0.066)		(0.054)	
2 years of subs. negative income (dummy)	-0.005		-0.006		-0.008	
	(0.782)		(0.743)		(0.660)	
log (board size)	-0.020		-0.017		-0.008	
	(0.165)		(0.237)		(0.615)	
% of independent outsiders	0.116	***	0.117	***	0.146	***
	(0.000)		(0.000)		(0.000)	
% of directors' shareholdings	-0.000		-0.000		-0.000	
	(0.197)		(0.312)		(0.316)	
Busy board	0.010		0.009		0.006	
	(0.102)		(0.127)		(0.361)	
E-Index	0.008	***	0.009	***	0.006	**
	(0.000)		(0.000)		(0.024)	
Tenure CEO	-0.001	***	-0.001	**	-0.002	***
	(0.009)		(0.011)		(0.006)	
log(CEO age)	-0.020		-0.023		-0.050	*
	(0.452)		(0.390)		(0.092)	
Obs.	2010		2010		1598	
LR chi-squared	254.600		258.560		229.234	
Prob. > chi-squared	0.000		0.000		0.000	

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