ABSTRACT

Title of dissertation: ESSAYS IN CORPORATE FINANCE

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This dissertation presents two essays about corporate finance, product market, and corporate governance.

The first essay shows that, depending on product market structure, firms adjust executive compensation differently in response to shocks to firm risk. Using a natural experiment that increases firm risk due to discoveries of carcinogens, I find that treated firms increase CEO risk-taking incentives to mitigate underinvestment. This result is mainly driven by treated firms in less affected industries, which suggests that firms respond to shocks more strongly when fewer rivals face the same shock, and extends existing work on executive compensation adjustments based on industry-level analyses.

The second essay provides evidence that the effect of product market competition on corporate performance depends on the overlap in customer base. Competition between firms supplying to a same customer mitigates the decline in firms' operating performance after the passage of a business combination law. This finding

is more evident when the common customer is the only major customer or when firms produce specific inputs. In addition, competition between firms supplying to different customers has little effect on firm performance. These results highlight the impact of the structure of production cluster, defined as a group of same-industry firms that supply to a same customer, on corporate outcomes.

ESSAYS IN CORPORATE FINANCE

by

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Dedication

This dissertation is dedicated to my mother, Jianqing, my father, Sheng, and my husband, Bobby.

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Chapter 1: Does Executive Compensation Depend on Product Market Structure? Evidence from Shocks to Firm Risk

1.1 Introduction

The design of executive compensation receives substantial interest from both practice and academia. The average real value of total CEO pay in S&P 500 firms climbed from $1.1 million in 1970 to $10.9 million in 2011. Most of the increase in CEO pay during this period is explained by the growth in stock option compensation (Murphy [1]). Agency theories suggest that firms use compensation contracts to align managers' and shareholders' interests. A number of studies recognize that risk-averse, under-diversified managers may underinvest in risky and valuable projects (e.g., Jensen and Meckling [2]; Smith and Stulz [3]; Guay [4]). To mitigate this risk-related agency conflict, firms may provide managers risk-taking incentives by using option compensation, the value of which increases with firms' stock return volatility (a measure for firm risk). However, existing evidence on the relationship between firm risk and executive compensation is mixed. In addition, it remains under-explored how firms take into account both their own and other firms' risk profiles when choosing compensation policies. In this paper, I examine how firms adjust executive compensation in response to unexpected shocks to their firm risk, and compare firms in two types of industries: *more affected industries*, in which a larger fraction of firms is affected by a same shock to firm risk, and *less affected industries*, in which a smaller fraction of firms is affected by the shock.

How firms respond to shocks may depend on whether their rival firms face the same shock. Subsequent to an unexpected change in firm risk, affected firms in a more affected industry may not need to adjust executive compensation. Since they account for a majority of firms in the industry, they may be able to increase product prices to reflect any unexpected increase in marginal costs. In other words, product prices may absorb common shocks within an industry. However, affected firms in a less affected industry may need to adjust compensation to remain competitive, because idiosyncratic shocks may not be absorbed in product prices.[1] By taking into account the risk profiles of all firms in an industry, this paper helps to understand within-industry variations in executive compensation policies.

It is also important to examine the relationship between firm risk and executive compensation, since the relationship between the two is controversial both theoretically and empirically. A group of theoretical studies suggests that convex payoffs like stock option grants induce managers to take risks, because managers share in the gains but not all the losses (e.g., Jensen and Meckling [2]; Smith and

[1]Despite the fact that firms may mitigate the impact of a domestic shock by trading in foreign markets, to the extent that firms cannot fully diversify away the risk, I expect to find different responses between more affected and less affected industries. In addition, firms with market power may mitigate the impact of the shock by adjusting product prices. Thus, I expect to find a larger difference between more affected and less affected industries when both types of industries are more competitive.

Stulz [3]; Edmans and Gabaix [5]). Another group of studies note that option compensation does not unambiguously lead to more risk-taking, because, apart from increasing convexity, it also increases risk-averse managers' exposure to firm risk (e.g., Lambert *et al.* [6]; Carpenter [7]; Ross [8]). The empirical literature documents a positive effect of convexity given to managers on managerial risk-taking, but mixed evidence on the effect of option grants.[2] Unlike this literature, my paper focuses on a different question: how firms adjust managerial risk-taking incentives when firm risk changes. Existing evidence on this question is also mixed. Based on an industry-level analysis, Gormley *et al.* [15] find that firms reduce managerial risk-taking incentives after firm risk increases. They suggest that firms may want to mitigate managers' exposure to firm-specific risk. In contrast, Panousi and Papanikolaou [16] and DeAngelis *et al.* [17] show that firms increase managerial risk-taking incentives when firm risk rises. Panousi and Papanikolaou [16] argue that firms may want to mitigate underinvestment by risk-averse managers.

One challenge to the related empirical research is that risk and compensation may be jointly determined. To identify a causal relationship, I exploit sudden increases in firm risk when the National Institutes of Health (NIH) discovers the carcinogenicity of a chemical produced or used by a firm. Under a congressional act[3], the NIH formally identifies and issues a list of carcinogens to the public. Between 1980 and 2014, the NIH updated the list 13 times, resulting in a total of 267 car-

[2]See, for example, Agrawal and Mandelker [9], DeFusco *et al.* [10], Guay [4], Rajgopal and Shevlin [11], Lewellen [12], Coles *et al.* [13], and Low [14].

[3]Section 301(b)(4) of the Public Health Service Act, amended in 1978.

cinogens. Newly discovered carcinogens attract attention from the public, academia, businesses, and policy-makers. Firms producing or using those carcinogens may face increased risk of litigation regarding issues like workplace injuries, consumer product safety, and environmental pollution. The litigation uncertainty may exist for a couple of years before regulatory agencies make specific decisions, which might then lead to sizable firm value losses. For example, formaldehyde was identified in 2011 as a known carcinogen. On February 22, 2016, when the Centers for Disease Control and Prevention confirmed that formaldehyde-containing products sold by Lumber Liquidators, a flooring retailer, can cause cancer, the company's stock price plunged by 23 percent.[4]

My sample consists of 7,614 treated and control firm-year observations during the period of 1987–2013 within six-year windows around discoveries of carcinogens in 1989, 1991, 2000, 2004, and 2011.[5] To identify treated and control firms, I first match the NIH's list of carcinogens with plant-level information on toxic chemical emissions (including carcinogens and non-carcinogenic toxic chemicals) from the Toxic Release Inventory (TRI) database maintained by the U.S. Environmental Protection Agency (EPA). A plant may emit a certain chemical because it produces the chemical for sales, or uses or processes the chemical to produce other products.[6] I then map the parent companies of TRI-reporting plants to firms listed in Compustat. Around

[4]Source: http://www.cbsnews.com.

[5]I focus on these five discoveries of carcinogens due to data availability.

[6]I exclude from my control sample firms that never own a TRI-reporting plant during my sample period, because those firms may have a much lower probability of emitting carcinogens and have different characteristics from firms with TRI-reporting plants. See Faulkender and Petersen [18] for a discussion on related empirical strategies.

one-fifth of the firms in the Compustat database own TRI-reporting plants. Finally, I restrict my sample to firms with available information on executive compensation from Execucomp and Yermack [19]. My final sample includes 370 unique treated firms, or 601 treatment events.

I first verify that the discovery of carcinogens leads to an increase in firm risk. Using a difference-in-difference methodology, I show that treated firms experience a 5% increase in firms' option-implied volatility within a 12-month window around the discovery, and a 18% increase in stock return variance within a six-year window.[7] I then examine how firms adjust managerial risk-taking incentives, measured mainly by CEO flow vega, the sensitivity of a CEO's current-year compensation to stock return volatility. Treated firms increase the value of CEOs' current-year compensation, on average, by $2,859 per 0.01 increase in the firms' stock return volatility, which accounts for around 10% of the sample mean of flow vega prior to the discovery of carcinogens. The result is mainly driven by less affected industries, in which treated firms increase flow vega by around 20%. Figure 1.1 illustrates the different incentive adjustments between more affected and less affected industries. Firms adjust CEO risk-taking incentives more strongly when they are among a smaller fraction of firms in the industry facing the same shock to firm risk. One possible explanation is that product prices may absorb common shocks within an industry, but not idiosyncratic shocks. Treated firms in more affected industries may increase

[7] I find that the average cumulative abnormal returns (CARs) of treated firms is only -0.7% to -0.5% within a three-day or five-day window. One possible reason for the small CARs is a survivorship bias, since the discovery induces some plants to exit. Alternatively, the market reactions could suggest that the discovery mainly affects firm risk rather than expected firm value.

product prices to reflect any increase in marginal costs when they need to switch to new inputs or products. Thus, those firms may not need to react strongly. However, treated firms in less affected industries may need to adjust incentive contracts to remain competitive. Consistent with this explanation, I find that the increases in option-implied and stock return volatility are both driven by treated firms in less affected industries.

The increase in CEO risk-taking incentives is consistent with the argument that firms want to mitigate underinvestment by risk-averse, undiversified executives when idiosyncratic risk rises (Panousi and Papanikolaou [16]). I provide further evidence that the increase in CEO incentives is accompanied by more R&D expenditures, which is also driven by less affected industries. Also, the increase in CEO incentives is more evident in treated firms that keep producing or using the newly discovered carcinogens than those firms that stop producing or using those carcinogens. In addition, I test several alternative explanations and find no consistent evidence. First, the increase in CEO vega does not seem to be mainly driven by an increase in total pay rather than risk-taking incentives, since treated firms in less affected industries grant more options to their CEOs, but do not significantly change cash or stock compensation. This finding remains robust after 2005, when the adoption of FAS 123R reduced the accounting advantage of using options. Second, the increase in vega is not driven by firms with greater risk-shifting incentives, measured by ex-ante higher leverage or financial distress. Finally, the results are not driven by firms with weaker governance strength, measured by ex-ante lower board independence or lower active institutional ownership.

I further explore firm heterogeneity, and show that the increase in CEO incentives is more evident in treated firms producing the newly discovered carcinogens for sales than those firms using the carcinogens to manufacture other products. Furthermore, the increase in incentives is more pronounced in firms with fewer foreign sales or subsidiaries. These results suggest that compensation adjustments depend on how easily a firm can substitute or outsource carcinogen-related production. Consistent with my main findings, less affected industries also drive the subsample results.

I conduct several robustness tests. For example, I find no preexisting trends in CEO incentives prior to the shock to firm risk. In addition, my results are robust to controlling for several firm and CEO characteristics and their interactions with the shock.[8] To further account for ex-ante differences between the treated and control groups, I match treated and control firms based on firm characteristics. Furthermore, I find robust results using the number of option grants to proxy for CEO risk-taking incentives, which suggests that the results are not purely driven by mechanical changes in stock volatility. My results also remain similar when I exclude the 2004 discovery of carcinogens, which affected the most number of firms. Finally, my findings are robust to using text-based industry classifications by Hoberg and Phillips [22] to define more affected and less affected industries.

This paper contributes to the literature on the linkage between product mar-

[8]Controlling for the interaction terms helps to mitigate the concern of bad controls. Simply including a number of control variables could bias the estimation results if the shock to firm risk affects the value of control variables or the way the dependent variable depends on the control variables. See Angrist and Pischke [20], pp. 64–66, and Roberts and Whited [21] for more details on the issue of bad controls.

kets and managerial compensation. My analysis indicates that, in response to shocks to firm risk, firms adjust managerial compensation more strongly when fewer rival firms face the same shock. A group of studies explore the effect of product market competition on managerial incentives (e.g., Holmstrom [23]; Hart [24]; Schmidt [25]; Raith [26]). Unlike these studies, this paper distinguishes between product markets in which firms face common shocks to firm risk and those in which few firms face idiosyncratic shocks. Another group of studies investigates how managerial compensation depends on firm performance relative to peers (e.g., Murphy [27]; Aggarwal and Samwick [28]). This paper emphasizes unexpected changes in firm risk rather than relative performance. Also, the evidence in this paper does not seem to suggest that discoveries of carcinogens reveal bad managerial decisions in less affected industries. Furthermore, a strand of literature shows that firms strategically choose peer companies to justify their own compensation policies (e.g., Bizjak *et al.* [29]; Faulkender and Yang [30]). Instead of focusing on self-selected peers, I investigate same-industry firms that face a similar exogenous shock. Finally, the industry equilibrium models of investment and financing decisions suggest that a production technology chosen by many firms in an industry becomes a natural hedge (e.g., Maksimovic and Zechner [31]; Williams [32]). Unlike this literature, I examine firms' compensation decisions and exploit shocks to firm risk. My findings suggest that firms' compensation adjustments are determined by not only their own risk, but also by the risk profiles of other firms in an industry.

In addition, this paper contributes to existing work on the effect of firm risk on executive compensation. Building on the industry-level analysis in Gormley *et* *al.* [15], this paper uncovers additional evidence on within-industry variations in compensation adjustments. Gormley *et al.* [15] highlight the importance of firm risk in the design of compensation contracts and examine exogenous increases in firm risk due to discoveries of carcinogens. Their study assumes that all firms in more affected industries were treated and all firms in less affected industries were controls due to data availability, and finds that more affected industries reduce CEO flow vega relative to less affected industries after the discovery of carcinogens.[9] I find a similar industry-level result by replicating their empirical strategy based on my sample. In addition, using micro-level data, I show that treated firms increase CEO flow vega, and the increase is driven by treated firms in less affected industries. The results suggest that firms may increase managerial risk-taking incentives in order to mitigate underinvestment by risk-averse, under-diversified managers.

The rest of this paper proceeds as follows. Section 1 reviews the related literature and develops the hypotheses. Section 2 discusses the data. Section 3 presents the results and robustness checks. Section 4 concludes.

1.2 Related Literature and Hypotheses Development

1.2.1 Industry Equilibrium of Corporate Decisions

A body of theoretical literature explores the industry equilibrium of investment and financing choices (e.g., Maksimovic and Zechner [31]; Williams [32]; Fulghieri

[9] Gormley *et al.* [15] use the 1981–1983 industry-level National Occupational Exposure Survey to identify treated and control groups.

and Suominen [33]). Maksimovic and Zechner [31] demonstrate that the risk of a firm's technology choice is endogenously determined and depends on the equilibrium number of firms choosing each type of technology in an industry. In their paper, each firm can invest in either a technology with a known marginal cost or another technology with an uncertain marginal cost. Suppose that a given firm faces an unexpectedly high cost. If most other firms in the industry have chosen the same production technology and therefore experience the same shock in production costs, then the higher production costs will translate into a higher price of the products sold. Thus, that technology becomes a better hedge and will generate less risky cash flows. In contrast, if a firm is one of few firms in the industry facing an unexpected increase in costs, prices will not reflect the higher production costs. Hence, that technology will exhibit riskier cash flows than the other one.

Unlike their study, this paper focuses on compensation decisions rather than investment and financing policies, and investigates unexpected shocks to firm risk rather than endogenous choices of technologies. Nevertheless, the theoretical intuition in their study can be extended to develop my hypothesis. In this paper, I explore how firms adjust executive compensation following shocks that increase firm risk. Specifically, I examine firms' responses to the discovery of carcinogenicity of a chemical produced or used by the firms. Because of potential litigation and reputation concerns, affected firms may switch to non-carcinogenic or less toxic chemicals, which may result in higher marginal costs of production. If most of the rival firms in the industry face the same shock to firm risk, affected firms may be able to increase product prices to reflect any increase in marginal costs. In contrast, if only a small fraction of firms in the industry experiences the shock, these firms may not be able to affect product prices. Hence, I expect that treated firms in less affected industries would experience a larger increase in firm risk, and thus would be more responsive to the shock in adjusting executive compensation. This leads to my first hypothesis:

Hypothesis 1. *If a firm is among a small fraction of companies in the industry facing the same shock to their firm risk, the firm would more actively adjust executive compensation, compared to the case in which most of the firms in the industry face a same shock to their firm risk.*

One implicit assumption of this hypothesis is that firms cannot fully diversify away the increased risk through international trade. In my setting, treated firms might be able to mitigate the impact of the discovery of carcinogens by outsourcing their carcinogen-related production to countries with less rigid regulations than the United States. Thus, I expect the above hypothesis to hold to the extent that firms cannot fully diversify away the risk.[10] In addition, treated firms with market power may be able to adjust product prices even if they are in a less affected industry. Hence, the hypothesis is more likely to hold in competitive markets in which firms take product prices as given.

This paper is also related to literature on the relationship between product market competition and corporate outcomes. A strand of studies explores the effect of competition on managerial incentives. For instance, Holmstrom [23], Hart [24], and Hermalin [34] argue that competition reveals additional information about man-

[10] This may occur, for example, due to uncertainties about foreign trade, imperfect foreign product markets, etc.

agerial ability if firms in a product market are hit by common productivity shocks. Hermalin [34] shows that the best response to other firms providing weak (strong) managerial incentives can be to provide strong (weak) incentives. Schmidt [25] and Raith [26] suggest that competition drives down firm profits, and thus may discourage managerial efforts but may also discipline managers and boost productivity. Unlike these studies, this paper distinguishes between product markets in which firms face common shocks to firm risk and those in which few firms face idiosyncratic shocks. In another related study, Hadlock and Sonti [35] examine how revisions to firms' asbestos liabilities affect market reactions to their competitors. Unlike their paper, I exploit exogenous shocks to firm risk rather than self-reported revisions to litigation liabilities and focus on treated firms rather than their competitors.

In addition, this paper is related to the literature on relative performance evaluation and compensation peer benchmarking. A strand of literature explores whether managerial compensation is determined by firm performance relative to peers (e.g., Murphy [27]; Aggarwal and Samwick [28]). This paper examines unexpected changes in firm risk rather than relative performance, and finds no evidence that discoveries of carcinogens reveal bad managerial decisions in less affected industries. Other studies document that firms strategically choose peer companies that pay higher executive compensation to justify their own compensation policies (e.g., Bizjak *et al.* [29]; Faulkender and Yang [30]; Albuquerque *et al.* [36]). Instead of focusing on self-selected peers, I investigate same-industry firms that face a similar exogenous shock to their firm risk.

1.2.2 Firm Risk and Executive Compensation

Existing literature provides mixed evidence on the effect of firm risk on compensation decisions. Using industry-level evidence, Gormley *et al.* [15] investigate how firms adjust managerial incentives when their liability risk rises due to workplace exposure to newly discovered carcinogens. Their study assumes that all firms in more affected industries were treated, and all firms in less affected industries were controls, and shows that more affected industries reduce CEO flow vega relative to less affected industries after the discovery of carcinogens.[11] This paper extends their work by using micro-level data and exploring within-industry variations in compensation adjustments.

Another strand of studies suggests a positive effect of firm risk on managerial risk-taking incentives. For instance, DeAngelis *et al.* [17] investigate a sudden increase in downside firm risk following removal of short-selling constraints, and show that treated firms grant relatively more stock options to their executives than restricted stocks. A recent study by Panousi and Papanikolaou [16] distinguishes between systematic and idiosyncratic components of risk, and argue that top executives can hedge away their exposure to systematic risk but not to idiosyncratic risk, since they are not permitted to buy put options or short their own company's stock. They use a theoretical model to show that risk-averse, undiversified managers may underinvest in projects characterized by idiosyncratic risk, and risk-neutral, well-

[11] The treated group in Gormley *et al.* [15] consists of a set of SIC industries in which above a threshold fraction of workers is exposed to newly discovered carcinogens.

diversified shareholders may want to increase managerial risk-taking incentives to mitigate the underinvestment.[12] Consistent with their paper, existing studies on option repricing also suggest that the wedge between managers' and shareholders' optimal decisions increases with idiosyncratic risk, managerial risk aversion, and the extent of under-diversification (e.g., Hall and Murphy [38]; Chidambaran and Prabhala [39]; Ingersoll [40]).[13]

The intuition on the relationship between idiosyncratic risk and managerial incentives can be extended to this paper. Under the assumption that managers are risk-averse and under-diversified and cannot flexibly sell or hedge against idiosyncratic risk, prior studies suggest two competing hypotheses on the effect of idiosyncratic risk on managerial incentives:

Hypothesis 2a. *Firms would reduce managerial risk-taking incentives if idiosyncratic risk increases.*

Hypothesis 2b. *Firms would increase managerial risk-taking incentives if idiosyncratic risk increases.*

The two competing hypotheses have different implications from an agency theory perspective. Hypothesis 2a implies that firms cut managerial incentives to meet managers' constraints, while Hypothesis 2b suggests that firms increase incentives to maximize shareholders' value. Following the theoretical framework of

[12] Armstrong and Vashishtha [37] also assume that managers can hedge against systematic risk rather than idiosyncratic risk and find that vega encourages managers to increase systematic risk more than idiosyncratic risk. Unlike their paper, I focus on a sudden increase in idiosyncractic risk.

[13] These studies show that repricers tend to be smaller, younger firms that experienced an abrupt decline in growth and profitability. In contrast, my sample consists of larger, older firms.

Holmstrom and Milgrom [41], I consider a firm that designs incentive compensation contracts to maximize shareholders' value subject to managers' participation constraint. Since the manager is risk-averse, a sudden increase in idiosyncratic risk may raise her marginal costs of exerting efforts. Hypothesis 2a suggests that the firm would cut managerial incentives to mitigate the increase in the costs of efforts and meet managers' constraints. The finding of Gormley *et al.* [15] is consistent with this hypothesis. In contrast, Hypothesis 2b, consistent with Panousi and Papanikolaou [16], argues that the firm would give managers a greater reward of risk-taking in order to maximize shareholders' value.

Another body of literature examines the impact of option compensation on managerial risk-taking activities. In general, this literature suggests that vega (sensitivity of a manager's wealth to firm volatility) induces managerial risk-taking, while the effect of delta (sensitivity of a manager's wealth to changes in stock price; also known as pay-for-performance sensitivity) or option grants depends on managerial risk aversion, ability to hedge, and outside wealth.[14] A number of theoretical studies demonstrate that option compensation increase vega, which encourages managers to take risks, because the expected payoff of an option increases in the volatility of the underlying stock's return (e.g., Jensen and Meckling [2]; Myers [42]; Haugen and Senbet [43]; Smith and Stulz [3]; Edmans and Gabaix [5]). Other studies argue that besides vega, option grants also increase delta, which makes managers' firm-specific wealth more sensitive to changes in stock prices (e.g., Lambert *et al.* [6]; Carpen-

[14]See also Armstrong and Vashishtha [37] for a discussion on this body of literature.

ter [7]; Ross [8]). Thus, if managers are risk-averse and cannot sell or hedge against the risk associated with their options, they may be less willing to take risks. Consistent with the theoretical predictions, empirical studies show that higher vega is associated with greater managerial risk-taking activities, measured by higher stock return volatility, more R&D investments, lower capital expenditures, and higher leverage (e.g., Coles *et al.* [13]; Low [14]). There is mixed evidence on the effect of delta or option grants on managerial risk-taking (e.g., Agrawal and Mandelker [9]; DeFusco *et al.* [10]; Guay [4]; Rajgopal and Shevlin [11]), which suggests that the effect may depend on empirical values of managers' risk aversion, wealth, and hedging ability. Unlike this literature, my paper investigates how firms adjust executive compensation in response to changes in firm risk, rather than how firms use compensation contracts to induce managerial risk-taking activities.

1.3 Data

I rely on several data sources to construct my sample. First, I collect the timing of discoveries of carcinogens from the Report on Carcinogens (RoC) prepared by the National Institutes of Health (NIH). Second, to identify treated and control groups in the event of discoveries, I match the RoC data with the plant-level Toxic Release Inventory (TRI) data from the U.S. Environmental Protection Agency (EPA), which differs from existing literature. The TRI data contains plant-level annual information on toxic chemical usage and emissions (including carcinogens and non-carcinogenic toxic chemicals). Third, I use the Compustat database to obtain firm-level financial data. I aggregate the plant-level chemical data to the firm level and match the parent companies of TRI-reporting plants to firms listed in Compustat. Finally, I focus on a sample with available information on executive compensation from the Execucomp database and Yermack [19].

I focus on the discoveries of carcinogens in 1989, 1991, 2000, 2004, and 2011, due to data availability. Following Gormley *et al.* [15], I construct a pooled sample, which consists of five cohorts of treated and control firm-year observations. Each cohort is a six-year period around the discovery. In robustness tests, I use alternative estimation windows. Next, I discuss the data and sample construction in details, and present summary statistics.

1.3.1 Discoveries of Carcinogens

In this paper, I exploit discoveries of carcinogens by the NIH as exogenous variations in firm risk. Data on the timing of discoveries is available through the RoC, which is a congressionally mandated, science-based, public health document prepared by the National Toxicology Program of the NIH.[15] Section 301(b)(4) of the Public Health Service Act, amended in 1978, requires that the NIH publishes and updates a list of chemicals, either known to be or reasonably anticipated to be human carcinogens. The RoC provides important information that supports decision-making by the public, businesses, and regulatory agencies.[16]

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[15] Available at `http://ntp.niehs.nih.gov/pubhealth/roc/`.

[16] The regulatory agencies that cite the RoC include Centers for Disease Control and Prevention (CDCP), EPA, Occupational Safety and Health Administration (OSHA), Consumer Product Safety Commission (CPSC), and the Food and Drug Administration (FDA).

Between 1980 and 2014, the NIH updated the RoC 13 times, resulting in a total of 267 listed carcinogens. Table A.1 in the appendix reports the years of discoveries (column 1). On average, each edition of the RoC includes around 20 newly discovered carcinogens (column 3) and affects 185 Compustat firms (column 5). Among all discoveries, the 2004 discovery affected the most firms (643 firms), followed by the 1989 discovery (312 firms) and the 2000 discovery (245 firms). I obtain the announcement dates of the RoC by searching the news articles published by the National Institute of Environmental Health Sciences (NIEHS) and the National Center for Biotechnology Information (NCBI).[17] The dates are available since 1994 and listed in column 2.[18]

I exclude all delisted chemicals and firms affected by delisting. The number of newly discovered carcinogens presented in Table A.1 already excludes delisted chemicals. Delisting is not common. Between 1980 and 2014, only nine chemicals were once discovered as carcinogens but then delisted later (column 4). Only 54 Compustat firms in 2000 were affected by two delisted chemicals (column 6). The reasons for delisting include a low possibility of human exposure and insufficient evidence of carcinogenicity after reevaluation.

In each edition of the RoC, the NIH indicates whether a chemical is known to

[17]Sources: http://www.niehs.nih.gov/news/newsroom/releases; http://www.ncbi.nlm.nih.gov/pmc.

[18]The NIH scheduled the 2004 discovery announcement in that year but actually released it to the public in January 2005. My findings could be biased for this particular year if firms adjusted executive compensation prior to the actual announcement based on leaked information. However, in robustness tests, I exclude the 2004 discovery and find similar and even stronger results for other years of discoveries. Thus, the inclusion of the 2004 discovery only works against finding the results in this paper.

be or reasonably anticipated to be a human carcinogen. I identify newly discovered carcinogens via their first appearance on the RoC, whether or not they are known or reasonably anticipated to be carcinogens. For instance, formaldehyde was listed as a reasonably anticipated carcinogen in 1981, and then updated to be a known carcinogen in 2011. I treat formaldehyde as discovered in 1981. For robustness checks, I identify newly discovered carcinogens via their first appearance as known carcinogens, that is, I treat formaldehyde as discovered in 2011. Alternatively, I examine a subsample of chemicals that first appeared on the RoC as known carcinogens, that is, I exclude formaldehyde.

1.3.2 Toxic Chemical Emissions

A key step in my analysis is to identify treated and control firms around discoveries of carcinogens. Unlike existing literature, I use a plant-level panel database on toxic chemical usage and emissions (including carcinogens and non-carcinogenic chemicals). I match the RoC data with the TRI data from the EPA.[19] TRI data provides plant characteristics, including plant name, industry, location, chemical characteristics, such as the name of chemicals emitted by a plant and how a chemical is used, and parent company name. A firm may emit a toxic chemical because it produces the chemical for sales or distribution purpose. Alternatively, the firm may use or process the chemical as an input during its production.

TRI data has been an important resource for regulators, investors, environ-

[19] Available at https://www.epa.gov/toxics-release-inventory-tri-program/tri-data-and-tools.

mentalists, and communities to assess plant-level and firm-level environmental performance. In 1986, Congress passed the Emergency Planning and Community Right-to-Know Act (EPCRA) to inform the public about toxic chemical emissions in the local community. Under the requirements of EPCRA, all U.S. plants that meet the following reporting criteria must submit annual TRI data to the EPA: (i) The plant is in a specific NAICS industry sector, including manufacturing, mining, utility, wholesalers, etc.[20], or is owned or operated by federal government; (ii) the plant employs 10 or more full-time equivalent employees; and (iii) the plant produces, processes, or otherwise uses one of the TRI-listed toxic chemicals in quantities above threshold levels in a given year.

I match the TRI data with the RoC data using chemical CAS regristry number and chemical names.[21] Between 1987 and 2014, TRI-reporting plants emitted 610 unique toxic chemicals, including 126 carcinogens and 484 non-carcinogenic toxic chemicals. I aggregate the plant-level chemical data to the firm level, and identify parent companies of TRI-reporting plants listed in Compustat (hereafter refered to as "TRI-Compustat firms"). Specifically, I map the name of a parent company in the TRI data to a firm name in Compsutat using a string matching algorithm, then manually check each potential match to improve accuracy. I exclude parent companies with no match from my sample, including potential unmatched Compustat

[20]Specifically, TRI-covered plant-level NAICS industries include mining (NAICS 212), utilities (221), manufacturing (31–33), miscellaneous manufacturing (1119, 1131, 2111, 4883, 5417, 8114), merchant wholesalers and non-durable goods (424), wholesale electronic markets and agents brokers (425), publishing (511, 512, 519), and hazardous waste (562).

[21]The Chemical Abstracts Service (CAS) has assigned a unqiue numerical identifier to every chemical described in the open scientific literature since 1957.

firms and private firms not listed in Compustat.

During the period 1987–2014, there are 58,076 unique TRI-reporting plants (656,592 plant-year observations). Around 80% of these observations have available information on parent companies. There are 11,357 unique parent companies with TRI-reporting plants (160,815 firm-year observations), among which 2,415 are identified as Compustat firms (39,939 firm-year observations). Since 1987, discoveries of carcinogens have affected 3,718 (28.0%) out of the 11,357 TRI firms and 878 (36.4%) out of the 2,415 TRI-Compustat firms. The median TRI-Compustat firm has more TRI-reporting plants than other TRI firms but emits a lower fraction of carcinogens. Specifically, the median TRI-Compustat firm has 15 TRI-reporting plants, among which 10 plants (66.7%) emit carcinogens. The median TRI-reporting plant of a Compsutat firm emits eight toxic chemicals, among which one chemical (12.5%) is carcinogenic. In comparison, the median TRI firm has six TRI-reporting plants, among which five plants (83.3%) emit carcinogens. The median TRI-reporting plant emits six toxic chemicals, among which one chemical (16.7%) is carcinogenic.

TRI-Compustat firms account for around one-fifth of the full Compustat database. Compared to other Compustat firms, TRI-Compustat firms are, on average, two times larger and older, grow slower, have better operating performance, and spend less in R&D.[22] My final sample consists of firms with available financial and compensation data, and thus my results mostly apply to large public firms.

Around 90% of the TRI-reporting plants belong to the manufacturing sector

[22]In comparison, the Compustat firms in the S&P 1500 with available compensation data from Execucomp are, on average, four times larger in size than other Compsutat firms.

(NAICS 31–33). Figure 1.2 presents the distribution of TRI-reporting plants by the Fama-French 48 industry. The plants are concentrated in a few industries, such as construction materials, chemicals, steel works, rubber and plastic products, and fabricated products. Six Fama-French industries, including real estate, banking, entertainment, insurance, communication, and trading, have no TRI-reporting plant between 1987 and 2014. Figure 1.2 also distinguishes between plants that emit existing carcinogens (black bars) and plants that emit non-carcinogenic toxic chemicals (white bars). Around 40% of the plants emit carcinogens.

Compared to the plant-level distribution, the industry distribution of TRI-Compustat firms is less concentrated, as shown in Figure 1.3.[23] Electronic equipment, machinery, chemicals, and construction materials have more TRI firms than other Fama-French industries. However, they are not necessarily the industries with higher proportions of firms releasing carcinogens. Aircraft industry has the highest fraction of firms releasing carcinogens (84.6%). In 21 Fama-French industries, including food products, computer software, rubber and plastic products, and electronic equipment, less than 50% of the TRI firms emit existing carcinogens (blue color). In the remaining 27 industries, including aircraft, utilities, chemicals, and petroleum and natural gas, over 50% of the TRI firms emit carcinogens (black color). Figure 1.3 indicates that there are within-industry variations in carcinogen emissions. Thus, the TRI data provides an opportunity to examine the difference between treated firms in more affected industries and treated firms in less affected

[23] One reseaon is that TRI-Compustat firms are more diversified. Compared to the median Compustat firm, the median TRI-Compustat firm has one more segment or unique segment-level 4-digit SIC code.

industries.

The use of the TRI data may introduce measurement errors if plants misreport their toxic chemical emissions, but this concern is mitigated in my paper, since I examine exogenous increases in the risk of releasing certain chemicals. Furthermore, the EPA's enforcement policies give plants an incentive to accurately report toxic chemical emissions. Under EPCRA, the EPA conducts compliance inspections, investigates cases of non-compliance, and can issue a maximum civil penalty of $25,000 per violation for not reporting or misreporting emissions.[24]

1.3.3 Sample Construction

Following Gormley *et al.* [15], I construct a pooled sample, which consists of five cohorts of treated and control firm-year observations. Each cohort is a six-year period (from year $T-3$ to year $T+2$) around the discovery of carcinogens (in year T, where $T = 1989$, 1991, 2000, 2004, or 2011). For instance, Air Products & Chemical Inc. (APC) was affected by the 1989 discovery and is included in my sample for the six-year period, between 1986 and 1991.[25] I use alternative windows around the discovery in robustness tests. I include firms affected for the second or more times during my sample period and keep overlapping years across cohorts. For example, APC was affected again in 1991, and is included in my sample for the six-year period of 1988 to 1991 for the 1991 cohort, in which 1998–1991 overlap

[24]In 2001, the EPA conducted 321 compliance inspections for TRI reporting. Between 1990 and 1999, the EPA brought 2,309 administrative actions against non-compliance (DeMarchi and Hamilton [44]).

[25]I do not require a firm in my sample to survive for all six years in a given cohort.

with the four years in the 1989 cohort. For robustness checks, I exclude overlapping years, and, alternatively, split the sample between firms affected for the first time and for the second or more times.

I focus on the discoveries in 1989, 1991, 2000, 2004, and 2011, because they have available TRI data and because the first four years overlap with the years in Gormley *et al.* [15]. Restricting my sample to these five years only excludes 7% of treated firms. In robustness tests, I include other years of discoveries. My final sample is restricted to firms with available data on CEO compensation from Execucomp and Yermack [19]. I identify 370 unique treated firms, in total 601 treatment events, since some firms were affected for multiple times. A firm is identified as treated if it owns at least one plant that produces or uses and thus, emits a chemical newly discovered as a carcinogen. Among the 601 events, 231 occurs in less affected 4-digit SIC industries, in which less than or equal to 50% of the TRI firms are simultaneously affected (e.g., cosmetics manufacturing in Figure 1.4); the remaining 370 events occur in more affected 4-digit SIC industries, in which over 50% of the TRI firms are simultaneously affected (e.g., plastics manufacturing in Figure 1.4). The pooled sample between 1986 and 2013 has 2,838 treated firm-year observations.

My control sample consists of firms with plants that emit non-carcinogenic toxic chemicals. In other words, I exclude firms that never had a plant that emitted any toxic chemical above a threshold value. The rationale is that those firms may have a much lower probability of being affected than TRI firms. In addition, they differ in other firm characteristics from TRI firms. This empirical strategy follows Faulkender and Petersen [18], who show that different empirical strategies can lead to different estimation results when there is a third group besides the traditional treated and control groups, and combining the third group with the control group may bias estimation results. I identify 624 unique control TRI firms with available compensation data. The pooled sample between 1986 and 2013 has 4,776 control firm-year observations. For robustness checks, I match treated and control firms based on firm size, firm age, and SIC industry. The matched sample consists of 218 unique treated firms (1,158 firm-year observations) and 411 unique control firms (1,683 firm-year observations).

1.3.4 CEO Compensation

I collect executive compensation data from Execucomp and Yermack [19]. Execucomp covers active and inactive firms in the S&P 1500 index since 1992. Yermack's sample consists of 792 U.S. firms (5,955 firm-year observations) that appeared in the *Forbes* magazine lists of the 500 largest public U.S. corporations between 1984 and 1991.[26]

My main measure for managerial risk-taking incentives is *Flow Vega*, defined as the change in a CEO's compensation (effectively the value of option grants) during a given year for a 0.01 increase in a firm's stock return volatility.[27] I use the Black and Scholes [45] formula to value options following Core and Guay [46] and account for the 2006 change in reporting format in Execucomp following Coles *et al.* [47]. To

[26] Yermack's sample provides information on CEO age, tenure, stock ownership, cash compensation, and option grants based on firms' proxy statements, 10-K, and 8-K filings.

[27] *Flow Vega* effectively only accounts for the value of option compensation, since the value of stock or cash compensation do not change with stock return volatility.

proxy for risk-free rates, I use the Treasury rate corresponding to the actual option maturity if the option maturity is less than or equal to 10 years, and use the 10-year Treasury rate if the option maturity exceeds 10 years. Stock return volatility is calculated as the annualized standard deviation of monthly stock returns during the past 60 months.

To examine whether firms adjust other aspects of compensation besides risk-taking incentives, I collect information on individual components of compensation, including the values of option grants (*Option Compensation*), restricted stock grants (*Stock Compensation*), salary and bonus (*Cash Compensation*), and total compensation (*Total Compensation*, computed as the sum of option, stock, and cash compensation). I use logged values of these measures to mitigate the concern that CEO compensation has a skewed distribution. For robustness checks, I adopt alternative measures for managerial incentives, including the number of option grants (*Option Compensation (N)*), flow delta (*Flow Delta*, computed as the change in the value of a CEO's compensation during a given year for a 1% increase in a firm's stock prices), and vega calculated using current-year plus previous years' option compensation. I use the number of option grants to test whether there is a real effect rather than a mechanical effect driven by changes in stock prices and volatility. I use vega to account for a total wealth effect.

1.3.5 Other Variables

Before preceding to my main analysis, I test how the discovery of carcinogens affects firm risk, measured by option-implied or stock return variance. I obtain data on implied volatility from OptionMetrics, and focus on at-the-money call options with at least 90 days to expiration (following DeFusco *et al.* [10]). I take the open-interest-weighted average value of implied volatility for each firm. The tests are based on a reduced sample, because OptionMetrics is available since 1996. Stock return variance is calculated using data from CRSP.

I examine whether weakly governed firms drive the results and use the fraction of independent directors on boards and potentially active institutional ownership (Almazan *et al.* [48]) to proxy for governance strength. The first test is based on a reduced sample, because board information from the ISS (formerly RiskMetrics) database is available since 1996. In addition, I examine risk-shifting as an alternative explanation for my results. Risk-shifting incentives are measured by leverage ratio and an indicator for distress based on the Altman [49] Z-score.

In robustness tests, I control for firm and CEO characteristics that may affect a firm's design of CEO compensation. Following Guay [4], I include firm size, CEO tenure, and CEO cash compensation as controls. To address the concern of potential bad controls, I interact these controls with the shock to firm risk. In addition, I also control for cash flows, leverage, and CEO age.

Detailed definitions of all variables are included in Appendix A.1.

1.3.6 Summary Statistics

Table 1.1 reports the mean comparison results between treated and control groups during three years prior to the discovery of carcinogens. Panel A presents the results based on the full sample, which consists of 370 unique treated firms and 624 unique control firms. Compared to an average control firm, an average treated firm has a similar ex-ante market-to-book ratio, leverage, cash flows, and fraction of option grants in total CEO compensation. However, the average treated firm is larger, older, and has lower stock volatility and higher CEO vega (calculated using compensation in current and previous years). These ex-ante differences are statistically significant. For robustness checks, I include firm characteristics as controls and use a matched sample to account for these differences. Specifically, I match treated and control firms by firm size decile, age decile, and 4-digit SIC industry. Panel B reports the comparison results based on the matched sample, which consists of 218 unique treated firms and 411 unique control firms. The ex-ante differences between the two groups become generally insignificant.

Panel C presents the comparison between treated and control groups based on an industry-level measure for the shock to firm risk, similar to the measure in Gormley *et al.* [15].[28] The treated group consists of a set of more affected 4-digit SIC industries, in which over 50% of the firms are affected by the discovery of

[28] Gormley *et al.* [15] rely on the 1981–1983 industry-level National Occupational Exposure Survey to identify treated and control groups. The survey used to be available from the website of the National Institute for Occupational Safety and Health (NIOSH), but was taken down because of the age of the data and because the data does not represent current exposures in U.S. industries, according to the NIOSH.

carcinogens in a given cohort. The control group consists of another set of less affected SIC industries. Compared to the original statistics in Gormley *et al.* [15] (panel D), the industry-level statistics based on my sample are similar except for vega. The vega in my sample is around three to four times larger than the vega in their sample. One possible reason is that their sample includes earlier cohorts than my sample, and that the vega is, on average, smaller in earlier years than in later years.[29]

1.4 Results and Robustness Analyses

1.4.1 First Stage: Effect on Firm Risk

Before preceding to my main tests, I first examine the effect of the discovery of carcinogens on firm risk to verify that treated firms face a material increase in their risk.

1.4.1.1 Option-implied and Stock Return Volatility

I use a difference-in-difference methodology and measure firm risk by option-implied variance as well as realized stock return variance. A firm is included in the analysis if it is listed in Compustat and owns at least one plant that emits toxic

[29] Gormley *et al.* [15] examine the discoveries in 1985, 1989, 1991, 2000, and 2004. I focus on the discoveries in 1989, 1991, 2000, 2004, and 2011. The average vega for the pre-1992 period, calculated using data from Yermack [19], is around \$10,000, while the average vega for the post-1992 period, based on the Execucomp data, is over \$100,000. This is partially because only options granted in a given year, but not previous years, are available in Yermack's sample, and thus option grants in previous years are approximated by options granted in the last year for the pre-1992 period.

chemicals (including carcinogens and non-carcinogens) during the sample period. The sample consists of several cohorts of treated and control observations during a window around the discovery of carcinogens. To analyze implied volatility, I use a 12-month window (from 180 days before to 180 days after the discovery announcement date) of firm-date observations. Around 60% of the firms in my sample have available information on implied volatility. To examine stock volatility, I construct a six-year window (from three years before to two years after the year of discovery) of firm-year observations. In addition to an ummatched sample, I construct a matched sample by firm size decile, age decile, and 4-digit SIC industry. I use the following specification:

$$Volatility_{ict} = \beta_0 + \beta_1 Discovery_{ict} + \alpha_{ct} + \omega_{ic} + \epsilon_{ict}, \tag{1.1}$$

where i denotes firm, c denotes cohort, and t denotes time (date or year). $Volatility_{ict}$ is one of the measures for firm risk (*Stock Volatility*, computed as the annualized sum of squared daily stock returns, or *Implied Volatility*, computed as the open-interest-weighted average of annualized daily option-implied volatility based on at-the-money call options with at least 90 days to expiration) for firm i in cohort c in time t. $Discovery_{ict}$ is an indicator equaling one, if the NIH has discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by firm i as of time t in cohort c. To account for unobserved heterogeneity over time and across firms, and to allow the heterogeneity to vary across cohorts, I include a month- or year-cohort fixed effect and a firm-cohort fixed effect, denoted by α_{ct} and ω_{ic}.[30]

The results from estimating Equation (1.1) are presented in the appendix (panel A of Table A.2). Subsequent to the discovery of carcinogens, treated firms experience significant increases in option-implied volatility and stock return variance. Based on the matched sample, annualized daily option-implied volatility (*Implied Volatility*), on average, rises by 0.019 during the 12 months around the discovery (column 1), which accounts for 5% of the sample mean (0.36) or 12% of the sample standard deviation (0.16) of implied volatility prior to the discovery. Annualized stock variance (*Stock Volatility*), on average, increases by 0.033 (column 4), which accounts for 18% of the sample mean (0.18) or 16% of the sample standard deviation (0.21). The results are statistically significant at the 1% level.

The increases in implied volatility and stock volatility are driven by treated firms in less affected 4-digit SIC industries, in which less than or equal to 50% of the firms are simultaneously affected. On average, implied volatility increases by 0.042 and stock volatility increases by 0.034 for treated firms in less affected industries, both significant at the 5% level (columns 2 and 5). In contrast, there is no statistically significant change in either implied or stock return volatility in more affected industries (columns 3 and 6). One possible explanation is that treated firms in more affected industries may be able to increase product prices to reflect any potential increase in marginal costs due to switching to new inputs or products.[31]

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[30] For example, consider a firm affected by the discovery of carcinogens in 1989, then again affected in 1991. The firm-cohort fixed effects treat the firm as two separate entities for the period around 1989 and the period around 1991.

[31] Alternatively, litigation and reputation concerns may induce some treated firms to exit, thus,

An ideal natural experiment would impose a significant impact on firm risk but zero impact on expected firm value, so that the results can be interpreted as solely driven by changes in risk rather than by changes in expected value. The discovery of carcinogens can negatively affect expected firm value. To examine the effect on firm value, I estimate cumulative abnormal returns (CARs) around the discovery announcement date using an event study methodology.[32] Treated firms only experience an average three-day CAR of -0.52% and an average five-day CAR of -0.75% (panel B of Table A.2). The market reactions suggest that the discovery of carcinogens may mainly affects firm risk rather than expected firm value.[33] In addition, I find that treated firms experience an increase in implied volatility skew, defined as the implied volatility of out-of-money put options minus the implied volatility of at-the-money call options (Xing *et al.* [50]). The result is mainly driven by less affected industries, in which implied volatility skew increases by around 40% of its subsample mean. These evidence suggests that treated firms in less affected industries experience a significant increase in left tail risk. For brevity, the results are not reported.

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the remaining treated firms may generate riskier cash flows. In an untabulated table, I find that the average exit rate of treated firms' plants increases by 5% or doubles the sample mean prior to the discovery.

[32]The event study is based on a reduced sample of 1,191 treated observations due to availability of announcement dates and data on stock returns. I use CRSP value-weighted returns to proxy for market returns and estimate CARs using the Fama-French three-factor model.

[33]In addition, the small CARs may be partially explained by a survivorship bias, since discoveries of carcinogens induce some plants to exit.

1.4.1.2 Anecdotal Cases and Medical Research

In this section, I provide evidence on the mechanisms of the effect on firm risk. I briefly discuss two anecdotal cases and examine related medical research subsequent to the discovery of carcinogens.

In practice, firms emitting newly discovered carcinogens may face litigation issues related to employee health, product safety, pollution, and investor losses. The following anecdotal cases suggest that the litigation uncertainty may exist for several years until regulatory agencies make decisions for specific industries or firms. For instance, asbestos was first listed in 1980 as a known carcinogen. In 1986, the Occupational Safety and Health Administration (OSHA) reduced the legal standards required in claims for workplace asbestos injuries, making it easier for plaintiffs to recover on these claims, and thus leading to an increase in the number of asbestos claims.[34] General Motors manufactured asbestos-containing brake linings from the 1930s through 1980s. However, the first asbestos claim against the company was in the 1990s, leaving a 10-year gap between the discovery of asbestos as a carcinogen and the actual litigation. By 2009, there were approximately 29,000 pending asbestos worker injury claims against General Motors, accounting for over $600 million liabilities.[35] In addition to workplace exposure, another example is consumer exposure to carcinogens like formaldehyde. Formaldehyde was first listed in 1981

[34]OSHA. 29 CFR Parts 1910 and 1926, Occupational Exposure to Asbestos, Tremolite, Anthophyllite, and Actinolite; Final Rules. 51 FR 22612-22790.

[35]Source: https://www.crowell.com/Practices/Bankruptcy-Creditors-Rights/History-of-Asbestos-Bankruptcies/.

as a reasonably anticipated carcinogen, then updated to a known carcinogen in 2011. On February 22, 2016, when the Centers for Disease Control and Prevention (CDCP) confirmed that the formaldehyde-containing products sold by Lumber Liquidators, a flooring retailer, can cause cancer, the company's stock price plunged by 23 percent.[36] In this case, there was a gap between the discovery and its material consequence on firm value. The announcement by the regulatory agency (CDCP) made it likely for consumers to sue the firm, which translated into sizable value losses.

The previous section provides evidence for increased firm risk in both a shorter period (three months) and a longer period (two years) following the discovery of carcinogens, which suggests that firm risk may keep increasing for two years. Alternatively, firm risk may jump in the first one or two months and then stop increasing. To distinguish between these two potential patterns, I explore related medical research. I collect a sample of 520 chemicals that were discovered as carcinogens between 1989 and 2011, and search the articles that mention each chemical during a six-year period around the discovery via Google Scholar. In an untabulated table, I find a significant increase in the number of articles about a chemical once discovered as a carcinogen. The average annual increase is around 3%, which accounts for around 20 more articles per year. In addition, the magnitude gradually increases from the year of discovery to two years after the discovery. This evidence suggests that the discovery of carcinogens attracts continuous attention from the academia

[36]Source: http://www.cbsnews.com.

in two years, and are consistent with the former hypothesis about changes in firm risk.

1.4.2 Main Results: Effect on CEO Risk-taking Incentives

1.4.2.1 Effect on Flow Vega

To test how firms adjust CEO risk-taking incentives subsequent to the discovery of carcinogens, I use a difference-in-difference methodology and estimate the following baseline model:

$$Flow\ Vega_{ict} = \gamma_0 + \gamma_1 Discovery_{ict} + \alpha_{ct} + \omega_{ic} + \epsilon_{ict}, \tag{1.2}$$

where i denotes firm, c denotes cohort, and t denotes year. $Flow\ Vega_{ict}$ is the sensitivity of a CEO's current-year compensation to stock return volatility for firm i in cohort c in year t. $Discovery_{ict}$ is an indicator equaling one, if the NIH has discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by firm i as of year t in cohort c. Following Gormley *et al.* [15], I include year-by-cohort fixed effect and firm-by-cohort fixed effect, denoted by α_{ct} and ω_{ic}, to account for unobserved heterogeneity across years and firms, and allow the heterogeneity to vary across cohorts. I use alternative fixed effects in robustness tests.

I do not include time-varying control variables due to potential concern of bad controls. Carcinogenicity discoveries may affect the control variables or the way flow vega depends on the controls, and therefore may bias the estimate of γ_1. In robustness tests, I control for firm size, CEO tenure and CEO cash compensation, and further address the concern by interacting the controls with the discovery indicator. In addition, I exclude firms that replace their CEOs in the year prior to the discovery of carcinogens, because vega before and after the shock correspond to different CEOs and thus are less informative.

To examine whether firms' executive compensation adjustments depend on both their own and other firms' risk profiles, I split my sample between two types of industries: the more affected 4-digit SIC industries, in which more than 50% of the firms face the same shock to firm risk due to the discovery of carcinogens, and the less affected 4-digit SIC industries, in which less than or equal to 50% of firms face the shock. For example, in 2004, plastics manufacturing (SIC 2821) is classified as a more affected industry, in which 68% of the firms are affected by the discovery in that year; at the same time, cosmetics manufacturing (SIC 2844) is classified as a less affected industry, in which only 6% of the firms are affected (Figure 1.4). In robustness tests, I adopt alternative threshold fractions to split the sample.

Table 1.2 presents the main results. Treated firms in less affected industries significantly increase CEO flow vega subsequent to the discovery of carcinogens (column 2). On average, those firms increase incumbent CEOs' value of current-year compensation (effectively the value of option grants) by \$6,018 per 0.01 increase in the firms' stock return volatility, which accounts for approximately 20% of the subsample mean (26,756) or standard deviation (32,529) of flow vega prior to the discovery. The coefficient estimate is statistically significant at the 5% level. In contrast, treated firms in more affected industries make no significant change to flow vega (column 3). Based on the full sample, treated firms significantly increase the value of CEOs' current-year compensation by $2,859 per 0.01 increase in the firms' stock return volatility, which accounts for approximately 10% of the sample mean or standard deviation (column 1). In unreported tables, I find that the results are robust to including industry-by-year fixed effects, which further accounts for unobserved heterogeneity across industries that may vary over time.

In addition, I adopt an alternative specification to compare less affected and more affected industries. I regress flow vega on the discovery of carcinogens and the interaction between the discovery indicator (*Discovery*) and an indicator for more affected industries (*More Affected Industry*). Unlike the previous subsample tests, this specification does not allow the year-by-cohort fixed effect to vary between less affected and more affected industries. Thus, I focus on subsample tests in later analyses. Nevertheless, the interaction term is significantly negative after controlling for unobserved heterogeneity across industries (column 6). Furthermore, as shown in a later table, the interaction term remains significant using a matched sample, which indicates a robust difference between the two types of industries.

The above evidence is consistent with Hypothesis 1, which implies that firms take into account not only their own but also other same-industry firms' risk profiles in designing an executive compensation contract. One possible explanation is that product prices may absorb common shocks within an industry, but not idiosyncratic shocks. Treated firms in more affected industries may increase product prices to reflect any increase in marginal costs if they have to switch to new inputs or products.

However, treated firms in less affected industries may not be able to increase product prices, and thus they may need to adjust CEO risk-taking incentives to remain competitive.

The results in Table 1.2 also explain why I arrive at a seemingly opposite sign to an existing study by Gormley *et al.* [15], which suggests a reduction in CEO flow vega when firm risk increases. Based on an industry-level workplace exposure survey, their study assumes that the treated group consists of all firms in more affected SIC industries, in which above a threshold fraction of workers is exposed to newly discovered carcinogens. They assume that the control group consists of all firms in less affected SIC industries. Their study shows that more affected industries reduce CEO flow vega relative to less affected industries. I replicate their empirical strategy using my sample and find similar results (column 4). Specifically, I replace the firm-level indicator for the discovery of carcinogens (*Discovery*) in Equation 1.2 with an industry-level indicator (*Discovery (Industry)*), which equals one for all firms within 4-digit SIC industries in which over 50% of the firms have been affected by the discovery as of a given year in a given cohort. More affected industries give incumbent CEOs, on average, \$3,319 less current-year compensation per 0.01 increase in the firms' stock return volatility than less affected industries.[37] Building on their industry-level analysis, this paper provides additional evidence on within-industry variations in compensation adjustments. My findings indicate that treated

[37] One minor difference between my industry-level estimates and that of Gormley *et al.* [15] is that they find a larger difference between more affected and less affected industries. One possible reason is that they consider earlier cohorts such as 1985, in which period the chemical data used in this paper is not available. The difference between more affected and less affected industries may be larger in those earlier periods.

firms increase CEO flow vega, which is driven by less affected industries. I explore the rationale for the increase in CEO incentives in the next section.

The above analysis also notes that identifying treated and control observations at a fine level is important. Figure 1.5 illustrates the differences between the firm-level and the industry-level identification strategies. Panel A presents the number of Compustat firms affected by the discovery of carcinogens in each cohort (excluding observations without available financial data and CEO compensation data). Panel B illustrates measurement errors resulted from identifying treated and control groups at the 4-digit SIC industry level. The number of unidentified treated observations using the industry-level measure (striped bars) accounts for around 40% of the treated observations in panel A. Another type of measurement errors is that the industry-level measure identifies some control firms as treated. Those observations (white bars) are equivalent to 50% of the number of treated observations in panel A.[38] The issue of measurement errors would be more severe if the number of treated firms is closer to the number of control firms within an industry, that is, if there are within-industry variations in the choice of chemicals. In the 1991, 2000, and 2011 cohorts, the total number of incorrectly identified observations (striped bars plus white bars) exceeds correctly identified treated observations (gray bars).

One potential concern with natural experiment studies is sample selection bias. My results might be subject to this concern if few control observations are comparable to treated observations. In previous tests, I use fixed effects and control

[38]This analysis uses 50% as a threshold value to construct the industry-level measure. Lowering the threshold value would lead to a decrease in the number of unidentified treated observations but an increase in false treated observations.

variables to account for heterogeneity between treated and control groups, but the sample selection bias may still exist if the two groups have different distributions of firm characteristics. I address this concern by matching the two groups by firm size deciles, age deciles, and 4-digit SIC industries. Around 60% of treated firms can be successfully matched based on these criteria. While using additional or more rigid matching criteria may improve matching quality, it would sacrifice power of the tests. Matching by the above three criteria actually generates a comparable treated and control sample in various ex-ante firm characteristics, such as firm size, stock returns, stock volatility, delta, and vega (panel B of Table 1.1). Table 1.3 shows that my findings are robust to using the matched sample.

1.4.2.2 Effect on Composition of Compensation

I further analyze how firms adjust the composition of CEO compensation. Table 1.4 presents the results from regressing CEO total compensation, cash compensation (salary plus bonus), the value of option grants, and the value of restricted stock grants on the discovery of carcinogens. I use the logged values for all dependent variables, since the distribution of CEO compensation is heavily skewed to the right. For each dependent variable, I conduct the test based on the full sample and subsamples of less affected and more affected industries.

Consistent with the increase in flow vega, I find that treated firms significantly increase the value of options grants (column 1 of panel B), which translates into an increase in total compensation (column 1 of panel A). The increases in option compensation and total compensation are mainly driven by less affected industries (columns 2 and 3 of both panels). In contrast, there is no significant change in the value of restricted stocks or cash compensation (columns 4–6 of both panels).[39]

The above results provide further support for the different responses to changes in firm risk between more affected and less affected industries. In addition, the results suggest that the increase in CEO vega does not seem to be mainly driven by an increase in total pay rather than risk-taking incentives. The results for the composition of CEO compensation also hold for the subsample of 2011 cohort, after the adoption of FAS 123R in 2005 reduced the accounting advantage of using options (Table A.3).

1.4.3 Interpretation

In the previous section, I provide evidence that firms give CEOs more risk-taking incentives when they face greater firm risk, which is consistent with Hypothesis 2b. A possible interpretation is that risk-averse, under-diversified managers may underinvest in valuable risky projects, and thus firms want to mitigate the under-investment and maximize shareholders' value (Panousi and Papanikolaou [16]). In this section, I investigate the effect of the discovery of carcinogens on investments and other corporate outcomes, and test alternative explanations.

[39] Around a quarter of the sample firms report other types of CEO compensation in addition to cash, restricted stock, and option compensation, which, on average, account for around 0.7% of total CEO pay. In an untabulated table, I find that treated firms make no significant adjustment to other CEO compensation.

1.4.3.1 Effect on Investments, Leverage and Cash Holdings

I examine how R&D investments and capital expenditures change following the discovery of carcinogens and present the results in Table 1.5. If firms increase CEO risk-taking incentives in order to mitigate underinvestment, one would expect that treated firms do not experience a significant decline, or even increase their investments, especially in less affected industries. Consistent with this hypothesis, I find that treated firms, on average, increase the ratio of R&D investments to total sales by 0.2 percentage point (column 1 of panel A). Similar to previous findings, the effect on R&D is also driven by less affected industries. Treated firms in less affected industries increase R&D investments by 0.5 percentage point (column 2), while their counterparts in more affected industries make no significant change (column 3). In addition, the increase in R&D investments is significantly larger among treated firms that increase CEO flow vega after the discovery of carcinogens. A one standard deviation greater CEO flow vega is associated with a 0.1 percentage-point larger increase in the ratio of R&D investments to total sales (column 4), which is also more evident in less affected industries, in which the magnitude is 0.7 percentage point (column 5).

The effect on capital expenditures is less evident (panel B). In less affected industries, an increase in CEO flow vega after the discovery of carcinogens is associated with a significant reduction in the ratio of capital expenditures to total assets (column 5), which implies a substitution effect between R&D and capital investments. Relatively more investment in R&D and less investment in capital expenditures are typically viewed as riskier investment choices (e.g., Coles *et al.* [13]).[40]

In addition, I distinguish between treated firms that keep producing or using newly discovered carcinogens and their counterparts that abandon those carcinogens. Keeping the newly discovered carcinogens may reflect a riskier investment or production decision. Consistent with the hypothesis that firms want to mitigate potential underinvestment, I find that the increase in CEO flow vega is more evident in treated firms that keep the newly discovered carcinogens. These results are unreported for brevity.

The above analysis shows that the increase in CEO risk-taking incentives is accompanied by an increase in risky investments in treated firms, consistent with Hypothesis 2b. Hence, my results for vega indicate that firms give CEOs a greater reward of risk-taking to mitigate underinvestment and maximize shareholders' value when idiosyncratic risk increases, contrary to Gormley *et al.* [15], who suggest that firms cut incentives to reduce CEOs' exposure to firm-specific risk and meet participation constraint.

Furthermore, I explore the effect of discoveries of carcinogens on other corporate outcomes. For the average treated firm, a one standard deviation greater flow vega is associated with a 1.3 percentage-point larger reduction in leverage ratio (column 1 of Table 1.6). The reduction in leverage ratio is more evident in less affected industries, in which the magnitude is 2.0 percentage points (column 2). In addition,

[40] In contrast, Table 1.5 shows that captial expenditures rise among treated firms in more affected industries that increase CEO flow vega (column 6 of panel B), which is consistent with the finding in Gormley *et al.* [51] that more affected industries increase acquistions relative to less affected industries after the discovery of carcinogens.

for the average treated firm in less affected industries, a one standard deviation increase in CEO flow vega is accompanied by a 2.4 percentage-point larger increase in the ratio of cash to total assets (column 5). The average treated firm in more affected industries does not experience significant change in leverage ratio or cash holdings (columns 3 and 6). One possible explanation is that CEOs at treated firms in less affected industries may want to reduce their exposure to firm-specific risk and increase precautionary savings when they are given more risk-taking incentives following an increase in tail risk.

1.4.3.2 Alternative Explanations

In this section, I provide evidence that my results do not seem to be driven by weak governance or risk-shifting incentives.

First, I investigate whether the compensation adjustments are dominated by firms with weaker governance strength. An alternative explanation to the increase in flow vega is that boards may be captured by the CEOs and raise CEO compensation at the expense of shareholders' value. However, Table 1.7 shows that the increase in flow vega is more evident in firms with ex-ante higher board independence, measured by the percentage of independent directors on board in the year prior to the discovery of carcinogens (columns 1 and 2 of panel A). In addition, the increase in flow vega is driven by firms with ex-ante higher potentially active institutional ownership, computed as in Almazan *et al.* [48] (columns 5 and 6).[41] This evidence suggests that

[41] Following Almazan *et al.* [48], potentially active institutional investors include investment companies and independent investment advisors. They are more likely to use shareholder proposals

the results do not seem to be driven by weakly governed firms. Within the subsample of firms with more independent boards or higher institutional ownership, the increase in flow vega is more pronounced in less affected industries (columns 3, 4, 7, 8), which implies that the different responses between less affected and more affected product markets cannot be purely explained by board structure or institutional ownership.

Second, I test the alternative explanation that firms may increase CEO risk-taking incentives to shift risk to debtholders, which predicts that firms closer to distress or highly levered prior to the discovery of carcinogens would drive the subsequent increase in CEO risk-taking incentives. Contrary to this prediction, I find that the increase in flow vega is driven by firms with lower leverage ratios in the year prior to the discovery (columns 1 and 2 of panel B). Also, the increase in flow vega is driven by ex-ante non-distressed firms, measured by the Altman [49] Z-score (columns 5 and 6). These results indicate that risk-shifting does not seem to be a main reason that treated firms increase CEO risk-taking incentives. Within the subsamples of lower-levered or non-distressed firms, treated firms in less affected industries drive the increase in flow vega (columns 3, 4, 7, 8), which suggests that the different responses between product markets cannot be attributed to differences in capital structure or financial health.

Finally, in previous tests, I exclude firms that replace their CEOs around the discovery of carcinogens, because the vega of different CEOs is less comparable. However, firms might also find it more efficient to replace the CEO and provide a

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and other mechanisms to monitor managers than potentially passive institutional investors, such as banks and insurance companies, which are more likely to "vote by feet."

new compensation contract rather than adjust the existing CEO's compensation.[42] I find that the increase in flow vega is robust to extending my sample to include new CEOs, but is mainly driven by firms that keep their CEOs after the discovery of carcinogens (Table A.4). To further analyze whether the increase in flow vega is an efficient decision to retain key executives or is driven by entrenched CEOs, I examine CEO turnover rates. The management entrenchment hypothesis predicts a lower turnover rate in treated firms that increase vega than in treated firms that do not increase vega. However, I find the opposite based on my sample. The average CEO turnover rate is 10.6% for the treated firms that increase vega and 6.7% for the treated firms that do not increase vega. The difference is statistically significant at the 5% level. The average CEO forced turnover rate (if CEOs are replaced before the age of 60) is 3.6% for the treated firms that increase vega and 1.9% for their counterparts. The difference is significant at the 10% level. This evidence suggests that my results are not driven by management entrenchment.

1.4.4 Firm Heterogeneity

The effect of the discovery of carcinogens may vary across firms. First, I distinguish between treated firms that use the newly discovered carcinogens to produce other products and those that produce the carcinogens for sales or distribution. It can be easier for a firm to substitute the carcinogen for a non-toxic chemical or conduct renovations if the carcinogen is a catalyst instead of a final product. Thus, the

[42] For example, some firms may be able to find a less risk-averse CEO, and thus do not need to give as many risk-taking incentives as to the incumbent CEO.

increase in litigation uncertainty may be greater for carcinogen producers than for carcinogen users. Hence, I expect that the effect on flow vega would be more evident for carcinogen producers. To test this hypothesis, I split the treated sample between carcinogen users and carcinogen producers. Table 1.8 presents the results. Consistent with the hypothesis, the discovery of carcinogens leads to a significant increase in CEO flow vega in carcinogen producers (column 2) but no significant change in flow vega by carcinogen users (column 1). In addition, the increase in flow vega is also driven by carcinogen producers in less affected industries (columns 3 and 4).

Second, I compare domestically focused firms with foreign-focused firms. Similar to the analysis on investments, I expect that the effect on flow vega would be less evident in foreign-focused firms, since they may face lower costs to outsource the carcinogen-related production. Foreign-focused firms may move the affected plant or product line to other countries with less rigid environmental or product safety regulations, which may mitigate the increase in firm risk. Consistent with this hypothesis, I find that more domestically focused treated firms significantly increase flow vega (by \$3,641; column 6), while more foreign-focused treated firms do not significantly adjust flow vega (column 5), where foreign focus is measured by the ratio of foreign sales to total sales in the year prior to the discovery of carcinogens. Similar to previous findings, the increase in flow vega is more pronounced in less affected industries within the domestically focused subsample (columns 7 and 8). The results in Table 1.8 indicate that the effect on CEO risk-taking incentives is mainly driven by firms with less substitutable carcinogens.

Finally, I test whether the results depend on industry competition. As discussed in previous sections, treated firms in more affected industries may face smaller potential losses, since they may increase product prices to mitigate any increase in marginal costs due to switching to new inputs or products. However, one implicit assumption is that firms take product prices as given. Firms with market power may mitigate the impact of discovery of carcinogens by adjusting product prices. Thus, I expect a larger increase in CEO incentives and a greater divergence in treated firms' responses between more affected and less affected industries when firms operate in more competitive industries. Consistent with these hypotheses, I find that the increase in CEO flow vega is mainly driven by treated firms in industries with a lower Herfindal-Hirschman index and thus more intense competition in the year before the discovery. Treated firms in more competitive industries significantly increase flow vega (by \$5,074; column 4 of Table 1.9), while treated firms in less competitive industries do no significantly change flow vega (column 1). In addition, the increase in flow vega is more evident in less affected industries within the subsample of more competitive industries (columns 5 and 6).

1.4.5 Robustness Analyses

I conduct a number of robustness checks. First, the results for vega are robust to controlling for firm size, CEO tenure, and CEO cash compensation (columns 1–3 of Table A.5). In untabulated tables, I find no significant effect of the discovery of carcinogens on any of these control variables, which mitigates the concern that time-varying controls may bias the estimates. To further address the concern of bad controls, I include the interactions between the control variables and the discovery indicator. The results for vega are similar and unreported for brevity. All estimates on the interaction terms are insignificant except for the interaction between cash compensation and the discovery indicator. The discovery of carcinogens leads to an average increase in flow vega, but the magnitude diminishes with higher levels of CEO cash compensation.[43] This finding is consistent with Guay [4], who suggests that CEOs with greater cash compensation have a larger capacity to invest outside the firm and diversify their portfolios. Those CEOs may be less likely to underinvest when idiosyncratic risk increases, and thus may not need as much risk-taking incentives as other CEOs. In addition, I control for the lagged value of flow vega to account for time-series correlation of option grants and find robust results (columns 4–6). Furthermore, my results are robust to controlling for cash flows, leverage, and CEO age, in addition to their interactions with the discovery indicator.

Second, I use alternative measures for managerial incentives. Treated firms in less affected industries significantly increase the number of option grants after the discovery of carcinogens (panel A of Table A.6), which suggests a real effect on managerial incentives rather than a pure mechanical effect. Since flow vega is a function of stock return volatility, any change in volatility will translate into a change in flow vega by definition. In addition, the discovery of carcinogens also leads to an increase in flow delta (panel B). Furthermore, I calculate vega using a

[43] In an untabulated table, I find that for one standard deviation higher of the logged value of cash compensation (0.60, based on my sample), the marginal increase in flow vega diminishes by $0.60 \times 4.197 \times 1{,}000$, which is \$2,520.

CEO's complete portfolio of compensation, including compensation in a given year and previous years, to account for a total wealth effect.[44] In an untabulated table, I find that the increase in flow vega translates into a significant increase in vega. Finally, the results remain robust using the change in CEO flow vega as dependent variable.

Third, I estimate the effect of the discovery by year. My results may be subject to the reversal causality concern if, for instance, firms with ex-ante lower CEO risk-taking incentives could lobby to prevent the NIH from establishing the carcinogenicity of a chemical emitted by those firms. To address this concern, I replace *Discovery* in Equation (1.2) with four indicators: *Discovery(-1)*, an indicator equaling one, if the NIH will discover the carcinogenicity of a chemical emitted by a given firm in the following year; *Discovery(0)*, an indicator for the discovery of carcinogens in the current year; *Discovery(+1)*, an indicator for the discovery in the last year; and *Discovery(+2)*, an indicator for the discovery occurred two years ago. The estimate on *Discovery(-1)* is statistically insignificant and smaller in magnitude than the estimates on indicators for later years (column 1 of Table A.7). In addition, the increase in flow vega two years after the discovery is significant at the 5% level and largest in magnitude compared to other years. The results remain similar when I use the number of option grants as dependent variable (column 4), and are mainly driven by the subsample of less affected industries. These evidence mitigates the reversal causality concern.

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[44] CEO vega is computed as flow vega plus the vega based on the option grants in previous years, minus the vega based on options exercised by the CEO.

Fourth, I use several alternative specifications to Equation (1.2). For example, the difference between more affected industries and less affected industries is robust to alternative cutoffs. Both the magnitude and statistical significance of the increase in flow vega are larger when the fraction of affected firms in an industry is lower (Figure A.1). In addition, I find similar results using alternative industry classifications, including 3-digit SIC codes and text-based industry classifications by Hoberg and Phillips [22] (Table A.8). Furthermore, the results remain robust when I use firm and year fixed effects and when I cluster standard errors at the firm level instead of the industry level. The firm-level clustering accounts for potential covariance among firm outcomes and over time, while the industry-level clustering accounts for covariance among firm outcomes within industry and over time. I also find similar results using alternative estimation windows, such as a four-year (year $T-2$ to year $T+1$) or eight-year period (year $T-4$ to year $T+3$) around the discovery of carcinogens (in year T). These results are unreported for brevity.

Finally, my results remain robust to several other subsample and extended sample tests. For example, I conduct placebo tests based on 500 bootstrapped samples where treated and control firms are randomly mixed up. The value of the original estimate lies on the tail of density distribution of placebo estimates (Figure A.2). In addition, the 2004 discovery of carcinogens affected the most firms (around 40% of total number of treated firms) among all discoveries in my sample, and had a several months' gap between scheduled and actual announcement, which could introduce biases if there was information leakage and firms responded prior to the actual announcement. However, the increase in flow vega is actually more evident once I exclude the 2004 cohort (Table A.9). In other words, the inclusion of the 2004 cohort only works against my main findings. Also, I extend my sample to 1994, 1998, and 2002 cohorts and find similar results. In addition, my findings remain similar when I exclude overlapping years across cohorts. Furthermore, I distinguish between firms affected for the first time in my sample and firms repeatedly affected, and find robust results in both subsamples. Finally, the results remain robust when I identify newly discovered carcinogens via their first appearance as known carcinogens instead of reasonably anticipated carcinogens, or focus on a subsample in which I exclude chemicals that first appeared on the carcinogen list as reasonably anticipated carcinogens. These results are untabulated for brevity.

1.5 Conclusion

This paper shows that firms' executive compensation choices depend on both their own and other firms' characteristics. I exploit exogenous increases in firm risk when a chemical produced or used by a firm is discovered as a carcinogen. Using a difference-in-difference methodology, I find that treated firms increase CEO risk-taking incentives. This result is mainly driven by treated firms in less affected industries, in which a smaller fraction of the firms face the shock to firm risk.

The discovery of carcinogens leads to an average 5% to 18% increase in treated firms' option-implied or realized stock volatility, which indicates a material change in firm risk. I show that treated firms significantly increase CEO risk-taking incentives, measured mainly by flow vega. Subsequent to the discovery of carcinogens, treated firms increase CEOs' current-year compensation by a \$2,859 per 0.01 increase in stock return volatility, which accounts for around 10% of the sample mean of flow vega. The increase in flow vega is mainly driven by less affected industries. Treated firms in those industries increase flow vega by 20%, which indicates that affected firms significantly adjust executive compensation when most of their rival firms in the industry do not face the same shock to firm risk. Furthermore, I show that treated firms in less affected industries increase option grants, but do not significantly change other components of compensation. My findings suggest that compensation adjustments depend on the risk profiles of all firms in an industry. When a firm is the only one or among few companies in an industry that face a same shock to firm risk, it significantly adjusts managerial incentives to remain competitive. In contrast, when a firm is among many companies in an industry that face the same shock, it may not react strongly.

By exploring within-industry variations in executive compensation adjustments, this paper provides additional evidence to the existing literature on the effect of firm risk on compensation decisions. I extend the industry-level analysis by Gormley *et al.* [15], who find that more affected industries reduce managerial incentives relative to less affected industries following the discovery of carcinogens. I show that firms give managers a greater reward of risk-taking subsequent to the discovery, and the incentive adjustments are driven by less affected industries. My findings are consistent with the prediction of Panousi and Papanikolaou [16] that firms may give risk-averse, under-diversified managers more risk-taking incentives to mitigate underinvestment when idiosyncratic risk rises. I provide supporting evidence that the increase in CEO incentives is accompanied by an increase in risky investments, measured by R&D expenditures. Consistent with my main results, the effect on R&D is also more evident in less affected industries. In addition, I test several alternative explanations such as risk-shifting and weak governance, and find no consistent evidence.

In addition, I show that the increase in CEO incentives is more evident in carcinogen producers than carcinogen users. Also, the increase in CEO incentives is more pronounced in firms with fewer foreign subsidiaries or sales. The subsample results are also driven by less affected industries.

My results are robust to several alternative specifications. For instance, I find no preexisting trends in CEO incentives prior to the discovery of caricnogens. In addition, my results are robust to controlling for firm and CEO characteristics, in addition to their interactions with the discovery. The results also remain similar, based on a matched sample, which further accounts for ex-ante differences between the treated and control groups. Furthermore, I find robust results using alternative measures for CEO incentives, including the number of option grants. I also find similar results after excluding the 2004 discovery, which affected the most number of firms. Finally, my results are robust to alternative cutoffs between more affected industries and less affected industries.

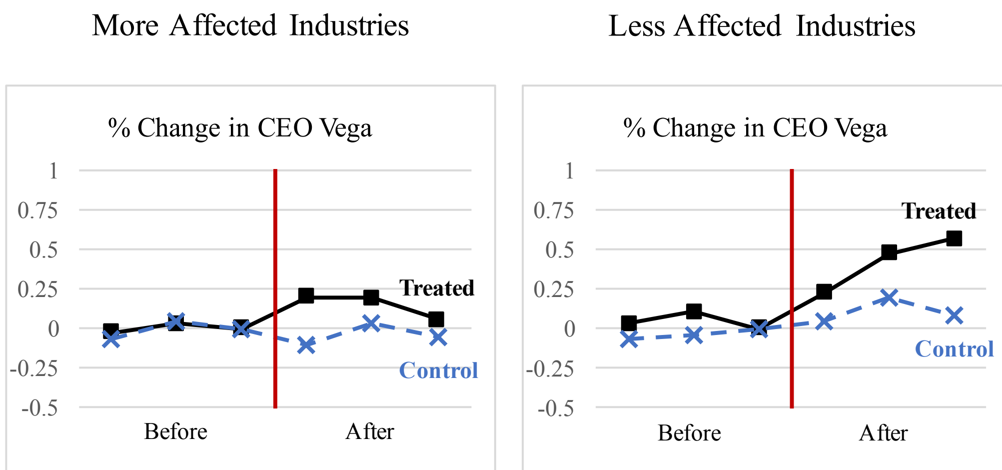

Figure 1.1: CEO Incentives: More Affected Industries vs. Less Affected Industries

This figure presents the percentage change in the average value of CEO incentives (*Flow Vega*) in a given year relative to the average value of CEO incentives in the year prior to the discovery of carcinogens, by treated and control groups, and by more affected 4-digit SIC industries and less affected 4-digit SIC industries. *Flow Vega* is defined as the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility.

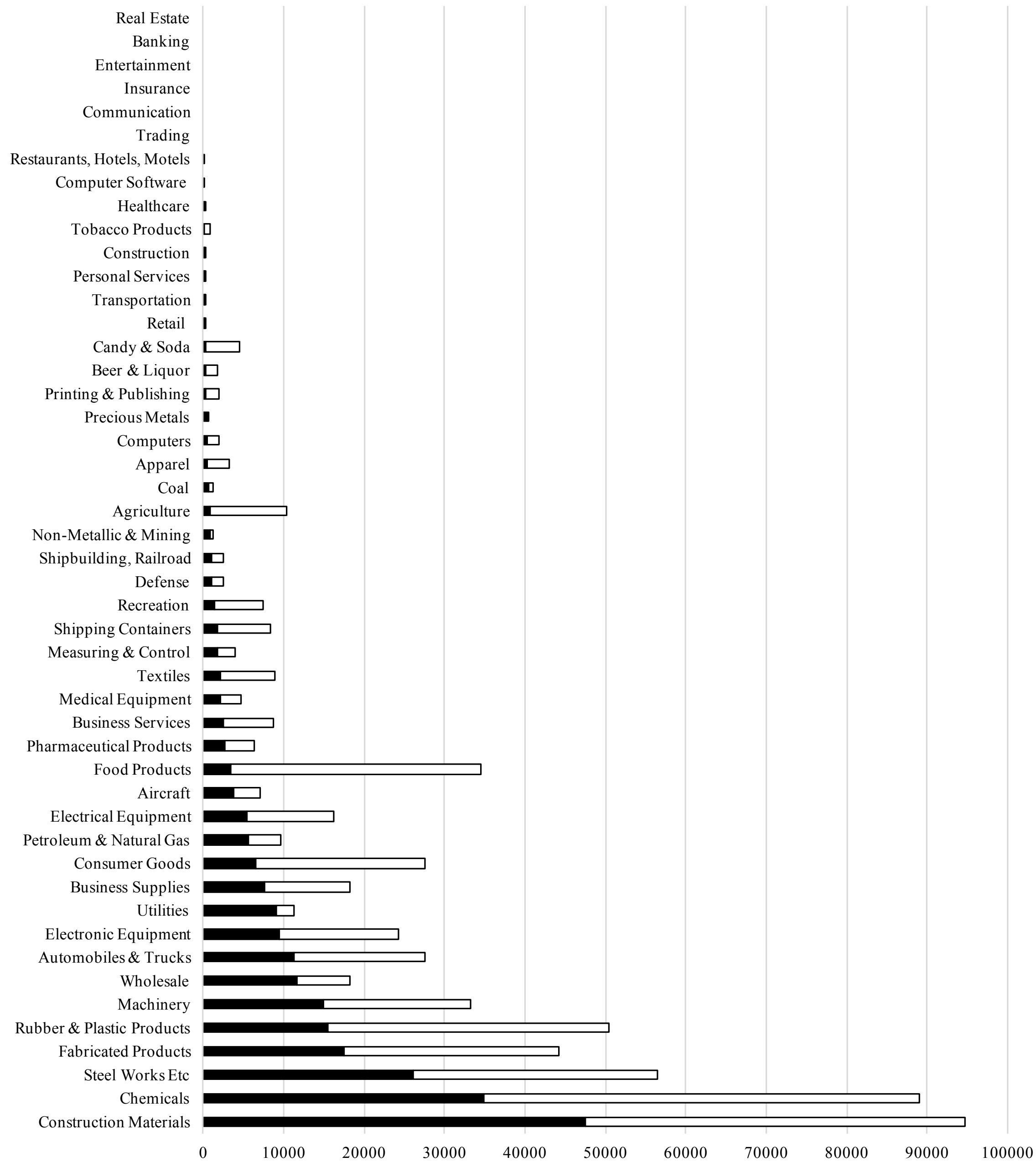

Figure 1.2: Plants in the Toxic Release Inventory Database by Industry

This figure presents the number of plant-year observations during 1987–2014 by Fama and French [52] 48 industry. Black bars indicate plants that emitted carcinogens, as reported in the Toxic Release Inventory (TRI) database. White bars indicate plants that emitted non-carcinogenic toxic chemicals.

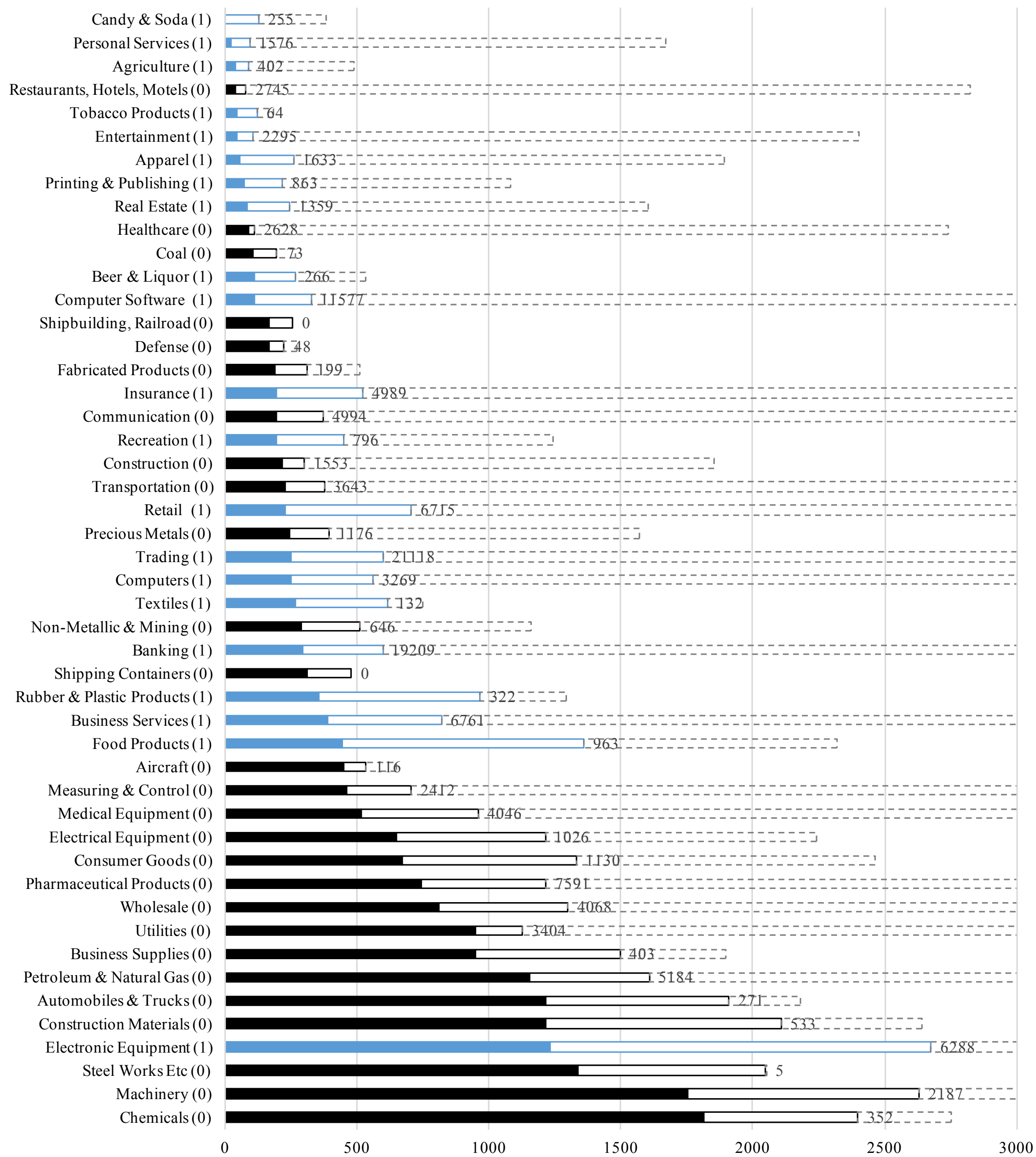

Figure 1.3: Compustat Firms by Industry

This figure presents the number of firm-year observations during 1987–2013 by Fama and French [52] 48 industry. Colored bars indicate firms with plants that emitted carcinogens, as reported in the Toxic Release Inventory (TRI) database. White bars indicate firms with plants that emitted non-carcinogenic toxic chemicals. Black color and zeros in parentheses indicate industries in which over 50% of the firms with TRI-reporting plants emitted carcinogens. Blue color and one in parentheses indicate industries in which less than or equal to 50% of the firms with TRI plants emitted carcinogens. Dashed bars with values indicate firms that emitted no toxic chemical.

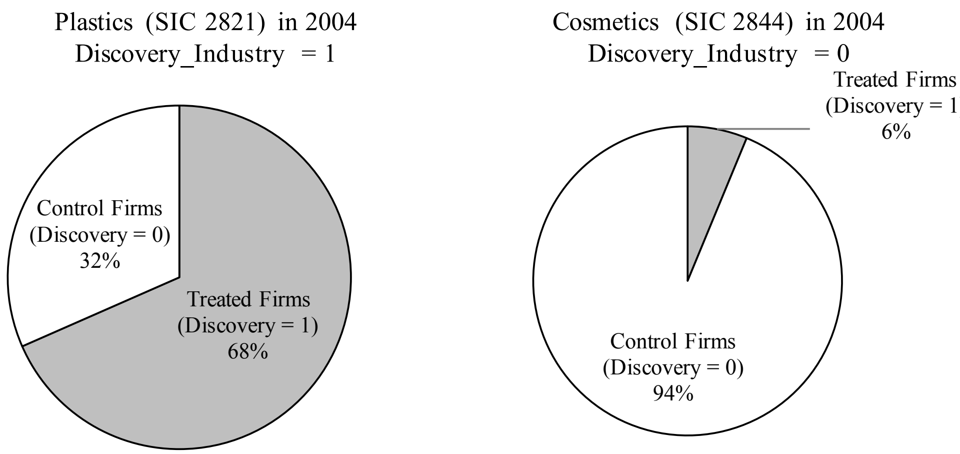

Figure 1.4: Example of a More Affected Industry and a Less Affected Industry

This figure illustrates an example of a more affected 4-digit SIC industry (plastics manufacturing, SIC 2821) and an example of a less affected 4-digit SIC industry (cosmetics manufacturing, SIC 2844) in the event of the 2004 discovery of carcinogens by the National Institutes of Health (NIH). Both industries belong to the chemical manufacturing sector. Grey color indicates the number of treated firms, i.e. firms that emitted carcinogens discovered in 2004. White color indicates the number of control firms, i.e. firms that did not emit any carcinogen discovered in 2004. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. *Discovery (Industry)* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were affected by the most recent discovery of carcinogens. The sample includes firms listed in Compustat that emitted toxic chemicals (including carcinogens and non-carcinogens) in 2004.

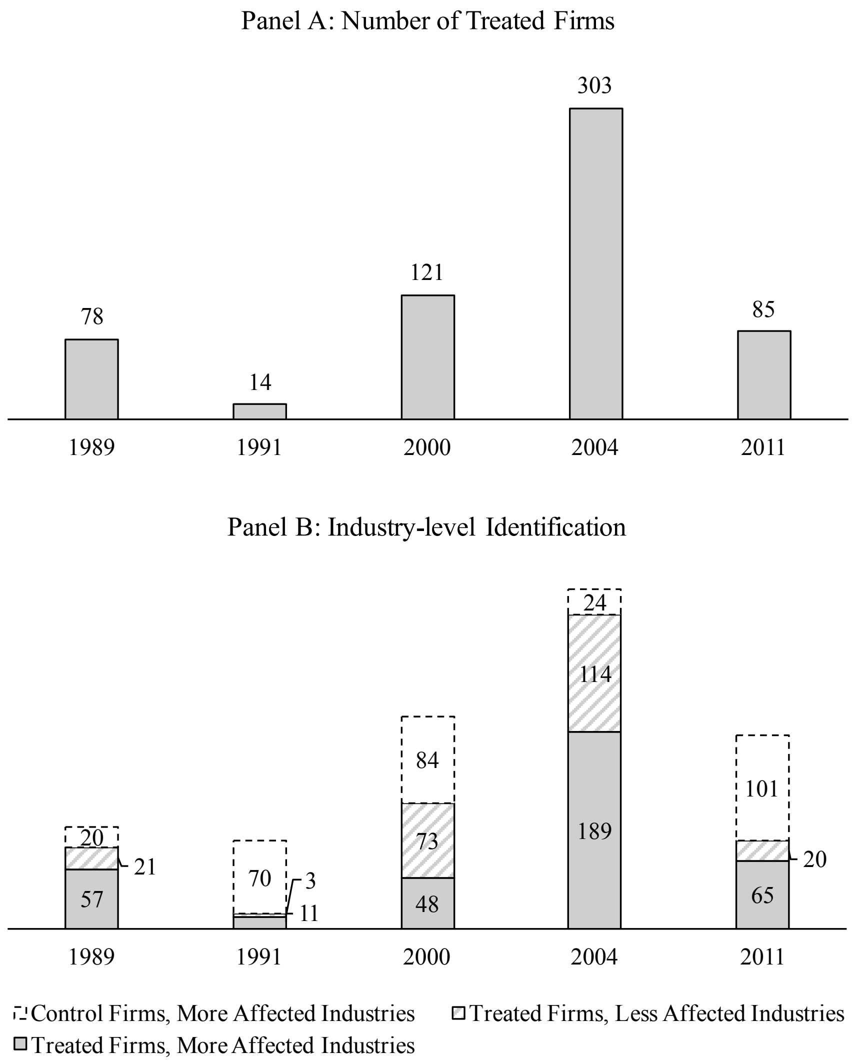

Figure 1.5: Identifying Treated and Control Groups

Panel A presents the number of firms affected by discoveries of carcinogens by year. The sample is restricted to firms with available financial and compensation data. A firm is considered treated if the National Institutes of Health (NIH) discovered the carcinogenicity of a chemical produced or used thus, emitted by the firm in a given year. Striped bars and white bars of panel B illustrate potential measurement errors resulted from identfiying treated and control groups by 4-digit SIC industry. For each year, panel B reports the following: The number of treated firms in more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery of carcinogens, i.e. firms identified as treated at both firm and industry levels (gray bars); the number of treated firms in less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery, i.e. firms identified as treated at the firm level but as control using the industry-level measure (striped bars); and the number of control firms in more affected 4-digit SIC industries, i.e. firms identified as treated using the industry-level measure but as control at the firm level (white bars).

Table 1.1: Summary Statistics

This table reports mean comparison results between treated and control groups. The sample includes firm-year observations in the three years prior to the discovery of carcinogens by the National Institutes of Health (NIH) occurred in 1989, 1991, 2000, 2004, or 2011. In panel A, a firm belongs to the treated group if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by the firm in the most recent edition of the Report on Carcinogens, as reported in the EPA's Toxic Release Inventory database. A firm belongs to the control group if it emitted at least one toxic chemical but no recently discovered carcinogen. In panel B, treated and control groups are matched based on firm size decile, firm age decile, and 4-digit SIC industry. In panel C, a firm is assigned to the treated group if it belongs to a 4-digit SIC industry in which more than 50% of the firms emitted a newly discovered carcinogen, and is assigned to the control group otherwise. Panel D reports original summary statistics from Gormley *et al.* [15]. Definitions of variables are presented in Appendix A.1. The last row in each panel reports the number of unique firms. Columns 3 and 6 present the p-value statistics from a t-test of the difference between treated and control groups, where standard errors are clustered at the industry level.

	Panel A: Full Sample			**Panel B: Matched Sample**		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Treated	Control	p-value	Treated	Control	p-value
Firm Size	8.09	7.33	0.02	8.08	7.48	0.10
Firm Age	36.77	21.18	0.00	36.94	28.7	0.06
Market-to-book	0.03	0.03	0.26	0.03	0.03	0.42
Leverage	0.57	0.54	0.30	0.58	0.54	0.25
Cash Flows	0.13	0.13	0.18	0.13	0.12	0.20
Stock Volatility	0.22	0.32	0.00	0.21	0.29	0.16
Delta	454.34	631.96	0.09	400.82	517.1	0.23
Vega	141.13	101.22	0.02	129.46	104.86	0.21
%Option Compensation	0.28	0.28	0.49	0.27	0.32	0.17
Firms	370	624		218	411	
	Panel C: Industry-level Measure			**Panel D: Gormley *et al.* [15]**		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Treated	Control	p-value	Treated	Control	p-value
Firm Size	7.78	7.43	0.15	7.15	7.44	0.47
Firm Age	35.47	21.33	0.00	N/A	N/A	N/A
Market-to-book	0.02	0.03	0.01	0.03	0.03	0.99
Leverage	0.57	0.54	0.30	N/A	N/A	N/A
Cash Flows	0.13	0.13	0.32	0.16	0.15	0.42
Stock Volatility	0.22	0.33	0.01	0.24	0.18	0.35
Delta	346.21	674.01	0.01	326.20	519.30	0.30
Vega	101.86	114.70	0.25	41.74	27.60	0.59
%Option Compensation	0.25	0.29	0.10	0.31	0.27	0.56
Firms	370	624		143	341	

Table 1.2: Effect of Discoveries of Carcinogens on CEO Flow Vega

This table reports estimates from regressing CEO flow vega on the discovery of carcinogens (Equation 1.2). The sample consists of five cohorts of firm-year observations during the six-year period (from year $T-3$ to year $T+2$) around the discovery of carcinogens by the National Institutes of Health (NIH) occurred in 1989, 1991, 2000, 2004, or 2011 (year T). A firm is included in the sample if it owned at least one plant that produced or used thus, emitted toxic chemicals (not necessarily carcinogens) during the sample period, as reported in the EPA's Toxic Release Inventory database. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. *Discovery (Industry)* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were affected by the most recent discovery of carcinogens. *More Affected Industry* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were or will be affected by the discovery of carcinogens. Detailed definitions of variables are presented in Appendix A.1. Column 2 presents subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Column 3 reports subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. In column 4, the sample is restricted to the Fama-French industries in which there is at least one firm in a less affected 4-digit SIC industry for every ten firms in a more affected 4-digit SIC industry, following Gormley *et al.* [15]. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. The even columns include 4-digit SIC industry-year fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: Flow Vega					
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Full Sample	Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	2.859**	6.018**	2.252		5.704**	10.19***
	(1.408)	(2.715)	(1.769)		(2.469)	(2.598)
Discovery (Industry)				-3.319**		
				(1.576)		
Discovery × More Affected Industry					-3.364	-7.098**
					(2.421)	(3.565)
Observations	5635	2207	3428	5501	5635	4402
Adjusted R^2	0.638	0.594	0.667	0.694	0.638	0.636
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year Fixed Effects	No	No	No	No	No	Yes

Table 1.3: Matched Sample

This table reports variants of Table 1.2 in which treated and control firms are matched based on firm size decile, firm age decile, and 4-digit SIC industry. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in \$000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. *Discovery (Industry)* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were affected by the most recent discovery of carcinogens. *More Affected Industry* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were or will be affected by the discovery of carcinogens. Detailed definitions of variables are presented in Appendix A.1. Column 2 presents subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Column 3 reports subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. In column 5, the sample is restricted to the Fama-French industries in which there is at least one firm in a less affected 4-digit SIC industry for every ten firms in a more affected 4-digit SIC industry, following Gormley *et al.* [15]. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: Flow Vega				
	Matched Sample	Less Affected Industries	More Affected Industries	Matched Sample	Matched Sample
	(1)	(2)	(3)	(4)	(5)
Discovery	3.742^{*}	8.699^{**}	1.632		7.522^{**}
	(2.002)	(3.132)	(1.959)		(2.968)
Discovery (Industry)				-4.072^{***}	
				(1.290)	
Discovery × More Affected Industry					-5.299^{**}
					(1.954)
Observations	2079	965	1114	2067	2079
Adjusted R^2	0.608	0.518	0.686	0.640	0.609
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes

Table 1.4: Effect of Discoveries of Carcinogens on Composition of Compensation

This table reports estimates from regressing individual components of CEO compensation on the discovery of carcinogens. The dependent variables are: *Total Compensation*, defined as the logged value of a CEO's total compensation in a given year in columns 1–3 of panel A; *Cash Compensation*, defined as the logged value of a CEO's salary plus bonus in a given year in columns 4–6 of panel A; *Option Compensation*, defined as the logged value of one plus the value of options granted to a CEO in a given year in columns 1–3 of panel B; and *Stock Compensation*, defined as the logged value of one plus the value of restricted stocks granted to a CEO in a given year in columns 4–6 of panel B. All raw values used to calculate dependent variables are in $000s. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Detailed definitions of variables are presented in Appendix A.1. Columns 2 and 5 in both panels present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 in both panels report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Total Compensation and Cash Compensation

	Total Compensation			Cash Compensation		
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	0.094**	0.151**	0.079	0.030	0.015	0.040
	(0.043)	(0.074)	(0.052)	(0.023)	(0.052)	(0.026)
Observations	5765	2201	3564	5795	2203	3592
Adjusted R^2	0.717	0.635	0.774	0.761	0.712	0.792
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Option Compensation and Stock Compensation

	Option Compensation			Stock Compensation		
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	0.324**	0.720**	0.227	0.045	0.082	0.092
	(0.148)	(0.344)	(0.171)	(0.141)	(0.299)	(0.176)
Observations	5573	1975	3598	5795	2203	3592
Adjusted R^2	0.541	0.478	0.578	0.642	0.638	0.643
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.5: Effect of Discoveries of Carcinogens on R&D and Capital Expenditures

This table reports estimates from regressing R&D and capital expenditures on the discovery of carcinogens and its interaction with the change in CEO flow vega. The dependent variables are: *R&D*, computed as R&D expenditures (missing values are replaced with zeros) scaled by total sales in panel A; and *Capital Expenditures*, computed as capital expenditures scaled by total assets in panel B. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. $\phi(\Delta \textit{Flow Vega})$ is computed as the average difference in CEO flow vega before and after the discovery scaled by the sample standard deviation of *Flow Vega*, where *Flow Vega* is defined as the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. Detailed definitions of variables are presented in Appendix A.1. Columns 2 and 5 in both panels present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: R&D Investments

	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	0.002	0.003	0.001	0.003	0.008	0.001
	(0.002)	(0.003)	(0.001)	(0.002)	(0.005)	(0.001)
Discovery × ϕ(ΔFlow Vega)				0.001*	0.007***	0.000
				(0.001)	(0.002)	(0.000)
Observations	5635	2207	3428	5635	2207	3428
Adjusted R^2	0.907	0.900	0.901	0.907	0.900	0.901
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Capital Expenditures

	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	-0.001	-0.001	-0.004	0.000	-0.004	-0.000
	(0.002)	(0.005)	(0.003)	(0.003)	(0.005)	(0.004)
Discovery × ϕ(ΔFlow Vega)				0.003	-0.004***	0.005*
				(0.003)	(0.001)	(0.003)
Observations	5601	2192	3409	5601	2192	3409
Adjusted R^2	0.677	0.671	0.674	0.677	0.671	0.674
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.6: Effect of Discoveries of Carcinogens on Leverage and Cash Holdings

This table reports estimates from regressing leverage and cash holdings on the discovery of carcinogens and its interaction with the change in CEO flow vega. The dependent variables are *Leverage*, computed as total liabilities divided by total assets in columns 1–3, and *Cash*, computed as cash divided by total assets in columns 4–6. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. $\phi(\Delta \mathit{Flow\ Vega})$ is computed as the average difference in CEO flow vega before and after the discovery scaled by the sample standard deviation of *Flow Vega*, where *Flow Vega* is defined as the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. Detailed definitions of variables are presented in Appendix A.1. Columns 2 and 5 present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1) Leverage	(2) Leverage	(3) Leverage	(4) Cash	(5) Cash	(6) Cash
Discovery	-0.002	-0.014	-0.002	-0.001	0.005	-0.003
	(0.007)	(0.012)	(0.010)	(0.006)	(0.012)	(0.004)
Discovery × $\phi(\Delta$Flow Vega$)$	-0.013**	-0.020**	-0.011*	0.004	0.024**	-0.002
	(0.006)	(0.009)	(0.006)	(0.004)	(0.010)	(0.004)
Observations	5544	2168	3376	5538	2186	3352
Adjusted R^2	0.877	0.856	0.886	0.718	0.723	0.701
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.7: Governance Strength and Risk-Shifting Incentives

This table reports results from subsample tests of column 1 in Table 1.2. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Column 1 of panel A presents subsample results for firms with below-median board independence in the year prior to the discovery of carcinogens. Columns 2–4 report subsample results for firms with above-median ex-ante board independence. Column 5 reports subsample results for firms with below-median ex-ante active institutional ownership. Columns 6–8 present subsample results for firms with above-median ex-ante active institutional ownership. Column 1 of panel B presents subsample results for firms with a top-tercile ex-ante leverage ratio. Columns 2–4 report subsample results for firms with a lower ex-ante leverage ratio. Column 5 reports subsample results for firms with an Altman [49] Z-score below 1.81 at the beginning of the year prior to the discovery. Columns 6–8 present subsample results for firms with a higher ex-ante Z-score. Columns 3 and 7 present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 4 and 8 report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery, and include firm-cohort and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Ex-ante Board and Institutional Investor Monitoring

	Dependent Variable: Flow Vega							
		More Independent Boards				Higher Institutional Ownership		
	Less Independent Boards	All Firms	Less Affected Industries	More Affected Industries	Lower Institutional Ownership	All Firms	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Discovery	0.605	4.335**	8.943**	2.583	1.211	4.033**	9.428**	1.982
	(2.382)	(1.999)	(3.918)	(2.363)	(1.781)	(1.947)	(4.170)	(2.807)
Observations	1813	2381	869	1512	2304	2228	936	1292
Adjusted R^2	0.632	0.678	0.646	0.691	0.698	0.615	0.567	0.651
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Ex-ante Risk-shifting Incentives

	Dependent Variable: Flow Vega							
		Not Highly Levered				Non-distressed		
	Highly Levered	All Firms	Less Affected Industries	More Affected Industries	Distressed	All Firms	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Discovery	1.967	3.496**	6.029**	1.716	1.584	3.552**	6.409**	2.538
	(3.512)	(1.541)	(2.658)	(2.107)	(2.213)	(1.412)	(2.817)	(2.078)
Observations	1921	3628	1572	2056	1248	4387	1783	2604
Adjusted R^2	0.650	0.628	0.589	0.657	0.683	0.646	0.609	0.671
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.8: Carcinogen Usage and Foreign Diversification

This table reports results from subsample tests of column 1 in Table 1.2. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Column 1 presents subsample results for treated carcinogen users and all control firms. Columns 2–4 report subsample results for treated carcinogen producers and all control firms. A treated firm is classified as a carcinogen user (rather than producer) if a greater fraction of newly discovered carcinogens were used or processed to produce other products rather than produced for sales or distribution purpose. Column 5 reports subsample results for firms with a higher ratio of foreign sales to total sales compared to the 33th percentile in the year prior to the discovery of carcinogens. Columns 6–8 report subsample results for firms with ex-ante foreign sales lower or equivalent to the 33th percentile. Columns 3 and 7 present further subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 4 and 8 present further subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: Flow Vega							
		Carcinogen Producers				Domestically Focused		
	Carcinogen Users	All Firms	Less Affected Industries	More Affected Industries	Foreign-focused	All Firms	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Discovery	2.390	3.304*	12.52**	1.971	2.423	3.641**	10.73**	2.564
	(1.502)	(1.841)	(5.900)	(2.142)	(2.589)	(1.733)	(5.201)	(1.920)
Observations	4826	4223	1838	2385	1761	3232	1121	2111
Adjusted R^2	0.660	0.656	0.632	0.674	0.643	0.629	0.593	0.649
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.9: Industry Competition

This table reports results from subsample tests of column 1 in Table 1.2. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Columns 1–3 present subsample results for the 4-digit SIC industries with above-median Herfindal-Hirschman index defined as the sum of squared shares of sales in the year prior to the discovery of carcinogens. Columns 4–6 report subsample results for the 4-digit SIC industries with ex-ante Herfindal-Hirschman indices lower or equivalent to the median. Columns 2 and 5 present further subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 present further subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: Flow Vega					
	Less Competitive Industries			More Competitive Industries		
	All Firms	Less Affected Industries	More Affected Industries	All Firms	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	0.654	-1.567	0.802	5.074***	9.395***	4.173
	(1.926)	(5.645)	(2.300)	(1.866)	(2.842)	(2.585)
Observations	2939	932	2007	2696	1275	1421
Adjusted R^2	0.658	0.575	0.694	0.614	0.607	0.614
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Appendix A: Appendix

A.1 Variable Definitions

- *Discovery* is an indicator equaling one, if the National Institutes of Health (NIH) discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens, as reported in the Toxic Release Inventory database maintained by the U.S. Environmental Protection Agency (EPA). Computed as a product of an indicator for treated firms and an indicator for post-treatment period.

- *Discovery (Industry)* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were affected by the discovery of carcinogens in the most recent edition of the Report on Carcinogens. Computed as a product of an indicator for treated industry and an indicator for post-treatment period.

- *More Affected Industry* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were or will be affected by the discovery of carcinogens in the most recent edition of the Report on Carcinogens, i.e. an indicator for treated industry.

- *Flow Vega* is the change in the value of a CEO's current-year compensation (effectively measured by the value of options granted in a given year; in $000s) for a 0.01 increase in the firm's stock return volatility (measured by the annualized standard deviation of stock returns). The calculation follows the methodology from Core and Guay [46] and Coles *et al.* [47] using compensation data from Execucomp for post-1992 and Yermack [19]'s sample for pre-1992.

- *Flow Delta* is the change in the value of a CEO's current-year compensation (effectively measured by the value of stock and options granted in a given year; in $000s) for a 1% increase in stock prices. See the definition for *Flow Vega* for more details.

- *Total Compensation* is the logged value of CEO total compensation (in $000s), calculated using *TDC1* from Execucomp and the sum of *SALBON*, *OTHCOMP*, and *GRANTVAL* from Yermack's sample.

- *Cash Compensation* is the logged value of CEO cash compensation (in $000s), calculated using *SALARY* plus *BONUS* from Execucomp and *SALBON* from Yermack's sample.

- *Option Compensation* is the logged value of one plus options granted to the CEO in a given year (in $000s), calculated using *OPTION_AWARDS_BLK_VALUE* for pre-2006 and *OPTION_AWARDS_FV* for post-2006 from Execucomp, and *GRANTVAL* from Yermack's sample.

- *Option Compensation (N)* is the Number of options granted to the CEO in a given year (in 000s), calculated using *OPTION_AWARDS_NUM* from Execucomp and *OPTGRANT* divided by 1000 from Yermack's sample.

- *Stock Compensation* is the logged value of one plus restricted stocks granted to the CEO in a given year (in $000s), calculated using *RSTKGRNT* for pre-2006 and *STOCK_AWARDS_FV* for post-2006 from Execucomp, and *OTHCOMP* from Yermack's sample.

- *CEO Tenure* is the logged value of one plus the number of years being CEO.

- *Firm Size* is the logged value of total assets (AT from Compustat; same source below).

- *Firm Age* is the number of years listed in Compustat.

- *Market-to-book* is the market-to-book ratio, calculated as $(CSHO \times PRCC\_F)/CEQ$.

- *Cash Flows* is computed as cash flows divided by total assets (AT). Cash flows are calculated by extracting *Accruals* from *OIADP*, where $Accruals = (ACT_t - ACT_{t-1}) - (CHE_t - CHE_{t-1}) - (LCT_t - LCT_{t-1}) + (DLC_t - DLC_{t-1}) - DP$ from Compustat.

- *Cash* is cash ($CASH$) divided by total assets (AT).

- *Capital Expenditures* is capital expenditures ($CAPX$) divided by total assets (AT).

- *R&D* is R&D expenditures (RD) divided by total sales ($SALE$). Missing values are replaced with zeros.

- *Leverage* is total liabilities (LT) divided by total assets (AT).

- *Z-score* is Altman [49] Z-score at the beginning of the year, calculated as $1.2 \times WCAP/AT + 1.4 \times RE/AT + 3.3 \times EBIT/AT + 0.6 \times CSHO \times PRCC\_F/LT + 0.999 \times SALE/AT$.

- *Foreign Sales* is the sum of sales from foreign segments ($SALE$, where $GEOTOP = 3$) plus export sales from domestic segments ($SALEXG$, where $GEOTOP = 2$) from Compustat Segment data.

- *Implied Volatility* is the open-interest-weighted average value of annualized daily implied volatility based on at-the-money call options with at least 90 days to expiration, using OptionMetrics data.

- *Stock Volatility* is the sum of squared daily returns during a given year multiplied by 252 and divided by the number of trading days, calculated using CRSP data.

- *Stock Returns* is the annualized holding period stock returns, calculated using CRSP data.

- *Board Independence* is the fraction of independent directors on board from ISS (formerly RiskMetrics).

- *Active Institutional Ownership* is the fraction of shares owned by potentially active institutional investors, using data from 13-f filings in Thomson Reuters, which distinguishes among five types of institutional investors: (1) investment companies; (2) independent investment advisors; (3) banks; (4) insurance companies, and (5) other institutions. Following Almazan *et al.* [48], potentially active investors refer to (1) and (2).

- *HHI* is the industry Herfindal-Hirschman index, computed as the sum of squared shares of sales within a 4-digit SIC industry.

A.2 Alternative Cutoffs

The following figure reports point estimates from subsample tests of column 1 in Appendix A.1. The fraction of treated firms in an industry is allowed to vary for each subsample. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. The independent variable of interest is *Discovery*, an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. All specifications include industry-cohort fixed effects and year-cohort fixed effects. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Dashed lines illustrate the 95% confidence intervals.

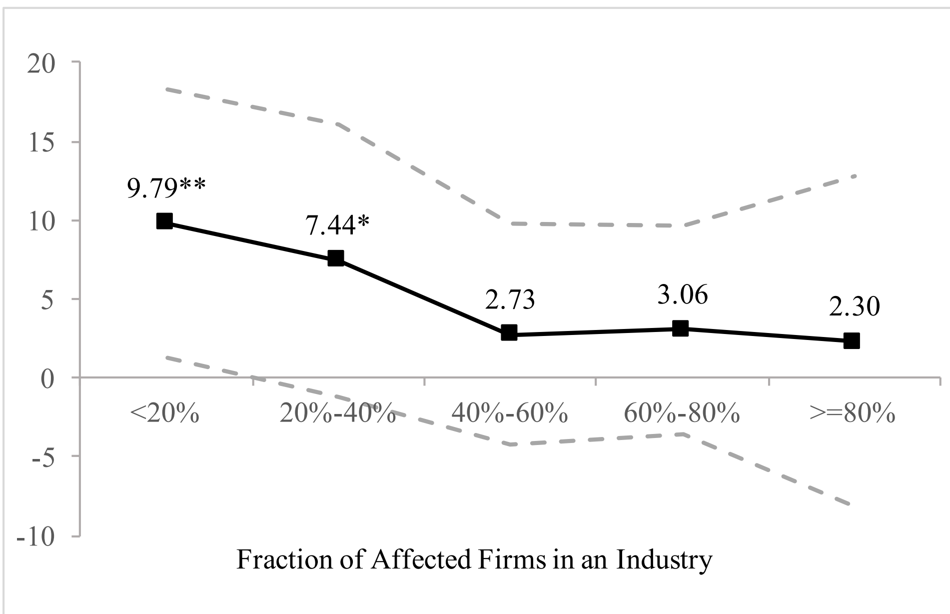

Figure A.1: Alternative Cutoffs between More Affected and Less Affected Industries

A.3 Placebo Tests

The following figure presents the density distribution of placebo estimates from a panel regression of flow vega on an indicator for discovery of carcinogens, based on 500 bootstrapped samples where treated and control firms are randomly mixed up. The actual diff-in-diff estimates are reported in column 1 of Appendix A.1. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in \$000s) to the firm's stock return volatility. The independent variable of interest is *Discovery*, an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects.

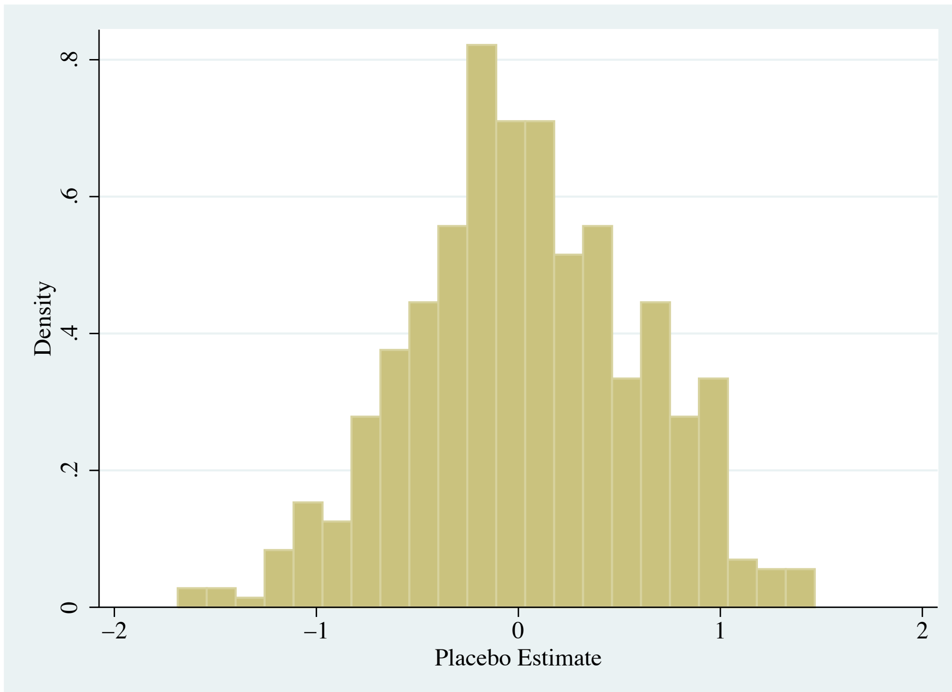

Figure A.2: Placebo Tests

A.4 Timing of Discoveries of Carcinogens

The following table presents the timing of discoveries of carcinogens. Column 1 reports the fiscal years in which the National Institutes of Health (NIH) released the Report on Carcinogens (RoC) to the public. Column 2 reports the dates on which the NIH released the RoC, as reported in news releases and articles by the National Institute of Environmental Health Sciences (NIEHS) and the National Center for Biotechnology Information (NCBI) available since 1994. Column 3 presents the number of chemicals newly discovered as carcinogens in a given edition of the RoC, excluding chemicals delisted in a later edition. Column 4 reports the number of chemicals previously discovered as carcinogens but delisted in a given edition of RoC due to a low possibility of human exposure and/or insufficient evidence of carcinogenicity after reevaluation. The last two columns report the number of Compustat firms releasing newly discovered carcinogens (excluding chemicals later delisted) and the number of Compustat firms releasing delisted carcinogens in a given fiscal year, as reported in the Toxic Release Inventory database available since 1987.

Table A.1: Timing of Discoveries of Carcinogens

(1) Year	(2) Date	(3) Newly Discovered Carcinogens	(4) Delisted Chemicals	(5) Firms Affected by Discoveries	(6) Firms Affected by Delisting
1980	N/A	27	0	N/A	N/A
1981	N/A	65	1	N/A	N/A
1983	N/A	29	0	N/A	N/A
1985	N/A	32	0	N/A	N/A
1989	N/A	18	4	312	0
1991	N/A	13	2	45	0
1994	June 24, 1994	8	0	7	0
1998	May 14, 1998	15	0	4	0
2000	May 15, 2000	16	2	245	54
2002	December 11, 2002	17	0	190	0
2004	January 31, 2005	17	0	643	0
2011	June 10, 2011	6	0	156	0
2014	October 2, 2014	4	0	60	0
Total		267	9	1662	54

A.5 Effects of Discoveries of Carcinogens on Volatility and Returns

Panel A of Table A.2 reports estimates from regressing volatility on the discovery of carcinogens (Equation 1.1). A firm is included in the analysis if it owned at least one plant that produced or used thus, emitted toxic chemicals (not necessarily carcinogens). In columns 1–3 of panel A, the sample consists of three cohorts of firm-date observations during the twelve-month period (from day $D - 180$ to day $D + 180$) around the discovery of carcinogens by the National Institutes of Health (NIH) occurred in 2000, 2004, or 2011 (on day D). The discovery announcement dates are reported in Table A.1. The dependent variable is *Implied Volatility*, the open-interest-weighted average value of annualized daily implied volatility based on at-the-money call options with at least 90 days to expiration. In columns 4–6 of panel A, the sample consists of five cohorts of firm-year observations during the six-year period (from year $T-3$ to year $T+2$) around the discovery occurred in 1989, 1991, 2000, 2004, or 2011 (year T). The dependent variables is *Stock Volatility*, the annualized sum of squared daily returns during the year. Treated and control firms in panel A are matched based on firm size decile, firm age decile, and 4-digit SIC industry. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Detailed definitions of variables are presented in Appendix A.1. All specifications include firm-cohort fixed effects and month-cohort or year-cohort fixed effects. Standard errors reported in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Panel B presents summary statistics on three-day and five-day average cumulative abnormal returns (CARs) around the discovery announcement date, estimated using the Fama-French model, using CRSP value-weighted returns to proxy for market returns. Columns 2 and 5 of panel A and column 4 of panel B present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 of panel A and column 5 of panel B report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery.

A.6 CEO Cash, Option, and Stock Compensation after FAS 123R

Table A.3 reports subsample tests of Table 1.4, where the sample is restricted to the 2011 cohort, after the FAS 123R accounting rule change in 2005. The dependent variables are: *Total Compensation*, defined as the logged value of a CEO's total compensation in a given year in columns 1–3 of panel A; *Cash Compensation*, defined as the logged value of a CEO's salary plus bonus in a given year in columns 4–6 of panel A; *Option Compensation*, defined as the logged value of one plus the value of options granted to a CEO in a given year in columns 1–3 of panel B; and *Stock Compensation*, defined as the logged value of one plus the value of restricted stocks granted to a CEO in a given year in columns 4–6 of panel B. All raw values used to calculate dependent variables are in $000s. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emit-

Table A.2: Effects of Discoveries of Carcinogens on Volatility and Returns

Panel A: Volatility

	Implied Volatility			Stock Volatility		
	Full Matched Sample	Less Affected Industries	More Affected Industries	Full Matched Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	0.019***	0.042**	0.018	0.033***	0.034**	-0.006
	(0.004)	(0.016)	(0.011)	(0.009)	(0.017)	(0.008)
Observations	143594	71175	72419	7382	3935	3447
Adjusted R^2	0.895	0.862	0.920	0.601	0.599	0.585
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month-cohort Fixed Effects	Yes	Yes	Yes			
Year-cohort Fixed Effects				Yes	Yes	Yes

Panel B: Cumulative Abnormal Returns (CARs; in %)

	Full Sample			Less Affected Industries	More Affected Industries	
	(1)	(2)	(3)	(4)	(5)	(6)
Days	CAR	p-value (t-test)	p-value (rank test)	CAR	CAR	p-value (difference)
(-1,+1)	-0.52	0.01	0.01	-0.61	-0.33	0.02
(-2,+2)	-0.75	0.00	0.00	-1.09	-0.46	0.00
Observations	1191			756	435	

ted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Detailed definitions of variables are presented in Appendix A.1. Columns 2 and 5 in both panels present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 in both panels report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

A.7 New CEOs

Table A.4 presents variants of columns 1–3 in Appendix A.1. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in

Table A.3: CEO Cash, Option, and Stock Compensation after FAS 123R

Panel A: Total Compensation and Cash Compensation

	Total Compensation			Cash Compensation		
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	0.077	0.077	0.044	-0.020	0.025	-0.035
	(0.051)	(0.169)	(0.054)	(0.021)	(0.068)	(0.024)
Observations	2106	858	1248	2106	858	1248
Adjusted R^2	0.728	0.687	0.757	0.884	0.788	0.914
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Option Compensation and Stock Compensation

	Option Compensation			Stock Compensation		
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	0.449^{*}	1.657^{**}	0.177	0.200	-1.424	0.427
	(0.234)	(0.797)	(0.241)	(0.231)	(1.272)	(0.260)
Observations	2032	771	1261	2106	858	1248
Adjusted R^2	0.666	0.594	0.715	0.610	0.584	0.628
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

$000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Detailed definitions of variables are reported in Appendix A.1. Columns 1–3 do not exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. Columns 4–6 include only observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. Columns 2 and 5 present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.4: New CEOs

	Dependent Variable: Flow Vega					
	All CEOs			New CEOs		
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	2.256**	3.029**	2.667*	-1.489	0.462	-0.815
	(1.095)	(1.402)	(1.494)	(1.933)	(3.055)	(2.956)
Observations	7468	2969	4499	1626	678	947
Adjusted R^2	0.637	0.611	0.654	0.724	0.759	0.699
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

A.8 Control Variables

The following table presents robustness checks to columns 1–3 of Appendix A.1. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Control variables include: lagged value of *Flow Vega*; *Firm Size*, defined as the logged value of total assets; *CEO Tenure*, defined as the logged value of one plus the number of years being CEO; and *Cash Compensation*, computed as the logged value of a CEO's salary and bonus in a given year. Detailed definitions of variables are reported in Appendix A.1. Columns 2 and 5 present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.5: Control Variables

	Dependent Variable: Flow Vega					
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	2.536*	5.467**	1.792	3.435**	7.375**	2.649
	(1.407)	(2.717)	(1.702)	(1.519)	(3.059)	(1.888)
Firm Size	5.327***	5.459**	5.244**			
	(1.663)	(2.463)	(2.343)			
CEO Tenure	-1.681	-1.588	-1.751			
	(1.415)	(2.662)	(1.380)			
Cash Compensation	4.397***	5.941**	3.164**			
	(1.308)	(2.656)	(1.477)			
L.Flow Vega				-0.039	-0.110***	0.019
				(0.024)	(0.033)	(0.027)
Observations	5635	2207	3428	4314	1653	2661
Adjusted R^2	0.641	0.598	0.669	0.672	0.626	0.704
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

A.9 Number of Option Grants and CEO Flow Delta

The following table reports robustness checks to Appendix A.1 using the number of option grants and flow delta as alternative measures for CEO incentives. The dependent variables are: *Option Compensation (N)*, the number of options granted to a CEO in a given year (in 000s), in panel A; and *Flow Delta*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock prices, in panel B. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. *Discovery (Industry)* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were affected by the most recent discovery of carcinogens. *More Affected Industry* is an indicator for all firms within a 4-digit SIC industry in which over 50% of the firms were or will be affected by the discovery of carcinogens. Detailed definitions of variables are reported in Appendix A.1. Column 2 in both panels presents subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Column 3 in both panels reports subsample results for more affected 4-digit SIC industries, in which over 50%

of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.6: Number of Option Grants and CEO Flow Delta

Panel A: Number of Option Grants					
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Full Sample
	(1)	(2)	(3)	(4)	(5)
Discovery	18.49*	69.53**	5.666		56.08***
	(10.55)	(27.94)	(10.82)		(20.15)
Discovery (Industry)				-27.35**	
				(11.02)	
Discovery × More Affected Industry					-43.89**
					(20.05)
Observations	5599	1975	3624	5286	5599
Adjusted R^2	0.503	0.420	0.571	0.470	0.504
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes

Panel B: Flow Delta					
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Full Sample
	(1)	(2)	(3)	(4)	(5)
Discovery	4.080**	11.03**	1.754		9.502***
	(1.901)	(4.590)	(2.098)		(3.597)
Discovery (Industry)				-5.875*	
				(3.321)	
Discovery × More Affected Industry					-6.404*
					(3.607)
Observations	5785	2277	3508	5630	5785
Adjusted R^2	0.536	0.437	0.610	0.553	0.536
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes

A.10 Dynamic Effects

Table A.7 reports robustness checks to columns 1–3 of Appendix A.1. The dependent variables are: *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility, in columns 1–3; and *Option Compensation (N)*, the number of options granted to a CEO in a given year (in 000s), in columns 4–6. *Discovery(-1)* is an indicator equaling one, if the National Institutes of Health (NIH) will discover the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the following year. *Discovery(0)* is an indicator equaling one, if the NIH discovers the carcinogenicity in the current year. *Discovery(+1)* is an indicator equaling one, if the NIH discovered the carcinogenicity in the previous year. *Discovery(+2)* is an indicator equaling one, if the NIH discovered the carcinogenicity two years ago. Detailed definitions of variables are presented in Appendix A.1. Columns 2 and 5 present subsample results for less affected 4-digit SIC industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Columns 3 and 6 report subsample results for more affected 4-digit SIC industries, in which over 50% of the firms were simultaneously affected by the discovery. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

A.11 Alternative Industry Classification

Table A.8 reports robustness checks to columns 2 and 3 of Appendix A.1 using alternative industry classification. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Detailed definitions of variables are reported in Appendix A.1. Odd columns present subsample results for less affected industries, in which less than or equal to a threshold fraction of the firms were simultaneously affected by the discovery, where the threshold fraction is 50% in colummns 1 and 2, and 33% in columns 3–6. Even columns report subsample results for more affected industries, in which over a threshold frac-

Table A.7: Dynamic Effects

	Flow Vega			Option Compensation (N)		
	Full Sample	Less Affected Industries	More Affected Industries	Full Sample	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery(-1)	0.111	-2.934	1.604	27.20	-23.27	38.01
	(1.524)	(3.405)	(1.825)	(18.92)	(35.77)	(24.42)
Discovery(0)	2.990*	1.597	3.669*	25.13	4.951	33.67*
	(1.644)	(3.635)	(1.946)	(17.42)	(38.45)	(16.69)
Discovery(+1)	1.999	4.969	1.887	28.79**	78.84*	12.33
	(1.769)	(4.113)	(2.075)	(13.01)	(43.52)	(13.31)
Discovery(+2)	3.911**	10.45**	2.311	31.98**	139.6***	4.078
	(1.936)	(4.508)	(2.267)	(14.10)	(45.97)	(16.37)
Observations	5635	2118	3517	5599	1975	3624
Adjusted R^2	0.654	0.631	0.668	0.503	0.422	0.572
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

tion of the firms were simultaneously affected by the discovery. Industry is defined by text-based fixed 400 industries classification (FIC-400) by []HobergPhillips:10 in columns 1 and 2, by FIC-300 in columns 3 and 4, and by 3-digit SIC in columns 5 and 6. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

A.12 Sub-sample Periods

Table A.9 reports subsample tests of columns 2 and 3 in Appendix A.1. The dependent variable is *Flow Vega*, the sensitivity of a CEO's current-year compensation (in $000s) to the firm's stock return volatility. *Discovery* is an indicator equaling one, if the NIH discovered the carcinogenicity of a chemical produced or used thus, emitted by one of the plants owned by the firm in the most recent edition of the Report on Carcinogens. Columns 1 and 2 present subsample results for the 2011 discovery. Columns 3 and 4 present subsample results for the 2004 discovery. Columns 5 and 6 report subsample results for 1989, 1991, and 2000 discoveries.

Table A.8: Alternative Industry Classification

	Dependent Variable: Flow Vega					
	FIC-400 Industries		FIC-300 Industries		3-digit SIC Industries	
	Less Affected Industries	More Affected Industries	Less Affected Industries	More Affected Industries	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	6.714***	0.624	6.968**	2.584*	5.007***	2.909*
	(2.024)	(1.496)	(3.075)	(1.342)	(1.818)	(1.697)
Observations	2721	2555	1679	3593	1967	3668
Adjusted R^2	0.611	0.681	0.613	0.668	0.598	0.659
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Odd columns present subsample results for less affected industries, in which less than or equal to 50% of the firms were simultaneously affected by the discovery. Even columns report subsample results for more affected industries, in which over 50% of the firms were simultaneously affected by the discovery. Detailed definitions of variables are reported in Appendix A.1. All specifications exclude observations for which firms replace their CEOs in the year prior to the discovery of carcinogens. All specifications include firm-cohort fixed effects and year-cohort fixed effects. Standard errors reported in parentheses are clustered at the industry level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.9: Sub-sample Periods

	Dependent Variable: Flow Vega					
	2011 Cohort		2004 Cohort		Pre-2004 Cohorts	
	Less Affected Industries	More Affected Industries	Less Affected Industries	More Affected Industries	Less Affected Industries	More Affected Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Discovery	14.01**	2.892	2.146	0.516	11.34**	2.863
	(7.034)	(2.194)	(3.546)	(2.703)	(5.373)	(2.510)
Observations	749	1082	696	1243	762	1103
Adjusted R^2	0.697	0.740	0.556	0.698	0.496	0.517
Exclude New CEOs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Chapter 2: Production Cluster Structure, Corporate Governance and Corporate Performance

2.1 Introduction

Over the past few decades, the use of bilateral trading contracts between a supplier firm and a customer firm has been growing extensively. Between 1980 and 2009, the total number of supplier-customer relationships reported by US public firms expanded from around 900 per year to 2,000. In 2000, the electronics industry purchased $75 billion from contract manufacturers, which accounts for 10% of the total production (Plambeck and Taylor [75]). A growing strand of literature has examined how bilateral trading relationships affect corporate policies. However, few studies explore the implications of overlapping customers. In practice, some supplier-customer relationships are exclusive, while others are not. For instance, IBM had been Apple's major supplier for PowerPC chips for over 20 years. In contrast, multiple chip manufacturers supply to Motorola.

This paper takes a supplier firm's perspective and investigates whether the effect of product market competition on corporate performance depends on the overlap in customer base. Existing studies demonstrate that competition may drive down firm profits, but may also act as a governance mechanism and discipline managers (e.g., Schmidt [25]; Raith [26]; Giroud and Mueller [76]). Building on this literature, I compare competition between firms supplying to a same customer versus competition between firms with non-overlapping customers. I explore variations in the structure of *production cluster*, defined as a single or a number of firms that supply substitutable inputs to a shared customer (Figure 2.1).

I compile a sample of 47,945 supplier-customer relationships in which both parties are US publicly listed firms during the period 1976–2009, including 5,863 unique supplier firms and 2,593 unique customer firms. The average customer in my sample has 18 suppliers. The number of suppliers increases with customer firm size, but varies widely within customer firm size deciles (Figure 2.2), which allows me to compare between supplier firms with few rivals and those with many rivals.

I first test how production cluster size affects supplier firm performance. To address the concern that badly-governed firms may endogenously select small production clusters to avoid competition, I exploit exogenous variations in takeover pressure due to passages of business combination (BC) laws, following Bertrand and Mullainathan [78] and Giroud and Mueller [76]. Consistent with these studies, I find an overall decline in supplier firms' operating performance, measured mainly by ROA, after a BC law passage in their state of incorporation. In addition, I show that each additional peer supplier within a production cluster mitigates the decline by 0.2 percentage point. If the sample is restricted to production clusters in which each supplier is observed to serve only one customer, the effect of each additional peer supplier on operating performance enlarges to 0.7 percentage point. The incremental value of an additional supplier generally declines with the production cluster size.

If the production cluster definition is extended to include same-industry suppliers that supply to different customers, the effects of peer suppliers on operating performance become weaker and less significant. This evidence is consistent with the theoretical work by Shaked and Sutton [79], who argue that industries are naturally segmented if consumers differ in their preferred choice of product. Firms may produce differentiated products and invest in relationship-specific assets according to their customer's requirements, even if they belong to a same industry. In other words, same-industry firms may not produce substitutable products and become close competitors if they supply to different customers.

I provide further evidence that the effect of competition depends on the relative importance of a supplier firm to its customer, measured by a supplier's share of input to the customer. When a firm is a peripheral supplier who supplies a small share of input to its customer, competition has a negative net effect on firm performance, because there may exist intense shadow competition between peripheral suppliers even when input shares are highly concentrated among supplier firms. However, when a firm is a major supplier who supplies a large share of input to its customer, competition has a positive net effect on performance, since the presence of competitors may discipline managers. This result is consistent with the existing theoretical prediction that large firms generally have more corporate governance issues and thus larger room for improvement.[1]

In addition, I find that the effect of competition depends on input specificity, proxied by industry specificity classifications (Giannetti *et al.* [80]) or supplier firms' prior R&D expenditures. It may be costly for a customer to replace an existing supplier with a new one outside the production cluster if large amounts of relationship-specific investments have been made and inputs have been tailored to the customer's needs. In other words, the existing supplier may face little threat from potential competitors outside the production cluster. I show that the effect of production cluster competition is more evident in the subsample in which the level of input specificity is high. In contrast, supplier firms' operating performance is insensitive to production cluster size when the level of input specificity is low. When inputs are specific, production cluster size matters because existing suppliers may compete to crowd out each other.

I also provide evidence on the effect of competition on supplier firms' performance after a BC law passage in customer companies' state of incorporation. Overall, production cluster competition drives down firm profits to a lesser extent when the firm's customer company faces a decline in takeover threat. One possible explanation is that a less governed customer may have a smaller incentive to monitor its suppliers, and production cluster competition substitutes for monitoring by the customer. Consistent with this explanation, I find that this result is mainly driven by major suppliers rather than peripheral suppliers.

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[1] Large publicly traded corporations are frequently characterized as having separated ownership and control. See, for example, Jensen and Meckling [2] and Fama and Jensen [92].

The findings are robust to several alternative specifications. For example, I exclude all time-varying controls and include additional fixed effects following Gormley and Matsa [100]. In addition to BC laws, I control for effects of other state-level anti-takeover laws such as poison pill laws and control share acquisition laws following Karpoff and Wittry [102]. Furthermore, I consider alternative windows around BC law passages.

This paper contributes to the literature on supplier-customer relationship and its implications on corporate outcomes. A group of studies provide evidence that supplier firms with more dependent customers maintain lower leverage (Banerjee *et al.* [81]), pay fewer dividends (Wang [82]), hold more cash (Itzkowitz [83]), have higher costs of equity (Dhaliwal *et al.* [84]), and innovate more (Chu *et al.* [85]). Another group of studies examine the propagation of idiosyncratic shocks in production networks (Kelly *et al.* [86]; Barrot and Sauvagnat [87]; Gao [88]). Unlike these studies, this paper emphasizes the role of shared customers and shows that corporate performance depends on production cluster structure.

This paper also contributes to the literature on the impact of product market competition on firm performance. Existing theoretical models and empirical work illustrate that competition has two competing effects on corporate performance: a negative effect through reducing profits and a positive effect through mitigating managerial slack (e.g., Schmidt [25]; Raith [26]; Giroud and Mueller [76]). My analysis provides extended evidence that competition between firms with overlapping customers is more relevant than competition between firms with non-overlapping customers, which suggests that even firms within a same industry may not be close competitors if they supply to different customers.

Finally, this paper contributes to the literature on the effect of inter-firm connections on corporate outcomes. Existing studies investigate the connections through strategic alliances (Chan *et al.* [89]), common institutional shareholders (He and Huang [90]), and common debtholders (Asker and Ljungqvist [91]). This paper adds to this literature by exploring the interconnections between firms through a common customer.

The rest of this paper proceeds as follows. Section 1 reviews the related literature. Section 2 develops the hypotheses. Section 3 discusses the empirical design. Section 4 presents the results and robustness analyses. Section 5 concludes.

2.2 Related Literature

This paper is related to several strands of literature. First, a growing strand of studies has examined the bilateral relationship between a customer firm and a supplier firm and its impact on corporate policies. Existing studies provide evidence that supplier firms with more dependent customers maintain lower leverage (Banerjee *et al.* [81]), pay fewer dividends (Wang [82]), hold more cash (Itzkowitz [83]), have higher costs of equity (Dhaliwal *et al.* [84]), and innovate more (Chu *et al.* [85]). Unlike these studies, this paper examines whether a supplier firm performs differently when its peer suppliers serve the same customer versus when its peer suppliers supply to different customers.

Several recent papers investigate the propagation of idiosyncratic shocks in supplier-customer networks. Kelly *et al.* [86] model the impact of customer growth shocks on supplier volatilities and find that larger suppliers with less concentrated customer networks tend to have lower volatilities. Barrot and Sauvagnat [87] use natural disasters to identify input shocks and show that disrupted suppliers impose output losses on their customers. Gao [88] find evidence that firms with more connections in supplier-customer networks play a stabilizing role and adopt more conservative financial policies. Unlike these studies, this paper investigates whether production cluster structure affects supplier firm performance, where production cluster refers to a group of suppliers who supply substitutable inputs to a shared customer.

This paper provides additional evidence to the literature on inter-firm connections. Firms in the same industry are connected to each other through various types of links, including strategic alliances (Chan *et al.* [89]), common institutional shareholders (He and Huang [90]), and common debtholders (Asker and Ljungqvist [91]). In this paper, I explore the linkage between suppliers that are closely connected through shared customers.

In addition, this paper is related to the literature on product market competition and its implications on firm performance. Schmidt [25] and Raith [26] theoretically analyze the impact of competition on firm profits and document two competing effects: competition drives down product prices and firm profits, but also motivates managers to improve productivity in order to avoid liquidation or to steal demand from rivals. Giroud and Mueller [76] find consistent evidence that firms operating in industries with more concentrated market shares are more sensitive to exogenous variations in external governance. This paper examines how firm performance depends on production cluster structure and suggests that same-industry firms may not directly compete with each other if they supply to different customers. Furthermore, I provide evidence that the effect of production cluster competition varies with customer companies' governance strength.

Finally, this paper complements the literature on endogenous product differentiation. Sutton [93] argues that firms invest in R&D and advertising in order to differentiate their products and create endogenous barriers to entry. Shaked and Sutton [79] suggest that industries are naturally segmented if consumers differ in their preferred choice of product. Several empirical studies find supporting evidence. For example, Ellickson [94] investigates the supermarket industry and provides evidence on endogenous investment in product variety in order to compete for customers. Hoberg and Phillips [95] use text-based network industry classifications to show that firms experience significant reductions in product similarity after heavily investing in advertising or R&D. In this paper, I present evidence consistent with the prediction of endogenous product differentiation. The choice of differentiation may be influenced by a firm's customers.

2.3 Hypotheses Development

Existing studies document two competing effects of product market competition on firm performance (Schmidt [25]; Raith [26]; Giroud and Mueller [76]). On the one hand, competition may reduce firm profits. On the other hand, competition may improve firm performance through a corporate governance channel, because of the pressure to survive in a competitive market. I expect that competition between peer supplier firms within a production cluster has similar effects on supplier firm performance. This leads to my first hypothesis:

Hypothesis 3. *Competition between peer suppliers within a production cluster has two competing effects on a supplier firm's performance: reducing firm profits but improving firm performance through a corporate governance channel.*

Hypothesis 3 predicts a tangible impact of production cluster structure on firm performance. Next, I argue that production cluster contains incremental information about closeness and competition than industry alone. Shaked and Sutton [79] find that industries are naturally segmented if consumers differ in their preferred choice of product. Firms may invest in relationship-specific assets and tailor products to their customers' needs. Thus, product market can be segmented by customer in addition to industry. An industry with a large number of firms or low concentrated market shares may still be non-competitive if each firm maintains a relationship with different customer. The extent to which production cluster captures incremental information about competition may vary by industry and may depend on the degree to which suppliers produce specialized inputs, which is addressed in a later hypothesis. In general, I expect that firm performance would be more strongly affected by peers within the production cluster than by potential rivals that supply to different customers. This leads to my second hypothesis:

Hypothesis 4. *Competition between peer supplier firms within a production clus-*

ter has stronger and more significant effects on a supplier firm's performance than competition between peer firms within a given industry.

The effect of competition may depend on a firm's relative importance to its customer. A customer firm may purchase a large fraction of inputs from several major suppliers and a small fraction of inputs from a group of contractors, or peripheral suppliers. Figure 2.1 illustrates a case where *Supplier 1* is a major supplier that supplies 80% of *Customer B*'s inputs, while other four suppliers are peripheral suppliers that in total only account for 20% of *Customer B*'s inputs. A customer firm may choose to diversify its input sources in order to insure against disruptions in production networks, which can impose substantial losses to firm value (Barrot and Sauvagnat [87]). However, a customer firm may maintain a stable relationship with its major supplier, while frequently replace its peripheral suppliers. Thus, there may exist shadow competition between peripheral suppliers that leads to lower supplier firm profits. In addition, if major suppliers tend to be larger firms than peripheral suppliers, they may have more corporate governance issues (e.g., Jensen and Meckling [2]; Fama and Jensen [92]), and thus have more room for improvement. Hence, competition may help major suppliers to discipline management and improve firm performance to a larger extent than peripheral suppliers. This leads to the following hypotheses:

Hypothesis 3a. *Production cluster competition drives down firm profits to a larger extent when the firm supplies a lower input share to its customer.*

Hypothesis 3b. *Production cluster competition improves firm performance through* *a governance channel to a larger extent when the firm supplies a higher input share to its customer.*

Furthermore, I expect that the effect of production cluster competition would vary according to input specificity. It may be costly for a customer to replace existing suppliers that produce specialized inputs, partially because the customer may also invest in such specific relationship, for example in training suppliers or in production processes (Cen *et al.* [96]). Thus, existing specialized suppliers may face little potential competition from other suppliers outside the production cluster, and thus may intensely compete with their peers within the production cluster to crowd out each other. However, it may be less costly for a customer to find and replace an existing supplier that produces standardized inputs. Hence, production cluster size may be more relevant when inputs are more specific. This leads to the next hypothesis:

Hypothesis 4. *Competition between peer suppliers within a production cluster has stronger and more significant effects on a supplier firm's performance when existing suppliers within the production cluster produce more specific inputs.*

Finally, I expect that a customer would affect supplier firms' corporate performance by monitoring and bargaining with them. Specifically, a change in a customer firm's governance strength may translate into changes in supplier firms' governance and performance. A less governed customer may have a smaller incentive to bargain with its suppliers for lower input prices, and thus may have a positive impact on supplier firm profits. This implies that competition between peer suppliers may drive down supplier firm profits to a lesser extent when the shared customer itself has more governance issues. However, a less governed customer may also have a smaller incentive to monitor its suppliers, and thus may have an adverse impact on supplier firms' governance and performance. This implies that competition between peer suppliers may improve supplier firm performance through a governance channel to a lesser extent when the shared customer itself has more governance issues. This leads to the following hypothesis:

Hypothesis 5. *The effect of competition between peer suppliers within a production cluster on supplier firm performance depends on the customer's governance strength.*

2.4 Empirical Design

2.4.1 Sample Selection and Summary Statistics

I identify supplier-customer relationships using Compustat Customer Segment Files. The Statement of Financial Accounting Standards No. 14 (SFAS 14) of the Financial Accounting Standard Board (FASB) has required firms to report principal customers representing over 10% of total sales since 1976. In 1997, SFAS 14 was superseded by FAS 131. As a result of the change in disclosure rules, some firms restated their segment information, but the requirement to report the principal customers remains intact for public companies under SEC Regulation S-K Item 101. In robustness tests, I restrict my sample to pre-1997 or post-1997 period. Firms can but are not required to report customers that represent less than 10% of their sales. Existing studies (e.g., Atalay *et al.* [97]) show that the truncation issue does not affect the shape of the supplier link distribution, and thus are less concerned about potential selection biases due to the voluntary disclosures. In robustness tests, I exclude voluntarily reported customers below the 10% sales threshold.

In each year, firms are required to report the names of their principal customers and the amount of sales to each of them. Customer names are mapped to CRSP permno and Compustat gvkey identifiers through a text algorithm combined with manual checks, which allows me to identify whether the customer is also listed in Compustat and CRSP.[2] I retain in my sample all supplier-customer pairs where both parties are US public firms over the period 1976–2009.[3]

My main sample consists of 13,083 distinct supplier-customer pairs, in total 47,945 pair-year observations with available financial data from Compustat. There are 5,863 unique suppliers and 2,593 unique customers, since some suppliers supply more than one customer and some customers source from more than one suppliers. The average customer in my sample has 18 suppliers that may come from different industries. The average supplier in my sample only supplies to 2 customers.[4] Figure 2.2 shows that the number of suppliers increases with customer firm size. More importantly, there are wide variations in the number of suppliers within each customer firm size decile, even when the calculation is based on the number of suppliers

[2] I thank Lauren Cohen for sharing the correspondences between customer names and CRSP permno identifiers they used in Cohen and Frazzini [99].

[3] Private and foreign suppliers and customers are not included in the sample due to data availability.

[4] One potential reason why suppliers are generally smaller than customers in my sample is that supplier-customer relationships are reported by suppliers. The Compustat Customer Segment Files do not include relationships in which the supplier is huge in size but supplies to customers below the 10% threshold.

from a same sector, so that the inputs are to some extent substitutable (panel B).

In around one-fifth of the sample supplier-customer pairs, supplier firms are observed to be the only source for their customer companies. In around one-third of the sample pairs, customer companies are observed to be the only buyer for their suppliers. In addition to the full sample, I also conduct analyses on a subsample of firms that are observed to serve only one customer, which mitigates potential noises from suppliers' outside options.

Figure 2.3 describes the sample by Fama and French [52] 48-industry classification and shows that supplier firms mainly belong to the Electronic Equipment sector (11.2%), Petroleum and Natural Gas (8.1%), Computer Software (6.9%), Pharmaceutical Products (6.3%), and Business Services (5.9%). Each of these sectors has approximately one-third of firms that supply to only one customer as observed in the sample. The average sector has a similar fraction of firms that supply to a single customer. The two sectors that have a large fraction of firms with a single customer are Tobacco Products (88.5%) and Beer and Liquor (84.6%). The two sectors that have a low fraction of firms with a single customer are Coal (19.4%) and Automobiles and Trucks (15.0%). In addition, customer firms mainly belong to the Retail sector (14.9%), Automobiles and Trucks (10.22%), Petroleum and Natural Gas (8.1%), Communication (7.4%), and Computers (7.2%). In later analyses, I refine the industry classification to the 4-digit SIC level.

Table 2.1 presents summary statistics for suppliers and customers in my sample. Definition of all variables are presented in the appendix (Appendix B.1). Compared with the median customer firm, the median supplier firm is much smaller, much younger, and maintains a lower leverage ratio. Supplier firms in my sample account for around 10% of the complete Compustat universe. Compared with the median out-of-sample Compustat firm, the median supplier firm in my sample is similar in firm size and firm age, but maintains a lower leverage ratio (by around 9%) and spends a higher fraction of its revenue in R&D (by around 4%), which implies that firms with principal customers tend to invest more in R&D than firms without principal customers, potentially because they want to differentiate their products according to their customers' requirements. These results are unreported for brevity.

2.4.2 Model Specification and Variable Definitions

I first test whether firm performance is affected by production cluster competition (Hypothesis 3). I follow Bertrand and Mullainathan [78] and Giroud and Mueller [76] to exploit exogenous variations in external corporate governance through passages of business combination (BC) laws. The BC laws were passed between 1985 and 1991 on a state-by-state basis and applied to the state of incorporation. Table B.1 in the appendix summarizes the timing of passages and describes the sample by state of incorporation and state of location. The main regression specification is:

$$\begin{aligned} ROA_{it} = \alpha_t + \beta_i + \gamma_1 BC_{it} + \gamma_2 \mathit{Number\ of\ Firms\ in\ Cluster}_{it} \\ + \gamma_3 BC \times \mathit{Number\ of\ Firms\ in\ Cluster}_{it} + \delta X_{it} + \epsilon_{it}, \end{aligned} \tag{2.1}$$

where i indexes supplier firm; t indexes year; ROA_{it} is the dependent variable, computed as net income divided by total assets; BC_{it} is an indicator that equals one if the supplier firm is incorporated in a state that has passed a BC law by year t; $Number\ of\ Firms\ in\ Cluster_{it}$ is the main measure for competition within a given production cluster, described below in more details; X_{it} is a vector of controls; α_t and β_i are year fixed effects and production cluster fixed effects (i.e. customer firm times supplier 4-digit SIC industry fixed effects) that account for unobserved heterogeneity across time and production clusters.

As a robustness check, I use an alternative specification, where I exclude all time-varying controls (X_{it}) and year fixed effects (α_t) in Equation (2.1) but include state-year fixed effects following Gormley and Matsa [100], who suggest that the inclusion of time-varying controls can introduce a bias if any of these control variables are affected by passages of BC laws.[5]

2.4.2.1 Production Cluster Structure

The main variables of interest in Equation (2.1) are production cluster size (*Number of Firms in Cluster*) and its interaction with supplier BC indicator ($BC \times$ *Number of Firms in Cluster*). *Number of Firms in Cluster* is computed as the number of firms that share a same customer and belong to a same 4-digit SIC industry in a given year, which is effectively an equally-weighted measure for competition within a production cluster. The value of *Number of Firms in Cluster* can run from

[5] I use an iterative procedure described in Gormley and Matsa [53] to overcome the computational difficulties associated with high-dimensional state-year fixed effects.

1 to infinity, where a higher value indicates more intense competition.

In addition, I consider a value-weighted measure for competition, *Cluster-level HHI*, the Herfindal-Hirschman index at the production-cluster level, computed as the sum of squared input shares of firms that supply to a same customer and belong to a same 4-digit SIC industry in a given year. The input shares are based on dollar sales. The value of *Cluster-level HHI* is bounded between 0 and 1, where a higher value indicates a lower level of competition.

Figure 2.1 presents two production cluster examples. In the first case, *Supplier 0* is the only firm in its industry j that supplies to *Customer A*. In the second case, five suppliers (*Supplier 1–5*) in industry j supply to *Customer B*. Based on the above definition, both *Number of Firms in Cluster* and *Cluster-level HHI* equal one for *Supplier 0*. Suppose in the second case, *Supplier 1* alone accounts for 80% of the total input share to *Customer B*, while each of other four suppliers accounts for 5% of the total input share. Then for *Supplier 1–5*, *Number of Firms in Cluster* equals five and *Cluster-level HHI* equals 0.65.

The above examples illustrate the difference between the two measures for production cluster competition. Both measures capture the same non-competitiveness in the first case in Figure 2.1, but they suggest different levels of competition in the second case. When the majority of input to a shared customer is provided by one major supplier as in the second case, the equally-weighted *Number of Firms in Cluster* would suggest a higher level of competition than the value-weighted *Cluster-level HHI*. In these cases, *Number of Firms in Cluster* captures shadow competition between peripheral suppliers when input sales are highly concentrated, while *Cluster-*

level HHI ignores such shadow competition. Thus, I adopt the equally-weighted measure *Number of Firms in Cluster* as a main measure for competition.

Based on my sample, the average production cluster size (*Number of Firms in Cluster*) is between 2 to 3 firms (panel A of Table 2.1). The average Herfindal-Hirschman index at the production-cluster level (*Cluster-level HHI*) is 0.85, which indicates that customers may source from major suppliers as well as peripheral suppliers.

Equation (2.1) adopts a difference-in-difference-in-difference (DDD) methodology. The DDD approach allows me to disentangle the two competing effects of production cluster competition. Hypothesis 3 predicts a negative γ_2 and a positive γ_3. When *Number of Firms in Cluster* is replaced with *Cluster-level HHI*, the predicted signs are reversed, since a higher concentration index indicates less competition.

To test Hypothesis 4, I replace *Number of Firms in Cluster* in Equation (2.1) with two measures defined broadly based on supplier industry: *Number of Firms in Industry*, defined as the number of firms in a same industry in a given year, and *Industry-level HHI*, the Herfindal-Hirschman index at the industry level, computed as the sum of squared market shares of firms in a same 4-digit SIC industry in a given year. Hypothesis 4 predicts that both γ_2 and γ_3 would become smaller and less significant when production cluster-level measures are replaced with industry-level measures.

Panel A of Table 2.1 indicates differences between production cluster structure and industry market structure. By definition, the average production cluster size (*Number of Firms in Cluster*) is much smaller than the average industry size (*Number of Firms in Industry*), and the average Herfindal-Hirschman index at the production cluster-level (*Cluster-level HHI*) is much higher than the average Herfindal-Hirschman index at the industry-level (*Industry-level HHI*).[6]

2.4.2.2 Control Variables

Table 2.2 reports results from mean comparison between firms incorporated in a state that passed a BC law during the sample period ("Eventually BC" group) and firms incorporated in a state that never passed a BC law during the sample period ("Never BC" group). The average firm in the "Eventually BC" group is significantly larger, older, and spend less in capital expenditures than the average firm in the "Never BC" group. Thus, in my main regression analyses, I control for firm size (*Log(Total Assets)*), firm age (*Log(1+Firm Age)*), leverage ratio (*Leverage*), and capital expenditures (*Capital Expenditures*). I also control for customer firm size (*Log(Customer's Total Assets)*). In robustness analyses, I include additional supplier and customer firms' characteristics as controls.

In addition, I follow Bertrand and Mullainathan [78] and Giroud and Mueller [76] to control for local and industry shocks by including a set of time-varying industry and state of location controls, computed as the mean value of *ROA* within a firm's industry and the mean value of *ROA* within a firm's state of location, excluding the firm itself (*Mean(ROA) by Industry-year* and *Mean(ROA) by State-year*).

[6]Based on my sample, the correlation between *Number of Firms in Cluster* and *Number of Firms in Industry* is 0.42. The correlation between *Cluster-level HHI* and *Industry-level HHI* is 0.21.

In robustness analyses, I include indicators for passages of other state-level anti-takeover laws and indicators for court decisions related to BC laws as additional controls. Karpoff and Wittry [102] suggest that the size and direction of a law's effect on a firm's takeover protection depend on coverage by other anti-takeover laws in addition to BC laws and important court decisions. Prior to passages of BC laws, first-generation anti-takeover laws were adopted by 38 states from 1968 through 1981, which regulated cash tender offers and imposed strong takeover protections. In the meantime of passages of BC laws, four other anti-takeover laws became prominent. These include poison pill laws, which explicitly authorize firms to adopt poison pill takeover defenses, control share acquisition laws, which require a bidder to obtain supermajority voting support to complete an acquisition, directors' duties laws, which expand board of directors' duties to include non-investor stakeholders, and fair price laws, which regulate the bidding price involving a large shareholder. The timing of passages of these laws is presented in the Appendix (Table B.2). Furthermore, the constitutionality of BC laws was first established by a court decision involving *Amanda Acquisition Corp. v. Universal Foods Corp.* in 1989, after the passages of BC laws in several states.

To mitigate potential biases caused by outliers, all financial variables are winsorized at the 1% level. Firm sizes are adjusted for inflation. Definition of all variables are presented in the appendix (Appendix B.1).

2.5 Results and Robustness Analyses

2.5.1 Main Results

I first examine the impact of production cluster competition on supplier firm performance by estimating Equation (2.1). Production cluster is defined as a single or a group of supplier firms in a given 4-digit SIC code industry that supply to a same customer. The results are presented in Table 2.3. In columns 1–4, production cluster competition is defined by the number of supplier firms in a given production cluster (*Number of Firms in Cluster*). Column 1 shows that supplier firm performance is negatively correlated with production cluster size (*Number of Firms in Cluster*) and positively correlated with its interaction term with the BC law dummy (*BC* $\times$ *Number of Firms in Cluster*), controlling for supplier and customer firms characteristics, industry-year effects, state-year effects, year fixed effects, and supplier industry fixed effects. The coefficient estimates are statistically significant at 1%-5% levels. In column 2, supplier industry fixed effects are replaced with production cluster fixed effects in order to control for unobserved heterogeneity across production clusters. The signs and significance levels remain similar. The coefficient estimate on *Number of Firms in Cluster* in column 2 suggests that each additional peer supplier within a production cluster is associated with a 0.4 percentage-point decline in a supplier firm's *ROA*. The coefficient estimate on the interaction term in column 2 indicates that a supplier firm in a larger production cluster experiences a smaller drop in *ROA* following the passage of a BC law in the firm's state of incorporation. The magnitude is 0.2 percentage point for each additional peer supplier, which suggests that competition substitutes for takeover pressure as a governance mechanism. Thus, a firm benefits from competing with peer supplier companies when the firm is exposed to less takeover threat.

The effect of production cluster competition may depend on supplier firms' outside options. The full sample includes firms that supply to more than one customers in a given year. In robustness analyses, I include control variables for supplier firms' outside options and obtain similar results. In columns 3 and 4 of Table 2.3, the sample is restricted to production clusters in which each supplier is observed to serve only one customer. In other words, I exclude all supplier firms that supply to more than one customers. The subsample observations can be uniquely identified by supplier-year combination. Estimates in columns 3 and 4 have the same signs with estimates in columns 1 and 2, and have a higher level of significance and a larger magnitude. The results in column 4 indicate that each additional peer supplier is associated with a 1.0 percentage-point drop in ROA, but mitigates the decline in ROA by 0.7 percentage point when a firm faces is less takeover pressure.

In unreported tables, I find that the incremental value of an additional peer supplier declines with the production cluster size. Intuitively, if a firm was previously the exclusive source for its customer, it may face intense competition once a new supplier enters the production cluster. If a supplier firm has been already competing with many peer suppliers in a production cluster, the firm may be less sensitive to an entrant supplier. When I restrict my attention to firms that are observed monopoly suppliers within their production clusters, I find that each additional peer supplier reduces *ROA* by 2.6 percentage points, and mitigates the decline in *ROA* by 1.6 percentage points after takeover pressure decreases. When I restrict my attention to observed duopoly suppliers, I find that above effects shrink to 0.7 percentage point and 0.1 percentage point, respectively.

In columns 5–8, production cluster competition is measured by *Cluster-level HHI*, the sum of squared input shares of supplier firms in a given production cluster, where input shares are based on sales. I find positive coefficient estimates on *Cluster-level HHI* and negative coefficient estimates on its interaction with the BC law dummy in all four specifications, consistent with the previous findings. The statistical significance is lower compared to columns 1–4. One possible reason is that equally-weighted production cluster size and value-weighted Herfindal-Hirschman index captures different information about production cluster structure. There may exist shadow competition between peripheral suppliers even if input sales are highly concentrated. Such shadow competition would be reflected in production cluster size but not in Herfindal-Hirschman index. In later analyses, I test whether the results depend on the shadow competition.

Overall, the results in Table 2.3 indicate that production cluster competition reduces firm profits, but mitigate the decline in firms' operating performance when firms face less takeover pressure, which is consistent with Hypothesis 3.

2.5.2 Dynamic Effects

To address the reverse causality concern and explore the dynamic effects of BC laws, I replace *BC* in Equation (2.1) with four indicators: *BC(-1)*, *BC(0)*, *BC(1)*, and *BC(2+)*. *BC(-1)* equals one if a firm is incorporated in a state that will pass a BC law in the following year. *BC(0)* equals one if a firm is incorporated in a state that passes a BC law in a given year. *BC(1)* equals one if a firm is incorporated in a state that passed a BC law one year ago. *BC(2+)* equals one if a firm is incorporated in a state that passed a BC law at least two years ago.

The results are reported in Table 2.4. The estimates on the interaction between *BC(-1)* and production cluster competition are insignificant in all specifications, which suggests no pre-existing trends in supplier firm performance. In addition, estimates on the interaction between *BC(2+)* and *Number of Firms in Cluster* are significant at 1%-5% levels in all specifications, and generally have larger magnitudes than estimates on the interaction between *BC(0)* and *Number of Firms in Cluster*, and the interaction between *BC(1)* and *Number of Firms in Cluster*. Overall, the results in Table 2.4 mitigate the concern that lobbying efforts by unproductive monopolists are driving passages of BC laws.

2.5.3 Potential Competitors

The main analysis provides evidence for a tangible impact of production cluster structure on firm performance. In this section, I explore the difference between production clusters and broadly defined industry groups. To test Hypothesis 4, I replace the production-cluster level measures for competition in Equation (2.1) with industry-level measures. Hypothesis 4 predicts that the inclusion of potential competitors outside the production cluster would make the effect of competition weaker.

Table 2.5 presents the results. The coefficient estimate on supplier industry size (*Number of Firms in Industry*) is significant but smaller in magnitude than the estimate on production cluster size (*Number of Firms in Cluster*) in Table 2.3. The coefficient estimates on industry-level Herfindal-Hirschman index (*Industry-level HHI*) and interaction terms (*BC* $\times$ *Number of Firms in Industry* are insignificant in all specifications. These results are consistent with Hypothesis 4, which suggests that supplier firm performance is more affected by peer suppliers within a production cluster than by potential rivals that supply to different customers.

2.5.4 Peripheral Suppliers and Major Suppliers

The two measures for production cluster structure (*Number of Firms in Cluster* and *Cluster-level HHI*) may capture different information about production cluster structure. In this section, I explore one potential reason for the difference between the two measures– shadow competition between peripheral suppliers. Barrot and Sauvagnat [87] find that suppliers hit by natural disasters impose substantial losses on their customers. Thus, customer companies may source from peripheral suppliers to insurance against a disrupted major supplier. However, when major suppliers and peripheral suppliers coexist within a production cluster, the two types of suppliers may face different levels of competition. Customer companies may develop stable relationships with few major suppliers that account for a larger proportion of input, while frequently replace the other peripheral suppliers that supply a smaller fraction of input. Unlike the equally-weighted production cluster size measure, the value-weighted Herfindal-Hirschman measure would ignore shadow competition between peripheral suppliers.

I test Hypothesis 3a and Hypothesis 3b by estimating Equation (2.1) on subsamples of peripheral suppliers and subsamples of major suppliers. I expect that the estimate on *Number of Firms in Cluster* for peripheral suppliers would be larger in magnitude and have a higher level of significance than the estimate on *Number of Firms in Cluster* for major suppliers, as predicted by Hypothesis 3a. In addition, I expect that the estimate on the interaction term for major suppliers would be larger in magnitude than the estimate for peripheral suppliers, as predicted by Hypothesis 3b.

The results are reported in Table 2.6. The sample is restricted to production clusters in which multiple suppliers are observed to supply to only one customer. Since a firm that supplies a large fraction of input to one customer may at the same time supply a small fraction of input to another customer, I exclude supplier firms with multiple customers. This sacrifices some power of the tests but mitigates the confusion in identifying peripheral and major suppliers. In all specifications, I include production cluster fixed effects.

Columns 1–4 in Table 2.6 report results for peripheral suppliers. A supplier is considered peripheral if it supplies no more than a threshold share of input to the common customer within a production cluster. The threshold is 20%, 30%, 40%, and 50% in columns 1–4, respectively. Columns 5–8 present results for the major suppliers. A supplier is considered major if it supplies at least a threshold share of input to the common customer within a production cluster. The threshold is 50%, 60%, 70%, and 80% in columns 5–8, respectively. Thus, the importance of a supplier to its customer increases from column 1 to column 8. Column 1 reports the results for the least important suppliers and column 8 reports the results for the most important suppliers.

The results in Table 2.6 lend some support for Hypothesis 3a. The coefficient estimates on *Number of Firms in Cluster* are significant in columns 1–4 but not in columns 5–8, which suggests that competition drives down firm profits only if the firm is a peripheral supplier within a production cluster. In addition, these estimates are smaller in magnitude in less peripheral subsamples. When a supplier firm's sales account for no more than 20%, each additional rival is associated with a 1.6 percentage-point drop in its *ROA* (column 1). When a supplier firm's sales account for no more than 40% of all the input to its customer, each additional rival within a production cluster is associated with a 1.3 percentage-point decline in its *ROA* (column 4).

In addition, the results in columns 5–8 provide some support for Hypothesis 3b. The coefficient estimates on the interaction between the BC dummy and production cluster size are significant in all specifications, and are slightly larger in magnitude for major suppliers than for peripheral suppliers. For peripheral suppliers that face less takeover threat, each additional peer supplier mitigates the decline in *ROA* by 0.9 to 1.1 percentage points. For major suppliers, each additional peer supplier mitigates the decline in ROA by 1.0 to 1.7 percentage points. Thus, the results show that the net effect of competition on performance is less negative for major suppliers. Furthermore, the coefficient estimates on the BC dummy are significant for major suppliers but not for peripheral suppliers, which indicates that major suppliers are more affected by the passage of BC laws. These results are consistent with existing evidence that larger firms generally have more issues in corporate governance than smaller firms (e.g., Jensen and Meckling [2]; Fama and Jensen [92]) and thus have larger room for improvement.

2.5.5 Input Specificity

In this section, I investigate whether the effect of production cluster competition varies by input specificity. I expect that the effect of competition between peer suppliers would be driven by the subsample in which firms supply highly specific inputs to their customers, as predicted by Hypothesis 4. The more specific or differentiated an input is, the more costly for a customer to replace an existing supplier, and thus production cluster size would be more relevant. I adopt two measures for input specificity following Giannetti *et al.* [80] and Barrot and Sauvagnat [87], and estimate Equation (2.1) based on subsamples of firms producing specific inputs and firms producing non-specific inputs.

The results are reported in Table 2.7. In columns 1 and 2, the level of input specificity is considered high if a firm belongs to a sector producing non-standardized goods, and is considered low if the firm belongs to a sector producing standardized goods. I follow the product classification in Giannetti *et al.* [80], which itself is based on Rauch [101], who classifies 2-digit SIC codes sectors into standardized (sectors that produce goods with a clear reference price listed in trade publications), differentiated (sectors that produce goods with multidimensional characteristics, and therefore highly heterogeneous prices), and services (the remaining sectors). Giannetti *et al.* [80] suggest that the standardized sectors are less expensive to liquidate and less adapted to the needs of specific buyers. According to their classifications, examples of standardized industries include Food and Chemicals; examples of differentiated industries include Machinery and Electronic Equipment. In columns 3 and 4, the level of input specificity is considered high if a firm has a higher lagged ratio of R&D expenditures over sales than the median firm within an industry, and is considered low otherwise. This definition can be extended to the past two years and the results are similar.

The results in Table 2.7 show that the effect of production cluster competition is more evident in the subsample in which suppliers produce highly specific inputs. The findings are consistent with Hypothesis 4 and indicate that existing suppliers face intense competition to crowd out each other when they are less likely to be replaced with new suppliers outside a production cluster.

2.5.6 Revenues or Costs?

In this section, I explore whether production cluster structure affects firm performance through revenues or costs. I restrict my attention to suppliers that serve an exclusive customer as observed in my sample, and estimate variants of Equation (2.1), where the dependent variables are replaced with sales and costs measures. The preliminary results are reported in Table 2.8.

In column 1, the dependent variable is annual growth rate of sales. The coefficient estimates on production cluster size and on the interaction term are both insignificant. In column 2, the dependent variable is costs of goods sold divided by sales. In column 3, the dependent variable is selling, general and administrative expenses divided by sales. The estimates on the interaction term between the BC dummy and production cluster size are negative and significant in both columns, which suggests that production cluster competition disciplines supplier firms through cost-savings.

2.5.7 Customer Governance Strength

Customer companies may play an important role in monitoring and bargaining with their suppliers. In this section, I explore whether the effect of production cluster competition on supplier firm performance depends on takeover threat faced by customer companies.

First, I conduct mean comparison tests based on a subsample of firms that supply to an observed exclusive customer. I split the sample according to whether a firm's customer company is incorporated in a state that passed a BC law during the sample period ("Customer Eventually BC" group vs. "Customer Never BC" group). The two groups do not have significantly different mean values of supplier firms' *ROA* (panel A of Table 2.9). In addition, I further distinguish between firms that are observed exclusive supplier within a production cluster and firms that compete with peer suppliers. When firms have observed exclusive relationships with their customers, firms in the "Customer Eventually BC" group have a similar mean value of *ROA* with firms in the "Customer Eventually BC" group (panel B). When firms face competition within a production cluster, firms in the "Customer Eventually BC" group have a lower mean value of *ROA* (panel C).

I then conduct regression analyses. Column 1 of Table 2.10 presents results from regressing *ROA* on an indicator for the passage of a BC law in a firm's customer company' state of incorporation (*Customer BC*), and the same set of control variables and fixed effects as in Table 2.3. The coefficient estimate on *Customer BC* is insignificant. One possible explanation is that two competing effects of customer BC laws exist. A less governed customer may bring benefits to its suppliers because the customer may have a smaller incentive to bargain for lower input prices. However, a less governed customer may adversely affect suppliers' productivity because the customer may have a smaller incentive to monitor its suppliers.

Column 2 reports results from estimating a variant of Equation (2.1), where the BC law dummy is replaced with the customer BC law dummy. I find a negative coefficient estimate on production cluster competition (*Number of Firms in Cluster*) and a positive estimate on the interaction term (*Customer BC*$\times$*Number of Firms in Cluster*).

Both estimates are statistically significant at the 1% level. These results are consistent with the evidence from mean comparison tests, and indicate that production cluster competition drives down firm profits to a lesser extent (by 0.7 percentage point) when the shared customer faces less takeover pressure, presumably because competition substitutes for monitoring by the customer.

Column 3 reports results from estimating a variant of Equation (2.1), where I include the customer BC law dummy and its interaction with production cluster competition and with the BC law dummy. The coefficient estimate on the interaction between customer BC dummy and supplier BC dummy is significantly negative, which suggests that customer governance issues may exacerbate supplier governance issues, and thus drive down supplier performance (by 7.3 percentage points). These results are consistent with the mean comparison of supplier firms' board governance variables in Table 2.9. Firms in the "Customer Eventually BC" group, on average, have larger boards and a lower fraction of independent directors than firms in the "Customer Never BC" group. These differences are statistically significant when supplier firms compete with peer suppliers (panel C).

In addition, I investigate whether the effect of customer depends on the importance of a supplier firm using a similar methodology as in Table 2.6. Table 2.11 reports results from subsample tests between peripheral suppliers and major suppliers. In general, estimates on the interaction term (*Customer BC* $\times$ *Number of Firms in Cluster*) are larger in magnitude and have a higher level of significance for major suppliers than for peripheral suppliers. This evidence is consistent with existing findings that larger firms have more room for improving governance strength.

Finally, I find that the effect of production cluster competition is mainly driven by the subsample in which at least one peer supplier is incorporated in a state that has passed a BC law (Table 2.12). One possible explanation is that the shared customer may put more efforts into monitoring its suppliers when more than one of them are weakly governed by takeover markets.

Overall, the results are consistent with Hypothesis 5, which suggests that customer companies' governance strength matter for supplier firm performance.

2.5.8 Robustness

I use several different specifications for robustness checks. First, I include sales growth and stock volatility as additional control variables. In addition, I identify whether a supplier firm is a customer of another firm, and control for supplier firms' upstream supply chain relationships.[7] Also, in full sample tests, I control for the total number of customers of a supplier firm, or, alternatively, the Herfindal-Hirschman index measure of customer industry concentration, in order to account for a supplier's outside options. Furthermore, I use EBIT and profit margins as alternative performance measures. The results are robust to these alternative specifications and are not reported for brevity.

I also investigate whether the results are subject to estimation biases due to the BC law approach. Karpoff and Wittry [102] suggest that the BC law's effect

[7] It is not common, however, for suppliers in my sample to have upstream supply chain relationship.

on a firm's takeover protection depends on pre-existing and other contemporary anti-takeover mechanisms, including first-generation anti-takeover laws, poison pill laws, control share acquisition laws, directors' duties laws, fair price laws, and court decisions that establish the constitutionality of BC laws. I control for these anti-takeover laws and court decisions, and find robust results (columns 1 and 2 of Table 2.13). In addition, I restrict the sample to pre-1997 period. Using an earlier sample period mitigates potential biases due to a long estimation window around passages of BC laws. Also, some firms restated their customer segment information following a change in the disclosure rules in 1997. The results actually become even stronger based on the pre-1997 sample. Furthermore, the results are robust to excluding firms incorporated in Delaware. These results are unreported for brevity.

In addition, I use an alternative regression model, where I replace all time-varying controls (X_{it}) and year fixed effects in Equation (2.1) with state-year fixed effects following Gormley and Matsa [100], who suggest that the inclusion of time-varying controls can introduce a bias if any of these controls is affected by passages of BC laws. The results remain similar (columns 3 and 4 of Table 2.13).

Finally, my results remain similar in several other robustness checks. The results are robust to using 3-digit SIC codes and text-based industry classifications by Hoberg and Phillips [95].[8] In addition, I exclude voluntarily reported customers below the 10% sales threshold. Furthermore, I use the logged value of production cluster size (*Number of Firms in Cluster*) in order to address potential outliers,

[8] However, using text-based industry classifications can lead to a loss of power, because the industry classification is available after 1996, when all existing BC laws have passed.

and use alternative performance measures such as EBIT and profit margins. In unreported tables, I find robust results using the above alternative specifications.

2.6 Conclusion

This paper explores the effect of production cluster structure on supplier firm performance, where production cluster refers to a group of suppliers that supply substitutable inputs to a common customer. Based on a sample of 47,945 supplier-customer pair-year observations reported by US public listed firms during the period 1976–2009, I find that each additional peer supplier within a production cluster mitigates the decline in supplier firms' ROA by 0.2 percentage point when takeover pressure suddenly decreases, which implies that competition substitutes for external governance. When the sample is restricted to firms that are observed to serve only one customer, each additional peer supplier mitigates the decline in ROA by 0.7 percentage point. The effect of competition becomes less significant when the cluster definition is extended to include supplier firms that supply to different customers.

In addition, the effect of production cluster competition depends on whether a firm is a major supplier or peripheral supplier. In the absence of changes in takeover pressure, competition reduces firm profits more pronouncedly for peripheral suppliers. When a supplier firm's sales account for no more than 20% of its customer's total input, each additional rival reduces the firm's ROA by 1.6 percentage points. However, when a supplier firm's sales account for more than 50% of its customer's total input, the supplier firm's ROA is insensitive to production cluster size. Furthermore, when a supplier firm faces less takeover pressure, competition mitigates the decline in operating performance more evidently for major suppliers. Each additional rival within a production cluster mitigates the decline in a major supplier's ROA by 1.7 percentage points, while mitigates the decline in a peripheral supplier's ROA by 1.0 percentage point. These results are consistent with existing evidence that larger firms generally have more governance issues and thus have more room for improvement.

Furthermore, the effect of production cluster competition is mainly driven by firms that have invested more heavily in R&D, and by firms that produce non-standardized goods. These results imply that customers may face higher costs of switching to new sources when supplier firms produce more specific inputs.

Finally, I provide preliminary evidence that the effect of production cluster competition varies by customer firms' governance strength. I find that production cluster competition drives down supplier firms' ROA to a lesser extent (by 0.7 percentage point) when the shared customer faces an exogenous decline in takeover pressure, which implies that competition substitutes for monitoring by the shared customer. In addition, this result is mainly driven by the subsample of major suppliers rather than peripheral suppliers. Furthermore, I find that the effect of production cluster competition is mainly driven by the subsample in which peer suppliers face less takeover pressure, which suggests that the shared customer may put more efforts into monitoring its suppliers when more than one of them are weakly governed.

Overall, my findings suggest that production cluster structure is an important determinant of supplier firm governance and performance.

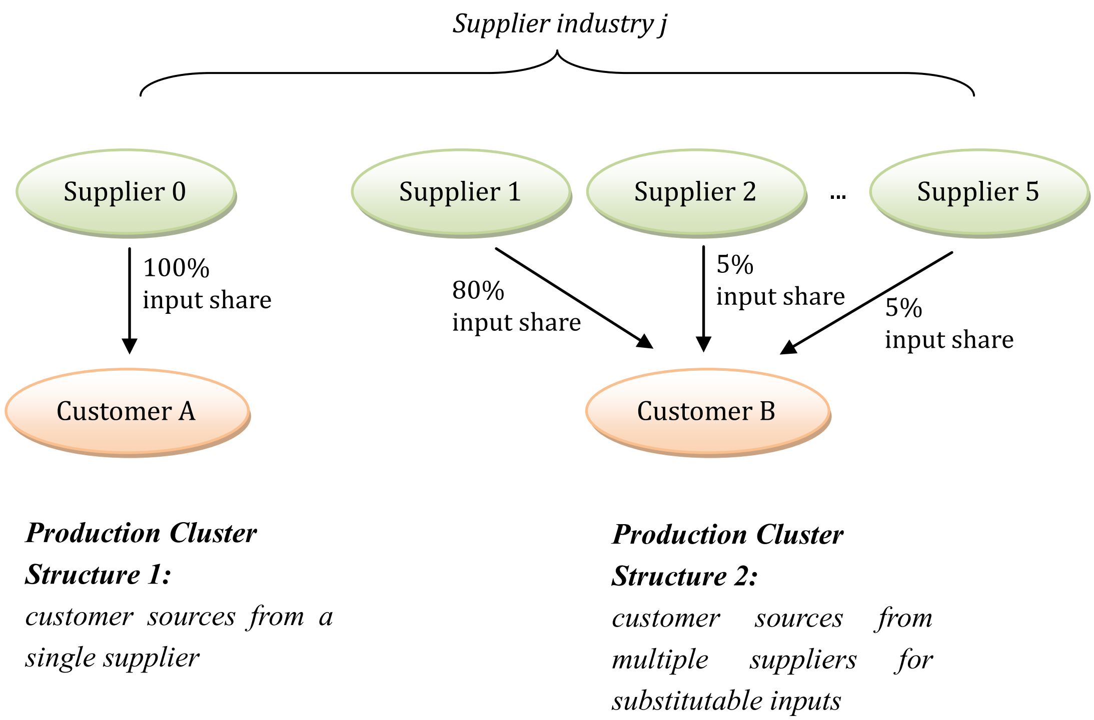
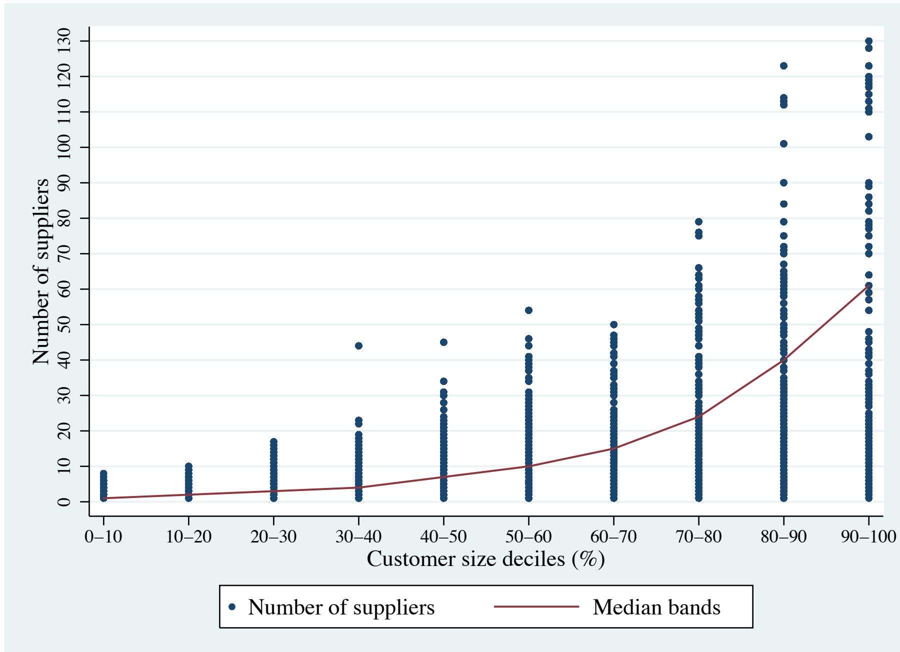


Figure 2.1: Examples of Production Clusters

Panel A: Suppliers from All Sector



Panel B: Suppliers from Same Sector

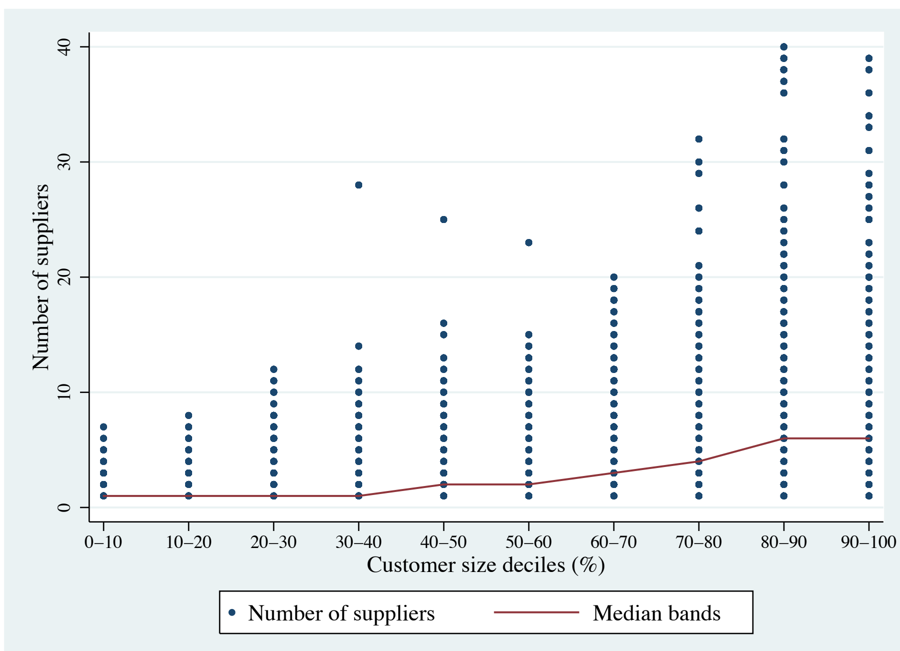


Figure 2.2: Number of Supplier Firms by Customer Size

This figure illustrates the relationship between customer size and the number of suppliers during 1976–2009. Customer size is defined by customer company's total sales. Panel A accounts for the total number of supplier firms to a given customer company. Panel B accounts for the number of supplier firms within a same 2-digit SIC sector to a given customer.

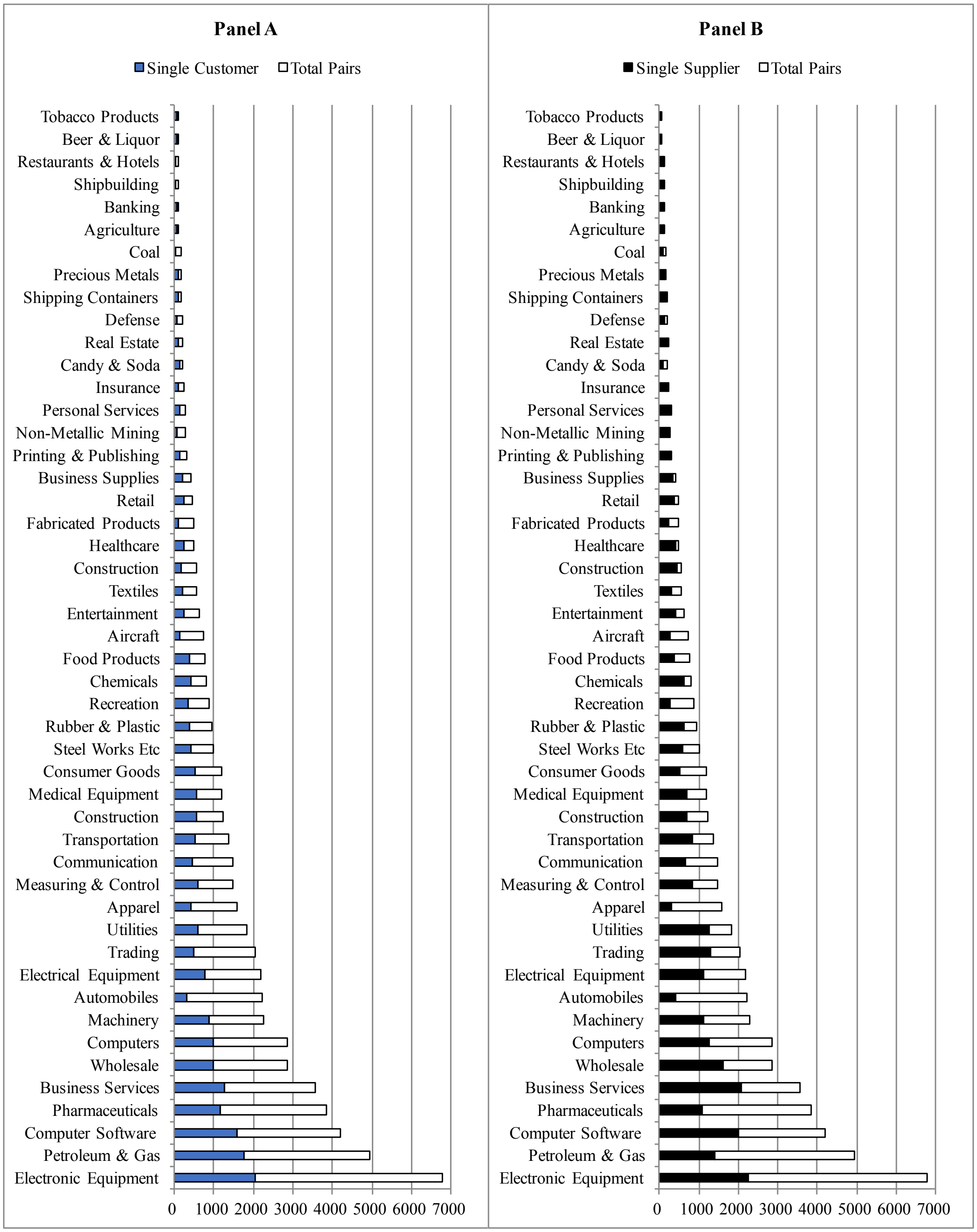


Figure 2.3: Number of Customer-Supplier Relationships by Industry

This figure presents the number of supplier-customer relationships during 1976–2009 by Fama and French [52] 48 industry. Blue bars in panel A indicate relationships in which supplier firms supply to an exclusive customer company as observed in the sample. Black bars in panel A indicate relationships in which customer companies source from an exclusive supplier firm as observed in the sample. White bars indicate the total number of relationships.

Table 2.1: Summary Statistics

This table reports summary statistics for supplier firms and for customer firms. The sample consists of US public firms during the period 1976–2009 with available financial data from Compustat and information on firms' relationships with US public customer companies from Compustat Customer Segment Files. Variables include: *Number of Firms in Cluster*, defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer; *Number of Firms in Industry*, defined as the number of firms within an 4-digit SIC industry; *Cluster-level HHI*, computed as the sum of squared market shares of all firms in a given production cluster; *Industry-level HHI*, computed as the sum of squared market shares of all firms in a given 4-digit SIC industry; *ROA*, computed as net income divided by total assets; *Log(Total Assets)*, computed as the logged value of total assets; *Firm Age*, defined as the number of years listed in Compustat; *Leverage*, computed as total liabilities divided by total assets; and *Capital Expenditures*, computed as capital expenditures divided by total assets. Detailed definitions of all variables are presented in Appendix B.1.

Panel A: Supplier Firms

	(1) Mean	(2) Std. Dev.	(3) *p*25	(4) Median	(5) *p*75
Number of Firms in Cluster	2.355	3.603	1.000	1.000	2.000
Number of Firms in Industry	33.479	42.828	5.000	14.000	44.000
Cluster-level HHI	0.845	0.256	0.695	1.000	1.000
Industry-level HHI	0.315	0.281	0.099	0.214	0.453
ROA	-0.071	0.365	-0.076	0.027	0.073
Log(Total Assets)	4.575	2.082	3.104	4.417	5.936
Firm Age	13.381	11.871	5.000	9.000	18.000
Leverage	0.455	0.229	0.264	0.457	0.626
Capital Expenditures	0.069	0.079	0.021	0.044	0.085
Observations	32802				

Panel B: Customer Firms

	(1) Mean	(2) Std. Dev.	(3) *p*25	(4) Median	(5) *p*75
ROA	0.026	0.160	0.012	0.041	0.073
Log(Total Assets)	7.807	2.003	6.576	7.969	9.296
Firm Age	23.617	15.741	9.000	22.000	36.000
Leverage	0.570	0.185	0.455	0.589	0.703
Capital Expenditures	0.069	0.059	0.033	0.055	0.089
Observations	17341				

Table 2.2: Mean Comparison Tests

This table presents mean comparison results. Column 1 reports mean values for the subsample of firms incorporated in a state that passed a business combination (BC) law during the sample period. Column 2 reports mean values for the subsample of firms incorporated in a state that never passed a BC law during the sample period. Column 3 reports p-value statistics from a t-test of the difference between "Eventually BC" and "Never BC" groups. Main variables include: *Number of Firms in Cluster*, defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer; *Number of Firms in Industry*, defined as the number of firms within an 4-digit SIC industry; *Cluster-level HHI*, computed as the sum of squared market shares of all firms in a given production cluster; *Industry-level HHI*, computed as the sum of squared market shares of all firms in a given 4-digit SIC industry; and *ROA*, computed as net income divided by total assets. Definitions of all variables are presented in Appendix B.1.

	(1) Mean for Firms Eventually BC	(2) Mean for Firms Never BC	(3) p-value (difference)
Number of Firms in Cluster	2.384	2.157	0.000
Number of Firms in Industry	33.542	33.044	0.500
Cluster-level HHI	0.843	0.858	0.000
Industry-level HHI	0.315	0.320	0.216
ROA	-0.068	-0.090	0.001
Log(Total Assets)	4.677	3.863	0.000
Firm Age	13.621	11.705	0.000
Leverage	0.458	0.435	0.000
Capital Expenditures	0.067	0.083	0.000
Observations	32802		

Table 2.3: BC Laws and Competition between Suppliers Sharing Customers

This table reports estimates from regressing operating performance on a business combination law dummy, competition, their interaction term, a set of control variables, and fixed effects (Equation 2.1). The dependent variable is *ROA*. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer. *Cluster-level HHI* is computed as the sum of squared market shares of all firms in a given production cluster. Columns 3, 4, 7, 8 report results for the subsample of firms supplying to an exclusive customer as observed in the sample. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the 4-digit SIC industry level in odd columns and at the production-cluster level in even columns. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA							
	Full Sample		Single Customer Subsample		Full Sample		Single Customer Subsample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BC	-0.013	-0.005	-0.022**	-0.024	0.015	0.015	0.013	0.007
	(0.009)	(0.009)	(0.010)	(0.017)	(0.009)	(0.010)	(0.017)	(0.022)
Number of Firms in Cluster	-0.002**	-0.004***	-0.007***	-0.010***				
	(0.001)	(0.001)	(0.001)	(0.003)				
BC × Number of Firms in Cluster	0.004***	0.002**	0.009***	0.007***				
	(0.001)	(0.001)	(0.001)	(0.002)				
Cluster-level HHI					0.012	0.010	0.025	0.014
					(0.008)	(0.009)	(0.017)	(0.021)
BC × Cluster-level HHI					-0.024**	-0.020*	-0.018	-0.008
					(0.011)	(0.010)	(0.019)	(0.024)
Mean(ROA) by Industry-year	0.301***	0.274***	0.071*	0.090***	0.301***	0.275***	0.072*	0.091***
	(0.034)	(0.023)	(0.038)	(0.021)	(0.034)	(0.023)	(0.038)	(0.021)
Mean(ROA) by State-year	0.263***	0.208***	0.116***	0.104**	0.265***	0.208***	0.117***	0.107**
	(0.036)	(0.030)	(0.029)	(0.051)	(0.036)	(0.030)	(0.029)	(0.051)
Log(Total Assets)	0.117***	0.124***	0.134***	0.153***	0.116***	0.124***	0.134***	0.154***
	(0.013)	(0.009)	(0.013)	(0.017)	(0.013)	(0.009)	(0.013)	(0.017)
Log(Total Assets)-squared	-0.007***	-0.007***	-0.008***	-0.009***	-0.007***	-0.007***	-0.008***	-0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(1+Firm Age)	0.011*	-0.001	0.012**	-0.001	0.010*	-0.000	0.013**	-0.001
	(0.006)	(0.004)	(0.005)	(0.007)	(0.006)	(0.004)	(0.005)	(0.008)
Leverage	-0.271***	-0.355***	-0.254***	-0.380***	-0.270***	-0.355***	-0.254***	-0.378***
	(0.024)	(0.013)	(0.029)	(0.030)	(0.024)	(0.013)	(0.030)	(0.030)
Capital Expenditures	-0.063	-0.065*	-0.133*	-0.176**	-0.062	-0.065*	-0.129	-0.171**
	(0.044)	(0.036)	(0.079)	(0.079)	(0.044)	(0.035)	(0.079)	(0.079)
Log(Customer's Total Assets)	-0.000	-0.006	0.001	0.006	-0.000	-0.009*	0.001	0.002
	(0.002)	(0.005)	(0.003)	(0.011)	(0.001)	(0.005)	(0.003)	(0.011)
Observations	47945	47945	14724	14724	47945	47945	14724	14724
Adjusted R^2	0.258	0.439	0.237	0.476	0.258	0.439	0.236	0.475
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Production Cluster Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Table 2.4: Dynamic Effects of Business Combination Laws

This table reports variants of Table 2.3, where *BC* is replaced with *BC(-1)*, an indicator equaling one if a firm is incorporated in a state that will pass a business combination (BC) law in the next year; *BC(0)*, an indicator equaling one if a firm is incorporated in a state that passes a BC law in a given year; *BC(1)*, an indicator equaling one if a firm is incorporated in a state that passed a BC law one year ago; and *BC(2+)*, an indicator equaling one if a firm is incorporated in a state that passed a BC law at least two years ago. The dependent variable is *ROA*. *Number of Firms in Cluster* is the number of firms within a production cluster, where production cluster refers to a group of firms in a 4-digit SIC industry that supply to a same customer. *Cluster-level HHI* is the sum of squared market shares of all firms in a production cluster. Columns 3, 4, 7, 8 report results for firms supplying to an exclusive customer as observed in the sample. Standard errors reported in parentheses are clustered at the industry level in odd columns and at the production-cluster level in even columns. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA							
	Full Sample		Single Customer Subsample		Full Sample		Single Customer Subsample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BC(-1)	-0.004	0.007	-0.014	-0.010	-0.004	0.019	0.008	0.023
	(0.010)	(0.009)	(0.014)	(0.017)	(0.013)	(0.012)	(0.023)	(0.029)
BC(0)	-0.020*	-0.001	-0.002	-0.003	-0.000	0.033**	0.006	0.044
	(0.012)	(0.011)	(0.016)	(0.021)	(0.013)	(0.014)	(0.026)	(0.037)
BC(1)	-0.015	-0.007	-0.005	-0.011	0.006	0.030**	0.032	0.055*
	(0.015)	(0.014)	(0.015)	(0.022)	(0.018)	(0.014)	(0.022)	(0.029)
BC(2+)	-0.011	-0.004	-0.027**	-0.034	0.017*	0.016	0.014	0.001
	(0.010)	(0.010)	(0.011)	(0.021)	(0.010)	(0.012)	(0.020)	(0.026)
Number of Firms in Cluster	-0.002**	-0.004***	-0.007***	-0.010***				
	(0.001)	(0.001)	(0.001)	(0.003)				
BC(-1) × Number of Firms in Cluster	0.000	0.001	0.003	0.003				
	(0.001)	(0.001)	(0.002)	(0.003)				
BC(0) × Number of Firms in Cluster	0.002**	0.002	0.002	0.003				
	(0.001)	(0.002)	(0.002)	(0.003)				
BC(1) × Number of Firms in Cluster	0.001	0.002	0.002	0.004				
	(0.001)	(0.001)	(0.003)	(0.003)				
BC(2+) × Number of Firms in Cluster	0.004***	0.002**	0.010***	0.008***				
	(0.001)	(0.001)	(0.001)	(0.002)				
Cluster-level HHI					0.012	0.010	0.027	0.017
					(0.008)	(0.010)	(0.019)	(0.022)
BC(-1) × Cluster-level HHI					0.002	-0.013	-0.019	-0.028
					(0.013)	(0.015)	(0.029)	(0.033)
BC(0) × Cluster-level HHI					-0.020	-0.039**	-0.004	-0.043
					(0.017)	(0.016)	(0.031)	(0.040)
BC(1) × Cluster-level HHI					-0.025	-0.044**	-0.037	-0.060*
					(0.022)	(0.018)	(0.024)	(0.034)
BC(2+) × Cluster-level HHI					-0.024**	-0.018	-0.020	-0.005
					(0.011)	(0.012)	(0.023)	(0.028)
Observations	47945	47945	14724	14724	47945	47945	14724	14724
Adjusted R^2	0.258	0.439	0.237	0.476	0.258	0.439	0.236	0.475
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Production Cluster Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Table 2.5: Potential Competitors

This table reports variants of Table 2.3, where proxies for competition are replaced with industry-level measures. The dependent variable is *ROA*. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Industry* is defined as the number of firms within an 4-digit SIC industry. *Industry-level HHI* is computed as the sum of squared market shares of all firms in a given 4-digit SIC industry. Columns 3, 4, 7, 8 report results for the subsample of firms supplying to an exclusive customer as observed in the sample. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the 4-digit SIC industry level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA			
	Full Sample		Single Customer Subsample	
	(1)	(2)	(3)	(4)
BC	-0.004	0.001	-0.001	0.001
	(0.009)	(0.010)	(0.012)	(0.019)
Number of Firms in Industry	-0.001***	-0.003***		
	(0.000)	(0.001)		
BC × Number of Firms in Industry	0.000	-0.000		
	(0.000)	(0.000)		
Industry-level HHI			0.026	0.012
			(0.019)	(0.025)
BC × Industry-level HHI			-0.006	-0.008
			(0.025)	(0.033)
Mean(ROA) by Industry-year	0.189***	0.059*	0.202***	0.073*
	(0.031)	(0.034)	(0.035)	(0.040)
Mean(ROA) by State-year	0.192***	0.106***	0.205***	0.117***
	(0.031)	(0.028)	(0.033)	(0.029)
Log(Total Assets)	0.124***	0.134***	0.125***	0.134***
	(0.012)	(0.014)	(0.012)	(0.014)
Log(Total Assets)-squared	-0.007***	-0.008***	-0.007***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.001)
Log(1+Firm Age)	0.011*	0.011**	0.011**	0.013**
	(0.005)	(0.005)	(0.005)	(0.005)
Leverage	-0.266***	-0.259***	-0.265***	-0.254***
	(0.024)	(0.030)	(0.024)	(0.030)
Capital Expenditures	-0.089*	-0.155*	-0.077	-0.128
	(0.053)	(0.081)	(0.052)	(0.079)
Log(Customer's Total Assets)		0.000		0.000
		(0.003)		(0.003)
Observations	29279	14724	29279	14724
Adjusted R^2	0.241	0.240	0.240	0.236
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes

Table 2.6: Peripheral Suppliers and Major Suppliers

This table reports subsample tests to column 4 of Table 2.3. The sample is restricted to firms that supply to an exclusive customer and have at least one peer supplier firm as observed in the data. The dependent variable is *ROA*. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer. Columns 1–4 report results for the subsample of peripheral suppliers. Columns 5–8 report results for the subsample of major suppliers. A firm is considered to be a peripheral supplier if it supplies no more than a certain share of input to the common customer within a production cluster. The threshold is 20%, 30%, 40%, and 50% in columns 1–4, respectively. A firm is considered to be a major supplier if it supplies at least a certain share of input to the common customer within a production cluster. The threshold is 50%, 60%, 70%, and 80% in columns 5–8, respectively. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the production-cluster level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA							
	Peripheral Supplier (Input Share Below Threshold)				Major Supplier (Input Share Above Threshold)			
	$\leq$ 20%	$\leq$ 30%	$\leq$ 40%	$\leq$ 50%	> 50%	$\geq$ 60%	$\geq$ 70%	$\geq$ 80%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BC	-0.054	-0.032	-0.037	-0.033	-0.076**	-0.083**	-0.088*	-0.094*
	(0.050)	(0.043)	(0.040)	(0.036)	(0.037)	(0.040)	(0.046)	(0.048)
Number of Firms in Cluster	-0.016**	-0.015**	-0.014**	-0.013**	-0.011	-0.012	-0.014	-0.016
	(0.008)	(0.006)	(0.006)	(0.005)	(0.007)	(0.008)	(0.010)	(0.010)
BC $\times$ Number of Firms in Cluster	0.011**	0.009**	0.009**	0.009**	0.010*	0.012*	0.015*	0.017*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.007)	(0.009)	(0.010)
Mean(ROA) by Industry-year	0.286	0.261	0.230	0.241*	0.098***	0.087***	0.076***	0.077**
	(0.225)	(0.167)	(0.148)	(0.141)	(0.032)	(0.031)	(0.029)	(0.031)
Mean(ROA) by State-year	0.153	0.137	0.131	0.117	0.037	0.016	0.021	-0.003
	(0.187)	(0.162)	(0.150)	(0.134)	(0.062)	(0.063)	(0.065)	(0.070)
Log(Total Assets)	0.155***	0.142***	0.149***	0.144***	0.160***	0.156***	0.161***	0.178***
	(0.034)	(0.027)	(0.029)	(0.026)	(0.039)	(0.040)	(0.041)	(0.045)
Log(Total Assets)-squared	-0.010***	-0.009***	-0.009***	-0.009***	-0.009***	-0.008***	-0.009***	-0.010***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Log(1+Firm Age)	0.009	0.003	-0.001	-0.002	-0.001	0.003	0.004	-0.002
	(0.018)	(0.015)	(0.013)	(0.012)	(0.011)	(0.012)	(0.013)	(0.014)
Leverage	-0.350***	-0.341***	-0.367***	-0.371***	-0.387***	-0.390***	-0.392***	-0.400***
	(0.073)	(0.058)	(0.061)	(0.055)	(0.068)	(0.072)	(0.075)	(0.081)
Capital Expenditures	-0.236	-0.191	-0.191	-0.177	-0.200	-0.190	-0.184	-0.240
	(0.213)	(0.186)	(0.186)	(0.169)	(0.125)	(0.125)	(0.136)	(0.148)
Log(Customer's Total Assets)	0.107*	0.053	0.066	0.060*	0.004	0.006	0.015	0.023
	(0.063)	(0.049)	(0.042)	(0.036)	(0.028)	(0.030)	(0.032)	(0.036)
Observations	1332	1661	1915	2171	2978	2782	2571	2368
Adjusted R^2	0.426	0.473	0.457	0.462	0.419	0.423	0.425	0.422
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Production Cluster Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.7: Input Specificity

This table reports subsample tests to column 2 of Table 2.3. The dependent variable is *ROA*. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer. Columns 1 and 3 report results for the subsample of firms that supply specific input. Columns 2 and 4 report results for the subsample of firms that supply non-specific input. In columns 1 and 2, the level of input specificity is considered high if a firm belongs to a 2-digit SIC codes sector producing non-standardized goods defined by Giannetti *et al.* [80]. In columns 3 and 4, the level of input specificity is considered high if a firm's ratio of R&D expenditures over sales in the previous year is above median within a 4-digit SIC industry. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the production-cluster level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA			
	Non-standardized	Standardized	High R&D	Low R&D
	(1)	(2)	(3)	(4)
BC	-0.011	-0.015	-0.006	-0.003
	(0.011)	(0.021)	(0.033)	(0.015)
Number of Firms in Cluster	-0.004**	-0.001	-0.007**	-0.002
	(0.002)	(0.001)	(0.004)	(0.002)
BC × Number of Firms in Cluster	0.002***	-0.000	0.008***	0.002
	(0.001)	(0.001)	(0.002)	(0.001)
Mean(ROA) by Industry-year	0.281***	0.192***	0.257***	0.168***
	(0.029)	(0.047)	(0.057)	(0.032)
Mean(ROA) by State-year	0.217***	0.073*	0.113	0.172***
	(0.037)	(0.042)	(0.095)	(0.051)
Log(Total Assets)	0.120***	0.180***	0.212***	0.088***
	(0.013)	(0.024)	(0.036)	(0.015)
Log(Total Assets)-squared	-0.007***	-0.009***	-0.011***	-0.005***
	(0.001)	(0.002)	(0.003)	(0.001)
Log(1+Firm Age)	-0.007*	0.004	0.002	-0.002
	(0.004)	(0.009)	(0.013)	(0.007)
Leverage	-0.376***	-0.356***	-0.544***	-0.304***
	(0.018)	(0.026)	(0.042)	(0.024)
Capital Expenditures	-0.138**	0.136	-0.516***	-0.169
	(0.056)	(0.083)	(0.142)	(0.130)
Log(Customer's Total Assets)	-0.005	-0.010	-0.001	-0.002
	(0.006)	(0.013)	(0.017)	(0.007)
Observations	31579	8322	6324	12048
Adjusted R^2	0.437	0.470	0.566	0.525
Year Fixed Effects	Yes	Yes	Yes	Yes
Production Cluster Fixed Effects	Yes	Yes	Yes	Yes

Table 2.8: Revenues and Costs

This table reports variants of column 4 of Table 2.3, where the dependent variables are replaced with operating revenues and costs. The sample is restricted to firms supplying to an exclusive customer as observed in the data. In column 1, the dependent variable is computed as annual growth rate of sales. In column 2, the dependent variable is computed as costs of goods sold divided by sales. In column 3, the dependent variable is computed as selling, general, and administrative expenses divided by sales. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the production-cluster level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	(1) Sales Growth	(2) Costs of Goods Sold	(3) SGA Expenses
BC	-0.046	0.109*	0.103**
	(0.046)	(0.064)	(0.049)
Number of Firms in Cluster	-0.001	0.017	0.016*
	(0.009)	(0.011)	(0.008)
BC × Number of Firms in Cluster	0.010	-0.012*	-0.014**
	(0.009)	(0.007)	(0.006)
Mean(ROA) by Industry-year	0.110**	-0.033	-0.074*
	(0.046)	(0.080)	(0.041)
Mean(ROA) by State-year	-0.015	-0.165	-0.122
	(0.077)	(0.167)	(0.097)
Log(Total Assets)	0.081***	-0.131***	-0.230***
	(0.029)	(0.050)	(0.040)
Log(Total Assets)-squared	-0.005**	0.005	0.014***
	(0.002)	(0.004)	(0.003)
Log(1+Firm Age)	-0.218***	-0.057*	-0.072***
	(0.025)	(0.032)	(0.024)
Leverage	-0.081	0.206	-0.205***
	(0.065)	(0.130)	(0.071)
Capital Expenditures	0.830***	-0.184	-0.003
	(0.201)	(0.255)	(0.304)
Log(Customer's Total Assets)	0.018	0.011	-0.012
	(0.033)	(0.067)	(0.028)
Observations	14425	14719	12740
Adjusted R^2	0.312	0.489	0.588
Year Fixed Effects	Yes	Yes	Yes
Production Cluster Fixed Effects	Yes	Yes	Yes

Table 2.9: Mean Comparison Tests for Customer Business Combination Laws

This table presents mean comparison results for firms with available information on board of directors that supply to an exclusive customer as observed in the sample. Column 1 reports mean values for the subsample of firms with customer companies incorporated in a state that passed a business combination (BC) law during the sample period. Column 2 reports mean values for the subsample of firms with customer companies incorporated in a state that never passed a BC law during the sample period. Column 3 reports p-value statistics from a t-test of the difference between "Customer Eventually BC" and "Customer Never BC" groups. Panel B reports results for the subsample of firms that have no peer supplier firm within a production cluster as observed in the sample. Panel C presents results for the subsample of firms that have at least one peer supplier in a production cluster. Definitions of all variables are presented in Appendix B.1.

Panel A: Full Sample

	(1) Mean for Firms with Customer Eventually BC	(2) Mean for Firms with Customer Never BC	(3) p-value (difference)
ROA	0.040	0.044	0.779
Log(1+Board Size)	2.255	2.173	0.012
Board Independence	0.689	0.747	0.001
Observations	1504		

Panel B: Single Supplier Subsample

	(1) Mean for Firms with Customer Eventually BC	(2) Mean for Firms with Customer Never BC	(3) p-value (difference)
ROA	0.042	0.038	0.767
Log(1+Board Size)	2.255	2.222	0.311
Board Independence	0.684	0.744	0.006
Observations	860		

Panel C: Multiple Suppliers Subsample

	(1) Mean for Firms with Customer Eventually BC	(2) Mean for Firms with Customer Never BC	(3) p-value (difference)
ROA	0.038	0.068	0.237
Log(1+Board Size)	2.255	1.961	0.003
Board Independence	0.695	0.762	0.015
Observations	644		

Table 2.10: Customer Business Combination Laws and Competition

This table reports estimates from regressing operating performance on business combination law dummies for a firm's customer company, competition, their interaction terms, a set of control variables, and fixed effects. The dependent variable is *ROA*. *Customer BC* is an indicator equaling one if a firm's customer company is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a BC law. All specifications include the same set of control variables as in Table 2.3. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the production-cluster level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA		
	(1)	(2)	(3)
Customer BC	0.022	0.006	0.043*
	(0.016)	(0.016)	(0.022)
Number of Firms in Cluster		-0.010***	-0.012***
		(0.003)	(0.004)
Customer BC × Number of Firms in Cluster		0.007***	0.002
		(0.002)	(0.004)
BC			0.046
			(0.029)
Customer BC × BC			-0.073**
			(0.033)
Customer BC × BC × Number of Firms in Cluster			0.004
			(0.011)
BC × Number of Firms in Cluster			0.002
			(0.010)
Observations	14647	14647	13280
Adjusted R^2	0.464	0.465	0.467
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Production Cluster Fixed Effects	Yes	Yes	Yes

Table 2.11: Customer Business Combination Laws and Supplier Input Shares

This table reports subsample tests to column 2 of Table 2.10. The sample is restricted to firms supplying to an exclusive customer as observed in the data. The dependent variable is *ROA*. *Customer BC* is an indicator equaling one if a firm's customer company is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer. Columns 1–4 report results for the subsample of peripheral suppliers. Columns 5–8 report results for the subsample of major suppliers. A firm is considered to be a peripheral supplier if it supplies no more than a certain share of input to the common customer within a production cluster. The threshold is 20%, 30%, 40%, and 50% in columns 1–4, respectively. A firm is considered to be a major supplier if it supplies at least a certain share of input to the common customer within a production cluster. The threshold is 50%, 60%, 70%, and 80% in columns 5–8, respectively. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the production-cluster level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA							
	Peripheral Supplier (Input Share Below Threshold)				Major Supplier (Input Share Above Threshold)			
	$\leq 20\%$	$\leq 30\%$	$\leq 40\%$	$\leq 50\%$	$> 50\%$	$\geq 60\%$	$\geq 70\%$	$\geq 80\%$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Customer BC	-0.104	-0.020	-0.031	-0.051	0.039	0.014	-0.001	-0.004
	(0.080)	(0.065)	(0.059)	(0.059)	(0.042)	(0.038)	(0.042)	(0.046)
Number of Firms in Cluster	-0.018*	-0.011	-0.013**	-0.012**	-0.018**	-0.019**	-0.024**	-0.027**
	(0.009)	(0.007)	(0.006)	(0.006)	(0.007)	(0.008)	(0.010)	(0.012)
Customer BC × Number of Firms in Cluster	0.011*	0.004	0.007	0.007	0.017***	0.020***	0.024**	0.029**
	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.010)	(0.011)
Mean(ROA) by Industry-year	0.291	0.275*	0.260*	0.271*	0.078***	0.068**	0.059*	0.054
	(0.213)	(0.160)	(0.141)	(0.138)	(0.030)	(0.030)	(0.030)	(0.033)
Log(Total Assets)	0.152***	0.136***	0.142***	0.134***	0.151***	0.145***	0.151***	0.164***
	(0.032)	(0.025)	(0.027)	(0.024)	(0.038)	(0.039)	(0.040)	(0.043)
Log(Total Assets)-squared	-0.009***	-0.008***	-0.009***	-0.008***	-0.008***	-0.007**	-0.008***	-0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Log(1+Firm Age)	0.010	0.004	-0.000	-0.004	-0.007	-0.002	-0.002	-0.006
	(0.016)	(0.014)	(0.013)	(0.012)	(0.010)	(0.010)	(0.012)	(0.013)
Leverage	-0.329***	-0.314***	-0.340***	-0.348***	-0.385***	-0.383***	-0.384***	-0.386***
	(0.066)	(0.053)	(0.056)	(0.051)	(0.064)	(0.068)	(0.071)	(0.077)
Capital Expenditures	-0.307	-0.266	-0.264	-0.252	-0.135	-0.114	-0.114	-0.176
	(0.200)	(0.178)	(0.177)	(0.164)	(0.129)	(0.128)	(0.139)	(0.151)
Log(Customer's Total Assets)	0.070	0.019	0.034	0.029	0.004	0.008	0.016	0.024
	(0.051)	(0.043)	(0.039)	(0.034)	(0.026)	(0.028)	(0.030)	(0.033)
Observations	1359	1684	1951	2211	2980	2789	2580	2362
Adjusted R^2	0.413	0.464	0.441	0.444	0.378	0.386	0.388	0.387
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Production Cluster Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.12: Peer Supplier Business Combination Laws

This table reports subsample tests to column 2 of Table 2.3. The dependent variable is *ROA*. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is defined as the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer. Column 1 reports results for the subsample of firms with at least one peer supplier firm within a production cluster is incorporated in a state that has passed a BC law. Column 2 reports results for the subsample of firms with no peer supplier within a production cluster is incorporated in a state that has passed a BC law. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the production-cluster level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA	
	Peer Supplier BC	No Peer Supplier BC
	(1)	(2)
BC	-0.060	-0.005
	(0.037)	(0.041)
Number of Firms in Cluster	-0.012***	-0.004
	(0.005)	(0.006)
BC × Number of Firms in Cluster	0.010**	0.002
	(0.004)	(0.007)
Mean(ROA) by Industry-year	0.071	0.095***
	(0.099)	(0.037)
Mean(ROA) by State-year	0.076	0.101
	(0.086)	(0.095)
Log(Total Assets)	0.158***	0.134***
	(0.025)	(0.029)
Log(Total Assets)-squared	-0.009***	-0.008***
	(0.002)	(0.002)
Log(1+Firm Age)	0.005	-0.012
	(0.013)	(0.011)
Leverage	-0.373***	-0.395***
	(0.049)	(0.075)
Capital Expenditures	-0.217	-0.163
	(0.144)	(0.130)
Log(Customer's Total Assets)	0.037	0.061*
	(0.031)	(0.034)
Observations	2669	2757
Adjusted R^2	0.414	0.410
Year Fixed Effects	Yes	Yes
Production Cluster Fixed Effects	Yes	Yes

Table 2.13: State-year Fixed Effects and Other State Laws

This table presents variants of columns 2 and 4 of Table 2.3, where additional anti-takeover laws and court decisions are included as controls following Karpoff and Wittry [102] in columns 1 and 2, and all control variables are replaced with state-year fixed effects in columns 3 and 4. In columns 3 and 4, the sample is expanded to include firms with missing values of controls in Table 2.3. The dependent variable is *ROA*. *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. *Number of Firms in Cluster* is the number of firms within a production cluster, where production cluster refers to a group of firms in a given industry that supply to a same customer. Columns 1, 2, and 4 report results for firms supplying to an exclusive customer as observed in the sample. Definition of all variables are in Appendix B.1. Standard errors reported in parentheses are clustered at the production-cluster level. *, **, *** indicate significance at 10%, 5%, 1%, respectively.

	Dependent Variable: ROA			
	Single Customer Subsample			
	Other State Laws	Court Decisions	Full Sample	Single Customer Subsample
	(1)	(2)	(3)	(4)
BC	-0.025	-0.018	0.008	-0.016
	(0.017)	(0.015)	(0.008)	(0.022)
Number of Firms in Cluster	-0.007**	-0.007**	-0.007***	-0.013***
	(0.003)	(0.003)	(0.001)	(0.005)
BC × Number of Firms in Cluster	0.006*	0.007*	0.004***	0.008**
	(0.003)	(0.003)	(0.001)	(0.004)
First-generation Law	0.039***			
	(0.014)			
Poison Pill	-0.010			
	(0.013)			
Control Share Acquisition	0.013			
	(0.020)			
Directors' Duties	0.006			
	(0.020)			
Fair Price	-0.010			
	(0.020)			
First-generation Law × Number of Firms in Cluster	-0.002			
	(0.002)			
Poison Pill × Number of Firms in Cluster	-0.001			
	(0.003)			
Control Share Acquisition × Number of Firms in Cluster	-0.001			
	(0.003)			
Directors' Duties × Number of Firms in Cluster	-0.003			
	(0.004)			
Fair Price × Number of Firms in Cluster	0.003			
	(0.004)			
Amanda Decision × BC		-0.031**		
		(0.015)		
Amanda Decision × Number of Firms in Cluster		-0.002		
		(0.004)		
Amanda Decision × BC × Number of Firms in Cluster		0.002		
		(0.005)		
Observations	14724	14724	51407	16971
Adjusted R^2	0.465	0.464	0.314	0.356
Year Fixed Effects	Yes	Yes		
State-year Fixed Effects			Yes	Yes
Production Cluster Fixed Effects	Yes	Yes	Yes	Yes

Appendix B: Appendix

B.1 Variable Definitions

- *Number of Firms in Cluster* is the number of firms within a production cluster, where production cluster refers to a group of supplier firms in a given 4-digit SIC industry that supply to a same customer.
- *Cluster-level HHI* is the sum of squared market shares of all firms in a given production cluster.
- *Number of Firms in Industry* is the number of firms within an 4-digit SIC industry.
- *Industry-level HHI* is the sum of squared market shares of all firms in a given 4-digit SIC industry.
- *BC* is an indicator equaling one if a firm is incorporated in a state that has passed a business combination (BC) law. The timing of passages is reported in Table B.1.
- *BC(-1)* is an indicator equaling one if a firm is incorporated in a state that has will pass a BC law.
- *BC(0)* is an indicator equaling one if a firm is incorporated in a state that passes a BC law.
- *BC(1)* is an indicator equaling one if a firm is incorporated in a state that passed a BC law one year ago.
- *BC(2+)* is an indicator equaling one if a firm is incorporated in a state that passed a BC law at least two years ago.
- *Customer BC* is an Indicator equaling one if a firm's customer company is incorporated in a state that has passed a BC law.

- *First-generation Law* is an indicator equaling one if a firm is incorporated in a state in which a first-generation anti-takeover law was effective. These laws were adopted by 38 states from 1968 through 1981 and regulated cash tender offers and imposed strong takeover protections (Karpoff and Wittry [102]). The timing of passages of these laws are listed in Table B.2 (same below).

- *Poison Pill* is an indicator equaling one if a firm is incorporated in a state that has adopted a poison pill law.

- *Control Share Acquisition* is an indicator equaling one if a firm is incorporated in a state that has adopted and has not repealed a control share acquisition law.

- *Directors' Duties* is an indicator equaling one if a firm is incorporated in a state that has adopted and has not repealed a directors' duties law.

- *Fair Price* is an indicator equaling one if a firm is incorporated in a state that has adopted a fair price law.

- *Amanda Decision* is an indicator equaling one for years after 1989, when the constitutionality of BC laws was first established by a court decision involving *Amanda Acquisition Corp.*

- *ROA* is net income divided by total assets (source: Compustat; same below).

- *Log(Total Assets)* is the logged value of total assets.

- *Log(Total Assets)-squared* is the squared value of logged value of total assets.

- *Log(1+Firm Age)* is the logged value of one plus the number of years listed in Compustat.

- *Leverage* is total liabilities divided by total assets.

- *Capital Expenditures* is capital expenditures divided by total assets.

- *Log(Customer's Total Assets)* is the logged value of total assets of a firm's customer company.

- *Sales Growth* is the annual growth rate of total sales.

- *Costs of Goods Sold* is costs of goods sold divided by sales

- *SGA Expenses* is selling, general, and administrative expenses divided by sales.
- *Log(1+Board Size)* is the logged value of one plus the number of directors on board (source: ISS; same below).
- *Board Independence* is the fraction of independent directors on board.

B.2 Timing of Business Combination Laws

Table B.1 reports the timing of passages of business combination (BC) laws. Column 2 presents the year in which a BC law was passed in a given state (Bertrand and Mullainathan [78]). Column 3 reports the number of sample firms incorporated in a given state. Column 4 reports the number of sample firms with headquarters located in a given state.

B.3 Timing of Other Anit-takeover State Laws

Table B.2 reports years in which first-generation anti-takeover laws, poison pill laws, control share acquisition laws, directors' duties laws, and fair price laws were adopted and were repealed effective (Karpoff and Wittry [102]).

Table B.1: Business Combination Laws

(1) State	(2) BC Year	(3) Number of Firms by State of Incorporation	(4) Number of Firms by State of Location
Alabama	-	21	166
Alaska	-	18	22
Arkansas	-	5	117
Arizona	1987	73	386
California	-	1207	6169
Colorado	-	424	1030
Connecticut	1989	147	810
District of Columbia	-	22	42
Delaware	1988	19405	127
Florida	-	717	1297
Georgia	1988	382	847
Hawaii	-	8	42
Idaho	1988	20	113
Illinois	1989	138	1106
Indiana	1986	403	456
Iowa	-	59	110
Kansas	1989	89	162
Kentucky	1987	51	173
Louisiana	-	85	192
Maine	1988	19	20
Maryland	1989	568	476
Massachusetts	1989	776	1827
Michigan	1989	513	1115
Minnesota	1987	958	1002
Mississippi	-	2	50
Missouri	1986	164	486
Montana	-	26	43
North Carolina	-	267	735
North Dakota	-	6	29
Nebraska	1988	16	74
New Hampshire	-	18	161
New Jersey	1986	463	1492
New Mexico	-	52	27
Nevada	1991	683	194
New York	1985	1040	2745
Ohio	1990	749	1115
Oklahoma	1991	150	435
Oregon	-	225	326
Pennsylvania	1989	642	1088
Rhode Island	1990	80	131
South Carolina	1988	62	103
South Dakota	1990	17	26
Tennessee	1988	89	264
Texas	-	700	3451
Utah	-	196	197
Vermont	-	44	63
Virginia	1988	359	616
Washington	1987	264	333
Wisconsin	1987	320	429
West Virginia	-	7	27
Wyoming	1989	53	35
Total		32802	32482

Table B.2: Other Anit-takeover State Laws

(1) State	(2) First-generation	(3) Repeal of First-generation	(4) Poison Pill	(5) Control Share Acquisition	(6) Directors' Duties	(7) Fair Price
Alabama	-	-	-	-	-	-
Alaska	1976	-	-	-	-	-
Arkansas	1977	2000	-	-	-	-
Arizona	-	-	-	1987	1987	1987
California	-	-	-	-	-	-
Colorado	1975	1984	1989	-	-	-
Connecticut	1976	-	2003	-	1988	1984
District of Columbia	-	-	-	-	-	-
Delaware	1976	1987	-	-	-	-
Florida	1977	1979	1989	1987	1989	1987
Georgia	1977	1986	1988	-	1989	1985
Hawaii	1974	1985	1988	1985	1989	-
Idaho	1975	1986	1988	1988	1988	1988
Illinois	1977	1984	1989	-	1985	1985
Indiana	1975	-	1986	1986	1986	1986
Iowa	1979	2005	1989	-	1989	-
Kansas	1974	1988	-	1988	-	-
Kentucky	1976	1986	1988	-	1988	1984
Louisiana	1976	1987	-	1987	1988	1984
Maine	1978	1986	2002	-	1985	-
Maryland	1976	1986	1999	1989	1999	1983
Massachusetts	1976	-	1989	1987	1989	-
Michigan	1976	1988	2001	1988[a]	-	1984
Minnesota	1973	-	-	1984	1987	1991
Mississippi	1977	-	-	1990	1990	1985
Missouri	1978	-	-	1984	1986	1986
Montana	-	-	-	-	-	-
Nebraska	1977	1988	-	1988	1988[b]	-
Nevada	1969	1991	1989	1987	1991	1991
New Hampshire	1977	-	-	-	-	-
New Jersey	1977	-	1989	-	1989	1986
New Mexico	-	-	-	-	1987	-
New York	1976	-	1988	-	1987	1985
North Carolina	1977	2001	1989	1987	1993	1987
North Dakota	-	-	-	-	1993	-
Ohio	1969	-	1986	1982	1984	1990
Oklahoma	1981	1985	-	1987	-	-
Oregon	-	-	1989	1987	1989	-
Pennsylvania	1976	-	1988	1990	1990	1988
Rhode Island	-	-	1990	-	1990	1990
South Carolina	1978	1989	1998	1988	-	1988
South Dakota	1975	1990	1990	1990	1990	1990
Tennessee	1976	-	1989	1988	1988	1988
Texas	1977	-	-	-	2003	-
Utah	1976	1983	1989	1987	-	-
Vermont	-	-	-	-	1998	-
Virginia	1968	1989	1990	1989	1988	1985
Washington	-	-	1998	-	-	1985
Wisconsin	1972	-	1987	1984[c]	1987	1984
West Virginia	-	-	-	-	-	-
Wyoming	-	-	-	1990		

[a]The Michigan control share statute was repealed effective in 2009.

[b]The Nebraska directors' duties statute was repealed effective in 1995, but was later reenacted effective in 2007.

[c]The Wisconsin control share statute was repealed effective in 1986.

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