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PH.D. THESIS

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To my parents

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ABSTRACT

This thesis consists of three chapters. The first chapter is sole-authored and is titled 'Cross-border mergers and acquisitions amid political uncertainty.' The second chapter is coauthored work with Professor Jon Garfinkel and Jaewoo Kim and is titled 'The interactive influence of external and internal governance on risk taking and outcomes: The importance of CEO career concerns.' The third chapter is coauthored work with Professor Erik Lie and Jaewoo Kim and is titled 'Dividend stickiness, debt covenants, and earnings management.'

First chapter examines the effects of political uncertainty surrounding national elections on cross-border mergers and acquisitions. I find that the volume of cross-border mergers and acquisitions between two countries declines before elections in the target country. Firms in industries that are more dependent on the quality of contract enforcement, labor, and government spending are less likely to be acquired during election years. In a cross-border merger deal announced during the target country's election year, acquirers tend to offer a lower bid premium, and the likelihood of an all-cash offer is significantly lower. The acquirer captures a greater fraction of merger gains relative to the target in such a deal. Overall, my findings suggest that political uncertainty importantly affects multiple aspects of cross-border mergers and acquisitions.

Second chapter studies the effects of multi-layered governance on firm risk by focusing on the interaction of two types of career concerns. Two Delaware court decisions, the validation of poison pill defenses (the Unitrin decision) implemented by staggered boards (the Wallace decision), reduced takeover-related career concerns. CEO age influences the response of Delaware firms to these shocks. Older CEOs in newly insulated firms reduce risk, while their younger counterparts *increase* risk. Ex-post, the differential behavior among young Delaware CEOs appears to be rewarded with abnormally positive stock performance and better future career outcomes. We conclude

that there is important variation in the effects of governance on firm (CEO) behavior, driven by multiple facets of career concerns.

Third chapter examines dividend stickiness. Consistent with the notion that dividends are very sticky, Daniel, Denis, and Naveen (2008) report evidence that firms manage earnings upward when pre-managed earnings are expected to fall short of dividend payments. However, we find that this evidence is not robust when controlling for firms' tendency to manage earnings upward to avoid reporting earnings declines. We further report that the decision to cut dividends depends on whether reported earnings fall short of past dividends, but not on earnings management that eliminates a shortfall in pre-managed earnings relative to dividend payments. Overall, our evidence suggests that firms that face dividend constraints are more likely to cut dividends than to manage earnings to avoid dividend cuts.

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CHAPTER 1

CROSS-BORDER MERGERS AND ACQUISITIONS AMID POLITICAL UNCERTAINTY

1.1 Introduction

Cross-border mergers and acquisitions (M&A) are among the largest corporate investments and resemble domestic M&A in many respects, except that they allow firms to extend boundaries across national borders. The volume of cross-border M&A transactions has risen substantially over the last two decades, and many firms nowadays choose cross-border M&A as a main entry mode when they expand into foreign markets (Economist, 2007; OECD, 2007). Spurred by recent growth in cross-border M&A activity around the world, many financial economists have attempted to identify factors contributing to a firm's decision to cross a border. The literature finds that corporate tax policy, economic nationalism, and differences in culture, financial development, investor protection, and valuation play a role in explaining patterns of cross-border M&A activity (Rossi and Volpin, 2004; di Giovanni, 2005; Huizinga and Voget, 2009; Erel, Liao, and Weisbach, 2012; Ahern, Daminelli, and Fracassi, 2012; Dinc and Erel, 2013).

In this paper, I look at political uncertainty as another key factor for cross-border M&A. Political uncertainty has been a key force shaping the global economy in recent years. The U.S. budget stand-off in 2013 and the European sovereign debt crisis in 2009, for instance, were surrounded by considerable uncertainty about policies. Motivated by academic researchers and policy makers' wide concerns and interests about heightened political uncertainty in recent times, many studies have assessed its effects on various economic activities. Julio and Yook (2012) and Durnev (2012), for example, show that uncertainty about government policy affects corporate investment decisions. Colak, Durnev, and Qian (2013) and Gao and Qi (2013) demonstrate that political uncertainty surrounding U.S. gubernatorial elections makes raising external capital (e.g., public debt financing and initial public offerings (IPO)) more expensive. Julio and Yook (2013) document that political uncertainty also influences U.S. foreign direct investment (FDI) flows. While these studies show greater awareness of the impact of political uncertainty on a wide range of economic decisions, their impact on cross-border corporate mergers and acquisitions, to the best of my knowledge, has not yet been systematically discussed. This paper fills this void in the literature by examining 1) the effects of political uncertainty on cross-border M&A activity between each pair of 43 countries, 2) whether and how foreign acquirers incorporate political uncertainty into their decision-making process during merger negotiations such as pricing and deal structure, and 3) how the capital markets respond to cross-border acquisitions subject to political uncertainty.

I posit that policy uncertainty has a deterrent effect on cross-border M&A activity. The value of mergers, like any other corporate investment, is subject to government policies such as fiscal, tax, and regulatory policies. The extant literature suggests that when there is uncertainty over future government policies firms likely delay mergers, which entail large sunk costs and are largely irreversible, until much of the uncertainty dissipates. Rodrik (1991), Pindyck and Solimano (1993), and Stokey (2013), for instance, show that in the face of uncertainty about the stability of future government policies, firms withhold investments, if they are partially irreversible. Analogous predictions about the effect of political uncertainty on investment can be drawn from theoretical models by Bernanke (1983), McDonald and Siegel (1986), and Bloom, Bond and Van Reenen (2007) in which uncertainty, regardless of its source, discourages irreversible investments as the option value of waiting increases.

The detrimental effect of a given country's political uncertainty is not restricted to domestic mergers alone; if anything, the effect might be stronger for cross-border mergers. Azzimonti and Sarte (2007) and Dixit (2011) suggest that foreign investors may be more susceptible to the host country's politics than domestic investors, as they face additional dimensions of uncertainty regarding the policies on cross-border economic activities. Examples include unexpected foreign exchange interventions, a sudden increase in the tax rate on foreign firms' incomes, and nationalization of foreign-owned assets without fair compensation, all of which may influence the profitability prospects of cross-border M&A to a large extent. Thus, I conjecture that political uncertainty has a significantly negative impact on cross-border M&A activity.

In order to empirically test the effects of political uncertainty on cross-border M&As, I follow recent studies (Boutchkova, Doshi, Durnev, and Molchanov, 2012; Julio and Yook, 2012; Durnev, 2012) and focus on political uncertainty surrounding national elections around the world. There are several advantages to examining cross-border M&A activity during national election periods. First, national elections are mostly exogenous events. As a natural experiment, national elections allow us to correct for possible endogeneity between political risk and economic conditions, which affect corporate investment decisions, and I can therefore make causal inference regarding political uncertainty's influence on cross-border M&As (Rodrik, 1991; Julio and Yook, 2012). Second, national elections can create dramatic political uncertainty. Across a wide range of areas such as fiscal, tax, and exchange rate policy, national political leaders can exert significant influence over the country's economic environment. If a newly elected political leader's policy orientation substantially differs from that of the predecessor, abrupt policy shifts may occur. Indeed, Bialkowski, Gottschalk, and Wisniewski (2008) and Boutchkova et al. (2012) document significantly higher stock return volatility during national election years. Third, elections are held at different times for different countries, and countries typically hold an election on a periodic basis, thereby providing abundant variation of policy uncertainty across countries and over time. Thus, national elections allow for a powerful and comprehensive analysis of political uncertainty and cross-border mergers. The empirical methodology is described in Section 3.

Using a sample of cross-border M&A activity among 43 countries announced during 1990-2011, I find that the volume of cross-border M&A activity between a pair of countries significantly declines in the year leading up to the target country's election. This result is robust to controlling for country-pair fixed effects and other factors known to influence cross-border acquisitions, such as differences in stock market valuation and differences in economic development, and stronger among close elections, in which the margin of victory is smaller. The findings are consistent with the hypothesis that political uncertainty dampens cross-border M&A activity.

I also address the potential problem of endogenous election timing. Prior work in the political economy literature documents that the timing of elections can be endogenously determined by the country's economic performance. For example, political leaders may opportunistically call an early election in an expanding economy in attempt to increase the likelihood of re-election (Ito, 1990; Kayser, 2005). This confuses inferences regarding the causal impact of national elections on cross-border M&A activity. I respond to the endogeneity concerns in two ways: 1) re-estimating the main results using only pre-scheduled elections (i.e., exogenously-timed elections), and 2) using the number of years before scheduled elections as an instrument for elections. I continue to find significantly negative effects of electoral uncertainty on cross-border M&A activity.

The results thus far suggest that in the face of political uncertainty foreign investors prefer to wait and see rather than to undertake cross-border M&A. However, there are still cross-border deals undertaken during election periods despite political uncertainty. This raises question: what is different about cross-border merger deals that occur during high political uncertainty periods? Do foreign investors pursue target firms that are less politically risky? How do foreign acquirers protect themselves against political risk in the target country?

To see what types of firms are acquired during politically uncertain times, I look at a target firm's sensitivity to political environment. I consider three industry characteristics: dependence on the quality of contract enforcement (Blanchard and

Kremer, 1997), labor intensity (Jorgenson, 1990), and exposure to government spending (Belo, Gala, and Li, 2013). First, due to the complexity of production structure, firms in certain industries tend to build business transactions primarily through contracts rather than other methods (e.g. long-term business relationship and vertical acquisitions). Given the heavy reliance on contracts, these firms' operating efficiency is more subject to the quality of contract enforcement, which in turn is shaped by the country's political environment. Therefore, I expect the deterrent effects of policy uncertainty on cross-border M&A activity to be stronger among industries with more complex input structures, which require better contract enforcement than those with less complex input structures.

Second, national elections can bring about significant changes in labor laws depending on the succeeding government's policy preferences (Botero, Djankov, La Porta, Lopez-de-Silanes, and Shleifer, 2004). Changes can occur to various policies regarding minimum wage, maximum work hours, and compensation for lay-offs, all of which affect a firm's future cash flows. If a firm's labor costs account for a larger fraction of total production costs, the impact of potential labor policy changes is expected to be greater. Accordingly, I conjecture that the negative effects of national elections are stronger for cross-border deals involving the target from more labor-intensive industries.

Lastly, I consider an industry's exposure to government spending. Given that political parties have different attitudes toward government spending, industries in which government sector purchases account for a substantial fraction of total sales (i.e., the government represents the industry's primary customer) are expected to be more subject to electoral uncertainty (Alesina, 1987). Therefore, I conjecture that firms in industries that are more dependent on government spending are less likely to be acquired during election years.

I find empirical evidence that supports my hypotheses. Target firms that belong to industries with more complex input structures, higher labor intensity, and higher exposure to government spending are less likely to be acquired during election periods. Overall, my findings suggest that foreign acquirers tend to pursue different target firms in the face of political uncertainty.

To further investigate how foreign acquirers mitigate political risk entailed in cross-border transactions during election periods, I look at the merger negotiation terms. The simplest strategy to reduce exposure to political risk would be to pay less for the targets. By acquiring a firm from a country having a national election, the foreign bidder becomes exposed to the target country's political risk. Considering the evidence so far on foreign investors' reluctance to undertake such cross-border mergers, I predict that foreign bidders likely offer lower takeover premiums as compensation for their exposure to the target country's political risk. Using target cumulative abnormal returns around announcement dates, *CAR(-3, +3)*, as a proxy for takeover premiums, I find that takeover premiums are significantly lower when there is a forthcoming national election in the target country. Takeover premiums tend to be even lower if target firms are from industries that rely more on contract enforcement and government spending. The results also lend support to the theoretical argument in recent asset pricing studies that policy uncertainty has valuation effects (Croce, Kung, Nguyen, and Schmid, 2012; Pastor and Veronesi, 2013; Kelly, Pastor, and Veronesi, 2013).

As an alternative strategy to mitigate political risk, foreign acquirers may use stock as a means of payment, because the value of stock depends on post-merger performance and therefore allows them to share political risk with the targets. Prior research supports the idea. Hansen (1987) shows that in the face of greater uncertainty regarding the value of the target, the acquirer prefers to pay in stock, which is a contingent payment rather than cash. Thus, I conjecture that the acquirer is less likely to make a cash-offer when there is a forthcoming election in the target country. Consistent with my hypothesis, I find that the likelihood of an all-cash offer is significantly lower during the target country's election year.

My deal-level analysis suggests that foreign investors tend to pay lower takeover premiums and use more stock to finance a cross-border deal if political uncertainty becomes higher in the target country. Finally, I investigate whether foreign acquirers extract more value from cross-border deals in politically uncertain times as a result of such favorable terms. I examine how merger gains are divided between the acquirer and the target, and I find that the acquirer captures a larger portion of merger gains than the target does during election periods. More interestingly, the acquirer captures an even larger portion of gains, if the deal is financed by stock. The result suggests that although higher political uncertainty does not provide an ideal environment for investment, it allows foreign investors to buy domestic assets on better terms, and thereby enjoying more gains.

My paper contributes to the literature on the determinants of cross-border mergers and acquisitions. Many explanations have been put forward to explain the size and direction of cross-border M&A flows. Stock market valuation, corporate tax policy, and culture are such examples (Huizinga and Voget, 2009; Erel et al., 2012; Ahern et al., 2012). I conduct a comprehensive analysis of how political uncertainty affects cross-border M&A activity between each pair of 43 countries and document that political uncertainty significantly depresses cross-border M&A activity between countries.

My paper contributes even more to the literature on the economic effects of political uncertainty in several important ways. First, besides domestic corporate investments, the quality of a firm's capital allocation, and capital raising activities (Julio and Yook, 2012; Durnev, 2012; Col, Durnev, and Molchanov, 2013; Colak et al., 2013; Gao and Qi, 2013), I show that political uncertainty has influence on a firm's cross-border M&A decision. Second, I look at political uncertainty's effects on not only the magnitude and direction of cross-border M&A flows but also stock market reaction and deal structure. Using detailed characteristics of cross-border mergers and acquisitions, we can extend our analysis to industry-/firm-levels. For example, using stock returns around announcement dates, we can look at the quality of cross-border acquisitions undertaken during high political uncertainty periods and how the capital markets view such investments. In particular, cross-border M&A is a good setting to test the effects of political uncertainty on asset prices, which has gained much attention from recent asset pricing studies (e.g., Croce et al., 2012; Kelly et al., 2013; Pastor and Veronesi, 2013), because political uncertainty's effects are immediately observable in announcement returns. These are important reasons why we look at cross-border M&A which is the subset of FDI. FDI data used by Julio and Yook (2013) do not allow such comprehensive analysis.[1]

My study also has implications for policy makers. My results suggest that political uncertainty can discourage foreign investment into a country. My findings further indicate that political uncertainty has adverse effects on terms at which domestic assets are sold to foreign investors. The evidence here indicates a need for policy efforts to reduce uncertainty in a country's political environment.

The remainder of the paper is organized as follows. Section 1.2 describes the M&A and political data. Section 1.3 introduces the empirical methodology and discusses country-level tests on cross-border M&A activity around national elections. In Section 1.4, I discuss deal-level tests on takeover premiums, the division of merger gains, and payment method. Section 1.5 concludes.

1.2 Data

My initial sample includes all mergers and acquisitions from Securities Data Corporation (SDC) Platinum database from the 43 largest economies where elections are held to elect a national leader. I require each deal to be announced between 1990 and 2011 and completed by the end of 2012. I exclude leveraged buyouts, spin-offs,

[1] Furthermore, their FDI data are restricted to U.S. bilateral flows. Unlike their data on FDI flows, my cross-border M&A data also include cross-border deals for each bilateral country-pair, excluding the U.S. That is, my data are more comprehensively bilateral.

recapitalizations, exchange offers, self-tender offers, repurchases, minority stake purchases, acquisitions of minority interest, and privatizations. In terms of the public status of the acquirer and target, I include public, private, and subsidiary acquirers and targets but exclude government agencies. As in Netter, Stegemoller, and Wintoki (2011), private or subsidiary targets account for a substantial majority (94.5%) of my M&A sample. Since firms are not required to announce the value of a deal, I impose no limitations on transaction value (di Giovanni, 2005). The above data screens yield a sample of 319,501 acquisitions covering 43 countries, of which 65,821 are cross-border. For each acquisition, I collect transaction-specific information from SDC such as the announcement date, the payment method, the acquirer and target's country of domicile, four-digit Standard Industrial Classification (SIC) code, and termination fees.

Next, I acquire detailed information for each election across the 43 countries from various sources. My primary source of election data is the World Bank's 2012 Database of Political Institutions (Beck, Groff, Keefer, and Walsh, 2001). I supplement the election data with data compiled by other institutions and web resources including *The Center on Democratic Performance*, *Elections around the World*, *Election Guide*, *Election Resources on the Internet*, *Journal of Democracy*, and *The CIA World Factbook*.

The World Development Indicators from the World Bank provide macroeconomic data including gross domestic product (GDP) per capita, GDP growth rate data, and market capitalization of listed companies as a percentage of GDP for each country in my sample. I obtain annual value-weighted national stock market return indices in local currency from Datastream and annual bilateral $U.S. exchange rates from the Penn World Table. Using the stock market returns and currency data in conjunction with the consumer price index (CPI) from Datastream, I then compute annual real stock market returns and real exchange rate returns.

For the public acquirers and targets, I obtain annual accounting information from Compustat for US firms and from Datastream for non-US firms. The accounting information includes book value of equity, cash holdings, market capitalization, long-term debt, return on equity, and total sales. I obtain daily stock returns from Center for Research in Security Prices (CRSP) for US firms and obtain $U.S.-denominated daily stock returns from Datastream for non-US firms. See Appendix A for definitions of variables.

Table 1.1 reports the average number of cross-border M&As in election vs. non-election years by target country. Since my main objective is to examine the influence of heightened political uncertainty during the period leading to an election, similarly to Julio and Yook (2012) I classify a given country-year as an election year if a national election is held in the country between 90 days prior to the end of the year and 274 days after the end of the year (i.e., between October 1st of the current year and September 30th of the following year), and as a non-election year otherwise. The table shows that, for the majority of countries in my sample, the average number of cross-border deals is greater during non-election than election years. Figure 1.1 further illustrates the time-series pattern of cross-border M&A around national elections, in which t = 0 is the election year. The figure shows the negative effect of political uncertainty on cross-border M&A activity. Approaching an election year, the average number of cross-border deals sharply decreases (from 98 to 91 deals) and then rebounds to the pre-election period level in the following year, in which political uncertainty is resolved.

The results in Table 1.1 and Figure 1.1 are at best suggestive and might be attributable to other factors that need to be accounted for. Hence, the next section introduces a multivariate regression framework to examine the relationship between political uncertainty and cross-border mergers.

1.3 Country-level analysis

1.3.1 Methodology

Here I examine whether and how political uncertainty surrounding national elections affects aggregate cross-border M&A activity between a pair of countries by estimating multivariate regression models. The multivariate regression models follow Rossi and Volpin (2004), Ferreira, Massa, and Matos (2009), and Erel, Liao, and Weisbach (2012) and are designed to examine what factors explain the variation in cross-border M&A flows from one country (acquirer) to another country (target).

Benefiting from the richness of M&A data, I create a matrix of (43x42) ordered country-pairs in each year.[2] I require that each country-pair have at least 5 cross-border deals during the entire sample period, which results in a total of 15,955 country-pair-year observations.[3] The specification of the panel regression model is as follows:

$$(Cross\text{–}\ border\ M\&A\ pair)_{i,j,t} = \alpha + \beta Election_{j,t} + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t} \qquad (1.1)$$

In the equation, *i* indexes the acquirer's home country, *j* indexes the target's home country, and *t* indexes years. For each country-pair observation in a given year *t*, I define $(Cross - border\ M\&A\ pair)_{i,j,t}$ as the number of cross-border M&A deals in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the sum of the number of domestic M&A deals in country *j* and the number of cross-border M&A deals involving acquirer country *i* and target country *j* in year *t*. $Election_{j,t}$ is a dummy set equal to one if a national election is held in target country *j* between October 1st of the current year and September 30th of the following year, and zero otherwise (see, for example, Julio and Yook, 2012).

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[2] For example, my sample includes both US-France and France-US pairs and treats them as separate observations.

[3] The results are robust to different cutoffs ranging from 0 to 10.

The coefficient β is our main interest. β captures the effects of a national election on cross-border M&A flows from the acquirer's country *i* to the target's country j. Under my hypothesis, β is expected to be negative and significant. In addition, the regression model includes differences in time-variant country-specific factors ($X_{i,j,t}$) such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, and financial development. I also include country-pair fixed effects ($\delta_{i,j}$) to control for time-invariant differences between two countries, and year fixed effects (θ_t) to control for time-series variation in the global macroeconomic environment. In all regressions, standard errors are clustered at the country-pair level.

1.3.2 Main results

Table 1.2 presents the main results. In the first column, I include as independent variables acquirer- and target-country fixed effects to control for time-invariant country characteristics and additional country-level variables such as differences in corporate tax rates, economic development, language, religion, and stock market valuation, which are expected to influence cross-border M&A activity. I find that the volume of cross-border M&A activity is 0.29% lower in the year leading up to the target country's election. Given that the average cross-border M&A pair ratio between two countries is 3.88% in my sample, the effects of elections are economically significant. This result implies that the target country's policy uncertainty has a deterrent effect on cross-border M&A inflows. It is also worth noting that the coefficients of other control variables are comparable to those documented in Ferreira et al. (2010) and Erel et al. (2012). Valuation difference, bilateral trade, and geographic distance between countries are positively associated with cross-border merger activity.

I further introduce more stringent model specification as in Erel et al. (2012); I use country-pair fixed effects to control for both observable and unobservable time-invariant differences between two countries. Column 2 reports the coefficients. I find that cross-border M&A activity is 0.28% lower during national election years, which provides strong support for my hypothesis. In Column 3, l add additional dummy variables, Pre-election and Post-election, which are assigned a value of one during the years before and after the election year, and zero otherwise. The coefficient on the election dummy continues to be significant and negative. Indeed, the coefficient becomes more negative. The estimated coefficients on the other two dummies are negative but insignificant. Overall, the evidence presented here lends strong support to the argument that political uncertainty depresses cross-border M&A activity.

In Table 1.3, I try three alternative dependent variables. *CBMA_1* is defined as the number of cross-border deals from acquirer country *i* to target *j* in year *t* divided by the average number of cross-border deals from acquirer country *i* to target *j* in previous two years, from *t*-2 to *t*-1. An assumption I make about this variable is that the average cross-border M&A activity over the last two years is the expected cross-border merger activity in the current year. *CBMA_1* measures the extent to which cross-border M&A activity between two countries varies with respect to the expected value. The result using *CBMA_1* is reported in Column 1. The result indicates that political uncertainty results in 4.6% lower than expected cross-border M&A activity. In Column 2, I use *CBMA_2*, which is constructed similarly to *CBMA_1*, but the denominator is the average cross-border M&A activity over the last three years. The result shows that during the election period, cross-border M&A activity is 5.5% lower than expected.

The above dependent variables are based on the number of merger deals. To gain insight into how political uncertainty influences aggregate dollar value of cross-border M&A flows between countries, in Column 3 in Table 1.3, I construct a dependent variable using transaction value reported by SDC. *CBMA value* is defined as aggregate dollar value of all cross-border deals in year *t* from acquirer country *i* to target country *j* ($i \neq j$) divided by the sum of aggregate dollar value of all domestic M&A deals in country *j* and aggregate dollar value of all cross-border deals involving acquirer country *i* and target country *j* in year *t*. The result in Column 3 suggests that aggregate cross-border transaction value drops by 0.63% before elections. Given that the average value of *CBMA value* is 6.75%, a decrease of 63 basis points is economically significant.

Although the coefficient on the election dummy is significant at a conventional level (10%), its statistical significance is slightly weaker compared to when dependent variables are constructed using the number of deals. This may be because SDC transaction value is more unavailable for target firms from civil law countries, in which political uncertainty is expected to have stronger influence. Civil law countries are characterized by weaker investor protection and less developed financial markets (La Porta et al., 1998). Such countries tend to have lower quality (i.e., less timely and less transparent) accounting standards than common law countries (Francis et al., 2001), which may compound the effects of political uncertainty. On average, SDC transaction value is available roughly for 40% of deals, depending on a country's disclosure requirements or the public status of the target company. Among deals in which the target firm is from a civil law country, only 29% of them have transaction value available, whereas the availability is higher at 46% for deals involving targets from common law countries. Thus, precisely where I expect political uncertainty's influence to be strongest, my sample is smaller due to lack of transaction value data. Consistent with the greater importance of political uncertainty on cross-border M&A activity in civil law countries, note the following: In the main results (Table 1.2), I document that cross-border merger activity, which is based on the number of deals, declines by 0.28% during election periods. This result is more pronounced for civil law countries (coefficient = -0.38%, *t*-stat = 2.24).[4] When I use transaction value to measure cross-border merger activity (i.e., *CBMA value*), the sample size for civil law countries shrinks by 49.3%, and the estimated coefficient becomes statistically weaker (coefficient = -0.94%, *t*-stat = -1.55).

[4] Julio and Yook (2012) also document stronger political uncertainty's influence among civil law countries, although their focus is on domestic investment.

1.3.3 Close elections

Uncertainty about future government policy is expected to be higher if a forthcoming election is harder to predict. Comprehensive data on the predictability of election outcomes are not available, especially for foreign countries. Instead, I use *ex post* electoral margins as a proxy for *ex ante* election predictability. I define a given election as a *close election* if its margin of victory belongs to the bottom tercile of all election margins. The results using close elections are reported in Table 1.4. The coefficient on *Election*Close election* is negative but not significant. However, the total effect of a close election, the sum of the coefficients on *Election* and *Election*Close election*, is -0.53%, which is statistically significant. The coefficient on *Election* by itself is not significant. The evidence suggests that political uncertainty may vary across elections, and elections that are harder to predict associate with greater political uncertainty, and therefore have more adverse effects on cross-border M&A activity.

1.3.4 Endogenously-timed elections

A potential endogeneity problem arises when election timing is not exogenous to a country's economic state, which in turn can affect cross-border merger decisions. Indeed, among many countries in my sample (25 out of 43 countries), governments are allowed to call early elections. Previous studies in the political economy literature suggest a close link between election timing and economic conditions. For instance, the incumbent governments may opportunistically set election dates prior to the ends of their terms in response to good economic performance, which is perceived to enhance the likelihood of their re-election (Kayser, 2005). Ito (1990) empirically documents that Japanese elections are more likely to be held earlier in an expanding economy. Considering the possibility that the negative causal impact of national elections on cross-border M&A activity could be attributable to endogenously-timed elections, I re-estimate the main regressions in Table 1.2 by using only exogenously-timed (i.e., regularly scheduled) elections as in Julio and Yook (2012). Columns 1 and 2 in Panel A, Table 1.5 present the estimation results. I find significant and negative effects of national elections on cross-border M&A deals among countries with exogenously-timed elections.

As another approach to the endogeneity problem, I now use as an instrument for actual elections, a dummy equal to one if an election is scheduled in a given year or a scheduled election has not occurred since the last election (Khemani, 2004). Using this instrument, I estimate the likelihood of elections (Panel B in Table 1.5). Not surprisingly, this instrument predicts the likelihood of election very well (Pseudo R^2 is 0.58). I calculate the inverse Mill's ratio (a.k.a. Heckman's lambda) using the predicted values from the first stage and re-estimate the regression model including the Mill's ratio. The results are reported in Panel C, Table 1.5. The coefficient on the election dummy is significantly negative. Indeed, the effect's magnitude becomes apparently larger after controlling for endogeneity.

The results using fixed elections and an instrument variable suggest that my results are not driven by endogeneity. If anything, the results seem to be weakened by it.

1.3.5 Industry characteristics

In this section, I conduct sub-sample analysis to gain further insight into possible mechanisms through which policy uncertainty adversely affects cross-border M&A activity. I conjecture that a foreign investor's decision to make or delay acquisitions largely depends on industry characteristics of the target under consideration: Foreign investors' tendency to delay cross-border acquisitions amid the host country's electoral uncertainty is expected to be stronger, especially when the target firm is from an industry that relies heavily on a country's political environment, thereby exposing it to higher political uncertainty. I consider three industry characteristics: input complexity (Blanchard and Kremer, 1997; Boutchkova et al., 2012), labor intensity (Jorgenson, 1990;

Boutchkova et al., 2012), and exposure to government spending (Belo, Gala, and Li, 2013).

The literature documents that industries with high input complexity demand a good governance environment that provides strong contract enforcement and good property right protection (Blanchard and Kremer, 1997; Rajan and Subramanian, 2007). Weak contract enforcement can have more adverse effects on industries with complex input structures, because such industries find it more efficient to arrange business transactions through contracts rather than using other methods such as vertical integration. Given that a country's governance quality is related to its political environment, I expect cross-border deals involving a target company with more complex input structures (i.e., more dependent on contract enforcement) to be more subject to electoral uncertainty.

Next, elections can be followed by dramatic changes in labor laws, depending on new national leaders' policy orientation (Botero et al., 2004). Potential changes to labor policies ranging from the minimum wage to layoff compensation can significantly affect a firm's future prospects, especially when the firm's main sources of profits are labor-intensive products. In this regard, I conjecture that the negative effects of election uncertainty are stronger for cross-border acquisitions of targets from more labor-intensive industries than for acquisitions of targets from less labor-intensive industries.

Lastly, industry reliance on government spending is considered. The political economy literature indicates that national elections are often accompanied by changes in government spending policies, in which political leaders' preferences differ (Alesina, 1987). If a large fraction of an industry's total sales is represented by the government sector, I expect that the industry is more subject to policy uncertainty and therefore less likely to be acquired during an election year.

To test the hypotheses, I use data on input-complexity and labor-intensity in Boutchkova et al. (2012) and classify each industry as a *high* or *low* input-complexity (labor-intensity) industry. If a given industry's input-complexity (labor-intensity) is above average (0.841 for input-complexity; 0.275 for labor-intensity), I classify it as a high input-complexity (labor-intensity) industry, otherwise as a low input-complexity (labor-intensity) industry. Regarding exposure to government spending, I borrow the classification proposed by Belo et al. (2013): if a given industry is one of the ten industries having the highest exposure to government spending, I classify it as a high government-spending industry, and otherwise as a low government-spending industry.[5] Next, I break down the entire sample of merger deals into subsamples based on whether the target is from a high vs. low input-complexity (labor-intensity) [government-spending] industry. For each subsample, I aggregate the deals to the county-pair level as before and estimate the equation (1.1).

First, Table 1.6 presents the estimation results using the subsamples of high and low input-complexity industries. In Column 1, I do not find the significant negative effects of election uncertainty on cross-border deals when the target is from a low input-complexity industry. Even the coefficient on the election dummy is positive. In contrast, I find a significantly negative coefficient (0.396%) on the election dummy in the subsample of high input-complexity (Column 2), which is consistent with my expectation. The results suggest that political uncertainty has stronger adverse effects on cross-border M&A activity when the target under consideration depends more on contract enforcement that is shaped by politics.

In Table 1.7, I present the estimates using the subsamples of high and low labor-intensity industries. Consistent with the hypothesis, I do not find a significant decline in

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[5] The ten industries with high exposure to government spending include guided missile and space vehicle manufacturing (I-O code: 336414), shipbuilding and repairing (336611), radio and television broadcasting (515100), scientific research and development services (541700), electric lamp bulb and part manufacturing (335110), oil and gas extraction (211000), newspaper publishers (511110), printed circuit assembly manufacturing (334418), broadcast and wireless communications equipment (334220), and paper mills (322120). Source: Belo, Gala, and Li (2013).

cross-border deals around elections in the subsample of deals in which the target is from a low labor-intensive industry (Column 1). However, among the deals in which the target operates in a highly labor-intensive industry, electoral uncertainty appears to have negative and significant effects on cross-border acquisition activity.

Now we turn to the last industry characteristic: government spending. Table 1.8 presents the results using the subsamples of high and low government-spending industries. While I do not find significant effects of electoral uncertainty among target industries with low exposure to government spending, I find significant and negative effects of electoral uncertainty among target industries with high exposure to government spending. The results here are consistent with my expectations.

Overall, the findings here indicate that the target firm's industry characteristics play an important role in explaining cross-border M&A decisions amid political uncertainty. Specifically, I find that firms in industries that are more dependent on the quality of contract enforcement, labor policies, and government spending are less likely to be acquired during election years.

1.4 Deal-level analysis

The results obtained so far indicate that foreign investors are reluctant to undertake cross-border M&A in the face of increased political uncertainty in the host country. Nevertheless, my data shows that despite heightened political risk, some firms still engage in cross-border mergers during election years. This naturally leads to the following question: what is different about cross-border mergers undertaken during election years, whereby acquirers find it profitable to make such deals? The next section is devoted to investigating this question. In particular, I examine how political uncertainty characterize negotiation terms over pricing and deal structure.

1.4.1 Takeover premium analysis

Foreign acquirers are reluctant to acquire local targets which are subject to high political risk, and therefore may demand compensation for their acquisition decisions. Such compensation can take the form of lower takeover premiums, which might make transactions more attractive during politically uncertain times. This argument is supported by recent developments in the theory of asset pricing, which suggest that political uncertainty has valuation effects (Croce, Kung, Nguyen, and Schmid, 2012; Kelly, Pastor, and Veronesi, 2013; Pastor and Veronesi, 2013). Accordingly, I propose hypotheses with regards to how cross-border deals with political risk affect shareholder wealth. I expect that the takeover premium paid to target shareholders is lower if there is a forthcoming national election in the target country.

Using a sample of individual cross-border deals, I analyze the effects of political uncertainty on takeover premiums. Specifically, I estimate the following model:

$$Target\ CAR(-3,+3)_{n,m,t} = \alpha + \beta Election\ year\ deal_{m,t} + \gamma X_{n,m,t} + \vartheta Y_{n,m,t} + \delta_{n,m} + \theta_t + \varepsilon_{n,m,t} \quad (1.2)$$

In the equation, *n* indexes acquirers, *m* indexes targets, and *t* indexes years. $Target\ CAR(-3,+3)_{n,m,t}$ is defined as cumulative abnormal returns over seven days around the merger announcement: stock returns minus returns predicted by a market model, over the systematic seven-day event window around the announcement date. Abnormal returns are estimated using a two-factor model with the equity market index for each country and the MSCI World index (Griffin, 2002). Specifically, for each company I estimate the following model over days -280 through -30, where day 0 is the takeover announcement day:

$$r_{n,t} = \alpha_n + \beta_{1,n} r_{country,t} + \beta_{2,n} r_{world,t} + \varepsilon_{n,t} \quad (1.3)$$

$Election\ year\ deal_{m,t}$ is a dummy variable which equals one if there is a national election in the target country within one year after the announcement of the deal and zero otherwise. I do not expect electoral uncertainty to be the only determinant of takeover premiums. To control for possible influences other than electoral uncertainty, I further require both acquirer and target's accounting information to be accessible via either Compustat (US firms) or Datastream (non-US firms). The data requirements on target stock returns and accounting information reduce the sample size to 877 cross-border M&A deals. $X_{n,m,t}$ includes a set of firm- and deal-level control variables such as firm-size, market-to-book ratio, leverage, cash, and diversifying acquisition dummy. $Y_{n,m,t}$ is a set of country-level characteristics. The coefficient β represents the effects of the target country's election on the takeover premium, which is our primary interest. All regressions include acquirer- and target-country fixed effects and year fixed effects. Standard errors are clustered in two dimensions: acquirer and target country levels. Table 1.9 reports the estimation results. Consistent with my prediction, takeover premiums in cross-border acquisitions are about 2.7% lower when there is a forthcoming election in the target country (Column 1).

In Column 2, I address a potential sample selection problem. I observe a takeover premium only if a deal is made. The likelihood of a cross-border deal, however, is affected by political uncertainty, and therefore my sample might not represent random sub-sample of cross-border M&A. In such a case, the OLS estimation may not be reliable. To correct for sample selection bias, I employ Heckman's two-step procedure. First, I model the likelihood that a cross-border deal is announced during an election period and calculate the inverse Mill's ratio.[6] In the second stage, I re-estimate the regression model including the inverse Mill's ratio as a control variable. Column 2 reports the estimation results. The effect of political uncertainty remains negative and significant (coefficient = -2.96%, t-stat = 3.08). This suggests that my takeover premium results are not driven by sample selection bias.

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[6] See Appendix B.

I further examine whether takeover premiums vary by industry characteristic. As in the previous section, I look at three industry characteristics: reliance on contract enforcement, labor intensity, and reliance on government spending. Given that these industries expose acquirers to higher political uncertainty (Tables 1.6, 1.7, and 1.8), I expect takeover premiums to be even lower. I interact an *Election year deal* dummy with the industry dummy variables and add them to the regression. Column 3 in Table 1.9 reports the estimation results. The coefficients on three interacted terms are negative, consistent with my prediction. However, none of them are statistically significant at conventional levels.

Column 4 restricts the sample to cross-border deals in which targets are from civil law countries. The quality of law enforcement is lower in civil law countries than common law countries (La Porta et al., 1998). For this reason, in civil law countries, targets which demand good contract enforcement (i.e., targets with complex input structures) might be even more unattractive. Thus, I expect political uncertainty's impact on bid premiums via input complexity to be much stronger in civil law countries. Consistent with my expectation, I find that the coefficient on *Election year deal*Complex input structure* is negative and significant.

In Column 5, I restrict the sample to target countries, in which government spending constitutes a large fraction of a country's GDP. In such countries, government sectors may represent larger buyers and have a stronger influence on supplier firms' profitability. Accordingly, I expect that in countries with a high ratio of government spending to GDP, targets relying on government purchases are even more strongly affected by political uncertainty, and thereby being offered lower premiums. I use data from The World Bank on general government final consumption expenditure as a percentage of GDP. I restrict the sample to target countries in which the ratio of government spending to GDP is above the mean (17.03%) and re-estimate the regression in Column 5. The results support this prediction. The coefficient on *Election year deal*Government spending* is negative and significant.

Overall, the results here suggest that foreign acquirers tend to pay lower premiums during politically uncertain times. The evidence is also consistent with recent asset pricing studies documenting the valuation effects of political uncertainty. Further, I find that the negative effects of political uncertainty on takeover premiums vary by industry exposure to political uncertainty.

1.4.2 Payment method

I examine whether political uncertainty affects the way an acquirer determines the payment method in a cross-border takeover offer. Previous studies in the M&A literature highlight the important differences between cash and stock offers, especially when there is uncertainty about the value of the target (Hansen, 1987; Fishman, 1989). A key distinction is that the value of a stock offer is contingent on the post-merger performance of the combined firm, and therefore the true value of the acquired firm, whereas the value of a cash offer is not. This feature of a stock offer allows the acquirer to share, to some extent, the risk of misvaluing the target with the target. Hansen (1987) presents a theoretical model and shows that if there is greater uncertainty over the value of the target, the acquirer is more likely to make a contingent payment in stock, rather than cash. Thus, I expect that the acquirer is less likely to make an all-cash offer when there is a forthcoming national election in the target country, which is associated with increased political uncertainty.

To test my hypothesis, I estimate the following logistic model:

$$All\ cash\ offer_{n,m,t} = \alpha + \beta Election\ year\ deal_{m,t} + \gamma X_{n,m,t} + \vartheta Y_{n,m,t} + \delta_{n,m} + \theta_t + \varepsilon_{n,m,t} \tag{1.4}$$

In the equation, *n* indexes acquirers, *m* indexes targets, and *t* indexes years. The dependent variable is a dummy variable equal to one if a cross-border deal is paid entirely in cash, and zero otherwise. The coefficient β is again my primary interest, and I expect it to be negative and significant. In all regressions, I include acquirer- and target-country fixed effects and year fixed effects. I cluster standard errors at the country-pair level. Columns 1 and 2 in Table 1.10 report the estimation results. In Column 1, I find that the likelihood of an all-cash bid is significantly lower when there is a forthcoming national election in the target country, consistent with my expectation. Column 2 includes additional control variables: the acquirer's size, market-to-book, leverage, and cash holdings. This reduces the sample size slightly. I continue to find a negative and significant coefficient on the election-year-deal dummy. Overall, the results here suggest that political uncertainty is an important consideration when determining the payment method in cross-border mergers, and acquirers are less likely to make all-cash offers in the face of increased political uncertainty.

1.4.3 Division of cross-border merger gains

The results thus far suggest that foreign acquirers pay less and pay more in stock when political uncertainty is high. Finally, I test whether foreign acquirers benefit from these advantageous negotiation terms and extract more value from cross-border merger deals during election periods. I construct two empirical measures (*Δ$CAR_1* and *Δ$CAR_2*) to calculate the target's fraction of merger gains (compared to the acquirer's) as follows. *Δ$CAR_1* is defined as the target's cumulative abnormal dollar returns minus the acquirer's cumulative abnormal dollar returns over seven days around the announcement, all scaled by the weighted average of acquirer and target's abnormal dollar returns in which the weights are based on acquirer and target's market values (in US dollars) fifty trading days before the announcement. The second measure, *Δ$CAR_2*, follows Ahern (2012) and is defined as the target's cumulative abnormal dollar returns minus the acquirer's cumulative abnormal dollar returns over seven days around the announcement, all scaled by the sum of acquirer and target market values (in US dollars) fifty trading days prior to the deal announcement. If the results so far truly indicate the target's weakened bargaining power during an election year, the target's fraction of merger gain relative to the acquirer is expected to be lower. I estimate the following model and present the results in Table 1.11:

$$\Delta\$CAR_{n,m,t} = \alpha + \beta Election\ year\ deal_{n,m,t} + \gamma X_{n,m,t} + \vartheta Y_{n,m,t} + \delta_{n,m} + \theta_t + \varepsilon_{n,m,t} \quad (1.5)$$

The estimation results in Columns (1) and (3) show that the target captures a smaller fraction of merger gains during election periods, which is consistent with my hypothesis. Considering shift of acquirers' preference towards stock payment, I further test whether the acquirer captures an even larger fraction of merger gains if she uses stock to finance the deal. I find that when a cross-border deal is financed at least partially by stock, the acquirer tends to capture even larger fraction of merger gains (Columns 2 and 4). This evidence is very intriguing given that the M&A literature often document negative association between stock payment and bidder shareholder wealth. However, if a merger deal faces high political risk, we observe the opposite; bidders are better off using stock, which can mitigate, to some extent, political risk.

The evidence suggests that acquirers exploit greater bargaining power during high political uncertainty periods extract more value from cross-border deals. The acquirer's gain is even greater if he/she uses stock as a payment method.

1.5 Concluding remarks

This study shows that political uncertainty is an important determinant of cross-border M&A activity. The volume of cross-border M&A activity significantly declines right before national elections which bring about significant political uncertainty. This result is robust to controlling for other macroeconomic variables that are known to affect cross-border M&A in the literature. Cross-border M&A activity declines even further if elections are harder to predict.

In addition, the deterrent effects of policy uncertainty vary by the target firm's industry characteristics. Firms in industries that depend more on the quality of contract enforcement, labor, and government spending are less likely to be acquired by foreign bidders during national election years.

My deal-level analysis reveals that policy uncertainty surrounding national elections negatively affect takeover premiums. Bid premiums paid to target shareholders are about 2.7% lower when a cross-border deal is announced in the year leading up to the target country's election. Further results indicate that the target captures a smaller share of merger gains relative to the acquirer in such cross-border deals. Lastly, the acquirer is less likely to make an all-cash offer if there is a forthcoming election in the target country, which suggests that the acquirer tends to share political risk with the target by making a contingent payment.

Table 1.1 The average number of cross-border M&A in 43 countries during 1990-2011

	The average number of cross-border M&A deals	
Country name	Non-election year	Election year
Argentina (AR)	33.17	43.00
Australia (AU)	113.87	116.71
Austria (AS)	32.80	36.86
Belgium (BL)	64.88	57.17
Brazil (BR)	54.59	68.60
Canada (CA)	191.73	218.43
Chile (CE)	17.83	21.25
Columbia (CO)	13.77	7.80
Czech Republic (CC)	37.29	27.40
Denmark (DN)	53.53	55.14
Finland (FN)	49.06	39.67
France (FR)	200.17	200.25
Germany (WG)	265.50	244.50
Greece (GR)	5.33	5.29
Hungary (HU)	18.77	13.20
India (IN)	63.57	47.00
Indonesia (ID)	24.82	19.50
Ireland (IR)	37.24	33.20
Israel (IS)	14.20	23.00
Italy (IT)	90.31	102.33
Japan (JP)	22.13	26.17
Luxembourg (LX)	9.72	9.25
Malaysia (MA)	22.06	14.80
Mexico (MX)	41.83	34.25
Netherlands (NT)	107.73	99.29
New Zealand (NZ)	39.21	35.75
Norway (NO)	45.12	51.40
Peru (PE)	10.59	11.60
Philippines (PH)	7.83	4.50

Table 1.1 Continued

Poland (PL)	32.87	30.43
Portugal (PO)	17.93	22.00
Russia (RU)	28.13	31.60
Singapore (SG)	30.24	34.60
South Africa (SA)	27.17	24.75
South Korea (SK)	18.00	9.50
Spain (SP)	88.56	100.17
Sweden (SW)	97.13	77.17
Switzerland (SZ)	69.81	77.67
Thailand (TH)	10.87	8.57
Turkey (TK)	16.44	14.00
United Kingdom (UK)	388.41	358.20
United States (US)	568.82	602.60
Venezuela (VE)	5.28	7.25

Note: This table reports the average number of cross-border M&A deals in election vs. non-election years by country. Cross-border M&A deals in 43 countries are obtained from SDC and include all public, private, and subsidiary acquirers and targets between 1990 and 2011.

Table 1.2 The effects of political uncertainty on cross-border M&A activity

	Dependent variable = Cross-border M&A pair		
	Average (Cross-border M&A pair) = 3.80%		
Variables	(1)	(2)	(3)
Pre-election			-0.219
			[-1.30]
Election	**-0.292****	**-0.277****	**-0.374****
	[-2.33]	***[-2.22]***	***[-2.26]***
Post-election			-0.087
			[-0.49]
ΔGDP per capita	1.461***	1.284***	1.277***
	[3.83]	*[3.45]*	*[3.43]*
ΔReal GDP growth rate	9.391***	11.247***	11.227***
	[2.96]	*[3.60]*	*[3.59]*
ΔStock market return	0.035**	0.030*	0.030*
	[2.22]	*[1.92]*	*[1.93]*
ΔReal exchange rate return	0.041	0.067	0.071
	[0.32]	*[0.53]*	*[0.57]*
ΔFinancial development	-0.001	-0.001	-0.001
	[-0.34]	*[-0.52]*	*[-0.51]*
ΔCorporate tax	-13.463**		
	[-2.16]		
Bilateral trade	0.033***	-0.037	-0.038
	[5.47]	*[-0.61]*	*[-0.62]*
Acquirer openness	0.015	0.022***	0.022***
	[1.28]	*[4.26]*	*[4.25]*
Target openness	0.460***	0.015	0.015
	[5.35]	*[1.31]*	*[1.32]*
Same legal origin	-17.486***		
	[-3.94]		
Acquirer legal origin	5.035*		
	[1.77]		

Table 1.2 Continued

Target legal origin	0.082		
	[0.23]		
Share border	4.460***		
	[5.80]		
Same language	1.315		
	[1.42]		
Same religion	0.103		
	[0.32]		
Geographical distance	-0.110		
	[-0.61]		
Year fixed effects (F.E.)	Yes	Yes	Yes
Acquirer-/target-country F.E.	Yes	No	No
Country-pair F.E.	No	Yes	Yes
No. of observations	15,955	15,955	15,955
No. of total M&A deals	292,951	292,951	292,951
No. of cross-border M&A deals	64,050	64,050	64,050
R^2	0.296	0.443	0.443

Note: This table presents estimates of the following panel regressions of cross-border M&A pairs in each year:

$$(Cross - border\ M\&A\ pair)_{i,j,t} = \alpha + \beta(Election)_t + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t}$$

The dependent variable is cross-border M&A country-pair, defined as the number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) as a percentage of the sum of the number of cross-border deals involving acquiring country *i* and target country *j* and that of domestic deals in target country *j* in year *t.* I require that a given country-pair have at least 5 cross-border deals during the entire sample period, which results in a total of 15,955 country-pair-year observations. $Election_t$ is a dummy variable set equal to one if a national election is held in the target country between 90 days prior to the end of year t and 274 days after the end of year t, and zero otherwise. $Pre - election_t$ is a dummy variable set equal to one during the year before the election year and zero otherwise. $Post - election_t$ is a dummy variable set equal to one during the year after the election year and zero otherwise. $X_{i,j,t}$ is a set of country-specific factors such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, financial development, etc. Refer to Appendix A for definitions of variables in more detail. $\delta_{i,j}$ represents acquirer-/target-country fixed effects in Column 1

Table 1.2 Continued

and country-pair fixed effects in Columns 2 and 3. Year fixed effects, θ_t, are included in all regressions. Standard errors are clustered at the country-pair level. T-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.3 Alternative dependent variables

Variable	*CBMA_1*	*CBMA_2*	*CBMA value*
Election	**-4.603****	**-5.523****	**-0.628***
	[-2.17]	***[1.98]***	***[-1.67]***
ΔGDP per capita	0.489	0.947	-1.570*
	[0.11]	*[0.16]*	*[-1.68]*
ΔReal GDP growth rate	46.117	61.661	17.940**
	[1.61]	*[1.54]*	*[2.23]*
ΔStock market return	0.275***	0.326***	0.041***
	[4.73]	*[4.77]*	*[2.80]*
ΔReal exchange rate return	1.959**	2.837**	-0.206
	[2.21]	*[2.08]*	*[-0.62]*
ΔFinancial development	0.055**	0.066*	0.012**
	[1.96]	*[1.68]*	*[2.42]*
Bilateral trade	1.211	0.262	0.136
	[1.47]	*[1.27]*	*[0.93]*
Acquirer openness	0.183**	0.300**	0.015
	[1.99]	*[2.15]*	*[1.17]*
Target openness	0.232***	1.549**	0.085***
	[2.74]	*[2.48]*	*[3.65]*
Year fixed effects (F.E.)	Yes	Yes	Yes
Country-pair F.E.	Yes	Yes	Yes
Number of observations	15,955	15,955	8,822
R^2	0.152	0.109	0.256

Note: The table presents estimates of the following panel regressions of cross-border M&A activity:

$$(Dependent\ variable)_{i,j,t} = \alpha + \beta(Election)_t + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t}$$

CBMA_1 is defined as the number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the average number of cross-border deals in years *t-2* and *t-1* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$). *CBMA_2* is defined as the number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the average number of cross-border deals in years *t-3* and *t-1* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$). *CBMA value* is defined as aggregate dollar value of all cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the sum of aggregate dollar value of all domestic M&A deals in country *j* and aggregate dollar value of all cross-border deals involving acquirer country *i* and target country *j* in year $t.Election_t$ is a dummy variable set equal to one if a national election is held in the target country between 90 days prior to

Table 1.3 Continued

the end of year t and 274 days after the end of year t, and zero otherwise. $Pre - election_t$ is a dummy variable set equal to one during the year before the election year and zero otherwise. $Post - election_t$ is a dummy variable set equal to one during the year after the election year and zero otherwise. $X_{i,j,t}$ is a set of country-specific factors such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, financial development, etc. Refer to Appendix A for definitions of variables in more detail. If the ruling party is able to call an early election prior to the regularly scheduled election date, a given country is classified as having a flexible election timing schedule. Otherwise, I classify a country as having a fixed election timing schedule. Country-pair fixed effects, $\delta_{i,j}$, and year fixed effects, θ_t, are included in all regressions. Standard errors are clustered at the country-pair level. *T*-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.4 Close elections

	Dependent variable = Cross-border M&A pair
Election	**-0.177**
	[-1.27]
Election*Close election	**-0.355**
	[-1.43]
ΔGDP per capita	1.276***
	[3.42]
ΔReal GDP growth rate	11.342***
	[3.62]
ΔStock market return	0.030**
	[1.92]
ΔReal exchange rate return	0.069
	[0.55]
ΔFinancial development	-0.001
	[-0.49]
Bilateral trade	-0.036
	[-0.60]
Acquirer openness	0.022***
	[4.26]
Target openness	0.015
	[1.33]
Country-pair fixed effects	Yes
Year fixed effects	Yes
Number of observations	15,955
R^2	0.443
β(Election)+β(Election*Close election)	**-0.533****
t-stat	***[2.47]***

Table 1.4 Continued

Note: This table presents estimates of the following panel regressions of cross-border M&A pairs in each year:

$$(Cross - border\ M\&A\ pair)_{i,j,t} = \alpha + \beta(Election)_t + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t}$$

The dependent variable is cross-border M&A country-pair, defined as the number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) as a percentage of the sum of the number of cross-border deals involving acquiring country *i* and target country *j* and that of domestic deals in target country *j* in year *t*. I require that a given country-pair have at least 5 cross-border deals during the entire sample period, which results in a total of 15,955 country-pair-year observations. $Election_t$ is a dummy variable set equal to one if a national election is held in the target country between 90 days prior to the end of year t and 274 days after the end of year t, and zero otherwise. $Pre - election_t$ is a dummy variable set equal to one during the year before the election year and zero otherwise. $Post - election_t$ is a dummy variable set equal to one during the year after the election year and zero otherwise. $X_{i,j,t}$ is a set of country-specific factors such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, financial development, etc. *Close election* is a dummy equal to one if a election's margin of victory belongs to the bottom tercile of all election margins. Refer to Appendix A for definitions of variables in more detail. $\delta_{i,j}$ represents acquirer-/target-country fixed effects in Column 1 and country-pair fixed effects in Columns 2 and 3. Year fixed effects, θ_t, are included in all regressions. Standard errors are clustered at the country-pair level. *T*-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.5 Endogenous elections

Panel A

	Fixed elections	
	Dependent variable = Cross-border M&A pair	
Variables	(1)	(2)
Pre-election		-0.309
		[-0.93]
Election	**-0.611****	**-0.697****
	[-2.16]	***[-2.17]***
Post-election		0.010
		[0.03]
ΔGDP per capita	0.940*	0.935*
	[1.72]	*[1.71]*
ΔReal GDP growth rate	13.643***	13.563***
	[2.66]	*[2.64]*
ΔStock market return	0.029*	0.029*
	[1.70]	*[1.70]*
ΔReal exchange rate return	0.062	0.073
	[0.37]	*[0.43]*
ΔFinancial development	0.002	0.002
	[0.53]	*[0.55]*
Bilateral trade	0.077	0.075
	[0.63]	*[0.61]*
Acquirer openness	0.030***	0.030***
	[2.85]	*[2.84]*
Target openness	0.011	0.011
	[0.53]	*[0.54]*
Year fixed effects	Yes	Yes
Country-pair fixed effects	Yes	Yes
Number of observations	6,539	6,539
R^2	0.411	0.412

Table 1.5 Continued

Panel B

Variable	*Dependent variable = Election*
Intercept	-1.303***
p-value	*(0.00)*
Scheduled election dummy	**2.607*** **
p-value	*(0.00)*
Number of observations	15,955
Likelihood ratio	7,960
Pseudo R^2	0.58

Panel C

Variable	*Dependent variable = Cross-border M&A pair*
Election	**-0.462**
	-2.12
ΔGDP per capita	1.268
	3.40
ΔReal GDP growth rate	11.306
	3.63
ΔStock market return	0.000
	1.91
ΔReal exchange rate return	0.067
	0.53
ΔFinancial development	-0.001
	-0.53
Bilateral trade	-0.037
	-0.62
Acquirer openness	0.022
	4.26
Target openness	0.015
	1.31
Heckman's lambda	-0.172
	-1.03

Table 1.5 Continued

Year fixed effects (F.E.)	Yes
Acquirer-/target-country F.E.	Yes
Country-pair F.E.	No
Number of observations	15,955
R^2	0.443

Note: Panel A presents estimates of the following panel regressions of cross-border M&A pairs for subsamples constructed based on election timing:

$$(Cross - border\ M\&A\ pair)_{i,j,t} = \alpha + \beta(Election)_t + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t}$$

The dependent variable is cross-border M&A country-pair, defined as the number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country j ($i \neq j$) as a percentage of the sum of the number of cross-border deals involving acquiring country *i* and target country j and that of domestic deals in target country j in year *t*. $Election_t$ is a dummy variable set equal to one if a national election is held in the target country between 90 days prior to the end of year t and 274 days after the end of year t, and zero otherwise. $X_{i,j,t}$ is a set of country-specific factors such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, financial development, etc. Refer to Appendix A for definitions of variables in more detail. If the ruling party is able to call an early election prior to the regularly scheduled election date, a given country is classified as having a flexible election timing schedule. Otherwise, I classify a country as having a fixed election timing schedule. Country-pair fixed effects, $\delta_{i,j}$, and year fixed effects, θ_t, are included in all regressions. Standard errors are clustered at the country-pair level. T-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.6 The effects of political uncertainty on cross-border M&A: Input complexity

	Dependent variable = Cross-border M&A pair	
	Low input complexity	High input complexity
Variable	(1)	(2)
Election	**0.484***	**-0.396*****
	[1.93]	***[-2.29]***
ΔGDP per capita	-0.288	1.161**
	[-0.39]	*[1.96]*
ΔReal GDP growth rate	9.847*	13.042***
	[1.74]	*[2.97]*
ΔStock market return	0.020	0.045***
	[0.79]	*[4.08]*
ΔReal exchange rate return	-0.125	0.322
	[-0.47]	*[1.55]*
ΔFinancial development	0.011***	-0.001
	[3.90]	*[-0.48]*
Bilateral trade	23.762***	19.505**
	[2.98]	*[1.98]*
Acquirer openness	0.024***	0.013*
	[2.83]	*[1.68]*
Target openness	0.051***	0.026
	[2.64]	*[1.25]*
Year fixed effects	Yes	Yes
Country-pair fixed effects	Yes	Yes
Number of observations	7,868	12,333
R^2	0.285	0.402

Note: This table presents estimates of the following panel regressions of cross-border M&A pairs in each year:

$$(Cross - border\ M\&A\ pair)_{i,j,t} = \alpha + \beta(Election)_t + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t}$$

Column 1 presents the estimation results using subsamples of deals in which the target is from an industry with low input-complexity. Column 2 presents the estimation results using subsamples of deals in which the target is from a high input-complexity industry. An industry is classified as a high input-complexity

Table 1.6 Continued

industry if the input-complexity is above the average (0.841), otherwise a low input-complexity industry. Industry-complexity measures are obtained from Boutchkova et al. (2012). The dependent variable is cross-border M&A country-pair, defined as the number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) as a percentage of the sum of the number of cross-border deals involving acquiring country i and target country *j* and that of domestic deals in target country *j* in year *t*. I require that a given country-pair have at least 5 cross-border deals during the entire sample period. $Election_t$ is a dummy variable set equal to one if a national election is held in the target country between 90 days prior to the end of year t and 274 days after the end of year t, and zero otherwise. $X_{i,j,t}$ is a set of country-specific factors such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, financial development, etc. Refer to Appendix A for definitions of variables in more detail. Country-pair fixed effects, $\delta_{i,j}$, and year fixed effects, θ_t, are included in all regressions. Standard errors are clustered at the country-pair level. T-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.7 The effects of political uncertainty on cross-border M&A: Labor intensity

	Dependent variable = Cross-border M&A pair	
	Low labor intensity	High labor intensity
Variable	(1)	(2)
Election	**-0.093**	**-0.589****
	[-0.43]	***[-2.18]***
ΔGDP per capita	1.044*	1.707*
	[1.67]	*[1.89]*
ΔReal GDP growth rate	8.801*	14.530*
	[1.77]	*[1.82]*
ΔStock market return	0.021	0.047
	[0.57]	*[1.32]*
ΔReal exchange rate return	0.040	0.013
	[0.19]	*[0.14]*
ΔFinancial development	0.001	0.007*
	[0.50]	*[1.65]*
Bilateral trade	22.394***	25.192***
	[2.99]	*[2.93]*
Acquirer openness	0.020**	0.019*
	[2.21]	*[1.67]*
Target openness	0.030	-0.023
	[1.64]	*[-0.96]*
Year fixed effects	Yes	Yes
Country-pair fixed effects	Yes	Yes
Number of observations	10,677	8,056
R^2	0.336	0.307

Note: This table presents estimates of the following panel regressions of cross-border M&A pairs in each year:

$$(Cross - border\ M\&A\ pair)_{i,j,t} = \alpha + \beta(Election)_t + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t}$$

Column 1 presents the estimation results using subsamples of deals in which the target is from an industry with low labor intensity. Columns 2 and 3 present the estimation results using subsamples of deals in which the target is from a high labor-intensity industry. An industry is classified as a high labor-intensity industry

Table 1.7 Continued

if the labor-intensity is above the average (0.275), otherwise a low labor-intensity industry. Labor-intensity measures are obtained from Boutchkova et al. (2012). The dependent variable is cross-border M&A country-pair, defined as the number of cross-border deals in year *t* in which the acquirer is from country I and the target is from country *j* ($i \neq j$) as a percentage of the sum of the number of cross-border deals involving acquiring country *i* and target country *j* and that of domestic deals in target country *j* in year *t.* I require that a given country-pair have at least 5 cross-border deals during the entire sample period. $Election_t$ is a dummy variable set equal to one if a national election is held in the target country between 90 days prior to the end of year t and 274 days after the end of year t, and zero otherwise. $X_{i,j,t}$ is a set of country-specific factors such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, financial development, etc. Refer to Appendix A for definitions of variables in more detail. Left-party is a dummy equal to one if a country's ruling party is defined as communist, socialist, social democratic, or left-wing according to the World Bank Database of Political Institutions. Country-pair fixed effects, $\delta_{i,j}$, and year fixed effects, θ_t, are included in all regressions. Standard errors are clustered at the country-pair level. *T*-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.8 The effects of political uncertainty on cross-border M&A: Gov. spending

	Dependent variable = Cross-border M&A pair	
	Low gov. spending	High gov. spending
Variable	(1)	(2)
Election	**-0.169**	**-0.831***
	[-1.34]	***[-1.77]***
ΔGDP per capita	0.866**	-0.105
	[2.32]	*[-0.07]*
ΔReal GDP growth rate	11.610***	25.680*
	[3.85]	*[1.88]*
ΔStock market return	0.032**	0.099***
	[2.50]	*[4.73]*
ΔReal exchange rate return	0.232*	-1.327
	[1.83]	*[-1.22]*
ΔFinancial development	0.000	0.018**
	[0.25]	*[2.14]*
Bilateral trade	20.946***	18.332
	[3.12]	*[1.52]*
Acquirer openness	0.019***	0.007
	[3.63]	*[0.26]*
Target openness	0.020*	0.049
	[1.91]	*[1.12]*
Year fixed effects	Yes	Yes
Country-pair fixed effects	Yes	Yes
Number of observations	15,490	4,522
R^2	0.438	0.257

Note: This table presents estimates of the following panel regressions of cross-border M&A pairs in each year:

$$(Cross-border\ M\&A\ pair)_{i,j,t} = \alpha + \beta(Election)_t + \gamma X_{i,j,t} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,t}$$

Column 1 presents the estimation results using subsamples of deals in which the target is from an industry with low exposure to government spending. Columns 2 and 3 present the estimation results using subsamples of deals in which the target is from an industry with high exposure to government spending. An

Table 1.8 Continued

industry is classified as a high government-spending industry if the industry's input-output (I-O) code is 336414, 336611, 515100, 541700, 335110, 211000, 511110, 334418, 334220, or 322120, otherwise a low labor-intensity industry (see Belo et al., 2013). The dependent variable is cross-border M&A country-pair, defined as the number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) as a percentage of the sum of the number of cross-border deals involving acquiring country *i* and target country *j* and that of domestic deals in target country *j* in year *t*. I require that a given country-pair have at least 5 cross-border deals during the entire sample period. $Election_t$ is a dummy variable set equal to one if a national election is held in the target country between 90 days prior to the end of year t and 274 days after the end of year t, and zero otherwise. $X_{i,j,t}$ is a set of country-specific factors such as differences in real stock market returns, real exchange rate returns, GDP per capita, GDP growth rate, financial development, etc. Refer to Appendix A for definitions of variables in more detail. Country-pair fixed effects, $\delta_{i,j}$, and year fixed effects, θ_t, are included in all regressions. Standard errors are clustered at the country-pair level. *T*-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.9 Political uncertainty and cross-border takeover premium

	Dependent variable = Target CAR[-3, +3]				
	All sample	*All sample*	*All sample*	*Civil Law*	*Gov't/GDP*
Variable	(1)	(2)	(3)	(4)	(5)
Election year deal (EYD)	**-2.661*** **	**-2.958*** **	**-1.368**	4.583***	-2.140
	[-2.91]	***[3.08]***	***[-0.34]***	*[3.22]*	*[-0.44]*
EYD*Complex input structure			**-2.202**	**-21.894*** **	-1.738
			[-0.44]	***[-3.19]***	*[-0.28]*
EYD*Labor intensive			**-0.036**	14.596	5.528
			[-0.01]	*[1.04]*	*[0.81]*
EYD*Government spending			**-1.506**	16.359	**-14.493* **
			[-0.45]	*[1.23]*	***[-1.77]***
Complex input structure			-2.728**	2.469	-0.988
			[-1.96]	*[0.44]*	*[-0.34]*
Labor intensive			-2.393	1.057	-4.026
			[-1.14]	*[0.15]*	*[-0.75]*
Government spending			3.118	-2.408	8.728
			[1.35]	*[-0.25]*	*[0.98]*
Acquirer size	2.246**	2.483***	2.433***	1.700	0.473
	[2.38]	*[2.63]*	*[2.58]*	*[1.54]*	*[0.90]*
Acquirer M/B	-0.018	0.016	0.017	-0.136	-0.003
	[-0.55]	*[0.42]*	*[0.44]*	*[-0.60]*	*[-0.06]*
Acquirer leverage	2.944	1.743	1.288	-7.514	2.938
	[1.26]	*[0.69]*	*[0.40]*	*[-0.66]*	*[0.46]*
Acquirer cash	7.873**	7.317***	7.081***	5.608	0.836
	[2.32]	*[2.76]*	*[3.14]*	*[0.37]*	*[0.09]*
Target size	-3.969***	-4.326***	-4.440***	-2.175	-2.285**
	[-3.07]	*[-3.26]*	*[-3.31]*	*[-1.18]*	*[-2.34]*
Target M/B	-0.008	-0.025***	-0.024***	-0.104	0.073
	[-0.60]	*[-7.58]*	*[-8.53]*	*[-0.53]*	*[0.70]*
Target leverage	12.703***	16.256***	15.131***	35.851**	14.977**
	[3.03]	*[5.09]*	*[4.83]*	*[2.02]*	*[2.43]*

Table 1.9 Continued

Target cash	27.751***	27.903***	27.105***	41.641***	20.249**
	[11.06]	*[17.72]*	*[21.82]*	*[2.62]*	*[2.30]*
Stock deal	-1.651	-1.518	-1.544	-5.572	-2.832
	[-0.59]	*[-0.55]*	*[-0.55]*	[-0.89]	*[-1.18]*
Diversifying acquisition	-3.727*	-3.934**	-3.909**	-42.213***	-3.431
	[-1.93]	*[-2.20]*	*[-2.29]*	*[-2.79]*	*[-1.33]*
Target termination fee	1.065	0.985	1.398	2.650	1.683
	[0.65]	*[0.53]*	*[0.78]*	*[0.75]*	*[0.51]*
Tender offer	7.390**	7.394***	7.814***	11.629*	3.354*
	[2.45]	*[2.65]*	*[2.69]*	*[1.93]*	*[1.66]*
ΔStock market return	0.031	0.018	0.021	0.074	0.099
	[0.81]	*[0.45]*	*[0.49]*	*[1.12]*	*[1.63]*
ΔReal exchange rate return	14.279**	26.873***	25.427***	60.355*	17.709
	[2.14]	*[2.94]*	*[2.77]*	*[1.77]*	*[0.70]*
ΔCorporate tax rate	0.471	0.219	0.039	6.055*	2.333
	[0.53]	*[0.19]*	*[0.04]*	*[1.91]*	*[1.41]*
ΔGDP per capita	-1.309	-1.708**	-1.681*	0.673	-3.086**
	[-1.35]	*[-2.06]*	*[-1.69]*	*[0.12]*	*[-2.53]*
Heckman's lambda		-10.427***	-9.640***	-11.469	-5.482
		[-5.40]	*[-4.40]*	*[-1.11]*	*[-0.83]*
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Acquirer country fixed effects	Yes	Yes	Yes	Yes	Yes
Target country fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	877	877	877	206	501
R^2	0.241	0.254	0.258	0.475	0.257

Note: This table presents the estimates of the following regression:

$$Target\ CAR(-3,+3)_{n,m,t} = \alpha + \beta Election\ year\ deal_{m,t} + \gamma X_{n,m,t} + \vartheta Y_{n,m,t} + \delta_{n,m} + \theta_t + \varepsilon_{n,m,t}$$

Table 1.9 Continued

In the equation, *n* indexes acquirers, *m* indexes targets, and *t* indexes years. $Target\ CAR(-3,+3)_{n,m,t}$ is cumulative abnormal returns over seven days around the announcement: stock returns minus returns predicted by a market model, over the systematic seven-day event window around the announcement date. $Election\ year\ deal_{m,t}$ is a dummy variable which equals one if there is a national election in the target's country within one year after the announcement of the deal, and zero otherwise. $X_{n,m,t}$ includes a set of firm- and deal-level control variables such as firm-size, market-to-book ratio, leverage, cash, and diversifying acquisition dummy. $Y_{n,m,t}$ is a set of country-level characteristics. Acquirer-/target-country fixed effects and year fixed effects are included in the regression. Standard errors are clustered in two dimensions: acquirer and target country levels. *T*-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.10 Payment method

	Dependent variable = All-cash offer	
	Logit	
	(1)	(2)
Election year deal	**-0.249***	**-0.533*****
	[0.09]	***[0.01]***
Acquirer size		0.196***
		[0.00]
Acquirer M/B		0.005
		[0.15]
Acquirer leverage		-0.889
		[0.28]
Acquirer cash		-1.571***
		[0.00]
Target size	-0.006	-0.189**
	[0.86]	*[0.02]*
Target M/B	-0.007	-0.002
	[0.31]	*[0.60]*
Target leverage	-1.829***	-2.066***
	[0.00]	*[0.00]*
Target cash	0.047	0.361
	[0.89]	*[0.54]*
Diversifying acquisition	0.368***	0.249
	[0.00]	*[0.24]*
Target termination fee dummy	0.024	-0.001
	[0.89]	*[1.00]*
Tender offer	1.363***	1.342***
	[0.00]	*[0.00]*
ΔStock market return	0.009**	0.013**
	[0.02]	*[0.05]*
ΔReal exchange rate return	-0.515	-0.411
	[0.48]	*[0.69]*

Table 1.10 Continued

ΔCorporate tax rate	-0.234***	-0.312*
	[0.01]	*[0.06]*
ΔGDP per capita	-0.236***	-0.294***
	[0.00]	*[0.01]*
Year fixed effects	Yes	Yes
Acquirer-country fixed effects	Yes	Yes
Target-country fixed effects	Yes	Yes
Number of observations	1,723	888
Likelihood ratio	510.95	309.09
Pseudo R^2	0.257	0.294

Note: This table presents the estimates of the following logistic models:

$$All\ cash\ offer_{n,m,t} = \alpha + \beta Election\ year\ deal_{n,m,t} + \gamma X_{n,m,t} + \vartheta Y_{n,m,t} + \delta_{n,m} + \theta_t + \varepsilon_{n,m,t}$$

In the equation, *n* indexes acquirers, *m* indexes targets, and *t* indexes years. The dependent variable is a dummy variable set equal to one if the cross-border acquisition is entirely paid in cash, and zero otherwise. $Election\ year\ deal_{m,t}$ is a dummy variable which equals one if there is a national election in the target's country within one year after the announcement of the deal, and zero otherwise. $X_{n,m,t}$ includes a set of firm- and deal-level control variables such as firm-size, market-to-book ratio, leverage, cash, and diversifying acquisition dummy. $Y_{n,m,t}$ is a set of country-level characteristics. Acquirer-/target-country fixed effects and year fixed effects are included in all regressions. Standard errors are clustered at the country-pair level. P-values are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 1.11 Target's merger gain relative to acquirer's gain in cross-border M&A

	Dep. Var. = Δ$CAR_1		Dep. Var. = Δ$CAR_2	
Variable	(1)	(2)	(3)	(4)
Election year deal	**-2.28***	**-0.42**	**-1.659***	**-1.115**
	[-1.72]	***[-0.53]***	***[-1.79]***	***[-1.22]***
Election year deal*Stock deal		**-8.122****		**-1.807***
		[-2.12]		***[-1.69]***
Acquirer size	0.212	0.208	**1.204**	-1.308***
	[1.18]	*[1.30]*	***[0.43]***	*[-6.64]*
Acquirer M/B	-0.005	-0.007	-0.045	0.019
	[-0.13]	*[-0.16]*	*[-0.24]*	*[0.90]*
Target size	0.057	0.067	-1.399	1.447***
	[0.08]	*[0.09]*	*[-0.70]*	*[9.30]*
Target M/B	0.002	0.005	0.007	-0.004
	[0.38]	*[0.72]*	*[1.38]*	*[-0.93]*
Stock deal	0.177	2.235**	0.577	0.918
	[0.07]	*[2.18]*	*[0.54]*	*[0.68]*
Target termination fee	0.895	1.046	1.352	-1.037
	[0.25]	*[0.30]*	*[1.34]*	*[-1.57]*
Diversifying acquisition	2.382	2.198	-1.252**	-0.783***
	[1.23]	*[1.20]*	*[-2.14]*	*[-3.26]*
Tender offer	0.834	0.751	-0.452	0.232
	[0.32]	*[0.30]*	*[-0.47]*	*[0.21]*
ΔStock market return	-0.006	-0.005	-0.065**	0.003
	[-0.26]	*[-0.24]*	*[-1.96]*	*[0.10]*
ΔReal FX return	-1.072	-2.103	5.371*	7.988***
	[-0.56]	*[-0.61]*	*[1.87]*	*[2.93]*
ΔCorporate tax rate	-1.538	-1.411	-0.266	-0.308
	[-1.41]	*[-1.33]*	*[-0.56]*	*[-0.66]*
ΔGDP per capita	-0.618	-0.620	-0.261***	-0.112
	[-0.98]	*[-1.03]*	*[-6.76]*	*[-1.12]*
Heckman's lambda	2.068	2.340	0.785	-3.051

Table 1.11 Continued

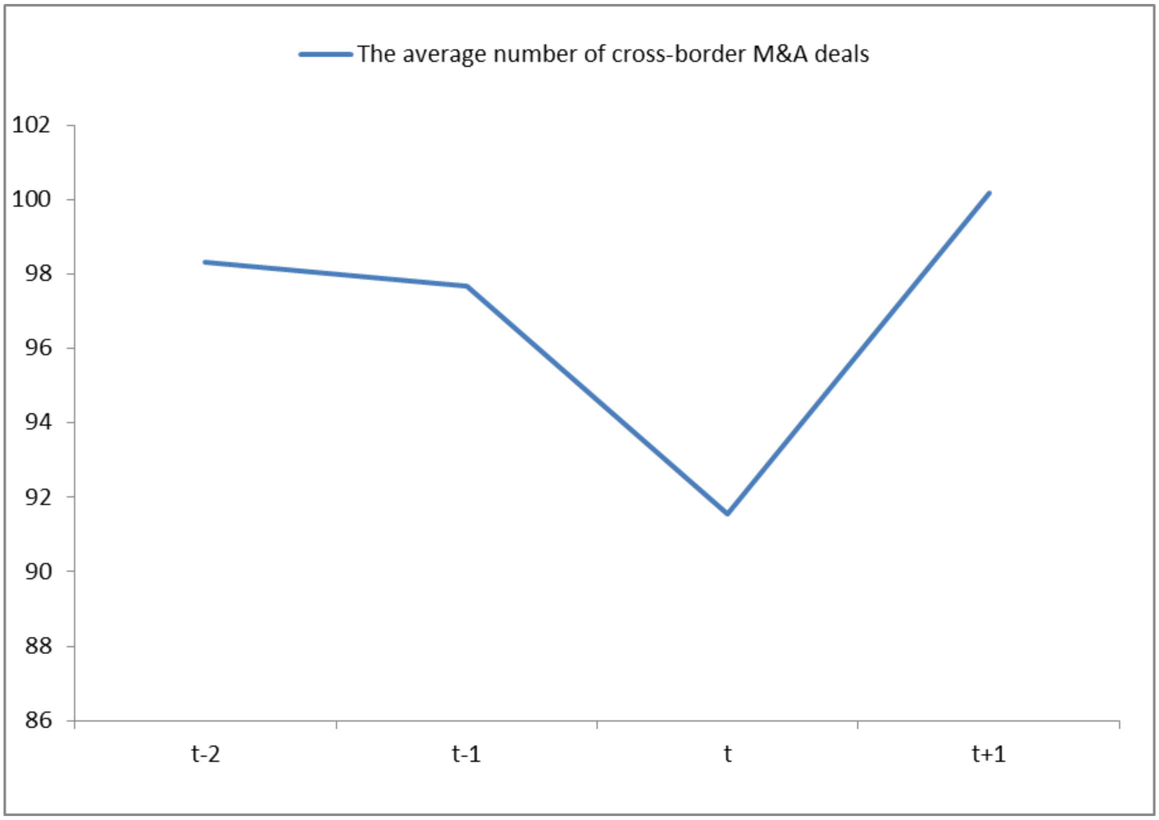
	[0.56]	*[0.61]*	*[0.57]*	*[-1.38]*
Year fixed effects	Yes	Yes	Yes	Yes
Acquirer-country fixed effects	Yes	Yes	Yes	Yes
Target-country fixed effects	Yes	Yes	Yes	Yes
Number of observations	934	934	934	934
R^2	0.342	0.344	0.126	0.206

Note: This table presents the estimates of the following regression:

$$Dependent\ variable_{n,m,t} = \alpha + \beta Election\ year\ deal_{m,t} + \gamma X_{n,m,t} + \vartheta Y_{n,m,t} + \delta_{n,m} + \theta_t + \varepsilon_{n,m,t}$$

In the equation, *n* indexes acquirers, *m* indexes targets, and *t* indexes years. In Columns 1 and 3, a dependent variable is $\Delta\$CAR\_1_{n,m,t}$,, which is defined as target $CAR(-3,+3) minus acquirer $CAR(-3,+3), all scaled by the weighted average of acquirer and target's cumulative abnormal dollar returns ($CAR(-3,+3)), in which the weights are based on acquirer and target market values (in US dollars) 50 trading days before the announcement. $\$CAR(-3,+3)$ is cumulative abnormal dollar returns over seven days around the announcement: the sum of daily abnormal dollar returns over the seven-day window, in which a daily abnormal dollar return is an abnormal percentage return multiplied by the firm's market equity in the previous day. In Columns 2 and 4, a dependent variable is $\Delta\$CAR\_2_{n,m,t}$, which is defined as target $CAR(-3,+3) minus acquirer $CAR(-3,+3), all scaled by the sum of acquirer and target market values (in US dollars) 50 trading days prior to the deal announcement. $Election\ year\ deal_{m,t}$ is a dummy variable which equals one if there is a national election in the target's country within one year after the announcement of the deal, and zero otherwise. $X_{n,m,t}$ includes a set of firm- and deal-level control variables such as firm-size, market-to-book ratio, leverage, cash, and diversifying acquisition dummy. $Y_{n,m,t}$ is a set of country-level characteristics. Acquirer-/target-country fixed effects and year fixed effects are included in the regression. Standard errors are clustered in two dimensions: acquirer and target country levels. *T*-statistics are reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Figure 1.1 Cross-border M&A activity around national elections



Note: This figure displays the average number of cross-border M&A deals around national elections. The horizontal axis denotes the years relative to the election year (t). Cross-border M&A deals in 43 countries are obtained from SDC and include all public, private, and subsidiary acquirers and targets between 1990 and 2011.

CHAPTER 2

THE INTERACTIVE INFLUENCE OF EXTERNAL AND INTERNAL GOVERNANCE ON RISK TAKING AND OUTCOMES: THE IMPORTANCE OF CEO CAREER CONCERNS

2.1 Introduction

The role of governance in corporate behavior is well-studied.[7] From investment to financing to compensation, the importance of both internal (e.g. boards) and external (e.g. stakeholders) governance is manifest.[8] Among the external factors that influence corporate behavior, the market for corporate control is a first order determinant. Low (2009) shows that it influences risk-taking, Bereskin and Cicero (2012) illustrate the effects on compensation policy, Israel (1991) and Billett (1996) highlight the effects on capital structure, Billett and Xue (2007) note its importance for payout policy.

More recently, the corporate governance literature has also begun to study the effects of CEO characteristics (e.g. Malmendier and Tate (2005, 2008)) on firm behavior. Among the individual traits considered, the CEO's age has received particular attention. Gibbons and Murphy (1992) find little variation in pay for performance across age. Dechow and Sloan (1991) find that CEOs in their final years manage discretionary investment expenditures to improve short-term earnings performance. Jenter and Lewellen (2011) show that CEOs reaching age 65 are more likely to receive a takeover bid, and that the bid premium tends to be lower. Chevalier and Ellison (1999) suggest

[7] See for example, Bebchuk and Weisbach (2010) and Coles and Li (2011) for reviews.

[8] Bertrand and Mullainathan (2003) and Giroud and Mueller (2010) conclude that poor governance associates with underinvestment. Harford, Mansi, and Maxwell (2008) and Billett, Garfinkel, and Jiang (2011) suggest the opposite. Coles, Daniel, and Naveen (2006) document two-way relationships between firm risk taking (via investment) and governance via pay structure. Bereskin and Cicero (2012) use shocks to the legal environment (the Unitrin and Wallace decisions and Delaware) to show that compensation policy is affected by external governance (takeover threats). Ovtchinnikov (2010) uses deregulatory shocks to conclude that the dynamic tradeoff theory of capital structure has empirical support.

that younger mutual fund managers wish to avoid unsystematic risk. Li, Low and Makhija (2011) find younger CEOs are more likely to enter new lines of business and exit existing lines. Serfling (2013) finds that lower risk firms *select* older CEOs and compensate them to discourage dramatic changes in risk.

Neither of these literatures (the work studying the influence of the takeover market, and the papers on CEO age effects) recognizes the influence of the other. This is particularly surprising as both the takeover market and the CEO's age are related through their influence on career concerns (e.g., Fama (1980)). Our paper seeks to fill this gap. Specifically, we study how both internal (CEO age) and external (takeover market) governance interact to influence the corporate behavior (outcome) of risk.

We begin by borrowing from Low (2009). She studies the influence of the Delaware Supreme Court's 1995 decisions that changed the previously takeover-friendly landscape to a more manager-friendly one (the Unitrin and Wallace decisions).[9] She shows that risk declined among Delaware firms in the wake of these decisions, particularly those with staggered boards. Her evidence is consistent with newly insulated managers reducing their risk in pursuit of the quiet life. We too study risk taking by CEOs (risk outcomes at firms) and how it changes around the Delaware shock. However as noted above, CEO age may affect the willingness of the manager to pursue a quiet life or something more aggressive or risky (Li, Low and Makhija (2011) and Serfling (2013)). Therefore, we ask whether the risk taking response to the Delaware court decisions varies by CEO age. Specifically, we use a differences-in-differences-in-differences (DDD) approach. We ask if risk is different for Delaware firms vs. non-Delaware firms, before vs. after 1995, for younger vs. older CEOs.

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[9] Bereskin and Cicero (2012) also argue that both the Wallace decision and its precedent, the Unitrin decision, represent shocks to the corporate landscape. In particular, while Wallace strengthened the "just say no" defense through the decision to uphold staggered boards, Unitrin expanded the circumstances when a poison pill would be allowed.

Our results are as follows. Career concerns of managers and their influence on firm risk are not uniform across CEO age, nor across takeover insulation. Younger CEOs of Delaware firms take significantly more risk, while their older counterparts take significantly less risk, after 1995. In other words, career concerns are many-faceted. Our evidence points to the influence of both the market for corporate control and the age of the CEO, with key interactions between the two, on risk taking. This is consistent with theoretical work suggesting younger CEOs are more sensitive to career concerns than older ones. But it also highlights the key role of the state of the takeover market for managerial behavior. In other words, governance is multi-faceted and both internal and external factors influence risk taking.

Confirming evidence of our main inferences is seen in cross-sectional sorts on firm-specific characteristics; CEO sensitivity of pay to risk (vega) and also sorts on board structure and corporate charter governance. Lower vega CEOs show a significantly more positive relation between young managers and firm risk post-regime change than higher vega CEOs. This suggests age mitigates paucity in the compensation incentives to take risk. Firms with better governance as measured by an absence of staggered boards and/or lower GIM index values show a more positive relation between young/Delaware and risk taking than other boards. When board structure-based governance is stronger, younger Delaware CEOs take on more risk than older ones in response to an insulating shock. Again, there is interaction between age-based and other forms of governance and we confirm causation between age and risk-taking.

We further show the interaction between external and internal governance by segmenting our samples based on whether the industry is dominated by younger or older CEOs. When at least 60% of the industry’s firms have CEOs that are older, then younger CEOs actively differentiate themselves from the older ones by taking more risk after the Delaware decisions. By contrast, when at least 40% of the industry’s firms have younger CEOs, the older ones do not reduce their risk after the shock and firms with younger

CEOs do not take significantly higher risk than older CEOs. CEO age-based career concerns are importantly influenced by the nature of the industry (at least the typical CEO's age within it).

Finally, we link risk increases among young Delaware CEOs with outcomes that benefit them. They preside over significantly positive stock performance (Fama/French alphas) over the five years following the Delaware court decisions. They also enjoy a positive relationship between risk taking and executive employment by a new firm, if they depart the firm they presided over at the time of the shocks.

Overall, our results suggest important variation in governance's effects on risk taking. While pursuit of the "quiet life" may characterize the *average* CEO's behavior in the face of greater insulation from the disciplinary effects of the takeover market (Low (2009)), the effect is nullified[10] among younger CEOs. The relationship is pronounced in industries where the majority of firms have older CEOs. And young Delaware CEOs benefit from increasing risk through both stock performance and future employment.

Our key contributions may be thought of as follows. First, our study of the interaction between takeover-related and age-based career concerns is new. It highlights that both external and internal governance matter for risk taking, and that they interact. We show that Low's (2009) evidence is incomplete – not all CEOs respond to corporate control insulating shocks the same way. Younger CEOs are dramatically different from the average in their risk taking. Serfling's (2013) evidence is also incomplete – not all younger CEOs take more risk. The nature of the industry is crucial, both in terms of its typical CEO age and in terms of its competitive stature (see below). And younger CEOs' risk-taking changes with the face of the corporate control market.

Second, our work is able to identify causal relationships between CEO age and corporate behavior by using an exogenous shock. The interaction between two facets of

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[10] And apparently reversed.

career concerns, corporate control considerations and CEO age, enables this. Thus, even if older (younger) CEOs select into firms wishing to pursue less (more) risky strategies, the Delaware court shocks should lead to *changes* in CEO risk taking (if age causes behavior) since risk-taking incentives have changed. Indeed, we find significant differences in the CEO age / firm behavior relationship among Delaware CEOs before and after the decision. Moreover, our DDD approach addresses a lingering endogeneity concern with Low (2009). Her evidence that Delaware CEOs' vegas change after the shock re-introduces the possibility of endogeneity in the measured risk outcome (because it's measured over the same window as the vega change). However, our further focus on the CEO's age, which is *not* influenced by the Delaware Court's decisions, re-establishes the validity of the exogenous shock approach to assigning causality.

Finally, we speak to the managerial labor market literature. We are the only paper to link risk increases that young Delaware CEOs presumably advocate, with outcomes that benefit them. They preside over significantly positive risk-adjusted returns over the five years following the Delaware decisions. They also enjoy a positive relationship between risk taking and appearance at a new employer.

The remainder of our research is organized as follows. Section 2.1 describes our data. We then present (in Section 2.2) methods and results on the tripartite relation between age, insulation from the takeover market, and risk taking by CEOs. Section 2.3 shows heterogeneity in this relationship, highlighting where prior studies' inferences are overturned. Section 2.4 presents evidence on the ex-post benefits (both for the CEO and shareholders) of increased risk taking after the shock. We offer conclusions in section 2.5. Finally, in Appendix C, we offer some insight into the mechanisms by which CEOs may be effecting the change in risk associated with the Delaware shock.

2.2 Data

Our sample is constructed from multiple data sources. We begin with all firms covered in the ExecuComp database during the period 1993-2000. This is necessary to obtain key compensation statistics and we also use it to obtain the age of CEOs. Motivated by prior studies (Gibbons and Murphy, 1992; Chevalier and Ellison, 1999; Holmstrom, 1999), we consider CEO age as a main measure of CEO career concerns and present results by classifying CEOs 50 years of age or younger as young CEOs.[11] We follow Core and Guay (2002) when we calculate the sensitivity of CEO's stock and option value to a 1% change in stock price (delta) and the sensitivity of CEO's option value to a 1% change in stock return volatility (vega).

Our main data source for information on the firm's state of incorporation is the RiskMetrics (formerly IRRC) database, which covers S&P1500 firms and several large firms every two or three years since 1990. Since we employ a statewide natural experiment in all our tests, it is essential to correctly identify a firm's state of incorporation. Compustat however, displays a firm's state of incorporation only for the latest available year without information on the firm's past (strategic) reincorporation and therefore potentially introduces measurement error. The use of RiskMetrics' historical state of incorporation data helps us mitigate this.[12] We obtain stock return data from CRSP to construct our primary measure of firm-risk, daily stock return volatility, on a fiscal year basis. We require a firm has at least 60 days of stock returns data during each fiscal year that we use. We further stratify firm-risk into systematic risk and unsystematic risk by using the expanded market model, with the return on the CRSP value-weighted market index as the proxy for market return, and five leads and five lags of the market

[11] Our results are robust to alternative thresholds, around the age of 50, for classification of young CEOs.

[12] Nevertheless, for firms with missing RiskMetrics state of incorporation data, we rely on Compustat. Our results are robust to using information on the state of incorporation drawn only from RiskMetrics.

index return to allow for non-synchronicity (Dimson (1979)). Systematic risk is the variance of the predicted component of stock returns, and unsystematic risk is the variance of the residual returns from the market model. We then annualize the variances and apply a natural log transformation to them.

We collect financial statement data from Compustat to measure firms' investment and financing activities as control variables.[13] We measure firms' investment activities with capital expenditures (CAPX), research and development (XRD) each deflated by the beginning-of-year total assets. We measure disinvestment with asset sales (SPPE) scaled by total assets.[14] Book leverage components are short-term debt (DLC) and long-term debt (DLTT). Acquisition expenditures are drawn from SDC. We exclude firms operating in financial (SIC Codes 6000 -6999) and utility (SIC Codes 4400-4999) industries. We also exclude all firm-year observations for which the book value of assets are negative.

Our resulting sample consists of 5,961 firm-year observations. We winsorize all variables at the1st and 99th percentile levels for each year to eliminate the effect of outliers. Our sample period starts at 1993 because ExecuComp data, which provides information on CEO age and compensation, are available from 1992 and our tests require CEO delta and vega in the preceding fiscal year end. We end our test period at 2000, five years from the Delaware decisions, because a longer post-event period could potentially be plagued by other economic events, and is also more likely to suffer from endogeneity concerns.

Table 2.1 contains descriptive statistics on our data. A few points are noteworthy. First, of all our observations, 8.6% of them have a young CEO (age 50 and under) of a Delaware firm, in the post-regime change environment. This compares with 40% of all

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[13] Our Appendix C examines corporate investment and financing behavior and its influence on risk taking.

[14] In our Appendix C tests below, we also recognize that the sale of assets can provide financing.

observations being Delaware firms after the regime change, and 14.7% of observations that have a young CEO after the regime change, but not in a Delaware firm. The difference in percentage of CEOs that are young in Delaware vs. elsewhere (post-Wallace) highlights a benefit of our use of court decisions as an exogenous shock. A CEO's age may influence their incentive to select into a company based on takeover insulation. This type of selection or endogeneity must be addressed in order to reach conclusions about causality.[15]

Other information from Table 2.1 highlights the comparability of our data with that analyzed by Low (2009). For example, total risk (natural log of annualized variance of stock returns) is 7.3 for our sample and about 6.9 for Low's. Similar comparability is seen in the breakouts of systematic and unsystematic risks. We also note that our firms are generally high growth (like Low's) with average market-to-book ratio of 2.1 and similar average book leverage (about 23%). R&D expenditures relative to assets are about 3% while CAPEX relative to assets is about 7%. Finally, CEO average and median delta and vega are close across the two studies. Delta averages 635,000 dollars in changed stock and option portfolio value per 1% change in firm's stock price. Vega average 63,000 dollars in changed option holding value per 1% change in the firm's stock return volatility. Variable definitions may be found in APPENDIX A.

Table 2.2 presents correlations among variables used in our analyses. Young CEOs are associated with higher firm risk. The correlations between YOUNG and total (systematic) [unsystematic] are as high as 0.230 (0.158) [0.233]. We also find that young CEOs tend to engage in "a busy life." Young CEOs are positively associated with both investment (capital expenditure and research and development) and restructuring activity (sale of property, plant, and equipment). While this evidence is consistent with Li, Low,

[15] Moreover, if there's selection then knowing the history of states of incorporationg also is important as firms may recognize the influence on CEO type they hire. This is an important benefit of using the historical state of incorporation data from RiskMetrics.

and Makhija (2011), it may be due to selection rather than active changes in behavior. Overall, correlation analyses suggest an important contribution from our natural experiment built around the 1995 Delaware court decisions.

2.3 Firm risk, regime change, and CEO age

As noted above, endogeneity complicates identification of a causal relationship between career concerns and risk-taking. Also, the implicit view of CEO age as sole determinant of career concerns' effects on firm behavior ignores the important and potentially interrelated influence of the market for corporate control on career concerns. Our empirical design overcomes both these issues.

2.3.1 Methodology

We assess the influence of CEO career concerns on firm risk via a differences-in-differences-in-differences (DDD) method. Bertrand and Mullainathan (2003) use DD to test the "quiet life" hypothesis, and Low (2009) explains risk changes due to the Wallace decision this way. We add another "layer" of differences in our estimation: CEO age. We offer two approaches to estimating DDD: differences in means after propensity-score-matching peers to treatment firms, and a regression framework.

2.3.1.1 Differences in means

We conduct DDD tests using a propensity score-matching procedure *for two samples – young and old Delaware CEOs.* Our design follows Armstrong et al. (2010) and is implemented in several steps. First, we estimate two logits (separately) explaining the incidence of Delaware firms with young (old) CEOs vs. non-Delaware firms.[16] Second, we use the coefficients from the logit(s) to construct a propensity score match (PSM) for each Delaware firm and each non-Delaware firm. These are fitted values from the logit using the individual firm's regressor values. We then select one matching firm from the non-Delaware sample, that has the closest propensity score to our Delaware firm, as our peer firm. We do this separately for each "Delaware-young" and each "Delaware-old" treatment firm. For this matching we also require peer firms to come from the same industry as the treatment firm. Our matching firms appear to be well-chosen under standard PSM criteria.[17]

Once we have our peers, we compute average annual risk changes around the Delaware court decisions, separately for each treatment group (young and old Delaware CEO firms) and for each (of the two) peer group(s). Specifically, average pre-event risk is calculated over the available years preceding 1995 (1993 is the earliest we start – see above). Average post-event risk is calculated over the five years following the Delaware decisions (1996-2000). The change in risk is the post-event average minus the pre-event average. We then calculate the difference between the risk change for young Delaware CEOs and their PSM peers, and we do the same for the old Delaware CEO treatment

[16] We include Size, MB, ROA, Sales Growth, Firm Focus, Number of Segments, and Firm Age as regressors.

[17] To assess covariate balance, we conduct t-tests of the difference in means for each variable, between each treatment group and its own matched sample. In untabulated results, we find that only 2 out of the 7 t-tests (Size, ROA) are significant between Delaware young firms and its matched sample. We also find that only 1 out of 7 t-tests (Sales Growth) is significant between Delaware old firms and the matched sample.

group. These are DD tests. They measure for each treatment group (Delaware old and Delaware young), whether the Delaware firms associate with different risk changes over the Delaware court decisions than their non-Delaware peer firms. Last, we test for differences in these DD results across the young and old subsamples. In other words, the triple-difference compares the difference-in-differences treatment effect in one group (young CEOs) to the difference-in-differences treatment effect of another group (older CEOs).

A propensity score-matching approach does not assume that the relations between dependent variables and independent variables (including control variables) are homogenous across the treatment and control samples (Armstrong et al., 2010). Nor does it assume that the relations between dependent variables and independent variables are linear. However, propensity-score matching is not without possible tradeoffs. It assumes that any factors that are not included as determinants of the outcomes in the first-stage propensity model, are random between the treatment and control groups and do not affect the outcomes of interest. In our context, this assumption is violated *only if* firms that are expected to change risk choose their state of incorporation and simultaneously decide what type of CEO to hire (i.e., young versus old). Given that the Delaware court decisions are largely exogenous (i.e. unanticipated), we believe that this possibility is remote.

<u>2.3.1.2 A regression framework</u>

The alternative approach to DD tests in means is a regression framework. This allows for controls explaining differences in the dependent variable that may not be appropriate to include in a PSM. We therefore estimate the following model:

$$y_{jt} = \alpha_t + \gamma_s + \delta_i + \beta \boldsymbol{X}_{jt} + \theta DEL_j * AFT_t + \varphi DEL_J * AFT_t * YOUNG_{jt} + \varepsilon_{jt} \tag{2.1}$$

In the equation, j indexes firms, t indexes years, i indexes industries, and s indexes states. The dependent variable y_{it} is firm risk, measured as the natural log of annualized variance of daily stock returns over the fiscal year (or in further tests, the systematic or unsystematic portion of risk). DEL is a dummy variable for Delaware-incorporated firms. AFT is a dummy variable for years after 1995 (the Delaware court decisions). YOUNG is a dummy equal to one if the CEO is 50 or younger. The regression is run over our full sample period 1993 through 2000. We cluster at the state level (see Yun (2009), Bertrand, Duflo, and Mullainathan (2004)). We eschew years of analysis beyond 2000 because of possible macro changes in much later years following the regime change. $\boldsymbol{X}$ includes our set of controls, typical to the literatures examining CEO incentives and firm behaviors and risk-taking. Again, variable definitions are provided in APPENDIX A.

In equation (2.1), the coefficients θ and ϕ are of primary interest. θ measures the change in risk due to the regime shift, among older CEOs of Delaware-incorporated firms. Under the "quiet life" hypothesis, we expect the coefficient on this to be significantly negative. Increased insulation from the market for corporate control (due to the Delaware court decisions) reduces the opportunity cost of managerial shirking.

By contrast, the coefficient ϕ is expected to be significantly positive. Following Holmstrom and Ricart i Costa (1986) and Hirshleifer (1993), younger managers are more sensitive to the effects of reputation on their perceived value and they seek to differentiate themselves from older managers.

Among Delaware CEOs, avoidance of the "quiet life" is one way to make this difference evident.

2.3.2 Results

2.3.2.1 Differences in means

Our results from the DDD in means tests are presented in Table 2.3. Panel A shows the effects of the Delaware decisions treatment on risk taking for young CEOs.

Among them in Delaware firms, risk increases post-shock. For total, systematic and unsystematic risk, the mean differences in natural logs of volatility (from before to after the shocks) are .366, .577, and .327 respectively. These compare with mean changes among their PSM peers of .232, .435, and .207 respectively. The DD estimator (difference in time-series changes between treatment and non-treatment firms) is significant at the 10% level for all three risk measures. The shock to takeover-related career concerns (the increase in insulation due to the Delaware court decisions) causes *more* risk-taking by young Delaware CEOs.

Next, Panel B documents the effects of Wallace for *older* Delaware CEOs. The change in risk for them is significantly *smaller* than the change in risk for their PSM peers. The evidence is consistent with Low (2009), suggesting the Delaware court decisions' insulation encourages pursuit of "the quiet life". However, the results in Panel A dispute this for a key subsample.

Finally, Panel C formally tests whether the apparent difference between younger and older Delaware CEOs' risk-taking response to the Delaware court decisions is significant. It is. The DDD estimates are all significantly positive. We conclude that age- and takeover-based career concern changes *cause* risk changes; it is not simply selection. Moreover, our results highlight an important interaction between two different facets of career concerns (takeover-related and age-based).

We assess the robustness of our DDD in means results to two changes. First, we "lock" our classification of young Delaware CEOs on the basis of their age in 1995 (the year of the Delaware court decisions). This has very little effect on our results[18] and does not change our conclusions. Second, we restrict our analysis period to 1993-1998. This focuses attention on the very few years around the decision and limits possible macro

[18] If anything, they appear slightly stronger than in Table 2.3.

changes that might be more likely over a wider window. Again, there is very little effect on our results and our conclusions do not change.

2.3.2.2 Regression

Table 2.4 presents results from estimating specification (2.1). There are three models in the table. The first assesses total risk (natural log of daily variance of stock returns) as the dependent variable. The next two columns break out risk into systematic and unsystematic components respectively.

Beginning with model one, our results are consistent with expectations. The coefficient θ, measuring the influence of the regime change on firm risk among older Delaware CEO firms, is significantly negative. There is evidence to support the "quiet life" hypothesis. This result is not new. Low (2009) highlights it and this result is very much like hers.

However, the model one results also show evidence consistent with age-based career concerns being paramount among younger CEOs, in a way that influences their risk taking response to changes in insulation. The coefficient ϕ is significantly positive. Compared to older CEOs, younger ones in Delaware firms after the regime change, preside over significantly higher risk. Younger, recently more insulated CEOs behave as if there is a new opportunity for them to differentiate themselves from local peers. Importantly, the sum of the coefficients θ and ϕ is significantly positive, with a p-value of 1%. Not only do younger (Delaware) CEOs preside over significantly different firm risk after the Delaware court decisions, but they even take on significantly more risk than they did prior to the decision. This indicates that age-based career concerns *are not uniform* across variations in other determinants of career concerns. Work that does not recognize this is potentially contaminated by endogeneity and our use of the court decisions as a natural experiment breaks that endogenous link. Age-based career concerns vary with the state of the takeover market. This interaction is key to our ability to make statements about causality. The shock to the state of the takeover market in Delaware breaks the endogenous link between multiple facets of CEO career concerns (age- and takeover-related). Our results strongly suggest that younger Delaware CEOs view the regime change as an opportunity to distinguish themselves that was previously unavailable, through their risk taking.

The coefficients on several control variables are also noteworthy. The variable "YOUNG" (by itself) also carries a significantly positive coefficient, as does the variable "AFT" (by itself). The former is consistent with either selection or causality, wherein younger CEOs are associated with greater firm risk. The latter is consistent with a general rise in firm risk from the early 1990s to the late 1990s (e.g. Schwert (2002)). Other control variables carry coefficients very similar to those found in Low (2009).

The results from disaggregating total risk into systematic and unsystematic proportions are shown in models 2 and 3. For systematic risk changes, we see that they are marginally different among older Delaware CEOs after the regime shift with a p-value of 0.1, and the coefficient (θ) is negative. Still, younger Delaware CEOs preside over more systematic risk than their older counterparts after the regime shift, with ϕ significantly positive. The sum of θ and ϕ is also significantly positive – younger Delaware CEOs actually raise their systematic risk after the regime change. The results for unsystematic risk are much closer to those for total risk, with both θ and ϕ significantly negative and positive respectively, and coefficients very close in magnitude to those in the total risk regressions. The takeaway from this is that older Delaware CEOs appear to react to the regime change by adjusting their unsystematic risk more than systematic risk while younger Delaware CEOs adjust both.

2.4 Heterogeneity in the treatment effect

The results thus far suggest that two facets of CEO career concerns (age and takeover threats) are key determinants of firm risk. Here, we further segment our analysis by likely candidates for the importance of career concerns to risk taking. We present results for two levels of segmentation; firm-specific and industry-based. Table 2.5, Panel A analyzes the influence of age and takeover related career concerns on risk for sub-samples that differentiate by other firm-level incentive determinants. We specifically create sub-samples of staggered and non-staggered board firms, high and low CEO vega firms, and high/low GIM (or G-Index, see Gompers, Ishii and Metrick (2003)) firms. Table 2.5, Panel B repeats the analysis but segments on industry-level factors likely to influence the risk taking response to age and takeover related career concerns. Specifically, we disaggregate our analysis based on the typical age of CEOs within the industry and we also analyze the effects of industry competitiveness on our results. We continue with equation (2.1) for our regression specification. The key coefficients of interest are θ and ϕ.

2.4.1 Firm-specific heterogeneity in the treatment effect

We first segment our analysis and tests according to whether the firm has a staggered board or not. Low (2009) emphasizes the importance of Wallace by highlighting the stronger deterrent effect on risk taking among firms with staggered boards. In other words, the Wallace decision made it more difficult to remove a poison pill (which was allowed under Unitrin) through board replacement, simply because of the time it takes to do so. Given that it takes time to dramatically change the structure of a board when it is staggered, there may be differences in the risk-taking changes that younger CEOs (who may wish to do so) can accomplish after the shock.

We first confirm Low's (2009) result – the risk deterring effect of greater takeover insulation (negative coefficient θ) – is restricted to the staggered board sub-sample. Turning to the interactive effects of CEO age and takeover insulation on risk, our tests reveal (at best) weak differences between groups of staggered board firms or not. When the sample is comprised of staggered board firms, the coefficient ϕ (0.141) is significantly positive at the 10% level. Staggered board firms with young CEOs see risk increase marginally more after the shock. When the sample is of firms without staggered boards, the coefficient ϕ (0.260) is significantly positive at the 1% level and nearly twice the economic magnitude than in the staggered board sample. However, the F-test for differences across the samples' coefficients is not significant (p-value = 0.29). We infer that without a staggered board, younger CEOs are less encumbered if they attempt to increase risk in response to the regime shift. When there is a staggered board, some younger CEOs may find it more difficult to significantly alter risk even if they wish to.

We next analyze whether CEO vega, measuring pay-based incentives, influences the relation between takeover and age-based career concerns and risk. First, the coefficient θ is significantly negative among low vega firms, but insignificant among high vega firms. When pay-based incentives to take risk are low, older CEOs respond to the insulating effects of Delaware's court decisions by taking even less risk. By contrast, younger CEOs *increase* their risk in response to the shock. The coefficient ϕ is significantly positive in the low vega (first tercile) sub-sample, as is the sum of θ and ϕ. Since low vega suggests weak risk-taking incentives from compensation, this suggests substitution of incentives due to career concerns for incentives due to pay. Precisely where incentives are low for one reason, career concerns provide an alternative incentive to take risk. Moreover, this is reliably different from the value of ϕ in the high vega sample.

Finally, we examine two sub-samples based on the GIM proxy for good/poor governance associated with the corporate charter. Specifically, we classify low (high) GIM firms as those in the lowest (highest) tercile of G-Index values in each year.[19] When GIM is low, governance is generally viewed as shareholder-friendly – there are fewer

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[19] In years without G-Index values, we follow Gompers, Ishii, and Metrick (2003) and use the prior available value. Also, our results are robust if we lock each firm's GIM as its 1995 value.

anti-takeover provisions. For this sub-sample, the coefficient ϕ is significantly positive with 90% confidence. When GIM is high (more anti-takeover oriented), the coefficient ϕ is negative but insignificant. The difference between the two ϕ's is significant (p=.07). Younger CEOs in good governance firms take on more risk in response to the shock than their counterparts in poor governance firms, and they take on more risk than their older counterparts in good governance firms. Also among poorer governance firms, the coefficient θ is significantly negative. This suggests that poor governance exacerbates quiet life incentives when there is an insulating shock and CEOs are older.

Overall, firm characteristics appear to influence the causal effects of age and takeover related career concerns on risk taking. These results also highlight the importance of recognizing the interaction between different governance elements. However, firms do not operate in a vacuum and industry characteristics may matter as well. We now turn to industry-level heterogeneity analysis.

2.4.2 Industry-level heterogeneity in treatment effect

We examine two types of industry variation that may influence the relation between risk taking and age and takeover related career concerns. First, we study whether industry concentration (HHI) influences the risk-career concerns relation. We classify firms as having high or low HHI as follows. We begin by cutting the Compustat universe into terciles of HHI. If the industry of one of our sample firms has an HHI that falls in the third tercile based on the Compustat ranking, it is classified as a high concentration (low competition) industry's firm. We place firms that belong to industries with HHI in the lowest Compustat tercile of HHIs, into the low concentration (high competition) group.[20] Since our own sample is not the entire Compustat universe (it's based on the Execucomp sample), we do not necessarily need to have similar numbers of observations in the low

[20] Our main results (the insignificant coefficient ϕ in the low competition sub-sample) is robust to grouping firms into competitiveness samples based on whether the industry HHI is above/below the median on Compustat.

and high concentration sub-samples. Indeed, only 625 of our sample observations are placed in the high concentration tercile, while 4,018 sample observations are placed in the low concentration tercile.

Running the equation (2.1) regression (separately) on the low and high concentration sub-samples provides interesting results. When concentration is low (competition is high), our typical result prevails; older CEOs pursue the quiet life more after the shock (θ is significantly negative) while younger CEOs increase their risk, both relative to older CEOs (ϕ is significantly positive) and over their prior level (the sum of θ and ϕ is significantly positive). Greater competition does not deter older CEOs from pursuing the quiet life, perhaps because they feel so insulated by the shocks (and also age related career concerns are less pronounced). On the other hand, more competition mitigates the incentive of younger CEOs to pursue the quiet life (driven by the shock) perhaps because they recognize the potential cost of eschewing risk in the face of stiff competition – it may lead to poorer industry-adjusted performance and worse career prospects.

When concentration is high (competition is low), the inferences are somewhat different. Though we still see quiet life incentives prevailing among older CEOs (θ is negative), we see similar behavior among younger CEOs (ϕ is insignificant). The opportunity cost of risk avoidance among younger CEOs is lower if their insulation from the takeover market is not compromised by a competitive product market.

We next segment our sample based on the proportion of young CEOs in the sample firm's industry. If there's a significant portion of firms with young CEOs in an industry, even the older CEOs may experience age related career concerns (due to their peers' actions). We classify industries with at least 40% of firms led by young CEOs as "young CEO industries", and conduct our tests separately on this sample and its corollary.[21] We find that older CEOs indeed react differently to the insulating shock when there's a significant proportion of young CEOs (40% or more) in the industry. The older CEOs do not reduce their risk ex-post. Thus age-related career concerns change the inferences from Low (2009) that CEOs reduce risk in response to insulating shocks, but this happens both directly and indirectly. Younger CEOs behave differently than older ones, and older CEOs behave differently when the industry is comprised of at least 40% young CEOs.

Our other results remain. Younger CEOs increase risk more than older CEOs in both sub-samples. Older CEOs significantly reduce risk in response to the insulating shock (just as Low (2009) finds) in the sub-sample where less than 40% of the industry's CEOs are young. Overall, our results reinforce the interactive nature of multi-layered governance effects.

2.4.3 Revisiting the prior literatures on age and takeover related career concerns and risk taking

Our evidence calls into question inferences regarding the effects of both age and corporate control considerations on risk taking by CEOs. Low's (2009) evidence regarding the effects on risk of shocks to insulation (from corporate control considerations), does not hold for younger CEOs. Our results also appear inconsistent with Serfling's (2013) inferences regarding age's effect on risk, when we sample on less competitive industries, or poorly governed (high GIM) firms, or firms where the CEO has other strong risk-taking incentives (pay – the high vega sample). To confirm these differences, especially those that question Serfling (2013) who does not examine exogenous shocks, we turn to panel regressions.

[21] We require that a majority of CEOs in the industry be young for the industry to be so classified, the sample size shrinks precipitously (to N=275 vs. N=1,042 in our current categorization). Nevertheless, our main inferences persist.

Table 2.6 presents regression results examining the influence of CEO age on risk (as Serfling (2013) does). However, we examine this relation for sub-samples of high and low GIM firms, and also for sub-samples of high and low competition industries (firms that belong to them). The coefficient on CEO age should be negative (younger CEOs take more risk) if Serfling's (2013) inferences persist. Indeed we see this for some sub-samples; strong governance (low GIM) firms and firms from more competitive industries. However, the effects of age on risk taking are substantially weaker and indeed insignificant among poorly governed (high GIM) firms and among firms that operate in less competitive industries. Not only is the statistical significance smaller, the coefficients indicate much smaller economic effects. Overall, these results confirm the importance of recognizing multi-faceted elements of governance (internal – age, and external – corporate control) for risk taking.

2.5 Risk taking by young CEOs and ex-post benefits

Our focus on career concerns' effects on risk-taking presumes that CEOs care about their future career prospects. Here we directly examine the tripartite relation between CEO risk taking, age and future career outcomes. In particular, we seek to answer the following question that the literature on career concerns and differential CEO behavior has not yet answered: do these career-motivated differential actions benefit either the CEO or the firm's shareholders or both? This section addresses the question of benefits to differentiation and it represents one of our key contributions to the literature on career concerns.

We address ex-post benefits to young CEO differentiation in two ways. First, we investigate the next step on young Delaware CEOs' career paths when they departed the ex-ante firm after the shocks. In this analysis, we focus on the influence of firm risk prior to the young CEO's departure, on their ex-post employment status. In our second analysis, we investigate the long-run stock performance of Delaware firms in the five years following the court decisions.

2.5.1 Subsequent employment

Our analysis of ex-post employment situations focuses on young Delaware CEOs who depart their employer post-shocks. In Panel A of Table 2.7 we describe their stated reasons for departure.[22]

There are 79 CEOs who depart their pre-shocks firm. By far, the most common reason given is resignation with nearly 40% of the observations. Forced departure and pursuit of other opportunities are the next most common reasons with roughly 25% each. Least common are the control and retire reasons, each near 5%.

What's more important in our view is the employment outcome post-departure. Panel B shows that nearly 60% gain new employment as an executive at a different firm. Moreover, of the 40% that don't, some are due to retirements. In other words, a clear majority of young CEOs that depart, subsequently find employment as an executive. Moreover, for those that are re-employed as executives, over 60% of them are CEOs again.

We further break down the ex-post employment situation by reason for departure (Panel B). For those that became CEOs at the new firm, 21% were forced out of the old firm and 21% pursued other opportunities. However, the most common departure reason among those CEOs that obtain CEO positions at new firms is resignation. We also see that the most common departure reason given for those that don't take a CEO position at the new firm (but do take an executive position) is to pursue other opportunities.

To ascertain the influence of risk-taking (at the old firm) on subsequent employment outcomes of young Delaware CEOs post-shock, we run logit regressions (Panel C). The dependent variable equals one if they find employment as an executive

[22] We use the methodology to Fee and Hadlock (2004) to classify "reasons for departure".

ex-post. The key regressor is risk, measured as the average of annual measures of stock volatility over the fiscal years between 1995 and the CEO's departure of the "old" firm. Control variables include reasons for departure dummies and firm performance.

The coefficient on firm risk is significantly positive with a p-value below .01. When a young Delaware CEO departs their firm, their risk-taking while at the "old" firm positively influences the chances of new employment as an executive later on. This is a personal benefit to risk-taking by young Delaware CEOs.

2.5.2 Long-run stock performance

We assess firm performance over the five years following the shocks using the Fama and French (1993) methodology. We form separate portfolios for young and older Delaware CEO firms. Portfolio returns are calculated using both value-weighting and equal-weighting and we report these results separately in Table 2.8. Panel A employs the standard 3-factor Fama/French model. Panel B reports results from the 4-factor model that includes a momentum factor. We report the intercepts and associated t-statistics.

The results in Panel A indicate outperformance by firms with younger Delaware CEOs in the five years following 1995 decision. Under value-weighting, the outperformance is by 71 bps per month, significantly different from zero with 90% confidence. Under equal-weighting, outperformance is nearly 2% per month, significant at the 1% level. Younger Delaware CEOs, who on average increase risk post-1995 (see Tables 2.3 and 2.4), outperform the benchmark during that same time window. The career concerns-encouraged risk taking benefits the firm's shareholders through superior risk-adjusted performance.

Panel B presents similar results, though a bit weaker. The intercept in the value-weighted portfolios regression is only 50 bps for younger Delaware CEOs, and not statistically significant. However, the equal-weighted portfolio regression results continue to indicate significant risk-adjusted outperformance.

One of the concerns with the results in Table 2.8 is that all intercepts appear positive with most of them significant. However, this appears to be due to sample selection. For the full CRSP sample over the same time period (five years post-1995), we re-run the Fama/French regressions and find negligible intercepts (negative under equal-weighting) with t-statistics less than one. Overall, the greater risk-taking by younger Delaware CEOs post-1995 appears to be rewarded.

2.6 Conclusions

We study the interactive influence of takeover related and age-based career concerns on firm/CEO risk taking. We use the Unitrin and Wallace decisions of the Delaware Supreme Court (in 1995) that effectively insulated CEOs from the corporate takeover market, as an exogenous shock. We also exploit the exogeneity of CEO age at the time of the shock. Our results highlight the importance of recognizing multi-layered governance effects on risk taking.

We find that the Delaware shock caused reduced risk-taking by older CEOs but increased risk-taking by younger CEOs. These results are pronounced among low vega CEOs and firms with good governance. They are less evident when industry competitiveness is low and when a significant proportion of the industry's CEOs are young. This heterogeneity further argues for recognizing the multi-layered nature of governance and its influence on risk-taking.

If career concerns drive younger managers to get noticed, we would expect those that act upon such concerns to be rewarded ex-post. Indeed we find that young Delaware CEOs who leave their "old" firm post-Wallace very often appear at another firm as an executive and often as a CEO. Moreover, the higher their risk-taking was, the greater the likelihood that they take an executive position at a new firm. Finally, younger Delaware CEOs outperform the benchmark (Fama/French) in the five years following 1995 (the shock year). Career concern-motivated risk-taking appears to benefit shareholders too.

Overall, our results are the first to suggest important interactions between takeover related and age-based career concerns. The effect of takeover insulation on risk taking is influenced by CEO age, while younger CEOs' risk taking varies by the level of takeover likelihood. Finally, we offer evidence that more strongly suggests CEOs cause risk taking, rather than the relationship reflecting selection.

Table 2.1 Descriptive statistics

Variable	Obs.	Mean	25th	Median	75th	Std. dev.
DEL*AFT*YOUNG	5,961	0.086	0.000	0.000	0.000	0.280
DEL*AFT	5,961	0.400	0.000	0.000	1.000	0.490
AFT*YOUNG	5,961	0.147	0.000	0.000	0.000	0.355
DEL*YOUNG	5,961	0.125	0.000	0.000	0.000	0.331
DEL	5,961	0.575	0.000	1.000	1.000	0.494
YOUNG	5,961	0.211	0.000	0.000	0.000	0.408
CEO age	5,961	56.232	51.000	56.000	61.000	7.661
Firm age	5,961	24.767	9.000	22.000	32.000	19.265
Total risk	5,961	7.285	6.655	7.236	7.885	0.857
Systematic risk	5,961	5.155	4.461	5.108	5.816	0.994
Unsystematic risk	5,961	7.123	6.479	7.085	7.730	0.874
Size	5,961	6.954	5.865	6.812	7.946	1.510
MB	5,961	2.108	1.282	1.658	2.371	1.395
ROA	5,961	0.049	0.025	0.058	0.093	0.094
CEO delta ($mil)	5,961	0.635	0.061	0.154	0.451	1.776
CEO vega ($mil)	5,961	0.063	0.008	0.026	0.064	0.115
Sales growth	5,961	0.131	0.016	0.089	0.197	0.258
Firm focus	5,961	0.640	0.333	0.603	1.000	0.339
Number of segments	5,961	3.654	1.000	2.000	4.000	3.874
Capital expenditure	5,961	0.074	0.036	0.059	0.091	0.058
Sale of PPE	5,961	0.004	0.000	0.000	0.002	0.012
R&D	5,961	0.033	0.000	0.003	0.040	0.059
Book leverage	5,961	0.231	0.097	0.222	0.337	0.168
Total acquisition exp.	5,961	101.229	0.000	0.000	0.000	2270.900

Note: This table presents descriptive statistics for the sample. The data set comprises 5,961 firm-year observations for all firms covered in ExecuComp during the period 1993-2000 with non-missing values for all required variables. CEO age is CEO's age during the sample year (from ExecuComp). DEL is a dummy equal to one if the firm is incorporated in Delaware, in which state incorporation data comes from RiskMetrics. For firms missing state of incorporation data on RiskMetrics, we use Compustat state of incorporation data. AFT is a dummy equal to one if the firm-year observation is during the period 1996-2000. YOUNG is a dummy variable equal to one if the CEO is 50 years old or younger as of the sample year. Firm age is calculated as the sample year minus the year in which the firm was first listed on CRSP. Total risk is the natural logarithm of the annualized variance of the firm's daily stock returns during the fiscal year. Systematic risk is the natural logarithm of the annualized variance of the predicted component of stock returns using the expanded market model as described in Section II. Unsystematic risk is the natural logarithm of the annualized variance of the residual returns from the market model. Size is the

Table 2.1 Continued

natural logarithm of total assets (AT) measured in the prior fiscal year end. MB is the ratio of the market value (AT – CEQ + PRCC_F*CSHO) to book value of assets (AT) in the prior fiscal year end. ROA is the ratio of net income (IB) to total assets (AT) in the prior fiscal year end. CEO delta is computed as the sensitivity of CEO's stock and option value to a 1% change in stock price in the prior fiscal year end. CEO vega is computed as the sensitivity of CEO's option value to a 1% change in stock return volatility in the prior fiscal year end. Sales growth is computed as the current year's sales (SALE) minus the prior year's sales, all scaled by the prior year's sales. Firm focus is the segment sales based Herfindahl index computed as the sum of squared segment sales-total segment sales ratios. Number of segments is the firm's number of business segments from the Compustat Segment Database. Capital Expenditure is the ratio of capital expenditure (CAPX) to total assets (AT). Sale of PPE is the ratio of sale of property, plant, and equipment (SPPE) to total assets (AT). R&D is the ratio of research and development expenditure (XRD) to total assets (AT). If SPPE or R&D is missing, it is set equal to zero. Book leverage is the sum of the firm's long-term debt (DLTT) and short-term debt (DLC), all scaled by total assets (AT). Total acquisition exp. is the sum of acquisitions deal values (from SDC) during the fiscal year.

Table 2.2 Correlation matrix

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1	DEL	1.00																			
2	YOUNG	0.02	1.00																		
3	CEO age	0.00	-0.71	1.00																	
4	Firm age	-0.12	-0.23	0.23	1.00																
5	Total risk	0.04	0.23	-0.23	-0.40	1.00															
6	Systematic risk	0.04	0.16	-0.18	-0.20	0.77	1.00														
7	Unsystematic risk	0.04	0.23	-0.23	-0.42	0.99	0.69	1.00													
8	Size	0.02	-0.22	0.17	0.50	-0.39	-0.05	-0.44	1.00												
9	MB	0.01	0.07	-0.10	-0.12	0.19	0.32	0.15	-0.10	1.00											
10	ROA	-0.08	-0.08	0.09	0.10	-0.28	-0.14	-0.30	0.13	0.26	1.00										
11	CEO delta ($mil)	0.04	0.00	0.01	0.01	0.04	0.16	0.01	0.21	0.35	0.16	1.00									
12	CEO vega ($mil)	0.04	-0.05	0.02	0.17	0.02	0.16	-0.01	0.45	0.15	0.06	0.32	1.00								
13	Sales growth	0.03	0.09	-0.11	-0.20	0.12	0.15	0.10	-0.12	0.25	0.01	0.06	0.02	1.00							
14	Firm focus	-0.01	0.10	-0.10	-0.31	-0.09	-0.17	-0.07	-0.29	0.04	0.01	-0.06	-0.23	0.11	1.00						
15	No. of segments	0.01	-0.07	0.08	0.26	0.12	0.16	0.11	0.29	-0.05	-0.01	0.09	0.27	-0.09	-0.77	1.00					
16	Capital exp.	0.04	0.06	-0.05	-0.11	0.02	0.03	0.01	-0.03	0.07	0.09	0.03	-0.04	0.08	0.12	-0.12	1.00				
17	Sale of PPE	0.00	0.04	-0.02	-0.01	0.06	0.00	0.07	-0.02	-0.09	-0.10	-0.03	-0.04	-0.05	-0.04	0.05	0.20	1.00			
18	R&D	0.06	0.13	-0.17	-0.18	0.35	0.36	0.33	-0.22	0.34	-0.24	0.00	0.02	0.09	0.11	-0.09	-0.08	-0.07	1.00		
19	Book leverage	0.04	-0.06	0.03	0.08	-0.06	-0.10	-0.05	0.26	-0.29	-0.21	-0.10	0.07	-0.06	-0.15	0.16	0.05	0.12	-0.31	1.00	
20	Total acq. exp.	0.02	0.03	-0.02	-0.03	0.02	0.04	0.01	0.04	0.11	0.02	0.17	0.11	0.04	-0.01	0.03	-0.01	-0.01	0.01	0.00	1.00

Table 2.2 Continued

Note: The table reports the pairwise correlations between main regression variables. The data set comprises 5,961 firm-year observations for all firms covered in ExecuComp during the period 1993-2000 with non-missing values for all required variables. CEO age is CEO's age during the sample year (from ExecuComp). DEL is a dummy equal to one if the firm is incorporated in Delaware, in which state incorporate data comes from RiskMetrics. For firms missing state of incorporation data on RiskMetrics, we use Compustat state of incorporation data. AFT is a dummy equal to one if the firm-year observation is during the period 1996-2000. YOUNG is a dummy variable equal to one if the CEO is 50 years old or younger as of the sample year. Firm age is calculated as the sample year minus the year in which the firm was first listed on CRSP. Total risk is the natural logarithm of the annualized variance of the firm's daily stock returns during the fiscal year. Systematic risk is the natural logarithm of the annualized variance of the predicted component of stock returns using the expanded market model as described in Section II. Unsystematic risk is the natural logarithm of the annualized variance of the residual returns from the market model. Size is the natural logarithm of total assets (AT) measured in the prior fiscal year end. MB is the ratio of the market value (AT – CEQ + PRCC_F*CSHO) to book value of assets (AT) in the prior fiscal year end. ROA is the ratio of net income (IB) to total assets (AT) in the prior fiscal year end. CEO delta is computed as the sensitivity of CEO's stock and option value to a 1% change in stock price in the prior fiscal year end. CEO vega is computed as the sensitivity of CEO's option value to a 1% change in stock return volatility in the prior fiscal year end. Sales growth is computed as the current year's sales (SALE) minus the prior year's sales, all scaled by the prior year's sales. Firm focus is the segment sales based Herfindahl index computed as the sum of squared segment sales-total segment sales ratios. Number of segments is the firm's number of business segments from the Compustat Segment Database. Capital expenditure is the ratio of capital expenditure (CAPX) to total assets (AT). Sale of PPE is the ratio of sale of property, plant, and equipment (SPPE) to total assets (AT). R&D is the ratio of research and development expenditure (XRD) to total assets (AT). If SPPE or R&D is missing, it is set equal to zero. Book leverage is the sum of the firm's long-term debt (DLTT) and short-term debt (DLC), all scaled by total assets (AT). Total acquisition exp. is the sum of acquisitions deal values (from SDC) during the fiscal year. *p*-values in parentheses represent the significance level of each correlation coefficient. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 2.3 CEO career concerns and firm risk: A propensity score matching framework

Panel A Delaware young CEOs vs. Non-Delaware CEOs

Variable	Delaware young CEOs (No. of obs. = 122)	Non-Delaware matching CEOs	Differences-in-differences
ΔTotal risk	0.366***	0.232***	0.134*
t-stat.	*[8.93]*	*[4.17]*	*[1.95]*
ΔSystematic risk	0.577***	0.435***	0.142*
t-stat.	*[10.42]*	*[6.90]*	*[1.69]*
ΔUnsystematic risk	0.327***	0.207***	0.121*
t-stat.	*[7.83]*	*[3.68]*	*[1.72]*

Panel B Delaware old CEOs vs. Non-Delaware CEOs

Variable	Delaware old CEOs (No. of obs. = 1,760)	Non-Delaware matching CEOs	Differences-in-differences
ΔTotal risk	0.183***	0.237***	-0.054***
t-stat.	*[12.91]*	*[17.62]*	*[-2.78]*
ΔSystematic risk	0.418***	0.463***	-0.045*
t-stat.	*[25.66]*	*[28.00]*	*[-1.93]*
ΔUnsystematic risk	0.153***	0.206***	-0.053***
t-stat.	*[10.71]*	*[15.29]*	*[-2.70]*

Table 2.3 Continued

Panel C Differences-in-differences-in-differences (DDD)

Variable	DDD
Total risk	**0.189*** **
t-stat.	*[2.63]*
Systematic risk	**0.187** **
t-stat.	*[2.15]*
Unsystematic risk	**0.173** **
t-stat.	*[2.38]*

Note: This table presents differences-in-differences-in-differences (DDD) estimates with a propensity score matching (PSM) method. To identify matching firms for Delaware firms with young and older CEOs respectively, we first estimate logit regressions to predict the incidence of the incorporation in Delaware (see APPENDIX A for variable definitions):

$$\begin{aligned} &Prob(Delaware\ young\ (old)CEO = 1) \\ &\quad = \alpha + \beta_1 Size + \beta_2 MB + \beta_3 ROA + \beta_4 Sales\ growth + \beta_5 Firm\ focus + \beta_6 Log(Number\ of\ segments) + \beta_7 Firm\ age + \varepsilon \end{aligned}$$

For each Delaware firm with a young CEO, we select a non-Delaware firm in the same industry (based on the two-digit SIC code) that has the closest propensity score, which is the predicted value from the logit regression. Among Delaware firms with young CEOs, we calculate the average change in firm risk around the Wallace decision; we calculate average firm risk (total, systematic, and unsystematic risks) during 1993-1995 and 1996-2000 respectively and then subtract the average firm risk during 1993-1995 from the average firm risk during 1996-2000 (Column 1 in Panel A). We repeat the calculation using the non-Delaware matching peers and present the results in Column 2 of Panel A. The difference between the average change in firm risk among Delaware young CEOs and the average change in firm risk among their non-Delaware matching peers is a differences-in-differences (DD) estimate, that we present in Column 3 of Panel A. We reproduce the results using Delaware firms with older CEOs and their matching peers in Panel B. We test differences between the DD estimate for Delaware young CEOs and that for Delaware old CEOs (i.e., DDD) and present the results in Panel C. The t-statistics is calculated under the null hypothesis that DD or DDD equals zero. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed)

Table 2.4 CEO career concerns and firm risk

	Total risk	Systematic risk	Unsystematic risk
Variable	(1)	(2)	(3)
DEL*AFT*YOUNG	**0.217*** **	**0.185*** **	**0.211*** **
	[0.00]	***[0.00]***	***[0.00]***
DEL*AFT	**-0.072*** **	**-0.064* **	**-0.069*** **
	[0.01]	***[0.10]***	***[0.01]***
AFT*YOUNG	-0.172***	-0.172***	-0.159***
	[0.01]	*[0.01]*	*[0.01]*
DEL*YOUNG	-0.051	-0.036	-0.046
	[0.47]	*[0.62]*	*[0.52]*
DEL	-0.176	-0.002	-0.201
	[0.19]	*[0.99]*	*[0.13]*
AFT	1.257***	1.311***	1.248***
	[0.00]	*[0.00]*	*[0.00]*
YOUNG	0.171**	0.211***	0.152**
	[0.02]	*[0.00]*	*[0.03]*
Size	-0.131***	0.095***	-0.170***
	[0.00]	*[0.00]*	*[0.00]*
MB	0.045***	0.144***	0.023***
	[0.00]	*[0.00]*	*[0.00]*
ROA	-1.723***	-1.657***	-1.727***
	[0.00]	*[0.00]*	*[0.00]*
CEO delta ($mil)	0.005*	0.003	0.004
	[0.10]	*[0.47]*	*[0.29]*
CEO vega ($mil)	0.379***	0.131	0.409***
	[0.00]	*[0.29]*	*[0.00]*
Sales growth	0.047*	0.253***	0.011
	[0.06]	*[0.00]*	*[0.70]*
Firm focus	0.319***	0.087	0.357***
	[0.00]	*[0.31]*	*[0.00]*
Ln(Number of segments)	0.065	0.023	0.07

Table 2.4 Continued

	[0.12]	*[0.56]*	*[0.11]*
Capital expenditure	0.019	0.670***	-0.042
	[0.90]	*[0.00]*	*[0.77]*
Sale of PPE	0.234	-0.956*	0.326
	[0.61]	*[0.06]*	*[0.47]*
R&D	2.957***	3.324***	2.924***
	[0.00]	*[0.00]*	*[0.00]*
Book leverage	0.248***	-0.114**	0.308***
	[0.00]	*[0.03]*	*[0.00]*
Firm age	-0.008***	-0.007***	-0.008***
	[0.00]	*[0.00]*	*[0.00]*
Firm-year observations	5,961	5,961	5,961
R^2	0.64	0.56	0.64
Industry fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
H_0: β(DEL*AFT*YOUNG) + β(DEL*AFT) = 0			
Coefficients sum	0.145***	0.121**	0.143***
F-test p-value	*0.01*	*0.05*	*0.01*

Note: The table reports differences-in-differences-in-differences (DDD) estimates that examine the effect of CEO career concerns on firm risk (Equation (2.1)). The data set comprises 5,961 firm-year observations for all firms covered in ExecuComp during the period 1993-2000 with non-missing values for all required variables. In Model 1, the dependent variable is Total Risk, which is the natural logarithm of the annualized variance of the firm's daily stock returns during the fiscal year. In Model 2, the dependent variable is Systematic Risk defined as the natural logarithm of the annualized variance of the predicted component of stock returns using the expanded market model as described in Section II. In Model 3, the dependent variable is Unsystematic Risk, which is the natural logarithm of the annualized variance of the residual returns from the market model. DEL is a dummy equal to one if the firm is incorporated in Delaware. AFT is a dummy equal to one if the firm-year observation is during the period 1996-2000. YOUNG is a dummy variable equal to one if the CEO is 50 years old or younger as of the sample year. Firm age is calculated as the sample year minus the year in which the firm was first listed on CRSP. Size is the natural logarithm of total assets (AT) measured in the prior fiscal year end. MB is the ratio of the market value (AT – CEQ + PRCC_F*CSHO) to book value of assets (AT) in the prior fiscal year end. ROA is the ratio of net income (IB) to total assets (AT) in the prior fiscal year end. CEO delta is computed as the sensitivity of CEO's stock and option value to a 1% change in stock price in the prior fiscal year end. CEO vega is computed as

Table 2.4 Continued

the sensitivity of CEO's option value to a 1% change in stock return volatility in the prior fiscal year end. Sales growth is computed as the current year's sales (SALE) minus the prior year's sales, all scaled by the prior year's sales. Firm focus is the segment sales based Herfindahl index computed as the sum of squared segment sales-total segment sales ratios. Number of segments is the firm's number of business segments from the Compustat Segment Database. Capital expenditure is the ratio of capital expenditure (CAPX) to total assets (AT). Sale of PPE is the ratio of sale of property, plant, and equipment (SPPE) to total assets (AT). R&D is the ratio of research and development expenditure (XRD) to total assets (AT). If SPPE or R&D is missing, it is set equal to zero. Book leverage is the sum of the firm's long-term debt (DLTT) and short-term debt (DLC), all scaled by total assets (AT). Standard errors are clustered at the state level (not shown). Intercepts, state-, industry (2-digit SIC)-, and year-fixed effects are not shown in the table. *p*-values represent the significance level of each coefficient. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 2.5 Heterogeneity in the treatment effect

Panel A Firm-level heterogeneity

	Staggered board		CEO vega		GIM index	
Variable	*Yes*	*No*	*Low*	*High*	*Low*	*High*
DEL*AFT*YOUNG	**0.141***	**0.260*****	**0.360*****	**0.035**	**0.156***	**-0.131**
	[0.06]	***[0.00]***	***[0.00]***	***[0.78]***	***[0.08]***	***[0.32]***
DEL*AFT	**-0.063****	**-0.073**	**-0.136*****	**-0.004**	**-0.057**	**-0.109***
	[0.03]	***[0.14]***	***[0.00]***	***[0.93]***	***[0.17]***	***[0.06]***
Control variabls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,412	2,325	1,983	1,988	1,682	1,607
R^2	0.64	0.68	0.63	0.72	0.71	0.67
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
H_0: β(DEL*AFT*YOUNG) + β(DEL*AFT) = 0						
Coefficients sum	0.078	0.183***	0.224**	0.031	0.099	-0.240**
F-test p-value	*[0.2]*	*[0.01]*	*[0.02]*	*[0.77]*	*[0.21]*	*[0.03]*
H_0: β1(DEL*AFT*YOUNG) = β2(DEL*AFT*YOUNG).						
t-test p-value	*[0.29]*		*[0.05**]*		*[0.07*]*	

Table 2.5 Continued

Panel B Industry-level heterogeneity

	Industry competitiveness		Majority of peers	
Variable	*High*	*Low*	*Young*	*Old*
DEL*AFT*YOUNG	**0.236*****	**0.043**	**0.273*****	**0.187****
	[0.00]	***[0.82]***	***[0.01]***	***[0.02]***
DEL*AFT	**-0.063****	**-0.107***	**-0.031**	**-0.069****
	[0.05]	***[0.10]***	***[0.55]***	***[0.03]***
Control variables	Yes	Yes	Yes	Yes
Firm-year observations	4,018	625	1,042	4,826
R^2	0.65	0.71	0.64	0.64
Industry fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
*H_0: β(DEL*AFT*YOUNG) + β(DEL*AFT) = 0*				
Coeff. sum	0.173***	-0.064	0.242***	0.117*
F-test p-value	*0.01*	*0.71*	*0.00*	*0.10*
*H_0: β1(DEL*AFT*YOUNG) = β2(DEL*AFT*YOUNG).*				
t-test p-value	*0.25*		*0.74*	

Note: The table reports the estimates of Equation (1) to examine whether the effect of CEO career concerns on firm risk varies for sub-samples that are constructed by the extent of other incentives to pursue the “quiet life”. In Panel A, sub-samples are created based on firm-level variables: staggered board, CEO vega, and GIM index. In Panel B, sub-samples are created based on industry-level variables: industry competitiveness and industry peer composition. In the first column of Panel A, 5,737 firm-year observations, for which the information on staggered board is available from RiskMetrics, are divided into two groups based on the presence of staggered board. In Columns 2 and 3 of Panel A, each year we sort firms into terciles based on CEO vega and GIM index (Gompers, Ishii, and Metrick, 2003). CEO vega is computed as the sensitivity of CEO’s option value to a 1% change in stock return volatility in the prior fiscal year end (Core and Guay, 2002). High (low) CEO vega refers to the highest (lowest) CEO vega tercile. High (low) GIM index refers to the highest (lowest) GIM index tercile. The information on staggered board and GIM index is available in 1993, 1995, 1998, and 2000 from the RiskMetrics publications. We assume that in between publication years, firms have the same staggered board and GIM as in the prior publication year. In the first column of Panel B, each year we sort firms into terciles based on the Herfindahl-Hirschman index (HHI) defined as the sum of squared market shares. Market share is the ratio of the firm’s sales (SALE) to the sum of sales of all Compustat firms in the same three-digit SIC code.

Table 2.5 Continued

High (low) industry competitiveness refers to the lowest (highest) HHI tercile. In the second column of Panel B, each year we sort firms into two groups based on industry peer composition. For each two-digit SIC industry in 1994, we calculate the percentage of young CEOs, who are 50 years old or younger. If the percentage is higher than or equal to 40%, a given industry is classified as young, and otherwise as old. We require each industry to have at least 5 CEOs with age information available from ExecuComp. The dependent variable is Total risk, which is the natural logarithm of the annualized variance of the firm's daily stock returns during the fiscal year. DEL is a dummy equal to one if the firm is incorporated in Delaware. AFT is a dummy equal to one if the firm-year observation is during the period 1996-2000. YOUNG is a dummy variable equal to one if the CEO is 50 years old or younger as of the sample year. Firm age is calculated as the sample year minus the year in which the firm was first listed on CRSP. Size is the natural logarithm of total assets (AT) measured in the prior fiscal year end. MB is the ratio of the market value (AT – CEQ + PRCC_F*CSHO) to book value of assets (AT) in the prior fiscal year end. ROA is the ratio of net income (IB) to total assets (AT) in the prior fiscal year end. CEO delta is computed as the sensitivity of CEO's stock and option value to a 1% change in stock price in the prior fiscal year end. Sales growth is computed as the current year's sales (SALE) minus the prior year's sales, all scaled by the prior year's sales. Firm focus is the segment sales based Herfindahl index computed as the sum of squared segment sales-total segment sales ratios. Number of segments is the firm's number of business segments from the Compustat Segment Database. Capital expenditure is the ratio of capital expenditure (CAPX) to total assets (AT). Sale of PPE is the ratio of sale of property, plant, and equipment (SPPE) to total assets (AT). R&D is the ratio of research and development expenditure (XRD) to total assets (AT). If SPPE or R&D is missing, it is set equal to zero. Book leverage is the sum of the firm's long-term debt (DLTT) and short-term debt (DLC), all scaled by total assets (AT). Standard errors are clustered at the state level (not shown). Intercepts, state-, industry (2-digit SIC)-, and year-fixed effects are not shown in the table. *P*-values represent the significance level of each coefficient. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 2.6 CEO age, firm risk, and external governance – no exogenous shock

	Dependent variable = Total risk			
	GIM		HHI	
Variable	Strong governance (Q1)	Weak governance (Q4)	High competition (Q1)	Low competition (Q4)
CEO age	-0.008***	-0.001	-0.011***	-0.004
	[0.00]	*[0.71]*	*[0.00]*	*[0.16]*
Firm size	-0.139***	-0.171***	-0.187***	-0.202***
	[0.00]	*[0.00]*	*[0.00]*	*[0.00]*
M/B	0.020**	0.054***	0.027***	0.052***
	[0.02]	*[0.00]*	*[0.00]*	*[0.01]*
ROA	-1.028***	-2.126***	-0.611***	-2.684***
	[0.00]	*[0.00]*	*[0.00]*	*[0.00]*
Book leverage	0.142	0.350**	0.077	0.521***
	[0.37]	*[0.02]*	*[0.31]*	*[0.00]*
Capital expenditure	0.071	-0.224	-0.138	0.228
	[0.86]	*[0.63]*	*[0.50]*	*[0.56]*
Sale of PPE	0.805	2.405**	-0.035	-0.261
	[0.50]	*[0.02]*	*[0.98]*	*[0.76]*
R&D	0.208	3.207***	1.470***	-1.109
	[0.31]	*[0.00]*	*[0.00]*	*[0.29]*
Log(No. of segments)	-0.114***	-0.072**	-0.076***	-0.064
	[0.01]	*[0.04]*	*[0.00]*	*[0.15]*
GIM	-0.035*	-0.025		
	[0.06]	*[0.11]*		
HHI			-2.380***	-0.42
			[0.00]	*[0.28]*
Firm-year obs.	2829	2705	8449	1993
R^2	0.6	0.6	0.62	0.59
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Table 2.6 Continued

Note: The initial sample comprises 10,906 firm-year observations for all firms covered in ExecuComp during the period 1992-2006 with non-missing values for all required variables. Each year, we sort firms into quartiles based on the GIM-index (Gompers, Ishii, and Metrick, 2003) in Columns 1 and 2, and industry competitiveness, measured by HHI, in Columns 3 and 4. The information on GIM index is available in 1990, 1993, 1995, 1998, 2000, 2002, and 2004 from the RiskMetrics publications. We assume that in between publication years, firms have the same GIM as in the prior publication year. For each quartile, we estimate the following OLS regression model:

$$Total\ risk_{j,t} = a_t + \delta_i + \beta * (CEO\ age) + \gamma X_{j,t} + \varepsilon_t$$

Strong governance refers to the lowest GIM quartile, and Weak governance refers to the highest GIM quartile. High competition refers to the lowest HHI quartile, and Low governance refers to the highest HHI quartile. The dependent variable is Total risk, which is the natural logarithm of the annualized variance of the firm's daily stock returns during the fiscal year. Standard errors are clustered at the state level (not shown). Intercepts, industry (2-digit SIC)- and year-fixed effects are not shown in the table. *P*-values represent the significance level of each coefficient. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 2.7 Young CEO departures and new employment opportunities

Panel A Reasons for young CEOs' departures

	Number of observations (*N*)	Percentage (%)
PURSUE	19	24.05%
FORCED	20	25.32%
CONTROL	5	6.33%
RETIRE	4	5.06%
RESIGN	31	39.24%
Total	79	100.00%

Panel B Career opportunities following young CEOs' departures

<table>
<tr><th></th><th>N</th><th>%</th><th></th><th>N</th><th>%</th><th>Reasons</th><th>N</th><th>%</th><th>New employer</th><th>N</th></tr>
<tr><td rowspan="23">CEOs who land a new executive position</td><td rowspan="23">46</td><td rowspan="23">58%</td><td rowspan="10">Receive CEO positions at a new employer</td><td rowspan="10">28</td><td rowspan="10">61%</td><td rowspan="2">PURSUE</td><td rowspan="2">6</td><td rowspan="2">21%</td><td>public</td><td>0</td></tr>
<tr><td>private</td><td>6</td></tr>
<tr><td rowspan="2">FORCED</td><td rowspan="2">6</td><td rowspan="2">21%</td><td>public</td><td>1</td></tr>
<tr><td>private</td><td>5</td></tr>
<tr><td rowspan="2">CONTROL</td><td rowspan="2">4</td><td rowspan="2">14%</td><td>public</td><td>1</td></tr>
<tr><td>private</td><td>3</td></tr>
<tr><td rowspan="2">RETIRE</td><td rowspan="2">0</td><td rowspan="2">0%</td><td>public</td><td>0</td></tr>
<tr><td>private</td><td>0</td></tr>
<tr><td rowspan="2">RESIGN</td><td rowspan="2">12</td><td rowspan="2">43%</td><td>public</td><td>5</td></tr>
<tr><td>private</td><td>7</td></tr>
<tr><td rowspan="11">Receive non-CEO positions at a new employer</td><td rowspan="11">18</td><td rowspan="11">39%</td><td rowspan="2">PURSUE</td><td rowspan="2">12</td><td rowspan="2">67%</td><td>public</td><td>5</td></tr>
<tr><td>private</td><td>7</td></tr>
<tr><td rowspan="3">FORCED</td><td rowspan="3">2</td><td rowspan="3">11%</td><td>public</td><td>0</td></tr>
<tr><td>private</td><td>1</td></tr>
<tr><td>governor</td><td>1</td></tr>
<tr><td rowspan="2">CONTROL</td><td rowspan="2">0</td><td rowspan="2">0%</td><td>public</td><td>0</td></tr>
<tr><td>private</td><td>0</td></tr>
<tr><td rowspan="2">RETIRE</td><td rowspan="2">0</td><td rowspan="2">0%</td><td>public</td><td>0</td></tr>
<tr><td>private</td><td>0</td></tr>
<tr><td rowspan="2">RESIGN</td><td rowspan="2">4</td><td rowspan="2">22%</td><td>public</td><td>2</td></tr>
<tr><td>private</td><td>2</td></tr>
<tr><td rowspan="5">CEOs who do not land a new executive position</td><td rowspan="5">33</td><td rowspan="5">42%</td><td rowspan="5"></td><td rowspan="5"></td><td rowspan="5"></td><td>PURSUE</td><td>1</td><td>3%</td><td></td><td></td></tr>
<tr><td>FORCED</td><td>12</td><td>36%</td><td></td><td></td></tr>
<tr><td>CONTROL</td><td>1</td><td>3%</td><td></td><td></td></tr>
<tr><td>RETIRE</td><td>4</td><td>12%</td><td></td><td></td></tr>
<tr><td>RESIGN</td><td>15</td><td>45%</td><td></td><td></td></tr>
<tr><td>Total</td><td>79</td><td>100%</td><td></td><td></td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
</table>

Table 2.7 Continued

Panel C Risk-taking and the likelihood of getting a new executive position

	Dependent variable = dummy equals 1 if a CEO obtains a new executive position.
Variable	(1)
CEO age	-0.058
	[0.61]
CEO tenure	0.354**
	[0.02]
Size	0.389
	[0.17]
Firm performance	0.229
	[0.95]
Risk-taking	1.783***
	[0.00]
FORCED	-0.073
	[0.98]
CONTROL	-1.968
	[0.35]
RETIRE	-18.892***
	[0.00]
PURSUE	13.494***
	[0.00]
Number of observations	73
Number of firms	65
R^2	0.796
Industry fixed effects	Yes
Year fixed effects	Yes
State fixed effects	No
State-level clustered standard error.	Yes

Note: Panel A presents summary statistics of stated reasons for young CEO departures. Following Fee and Hadlock (2004), we identify young CEO departures for firms covered in ExecuComp during 1996 and 2000 and collect information on the reasons behind their departures from news articles, mainly using the Factiva search engine. Each of the young CEO departures is assigned to a single category based on its reason. We assign a departure to the PURSUE category if a news article specifies that the young CEO

Table 2.7 Continued

leaves the company to pursue other interests or similar expression. Among the remaining departures, we assign the departure to the FORCED category if the young CEO is ousted from office. For the remaining departures, we assign the departure to the CONTROL category if the departure is associated with corporate control activity such as M&As, divestitures, and ownership transitions. The remaining departures are assigned to the RETIRE category, if a news article explicitly describes the departure using the word “retire”. All the remaining departures are assigned to the RESIGN category. Panel B reports summary statistics of young CEOs’ new employment opportunities following departures. Panel C presents the estimates of logit regression coefficients in order to examine the relationship between young CEOs’ risk-taking and their subsequent employment opportunities. Intercepts, industry (2-digit SIC)-, and year-fixed effects are not shown in the table. *p*-values represent the significance level of each coefficient. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

Table 2.8 Long-run stock performance

Panel A Fama-French three-factor model

	Value-weighted portfolios	Equal-weighted portfolios
	Intercept	*Intercept*
Young Delaware CEOs	0.714*	1.892***
t-stat.	*[1.91]*	*[5.39]*
Adj. R^2	0.89	0.86
Older Delaware CEOs	0.146	0.677**
t-stat.	*[1.05]*	*[2.18]*
Adj. R^2	0.94	0.79
Difference	0.568	1.216***
t-stat.	*[1.42]*	*[2.60]*

Panel B Carhart four-factor model

	Value-weighted portfolios	Equal-weighted portfolios
	Intercept	*Intercept*
Young Delaware CEOs	0.501	1.812***
t-stat.	*[1.17]*	*[4.35]*
Adj. R^2	*0.89*	0.86
Older Delaware CEOs	0.276*	0.825**
t-stat.	*[1.74]*	*[2.20]*
Adj. R^2	0.94	0.79
Difference	0.225	0.987*
t-stat.	*[0.49]*	*[1.76]*

Note: The table presents abnormal returns to portfolios of Delaware firms having a young or older CEO during the period 1996-2000. In each month, we form a portfolio that consists of Delaware firms having a young CEO and another portfolio that consists of Delaware firms with an older CEO. The time series of each portfolio's monthly excess returns are then regressed on the three/four return factors. Panel A reports the estimates of abnormal returns to the portfolios using Fama and French's (1993) three factor model: $R_{p,t} - R_{f,t} = \alpha + \beta(R_{m,t} - R_{f,t}) + \gamma SMB_t + \delta HML_t + \varepsilon$ where $R_{p,t}$ is the monthly portfolio return of young/older Delaware firms in month *t*; $R_{f,t}$ is the three-month Treasury bill yield in month *t*; $R_{m,t}$ is the return on the value-weighted/equal-weighted index of stocks listed on NYSE, AMEX, and NASDAQ in month *t*; SMB_t is the return on small firms net the return on large firms in month *t*; HML_t is the return on high book-to-market firms net the return on low book-to-market firms in month *t*. Panel B reports the estimates of abnormal returns to the portfolios using Carhart's (1997) four factor model: $R_{p,t} - R_{f,t} = \alpha + \beta(R_{m,t} - R_{f,t}) + \gamma SMB_t + \delta HML_t + \theta MOM_t + \varepsilon$ where MOM_t is the momentum factor in month *t*. *t*-statistics are presented in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

CHAPTER 3

DIVIDEND STICKINESS, DEBT COVENANTS, AND EARNINGS MANAGEMENT

3.1 Introduction

In his seminal study, Lintner (1956) concluded that firms' dividend payments are sticky, in that managers tie dividends to long-term sustainable dividends and avoid increasing dividends that might have to be cut later. Subsequent studies support this view, but it is not clear how sticky dividends are or whether dividends are less sticky than they used to be. Brav et al. (2005) "find that dividend decisions are still made conservatively but that the importance of targeting the payout ratio has declined" (p. 499). Based on a survey of financial executives, 23% of firms did not really have a dividend target. Moreover, results from estimating a partial adjustment model of dividend policy indicate that "the target payout ratio is no longer the central focus of dividend policy at many firms" (p. 506). In addition, Guttman, Kadan, and Kandel (2010) "calculated that 38% of all firms that ever paid dividends two years in a row between 1966 and 2005 never show a sticky dividend even once during those 40 years" (p. 4472).

Even if dividends are sticky, Lintner (1956) concedes that "stockholders would understand and accept the cut" in dividends in the face of "any substantial or continued decline in earnings" (p. 101). Indeed, Grullon et al. (2002) and Lie (2005a, 2005b) report that earnings declines foreshadow dividend cuts. There are two possible reasons for this. First, declining earnings can deplete resources needed for continued operations and a healthy balance sheet. Cutting dividends naturally offsets this depletion. Second, low earnings can trigger violations of dividend covenant restrictions, which would require a cut in dividends. However, in these cases managers have a different way out: manipulate the earnings upward to dodge the dividend covenant. The downside is that earnings management is hazardous, because it can result in shareholder lawsuits, SEC enforcement actions, and executive turnover (DuCharme et al., 2004; Karpoff et al., 2008; Hazarika et al., 2012). Thus, any evidence that managers prefer earnings management to dividend cuts would provide evidence that dividends are extremely sticky.

Healy and Palepu (1990) find that firms that are close to violating dividend covenant restrictions often cut dividends, but do not manage earnings to avoid violations. Furthermore, DeAngelo et al. (1994) find that for a sample of financially distressed firms that decrease dividends, those with binding debt covenants are no more likely to make income-increasing accounting changes than other firms. In contrast, Daniel, Denis, and Naveen (2008) (henceforth DDN), argue that the samples in the studies mentioned above are small and prone to selection bias. Thus, they revisit the issue using a large sample of S&P 1500 firms between 1992 and 2005. They posit that firms manage earnings upward when pre-managed earnings are expected to fall short of dividend payments, in part because debt covenants often include some form of dividend restriction. To test their hypothesis, DDN relate discretionary accruals (their measure of earnings managements) to the dividend deficit *D_Deficit*, defined as Max (0, previous year's dividends – pre-managed earnings). They find that discretionary accruals are positively related to *D_Deficit* for dividend payers, especially before the passing of the Sarbanes-Oxley Act (SOX), after the 2003 dividend tax cut, and for firms with positive debt. They also report evidence that earnings management that eliminates *D_Deficit* significantly reduces the probability of dividend cuts. In sum, DDN interpret their results to mean that "(i) reported earnings are a binding constraint on dividend levels, and (ii) firms actively manage earnings in order to maintain dividends by circumventing this constraint" (p. 4).

By construction, DDN's primary variable of interest, *D_Deficit*, is correlated with pre-managed earnings' shortfall relative to the previous year's earnings, because dividend levels are highly correlated with earnings levels. That is, firm-years with positive *D_Deficit* are likely to have pre-managed earnings below the previous year's earnings. The earnings management literature shows that firms manage earnings upward to avoid reporting earnings decreases (Burgstahler and Dichev, 1997; Graham et al., 2005). Accordingly, it is critical to control for firms' incentive to manage earnings upward to meet or beat the previous year's earnings in testing whether firms manage earnings toward expected dividends (i.e., the previous year's dividends). DDN "include the prior year's earnings and current year's forecasted earnings (obtained from IBES) to control for the tendency of firms to manage earnings to meet prior year's earnings levels (Burgstahler and Dichev, 1997)..." (p. 10). However, firms' incentive to engage in earnings management is not a function of prior year's earnings *levels* but whether pre-managed earnings *fall short of* prior year's earnings levels. Therefore, the findings of DDN are potentially subject to bias arising from an omitted variable.

We examine whether the findings reported in DDN are robust to controlling for firms' tendency to manage earnings to avoid reporting earnings decreases. To address this omitted variable problem, we construct a variable that captures firms' incentive to engage in earnings management to avoid reporting earnings decreases. Similar in spirit to DDN's *D_Deficit* measure, we define *E_Deficit* as Max (0, previous year's earnings – pre-managed earnings). That is, *E_Deficit* measures the distance between pre-managed earnings and the prior year's earnings conditional on the distance being positive. Then we explore whether the association between discretionary accruals and *D_Deficit* documented by DDN is robust to controlling for *E_Deficit*. When we include *E_Deficit* in regressions of discretionary accruals on *D_Deficit* and a set of control variables, we find that the coefficient estimate on *D_Deficit* diminishes in magnitude and becomes statistically insignificant. Subsequent analyses suggest that this result is not merely attributable to the correlation between *E_Deficit* and *D_Deficit*. Overall, our multivariate analyses indicate that discretionary accruals are not associated with *D_Deficit* after controlling for firms' tendency to manage accruals to avoid reporting earnings decreases.

Next, we investigate firms' dividend cut decisions. DDN find that eliminating *D_Deficit* decreases the probability of cutting dividends. But this result seems inconsistent with our result that firms do not manage earnings to avoid cutting dividends when expected earnings fall short of dividend payments. Instead, we expect that firms are more likely to cut dividends when reported earnings fall short of expected dividends. We begin by replicating DDN's findings. DDN estimate a logit model in which the dependent variable is an indicator variable equal to one for dividend cuts, and the independent variables include an indicator variable equal to one if there is a shortfall before discretionary accruals are added (i.e., *D_Deficit* is positive), an indicator variable equal to one if there is a shortfall before but a surplus after discretionary accruals are added, and continuous control variables. Consistent with their conjecture that earnings management that eliminates *D_Deficit* helps firms avoid dividend cuts, DDN find that the coefficients on their two indicator variables are positive and negative, respectively, and both are statistically different from zero. We find the same results when we replicate their logit specification.

To investigate whether firms tend to cut dividends when reported earnings fall short of expected dividends, we construct an indicator variable equal to one if there is a shortfall after discretionary accruals are added and then estimate the above logit model along with this new indicator variable. Consistent with our prediction that firms cut dividends when reported earnings fall short of past dividends, we find that the coefficient in this new indicator variable is positively significant. Furthermore, we find that DDN's results are not robust to controlling for the level of reported earnings. The indicator variable for when there is a shortfall before but a surplus after discretionary accruals are added (i.e., DDN's proxy for earnings management) becomes insignificant after controlling for the new indicator variable that captures reported earnings' shortfall relative to past dividend levels. Thus, what appears to matter to the decision to cut dividends is whether reported earnings fall short of past dividends, whereas earnings management has no discernible effect, consistent with the findings and arguments of Healy and Palepu (1990).

We also conduct several additional analyses. First, we investigate whether *D_Deficit* is related to discretionary accruals during firm-years in which the propensity to manage earnings to meet dividend thresholds is predicted to be most pronounced. Following DDN, we conduct regression analyses for multiple sub-samples: the pre-SOX period versus the post-SOX period, the pre-2003 dividend tax cut period versus the post-2003 dividend tax cut period, and firm-years with positive debt versus firm-years with zero debt. DDN find that the propensity to manage earnings to meet or beat dividend thresholds is more pronounced for the pre-SOX sub-sample, the post-2003 dividend tax cut sub-sample, and the positive debt sub-sample. Our analyses, however, reveal that after controlling for *E_Deficit*, the coefficient estimates on *D_Deficit* become insignificant even for these three sub-samples.

Second, we examine the frequency distribution of EPS (earnings per share) less DPS (dividends per share) for evidence of earnings management. DDN show that there are abnormally many observations with values equal to zero or just above zero, consistent with earnings management to meet dividend thresholds. But we find that this pattern disappears when we form the distribution on the basis of lagged DPS (i.e., $EPS_t - DPS_{t-1}$) instead of contemporaneous DPS (i.e., $EPS_t - DPS_t$) as DDN do. The pattern is absent even when we constrain the sample to firm-years before SOX or that have positive debt. This suggests that the pattern documented in DDN is due to a tendency to set dividends relative to expected earnings rather than a tendency to manage earnings to meet dividend thresholds.

Finally, we repeat our main analyses for a sample of restatement firms to alleviate concerns that the 'backing-out' approach to measuring pre-managed earnings and managed earnings (i.e., discretionary accruals) leads to a spurious relation between discretionary accruals and *E_Deficit* or *D_Deficit* (Elgers et al. (2002)). For the restatement sample, we re-define *E_Deficit* as Max (0, the previous year's earnings – restated earnings) and *D_Deficit* as Max (0, previous year's dividends – restated earnings). We also use the difference between originally reported earnings and restated earnings as a proxy for earnings management. For the restatement sample, we find that *E_Deficit* is positively associated with managed earnings (i.e., the difference between original earnings and restated earnings) but not *D_Deficit*.

In summary, our analyses do not support the notion that firms manage earnings in order to avoid cutting dividends. Unlike DDN, we find no evidence that firms manage earnings upward when expected earnings fall short of expected dividends. Instead, our findings suggest that firms cut dividends when reported earnings fall short of past dividend levels.

Our paper contributes to the literature on the stickiness of dividends, which we summarized earlier. DDN find evidence that dividends are so sticky that instead of cutting dividends, managers prefer to manage earnings, despite the associated risk of shareholder lawsuits, regulatory enforcement actions, and executive turnover. While we agree that managers are reluctant to cutting dividends, our results suggest that they are not so reluctant that they prefer to manipulate earnings. As such, our study sheds light on how sticky dividends are.

Our paper also contributes to the literature that examines the debt covenant hypothesis that firms make accounting choice or manage earnings to avoid violating covenants (Watts and Zimmerman, 1986). Prior work provides mixed evidence on this hypothesis. Healy and Palepu (1990) and DeAngelo et al. (1994) offer no supportive evidence when focusing on dividend constraints, whereas Sweeney (1994) and DeFond and Jiambalvo (1994) provide supportive evidence when examining a broader set of accounting-based restrictions. The combined evidence in these four studies supports the more nuanced view of Healy and Palepu (1990) and DeFond and Jiambalvo (1994) that violations of dividend constraints can be mitigated by cutting dividends and are therefore less likely to induce earnings manipulation than other debt covenants. On this backdrop, the evidence in DDN that firms materially manage earnings to avoid dividend covenants violations is disquieting. However, our large-sample evidence shows that the results in DDN suffer from serious bias, and that firms that are close to binding dividend constraints often cut dividends but do not engage in any discernible earnings management.

Our paper is organized as follows. Section 3.2 discusses sample selection procedure and variable measurements. Section 3.3 presents main empirical results. Section 3.4 provides additional analyses results. Section 3.5 concludes.

3.2 Sample selection and variable measurements

3.2.1 Sample selection

We duplicate DDN's sample selection as closely as possible. We begin with the S&P 1500 firms on Execucomp database for the period between 1992 and 2005. We delete firms that operate in financial (6000 ≤ SIC ≤ 6999) and utility (4400 ≤ SIC ≤ 4999) industries. We also exclude firms with a CRSP share code other than 10 and 11. Finally, we require observations to have sufficient information to estimate discretionary accruals.[23]

3.2.2 Variable measurements

3.2.2.1 Discretionary accruals

Like DDN, we employ a cross-sectional Jones (1991) model to estimate discretionary accruals by regressing total accruals on changes in sales, and gross property, plant, and equipment for each year and for each two-digit SIC.[24] All variables are deflated by lagged total assets. Following Hribar and Collins (2002), we compute

[23] Observations are required to have income before extraordinary items (Compustat Acronym: EBEXTRA), cash flows from operation (Compustat Acronym: OANCF), sales (Compustat Acronym: SALE), and gross property, plant, and equipment (Compustat Acronym: PPEGT).

[24] We require at least five observations for each year and for each two-digit SIC as in DDN.

total accruals as the difference between income before extraordinary items (Compustat Acronym: EBEXTRA) and cash flows from operation (Compustat Acronym: OANCF). Discretionary and non-discretionary accruals are the residuals and the fitted values from the above regressions, respectively. We then multiply the estimated discretionary and non-discretionary accruals by lagged total assets to obtain the dollar values of discretionary and non-discretionary accruals.

3.2.2.2 Deficits to expected dividends

To test the hypothesis that firms engage in upward earnings management to meet or beat dividend thresholds, DDN define a variable that captures firms' incentive to manage earnings upward toward dividend thresholds. Like DDN, we define *D_Deficit* as Max (0, expected dividends – pre-managed earnings), where expected dividends are proxied by the prior year's dividend and pre-managed earnings are proxied by non-discretionary accruals plus cash flows from operation minus preferred dividends.[25] Consistent with DDN, we employ a non-linear specification because DDN have no predictions for firm-years in which firms have pre-managed earnings that do not fall short of expected dividends. That is, *D_Deficit* is set to zero if pre-managed earnings exceed expected dividend levels (i.e., the prior year's dividends). However, *D_Deficit* is set to the difference between pre-managed earnings and expected dividends if pre-managed earnings fall short of the expected dividends.

3.2.2.3 Deficits to prior year's earnings

The earnings management literature provides evidence that firms manage earnings to avoid reporting earnings decreases. Burgstahler and Dichev (1997) examine the frequency distribution of earnings changes and find that there are too few

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[25] DDN refer to Max (0, expected dividends – pre-managed earnings) as *Deficit*. In this paper, we label this variable as *D_Deficit* in order to differentiate it from *E_Deficit*, a variable that is a proxy for firms' incentive to manage earnings to avoid reporting earnings decreases.

observations in the bin just left to zero and too many observations in the bin just right to zero. They interpret this discontinuity in the distribution of earnings changes to mean that firms manage earnings to avoid reporting earnings decreases. Consistent with this empirical evidence, the survey conducted by Graham et al. (2005) reveals that CFOs view the prior year's earnings as a primary earnings target.

In order to measure the tendency to manage earnings to meet or beat the previous year's earnings, we define *E_Deficit* as Max (0, the previous year's earnings – pre-managed earnings). Thus, *E_Deficit* captures firms' incentive to manage earnings upward when they expect pre-managed earnings to be lower than the prior year's earnings. Just like DDN utilize a non-linear specification in constructing *D_Deficit*, we use a non-linear specification in constructing *E_Deficit*, because we have no predictions for discretionary accruals when firms have pre-managed earnings that exceed the previous year's earnings.

3.3 Main results

3.3.1 Profile analyses

To test whether firms manage earnings upward when they expect pre-managed earnings to fall short of expected dividends, DDN conduct two sets of univariate analyses. First, they examine whether firms that have a positive *D_Deficit* are more likely to report positive discretionary accruals than those that have a *D_Deficit* equal to zero. DDN find that 81% of firms with positive *D_Deficit* exhibit positive discretionary accruals, whereas only 42% of firms with *D_Deficit* equal to zero exhibit positive discretionary accruals. Second, DDN investigate whether discretionary accruals are increasing with the level of *D_Deficit*. DDN document that discretionary accruals are positive and increasing with *D_Deficit* for firms with positive *D_Deficit*. In contrast, DDN find that discretionary accruals are, on average, negative for firms with *D_Deficit* equal to zero. DDN interpret these findings to mean that firms are more likely to manage accruals upward when pre-managed earnings fall short of expected dividend levels.

We replicate DDN's analyses and provide the results in Table 3.1. In Panel A, we bifurcate the observations into groups of firm-years with *D_Deficit* equal to zero and firm-years with positive *D_Deficit*. We further partition each group into observations with non-positive discretionary accruals and observations with positive discretionary accruals. The results in Panel A reveal that the proportion of firm-years with positive discretionary accruals is 83% for the positive *D_Deficit* group, whereas the fraction of firm-years with positive discretionary accruals is 46% for the zero *D_Deficit* group. In Panel B, we sort observations in the positive *D_Deficit* group into quintiles on the basis of *D_Deficit*. We then compute the average discretionary accruals separately for each quintile and for the zero *D_Deficit* group. The results displayed in Panel B show that average discretionary accruals are monotonically increasing with the level of *D_Deficit.* In addition, we find that average discretionary accruals are negative for the zero *D_Deficit* group and positive for the positive *D_Deficit* group. Overall, our findings in Table 3.1 are consistent with the evidence in DDN.

Next, we conduct the same sets of analyses to examine how *E_Deficit* is associated with discretionary accruals. Because *E_Deficit* measures firms' incentives to engage in earnings management when they anticipate that pre-managed earnings fall short of the prior year's earnings, we expect *E_Deficit* to have a positive relation with discretionary accruals. The results presented in Table 3.2 indicate that this is indeed the case. In Panel A, we partition observations into one group consisting of firm-years with *E_Deficit* equal to zero and another group consisting of firm-years with positive *E_Deficit*. We further decompose each group into firm-years with non-positive discretionary accruals and firm-years with positive discretionary accruals. We find that the fraction of firm-years with positive discretionary accruals is 83% for the positive *E_Deficit* group, whereas the proportion of firm-years with positive discretionary accruals is 29% for the zero *E_Deficit* group. In Panel B, we form quintiles on the basis of *E_Deficit* for firm-years in the positive *E_Deficit* sample and report average discretionary accruals for each quintile along with average discretionary accruals for the zero *E_Deficit* sample. The average discretionary accruals are negative for the zero *E_Deficit* sample and positive and monotonically increasing with the level of *E_Deficit* for the positive *E_Deficit* sample. Collectively, our findings to this point suggest that *E_Deficit* and *D_Deficit* are associated with discretionary accruals in strikingly similar ways.

Finally, we investigate the extent to which *D_Deficit* and *E_Deficit* are correlated with each other and provide results in Table 3.3. We expect *D_Deficit* to be highly correlated with *E_Deficit*, because dividend levels likely have a high correlation with earnings levels. Consistent with our expectation, Panel A reports that the Pearson (Spearman) correlation between *D_Deficit* and *E_Deficit* is as high as 0.73 (0.55) for firms that pay dividends. In order to shed more light on the relation between *D_Deficit* and *E_Deficit*, we independently sort firm-years into five groups based on *D_Deficit* and *E_Deficit* to construct twenty five portfolios. Because *D_Deficit* and *E_Deficit* are positively correlated with discretionary accruals and each other, we expect firm-year observations to cluster in a diagonal. Consistent with our prediction, in Panel B, we find that the number of observations in diagonal cells is much higher than the number of observations in the other cells. Taken together, the results reported in this section indicate that *E_Deficit* is an omitted correlated variable in DDN's study.

3.3.2 Multivariate analyses

DDN conduct multivariate analyses to examine whether *D_Deficit* is significantly associated with discretionary accruals after controlling for factors known to be associated with discretionary accruals. Specifically, DDN run several sets of pooled cross-sectional regressions of discretionary accruals on *D_Deficit* and a set of control variables. The control variables include an indicator variable that takes the value of one if a firm pays dividend in the prior year, retained earnings, proxies for managerial incentives (e.g., CEO portfolio delta, CEO portfolio vega, and cash compensation), firm size, firm leverage, and the ratio of market value of equity to book value of equity.[26] DDN's main results (Model 2 of Table 3 (p. 9)), which are reprinted as our Model 1 in Table 3.4, show that the coefficient estimate on *D_Deficit* is positive and statistically significant. DDN interpret this finding as indicating that firms manage earnings to meet or beat dividends thresholds.[27]

We begin by replicating DDN's main results (for a sample of both dividends payers and non-payers). The replication results are presented as Model 2 of Table 3.4. Our results correspond closely to DDN's results. The coefficient estimates on *D_Deficit* is positive and statistically significant both for our sample (coefficient = 0.958; *t*-statistic = 13.78) and for DDN's sample (coefficient = 0.937; *t*-statistic = 8.4). The estimation results for other variables are also similar to those of DDN. In particular, we find that the indicator variable that equals one if a firm pays dividend in the prior year, cash compensation, and firm size have statistically significant coefficient estimates and the same signs as in DDN. However, we find that the coefficients on Delta and Vega do not differ statistically from zero in our study, while they do (at the 0.05 level of significance) in DDN's study.

As shown in the previous section, DDN's primary variable of interest, *D_Deficit*, is highly correlated with *E_Deficit* (i.e., the shortfall in pre-managed earnings to the prior year's earnings). The earnings management literature finds that firms have incentives to manage earnings upward to avoid reporting earnings decreases (Burgstahler and Dichev,

[26] CEO portfolio delta is expected dollar change in CEO wealth for a 1% change in stock price (Core and Guay (2002)), CEO portfolio vega is expected dollar change in CEO wealth for a 0.01 change in stock return volatility (Core and Guay (2002)), cash compensations is equal to salary plus bonus, firm size is the log of total assets, and leverage is the ratio of short-term plus long-term debt to total assets.

[27] DDN also run a pooled regression in which they include the prior year's earnings level and the current year's forecasted earnings level as additional control variables. They continue to find a significantly positive coefficient on *D_Deficit*.

1997; Graham et al., 2005). Accordingly, it is critical to control for firms' propensity to manage earnings upward to meet the prior year's earnings when studying whether firms manage earnings upward to meet dividend thresholds. DDN include the prior year's earnings levels in order to control for firms' tendency to manage earnings to avoid reporting earnings decreases. However, what matters for firms to engage in earnings management is the *shortfall* in pre-managed earnings with respect to the previous year's earnings rather than the prior earnings *level.* Consequently, DDN's multivariate analyses results are subject to an omitted variable bias.

In order to assess whether the significant association between discretionary accruals and *D_Deficit* documented in DDN is attributable to an omitted variable bias, we run pooled cross-sectional regressions of discretionary accruals on *E_Deficit* and *D_Deficit* along with a set of previously mentioned control variables. When both *E_Deficit* and *D_Deficit* are included along with an array of control variables, the coefficient estimate on *D_Deficit* diminishes in magnitude and becomes marginally significant. In particular, the coefficient changes from 0.958 to 0.131 and the accompanying *t*-statistic changes from 13.78 to 1.76. This indicates that *D_Deficit* has only marginally incremental effect on discretionary accruals after controlling for *E_Deficit* and a set of control variables.

DDN perform multivariate analyses for a sample of firm-years that include both dividend payers and non-payers. However, there is naturally no incentive to manage earnings to meet or beat dividends thresholds for non-payers. DDN also acknowledge that *D_Deificit* for non-payers "effectively represents the shortfall in pre-managed earnings with respect to zero, which is just the shortfall with respect to the "loss-avoidance" threshold of Burgstahler and Dichev (1997)" (p. 10). Furthermore, DDN restrict their subsequent analyses to a sample of firms that pay dividends. Accordingly, we restrict our further analyses to a sample of firm-years in which firms pay dividends in the previous year.

To determine whether DDN's main results hold when the sample includes only payers, we run pooled cross-sectional regressions of discretionary accruals on *D_Deficit* and a set of previously mentioned control variables for the sample of payers. The results presented in the Model 4 column of Table 3.4 indicate that DDN's main results are robust to this modification. We find that the coefficient estimate on *D_Deficit* is 0.809 and significantly different from zero (*t*-statistic = 9.15), and it is similar in magnitude to the corresponding coefficient in Model 2 of 0.958.

Next, we repeat the regressions of discretionary accruals on *E_Deficit* and *D_Deficit* along with a set of previously mentioned control variables only for payers. The estimation results presented in the Model 5 column of Table 3.4 confirm our conjecture that *E_Deficit* is an omitted correlated variable. When both *E_Deficit* and *D_Deficit* are included along with an array of control variables, the coefficient estimate on *D_Deficit* diminishes in magnitude and becomes statistically insignificant. We find that the coefficient changes from 0.809 to -0.009 and the corresponding *t*-statistic changes from 9.15 to -0.09. This suggests that *D_Deficit* has little incremental effect on discretionary accruals after controlling for *E_Deficit* and a set of control variables. That is, we fail to find evidence that firms manage accruals upward to meet or beat dividend thresholds after controlling for firms' tendency to manage earnings upward to avoid reporting earnings decreases.[28]

Although *D_Deficit* loses explanatory power for discretionary accruals after controlling for *E_Deficit* and a set of control variables, there are several issues that cloud our interpretation. First, the high correlation (Pearson correlation = 0.73) between *D_Deficit* and *E_Deficit* raises concerns of multi-collinearity in regressions in which both variables are included. To address this concern, we conduct regression analyses for a

[28] We also note that the R^2 more than doubles when *E_Deficit* is included, which suggests that *E_Deficit* explains a substantial portion of cross-sectional variation in discretionary accruals.

sample of firm-years with *E_Deficit* equal to zero in which firms have little incentive to manage earnings upward to avoid reporting earnings decreases. Note that as we do not include *E_Deficit* as an indendent variable, multi-collinearity is not of an issue. The results reported in the Model 1 column of Table 3.4, Panel B reveal that the coefficient estimate on *D_Deficit* is statistically insignificant and trivial in magnitude. This finding suggests that the insignificant coefficient estimate on *D_Deficit* in the Model 5 column of Table 3.4, Panel A is unlikely to be attributable to overestimated standard errors that can arise from multi-collinearity.

One might suspect that the insignificant coefficient estimate on *D_Deficit* for a sample of firm-year with *E_Deficit* equal to zero is due to the lack of explanatory power associated with *D_Deficit*, because it is often a subset of *E_Deficit*. It is indeed the case that *D_Deficit* is generally smaller than *E_Deficit* in our dataset, because past earnings generally exceed past dividends. But this is not always so. In our dataset, out of 3,281 firm-years with *E_Deficit* equal to zero, we identify 668 firm-years in which prior years' dividend levels are larger than earnings levels. And even using this subsample, we still find that the coefficient on *D_Deficit* is statistically indistinguishable from zero, as reported in the Model 2 column of Table 3.4, Panel B.

Second, but related to the first, firms' propensity to manage earnings to avoid earnings declines that has been documented in prior studies (Burgstahler and Dichev, 1997; Graham et al., 2005) might be due to the firms' incentive to manage earnings to avoid dividend cuts. In other words, the tendency to manage earnings to avoid earnings declines potentially captures firms' propensity to manage earnings to sustain dividend levels. Although earnings and dividends levels are identified as important thresholds, to the best of our knowledge, it is unknown whether the zero earnings growth threshold is the primitive compared to the dividend threshold, or vice versa.

To address this issue, we conduct several tests. We run regressions for a sample of firm-years with *D_Deficit* equal to zero in which firms have little incentive to manage earnings upward to avoid cutting dividends. If firms' tendency to manage earnings to avoid earnings decreases is due to firms' incentive to manage earnings to avoid dividend cuts, we expect *E_Deficit* to be insignificant. The estimation results are reported in the Model 3 column of Table 3.4, Panel B. The coefficient estimate on *E_Deficit* is positive and statistically significant (*t*-statistics = 8.17). This indicates that firms' incentive to manage earnings to avoid earnings declines is not simply firms' incentive to avoid dividend cuts. We also conduct regressions analyses for a sample of payers and non-payers, respectively. If firms' propensity to manage earnings to avoid reporting earnings declines is attributable to firms' tendency to manage earnings to sustain dividend levels, we expect the coefficient estimate on *E_Deficit* to be insignificant for non-payers but significant for payers because non-payers have no incentive to manage earnings upward to avoid cutting dividends. The estimation results presented in the Model 4 and Model 5 columns of Table 3.4, Panel B show that the coefficient estimates on *E_Deficit* are significantly positive for both payers and non-payers. This finding contradicts the notion that firms' tendency to manage earnings in order to avoid earnings declines are primarily due to a binding constraint on dividends.

Taken as a whole, our results reported in Table 3.4 firmly indicate that the significant association between discretionary accruals and *D_Deficit* reported by DDN is not robust to controlling for firms' propensity to manage accruals to ensure that earnings do not drop. More importantly, the findings documented in Table 3.4 suggest that firms do not manage earnings to avoid cutting dividends after controlling for firms' tendency to manage earnings to avoid earnings decreases.

3.3.3 Dividend cut analysis

In contrast to DDN's findings, the results reported in the previous section suggest that firms do not manage earnings to avoid dividend cuts after controlling for firms' propensity to manage earnings in order to avoid earnings declines. In this section, we examine determinants of the decision to cut dividends. If firms do not engage in earnings management to avoid dividend cuts, we expect firms to cut dividend when reported earnings fall short of past dividends. Alternatively, if firms manage earnings to sustain dividend levels as DDN argue, eliminating a shortfall in pre-managed earnings should reduce the probability of dividend cuts. Indeed, DDN find evidence of the latter effect.

We begin by replicating DDN's dividend cut analyses. DDN posit that if firms engage in earnings management to meet or beat dividend thresholds, firms that eliminate a shortfall in pre-managed earnings are less likely to cut dividends. To address this question, DDN estimate a logit model in which an indicator variable that equals one if a firm cuts its dividend is regressed on an indicator variable equal to one if there is a shortfall before discretionary accruals are added (i.e., *D_Deficit* is positive), an indicator variable equal to one if there is a shortfall before but a surplus after discretionary accruals are added, and a set of control variables. The control variables include dividend per share, earnings per share, contemporaneous and lagged annual stock returns, and a cash flow shock measure (the average cash flow deflated by lagged total assets over years *t* and *t-1* minus the average cash flows deflated by lagged total assets over years *t-2* and *t-4*).

DDN find that the coefficient estimates on the two indicator variables are positive and negative, respectively, and both are significantly different from zero. DDN interpret these findings to mean that firms are more likely to cut dividends if their pre-managed earnings fall short of the expected dividend payments, but managing earnings in these cases such that reported earnings exceed expected dividend payments reduce the likelihood of dividend cuts.

We replicate DDN's main analysis (Column 2 of Table 7, p. 21) and display the results in the Model 1 column of Table 3.5. Consistent with DDN, we find that the coefficient on the indicator variable equal to one if there is a shortfall before discretionary accruals are added is significantly positive (coefficient = 1.102; p-value < 0.001), whereas the coefficient on the indicator variable equal to one if there is a shortfall before but surplus after discretionary accruals are added is significantly negative (coefficient = -1.327; *p*-value < 0.001).

DDN does not, however, control for the relation between reported earnings and expected dividend payments. Firms are more (less) likely to cut dividends when reported earnings fall short of expected (exceed) dividends. Thus, it is critical to control for the level of reported earnings to assess whether earnings management affects a likelihood of dividend cuts.

To test our conjecture, we run a logit regression of dividend cuts against an indicator variable equal to one if firms exhibit an earnings shortfall after discretionary accruals are added in addition to all of the variables used by DDN. The results reported in the Model 2 column of Table 3.5 reveal that the coefficient estimate on the new indicator variable is 1.263 with a *p*-value less than 0.001. This suggests that firms tend to cut dividends when reported earnings fall short of past dividend levels. Importantly, the coefficient estimate on an indicator variable that captures earnings management to eliminate a shortfall in pre-managed earnings relative to expected dividend payments (i.e., an indicator variable equal to one if there is a shortfall before but surplus after discretionary accruals are added) becomes statistically insignificant (*p*-value = 0.221). These findings suggest that earnings management that closes the gap between reported earnings and expected dividends has no discernible effect on the dividend cut decision, whereas when reported earnings fall short of expected dividends, firms are more likely to cut dividends.[29]

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[29] DDN also estimate a logit model in which the independent variables include an indicator variable equal to one if there is a surplus both before and after discretionary accruals are added, an indicator variable equal to one if there is a shortfall both before and after discretionary accruals are added, and an indicator variable equal to one if there is a surplus before but shortfall after discretionary accruals are added. The coefficient on the first of these indicator variables is statistically indistinguishable from zero, while the coefficients on the other indicator variables are both positive and statistically different from zero. This suggests that whether the firm has a surplus after discretionary accruals are added (i.e., reported earnings) has a significant impact on the dividend cut decisions, but among firms that have a surplus in reported earnings, it does not seem to matter whether they also had a surplus in pre-managed earnings.

Collectively, the results reported thus far indicate that firms do not manage earnings in order to avoid dividend cuts in response to an increase in dividend constraints. Instead, we find that firms are more likely to cut dividends when reported earnings fall short of expected dividends. While our findings contrast those of DDN, they are consistent with Healy and Palepu's (1990) findings that firms close to dividend covenant violations do not make income-increasing accounting choices but often cut dividends.

3.4 Additional analyses

3.4.1 Sub-sample analyses

DDN conduct several triangulation analyses to examine inter-temporal and cross-sectional variations in the effect of *D_Deficit* on discretionary accruals. They hypothesize that firms are more likely to manage earnings to meet or beat dividend thresholds before the passing of the SOX, after the 2003 dividend tax cut, and when they have positive debt. DDN argue that SOX makes it more difficult and costly for firms to engage in accruals management, thereby decreasing firms' incentives to engage in earnings management to avoid cutting dividends (Cohen et al., 2005). They further argue that the 2003 dividend tax cut makes it more attractive for firms to pay dividends, thereby increasing the incentives to manage earnings to meet or beat dividend thresholds (Chetty and Saez, 2005). DDN provide evidence consistent with their hypotheses. Specifically, they find that the coefficient estimates on *D_Deficit* obtained from the regressions of discretionary accruals on *D_Deficit* and a set of control variables are larger for the pre-

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Finally, DDN estimate a logit model in which the independent variables include an indicator variable equal to one if there is a shortfall after discretionary accruals are added and an indicator variable equal to one if the earnings exceed the previous year's dividends but fall short of the previous year's earnings. The coefficient on the former indicator variable is positive and statistically significant, consistent with our results. The coefficient on the latter indicator variable is statistically indistinguishable from zero. They interpret this to mean that firms do not cut dividends primarily because of reductions in earnings, but rather because they cannot meet the dividend thresholds. Of course, this does not mean that firms manage earnings to meet those thresholds or that doing so affects the dividend cut decision.

SOX period than for the post-SOX period. They also show that the coefficient estimates on *D_Deficit* are larger after than before the 2003 dividend tax cut.[30] In addition, DDN document that the coefficient estimates on *D_Deficit* are positive and statistically significant for firms with debt, but statistically insignificant for firms without debt. They interpret these results as evidence that the regulatory environment (i.e., SOX and 2003 dividend tax cut) and the existence of debt-related dividend constraints affect the extent of earnings management to meet or beat dividend thresholds.[31]

We first replicate DDN's key results by regressing discretionary accruals on *D_Deficit* and a set of control variables for six sub-samples: the pre-SOX period versus the post-SOX period, the pre-2003 dividend tax cut period versus the post-2003 dividend tax cut period, and firm-years with positive debt versus firm-years with zero debt. Following DDN, we define the pre-SOX period as years between 1992 and 2001 and the post-SOX period as 2002. We also designate 2002 as the pre-2003 dividend tax cut period and years between 2003 and 2005 as the post-2003 dividend tax cut period. Consequently, the pre-2003 dividend tax cut period is effectively the same as the post-SOX period.[32] The replication results are presented in Panel A of Table 3.6. For the sake of brevity, we only provide coefficient estimates on *D_Deficit*. Consistent with DDN, we find that the coefficient estimates on *D_Deficit* are greater in magnitude and more significant for the pre-SOX period, the post-2003 dividend tax cut period, and firm-years with positive debt.

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[30] DDN also find that the coefficient estimates on *D_Deficit* are positive and statistically significant for the pre-SOX period (but not for the post-SOX period) and after the 2003 dividend tax cut (but not before).

[31] DDN state that "*tightness* of debt-related covenants has an important influence on the likelihood of the firm engaging in earnings management to meet a dividend threshold" [emphasis added] (p. 18). The choice of tightness is somewhat misleading because firms without debt have no debt covenants which possibly include some form of dividend restriction.

[32] DDN do so "[t]o ensure we isolate the effects of these two regulations," (p. 16.)

Next, we examine how robust these sub-sample results are to controlling for firms' incentive to manage earnings to meet or beat the prior year's earnings. Specifically, we conduct the regressions in which discretionary accruals are regressed on *D_Deficit* and *E_Deficit* along with control variables for the six sub-samples. If, as DDN argue, firms manage accruals to meet or beat dividend thresholds and such propensity varies inter-temporally and cross-sectionally, we expect that even after controlling for *E_Deficit*, the coefficients on *D_Deficit* continue to be greater and more significant for the pre-SOX period, for the pre-2003 dividend tax cut period, and for firm-years with positive debt.

We display the results in Panel B of Table 3.6. The results reveal that the coefficient estimates on *D_Deficit* diminish in magnitude and become statistically insignificant even for the pre-SOX period, the post-2003 dividend tax cut period, and firm-years with positive debt. The coefficient estimates on *D_Deficit* are 0.086 (*p*-value *= 0.447*), 0.004 (*p*-value *= 0.984*), and 0.020 (*p*-value *= 0.834*) for the pre-SOX period, the post-2003 dividend tax cut period, and firm-years with positive debt, respectively.[33] Thus, there is no evidence that firms manage earnings to meet or beat dividend thresholds even in the subsamples where such behavior should be most pronounced. Overall, the results reported in Table 3.6 corroborate our argument that DDN's findings are attributable to the failure to control for firms' incentive to manage earnings to avoid reporting earnings decreases.

3.4.2 Alternate earnings management measures

In our analysis above we use the same measure of discretionary accruals as DDN to facilitate comparison of the results. This measure is based on Jones (1991). But many

[33] The coefficient on *D_Deficit* is greater for pre-regulation period than for post-regulation period (p-value = 0.068). However, the significant difference between two subsamples appears to be attributable to a negative coefficient on *D_Deficit* for post-regulation period. Because the predicted sign on *D_Deficit* is positive, it is impossible to interpret this difference as suggesting that firms' incentive to manage earnings to avoid cutting dividends changes from pre- to post-regulation period.

refinements have subsequently been developed to improve the precision of the estimate and to remove various sources of bias, including those of Dechow et al. (1995), Kothari et al. (2005), and Collins et al. (2011). To test the robustness of our results, we rerun the regressions from Table 3.4 with alternate discretionary accrual measures.

Table 3.7 presents the major coefficients from regressions using discretionary accruals based on the Jones (1991), just like Table 3.4, as well as Dechow et al. (1995), Kothari et al. (2005), and Collins et al. (2011), respectively. The results for dividend payers are qualitatively the same across the measures. That is, for all measures, the coefficient on *D_Deficit* is positive and statistically significant for dividend payers when *E_Deficit* is excluded, but it becomes statistically indistinguishable from zero when *E_Deficit* is included as a control variable. Thus, our primary conclusion is robust to the choice of discretionary accrual measure.[34]

3.4.3 Measuring earnings managements based on earnings restatements

A concern with the previous analyses is that the approach of 'backing-out' pre-managed earnings gives rise to a mechanical association between discretionary accruals and earnings deficit (*E_Deficit*).[35] For example, Elgers et al. (2002) argue that the 'backing-out' approach is not capable of testing the anticipatory income smoothing

[34] For comparison purposes, we also ran the same regressions for non-payers. The coefficient on *D_Deficit* for non-payers is positive and statistically different from zero when *E_Deficit* is excluded from the regressions, irrespective of the discretionary accrual measure. Naturally, this cannot be attributable to DDN's conjecture that firms manage earnings to meet dividend thresholds, because non-payers have no such incentives. The positive coefficient on *D_Deficit* vanishes when *E_Deficit* is included as a control variable. But, surprisingly, it turns negative and statistically different from zero when we use the measures in Jones (1991) or Dechow et al. (1995). This puzzling result is not robust, however, as the same coefficient is statistically indistinguishable from zero when we use the measures in Kothari et al. (2005), and Collins et al. (2011).

[35] "The backing-out" approach refers to a research design that estimates pre-managed earnings from running regressions of total accruals on a set of determinants. The fitted values of these regressions serve as a proxy for pre-managed earnings, whereas the residuals (i.e., discretionary accruals) serve as a proxy for earnings management.

hypothesis that firms manage earnings upward (downward) in anticipation of future earnings.

To mitigate this concern, we repeat our main analyses by using a sample of firms that restated originally reported earnings. A sample of restatement firms are obtained from the General Accounting Office (GAO) report (GAO-38-138) (Badertscher et al. 2012).[36] For the restatement sample, we re-define *E_Deficit* as Max (0, the previous year's earnings – restated earnings) and *D_Deficit* as Max (0, the previous year's dividends – restated earnings). We also construct a proxy for earnings management as the difference between originally reported earnings and restated earnings. Note that both the dependent (managed earnings = originally reported earnings – restated earnings) and independent variables (*D_Deficit* and *E_Deficit*) are measured without errors for the restatement sample. Consequently, there should not be a spurious positive association between a proxy for earnings management (i.e., originally reported earnings minus restated earnings) and *E_Deficit* (i.e., the extent to which restated earnings fall short of the previous year's earnings) for this sample.

The estimation results for a sample of 295 restatement firm-years presented in Table 3.8 support our earlier results that firms manage earnings to avoid reporting decreases in earnings, but not to avoid dividend cuts. Model 1 of Table 3.8 shows the results from replicating DDL for the restatement sample. The coefficient on *D_Deficit* is positive and statistically different from zero (β_1 = 0.236; *t*-statistic = 1.70). However, as shown in Model 2, the coefficient on *D_Deficit* becomes statistically insignificant when *E_Deficit* is included, whereas the coefficient on *E_Deficit* is positive and statistically different from zero (β_1 = 0.149; *t*-statistic = 3.36). The results are similar when we restrict our analyses to dividend payers, as reported in Model 3 and Model 4. Model 4 reveals that the coefficient on *E_Deficit* is significantly positive (β_1 = 0.117; *t*-statistic = 3.54),

[36] We thank Brad Badertscher for sharing his restatement data.

even though the sample size shrinks substantially from 295 to 98. In sum, the restatement sample results corroborate our earlier results and interpretations.

3.4.4 Frequency distribution of earnings less dividends

DDN also examine the frequency distribution of EPS (earnings per share) minus DPS (dividends per share) for the sample of dividend payers. Their distribution ranges from -$1 to $3 and is based on 2 cent bin sizes. If firms frequently manage earnings to meet dividend thresholds, the distribution should reveal an unusually high frequency of observations immediately to the right of zero. Burgstahler and Dichev (1997) use a similar methodology to show a tendency to manage earnings upward if they otherwise would be slightly negative or if they otherwise would fall short of the previous years' earnings.

DDN report that the bin immediately to the right of zero contains 52 observations, which is about twice the average number of observations in adjacent bins of 25 observations. We replicate this in part a of Figure 1, and find similar numbers. The excess of about 27 observations is not very large (it represents only 0.3% of the total observations), but it is nevertheless statistically different from zero at the 0.01 level. The frequency distribution reveals no other pattern of earnings management to meet dividend thresholds.

In other tests of earnings management to meet dividend thresholds, DDN use the previous year's dividends as a proxy for expected dividends. But in their analysis of the frequency distribution they use realized dividends for the current year. As a result, any abnormality in frequency distribution could be attributable to a tendency to manage earnings relative to expected dividends or a tendency to set the dividends relative to expected earnings. An example of the latter would be that managers decide to hold back on a dividend increase in a period due to expectations of poor earnings. To remove the tendency to set dividends relative to expected earnings from the frequency distribution, we use the lagged value of DPS instead. That is, we form the distribution for the difference between EPS in the current year and DPS in the previous year. Our revised distribution is given in part b of Figure 1. The number of observations in the bin immediately to the right of zero is no longer abnormally high. In fact, there is no clear pattern in any of the bins around zero that would suggest earnings management to meet dividend thresholds. In parts c and d of Figure 1, we replicate the distribution in part b for the subsamples of firms-years before SOX and firm-years with positive debt, respectively. Any earnings management should be more pronounced for these subsamples, but there is no clear pattern suggestive of earnings management even in those distributions. Thus, the abnormal pattern in the frequency distribution reported in DDN seems to be due to a tendency to set dividends relative to expected earnings rather than earnings management to meet dividend thresholds.

3.5 Conclusions

DDN hypothesize that firms manage earnings upward when pre-managed earnings fall short of expected dividends, perhaps because of debt covenants that constrain dividend payments, and that such earnings management help preserve the dividend level. These hypotheses seem reasonable given past evidence of opportunistic earnings management and a resistance to cutting dividends. DDN report results that they interpret as consistent with their hypotheses. In this study, we question the interpretation of the results in DDN. In particular, we conjecture that their multivariate specifications omit variables that are correlated with the independent variables of interest. This omission induces bias and makes the interpretation of their results difficult.

DDN regress discretionary accruals against the dividend deficit, *D_Deficit*, defined as Max (0, expected dividends – pre-managed earnings), and control variables. They find a positive coefficient on *D_Deficit* for firms that pay dividends, and coefficient is larger before the passing of the Sarbanes-Oxley Act (SOX), after the 2003 dividend tax cut, and for firms with positive debt. We find, however, that the coefficient on *D_Deficit* for firms that pay dividends become insignificant when we control for firm's tendency to manage earnings to avoid declines in reported earnings relative to those in the prior year. This holds even for the subsamples in which DDN argue the tendency to manage earnings to eliminate shortfalls in the dividend deficit is most pronounced. Moreover, our results are robust to alternative earnings management measures, including a measure based on ex post restatements. As an alternative approach, DDN report an abnormality in the frequency distribution of earnings less dividends in support of their view, but we present evidence that the abnormality is attributable to measurement error.

DDN further regress the dividend cut on an indicator variable for whether firms eliminate the dividend deficit and control variables. They find a positive coefficient on the indicator variable, which they interpret as evidence that managing earnings upward to close a dividend deficit helps preserve the dividend level. However, we find that this coefficient becomes insignificant when we control for whether the reported earnings fall short of expected dividends. Importantly, we find that an indicator variable equals one if reported earnings fall short of expected dividends is positively associated with dividend cuts. These findings suggest that the level of earnings relative to dividends matter for the decision to cut dividends, but any earnings management designed to close a shortfall in earnings relative to dividends has no significant impact.

Our study resolves some inconsistencies in the past literature. Numerous studies dating back to Watts and Zimmerman (1986) conjecture that when firms are close to violating debt covenant violations, they are likely to engage in earnings management. However, Healy and Palepu (1990) and DeFond and Jiambalvo (1994) argue that debt covenants covering dividend payments are different, because the firms can simply cut the dividends to comply, and their evidence supports this argument. More recently, DDN present large sample evidence that challenges both the arguments and empirical evidence in Healy and Palepu (1990) and DeFond and Jiambalvo (1994), and leaves the literature in limbo. This study shows that the results in DDN are due to methodological errors. Once we correct the errors, we return to the earlier conclusion in the literature that firms do not materially manage earnings to dodge dividend restrictions, but rather cut the dividends.

Table 3.1 Deficit to the expected dividends and discretionary accruals

Panel A Frequency of firm-years with positive and non-positive discretionary accruals

	Discretionary accruals ≤ 0	Discretionary accruals > 0	Total
Firm-years with D_Deficit = 0	3,061	2,631	5,692
	54%	46%	100%
Firm-years with D_Deficit > 0	355	1,724	2,079
	17%	83%	100%
Total	3,416	4,355	7,771
	44%	56%	100%

Panel B Discretionary accruals across quintiles formed on positive D_Deficit and a sample of firm-years with D_Deficit = 0

	N	Average D_Deficit	Average discretionary accruals
D_Deficit = 0	5,692	0	-25.1
All firm-years with D_Deficit > 0	2,079	141.5	137.6
Low = 1	415	4.4	16.8
2	416	17.1	28.7
3	416	43.9	65.5
4	416	124.0	111.9
High = 5	416	518.0	464.8

Note: Panel A presents frequency of observations that have either non-positive or positive discretionary accruals for a sample of firm-years with *D_Deficit* = 0 and *D_Deficit* > 0, respectively. Panel B presents average *D_Deficit* and average discretionary accruals across quintile portfolios formed on positive *D_Deficit* and a set of firm-years with *D_Deficit* = 0. *D_Deficit* is measured as Max (0, Expected dividends – Pre-managed earnings).

Table 3.2 Deficit to the previous year's earnings and discretionary accruals

Panel A Frequency of firm-years with positive and non-positive discretionary accruals

	Discretionary accruals ≤ 0	Discretionary accruals > 0	Total
Firm-years with E_Deficit = 0	2,748	1,130	3,878
	71%	29%	100%
Firm-years with E_Deficit > 0	669	3,229	3,898
	17%	83%	100%
Total	3,417	4,359	7,776
	44%	56%	100%

Panel B Discretionary accruals across quintiles formed on positive E_Deficit and a sample of firm-years with E_Deficit = 0

	N	Average E_Deficit	Average discretionary accruals
E_Deficit = 0	3,878	0	-70.6
All firm-years with E_Deficit > 0	3,893	149.9	107.1
Low = 1	778	4.9	8.0
2	779	18.6	15.9
3	779	45.5	38.3
4	779	115.5	85.0
High = 5	779	565.2	388.5

Note: Panel A presents frequency of observations that have either non-positive or positive discretionary accruals for a sample of firm-years with *E_Deficit* = 0 and *E_Deficit* > 0, respectively. Panel B presents average *E_Deficit* and average discretionary accruals across quintile portfolios formed on positive *E_Deficit* and a set of firm-years with *E_Deficit* = 0. *E_Deficit* is measured as Max (0, Prior year's earnings – Pre-managed earnings).

Table 3.3 The relation between D_Deficit and E_Deficit

Panel A Correlation between D_Deficit and E_Deficit

	Total	Payers	Non-payers
Pearson	0.70	0.73	0.63
Spearman	0.51	0.55	0.50

Panel B Distribution of firm-years across D_Deficit and E_Deficit quintile combinations

	E_Deficit	Low				High
D_Deficit		1	2	3	4	5
Low	1	213	114	24	8	4
	2	99	146	90	25	4
	3	33	73	175	74	8
	4	17	24	61	202	60
High	5	1	7	13	55	287

Note: Panel A presents Pearson and Spearman correlations between *D_Deficit* and *E_Deficit*. Panel B presents distributions of firm-years across each *D_Deficit* and *E_Deficit* quintile combinations. *D_Deficit* is measured as Max (0, Expected dividends – Pre-managed earnings). *E_Deficit* is measured as Max (0, Prior year's earnings – Pre-managed earnings).

Table 3.4 OLS regression of discretionary accruals on D_Deficit, E_Deficit, and control variables

Panel A Discretionary Accruals, D_Deficit, and E_Deficit

	Model 1	Model 2	Model 3	Model 4	Model 5
	Model 2 of Table 3 in DDN	Replication	Include E_Deficit	Replication only for payers	Include E_Deficit
D_Deficit for payers	**0.937*** **	**0.958*** **	**0.131* **	**0.809*** **	**-0.009**
	[8.4]	***[13.78]***	***[1.76]***	***[9.15]***	***[-0.09]***
D_Deficit for non-payers	-0.138	0.105	-0.501***		
	[0.9]	*[0.84]*	*[-4.32]*		
Payer	26.372***	10.891*	17.719***		
	[3.9]	*[1.92]*	*[3.45]*		
Retained earnings $_{t-1}$	-0.013**	0.002	-0.011**	-0.003	-0.014**
	[2.1]	*[0.45]*	*[-2.20]*	*[-0.43]*	*[-2.18]*
Delta $_{t-1}$	-0.015**	-0.004	-0.004	0.005	0.001
	[2.4]	*[-1.00]*	*[-1.24]*	*[0.68]*	*[0.15]*
Vega $_{t-1}$	-0.139**	0.028	0.014	0.091	0.051
	[2.1]	*[0.66]*	*[0.40]*	*[1.36]*	*[1.01]*
Cash compensation $_{t-1}$	0.040***	0.029***	0.016***	0.036***	0.020**
	[4.3]	*[4.38]*	*[2.79]*	*[3.39]*	*[2.33]*
Firm size $_{t-1}$	-27.135***	-28.330***	-31.370***	-31.350***	-31.160***
	[5.7]	*[-7.05]*	*[-8.06]*	*[-4.36]*	*[-5.42]*
Leverage $_{t-1}$	0.331	15.480	31.538	-17.372	5.542
	[0.0]	*[0.70]*	*[1.56]*	*[-0.29]*	*[0.11]*

Table 3.4 Continued

Market-to-book $_{t-1}$	1.317	-2.543	-0.438	-3.198	3.146
	[0.7]	*[-1.19]*	*[-1.25]*	*[-0.56]*	*[0.67]*
E_Deficit			**0.779***		**0.764***
			[20.75]		***[11.25]***
Observations	13,425	12,701	12,701	6,176	6,176
R^2	0.143	0.150	0.264	0.132	0.274

Panel B Further Evidence on Discretionary Accruals, D_Deficit, and E_Deficit

	Model 1	Model 2	Model 3	Model 4	Model 5
	Firm-years with E_Deficit = 0	Firm-years E_Deficit = 0 and past dividends > past earnings	Firm-years with D_Deficit =0	Non-payers	Payers
D_Deficit for payers	**0.012**	**-0.060**			
	[0.06]	***[-0.30]***			
Retained earnings$_{t-1}$	-0.018**	0.000	-0.016**	-0.014	-0.014**
	[-2.27]	*[0.03]*	*[-2.18]*	*[-1.30]*	*[-2.16]*
Delta $_{t-1}$	0.005	-1.014	0.005	-0.007	0.001
	[0.81]	*[-1.03]*	*[0.99]*	*[-1.64]*	*[0.16]*
Vega $_{t-1}$	0.043	0.095	0.026	0.027	0.051
	[0.78]	*[0.57]*	*[0.52]*	*[0.75]*	*[1.01]*

Table 3.4 Continued

Cash compensation $_{t-1}$	0.023**	0.063**	0.026***	0.013*	0.020**
	[2.24]	*[2.02]*	*[3.05]*	*[1.86]*	*[2.29]*
Firm size $_{t-1}$	-46.377***	-56.108***	-38.390***	-27.262***	-31.313***
	[-5.97]	*[-3.21]*	*[-5.68]*	*[-6.08]*	*[-5.04]*
Leverage $_{t-1}$	-20.776	-23.714	-13.430	23.632	-5.449
	[-0.30]	*[-0.26]*	*[-0.25]*	*[1.63]*	*[0.11]*
Market-to-book $_{t-1}$	3.019	1.890	4.116	-3.508*	3.171
	[0.51]	*[0.12]*	*[0.87]*	*[-1.85]*	*[0.67]*
E_Deficit			**0.832*** **	**0.560*** **	**0.761*** **
			[8.17]	***[8.82]***	***[16.26]***
Observations	3,281	668	4,727	5,703	6,176
R^2	0.158	0.162	0.192	0.134	0.274

Note: The table presents the results of regressions of discretionary accruals on *D_Deficit*, *E_Deficit*, and a set of control variables. *D_Deficit* is measured as Max (0, Expected dividends – Pre-managed earnings). *E_Deficit* is measured as Max (0, Prior year's earnings – Pre-managed earnings). Payer is an indicator variable that is equal to one if a firm pays dividends in year t-1, and zero otherwise. Delta is the expected dollar change in CEO wealth for a 1% change in stock price, and Vega is the expected dollar change in CEO wealth for a 0.01 change in stock return volatility (Core and Guay (2002)). Cash compensations equals to salary plus bonus. Firm size is the log of total assets. Leverage is the ratio of short-term plus long-term debt to total assets. Market-to-book is the ratio of the market value of equity to the book value of equity. *t*-statistics are reported in brackets (As in Daniel et al. (2008), unsigned *t*-statistics are reported in brackets in Model 1). ***,**,* indicate significance at the 0.01, 0.05, and 0.10 levels, respectively (two-tailed).

Table 3.5 Logit regressions of dividend cuts

	Model 1	Model 2
DPS_{t-1}	0.877*** *[0.00]*	0.753*** *[0.00]*
EPS_t	-0.276*** *[0.00]*	-0.158** *[0.01]*
Stock $return_t$	0.263 *[0.30]*	0.350 *[0.15]*
Stock $return_{t-1}$	0.024 *[0.92]*	0.122 *[0.61]*
Cash flow $shock_t$	-1.114 *[0.42]*	-0.831 *[0.54]*
Dummy = 1 if *shortfall before* discretionary accruals are added	1.102*** *[0.00]*	0.413* *[0.08]*
Dummy = 1 if *shortfall before* but *surplus after* discretionary accruals are added	**-1.327*** *[0.00]***	**-0.399 *[0.22]***
Dummy = 1 if *surplus after* discretionary accruals are added		
Dummy = 1 if *shortfall after* discretionary accruals are added		**1.263*** *[0.00]***
Observations	6,182	6,182
Pseudo R^2	0.164	0.175

Note: The table presents the results of logit regressions of dividend cuts on variables measuring whether firms meet dividend thresholds before and after discretionary accruals are added for all firm-years in which firms pay dividends in year t-1. A firm-year is classified as having a shortfall before discretionary accruals are added if dividends paid at t-1 > Pre-managed Earnings at t. A firm-year is classified as having a surplus after discretionary accruals are added if dividends paid at t-1 < Discretionary accruals + Pre-managed Earnings at t. DPS is dividend per share. EPS is earnings per share. Stock returns are stock returns corresponding to the fiscal year. Cash flow shock is the average cash flow deflated by lagged total assets during years t and t-1 minus the average cash flows deflated by lagged total assets during years t-4 through t-2. *p*-values are given in brackets. ***,**,* indicate significance at the 0.01, 0.05, and 0.10 levels (two-tailed), respectively.

Table 3.6 Inter-temporal and cross-sectional associations between discretionary accruals and D_Deficit

Panel A: Coefficients on D_Deficit from the regression without E_Deficit

	Post-regulation [positive debt]	Pre-regulation [zero debt]	Difference
Sarbanes-Oxley	0.488*	0.928***	-0.440**
	[0.06]	*[0.00]*	*[0.05]*
Tax reform act	0.695***	0.488*	0.207
	[0.00]	*[0.06]*	*[0.26]*
Debt (t)	0.827***	0.222	0.605***
	[0.00]	*[0.25]*	*[0.00]*

Panel B: Coefficients on D_Deficit from the regression with E_Deficit

	Post-regulation [positive debt]	Pre-regulation [zero debt]	Difference
Sarbanes-Oxley	-0.260	0.086	-0.346*
	[0.20]	*[0.45]*	*[0.07]*
Tax reform act	0.004	-0.260	0.264
	[0.98]	*[0.20]*	*[0.19]*
Debt (t)	0.020	0.067	-0.047
	[0.83]	*[0.63]*	*[0.61]*

Note: Panel A presents coefficient estimates on *D_Deficit* from the regressions of discretionary accruals on *D_Deficit* along with a set of control variables for a sample of firm-years in which firms pay dividends in year t-1. Panel B presents coefficient estimates on *D_Deficit* from the regressions of discretionary accruals on *D_Deficit*, *E_Deficit*, and a set of control variables for the same sample. The pre-SOX period corresponds to years between 1992 and 2001. The post-SOX period corresponds to 2002. The pre-Tax reform act corresponds to 2002. The post-Tax reform act corresponds to years between 2003 and 2005. (Positive) Zero debt refers to firm-years in which a firm has (positive) no short-term and long-term debt. *D_Deficit* is measured as Max (0, Expected dividends – Pre-managed earnings). *E_Deficit* is measured as Max (0, Prior year's earnings – Pre-managed earnings). *p*-values are given in brackets. *p*-values for the *Difference* column are based in one-tailed tests. ***,**,* indicate significance at the 0.01, 0.05, and 0.10 levels (one-tailed for the *Difference* column and two-tailed in the other columns), respectively.

Table 3.7 Alternative measures of discretionary accruals

	Jones (1991)		Dechow et al. (1995)		Kothari et al. (2005)		Collins et al. (2011)	
D_Deficit	0.809***	-0.009	0.716***	-0.053	1.143***	0.085	0.921***	-0.076
	[9.15]	*[-0.09]*	*[7.59]*	*[0.49]*	*[17.50]*	*[0.84]*	*[9.09]*	*[0.65]*
E_Deficit		0.764***		0.761***		0.969***		1.098***
		[11.25]		*[10.30]*		*[11.57]*		*[13.57]*
Obs.	6,176	6,176	6,134	6,134	6,164	6,164	2,437	2,437
R^2	0.132	0.274	0.117	0.263	0.287	0.429	0.242	0.408

Note: The table presents the results of regressions of alternative measures of discretionary accruals on *D_Deficit*, *E_Deficit,* and a set of control variables for a sample of firm-years in which firms pay dividends in year t-1. For the sake of brevity, we only display coefficient estimates on *D_Deficit* and *E_Deficit*. In the Jones (1991) column, discretionary accruals are estimated by regressing total accruals on changes in revenues and property, plant, and equipment. In the Dechow et al. (1995) column, discretionary accruals are estimated by subtracting changes in receivables from the Jones (1991) discretionary accruals. In the Kothari et al. (2005), discretionary accruals are estimated by subtracting discretionary accruals of a ROA-matched firm from the Jones (1991) discretionary accruals. In the Collins et al. (2011) column, discretionary accruals are estimated by subtracting discretionary accruals of a ROA- and Sales Growth-matched firm from the Jones (1991) discretionary accruals. *D_Deficit* is measured as Max (0, Expected dividends – Pre-managed earnings). *E_Deficit* is measured as Max (0, Prior year's earnings – Pre-managed earnings). *t*-statistics are reported in brackets. ***,**,* indicate significance at the 0.01, 0.05, and 0.10 levels, respectively (two-tailed).

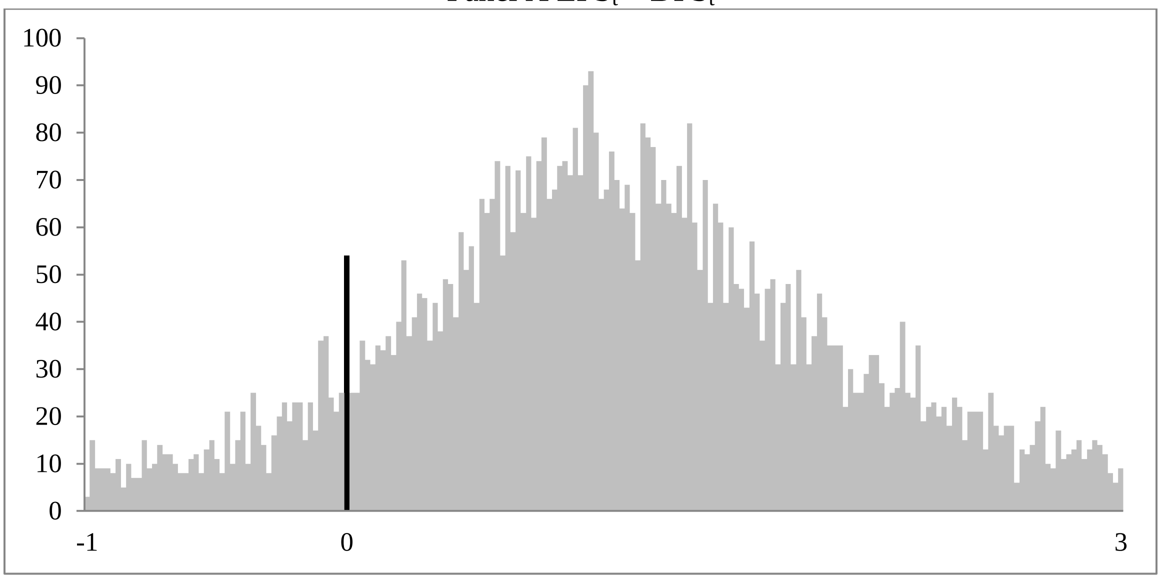
Table 3.8 Restatement firms

	Model 1	Model 2	Model 3	Model 4
	Both payers and non-payers		Only payers	
D_Deficit for payers	**0.236***	**0.122**	**0.241**	**0.155**
	[1.70]	***[1.11]***	***[1.33]***	***[0.97]***
D_Deficit for non-payers	0.430	0.330		
	[1.47]	*[1.23]*		
Payer	5.896	8.162		
	[0.70]	*[1.03]*		
Retained earnings$_{t-1}$	0.002	-0.001	0.004	-0.002
	[0.66]	*[-0.56]*	*[0.61]*	*[-0.29]*
Firm size $_{t-1}$	0.140	-1.320	-0.698	-1.709
	[0.07]	*[-0.67]*	*[-0.15]*	*[-0.36]*
Leverage $_{t-1}$	12.375	13.757	105.149*	97.032
	[1.30]	*[1.19]*	*[1.81]*	*[1.66]*
Market-to-book $_{t-1}$	-0.048	-0.087	2.130	0.995
	[-0.29]	*[-0.53]*	*[1.28]*	*[0.50]*
E_Deficit		**0.149*****		**0.117*****
		[3.36]		***[3.54]***
Observations	295	295	98	98
R^2	0.432	0.478	0.545	0.575

Note: The table presents the results of regressions of the difference between originally reported earnings and restated earnings on *D_Deficit*, *E_Deficit*, and a set of control variables for firms that restate their originally reported earnings. *D_Deficit* is measured as Max (0, Expected dividends – Restated Earnings). *E_Deficit* is measured as Max (0, Prior year's earnings – Restated Earnings). Payer is an indicator variable that is equal to one if a firm pays dividends in year t-1, and zero otherwise. Firm size is the log of total assets. Leverage is the ratio of short-term plus long-term debt to total assets. Market-to-book is the ratio of the market value of equity to the book value of equity. *t*-statistics are reported in brackets. ***,**,* indicate significance at the 0.01, 0.05, and 0.10 levels, respectively (two-tailed).

Figure 3.1 Frequency distribution of EPS and DPS

Panel A $EPS_t - DPS_t$



Panel B $EPS_t - DPS_{t-1}$

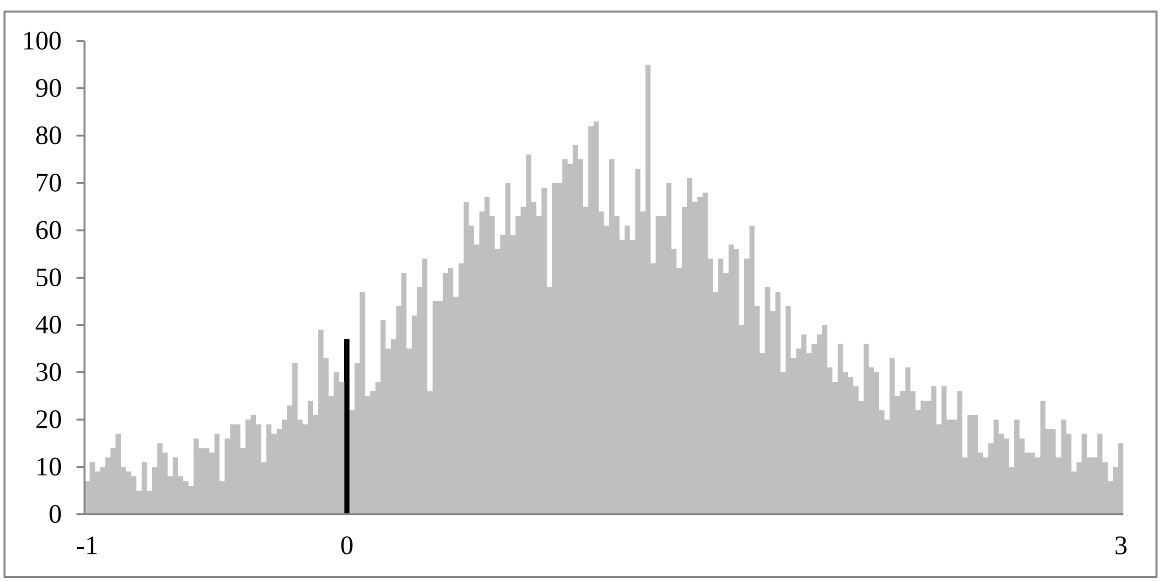
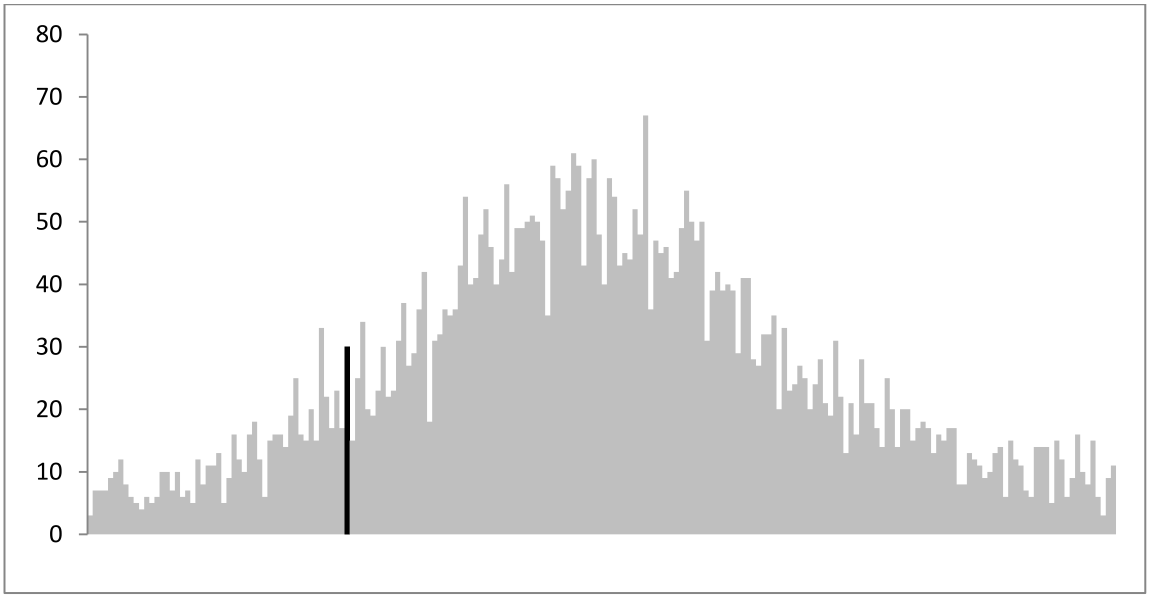


Figure 3.1 Continued

Panel C $EPS_t - DPS_{t-1}$ (Firms in the pre-SOX period)



Panel D $EPS_t - DPS_{t-1}$ (Firms with positive debt)

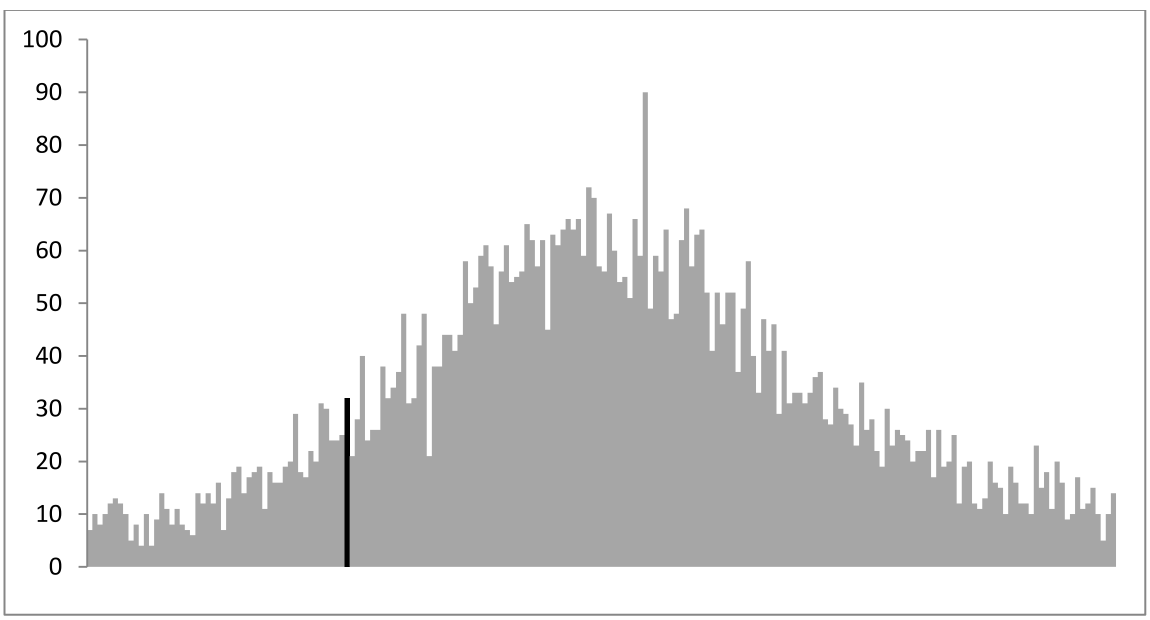


Figure 3.1 Continued

Note: Frequency distribution of EPS (income before extraordinary items divided by number of shares) minus DPS (dividends divided by number of shares) from -$1 to $3 using 2 cent bin sizes. The column for the bin size from 0 to 2 cents is black.

APPENDIX A VARIABLE DEFINITIONS

$(Cross-border\ M\&A\ pair)_{i,j,t}$: The number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the sum of the number of domestic M&A deals in country *j* and the number of cross-border M&A deals involving acquirer country *i* and target country *j* in year *t.* Source: SDC.

CBMA-1: The number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the average number of cross-border deals in years *t-2* and *t-1* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$). Source: SDC.

CBMA-2: The number of cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the average number of cross-border deals in years *t-3* and *t-1* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$). Source: SDC.

CBMA value: Aggregate dollar value of all cross-border deals in year *t* in which the acquirer is from country *i* and the target is from country *j* ($i \neq j$) divided by the sum of aggregate dollar value of all domestic M&A deals in country *j* and aggregate dollar value of all cross-border deals involving acquirer country *i* and target country *j* in year *t*. Source: SDC.

$CAR(-3,+3)$: Cumulative abnormal returns over seven days around the merger announcement: stock returns minus returns predicted by a market model, over the systematic seven-day event window around the announcement date. Abnormal returns are estimated using a two-factor model with the equity market index for each country and the MSCI World index (Griffin, 2002). The two-factor model is estimated over days -280 through -30, where day 0 is the cross-border deal announcement day. Source: CRSP (US) and Datastream (non-US).

$\$CAR(-3,+3)$: Cumulative abnormal dollar returns over seven days around the merger announcement. The sum of daily abnormal dollar returns over the seven-day window, in which a daily abnormal dollar return is an abnormal percentage return multiplied by the firm's market equity in the previous day. Source: CRSP (US) and Datastream (non-US).

$\Delta\$CAR\_1$: Target $CAR(-3,+3) minus acquirer $CAR(-3,+3), all scaled by the weighted average of acquirer and target's cumulative abnormal dollar returns ($CAR(-3,+3)), in which the weights are based on acquirer and target market values (in US dollars) 50 trading days prior to the deal announcement. Source: CRSP (US) and Datastream (non-US).

$\Delta\$CAR\_2$: Target $CAR(-3,+3) minus acquirer $CAR(-3,+3), all scaled by the sum of acquirer and target market values (in US dollars) 50 trading days prior to the deal announcement. Source: CRSP (US) and Datastream (non-US).

All-cash offer: Dummy variable set equal to one if a cross-border deal is entirely paid in cash, and zero otherwise.

$Election_{j,t}$: Dummy variable set equal to one if a national election is held in target country j between 90 days prior to the end of year t and 274 days after the end of year t (October 1st of the current year and September 30th of the following year), and zero otherwise.

$Pre-election_t$: Dummy variable set equal to a one during the year before the election year and zero otherwise.

$Post-election_t$: Dummy variable set equal to a one during the year after the election year and zero otherwise.

$Election\ year\ deal_{n,t}$: Dummy variable which equals one if there is a national election in the target's country within one year after the announcement of the deal and zero otherwise.

Civil-law: Dummy variable set equal to one if the target country's legal systems are based on civil law. Source: La Porta et al. (1998).

Close election: Dummy variable set equal to one if the difference between the percentage of votes obtained by the winner and the runner-up is in the first tercile of the entire population, and zero otherwise.

Flexible timing: If the ruling party is able to call an early election prior to the regularly scheduled election date, a given country is classified as having a flexible election timing schedule. Otherwise, I classify a country as having a fixed election timing schedule. Source: Julio and Yook (2012).

Left party: Dummy set equal to one if a country's ruling party is defined as communist, socialist, social democratic, or left-wing according to the World Bank Database of Political Institutions, and zero otherwise.

National preference affinity: Affinity score between acquirer and target countries (*s2un*), based on roll-call votes data from the United Nations General Assembly. Source: Erik Voeten Dataverse.

$\Delta Exchange\ rate_{ij}$: Difference between acquirer (*i*) and target (*j*) countries in the annual real bilateral $U.S. exchange rate changes between year *t*-1 and *t*-2 expressed in percentage points. Source: Datastream and The Penn World Table.

ΔPPP_{ij}: Difference in the two countries' logarithm of annual gross domestic product (GDP) per capita (in U.S. dollars) lagged by 1 year. Source: Worldbank.

$\Delta GDP\ growth_{ij}$: Difference in the two countries' annual real growth rate of the GDP expressed in percentage points lagged by 1 year. Source: Worldbank.

$\Delta Stock\ index\ returns_{ij}$: Difference in the two countries' annual local stock market returns expressed in percentage points lagged by 1 year. Source: Datastream.

$\Delta Financial\ development_{ij}$: Difference between acquirer (*i*) and target (*j*) countries in the ratio of stock market capitalization to GDP lagged by 1 year. Source: Worldbank.

$Bilateral\ trade_{ij}$: Natural logarithm of the dollar volume of all trade flow (based on the Harmonized System classification) from an acquirer country (*i*) to a target country (*j*) divided by total imports of target country (*j*). Source: UN commodity trade database.

Trade openness: Sum of exports and imports scaled by GDP. Source: Penn World Tables 7.1.

Same legal origin: Dummy set equal to one if an acquirer country and a target country have the same legal origin, and zero otherwise. Source: La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998).

Acquirer (target) legal origin: Dummy set equal to one if the legal origin of an acquirer (target) country is French civil law, and zero otherwise. Source: La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998).

Share border: Dummy set equal to one if acquirer and target countries share the same national border, and zero otherwise. Source: CIA World Factbook 2013.

Same language: Dummy set equal to one if acquirer and target countries' primary language are identical, and zero otherwise. Source: CIA World Factbook 2013.

Same religion: Dummy set equal to one if acquirer and target countries' primary religion are identical, and zero otherwise. Source: CIA World Factbook 2013.

Geographical distance: Natural logarithm of the great circle distance between acquirer and target countries. Source: www.mapsofworld.com.

Firm size: Logarithm of market capitalization in US dollars (WC08001). Source: Compustat (US) and Datastream (non-US).

M/B: Ratio of market value of equity (WC08001) to book value of equity (03501). Source: Compustat (US) and Datastream (non-US).

Leverage: Ratio of total debt (WC03255) to book value of assets (WC02999). Source: Compustat (US) and Datastream (non-US).

Cash: Ratio of cash and short term investments (WC02001) to book value of assets (WC02999). Source: Compustat (US) and Datastream (non-US).

Acquirer market capitalization: Logarithm of the acquirer's market capitalization 11 days before the announcement date. Source: CRSP (US) and Datastream (non-US).

Acquirer stock run-up: Acquirer buy-and-hold-abnormal return from 210 days before to 11 days before the announcement date. Source: CRSP (US) and Datastream (non-US).

Public target: Dummy set equal to one if the target is a public firm and zero otherwise. Source: SDC.

Private target: Dummy set equal to one if the target is a private firm and zero otherwise. Source: SDC.

All cash deal: Dummy set equal to one if the deal is financed with cash only and zero otherwise. Source: SDC.

Diversifying acquisition: Dummy set equal to one if the acquirer and target are from a different industry (two-digit SIC) and zero otherwise. Source: SDC.

Tender offer: Dummy set equal to one if the deal is a tender offer and zero otherwise. Source: SDC.

Deal value: Logarithm of reported U.S. dollar value of deal. Source: SDC.

Relative size: U.S. dollar value of deal divided by the acquirer's market capitalization 11 days before the announcement date. Source: market capitalization data from CRSP (US) and Datastream (non-US) and dollar value of deal from SDC.

Target termination fee: Dummy set equal to one if a target termination fee deal is positive, and zero otherwise. Source: SDC.

High (low) Input complexity industry: If a given industry's input-complexity is above the average (0.841), the industry is classified as a high input-complexity industry. Otherwise, I classify it as a low input-complexity industry. Source: Boutchkova, Doshi, Durnev, and Molchanov (2012).

High (low) Labor intensity: If a given industry's labor-intensity is above the average (0.275), the industry is classified as a high labor-intensity industry. Otherwise, I classify it as a low labor-intensity industry. Source: Boutchkova, Doshi, Durnev, and Molchanov (2012).

High (low) exposure to government spending: If a given industry's input-output (I-O) code is 336414, 336611, 515100, 541700, 335110, 211000, 511110, 334418, 334220, or 322120, then the industry is classified as having high exposure to government spending. Otherwise, I classify it as having low exposure to government spending. Source: Belo, Gala, and Li (2013).

CEO age: CEO's age during the sample year (from ExecuComp)

DEL: A dummy equal to one if the firm is incorporated in Delaware, in which state incorporation data comes from RiskMetrics. For firms missing state of incorporation data on RiskMetrics, we use Compustat state of incorporation data

AFT: A dummy equal to one if the firm-year observation is during the period 1996-2000

YOUNG: A dummy variable equal to one if the CEO is 50 years old or younger as of the sample year

Firm age: The sample year minus the year in which the firm was first listed on CRSP

Total risk: The natural logarithm of the annualized variance of the firm's daily stock returns during the fiscal year

Systematic risk: The natural logarithm of the annualized variance of the predicted component of stock returns using the expanded market model as described in Section 2.2

Unsystematic risk: The natural logarithm of the annualized variance of the residual returns from the market model

Size: The natural logarithm of total assets (AT) measured in the prior fiscal year end

MB: The ratio of the market value (AT – CEQ + PRCC_F*CSHO) to book value of assets (AT) in the prior fiscal year end

ROA: The ratio of net income (IB) to total assets (AT) in the prior fiscal year end

CEO delta: The sensitivity of CEO's stock and option value to a 1% change in stock price in the prior fiscal year end

CEO vega: The sensitivity of CEO's option value to a 1% change in stock return volatility in the prior fiscal year end

Sales growth: The current year's sales (SALE) minus the prior year's sales, all scaled by the prior year's sales

Firm focus: The segment sales based Herfindahl index computed as the sum of squared segment sales-total segment sales ratios

Number of segments: The firm's number of business segments from the Compustat Segment Database

Capital expenditure: The ratio of capital expenditure (CAPX) to total assets (AT)

Sale of PPE: The ratio of sale of property, plant, and equipment (SPPE) to total assets (AT) (If SPPE is missing, it is set equal to zero)

R&D: The ratio of research and development expenditure (XRD) to total assets (AT) (If R&D is missing, it is set equal to zero)

Book leverage: The sum of the firm's long-term debt (DLTT) and short-term debt (DLC), all scaled by total assets (AT)

Total acquisition exp.: The sum of acquisitions deal values (from SDC) during the fiscal year

APPENDIX B THE DETERMINANTS OF AN ELECTION YEAR CROSS-BORDER DEAL

	Dependent variable = Election year deal	
	Probit model	
Variable	Coefficient	p-value
Input-complex industry	-0.021	*[0.83]*
Labor-intensive industry	-0.063	*[0.60]*
Gov. spending dependent industry	0.062	*[0.66]*
Election in neighboring countries	0.495***	*[0.00]*
National preference affinity	0.074	*[0.60]*
Acquirer size	0.301**	*[0.02]*
Acquirer M/B	0.015	*[0.56]*
Target size	-0.003	*[0.56]*
Target M/B	-0.011	*[0.71]*
Target from a common law country	0.016**	*[0.05]*
Acquirer country stock market return	0.367	*[0.11]*
Target country stock market return	0.433*	*[0.08]*
Acquirer country real exchange rate return	-3.213***	*[0.00]*
Target country real exchange rate return	1.341*	*[0.09]*
Acquirer country GDP per capita	0.000	*[0.32]*
Target country GDP per capita	0.000	*[0.13]*
Acquirer country real GDP growth rate	-3.407	*[0.25]*
Target country real GDP growth rate	-3.639	*[0.20]*
Number of observations	934	
Likelihood ratio	70.65	*[0.00]*
Pseudo-R^2	0.106	

This table reports the estimation of the probit. The dependent variable is a dummy that equals one if there is a national election in the target's country within one year after the announcement of the deal, and zero otherwise.

APPENDIX C THE EFFECT OF CEO CAREER CONCERNS ON FIRM RISK VIA CAPEX AND R&D ACROSS FINANCING ACTIVITIES

Panel A Firm risk via capital expenditure

			Total risk	Systematic risk	Unsystematic risk
			(1)	(2)	(3)
Full sample			0.46	1.746*	0.282
			[0.61]	*[0.08]*	*[0.76]*
	SPPE > 0		-1.524	0.311	-1.729*
			[0.12]	*[0.83]*	*[0.10]*
		Capital ↑	4.867	-0.29	5.485
			[0.19]	*[0.94]*	*[0.16]*
		Leverage ↑	-7.784***	-2.951	-8.192***
			[0.00]	*[0.24]*	*[0.00]*
		Leverage ↓	-5.805**	3.895	-7.181***
			[0.02]	*[0.34]*	*[0.00]*
		Capital ↓	-13.288*	-7.803	-14.303**
			[0.06]	*[0.38]*	*[0.05]*
	SPPE = 0		2.151	3.171**	1.988
			[0.11]	*[0.03]*	*[0.16]*
		Capital ↑	1.983	3.887**	1.831
			[0.28]	*[0.04]*	*[0.36]*
		Leverage ↑	1.095	-3.518	1.174
			[0.83]	*[0.22]*	*[0.82]*
		Leverage ↓	10.807***	10.673**	10.212***
			[0.00]	*[0.02]*	*[0.01]*
		Capital ↓	4.455	8.289*	4.334
			[0.23]	*[0.06]*	*[0.25]*

Panel B Firm risk via R&D

			Total risk	Systematic risk	Unsystematic risk
			(1)	(2)	(3)
Full sample			-2.081***	-1.703	-2.180***
			[0.00]	*[0.27]*	*[0.00]*
	SPPE > 0		4.179	-0.711	4.796
			[0.37]	*[0.91]*	*[0.31]*
		Capital ↑	20.593*	40.018***	15.417
			[0.06]	*[0.00]*	*[0.17]*
		Leverage ↑	9.003	-2.077	9.412
			[0.34]	*[0.85]*	*[0.31]*
		Leverage ↓	-1.014	-5.383	-0.293
			[0.86]	*[0.57]*	*[0.96]*
		Capital ↓	6.749	2.035	7.41
			[0.36]	*[0.82]*	*[0.31]*
	SPPE = 0		-2.047***	-1.797	-2.146***
			[0.01]	*[0.19]*	*[0.01]*
		Capital ↑	-8.159***	-8.009***	-8.180***
			[0.00]	*[0.00]*	*[0.00]*
		Leverage ↑	-9.341	-5.958	-10.264
			[0.25]	*[0.22]*	*[0.24]*
		Leverage ↓	0.731	2.193	0.509
			[0.59]	*[0.46]*	*[0.74]*
		Capital ↓	-3.725**	-2.496	-3.969***
			[0.02]	*[0.40]*	*[0.01]*

Panel A presents the estimates of Equation (2) that examines whether CEO career concerns influence firm risk through their impact on capital expenditure (CAPEX) across varying financing activities. The category SPPE > 0 includes 2,379 firm-year observations in which SPPE is positive during the fiscal-year. The category SPPE = 0 contains 3,582 firm-year observations in which SPPE is zero or missing during the fiscal-year. We further stratify the SPPE > 0 (SPPE = 0) category into four sets of subgroups based on the firm's financing activities. The category Capital ↑ includes 410 (585) firm-year observations where the firm's net debt issuance (total debt issuance net debt retirement) and net equity issuance (equity sales net equity purchases) are both positive during the fiscal year. The Capital ↓ category comprises of 578 (833) firm-year observations in which both of net debt issuance and net equity issuance are less than or equal to zero during the fiscal year. The Leverage ↑ category consists of 585 (918) firm-year observations in which firm's net debt issuance is positive but net equity issuance is less than or equal to zero during the fiscal year. The Leverage ↓ category includes 806 (1,246) firm-year observations where the firm's net debt issuance is less than or equal to zero but net equity issuance is positive during the fiscal year. Panel B reports the estimates of Equation (2) with research and development expenditure (RD). Total risk is the natural logarithm of the annualized variance of the firm's daily stock returns during the fiscal year. Systematic risk is the natural logarithm of the annualized variance of the predicted component of stock returns using the expanded market model as described in Section II. Unsystematic risk is the natural logarithm of the annualized variance of the residual returns from the market model. Standard errors are clustered at the state level (not shown). Intercepts, state-, industry (2-digit SIC)-, and year-fixed effects are not shown in the table. *p*-values represent the significance level of each coefficient. ***, **, and * indicate significance at the 1%, 5%, and 10%, respectively (two-tailed).

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