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The financial crisis renewed interest in the potential for pay-for-performance compensation to affect managerial risk-taking. We examine whether paying top executives with stock options induces them to take more risk. To identify the causal effect of options, we exploit two distinct sources of variation in option compensation that arise from institutional features of multi-year grant cycles. We find that a 10 percent increase in the value of new options granted leads to a 2–6 percent increase in firm equity volatility. This increase in risk is driven largely by an increase in leverage. We also find that an increase in stock options leads to lower dividend growth, with mixed effects on investment and firm performance.

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Stock options had potentially unlimited upside, while the downside was simply to receive nothing if the stock didn't rise to the predetermined price. The same applied to plans that tied pay to return on equity: they meant that executives could win more than they could lose. These pay structures had the unintended consequence of creating incentives to increase both risk and leverage.

–Financial Crisis Inquiry Commission

1 Introduction

Performance-sensitive pay for executives surged in the last 30 years. During the 1990s, stock options became the largest component of executive compensation, and by 2000 accounted for 49 percent of total compensation for S&P 500 CEOs (Frydman and Jenter, 2010). Today, options continue to be prevalent, accounting for 25 percent of total compensation. Moreover, other forms of compensation with option-like payoffs have grown increasingly popular in recent years. For example, performance vesting shares tripled between 1998 and 2008, and now represent over 30 percent of equity-linked pay (Bettis et al., 2012). After the recent financial crisis, many argued that options and option-like compensation induced firms to take excessive risk, as executives stood to gain more than they stood to lose. However, the theoretical predictions for how options should affect risk-taking are actually ambiguous, and empirical measurement has been difficult due to the endogeneity of compensation. In this paper, we measure the direction and magnitude of the effect of options on risk-taking. To overcome the endogeneity problem, we exploit quasi-exogenous variation in option pay resulting from institutional features of multi-year option grant cycles.

The common intuition that stock options incentivize greater risk-taking stems from the fact that the Black-Scholes value of an option increases with the volatility of the underlying stock (see, e.g., Haugen and Senbet, 1981; Smith Jr. and Watts, 1982; Smith and Stulz, 1985). This is due to the convexity of option payoffs: if the underlying stock price rises above the strike price, the option holder earns the difference, but if the stock price drops below the strike price, the option holder does not lose the difference. However, in addition to this "convexity effect," Ross (2004) shows that options can affect risk-averse executives in two other ways. First, convex compensation increases the sensitivity of an executive's wealth to the underlying stock price. This "magnification effect" pushes risk-averse executives to decrease risk.[1] Second, options increase an executive's wealth, moving

[1]This magnification effect has also been noted by Lambert et al. (1991), Carpenter (2000), Hall and Murphy

him to a different part of his utility function. This "translation effect" may push an executive to increase or decrease risk depending on whether his utility function has increasing or decreasing risk aversion.[2] Finally, options may have no effect on behavior if executives are able to fully hedge them (Garvey and Milbourn, 2003) or if executives are already well monitored. Thus, it is theoretically ambiguous how options should affect risk-taking in practice.

A substantial existing literature explores the relationship between executive stock options and various measures of risk-taking behavior. However, the evidence remains mixed. Many papers find a positive relationship, but the magnitudes vary from large to near-zero. For example, Agrawal and Mandelker (1987) and DeFusco et al. (1990) find a positive relationship between options and firm volatility.[3] In contrast, Hayes et al. (2012) find no obvious relationship between options and risk-taking following a decline in options. Another line of research has focused on "vega" (the sensitivity of the total Black-Scholes value of all unexercised options to changes in volatility). Several papers find a positive association between vega and equity volatility as well as leverage (Guay, 1999; Cohen et al., 2000; Coles et al., 2006). However, Cohen et al. (2000) note that the association is very small in magnitude, and Guay (1999) points out that vega does not take risk aversion into account.

Most importantly, establishing a causal effect of options on risk-taking has been difficult due to endogeneity concerns. For example, Prendergast (2002) argues that firms that are fundamentally more risky choose to award more equity-linked compensation because these firms face more difficulty in monitoring managerial effort. Alternatively, (over)confident CEOs may select into firms that offer more options and other performance-sensitive pay (Lazear, 2000). These CEOs may also prefer risky projects. Finally, firms that are naturally more risky must offer higher total pay to satisfy a risk-averse executive's participation constraint. The higher total pay may partly consist of more options (Cheng et al., 2012). Thus, omitted variables may bias simple cross-sectional estimates of the impact of option-like compensation on risk-taking. Moreover, even within-firm or within-executive analysis suffers from dynamic versions of these concerns; periods in which a firm or executive chooses high option compensation may also be periods in which the firm or executive wishes to take high risk.

(2002), and Lewellen (2006), among others.

[2] In addition, options may have other ambiguous implications for risk. For example, options increase in value with firm performance, and managers may increase or decrease firm risk in the pursuit of stronger firm performance.

[3] For more work along these lines, see Saunders et al. (1990); Mehran (1992); May (1995); Tufano (1996); Berger et al. (1997); Denis et al. (1997); Esty (1997); Schrand and Unal (1998); Aggarwal and Samwick (1999); Core and Guay (1999); Knopf et al. (2002).

Similarly, changes in compensation may be accompanied by unobservable changes in governance or strategy that directly affect risk-taking.

A few recent studies attempt to address these endogeneity issues by examining how executive risk-taking changed after accounting rules that made options advantageous were eliminated. These studies deliver mixed results: Chava and Purnanandam (2010) find that options increase risk-taking, while Hayes et al. (2012) find that options do not affect risk-taking. In addition, the change in the accounting rules was likely anticipated and affected all firms simultaneously. Thus, subsequent changes in firm policies may have been caused by other changes in the business environment. Using a different strategy, Gormley et al. (2013) examine how executives that endogenously differ in their unexercised option holdings respond to an exogenous increase in firm risk that stems from the discovery of carcinogens used by their firm. The exogenous nature of the shock helps rule out reverse causality and allows the authors to explore a related question: how does a change in risk affect option compensation? However, to identify a causal effect of options on risk-taking, the ideal test would utilize exogenous variation in option pay rather than in the risk environment. In this paper, we exploit a natural experiment that delivers such variation.

Our identification strategy builds on Hall's (1999) observation that firms often award options according to multi-year plans.[4] Two types of plans are commonly used: fixed number and fixed value. On a fixed number plan, an executive receives the same *number* of options each year within a cycle. On a fixed value plan, an executive receives the same *value* of options each year within a cycle. Cycles are generally short, lasting only two years, after which a new cycle typically begins.

Firms are not required to disclose intended schedules for multi-year cycles. Conversations with compensation consultants suggest that these cycles are a common norm rather than a formal contract. Therefore, we infer the presence of cycles from the data in a manner similar to Hall (1999). While there is surely measurement error involved in our procedure, this should not introduce bias into our instrumental variables framework (see the Appendix for a detailed discussion). Using our procedure, we find that multi-year plans are pervasive, accounting for more than 40 percent of executive-years with option pay in our sample.

These multi-year plans give us two distinct instruments for changes in option compensation.

[4]Hall (1999) describes multi-year grant cycles in detail, but does not use them as an instrument to explore the effect of options on managerial behavior.

Our first instrument uses only executives on fixed value plans. We show that option compensation for these executives tends to follow an increasing step function. During a fixed value cycle, the value of options granted is held constant. At the beginning of a new cycle, there is a discrete increase in the value of option grants, on average. The timing of when these steps occur is staggered across executives and firms.

These staggered steps motivate our first instrument: an indicator variable for whether each executive-year is *predicted* to be the first year of a new fixed value cycle. Predictions are key to our analysis. We do not use *actual* cycle first years as our instrument because the timing of when new cycles actually begin may be endogenously renegotiated between the manager and the board. For example, a manager may negotiate to prematurely start a new cycle for some unobserved reason that also directly affects the firm's risk. Instead, we use a predicted first year indicator, which corresponds to when new cycles would likely have started if renegotiation had not taken place. Our predictions exploit the fact that firms tend to use repeated cycles of equal length. We use the length of a manager's previous cycle to predict when his next cycle will begin. Thus, predictions are based only on past information. For example, if a manager had cycles starting in 1990 and 1992, we would predict that a new cycle would start in 1994. Assuming that firms do not set the length of the current cycle in anticipation of risk-taking conditions at the start of future cycles, the predicted first year instrument should purge the estimation of bias from renegotiation.

A potential concern is that our instrument delivers exogenously timed but anticipated changes in option pay. In Section 3, we describe why this would not explain our findings and if anything should dampen our results. A second potential concern is that years coinciding with the start of new fixed value cycles may be special in other ways that affect risk-taking. For example, (predicted) cycle first years may coincide with periods of decreased turnover risk, performance reviews, or new product launches. Empirically, we show that these years do not have unusual turnover risk, and conversations with compensation consultants suggest that cycles are unrelated to performance reviews as well. However, to ensure that other unobservable differences in cycle first years are not driving our results, we use a second instrumental variables strategy that is robust to these concerns.

Our second instrumental variables strategy does not use the timing of cycle first years. Rather, it uses variation in the value of options granted *within* fixed number and fixed value cycles. We exploit the fact that the Black-Scholes value of an at-the-money option increases proportionally with its strike price. As again noted by Hall (1999), this means that executives on fixed number plans receive new grants with higher value when their firm's stock price increases. In contrast, executives on fixed value plans receive new grants with the same value (and a lower number of options) when their firm's stock price increases. Thus, the value of new option grants is fundamentally more sensitive to stock price movements for executives on fixed number plans than for executives on fixed value plans. Of course, stock price movements are partially driven by market and industry shocks that are beyond an executive's control. Thus, our second instrument for the change in the value of options granted is the interaction between plan type and aggregate returns.

Given that our second instrument is an interaction term, the identifying assumption is subtle. While Hall (1999) suggests that firms choose between fixed number and fixed value plans somewhat arbitrarily, we do not assume that the choice between fixed number and fixed value is random. Rather, our identifying assumption is that fixed number and fixed value firms do not differ in their response to aggregate returns for reasons other than the differential sensitivity of their option compensation. Fixed number firms may systematically differ from fixed value firms, but we assume they do not differ in how their non-compensation-related risk-taking moves with aggregate returns. To examine whether the data support this assumption, we perform a placebo test that compares how firm risk moves with aggregate returns for firms that are not on either type of plan, but at some point used fixed number or fixed value plans. Consistent with the assumption, we find no differences in this case. In addition, our first instrumental variables strategy does not require this assumption.

As is common in the literature (e.g. Guay, 1999; Cohen et al., 2000; Hayes et al., 2012; Gormley et al., 2013), we use realized equity volatility as our primary measure of risk-taking. We find a significant positive effect of option compensation on risk-taking. A 10 percent increase in the value of new options granted leads to a 2–6 percent increase in volatility. We further find that the increase in volatility is driven largely by increases in leverage. In addition, we find that options have a positive effect on investment, but the results here are less robust and more subject to interpretation issues. In theory, investing in riskier projects may significantly contribute to firm risk. However, it is difficult to discern from accounting data whether investment actually represents investment in riskier projects. Moreover, an executive who holds equity-linked compensation may overinvest to sustain the pretense that the firm possesses good investment opportunities (Benmelech et al., 2010). Therefore, we present suggestive results that options increase overall investment, but we do not draw strong conclusions.

We also examine how options affect dividend policy. Here, the theoretical prediction is unambiguous. All else equal, dividend payments should reduce a firm's stock price. Most executive stock options are not "dividend protected" and therefore decrease in value following dividend payouts. As a result, option compensation gives executives incentives to pay out less in dividends.[5] Consistent with this prediction, we find that options lead to lower dividend growth among dividend-paying firms. Our dividend results also highlight the importance of the IV strategy in addressing endogeneity issues. We show that a naive OLS estimation finds a strong positive relationship between dividends and options despite theoretical predictions to the contrary.

Finally, we find that options have little effect on firm returns, and if anything, the relationship is negative. We also find that options lead to weakly lower accounting measures of performance. However, these results are harder to interpret and may reflect increased investment or a shift toward long-term projects with higher future cash flows rather than worse firm performance.

Overall, our estimates should be viewed as a lower bound for the effect of a moderate increase in options on executive risk-taking. Executive stock options typically vest linearly over several years, implying that new grants can affect behavior beyond a one-year horizon. Our methodology uses annual variation in new option grants, which constrains us to study annual changes in behavior. If risk-taking continues to increase beyond a one-year horizon, we will underestimate the total effect of options. We also cannot capture incentives to manipulate firm outcomes shortly before long-vesting options are exercised (e.g. Oyer, 1998). Another consequence of vesting is that most executives also hold previously granted unexercised options. However, our instruments only affect new option grants. One might expect that the marginal effect of new option grants would be weaker if the executive already holds a sizable portfolio of unexercised options. Consistent with this, we find that the effect of new option grants on volatility is greater in subsamples where the value of new option grants is high relative to the total value of unexercised options held by the executive. We also find that the effect of options on risk-taking is greater for firms in the financial and high-tech sectors, where executives may have greater ability to manipulate risk beyond changing leverage.

[5] For more on this, see Lambert et al. (1989); Lewellen et al. (1987); Jolls (1998); Fenn and Liang (2001).

2 Data

2.1 Sources

To create a comprehensive panel of compensation data, we pool information from three separate sources. The first source is a dataset assembled by David Yermack that covers firms in the Forbes 800 from 1983-1991. The second source is Execucomp, which covers firms in the S&P 1500 from 1992-2010. The third source is Equilar, which covers firms in the Russell 3000 from 1999-2009. When a firm-year is covered by both Execucomp and Equilar, we use data from Execucomp.

All three data sources are derived from firms' annual proxy statements and contain information regarding the compensation paid to top executives in various forms during the fiscal year. In some cases, executives receive more than one option grant during a fiscal year. Equilar and Execucomp have detailed grant-level data with information on the date and amount of each option grant made. This allows us to better identify executives on fixed number and fixed value plans in cases where an executive has multiple grants per fiscal year but only one is associated with the plan. Having the exact date of the grant also allows us to more precisely measure aggregate returns between consecutive grants and volatility following a grant. In 2006, firms were required to begin reporting the fair value of option compensation. For data prior to 2006, we use the firm's reported value of option compensation if available and also compute the Black-Scholes value of option grants ourselves. In 2006, firms were also required to begin reporting information on unexercised options held by executives at the end of each fiscal year. Equilar and Execucomp both collect these data.

Accounting data come from Compustat. Following standard practice, financial firms (6000-6999) and regulated utilities (4900-4999) are excluded from the sample when accounting-based outcomes are used. However, financial firms are included in some samples, as noted, to assess the effect of options on equity volatility. Market and firm return data come from the Center for Research in Security Prices (CRSP) and the Fama-French Data Library.

2.2 Detecting Cycles

Firms are not required to disclose intended schedules for multi-year compensation cycles, and therefore, few report them. Our conversations with compensation consults suggest that the use of multi-year cycles is a common norm rather than a formal contract. Following Hall (1999), we instead back out these cycles using the data.

Ideally, we would use the firm's pre-planned intended cycle structure in our IV analysis. Inferring the cycle structure from realized option grants necessarily introduces measurement error. In particular, we infer planned cycles with error if the firm did not intend to adopt a cycle schedule but awarded the same number or value of options across consecutive years for potentially endogenous reasons. We will also infer planned cycles with error if the firm departs from a pre-planned cycle schedule for potentially endogenous reasons. As will be discussed in later sections, our methodology is robust to both of these types of errors. In general, measurement error will reduce the precision of our estimates but not lead to bias. For a more detailed discussion of measurement error issues, see the Appendix.

2.2.1 Fixed Number

An executive is inferred to be on a fixed number cycle in two consecutive years if he receives the exact same number of options in both years. An executive who receives multiple grants in a fiscal year is inferred to be on a fixed number plan if one of the individual grants is equal to another in consecutive years. This is done because an executive may receive one grant as part of a long-term incentive plan that is common among all executives in the firm as well as another grant that is part of a fixed number plan. In this case, to ensure that the fixed number grants are significant relative to other option grants, we require that the number of options in the fixed number grants constitute more than 50 percent of the total number of options granted over the years of the cycle, adjusting for stock splits. Our results are not sensitive to these assumptions. In 80 percent of cases, executives receive a single option grant. As we will show, limiting our analysis to this subsample yields qualitatively similar results.

2.2.2 Fixed Value

There are a few additional issues to consider when we detect fixed value cycles. First, we must decide how to value an option grant. While Black-Scholes is currently the most popular method of valuing options, firms may use different methodologies internally to implement fixed value plans. The most common alternative valuation used in practice is the "face value," i.e., the number of options granted multiplied by the grant-date price of the underlying stock.[6] Among the firms that value option grants using the Black-Scholes methodology, a variety of assumptions can be made regarding key parameters such as volatility. In addition, firms often grant options in round lots, so that the value is not exactly fixed even by their own internal methodology. Finally, rather than holding the value of option grants fixed, firms sometimes hold the value as a proportion of salary or salary plus bonus fixed.

Accordingly, we consider an executive to be on a fixed value cycle in two consecutive years if the value of the options he receives (possibly as a proportion of salary or salary plus bonus) is within 3 percent of the previous year. Value is computed as the Black-Scholes value, face value, or company self-reported value.[7] We require that a fixed value cycle be defined using the same valuation methodology in all years. Again, if multiple grants are awarded per year, then the individual grants are also compared and can form the basis of a fixed value cycle if they are significant relative to other options granted, using the same criteria as before.

One potential concern with allowing fixed value cycles to be defined as a proportion of salary or salary plus bonus is that the value of options does not remain "fixed" within a cycle if salary or salary plus bonus moves within a cycle. In practice, salary and bonus grow slowly in comparison to other forms of executive compensation, on average. In unreported results, we find that executives on fixed value cycles that are defined as multiples of salary and bonus tend to receive small increases in options within cycles and larger jumps in options at the start of a new cycle, so option grants still tend to follow a step function. We also find very similar results if we drop these executives from our sample.

[6]See "Raising the Stakes: A Look at Current Stock Option Granting Practices," 1998, Towers Perrin CompScan Report. In addition, note that holding "face value" constant is equivalent to holding "potential realizable value" constant, where "potential realizable value" is the value of the option at expiration, assuming a constant rate of appreciation of the underlying stock, e.g. 5 percent.

[7]The Black-Scholes value is calculated based on the Black-Scholes formula for valuing European call options, as modified to account for dividend payouts by Merton (1973): $Se^{-dT}N(Z) - Xe^{-rT}N(Z - \sigma T^{(1/2)})$, where $Z = [ln(S/X) + T(r - d + \frac{\sigma}{2})]/\sigma T^{(1/2)}$. The parameters in the Black-Scholes model are as follows: S = price of the underlying stock at the grant date; E = exercise price of the option; σ = annualized volatility, estimated as the standard deviation of daily returns over the 120 trading days prior to the grant date multiplied by $\sqrt{252}$; r = 1 + risk-free interest rate, where the risk-free interest rate is the yield on a U.S. Treasury strip with the same time to maturity as the option; T = time to maturity of the option in years; and d = 1 + expected dividend rate, where the expected dividend rate is set equal to the dividends paid at the end of the previous fiscal year end divided by the stock price.

2.3 Measuring Risk

As is standard in the literature, our primary measure of risk-taking is realized equity volatility (e.g. Guay, 1999; Cohen et al., 2000; Hayes et al., 2012; Gormley et al., 2013). Equity volatility is the most natural measure of risk, as it is ultimately what an executive would be incentivized to manipulate to affect the value of his options. We also examine other outcomes that may drive changes in volatility, such as leverage and investment. Standard capital structure theory implies that leverage unambiguously increases equity volatility. Riskier investment can also contribute to volatility, although it is not obvious whether accounting measures of investment increase or decrease risk—a concern we discuss in later sections. In unreported tests, we also estimate the effect of options on implied volatility. We find that implied volatility is highly correlated with realized volatility. However, since the OptionMetrics data do not start until 1996 and do not cover many of the firms in our sample, we lose significant power in tests using this dependent variable, as our sample size drops by roughly 80 percent. Another possibility would be to use cash flow volatility. However, as will be discussed shortly, our methodology is constrained to look at year-to-year changes in risk-taking and within a year, there are insufficient cash flow observations to make this possible.

2.4 Summary Statistics

Figure 1 shows the prevalence of multi-year plans over time. Overall, fixed value plans represent 24 percent of executive-years in which options are paid compared to 18 percent for fixed number plans. Fixed number plans peaked at 22 percent in 2003 and then declined to only 8 percent in 2010. Fixed value plans peaked at 31 percent in 2007, but remain common. Our conversations with compensation consultants suggest that the decline of fixed number plans can be attributed to the rising acceptance of the Black-Scholes option valuation methodology.

In very recent years, there has been a decline in both types of plans, possibly due to disclosure and benchmarking regulations that have led firms to adjust options annually. The recent decline in the popularity of multi-year plans is not problematic for our analysis because we are not interested in multi-year plans per se; we merely use them to generate exogenous variation in option grants. It is true, however, that we can only estimate the causal effect of options on risk-taking for the subset of firms that use these plans. We see no reason that the effect should differ by usage of these plans, but we acknowledge that we cannot rule out this possibility. Even so, our sample represents a large proportion of firms (42 percent) that paid options over this time period and thus is important in and of itself.

Panel A of Table 1 shows the distribution of cycle length. The modal cycle length is two years for both fixed number and fixed value plans. Two-year cycles account for 92 percent of executive-years corresponding to fixed value plans and 67 percent of executive-years corresponding to fixed number plans.[8] Conversations with compensation consultants indicate that two-year cycles are indeed common.

We also find evidence that cycles tend to be coordinated across executives in the same firm. For brevity, we summarize these results below instead of reporting them in table format. Conditional on an executive in a firm being on a fixed number cycle and the CEO of the same firm being at the start of a cycle, the (sample) probability that the executive is also at the start of a cycle is 79.4 percent. For fixed value, this probability is 70.4 percent. Another way to test whether cycles are coordinated is to regress the cycle first year indicator variable on a full set of firm by year fixed effects. If these fixed effects are jointly significant, it indicates that cycle first years are not randomly distributed within firms. Consistent with this, we find that firm by year fixed effects are jointly significant with p-values less than 0.0001 for both fixed number and fixed value.

Next, we explore the extent to which firms that use fixed number, fixed value, or neither plan differ in their observable characteristics. Because there are likely to be time trends in these variables and the relative prevalence of the two types of plans have changed over time, we examine three cross-sections of the data rather than pool all years together. Table 1 presents the year 2000, while 1995 and 2005 are presented in the Appendix. Panel B of Table 1 shows the industry distribution for firm-years, categorized by the CEO's plan type. We find that multi-year cycles are distributed across many industries and that the industry distribution is similar across plan types. Panel C of Table 1 compares other firm and executive characteristics across plan types. In general, fixed number and fixed value firms appear similar in terms of market to book, volatility, investment,

[8]Our finding that two-year cycles are relatively more common among fixed value plans than among fixed number plans may partly be due to relatively more measurement error in the process of detecting fixed value grants. We explain in the Appendix why, in our instrumental variables framework, error in detection should reduce the precision of our estimates but should not bias our results.

leverage, and profitability. In terms of assets and sales, fixed value firms tend to be larger than fixed number firms, which are in turn larger than firms using neither type of plan. Overall, we find that firms do not differ sharply across the three categories, consistent with Hall's claim that firms sort approximately randomly into these plans. Nevertheless, as will be discussed in Section 3, our analysis will never assume that firms choose randomly between fixed number and fixed value plans.

Finally, we define various measures of option grants. The Black-Scholes (B-S) value is as defined earlier. The delta of options equals the change in the B-S value of all options for a 1 percent change in underlying. The vega of options equals the change in the B-S value of all options granted for a 0.01 unit change in the volatility of the underlying. All options are granted at-the-money during our sample period; i.e., the strike price is equal to the price of the underlying on the grant date. The B-S value, delta, and vega of new option grants are all highly positively correlated in our data. Therefore, an exogenous increase in new option grants implies that all three values increase together.

3 Empirical Strategy

3.1 Instrumental Variables Strategy 1

Our first instrumental variables strategy uses only observations corresponding to fixed value plans. Thus, it is not subject to the concern that fixed value firms may be different from fixed number firms due to the fact that plans are endogenously chosen. To help fix ideas, Figure 2 illustrates three real examples of fixed value cycles taken from the data. From these examples, two patterns emerge that turn out to be true more generally. First, option compensation tends to follow an increasing step function for executives on fixed value plans. This is likely because compensation tends to drift upward over time, yet executives on fixed value plans cannot experience an upward drift within a cycle. As a result, they experience a discrete increase, on average, in the year following the completion of a cycle. Second, executives tend to have repeated cycles of equal length that are staggered across executives. For example, the executive in Panel A completes cycles in 2006, 2008, and 2010, while the executive in Panel B completes cycles in 2007, 2009, and 2011. While these two stylized facts do not hold in all cases—as can also be seen in Figure 2—our identification strategy only requires that they hold true on average.

Panel A of Table 2 confirms that the increasing step function pattern holds true on average. We regress the change in log option compensation on an indicator variable equal to one in the first year following the end of a fixed value cycle. The first year indicator is equal to one for any first year following a completed cycle, even if that observation does not represent the start of a new cycle. This is because option pay tends to jump substantially after being fixed for two or more years, even if the firm chooses to discontinue fixed value plans in the future. Accordingly, the sample is limited to years that are part of fixed value cycles as well as years that immediately follow a completed fixed value cycle. Because the first year indicator is staggered across firms and executives, we can include year fixed effects and firm fixed effects in the regressions. Columns 1 and 2 show that executives experience approximately an 8 percent larger increase in the Black-Scholes value of their option grant following the end of a fixed value cycle relative to other years. This is true for all top executives as well as for the subsample of CEOs and CFOs. Columns 3–6 show that the first year indicator is also associated with significant increases in the delta and vega of the option package.

However, we do not use the simple first year indicator as our instrument because of the possibility that the timing of cycle termination may be renegotiated mid-way through a cycle. For example, in good times, executives may seek to prematurely begin new fixed value cycles and receive a raise. In this case, actual first years may coincide with periods in which risk-taking is expected to increase or decrease for reasons unrelated to the incentives provided by option compensation. This, in turn, would lead to a violation of the exclusion restriction required of a valid instrument.

Due to this concern, we use an indicator for whether a year is *predicted* to be the first year of a new fixed value cycle as our first instrument. Predicted first years correspond to when new cycles would likely have started if renegotiation had not taken place. To make these predictions, we use the fact that executives tend to have repeated cycles of equal length. Conditional on being on a fixed value cycle, the length of the cycle is equal to that of the previous cycle in 90 percent of cases. Thus, we can use the length of an executive's previous cycle to predict the length of his next cycle. For example, if an executive had cycles starting in 1990 and 1992, we would predict that a new cycle would start in 1994. Importantly, the predictions are made without using any contemporaneous information.

We use the following simple prediction algorithm. Let k be the length of the executive's last completed fixed value cycle. If there was no previous cycle, let $k = 2$, because this is the modal cycle length in the data as shown in Table 1. In year t, let n_t be the number of consecutive years, inclusive, in which the executive received the same value of options (within the aforementioned tolerance of 3 percent). We predict that year $t + 1$ will be a first year if $n_t \geq k$. Note again that these predictions do not use any contemporaneous information. We also experimented with more sophisticated prediction methods such as using the length of the last completed fixed value cycle for other executives in the same firm. This leads to similar results (because cycle length tends to be similar across executives in the same firm), but we use the above methodology, as it is the simplest and most transparent. Finally, we also exclude the first year of each executive's tenure from the analysis because those years are likely to be special in other ways besides being the first year of a new cycle.

To illustrate how this works in practice, the dotted vertical lines in Figure 2 indicate years that we predict to be cycle first years. Panels A and B both show 3 cycles of length 2. In these cases, we correctly predict all of the cycle first years (e.g., for Panel A, these occur in 2006, 2008, and 2010). The example in Panel C shows a cycle of length 2 followed by two cycles of length 3. In this case, we correctly predict a cycle first year in 2000, incorrectly predict a first year in 2002 due to the change in cycle length, and then correctly predict a first year in 2003 and 2006. Incorrect predictions reduce the power of the first stage of our IV estimation, but do not bias our results. In fact, they purge the instrument of potential bias arising from endogenous renegotiation.

As can be seen from the examples above, we use only past information to predict cycle status in the current year. This should purge the estimates of bias that would arise if actual cycle status is correlated with contemporaneous determinants of risk-taking. In fact, we only use past information with a minimum one-year lag to predict current cycle status. This should also purge the estimates of any potential bias that would arise if actual cycle status is correlated with recent past conditions. Consistent with this, we find that lagged returns are not correlated with predicted cycle first years. However, we do use information from the more distant past to form predictions, so we need to assume that firms are not very forward-looking; i.e., managers and boards do not set the length of the current cycle in anticipation of risk-taking conditions two or more years in the future. If this assumption holds, then the predicted first year indicator purges the estimation of potential bias. For a more in-depth discussion of this, see the Appendix.

Using the predicted first year variable, we then estimate the effect of changes in option compensation in an instrumental variables framework. Specifically, we estimate first- and second-stage equations of the form:

$$\triangle O_{ijt} = \beta_0 + \beta_1 I_{ijt}^{PredictedFirstYear} + \gamma_t + v_j + \epsilon_{ijt} \qquad \text{(1st stage)}$$

$$Y_{ijt} = \delta_0 + \delta_1 \widehat{\triangle O_{ijt}} + \gamma_t + v_j + \mu_{ijt}, \qquad \text{(2nd stage)}$$

where i indexes executives, j indexes firms, and t indexes years. The variable $I_{ijt}^{PredictedFirstYear}$ is the indicator for predicted first year, O_{ijt} is a measure of the value of the option grant, and Y_{ijt} are the outcome variables measured as annual changes for stock variables and levels for flow variables. Year fixed effects and firm fixed effects are represented by γ_t and v_j, respectively. Standard errors are clustered by firm to account for the fact that we observe multiple executives from the same firm. The main coefficient of interest, δ_1, represents the effect of an increase in options on outcomes Y_{ijt}.

Importantly, in the second stage, we do not regress firm outcomes on the actual change in option compensation that a particular executive experienced at the start of a new cycle. Doing so would be problematic, as the size of that change may be related to executive and firm unobservables that directly affect risk-taking. Instead, we use the fact that the indicator for predicted first year corresponds to pay raises *on average* and is staggered across executives. Our analysis essentially compares average changes in risk-taking in years when the indicator is equal to one to years in which the indicator is equal to zero. We also do not assume that firms randomly choose cycle length. Even among executives on cycles of only length 2, predicted cycle first years will be staggered (with some executives starting new cycles in even years and others in odd years). Restricting our sample to these executives yields similar results (see Table 11).

One might be concerned that predicted first years provide exogenously timed but potentially *anticipated* increases in option compensation. However, this is not an issue for our empirical strategy. To see this, first suppose that a manager could change risk instantaneously. He would have no incentive to increase risk prior to an anticipated increase in the value of his option compensation next period. In fact, doing so would actually lead him to receive fewer options next period, because with increased volatility, fewer options would be needed for him to be (nominally) paid a given Black-Scholes value. However, if a manager could only adjust risk slowly, he might wish to begin doing so prior to receiving the increase in options. Yet, if anything, this would bias us against finding larger increases in risk during predicted first years than in other years.[9]

A related concern is that, if a manager could change risk quickly, he may seek to depress it temporarily to increase the real value of his next option grant. For example, suppose a manager knew that, next year, he would receive $1 million Black-Scholes value of options, calculated using the firm's equity volatility in the 90 days before the grant. In this case, the manager might try to decrease volatility before the grant so that a greater number of options would need to be awarded to total $1 million in Black-Scholes value. After the grant, he may then restore volatility to its previous level and hold options worth more than $1 million. Short-run manipulation of volatility is not a problem for our methodology because we examine the *annual change* in volatility as our outcome. If the incentive to engage in short-run risk manipulation is the same before each annual fixed value grant, then the risk manipulation in two adjacent years should net to zero when we calculate the annual change in volatility. If the incentive to engage in short-run manipulation is increasing with the size of the option grant, then it should be a bias against our findings that the annual change in volatility is greater following exogenously timed increases in option pay. Further, we find similar results if we analyze the change in volatility excluding the 120 trading days around each option grant or using the first 120 trading days following the option grant, which presumably is less affected by short-run risk manipulation, as it is further removed from the next option grant.

Finally, one may be concerned that predicted cycle first years are unusual in ways other than the increase in option compensation. For example, it may be that turnover risk is lower during these years if they are also the first year of an employment agreement (Xu, 2011). In this case, executives may increase risk-taking because they are less likely to be terminated. In unreported results, we find that cycle termination is unrelated to turnover. Conversations with compensation consultants also suggest that cycle first years are not accompanied by unusual performance evaluations. However, we cannot rule out other unobservable differences in these years. For example, predicted cycle first years may tend to be the first year of new product cycles. Instead, we complement our analysis with a second instrumental variables strategy that does not use the timing of cycle first years.[10]

[9] One might also be concerned that if the market anticipates an increase in risk during the next period, equity volatility may increase this period. However, it is straightforward to show that, under standard assumptions, this is not the case.

[10] If heterogeneous treatment effects are present, another potential concern is that our first instrument may not satisfy the strict monotonicity assumption required to interpret our IV estimate as a local average treatment effect. On average, executives are significantly more likely to receive increases in options in predicted first years. However, we cannot exclude the presence of a subset of defiers. We partially address this concern by noting that the first stage

3.2 Instrumental Variables Strategy 2

Our second instrumental variables strategy uses differences in the way that option compensation moves *within* a cycle for executives on fixed number and fixed value plans. The value of new option grants remains approximately fixed within a cycle for executives on fixed value plans. In contrast, the value of new option grants within a fixed number cycle changes with the price of the underlying stock. This is because the Black-Scholes value of each share of an at-the-money option increases in proportion to the strike price. Thus, if a firm using a fixed number plan experiences an increase in its stock price, the total value of new options awarded to its executives increases as well.

This is illustrated via an example in Table A.2, adapted from Hall (1999). The example shows how option compensation would evolve for an executive at the same firm if he were on a fixed value or fixed number plan. The executive is paid 28,128 options valued at $1 million under both plans in Year 1. The firm's stock price then increases by 20 percent in each of the next two years. Under a fixed value plan, the firm grants the executive fewer options each year to keep the value of those options constant at $1 million. Under a fixed number plan, the firm continues to grant the executive 28,128 options each year, and as a result, the value of those options increases by 20 percent each year along with the stock price. This illustrates how the value of new grants is more sensitive to stock price movements for executives on fixed number plans than for executives on fixed value plans. Of course, stock price movements are partially driven by market and industry shocks, which are beyond an executive's control. Thus, our second instrument for changes in option compensation is the interaction between plan type and aggregate returns.

Specifically, we estimate first- and second-stage equations of the form:

$$\triangle O_{ijt} = \beta_0 + \beta_1 I_{ijt}^{FN} + \beta_2 R_{kt} + \beta_3 I_{ijt}^{FN} R_{kt} + \gamma_t + v_j + \epsilon_{ijt} \qquad \text{(1st stage)}$$

$$Y_{ijt} = \delta_0 + \delta_1 I_{ijt}^{FN} + \delta_2 R_{kt} + \delta_3 \widehat{\triangle O_{ijt}} + \gamma_t + v_j + \mu_{ijt}, \qquad \text{(2nd stage)}$$

where I_{ijt}^{FN} is an indicator equal to one if the executive is on a fixed number plan, and R_{kt} is the Fama-French (49) industry return over the 12 months prior to the grant date. The interaction term,

of our IV is strong, with F-statistics exceeding 40, and the severity of the monotonicity problem is inversely related to the strength of the first stage. We also present a second instrument for which strict monotonicity is more likely to hold. Specifically, for our second instrument, strict monotonicity requires that the value of a fixed number of options weakly increases with industry returns.

$I_{ijt}^{FN} R_{kt}$, is the excluded instrument. The coefficient, δ_3, is the effect of an increase in new option grants on our outcome of interest, Y_{ijt}.

Note that I_{ijt}^{FN} and R_{kt} are not excluded instruments, as they appear in the second-stage regression as well. Thus, our identification strategy allows for the possibility that plan type or aggregate returns directly relate to risk-taking. It may well be, for example, that fixed number firms tend to take on more risk or that firms in general increase risk when industry returns are high. We do not need to assume away these types of relations.

The exclusion restriction instead requires that the interaction term, $I_{ijt}^{FN} R_{kt}$, only relates to risk-taking, Y_{ijt}, through its effect on compensation. In other words, we assume that fixed value and fixed number executives do not have different non-compensation induced responses to changes in aggregate returns. We examine whether there is support for this assumption in the data by performing a placebo test that compares how firm risk moves with aggregate returns for firms that are not on either type of plan but at some other point used fixed number or fixed value plans. In addition, our first instrumental variables strategy does not require this assumption.

Finally, the sample is restricted to executives on fixed number or fixed value cycles, as we wish our identification to be based on the comparison of executives whose compensation is mechanically sensitive to industry returns with those whose compensation is mechanically insensitive to industry returns. We also exclude observations corresponding to the first years of cycles, because our first stage outcome is the annual change in option compensation. In the first year of a new cycle, the change in option compensation relative to the previous year is not necessarily more sensitive to returns for fixed number executives—in the first year, fixed number (value) executives do not receive the same number (value) of options as in the previous year, while in later years they do. Another consequence of restricting our second IV sample to cycle continuation years (excluding the first year) is that we are identifying off of variation within cycles rather than between cycles (as in the first IV).

3.3 Other Empirical Considerations

Before turning to the results, we address other important considerations that apply to both strategies described above. First, our instruments generate annual variation in new option grants, which constrains us to study annual changes in behavior. However, executive stock options are not typically exercisable immediately upon being granted but, rather, vest over a period of several years.[11] Cadman et al. (2012) show that roughly 35 percent of a typical option grant does vest within our one-year window. For these options, it is obvious why executives may seek to increase volatility in the year following the option grant. For options that vest over a longer horizon, executives may still have an incentive to increase risk in the first year. Increasing volatility in the first year (even temporarily) would lead to more extreme prices in expectation when the option becomes exercisable, thus increasing the option's value. However, we will underestimate the total effect of options if risk-taking continues to increase beyond a one-year horizon. We also cannot capture incentives to manipulate firm outcomes immediately before long-vesting options are exercised. For the above reasons, we believe that our estimates likely represent a lower bound.

A related consequence of long-vesting options is that most executives also hold previously granted unexercised options, while our instruments only affect new option grants. One might expect that the marginal effect of new option grants would be weaker if the executive already holds a sizable portfolio of previously granted options. Consistent with this, we will show that the effect of new option grants is greater in subsamples where the value of new option grants is high relative to the total value of unexercised options held by the executive.

A second consideration relates to the possibility that other types of compensation, such as salary, bonus, and restricted stock, move with multi-year option grant cycles. In unreported tests, we find that salary and bonus do not increase with our instruments. If anything, cash compensation appears to offset changes in options induced by multi-year option cycles, although the effect is only marginally significant.[12] We also find that our instrument does not predict changes in restricted stock. Moreover, when we limit the sample to years in which no restricted stock is awarded, we find similar results (see Table 11).

A third consideration is that the Black-Scholes value, delta (the change in B-S value of all options for a 1 percent change in the underlying), and vega (change in B-S value of all options granted for a 0.01 unit change in the volatility of the underlying) of new at-the-money options are

[11] The most common schedules vest linearly, for example, 33 percent annually over 3 years (Hall and Murphy, 2002).

[12] For example, in fixed value first years when options jump, cash compensation increases by less than in other years. While this effect is only marginally significant, it is consistent with the story that cycles induce jumps in options that are partially offset by movements in cash compensation. However, even if cash pay adjusts completely, such that total pay never jumps, variation in the proportion of total pay awarded as options would still affect risk-taking incentives, as most other forms of compensation are less sensitive to volatility. In later tests, we show that options affect risk-taking even with controls for various other components of pay or total pay.

all highly correlated and affected by our instruments. Therefore, we cannot identify the effect of each of these on risk-taking, holding the other two constant. Instead, we measure the overall effect of an increase in options (something that should be of interest to boards and policy makers) when B-S value, delta, and vega increase simultaneously.[13] For brevity, we instrument for Black-Scholes value in our two-stage least squares estimates because we believe this to be the clearest summary measure of the magnitude of a grant and because it is the measure most commonly targeted by boards. However, instrumenting for delta or vega yields similar results. To emphasize this point, we also present reduced-form estimates of our outcomes regressed directly on our excluded instruments and controls, with the understanding that the coefficient on the excluded instrument represents a general effect of higher option value and associated higher delta and vega.

A fourth consideration is that we instrument for annual changes rather than levels in the value of new options granted. Fixed value plans tend to resemble a step function, so predicted first years do not necessarily correspond to higher option *levels* than other years if compensation is increasing over time. Instead, predicted first years correspond to above-average annual *changes* in options. If levels of outcomes are approximately linear functions of levels of options, then exogenous changes in options should affect annual changes in the level of outcomes. The same intuition applies even if there is also mean reversion in the outcome variables. Thus, for our main dependent variable, we use the annual change in volatility. For other firm outcomes, we use the annual changes for stock variables and levels for flow variables.

Finally, we have two instruments, which could typically be used together to obtain a more efficient estimator. However, we present the IV results separately for two reasons. First, we wish to use the two distinct IV strategies to cross-validate one another. As outlined above, each IV strategy requires a different identifying assumption, yet we show that our two instruments yield similar results across a range of firm outcomes. Thus, for both IV strategies to spuriously lead to similar results, both identifying assumptions would have to be violated. The second reason we do not use both instruments simultaneously is that the two instruments are defined with respect to different samples (i.e., the first instrument uses variation only within the set of fixed value executives while the second instrument compares fixed value to fixed number executives).

[13]The overall effect we measure will include convexity, magnification, and translation (wealth) effects from an increase in options. In later tests, we find that wealth effects do not drive our results by controlling for changes in total compensation.

4 Results

4.1 Instrumental Variables Strategy 1

We begin by instrumenting for the change in the value of new option grants using the indicator for whether a given year is predicted to be the first year of a new fixed value cycle. As described in Section 3 and the Appendix, the sample is restricted to executives on fixed value cycles, and we use predicted first years rather than actual first years to purge the estimation of bias from endogenous renegotiation and measurement error regarding the timing of cycles. In unreported results, we find that the predicted first year dummy indeed strongly predicts true fixed value first years in the data, with a t-statistic exceeding 100.

Panel B of Table 2 shows that the predicted first year indicator is strongly correlated with changes in the value of new options granted. Using the full sample of executives, predicted first years corresponds to a 7.2 percent increase in the Black-Scholes value of new options, a 7.7 percent increase in the delta of new options, and a 5.6 percent increase in the vega of new options. If we restrict the sample to CEOs and CFOs, the results are very similar, with slightly larger point estimates. All estimates are highly significant, with F-statistics greatly exceeding 10, the rule of thumb threshold for concerns relating to weak instruments. Again, the Black-Scholes value, delta, and vega of new at-the-money options are all highly correlated. For brevity, we focus on Black-Scholes value in the remainder of the analysis as our measure of the magnitude of an option grant.

In Table 3, we explore the effect of an increase in options on equity volatility, our primary measure of risk-taking. We measure volatility in two ways: the volatility of daily returns in the 12 months (252 trading days) following the grant date and the volatility of daily returns in the middle six months (120 trading days) of the 12 months following the grant date, excluding the first and last three months. The latter measure is designed to be less sensitive to temporary manipulation of volatility around option grants (although the 12-month volatility measure should also be robust to this concern, as discussed in Section 3.1). Both measures are annualized. Because our instrument predicts *changes* in option compensation, we use annual *changes* in volatility as our outcome.[14] The top panel presents the IV estimates from regressing the change in volatility on the change in the log Black-Scholes value of new option grants, as instrumented by the predicted first year indicator. The

[14]In unreported results, we also find a significant positive effect of an increase in options on the *level* of volatility.

bottom panel presents the reduced-form estimates from regressing the change in volatility directly on the instrument and other controls. In all specifications, for both the full sample and the subsample of CEOs and CFOs, we find that an increase in options leads to an increase in equity volatility. The results in Column 3 imply that a 10 percent increase in the value of new options corresponds to approximately a 0.02 unit increase in equity volatility relative to the median of 0.35, or a 5.7 percent increase in volatility.

Next, we explore possible channels that may drive this change in equity volatility. One prime candidate is leverage. Basic capital structure theory implies that, holding the assets and real activity of the firm constant, an increase in leverage will mechanically lead to an increase in equity volatility. Columns 1–3 of Table 4 show that an increase in option compensation does indeed lead to significant increases in leverage. Specifically, Column 1 implies that a 10 percent increase in the value of new options granted corresponds to a 0.0046 unit increase in market leverage. One issue with using market leverage, however, is that it may reflect changes in the market price of equity rather than active debt management. To address this, we use book leverage in Column 2 and an indicator for positive net issuance in Column 3. The results imply that a 10 percent increase in the value of new options granted corresponds to a 0.0074 unit increase in book leverage and a 2.7 percent greater probability of positive net issuance. Thus, the increase in leverage appears to be due to active debt management.

We can also estimate the proportion of the increase in equity volatility that can be explained by the increase in leverage. In unreported results, we find that a 10 percent increase in the value of new options is associated with a statistically significant 2.6 percent decline in the equity to assets ratio, which in turn implies approximately a 2.6 percent increase in equity volatility.[15] Thus, the

[15]This approximation is made by observing that

$$\begin{aligned} r_A &= r_E\left(\frac{E}{A}\right) + r_D\left(\frac{D}{A}\right) \\ \Rightarrow \sigma_A^2 &= \sigma_E^2\left(\frac{E}{A}\right)^2 + \sigma_D^2\left(\frac{D}{A}\right)^2 + 2\sigma_{DE}\left(\frac{E}{A}\right)\left(\frac{D}{A}\right) \\ \Rightarrow \sigma_A^2 &= \sigma_E^2\left(\frac{E}{A}\right)^2 \\ \Rightarrow \ln(\sigma_E) &= \ln(\sigma_A) - \ln(\frac{E}{A}) \end{aligned}$$

where the third line follows from the second, assuming that debt is approximately risk-free ($\sigma_D^2 = 0$) and uncorrelated with equity $\sigma_{DE} = 0$. Thus, a $X\%$ decline in $\frac{E}{A}$ leads to an approximately $X\%$ increase in σ^E.

increase in leverage accounts for nearly half ($2.6/5.7 = 46\%$) of the increase in volatility.

Next, we explore the effect of options on investment. These tests should be viewed as exploratory because it is not clear how an increase in investment should affect firm risk. While it may seem intuitive that investment increases risk, some argue that certain forms of investment, such as capital expenditures, decrease risk (Coles et al., 2006). Therefore, we examine the effect of option compensation on investment, leaving open the question of whether this contributes to the increase in volatility. In Columns 4–5 of Table 4, we find that a 10 percent increase in options leads to a significant 1.9 percent increase in capital expenditures and a 3.1 percent increase in total investment (defined as the sum of capital expenditures, R&D, acquisitions, and advertising expenses). In unreported tests, we also explore how options affect R&D, diversifying acquisitions, and non-diversifying acquisitions separately and find positive, albeit noisily estimated effects.

Columns 6–7 of Table 4 explore the effect of options on dividends. Column 6 shows that, among firms that already pay dividends, a 10 percent increase in options leads to 1.8 percent lower dividends. In Column 7, the effect of options on the decision to pay any dividends is also estimated to be negative, although the magnitude of the effect is small and insignificant. The decrease in dividends among dividend payers supports the validity of the instrumental variables methodology. We expect that, all else equal, an increase in options should lead to lower dividend payments because most executive stock options over the sample period are not dividend-protected. Therefore, option holders gain from reducing dividend payouts. These IV results also stand in stark contrast to the positive correlation between dividend growth and options, as shown later in Table 10. The OLS results are likely driven by the problem that firms that are doing well are likely to increase both dividend payouts and option payouts. This issue highlights the importance of the instrumental variables strategy in estimating the true effects of options on executive behavior.

Finally, Columns 8–10 of Table 4 show that options lead to flat or negative changes in firm performance. Equity returns in the 12 months following the increase in option grants are flat, while measures of operating performance such as ROA and cash flow to assets are significantly lower. However, we do not interpret the reduction in short-term operating performance as conclusive evidence that executives increase volatility at the cost of firm performance. A short-run decline in ROA or cash flows can also reflect a shift toward future-oriented projects that deliver back-loaded cash flows.

As discussed in detail in Section 3, one may be concerned that predicted first years will tend to coincide with turnover, product cycles, or major performance reviews. Empirically, we find that expected cycle termination is uncorrelated with turnover, and our conversations with compensation consultants suggest that performance reviews are typically performed annually instead of at cycle termination. However, we want to ensure that our results are robust to the possibility that cycle termination is correlated with firm unobservables that may directly affect firm risk. Therefore, in the next section, we explore a second instrument for changes in option pay that exploits variation in pay *within* cycles across executives rather than at cycle termination.

4.2 Instrumental Variables Strategy 2

We turn now to our second source of variation, which exploits the fact that the value of options granted within fixed number cycles is more sensitive to market movements than the value of options granted within fixed value cycles. Following the methodology described in Section 3.2, the excluded instrument is the interaction between the fixed number indicator and the industry return. Again, our analysis does not assume that plan type is randomly assigned. Instead, our IV strategy requires that fixed value and fixed number executives do not have different non-compensation-induced responses to changes in aggregate returns, an assumption we find support for in later placebo tests.

In Table 5, we show that the instrument significantly predicts changes in the Black-Scholes value, delta, and vega of new option grants. For a one standard deviation change in the industry return, executives on fixed number plans receive an additional 12 percent increase in the value of option grants relative to executives on fixed value plans. Again, for brevity, we instrument for changes in Black-Scholes value in the remainder of our analysis. However, we also present reduced-form estimates of outcomes regressed directly on our excluded instrument and controls. The coefficient on the excluded instrument represents a general effect of higher option value and associated higher delta and vega on behavior.

We begin again by exploring the effect of an increase in options on changes in volatility. Table 6 reports results for the full sample of executives as well as the CEO/CFO subsample and clusters standard errors by firm to adjust for within-firm correlations. Using this second instrumental variables strategy, we again find that an increase in the value of new option grants leads to an increase in equity volatility. The estimated magnitudes are smaller, but qualitatively similar. The result in Column 3 implies that a 10 percent increase in the value of new options granted leads to a 0.0084 increase in equity volatility, or a 2.4 percent increase relative to median volatility.

We again find that a major mechanism driving the change in volatility is an increase in firm leverage. Columns 1–3 of Table 7 show that a 10 percent increase in the value of new options granted leads to a .0049 unit increase in market leverage, a 0.0061 unit increase in book leverage, and a 1.9 percent increase in the probability of positive net issuance. We estimate that 38 percent of the increase in equity volatility is due to increased leverage. Thus, the results once again suggest that leverage is actively increased in response to increases in option compensation.

Columns 4–10 of Table 7 again explore the effect of changes in option compensation on investment, dividend policy, and firm performance. The results are similar to those found using the first instrument, although the magnitudes differ slightly. We find that an increase in options leads to a marginally significant positive increase in capital expenditures with noisily estimated effects for total investment. Dividend growth falls significantly, which is again consistent with the view that options incentivize against dividend payouts because most executive stock options are not dividend-protected. Finally, an increase in options leads to lower returns and operating performance. In contrast to the results using the first instrument, here, the decline in returns is marginally significant, while the decline in cash flows to assets becomes insignificant.[16] Overall, we again find that increased option compensation leads to increased volatility, driven in large part by increases in leverage.

As described earlier, the validity of our second instrumental variables strategy rests upon the assumption that fixed number and fixed value executives do not have differential non-compensation-related responses to industry returns. If this assumption holds, then the differential sensitivity of firm outcomes to industry returns for fixed number firms must be due to the differential sensitivity of their option compensation. To test whether the data support this assumption, we compare the responses of fixed number and fixed value executives to industry returns in years in which the executive is not awarded options according to any multi-year plan. This placebo test exploits the fact that both fixed number and fixed value cycles grew in popularity prior to the 1990s (due to the

[16] Using both instruments, we find that options lead to a smaller decline in cash flow to assets than in ROA. This is unsurprising given that options lead to increased leverage. The increase in leverage reduces a firm's tax burden through the debt tax shield, which contributes to after tax cash flows, but not to ROA. Therefore, increased leverage dampens the reduction in cash flows but not the reduction in ROA.

rise of options compensation more generally) and fell in popularity after 2005, which is likely due to peer benchmarking disclosure requirements that led to option grants being adjusted annually. We estimate the following regression:

$$Y_{ijt} = \beta_0 + \beta_1 I_{ijt}^{FN\,Placebo} + \beta_2 R_{kt} + \beta_3 I_{ijt}^{FN\,Placebo} R_{kt} + \gamma_t + v_j + \epsilon_{ijt},$$

restricting the sample to executives who are not currently on a cycle but were on a fixed number or fixed value cycle in some other year. The variable $I_{ijt}^{FN\,Placebo}$ is an indicator for whether the executive was on a fixed number cycle in some other year. A β_3 close to zero would provide evidence that fixed number and fixed value executives respond similarly to market movements absent compensation effects.

Table 8 shows that, across all the previously examined outcomes, fixed number and fixed value executives react similarly to changes in industry returns in years in which the executive is not awarded options according to either type of multi-year plan. It is further reassuring that the placebo sample is similar in size to the IV sample and that the point estimates are close to zero with small standard errors, suggesting that β_3 is a well-estimated zero effect. These placebo results support the view that the differential responses of fixed number and fixed value executives to industry returns in the years when options are awarded according to these cycles are due to the incentives from option compensation rather than other factors.

4.3 Heterogeneity

Thus far, we have reported the average effect of changes in the value of new option grants on executive risk-taking. In this section, we explore whether this effect varies with the total amount of options held by the executive as well as by the executive's position within the firm and the firm's industry.

We suspect that the marginal effect of new option grants on risk-taking may be weaker if the executive already holds a sizable portfolio of unexercised options that were granted in the past. Executives tend to hold previous granted options because only 35 percent of a typical option grant vests within one year (Cadman et al., 2012). Options usually vest linearly, for example, 33 percent annually over three years (Hall and Murphy, 2002). While we do not have precise measures of each executive's portfolio of unexercised options prior to 2006, we approximate option holdings during these years following the procedure of Core and Guay (2002).

In Panel A of Table 9, we re-estimate our baseline reduced form specifications from Column 3 of Tables 3B and 6B. Specifically, the change in volatility is regressed on Pred First Year in Columns 1–3 and on FN $\times$ Ind Return in Columns 4–6.[17] For each observation, we calculate the ratio of the value of new options to all unexercised options at the grant date. Using this ratio, we divide the sample into terciles corresponding to whether new option grants account for a low, medium, or high fraction of all options held. For both instruments, we find that the effect of new options on risk-taking is indeed greater when new options are a higher fraction of all options held. Moreover, the p-values in the bottom row show that the differences between the first and third terciles are significant at approximately the 5 percent level for both instruments.

In Panel B, we instead split the sample based on whether the firm belongs to the financial or high-tech sectors. Executives in these sectors may have relatively greater ability to manipulate risk beyond merely changing leverage. For example, many allege that the recent rise of complicated derivative products combined with deregulation allowed executives in the financial sector to be particularly sensitive to risk-taking incentives (Rajan, 2005; French et al., 2010). Similarly, the high-tech sector is characterized by high rates of innovation and high information asymmetry, which may allow executives to increase risk by pursuing riskier product development or by manipulating the release of information (Balkin et al., 2000; Benmelech et al., 2010; Graham et al., 2005). We test these theories by comparing the effect of options on risk-taking within the finance and high-tech sectors with the effect in other industries. Using the first instrument in Columns 1–2, we find that the effect of options on risk-taking is approximately 50 percent larger in these sectors. Using the second instrument in Columns 3–4, we find an even larger difference of 150 percent. The large differences in magnitudes support the hypothesis that executives in the finance and high-tech sectors respond more strongly to changes in options. However, we caution that these results are only suggestive. After splitting the sample, we lack statistical power to establish significant *differences* between finance/high-tech executives and other executives (p-values for the test of differences

[17] Instead of showing the IV specifications, Table 9 shows the reduced form in which the outcome is regressed directly on the instrument and controls. This is done so that we can more easily report p-values, which test whether coefficients are different across columns. We arrive at these p-values by, e.g. estimating Columns 1–3 in Panel A within a single OLS regression, where standard errors are clustered by firm.

between coefficients are in the range of 0.3).

Finally, in Panel C, we split the sample based on whether the executive is a CEO/CFO or a lower-level executive (our sample usually covers the top five executives within each firm). Given that CEOs and CFOs have greater power than other top executives, one might expect the effect of options on risk-taking to be greater for them. On the other hand, recent survey evidence in Graham et al. (2011) suggests that CEOs and CFOs delegate many decisions; thus, other executives may be able to affect risk-taking. Using both instruments, we find larger point estimates for CEOs and CFOs, but the differences are not statistically significant. The reason for the insignificant differences may be two-fold. First, other top executives may indeed have effects on risk-taking. Second, our methodology may lack the power to distinguish between CEOs/CFOs and other executives. As mentioned in Section 2.4, cycles tend to be coordinated across executives within the same firm. Conditional on an executive in a firm being on a fixed number cycle and the CEO of the same firm being at the start of a cycle, the (sample) probability that the executive is also at the start of a cycle is 79.4 percent. For fixed value, this probability is 70.4 percent. This implies that, even when the sample is restricted to the non-CEO/CFOs, the instrument could still pick up changes in risk operating through the CEO or CFO.

4.4 Comparison of IV with Endogenous OLS

We exploit cycle-induced variation in option grants because we suspect that the correlation between firm outcomes and option grants may be driven by other unobserved factors. In Table 10, we show the endogenous relationships between option grants and firm outcomes as estimated using OLS. The sample is limited to the set of executive-firm-years used in at least one of the two instrumental variables strategies. As in the IV estimation, we include firm fixed effects to control for fixed differences in mean growth rates across firms. The OLS procedure leads to estimates that are very different, often of the opposite sign, relative to those from the IV procedures. Using OLS, an increase in option grants is correlated with significant decreases in firm returns and leverage, and significant increases in volatility, investment, dividends, and operating performance. The results are suggestive of strong endogeneity bias in the OLS estimation. For example, it may be the case that firms that have done well in the past tend to increase options, and these firms also tend to have lower returns in the year following the pay raise relative to the high returns in the previous year.

Growth firms may tend to award more in options and engage in high levels of investment. Finally, firms that have have done well may tend to increase both dividends and option grants, resulting in a positive correlation between the two. This stands in sharp contrast to the IV results, which find a negative causal relationship between options and dividends, as predicted by the fact that most executive options are not dividend-protected and decline in value following dividend payments.

4.5 Discussion and Robustness

Our analysis shows that, *all else equal*, an increase in option compensation leads to an increase in firm volatility that is driven in part by increases in leverage. However, aggregate equity volatility and leverage among large public firms remained approximately stable over our sample period. In light of these aggregate trends, our results suggest that other factors are needed to explain broad changes in volatility and leverage. For example, it is possible that other factors pushed executives to reduce risk, but the rise in options dampened their response. Similarly, our findings that, *all else equal*, options lead to lower dividend payouts (among dividend payers) cannot fully explain aggregate trends in dividend payouts during our sample period. Instead, other factors, such as tax incentives, likely affected dividend policy, and the increase in options led executives to increase dividends *less* than they would have absent the growth in options.

We also note that, so far, we have explored the overall net effect of an increase in options on executive behavior. Again, options can affect behavior through a translation (wealth) channel in addition to various risk incentives tied to convexity. For example, an increase in options (like an increase in cash compensation), increases an executive's wealth, which in turn could lead to an increase in risk tolerance. Columns 1 and 2 of Table 11A presents suggestive evidence that the wealth channel does not drive our results. We control for the change in log total compensation (the sum of the grant date values of salary, bonus, restricted stock, options, and other compensation) and continue to find a strong effect of options on risk-taking behavior. This suggests that the composition of pay matters. Risk-taking increases when options as a fraction of total pay increase.

In Columns 3 and 4 of Panel A, we perform a related test. Using our two IV strategies, we find similar results for the effect of options on risk-taking after controlling separately for all other components of compensation: salary, bonus, restricted, stock, and any other compensation. As noted in Section 3.3, we find that salary and bonus do not increase with our instruments. If anything, cash compensation appears to offset changes in options induced by multi-year option cycles, although the effect is only marginally significant. We also find that our instrument does not predict changes in restricted stock. In Columns 5 and 6, we limit the sample to years in which no restricted stock is awarded, and find similar results.

Panel B of Table 11 presents additional robustness checks. Columns 1 and 2 show that our results cannot be explained by the potentially endogenous choice of cycle length. In particular, our first set of IV results using the predicted first year instrument are instead driven by the staggering of when first years occur among executives on cycles of equal length. We re-estimate our two IV specifications after restricting the sample to executives on two-year cycles and find similar results. Next, we show that our results are insensitive to assumptions regarding the treatment of executives who receive more than one option grant per year. As noted previously, an executive may receive one grant as part of a firm-wide long-term incentive plan as well as another grant that is part of a fixed value or number plan. Columns 3 and 4 show that limiting our analysis to the subsample of executive-years with a single grant yields similar results. Finally, Columns 5 and 6 show that our results are robust to the choice of fixed effects. In all specifications, we use annual changes in firm outcomes as our dependent variable, so our results cannot be explained by fixed level differences across firms. In all specifications, we also include firm fixed effects to control for fixed growth rate differences across firms. Adding executive by firm fixed effects further controls for fixed growth rate differences across executive by firm regimes. We find very similar results using executive by firm fixed effects.

5 Conclusion

In this paper, we explore the effect of executive option grants on risk-taking using two sources of variation induced by the institutional features of multi-year grant cycles. First, the value of new options grants increases by a large discrete amount in years that are predicted to be the start of a new fixed value cycle. Second, fixed number executives receive option grants that are more sensitive to market movements than fixed value executives. These two types of variation help to cross-validate one another: our two IV methodologies yield similar results across a range of firm outcomes.

We find that, on average, executives lie in a region of their utility function in which moderate increases in options lead to increased firm equity volatility. A significant portion of this increase in volatility is driven by increases in leverage. An increase in option grants also leads to significantly lower dividend growth, weakly higher investment, and weakly lower firm performance. Returning to the theory, we know that the effect of options on risk-taking may be non-monotonic. Very large option grants that are awarded to risk-averse and undiversified executives may lead to reduced risk-taking. Nevertheless, our estimates should be informative for policy makers and boards who are interested in the effects of moderate changes to existing options packages.

We think that this study represents a significant step forward in quantifying the average direction and magnitude of the causal effect of options on risk-taking. Whether options lead to excessive risk-taking or simply increase risk-taking to its optimal level may be more firm-specific. We believe that both effects could be in play. In the case of our natural experiment, we are skeptical that optimal risk-taking follows the exact same idiosyncratic patterns as the quasi-exogenous changes in option compensation that we observe. This suggests that increases in options can lead to unintended consequences in terms of excessive risk-taking and leverage. On the other hand, if boards or policy makers believe that risk-averse executives are taking suboptimally low amounts of risk, moderate increases in options may be an effective way to encourage executives to increase risk-taking.

Figure 1

Prevalence of Multi-Year Plans over Time

This figure illustrates the prevalence of multi-year plans over time. The area under the bottom curve represents the percent of executives that are on fixed-number plans, conditional on being paid options that year. The area between the top and bottom curves represents the percentage of executives that are on fixed-value plans, conditional on being paid options that year.

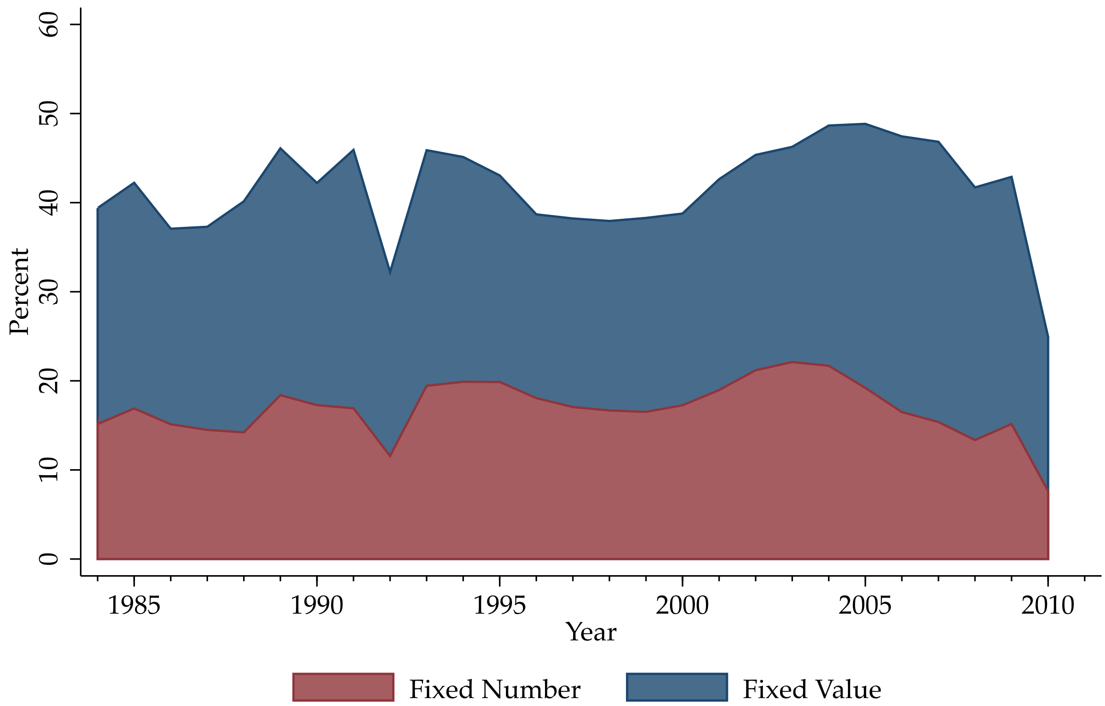
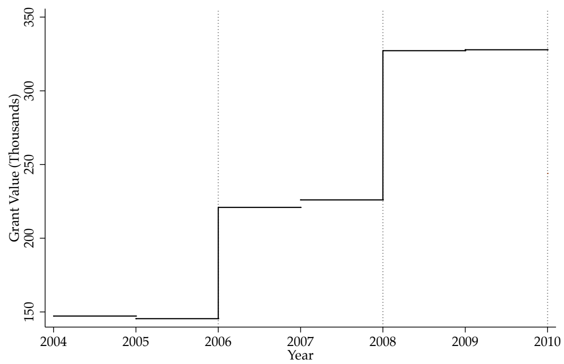


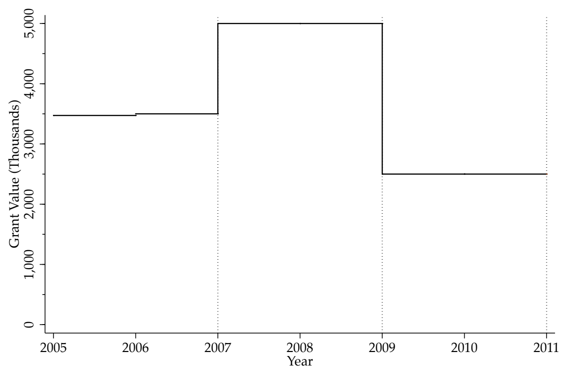
Figure 2
Real Examples of Fixed Value Cycles and Predictions

This figure represents three examples of fixed value cycles taken from the data. Years that we predict to be cycle first years are indicated by dotted vertical lines.

Panel A: Nucor



Panel B: US Bancorp



Panel C: Thomas & Betts

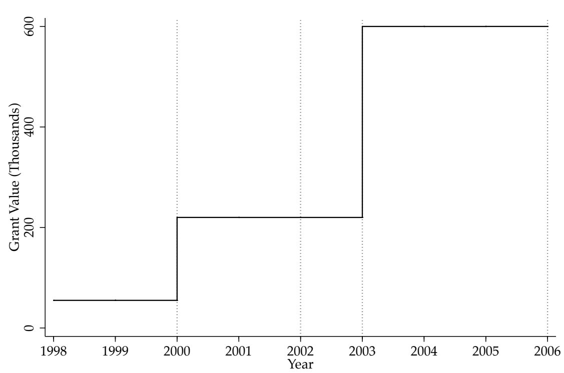


Table 1
Summary Statistics

This table shows various summary statistics. Panel A shows the distribution of cycle length, with observations at the executive-year level. Panel B shows the industry distribution, broken down by the CEO's plan type. Industries are categorized using the Fama-French 12-industry classification scheme. Panel C compares other firm and executive characteristics across cycle types, showing the 25th, 50th, and 75th percentiles of the distributions. Because there are time trends in the prevalence of fixed number and fixed value cycles, we do not pool all years. Panels B and C show only summary statistics from fiscal year 2000. Fiscal years 1995 and 2005 are shown in the Appendix.

Panel A: Length of Cycles

	Fixed Number		Fixed Value	
	Freq	Percent	Freq	Percent
2	20516	66.88	40036	92.59
3	6180	20.15	2769	6.40
4	2288	7.46	332	0.77
5	900	2.93	45	0.10
≥ 6	792	2.58	56	0.13
Total	30676	100.00	43238	100.00

Panel B: Industry Distribution

Year: 2000	Fixed Number	Fixed Value	Other
	Percent	Percent	Percent
Consumer Non-Durables	7.10	6.11	5.70
Consumer Durables	2.56	3.67	2.06
Manufacturing	11.93	13.65	10.66
Energy	3.98	3.87	3.80
Chemicals	2.56	3.46	2.37
Business Equipment	17.33	14.87	18.73
Telecommunications	3.41	2.24	2.96
Utilities	5.11	6.11	3.75
Shops	9.38	10.59	10.82
Health	8.52	6.11	9.29
Finance	16.19	19.35	17.94
Other	11.93	9.98	11.93
Total	100.00	100.00	100.00

Table 1
(continued)

Panel C: Other Characteristics

Year: 2000	Fixed Number			Fixed Value			Other		
	p25	p50	p75	p25	p50	p75	p25	p50	p75
Firm-Level:									
Assets (Millions)	461.58	1306.66	4954.32	563.47	1492.38	6358.76	278.02	805.48	2552.21
Sales (Millions)	349.77	877.12	2482.68	389.60	1103.31	3867.47	180.95	540.79	1778.17
Market to Book	1.09	1.36	2.22	1.09	1.37	2.14	1.04	1.36	2.30
Volatility (12 Months)	0.33	0.46	0.70	0.32	0.43	0.63	0.34	0.51	0.80
Volatility (120 Trading Days)	0.38	0.50	0.72	0.38	0.49	0.67	0.41	0.55	0.81
CAPX / PPE	0.14	0.25	0.44	0.14	0.22	0.42	0.15	0.27	0.56
Acquisitions (Millions)	0.00	0.00	44.56	0.00	0.00	40.27	0.00	0.00	16.18
Market Leverage	0.05	0.23	0.46	0.06	0.22	0.43	0.03	0.20	0.47
Book Leverage	0.16	0.39	0.57	0.17	0.39	0.57	0.06	0.36	0.58
Total Dividends (Millions)	0.00	2.10	33.07	0.00	6.52	50.84	0.00	0.00	13.69
Firm Return	-0.22	0.08	0.39	-0.22	0.09	0.42	-0.32	0.02	0.41
Return on Assets	0.08	0.14	0.23	0.08	0.15	0.22	0.04	0.13	0.22
Cash Flow / Assets	0.07	0.10	0.17	0.07	0.11	0.17	0.03	0.09	0.16
Executive-Level:									
Salary (Thousands)	218.40	310.00	495.12	240.00	345.00	516.91	215.00	301.08	454.00
Bonus (Thousands)	64.00	185.00	406.25	85.00	207.48	450.77	35.70	149.50	362.30
Number New Options	20.00	50.00	100.00	24.00	50.00	116.70	28.00	68.75	176.80
Number Prev Options	76.00	173.06	430.00	76.00	170.25	434.65	33.42	139.08	388.00
B-S Value New Options (Thousands)	201.59	554.49	1493.27	231.84	663.82	1807.65	256.02	766.52	2522.95
B-S Value Prev Options (Thousands)	603.62	2115.94	6836.39	599.14	2017.49	6706.96	171.73	1388.81	5313.79
Delta New Options	2.74	7.45	19.60	3.25	9.00	24.34	3.47	10.14	31.56
Delta Prev Options	9.14	29.97	90.49	9.71	30.13	95.90	2.98	20.15	73.37
Vega New Options	1.82	5.16	14.11	2.49	6.80	18.80	2.08	6.34	20.74
Vega Prev Options	5.03	14.82	41.27	6.00	16.88	48.03	1.33	9.10	31.32

Table 2
IV1: First Stage

Panel A of this table shows how option compensation changes in fixed value cycle first years. Panel B shows how option compensation changes in fixed value cycle *predicted* first years. Observations are at the executive-year level. The sample is limited to executives who are currently on fixed value cycles or were in the previous year. First Year is an indicator variable equal to one in the year following the final year of a cycle. Predicted First Year is an indicator variable equal to one if the year is predicted to be a cycle first year based on the length of the previous cycle (see Section 3 for a detailed discussion of our predictions methodology). The variable B-S Value equals the Black-Scholes value of new option compensation, Delta equals the change in the Black-Scholes value of new option compensation associated with a 1 percent change in the price of the underlying, and Vega equals the change in the Black-Scholes value of new option compensation associated with a 0.01 change in the annualized volatility of the underlying. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Real First Years

	Δ Log B-S Value		Δ Log Delta		Δ Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
First Year	0.0768***	0.0790***	0.0831***	0.0875***	0.0595***	0.0610***
	(0.00937)	(0.0121)	(0.00900)	(0.0117)	(0.0109)	(0.0140)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO	All	CEO/CFO
R^2	0.178	0.223	0.166	0.217	0.227	0.280
Observations	37589	16464	36863	16175	36863	16175

Panel B: Predicted First Years

	Δ Log B-S Value		Δ Log Delta		Δ Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
Pred First Year	0.0719***	0.0748***	0.0771***	0.0815***	0.0563***	0.0586***
	(0.00900)	(0.0117)	(0.00870)	(0.0114)	(0.0105)	(0.0136)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO	All	CEO/CFO
R^2	0.177	0.223	0.165	0.217	0.227	0.280
Observations	37589	16464	36863	16175	36863	16175

Table 3
IV1: Volatility

Panel A shows IV estimation results, where the variable ΔLog B-S Value is instrumented using the Predicted First Year indicator, as defined in Table 2. Observations are at the executive-year level. The sample is limited to executives who are currently on fixed value cycles or were in the previous year. We measure volatility in two ways: 1) the annualized volatility of daily returns in the first 252 trading days following the grant date, i.e., approximately 12 months, and 2) the annualized volatility of daily returns in the middle 120 trading days of the year following the grant date, i.e., excluding the beginning and end of the year. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the Predicted First Year instrument. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IV Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Δ Log B-S Value	0.145***	0.158***	0.199***	0.215***
	(0.0286)	(0.0369)	(0.0381)	(0.0499)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
F-Stat (1st Stage)	66.83	41.60	66.34	41.02
Observations	36927	16171	37037	16212

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First	0.0107***	0.0120***	0.0146***	0.0162***
	(0.00177)	(0.00228)	(0.00231)	(0.00306)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
R^2	0.518	0.550	0.454	0.487
Observations	36927	16171	37037	16212

Table 4

IV1: Other Outcomes

Panel A shows IV estimation results, where the variable ΔLog B-S Value is instrumented using the Predicted First Year indicator, as defined in Table 2. Observations are at the executive-year level. The sample is limited to executives who are currently on fixed value cycles or were in the previous year. The variable Mkt Lev represents market leverage, which is defined as total debt (short-term plus long-term) divided by the market value of assets. The variable Bk Lev represents book leverage, which is defined as total debt (short-term plus long-term) divided by the book value of assets. The variable Debt ↑ is an indicator equal to one if net debt issuance is positive, i.e., if total debt increased relative to the previous year. The variable Capx represents capital expenditures, and Tot Inv represents total investment, i.e., the sum of capital expenditures, R&D, acquisitions, and advertising expenses. The variable Return represents the stock return in the 12 months following the grant date, ROA represents the return on assets, and CF/Assets represents the ratio of cash flow to assets. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the Predicted First Year instrument. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IV Estimation

	Debt			Investment		Dividends		Performance		
	(1) Δ Mkt Lev	(2) Δ Bk Lev	(3) Debt ↑	(4) Log Capx	(5) Log Tot Inv	(6) Δ Log Div	(7) Δ Div Pay	(8) Return	(9) ROA	(10) CF/Assets
Δ Log B-S Value	0.0464**	0.0740***	0.274***	0.187**	0.312**	-0.177**	-0.00877	-0.0960	-0.0599***	-0.0536***
	(0.0210)	(0.0274)	(0.106)	(0.0944)	(0.131)	(0.0768)	(0.0357)	(0.0884)	(0.0187)	(0.0192)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	55.82	55.39	55.74	55.55	58.02	48.44	55.90	64.95	46.01	40.45
Observations	30424	30529	30640	30033	30325	16972	30634	37192	27537	27935

Panel B: Reduced Form Estimation

	Debt			Investment		Dividends		Performance		
	(1) Δ Mkt Lev	(2) Δ Bk Lev	(3) Debt ↑	(4) Log Capx	(5) Log Tot Inv	(6) Δ Log Div	(7) Δ Div Pay	(8) Return	(9) ROA	(10) CF/Assets
Predicted First	0.00342**	0.00542***	0.0201***	0.0138**	0.0235**	-0.0154**	-0.000645	-0.00697	-0.00416***	-0.00353***
	(0.00148)	(0.00186)	(0.00739)	(0.00690)	(0.00947)	(0.00615)	(0.00262)	(0.00650)	(0.00108)	(0.00108)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.256	0.184	0.281	0.931	0.869	0.308	0.166	0.314	0.777	0.724
Observations	30424	30529	30640	30033	30325	16972	30634	37192	27537	27935

Table 5
IV2: First Stage

This table shows the differential sensitivity of the option compensation of fixed number and fixed value executives to industry returns. Observations are at the executive-year level. The sample is limited to executives are on either fixed number or fixed value plans (excluding the first years of cycles). The variable FN is an indicator equal to one if the executive is on a fixed number plan. Industry returns are defined as the Fama-French (49) industry return of the executive's firm in the 12 months preceding the option grant associated with the cycle. Other variables are defined as in Table 2. The main effects of interaction terms are included in all specifications but not shown. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Δ Log B-S Value		Δ Log Delta		Δ Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
FN $\times$ Ind Return	0.481***	0.524***	0.511***	0.583***	0.451***	0.513***
	(0.0339)	(0.0474)	(0.0328)	(0.0427)	(0.0540)	(0.0724)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO	All	CEO/CFO
R^2	0.345	0.401	0.330	0.402	0.381	0.448
Observations	23466	10207	22974	10002	22974	10002

Table 6

IV2: Volatility

This table shows the IV and reduced form results for volatility, where ΔLog B-S Value is instrumented using FN × Ind Return as defined in Table 5. Observations are at the executive-year level. The sample is limited to executives are on either fixed number or fixed value plans (excluding the first years of cycles). All other variables are as defined in Table 3. The main effects of interaction terms are included in all specifications but not shown. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IV Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Δ Log B-S Value	0.0578**	0.0698**	0.0837**	0.108***
	(0.0263)	(0.0316)	(0.0327)	(0.0395)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
F-Stat (1st Stage)	194.7	116.2	195.6	117.8
Observations	23099	10043	23153	10062

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
FN x Ind Return	0.0275**	0.0360**	0.0399**	0.0561***
	(0.0125)	(0.0163)	(0.0155)	(0.0206)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
R^2	0.579	0.625	0.524	0.577
Observations	23099	10043	23153	10062

Table 7

IV2: Other Outcomes

This table shows the IV and reduced form results for other firm outcomes, where ΔLog B-S Value is instrumented using FN × Ind Return as defined in Table 5. Observations are at the executive-year level. The sample is limited to executives are on either fixed number or fixed value plans (excluding the first years of cycles). All other variables are as defined in Table 4. The main effects of interaction terms are included in all specifications but not shown. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IV Estimation

	Debt			Investment		Dividends		Performance		
	(1) Δ Mkt Lev	(2) Δ Bk Lev	(3) Debt ↑	(4) Log Capx	(5) Log Tot Inv	(6) Δ Log Div	(7) Δ Div Pay	(8) Return	(9) ROA	(10) CF/Assets
Δ Log B-S Value	0.0485***	0.0611***	0.188**	0.184*	-0.00453	-0.238**	-0.0475	-0.173*	-0.0309*	-0.0136
	(0.0158)	(0.0194)	(0.0847)	(0.107)	(0.118)	(0.118)	(0.0296)	(0.0905)	(0.0163)	(0.0145)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	190.2	190.3	186.4	187.1	186.1	61.05	188.5	198.6	177.0	173.3
Observations	19157	19210	19284	18918	19104	10232	19279	23256	17372	17579

Panel B: Reduced Form Estimation

	Debt			Investment		Dividends		Performance		
	(1) Δ Mkt Lev	(2) Δ Bk Lev	(3) Debt ↑	(4) Log Capx	(5) Log Tot Inv	(6) Δ Log Div	(7) Δ Div Pay	(8) Return	(9) ROA	(10) CF/Assets
FN x Ind Return	0.0247***	0.0311***	0.0944**	0.0933*	-0.00228	-0.0862**	-0.0240	-0.0831*	-0.0158**	-0.00694
	(0.00773)	(0.00964)	(0.0420)	(0.0537)	(0.0592)	(0.0395)	(0.0146)	(0.0441)	(0.00800)	(0.00723)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.355	0.283	0.355	0.932	0.874	0.402	0.263	0.387	0.809	0.765
Observations	19157	19210	19284	18918	19104	10232	19279	23256	17372	17579

Table 8

IV2: Placebo Test

This table shows the reduced form results for outcomes from Tables 5-7 using a placebo sample restricted to executives who are not currently on a cycle, but were on a fixed number or fixed value cycle in some other year (in the past or future). FN Placebo is an indicator variable equal to one if the executive was on a fixed number cycle in some other year. The main effects of the interaction terms are included in all specifications but not shown. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Reduced Form - Option Compensation, Volatility, Debt

	First Stage	Volatility		Debt		
	(1) Δ Log B-S Value	(2) Δ 12 Month Vol	(3) Δ 120 TD Vol	(4) Δ Mkt Lev	(5) Δ Bk Lev	(6) Debt $\uparrow$
FN Placebo × Ind Return	0.00710	0.0180	0.0101	-0.00449	-0.00963	-0.0243
	(0.107)	(0.0150)	(0.0183)	(0.00890)	(0.0111)	(0.0380)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.239	0.557	0.516	0.300	0.248	0.311
Observations	13955	18260	18396	15379	15673	18341

Panel B: Reduced Form - Investment, Dividends, Performance

	Investment		Dividends		Performance		
	(1) Log Capx	(2) Log Tot Inv	(3) Δ Log Div	(4) Δ Div Pay	(5) Return	(6) ROA	(7) CF/Assets
FN Placebo × Ind Return	0.00123	-0.0334	0.0415	-0.0120	-0.00507	-0.0101	-0.00555
	(0.0614)	(0.0626)	(0.0596)	(0.0169)	(0.0536)	(0.00863)	(0.00978)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.918	0.862	0.411	0.243	0.337	0.749	0.676
Observations	17613	17934	7550	15777	21536	16124	16290

Table 9
Heterogeneity

This table re-estimates the baseline reduced form specifications from Column 3 of Tables 3B and 6B. The change in 120 trading day volatility is regressed on Pred First Year in the left-most columns and on FN $\times$ Ind Return in the right-most columns. In Panel A, the sample is split into terciles based upon the ratio of the value of new options to all unexercised options as of the grant date of the new options. In Panel B, the sample is split based upon whether the company is in the finance (SIC 6000-6099) or high-tech sectors ("high-tech" in Fama-French 5-Industry Classification). In Panel C, the sample is split based upon whether the executive is a CEO or CFO. The p-values of differences between coefficients are shown below. The main effects of the interaction terms are included but not shown. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Percent New Options

DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year			Instrument = FN $\times$ Ind Return		
	(1) Low	(2) Med	(3) High	(4) Low	(5) Med	(6) High
Instrument	0.00993***	0.0129***	0.0204***	-0.0106	0.0278	0.0690**
	(0.00346)	(0.00391)	(0.00441)	(0.0243)	(0.0280)	(0.0304)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
P-Value (Diff Low)	-	0.550	0.0576	-	0.287	0.0407
R^2	0.341	0.357	0.375	0.353	0.347	0.344
Observations	11438	11438	11438	7168	7168	7167

Panel B: Finance and High Tech

DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year		Instrument = FN $\times$ Ind Return	
	(1) No Fin/Tech	(2) Fin/Tech	(3) No Fin/Tech	(4) Fin/Tech
Instrument	0.0124***	0.0181***	0.0193	0.0480**
	(0.00276)	(0.00413)	(0.0213)	(0.0221)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
P-Value (Diff)	-	0.248	-	0.350
R^2	0.351	0.383	0.327	0.392
Observations	24045	12992	15020	8133

Panel C: CEO/CFO

DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year		Instrument = FN $\times$ Ind Return	
	(1) No CEO/CFO	(2) CEO/CFO	(3) No CEO/CFO	(4) CEO/CFO
Instrument	0.0125***	0.0162***	0.0321*	0.0561***
	(0.00282)	(0.00306)	(0.0192)	(0.0206)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
P-Value (Diff)	-	0.292	-	0.300
R^2	0.345	0.374	0.334	0.360
Observations	20825	16212	13091	10062

Table 10
OLS Endogenous Correlations

This table shows the OLS results of regressing various firm outcomes on Δ Log B-S Value. The sample is limited to the set of observations used in at least one of the two instrumental variables strategies. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Volatility, Investment, Dividends

	Volatility		Investment		Dividends	
	(1) Δ 12 Month Vol	(2) Δ 120 TD Vol	(3) Log Capx	(4) Log Tot Inv	(5) Δ Log Div	(6) Δ Div Pay
Δ Log B-S Value	0.00815***	0.0128***	0.0248***	0.0400***	0.0383***	0.00613**
	(0.00185)	(0.00224)	(0.00796)	(0.00960)	(0.00726)	(0.00260)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.517	0.453	0.928	0.866	0.311	0.169
Observations	41109	41229	33570	33907	18566	34253

Panel B: Debt, Performance

	Debt			Performance		
	(1) Δ Mkt Lev	(2) Δ Bk Lev	(3) Debt ↑	(4) Return	(5) ROA	(6) CF/Assets
Δ Log B-S Value	-0.00939***	-0.00762***	-0.00217	-0.0361***	0.0157***	0.0136***
	(0.00145)	(0.00178)	(0.00629)	(0.00715)	(0.00119)	(0.00127)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.262	0.183	0.281	0.309	0.780	0.725
Observations	34022	34135	34261	41392	30803	31204

Table 11
Robustness

This table re-estimates the baseline IV specifications from Column 3 of Tables 3A and 6A. In both panels, changes in option compensation are instrumented using the Predicted First Year indicator in odd-numbered columns and FN $\times$ Ind Return in even-numbered columns. In the first two columns of Panel A, controls for the annual change in log total compensation (the sum of the grant date values of salary, bonus, restricted stock grants, option grants, and all other compensation) are added to the baseline IV specifications. In Columns 3 and 4, separate controls for the annual changes in the grant date values of the logarithms of salary, bonus, restricted stock grants, and all other compensation are added to the baseline IV specifications. In Columns 5 and 6, the sample is restricted to executive-years in which no restricted stock is granted. In the first two columns of Panel B, the sample is restricted to two-year cycles. In Columns 3 and 4, the sample is restricted to the set of executive-years in which only one option grant was awarded. In Columns 5 and 6, we include executive$\times$firm fixed effects instead of firm fixed effects. The main effects of interaction terms are included in all specifications but not shown. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Other Compensation

DEP VAR: Δ 120 TD VOL	Total Comp Control		Component Controls		No Restricted Stock	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log B-S Value	0.247***	0.0980**	0.176***	0.0822**	0.142***	0.109***
	(0.0486)	(0.0399)	(0.0337)	(0.0330)	(0.0355)	(0.0348)
IV Type	IV1	IV2	IV1	IV2	IV1	IV2
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	58.26	177.5	79.14	195.4	61.58	165.1
Observations	35993	22230	35993	22230	23532	15673

Panel B: Additional Tests

DEP VAR: Δ 120 TD VOL	2-Yr Cycles		1 Grant/Yr		Executive FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log B-S Value	0.190***	0.108**	0.228***	0.105**	0.174***	0.105**
	(0.0392)	(0.0424)	(0.0474)	(0.0414)	(0.0344)	(0.0424)
IV Type	IV1	IV2	IV1	IV2	IV1	IV2
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	No	No
Exec FE	No	No	No	No	Yes	Yes
F-Stat (1st Stage)	60.46	114.2	50.77	151.9	78.96	109.9
Observations	34231	16562	28617	15700	37037	23153

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Appendix

A Error in the Estimation of Cycles

Ideally, we would use the firm's pre-planned intended cycle structure in our IV analysis. Inferring the cycle structure from realized option grants necessarily introduces measurement error. In this section, we discuss possible sources of measurement error and why measurement error should reduce the precision of our IV estimates but should not lead to bias.[18] For simplicity, we discuss measurement error in inferring fixed value cycles, although the same logic applies to fixed number cycles.

We measure years as the 12 months following each option grant (so grants occur at the very beginning of each year). We infer that two consecutive years correspond to a fixed value cycle if the value of options granted in consecutive years is the same (within a tolerance band as discussed in Section 2.2). Suppose we observe the following option awards for an executive: Year 1—\$1.0M, Year 2—\$1.5M, Year 3—\$1.5M, Year 4—\$2.0M, Year 5—\$2.0M, Year 6—\$2.5M. From this data, we would infer that Years 2 and 3 are part of a two-year fixed value cycle and Years 4 and 5 are part of a second two-year fixed value cycle. Our inference procedure cannot distinguish between the following cases:

1. The firm planned prior to Years 2 and 4 to award options according to two-year fixed value cycles. The firm did not deviate from the planned schedule.

2. For a variety of possible reasons, the firm chose to award the same value of options in Years 2 and 3 and again in Years 4 and 5. However, the firm did not consciously plan in advance to award options according to fixed value cycles on a regular repeating schedule.

3. The firm planned in advance to award options according a fixed value cycle over a different set of years (e.g., a three-year fixed value cycle covering Years 2, 3 and 4). However, the original plan was endogenously renegotiated such that the first cycle lasted only two years and a new cycle began in year 4.

4. As in Case 1, the firm planned to award fixed value cycles and did not deviate from the

[18] Measurement error weakens the power of our instrument in the first stage of the IV procedure. However, in practice, we do not have a weak instruments problem, as our first stage is highly significant, with F-statistics exceeding 40.

planned schedule. However, the firm did not report how it valued options in each year. We calculated B-S value using a different methodology than the one used by the firm, leading us to infer a different set of cycles from those intended by the firm.

Case 1 is the ideal scenario. It represents pre-planned fixed value cycles that were never renegotiated.

Case 2 represents option grants that are fixed in value across years but were not necessarily planned as such. In Case 2, if the firm awards options that are fixed in value on a regular repeated schedule (e.g., new fixed value cycles in years 2, 4, 6, and so on) then the option schedule is fixed value with two-year cycles in all but name. The difference between Cases 1 and 2 reduces to the difference between *de jure* (in law) and *de facto* (in practice) regimes, and we would wish to use both in our analysis. The problem with Case 2 is that the timing of when cycles terminate and new cycles begin may not follow a regular repeated schedule. Instead, cycle start and termination could be determined "on the fly" by unobserved firm or market conditions that directly affect risk-taking. For example, the firm may choose to hold compensation constant (so value is fixed) during tough times.

Case 3 represents a deviation from the planned cycle schedule. The problem is similar to that in Case 2. The timing of deviations from schedule may be determined by contemporaneous unobserved firm or market conditions that directly affect risk-taking.

Case 4 represents a mismatch between our calculated Black-Scholes value and the firm's internal valuation of the option. This mismatch is more likely to occur when we infer fixed value cycles than when we infer fixed number cycles (the number of options is always clearly reported). In contrast to Cases 2 and 3, the timing of when valuation methodologies deliver a mismatch is unlikely to be related to other unobserved determinants of risk-taking.

In our first instrumental variables analysis, we remove the potential biases caused by Cases 2, 3, and 4 by using an indicator variable for whether the year of each observation is predicted to be the first year of a new cycle. Our predictions use the fact that executives tend to experience repeated cycles of equal length and that the modal length of cycles in the data is two years. For the example above, at the very beginning of Year 5, we have enough information to predict that Year 6 will be a first year because the executive has received two years of options that are the same in value and has previously received a completed fixed value cycle of length 2 (in Years 2 and 3).

At the very beginning of Year 3, we also predict that Year 4 will be the start of a new fixed value cycle because the executive received the same value of options in Years 2 and 3 (we do not observe a previous completed cycle, so our best guess for cycle length is the modal length of cycles in the data: 2 years).

As can be seen from the example above, we use only past information to predict cycle status in the current year. This should purge the estimates of bias that would arise if actual cycle status is correlated with contemporaneous determinants of risk-taking. In fact, we only use past information with a minimum one-year lag to predict current cycle status. This should also purge the estimates of any potential bias that would arise if actual cycle status is correlated with recent past conditions. Consistent with this, we find that lagged returns are not correlated with predicted cycle first years. However, we do use information from the more distant past to form predictions, so we need to assume that firms are not very forward-looking, i.e., managers and boards do not set the length of the current cycle in anticipation of risk-taking conditions two or more years in the future. If this assumption holds, then the predicted first year indicator purges the estimation of bias from irregular, endogenously determined, or renegotiated cycles.

With respect to Case 2 specifically, irregular cycles reduce the power of our instrument. If cycle termination is determined on the fly by unobserved firm or market conditions, then our predicted first year indicator will not correspond to real first years in the data. If irregular cycles are prevalent in our data, then our IV procedure should lead to very noisy but unbiased estimates.

Similarly, deviations from the planned cycle schedule as in Case 3 reduce the power of our instrument but does not introduce a source of bias. The predicted first year indicator corresponds to when new cycles would likely have started if the endogenous renegotiation had not taken place. If cycle renegotiation is prevalent in our data, then the predicted first year indicator should lead to noisy but unbiased IV estimates. Finally, in Case 4, we may be estimating the length of previous cycles with error and may predict future cycles with error. This will again reduce the power of our instrument, but should not lead to systematic bias.

So far, we have discussed how error in cycle detection can affect our first instrumental variable strategy. We now turn to our second instrumental variables strategy, which exploits the differential sensitivity of fixed number and fixed value grants to aggregate returns within cycles. Our second instrumental variable strategy does not use the timing of when cycles begin, so it should not be biased by estimates of when new cycles begin. Specifically, in light of measurement error, our instrument is an interaction term between *inferred cycle type* (fixed number or fixed value) and industry return. We do not assume that inferred cycle type is randomly assigned. Rather, our identifying assumption is that firms that we infer to be on fixed number and fixed value plans do not differ in their response to aggregate returns for reasons other than the differential sensitivity of their option compensation to aggregate returns. To examine whether the data support this assumption, we perform a placebo test that compares how firm risk-taking moves with aggregate returns for firms that are not inferred to be on either type of plan but at some point were inferred to be on fixed number or fixed value plans.

Table A.1
Summary Statistics

This table shows various summary statistics. Panel A shows the industry distribution, broken down by the type of plan the CEO was on. Industries are categorized using the Fama-French 12-industry classification scheme. Panel B compares other firm and executive characteristics across cycle types, showing the 25th, 50th, and 75th percentiles of the distributions. Because there are likely to be time trends in these variables, we show cross sections from fiscal years 1995 and 2005 rather than pool all years.

Panel A: Industry Distribution

Year: 1995	Fixed Number	Fixed Value	Other
	Percent	Percent	Percent
Consumer Non-Durables	6.25	4.95	8.10
Consumer Durables	3.91	5.77	2.52
Manufacturing	19.92	20.05	12.06
Energy	6.64	5.22	3.78
Chemicals	3.12	4.40	3.51
Business Equipment	12.11	11.54	12.60
Telecommunications	1.56	1.65	3.15
Utilities	5.86	6.04	8.01
Shops	13.67	10.16	13.23
Health	7.81	7.69	8.01
Finance	12.50	14.29	12.06
Other	6.64	8.24	12.96
Total	100.00	100.00	100.00

Year: 2005	Fixed Number	Fixed Value	Other
	Percent	Percent	Percent
Consumer Non-Durables	4.84	5.12	5.35
Consumer Durables	3.49	3.24	1.87
Manufacturing	10.75	12.12	9.74
Energy	2.15	4.61	4.34
Chemicals	2.15	3.58	2.24
Business Equipment	19.35	15.53	16.55
Telecommunications	2.15	2.05	3.06
Utilities	1.08	3.07	3.93
Shops	11.02	9.90	9.69
Health	15.86	12.29	9.10
Finance	16.40	18.60	20.76
Other	10.75	9.90	13.35
Total	100.00	100.00	100.00

Table A.1
(continued)

Panel B: Other Characteristics

Year: 1995	Fixed Number			Fixed Value			Other		
	p25	p50	p75	p25	p50	p75	p25	p50	p75
Firm-Level:									
Assets (Millions)	370.55	1193.36	4307.90	434.71	1281.08	4504.74	323.00	853.06	3288.26
Sales (Millions)	385.96	1110.41	3596.84	409.85	1165.17	3276.91	340.86	809.39	2403.10
Market to Book	1.16	1.45	1.99	1.14	1.48	2.00	1.13	1.45	2.13
Volatility (12 Months)	0.18	0.25	0.35	0.18	0.25	0.35	0.19	0.27	0.40
Volatility (120 Trading Days)	0.21	0.28	0.36	0.21	0.27	0.37	0.22	0.31	0.43
CAPX / PPE	0.16	0.23	0.37	0.16	0.24	0.37	0.13	0.22	0.38
Acquisitions (Millions)	0.00	0.00	12.56	0.00	0.00	14.96	0.00	0.00	15.40
Market Leverage	0.08	0.18	0.36	0.09	0.20	0.35	0.05	0.20	0.40
Book Leverage	0.20	0.36	0.51	0.22	0.38	0.51	0.14	0.36	0.54
Total Dividends (Millions)	0.00	9.14	46.56	0.10	12.70	60.62	0.00	5.45	39.73
Firm Return	0.04	0.24	0.45	0.03	0.20	0.42	-0.02	0.23	0.46
Return on Assets	0.11	0.16	0.22	0.11	0.17	0.23	0.09	0.15	0.22
Cash Flow / Assets	0.08	0.12	0.16	0.08	0.12	0.17	0.06	0.11	0.16
Executive-Level:									
Salary (Thousands)	205.00	287.46	439.58	201.20	287.51	411.67	185.31	275.00	415.01
Bonus (Thousands)	61.25	160.68	350.00	60.00	156.06	315.37	30.95	120.00	288.01
Number New Options	10.00	22.60	50.00	10.50	21.00	46.12	15.00	34.00	80.00
Number Prev Options	41.66	96.04	206.74	36.92	81.17	181.51	27.40	77.90	186.39
B-S Value New Options (Thousands)	100.68	235.19	532.54	96.57	224.87	496.62	123.20	310.86	830.39
B-S Value Prev Options (Thousands)	359.17	997.34	2567.03	304.36	867.39	2281.31	173.02	710.96	2198.63
Delta New Options	1.79	3.95	9.00	1.73	3.95	8.47	2.01	5.05	13.80
Delta Prev Options	6.70	18.07	44.26	6.08	15.84	38.99	3.06	12.13	36.76
Vega New Options	1.49	3.48	8.29	1.57	3.53	7.69	1.58	4.23	11.65
Vega Prev Options	3.01	7.82	19.03	2.88	7.59	17.23	1.31	5.25	14.97

Table A.1
(continued)

Year: 2000	Fixed Number			Fixed Value			Other		
	p25	p50	p75	p25	p50	p75	p25	p50	p75
Firm-Level:									
Assets (Millions)	461.58	1306.66	4954.32	563.47	1492.38	6358.76	278.02	805.48	2552.21
Sales (Millions)	349.77	877.12	2482.68	389.60	1103.31	3867.47	180.95	540.79	1778.17
Market to Book	1.09	1.36	2.22	1.09	1.37	2.14	1.04	1.36	2.30
Volatility (12 Months)	0.33	0.46	0.70	0.32	0.43	0.63	0.34	0.51	0.80
Volatility (120 Trading Days)	0.38	0.50	0.72	0.38	0.49	0.67	0.41	0.55	0.81
CAPX / PPE	0.14	0.25	0.44	0.14	0.22	0.42	0.15	0.27	0.56
Acquisitions (Millions)	0.00	0.00	44.56	0.00	0.00	40.27	0.00	0.00	16.18
Market Leverage	0.05	0.23	0.46	0.06	0.22	0.43	0.03	0.20	0.47
Book Leverage	0.16	0.39	0.57	0.17	0.39	0.57	0.06	0.36	0.58
Total Dividends (Millions)	0.00	2.10	33.07	0.00	6.52	50.84	0.00	0.00	13.69
Firm Return	-0.22	0.08	0.39	-0.22	0.09	0.42	-0.32	0.02	0.41
Return on Assets	0.08	0.14	0.23	0.08	0.15	0.22	0.04	0.13	0.22
Cash Flow / Assets	0.07	0.10	0.17	0.07	0.11	0.17	0.03	0.09	0.16
Executive-Level:									
Salary (Thousands)	218.40	310.00	495.12	240.00	345.00	516.91	215.00	301.08	454.00
Bonus (Thousands)	64.00	185.00	406.25	85.00	207.48	450.77	35.70	149.50	362.30
Number New Options	20.00	50.00	100.00	24.00	50.00	116.70	28.00	68.75	176.80
Number Prev Options	76.00	173.06	430.00	76.00	170.25	434.65	33.42	139.08	388.00
B-S Value New Options (Thousands)	201.59	554.49	1493.27	231.84	663.82	1807.65	256.02	766.52	2522.95
B-S Value Prev Options (Thousands)	603.62	2115.94	6836.39	599.14	2017.49	6706.96	171.73	1388.81	5313.79
Delta New Options	2.74	7.45	19.60	3.25	9.00	24.34	3.47	10.14	31.56
Delta Prev Options	9.14	29.97	90.49	9.71	30.13	95.90	2.98	20.15	73.37
Vega New Options	1.82	5.16	14.11	2.49	6.80	18.80	2.08	6.34	20.74
Vega Prev Options	5.03	14.82	41.27	6.00	16.88	48.03	1.33	9.10	31.32

Table A.2

Sensitivity of New Grants to Stock Price: Fixed Value vs. Fixed Number

This is a simple example adapted from Hall (1999) to illustrate how the Black-Scholes value of new at-the-money option grants and the number of options granted varies with stock price fluctuations for executives on fixed number and fixed value plans. For illustrative purposes, we assume the annual volatility is 32 percent, the risk-free rate is 6 percent, the dividend rate is 3 percent and the maturity is 10 years.

		Stock price		
		Year 1 Grant	**Year 2 Grant**	**Year 3 Grant**
Plan		**100**	**120**	**144**
Fixed Value	Value of Options	\$1,000,000	\$1,000,000	\$1,000,000
	Number of Options	28,128	23,440	18,752
Fixed Number	Value of Options	\$1,000,000	\$1,200,000	\$1,440,000
	Number of Options	28,128	28,128	28,128