Swinging for the Fences: Executive Reactions to Quasi-Random Option Grants*

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Abstract

The financial crisis renewed interest in the potential for pay-for-performance compensation to affect managerial risk-taking. We examine how executive stock options affect risk-taking by exploiting two distinct sources of variation in option compensation that arise from institutional features of multi-year grant cycles. We find that, given average grant levels during our sample period, a 10 percent increase in new options granted leads to a 2–6 percent increase in equity volatility. This increase in risk is driven largely by increased leverage. We also find that increased options lead to lower dividend growth, with mixed effects on investment and 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. 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. While we do not attempt to determine the causes of the financial crisis in this paper, our goal is to determine whether the intuition espoused by these commentators is generally correct. 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 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 number of empirical studies have explored the relationship between executive stock options and various measures of risk-taking behavior. However, the evidence remains mixed. Most of the early work in this area finds a positive relationship between options and risk-taking. For example, Agrawal and Mandelker (1987) find that managers with higher stock and option ownership make more variance-increasing acquisitions. DeFusco et al. (1990) find that firms that approve stock option plans exhibit an increase in volatility. Subsequent research has focused on the relation between a manager's "vega" (the sensitivity of the Black-Scholes value of all unexercised options to volatility) and risk-taking. Guay (1999) shows that the vega of CEO compensation is positively related in the cross section to growth opportunities, which may proxy for risk-taking demand. Coles et al. (2006) and Chava and Purnanandam (2010) find that vega is also positively associated with leverage as well as R&D.

On the other hand, a number of recent papers have called into question the positive relationship between options and risk-taking. Lewellen (2006) finds that higher option ownership tends to decrease managers' preference for debt financing. Along similar lines, Liu and Mauer (2011) find that higher option ownership leads to greater cash-holdings. Bettis et al. (2005) also find that executives exercise their options earlier when volatility increases, suggesting that subjective option values actually decrease with volatility. Finally, Hayes et al. (2012) find no change in risk-taking following the large decline in option pay that resulted from a change in the accounting standards, suggesting no effect.

Establishing a causal effect of options on risk-taking has been difficult due to endogeneity concerns. The main measurement challenge is that a third omitted factor could drive both options

[1] This magnification effect has also been noted by Lambert et al. (1991), Carpenter (2000), Hall and Murphy (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. See Frydman and Jenter (2010) for a survey of the potential incentive effects of options on behavior.

and risk-taking. For example, firms that are fundamentally more risky may choose to award more equity-linked compensation because these firms face difficulty in monitoring managerial effort (Prendergast, 2002), or must satisfy the participation constraint for risk-averse executives (Cheng et al., 2014). 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. In the other direction, one could imagine that firms pay more options when they are doing well and thus accumulating cash or reducing leverage, leading to a negative relationship between risk-taking and option pay. More generally, 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 when option use declined following a change in the accounting treatment of options. However, 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. Moreover, a potential issue is that this regulatory change affected all firms simultaneously, so it is difficult to estimate a counterfactual time trend based on a control group. For example, if risk-taking would have increased over this time period absent the decline in option pay, the lack of a change would actually indicate a positive effect. Bettis et al. (2012) also show that the regulatory change coincided with an increase in performance-vesting shares, a convex form of compensation which may have offset the decline in option grants. They conclude that, "while option usage has declined since 2006, our analysis indicates that compensation convexity has not, which explains the lack of decline in firm risk-taking after 2006 that is purported by Hayes et al. (2012) to be a puzzle."

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 litigation 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 an important 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.[3] 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 Appendix A 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. 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

[3] 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.

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. Empirically, we show that these years do not have unusual turnover risk. In addition, conversations with compensation consultants suggest that option grant cycles are unrelated to performance reviews. As a further check to ensure that other unobservable differences in predicted cycle first years do not drive 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, movements in each firm's stock price are partially driven by market and industry shocks. These industry shocks are beyond an executive's control and are also difficult to predict even by sophisticated agents. 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. Given average grant levels during our sample period, a 10 percent increase in 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.[4] However, it is difficult to discern from accounting data whether investment actually represents investment in riskier projects. 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 lead to weakly lower accounting measures of performance. However, the latter results 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 can vest over several years, implying that new grants can affect behavior beyond our measured one-year horizon. In particular, we may not

[4]Moreover, an executive who holds equity-linked compensation may overinvest to sustain the pretense that the firm possesses good investment opportunities (Bebchuk and Stole, 1993; Benmelech et al., 2010).

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

capture incentives to manipulate firm outcomes shortly before long-vesting options are exercised (e.g., Oyer, 1998). In supplementary tests, we also 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. Finally, we find suggestive evidence 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.

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.

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 grant date 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 consultants 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 Appendix A.

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. 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, adjusted for stock splits. Our results are not sensitive to these assumptions. In 80 percent of cases, executives receive a single option grant and 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 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.[7] Value is computed as the Black-Scholes value, face value, or company self-reported value.[8] 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.

[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] 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.

[8] 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, conditional on granting options. 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 of options on risk-taking should differ by whether firms use 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.[9] 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.

Finally, 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, 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

[9] 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 Appendix A why, in our instrumental variables framework, errors in detection should reduce the precision of our estimates but should not bias our results.

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.

3 Empirical Strategy

We introduce two instruments that provide exogenous variation in the amount of new at-the-money options granted. Before going into detail, we note that the Black-Scholes value, delta (the change in the B-S value of a grant associated with a 1 percent change in the underlying), and vega (the change in the B-S value of a grant associated with a 0.01 unit change in the volatility of the underlying) of new at-the-money option grants are all highly correlated and affected by our instruments. An exogenous increase in new option grants implies that all three values increase together. 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. For brevity, we instrument for Black-Scholes value in our two-stage least squares estimates because this is a simple 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.[10]

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. Instead, we use the staggered timing of predicted increases in option grants within the fixed value sample to estimate the effect of options

[10] Note that our instruments are valid despite the fact that they affect B-S value, delta, and vega simultaneously. The reason is that all of the variables affected by our instruments are intrinsically related in the sense that they are all calculated from formulas involving the same underlying parameters. If any one of the B-S value, delta, or vega of an at-the-money option grant is known (along with the stock price, risk-free rate, and dividend yield), the other two can be calculated from it. The exclusion restriction does not require that the instrument not affect linear/non-linear transformations of the endogenous variable being instrumented.

on risk-taking.[11] 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 are true more generally. First, option compensation tends to follow an increasing step function for executives on fixed value plans. This is 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 compensation following the end of a fixed value cycle relative to other years. Columns 3–6 show that the first year indicator is also associated with a 9 percent larger increase in the delta of option compensation and a 7 percent larger increase in the vega of option compensation. This pattern holds for all top executives as well as for the subsample of CEOs and CFOs.

However, we do not use the simple first year indicator as our instrument because of the possibility

[11] Note, estimating a causal effect within an endogenously selected sample is very common in papers exploiting natural experiments for identification. For example, this is the case in any regression discontinuity (RD) design. Consider the classic RD measuring the effect of education on earnings. Students take entrance exams, and all students with scores above a certain cutoff are admitted to college. The RD compares the earnings of students just to the left and right of this cutoff. This RD delivers a true causal effect, but among the set of students who receive scores *close* to the cutoff. The relevant concern here is not about identification of causation (internal validity), but whether the causal effect of education on earnings would be similar for students who received very low or high test scores (external validity).

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. At the start of 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 $n_t \geq k$. 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 (Pan, Wang, and Weisbach, 2013).

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 three 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 only use past information to predict current cycle status. This is designed to purge the estimates of potential bias that would arise if actual cycle status is correlated with current conditions. Consistent with this, we find that one-year lagged returns are not correlated with our predicted cycle first year instrument. More generally, as long as managers and boards do not set the length of the current cycle in anticipation of risk-taking conditions at the start of future cycles, then the predicted first year indicator should correspond to exogenously timed increases in option pay.[12] Also, our second IV strategy will not require this assumption. This is the sense in which the two identification strategies help to cross-validate one another.

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:

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

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

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 $\triangle Y_{ijt}$ 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.

The main coefficient of interest, δ_1, represents the effect of an increase in options on outcomes $\triangle Y_{ijt}$. Standard errors are clustered by firm to account for the fact that we observe multiple executives from the same firm.[13]

[12] For a more in-depth discussion of this, see Appendix A.

[13] Note that we do not need to further adjust our standard errors to account for the fact that our instrument is a "predicted" variable. In contrast to generated regressors in OLS, generated instruments in IV do not require standard errors to be adjusted (Wooldridge, 2002). Also, our predicted first year instrument is not what would typically be considered a generated instrument/regressor. It does not come out of a pre-first-stage regression model estimated with error. In other words, we are not estimating a 3-Stage Least Squares (3SLS) specification in which the predicted first year dummy is used to instrument for the true first year dummy, which is then used to instrument for changes in option compensation. Instead, we use a standard 2SLS IV specification. The true first year dummy does not

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 affect risk-taking. Instead, we use the fact that the indicator for predicted first year corresponds to increases in option pay *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.[14]

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 receiving the grant, he may then restore volatility to its previous level and hold options worth more than $1 million. Short-run manipulation of

factor directly into this 2SLS IV estimation. Instead, the first stage directly instruments for the change in option compensation using the predicted first year dummy. That is, our instrument literally is whether an observation is a *predicted* first year.

[14]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, unlike prices, volatility is not forward looking.

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. Further, we show that we find similar results if we analyze the change in volatility excluding the 120 trading days around each option grant (i.e., limit attention to the middle 120 trading days of the year following a grant relative to the same period in the previous year). Thus, our results do not seem to be driven by short-run manipulation immediately before/after a 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 grant cycles are 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. Instead, we complement our analysis with a second instrumental variables strategy that does not use the timing of cycle first years.[15]

3.2 Instrumental Variables Strategy 2

Our second instrumental variables strategy uses only observations corresponding to fixed value and fixed number plans. Specifically, we exploit differences in the way that option compensation moves *within* a cycle for executives on these two types of 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,

[15]This paper uses 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 cannot 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).

the total value of new options awarded to its executives increases as well.

It may not initially seem intuitive that the value of an at-the-money option increases in proportion with its strike price. However, this follows directly from the Black-Scholes formula defined in Footnote 8. In particular, for an at-the-money option with a strike price X that is equal to the stock price S, the Black-Scholes formula reduces to:

$$S * [e^{-dT} N(Z) - e^{-rT} N(Z - \sigma T^{(1/2)})],$$

where $Z = [T(r - d + \frac{\sigma}{2})]/\sigma T^{(1/2)}$. From the formula, it is clear that doubling S will double the value of the option. To gain further intuition, consider what occurs in a reverse stock split, where two shares become one share and the stock price doubles. In this case, two options also become one. Thus, two at-the-money options on the old stock must be worth the same as one at-the-money option on the new stock. In other words, when the stock price doubles in the split, the value of an at-the-money option must double as well. While we presented this intuition in terms of Black-Scholes value, this value is intrinsically linked to the delta and vega of at-the-money options. An increase in the underlying stock price will also entail an increase in the grant date delta and vega for at-the-money options under fixed number plans.

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, movements in each firm's stock price are partially driven by market and industry shocks. These industry shocks are beyond an executive's control and are also difficult to predict even by sophisticated agents.[16] Thus, our second instrument for changes in option compensation is

[16] A large body of work in the field of asset pricing shows that returns are very difficult to forecast. More specific to our empirical strategy which uses one-year industry-level returns, Kelly and Pruitt (2013) show that a rich forecasting

the interaction between plan type and aggregate returns.

Specifically, for executives on fixed number or fixed value cycles, we estimate first- and second-stage equations of the form:

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

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

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, $I_{ijt}^{FN} R_{kt}$, is the excluded instrument. The coefficient, δ_3, is the effect of an increase in new option grants on our outcome of interest, $\triangle Y_{ijt}$, measured as annual changes for stock variables and levels for flow variables.

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,$\triangle Y_{ijt}$, 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,

model can predict at most 5% of the variation in yearly industry returns.

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 induced by industry returns *within* cycles, so this second methodology is robust to the concern that the predicted start of cycles may be correlated with other unobserved cycles of activity within the firm.

3.3 Long Vesting Options and Changes in Behavior

Our analysis focuses on annual changes in behavior induced by annual shocks to the amount of new options granted. However, many option grants do not fully vest for several years. It is natural to ask why managers would increase volatility in the first year following a shock to option grants since the options may not fully vest for several years.

First, a considerable portion of options do vest within one year and most longer-vesting options partially vest in each year, including in the first year after the option grant. Cadman et al. (2012) shows that, despite the fact that the average option takes three years to fully vest, roughly 42 percent of a typical option grant does vest within a one-year window. For these options, it is obvious why executives may seek to increase volatility in the year following a shock to option grants.

Second, our empirical strategy will also capture risk incentives induced by longer vesting options. After an option is granted, increasing volatility above its natural level over any time interval prior to option exercise will increase its value by leading to more extreme prices in expectation when the option becomes exercisable (see Appendix B for an illustrative simulation). Therefore, executives have incentives to increase volatility above its natural level in each year between option grant and exercise. For this reason, our main outcome of interest is the *change* in volatility relative to the level in the previous year. In other words, we don't necessarily expect that the level of volatility will be higher in the first year after a shock to option grants than in subsequent years. However, we should see a larger change in volatility (relative to observed volatility in the previous year) at the start of a new cycle than in other years. This is because there is a larger change in option grants at the start of a new cycle than in other years (where there is no change). An analogous argument can be made regarding our second instrument. Appendix B discusses these issues in more detail.

For the reasons above, our methodology should capture risk incentives of options, even if some have long vesting schedules. However, we will not not capture incentives to manipulate firm outcomes, including short run price manipulation, immediately before long-vesting options are exercised. A related consequence of long-vesting options is that most executives also hold previously granted unexercised options, while our instruments only shock 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.

4 Results

4.1 Instrumental Variables Strategy 1

We begin by using the predicted first year indicator as an instrument for changes in option grants. As described in Section 3 and Appendix A, the sample is restricted to executives on fixed value cycles, so the results do not rely on any comparisons between fixed number and fixed value observations. 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 Black-Scholes value, delta, and vega of new options granted. Using the full sample of executives, predicted first years corresponds to a 7.4 percent increase in the Black-Scholes value of new options, an 8.1 percent increase in the delta of new options, and a 6.3 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. This is because they are intrinsically linked through option valuation formulas. If any one of the B-S value, delta, or vega of an at-the-money option grant is known (along with the stock price, risk-

free rate, and dividend yield), the other two can be calculated from it. For brevity, we focus on Black-Scholes value in the remainder of the analysis as our measure of the magnitude of an option grant, but remind the reader that we measure the effect of an overall increase in the amount of new at-the-money options, when these three measures increase simultaneously.

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. As discussed previously, we use annual *changes* in volatility as our outcome. 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 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.

If our instruments are valid, they should be orthogonal to time-varying firm and executive characteristics. Consistent with this, we confirm in Appendix Table A.3 that results remain similar when we include controls for executive tenure, log of cash compensation (salary + bonus), log sales, log assets, sales growth, market to book, tangibility ratio, a dummy variable for whether the firm has rated debt, and the log delta and vega of previously granted (unexercised) options and stock holdings. We include only time-varying controls because our firm fixed effects should control for all time-invariant differences across firms.

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 0.0044 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 0.007 unit increase in book leverage and a 2.6 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.[17] Thus, the 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 2 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

[17] 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.

separately and find positive, albeit noisily estimated, effects.

Columns 6–7 of Table 4 explore the effect of options on dividend growth. Column 6 shows that, among firms that already pay dividends, a 10 percent increase in options leads to 1.7 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 growth. 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 grants. 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. Industry-adjusted equity returns in the 12 months following the increase in option grants are insignificantly different from zero, 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 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.

As with our first instrument, we begin 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 sub-sample 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 0.0083 increase in equity volatility, or a 2.4 percent increase relative to median volatility. Again, results remain similar when we include a battery of controls for time-varying firm and executive characteristics (see Appendix Table A.4).

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 0.0062 unit increase in book leverage, and a 1.8 percent increase in the probability of positive net issuance. We estimate that 39 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 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, conditional on paying dividends, 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 ROA, with insignificant and noisily estimated negative effects on returns and cash flows.[18] Overall, we again find that increased option compensation leads to increased volatility, driven in large part by increases in leverage.

As described earlier, our second instrumental variables strategy requires 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. Note that our first IV strategy already offers a validity check showing that our results are not dependent on this assumption. The first instrument only uses data for fixed value executives, and does not require assumptions about how executives would react to industry returns in the absence of differences in compensation. Using the first instrument, we estimate similar results across a range of firm outcomes.

To further test whether the data support this assumption, we compare the responses of fixed number and fixed value executives to industry returns during years in which the executive is awarded options on a non-FN/FV basis. Among other things, this test exploits the fact that multi-year plans grew in popularity in the early 1990s and declined in popularity in the mid 2000s due to peer benchmarking requirements that led firms to justify their level of executive pay annually. We

[18] Using both instruments, we find that options lead to a smaller decline in cash flow to assets than in ROA. This is unsurprising given our earlier results showing that options lead executives to increase 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.

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. 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 × Ind Return in Columns 4–6.[19] 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 between coefficients are in the range of 0.18–0.35).

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

[19] 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, for example, estimating Columns 1–3 in Panel A within a single OLS regression, where standard errors are clustered by firm.

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 general 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 increase both options and investment at higher rates. Finally, firms that 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 finding 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 (Becker, 2006). Columns 1 and 2 of Table 11A presents suggestive evidence that the wealth channel does not drive our results. In particular, 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. Of course, current total compensation may only be a small fraction of a manager's wealth. However, all of our regressions are in terms of changes and a manager's total compensation is a reasonable proxy for the *change* in his wealth relative to the previous year. Overall, the results suggest that the composition of pay matters. Risk-taking increases when options as a fraction of total pay increase.

Next, we explore the possibility that other types of compensation, such as salary, bonus, and restricted stock, move with our shocks to option grants. In Columns 3 and 4 of Table 11A, 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. In un-

reported tests, we also find that salary and bonus do not increase with our instruments.[20] Likewise, we find that our instruments do not predict changes in restricted stock. Moreover, in Columns 5 and 6 when we limit the sample to years in which no restricted stock is awarded, we 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 option 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

[20] Even if cash pay adjusted completely, such that total pay never jumped, 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.

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 convex compensation 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. 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.

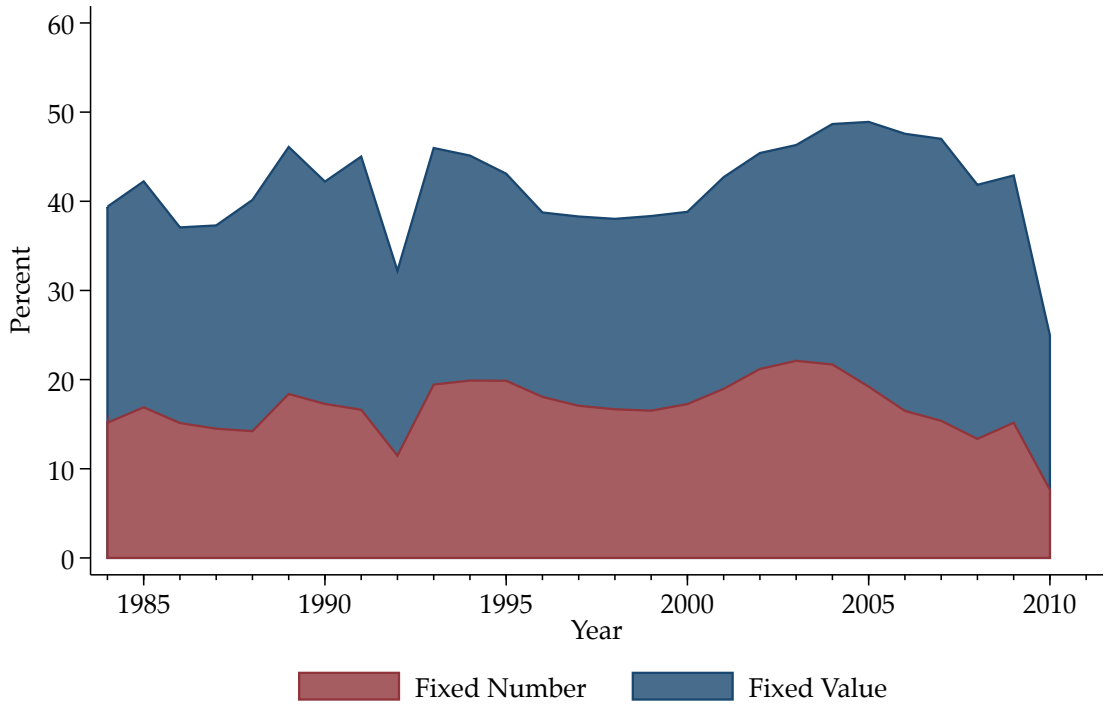
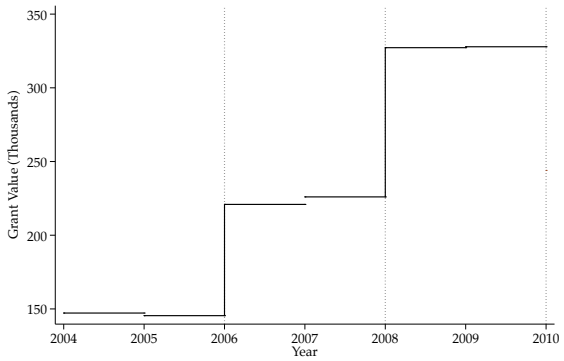


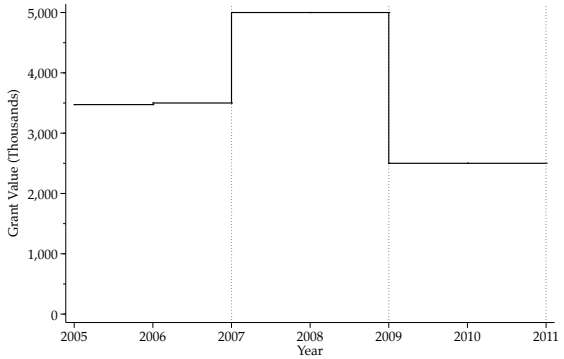
Figure 2
Real Examples of Fixed Value Cycles and Predictions

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

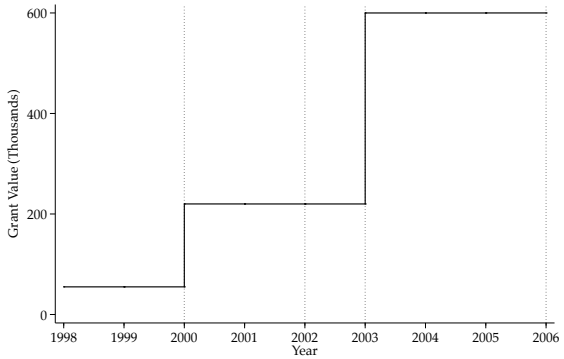


Table 1
Summary Statistics

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	20514	66.88	40148	92.61
3	6180	20.15	2769	6.39
4	2288	7.46	336	0.78
5	900	2.93	45	0.10
≥ 6	792	2.58	56	0.13
Total	30674	100.00	43354	100.00

Panel B: Industry Distribution

Year: 2000	Fixed Number	Fixed Value	Other
	Percent	Percent	Percent
Consumer Non-Durables	7.10	6.09	5.71
Consumer Durables	2.56	3.65	2.06
Manufacturing	11.93	13.59	10.67
Energy	3.98	3.85	3.80
Chemicals	2.56	3.45	2.38
Business Equipment	17.33	14.81	18.75
Telecommunications	3.41	2.23	2.96
Utilities	5.11	6.09	3.75
Shops	9.38	10.55	10.83
Health	8.52	6.09	9.30
Finance	16.19	19.68	17.86
Other	11.93	9.94	11.94
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	563.46	1492.38	6376.03	278.02	805.48	2551.24
Sales (Millions)	349.77	877.12	2482.68	385.07	1103.31	3874.17	181.01	540.79	1773.14
Market to Book	1.09	1.36	2.22	1.09	1.37	2.14	1.05	1.36	2.30
Volatility (12 Months)	0.33	0.46	0.70	0.31	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	51.99	0.00	0.00	13.67
Firm Return	-0.22	0.08	0.39	-0.22	0.09	0.41	-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)	218.22	310.00	499.98	240.00	345.00	518.75	215.00	302.53	455.17
Bonus (Thousands)	63.71	185.00	406.25	85.00	207.24	450.66	35.54	149.18	360.00
Number New Options	20.00	50.00	100.00	23.96	50.00	115.50	28.18	69.45	177.62
Number Prev Options	73.20	170.00	430.00	75.22	169.10	428.82	30.00	135.50	382.08
Value New Options (Thousands)	201.29	558.94	1490.00	230.53	660.79	1786.15	258.54	774.29	2559.18
Value Prev Options (Thousands)	599.66	2109.15	6814.61	596.85	1994.65	6591.09	142.29	1336.98	5229.50
Delta New Options	2.73	7.47	19.43	3.23	8.87	24.18	3.51	10.24	31.88
Delta Prev Options	9.07	29.85	90.34	9.66	30.01	92.18	2.29	19.37	72.54
Vega New Options	1.82	5.07	14.11	2.48	6.76	18.63	2.12	6.41	20.81
Vega Prev Options	5.06	14.82	41.93	5.99	16.83	47.71	1.12	8.81	30.87
Value Prev Options + Stock	635.25	2306.44	7212.42	678.04	2252.48	7212.42	175.62	1467.43	5698.75
Delta Prev Options + Stock	9.55	31.98	95.35	10.38	31.83	100.76	2.76	20.74	75.50

Table 2
IV1: First Stage

Panel A 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	0.0794***	0.0816***	0.0872***	0.0921***	0.0670***	0.0717***
	(0.00933)	(0.0121)	(0.00909)	(0.0117)	(0.0108)	(0.0139)
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	0.176	0.222	0.167	0.218	0.215	0.266
Observations	37440	16396	37440	16396	37440	16396

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	0.0743***	0.0772***	0.0810***	0.0857***	0.0632***	0.0684***
	(0.00897)	(0.0117)	(0.00879)	(0.0114)	(0.0105)	(0.0135)
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	0.176	0.222	0.166	0.217	0.215	0.266
Observations	37440	16396	37440	16396	37440	16396

Table 3
IV1: Volatility

Panel A shows IV estimation results, where the variable ΔLog Vega 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	0.143***	0.158***	0.196***	0.213***
	(0.0282)	(0.0368)	(0.0376)	(0.0496)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
F-Stat (1st Stage)	67.91	42.02	67.49	41.48
Observations	36859	16142	36967	16181

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First	0.0106***	0.0121***	0.0145***	0.0161***
	(0.00177)	(0.00229)	(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	0.517	0.547	0.451	0.482
Observations	36859	16142	36967	16181

Table 4
IV1: Other Outcomes

Panel A shows IV estimation results, where the variable ΔLog Vega 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 industry-adjusted 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	0.0440**	0.0694***	0.259**	0.199**	0.310**	-0.174**	-0.00878	0.0182	-0.0571***	-0.0520***
	(0.0205)	(0.0266)	(0.103)	(0.0925)	(0.128)	(0.0688)	(0.0302)	(0.168)	(0.0179)	(0.0189)
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)	58.49	57.95	58.22	58.17	60.63	57.22	68.46	66.66	48.55	42.02
Observations	30366	30463	30575	29970	30262	20503	37426	37111	27475	27864

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	0.00331**	0.00519***	0.0194***	0.0150**	0.0238**	-0.0152***	-0.000652	0.00134	-0.00406***	-0.00349***
	(0.00147)	(0.00186)	(0.00740)	(0.00690)	(0.00948)	(0.00552)	(0.00224)	(0.0123)	(0.00107)	(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	0.259	0.185	0.280	0.931	0.868	0.302	0.172	0.201	0.776	0.722
Observations	30366	30463	30575	29970	30262	20503	37426	37111	27475	27864

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 who 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 × Ind Return	0.480***	0.523***	0.520***	0.596***	0.457***	0.536***
	(0.0340)	(0.0475)	(0.0329)	(0.0439)	(0.0538)	(0.0731)
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	0.343	0.400	0.331	0.406	0.377	0.446
Observations	23441	10199	23441	10199	23441	10199

Table 6
IV2: Volatility

Panel A shows IV estimation results, where ΔLog Vega is instrumented using FN × Ind Return as defined in Table 5. Observations are at the executive-year level. The sample is limited to executives who are on either fixed number or fixed value plans (excluding the first years of cycles). All other variables are as defined in Table 3. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the FN × Ind Return instrument. 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	0.0570**	0.0699**	0.0829**	0.108***
	(0.0263)	(0.0315)	(0.0326)	(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)	195.0	116.4	196.2	118.0
Observations	23109	10048	23166	10067

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
FN × Ind Return	0.0272**	0.0361**	0.0396**	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	0.581	0.625	0.524	0.576
Observations	23109	10048	23166	10067

Table 7
IV2: Other Outcomes

Panel A shows IV estimation results, where ΔLog Vega is instrumented using FN × Ind Return as defined in Table 5. Observations are at the executive-year level. The sample is limited to executives who are on either fixed number or fixed value plans (excluding the first years of cycles). All other variables are as defined in Table 4. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the FN × Ind Return instrument. 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	0.0491***	0.0620***	0.184**	0.185*	0.00221	-0.220**	-0.0415	-0.162	-0.0317*	-0.0142
	(0.0157)	(0.0195)	(0.0847)	(0.107)	(0.117)	(0.108)	(0.0280)	(0.108)	(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)	190.6	190.7	186.6	187.7	186.5	60.47	201.5	198.4	177.0	172.9
Observations	19160	19210	19285	18921	19106	12357	23435	23276	17372	17574

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 × Ind Return	0.0250***	0.0316***	0.0929**	0.0938*	0.00111	-0.0768**	-0.0200	-0.0777	-0.0163**	-0.00722
	(0.00772)	(0.00968)	(0.0421)	(0.0537)	(0.0592)	(0.0352)	(0.0133)	(0.0527)	(0.00799)	(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	0.284	0.354	0.932	0.874	0.399	0.265	0.350	0.809	0.765
Observations	19160	19210	19285	18921	19106	12357	23435	23276	17372	17574

Table 8
IV2: Placebo Test

This table shows reduced form regression results where the outcomes in Tables 5–7 are regressed directly on the FN Placebo × Ind Return placebo instrument. The placebo sample is restricted to executives receiving option pay 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)	(2)	(3)	(4)	(5)	(6)
	Δ Log B-S Value	Δ 12 Month Vol	Δ 120 TD Vol	Δ Mkt Lev	Δ Bk Lev	Debt ↑
FN Placebo × Ind Return	0.00673	0.0179	0.00972	-0.00500	-0.0100	-0.0217
	(0.106)	(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	0.301	0.247	0.310
Observations	13958	18271	18405	15378	15664	18321

Panel B: Reduced Form - Investment, Dividends, Performance

	Investment		Dividends		Performance		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log Capx	Log Tot Inv	Δ Log Div	Δ Div Pay	Return	ROA	CF/Assets
FN Placebo × Ind Return	0.000965	-0.0348	0.0377	-0.00632	0.0176	-0.00994	-0.00551
	(0.0614)	(0.0624)	(0.0527)	(0.0159)	(0.0653)	(0.00864)	(0.00977)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.918	0.862	0.399	0.256	0.213	0.749	0.678
Observations	17603	17916	9241	19153	21546	16116	16275

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 × 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 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 × Ind Return		
	(1) Low	(2) Med	(3) High	(4) Low	(5) Med	(6) High
Instrument	0.0102***	0.0112***	0.0210***	-0.0125	0.0346	0.0595**
	(0.00331)	(0.00390)	(0.00450)	(0.0246)	(0.0272)	(0.0284)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
P-Value (Diff Low)	-	0.843	0.0525	-	0.187	0.0542
R^2	0.339	0.355	0.380	0.351	0.336	0.347
Observations	12062	12062	12062	7557	7556	7556

Panel B: Finance and High Tech

DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year		Instrument = FN × Ind Return	
	(1) No Fin/Tech	(2) Fin/Tech	(3) No Fin/Tech	(4) Fin/Tech
Instrument	0.0120***	0.0187***	0.0189	0.0477**
	(0.00276)	(0.00414)	(0.0213)	(0.0221)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
P-Value (Diff)	-	0.181	-	0.346
R^2	0.351	0.381	0.327	0.392
Observations	24010	12957	15027	8139

Panel C: CEO/CFO

DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year		Instrument = FN × Ind Return	
	(1) No CEO/CFO	(2) CEO/CFO	(3) No CEO/CFO	(4) CEO/CFO
Instrument	0.0124***	0.0161***	0.0317*	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)	-	0.296	-	0.291
R^2	0.345	0.372	0.334	0.360
Observations	20786	16181	13099	10067

Table 10
OLS Endogenous Correlations

This table shows the OLS results of regressing various firm outcomes on Δ Log Vega. 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	0.00825***	0.0128***	0.0251***	0.0398***	0.0387***	0.00608**
	(0.00185)	(0.00225)	(0.00793)	(0.00963)	(0.00725)	(0.00260)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.515	0.450	0.928	0.866	0.312	0.171
Observations	41025	41143	33492	33827	18556	34171

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	-0.00944***	-0.00792***	-0.00223	-0.0416**	0.0156***	0.0137***
	(0.00145)	(0.00178)	(0.00631)	(0.0170)	(0.00119)	(0.00127)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.262	0.182	0.279	0.197	0.779	0.723
Observations	33947	34051	34178	41295	30725	31117

Table 11
Robustness

This table re-estimates the baseline IV specifications from Column 3 of Tables 3A and 6A. In both panels, Δ Log Vega are instrumented using the Predicted First Year indicator in odd-numbered columns and FN × 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×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	0.234***	0.0984**	0.166***	0.0827**	0.143***	0.108***
	(0.0449)	(0.0394)	(0.0311)	(0.0327)	(0.0358)	(0.0347)
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)	64.83	180.8	89.01	199.2	61.02	165.5
Observations	36963	23163	36963	23163	23477	15676

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	0.187***	0.107**	0.225***	0.104**	0.172***	0.103**
	(0.0386)	(0.0422)	(0.0468)	(0.0411)	(0.0341)	(0.0422)
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)	61.61	114.9	51.63	153.2	79.83	110.3
Observations	34157	16573	28864	15709	36967	23166

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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.[21] 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 to 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 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.

[21] 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.

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: two 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 do 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.

B Vesting

In general, executives have increased incentives to manipulate volatility *after* receiving a shock to option grants rather than *before*. This is true even if the shock is anticipated as is the case with our first instrument. Suppose the manager anticipates receiving a $1M B-S value grant a year from now. He would have little incentive to increase risk prior to receiving this anticipated fixed value grant. 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 $1M B-S value at the grant date (assuming the B-S value is calculated using volatility measured prior to the grant date).

Given that executives have incentives to increase volatility *after* options are granted, it is natural to ask why managers would increase volatility in the first year of the shock to option grants since the options may not fully vest for several years. There are at least two reasons why executives would increase risk-taking in the year following an option grant, even if the "nominal" vesting period (i.e., time to full vesting) is longer than one year.[22]

First, even with a nominal vesting period longer than one year, a significant portion of an option grant typically does become exercisable in the first year. Consider an option grant of 300 shares with a nominal vesting period of three years. There are two types of vesting schedules that typically apply within this three-year period: cliff vesting and graded vesting. Under cliff vesting, none of the option grant is exercisable until after three years have elapsed. Under graded vesting, portions of the grant become exercisable throughout the vesting period. For example, vesting could be linear (e.g. 100 shares become exercisable each year), frontloaded (e.g., 200 shares become exercisable in Year 1, 50 in Years 2 and 3) or backloaded (e.g., 50 shares become exercisable in Years 1 and 2, 200 in Year 3). Anecdotally, graded vesting with a linear schedule is most commonly used in practice (Hall and Murphy, 2002).

Cadman et al. (2012) collect detailed data on vesting schedules from 1997-2008 using Form 4 filings. The SEC requires firms to file a Form 4 when there is a change in ownership by an insider. Options granted to the CEO fall within this requirement. In their sample, Cadman et al. find a mean nominal vesting period of 2.97 years. However, they also find that, on average, 42 percent of an option grant is exercisable within one year. The reason is that some firms do grant options with a one-year nominal vesting period and when the nominal vesting period is longer, cliff vesting schedules are rare. Given that a significant portion of a typical option grant does become exercisable within the first year, it seems reasonable that executives may increase risk in that time frame.

Second, even if firms did (counterfactually) tend to use cliff vesting with nominal vesting horizons longer than a year, it is not clear that it would be optimal for executives to delay increases in risk-taking until close to the vesting date.

To illustrate this point, consider the following example. Suppose an executive is granted an at-the-money call option with a strike price of $50 and a three-year time to maturity. For simplicity, assume it is a European option that can only be exercised at the end of the third year, not before or after. Suppose the "natural" level of volatility for the executive's firm is 20 percent but the executive can increase volatility if he so desires. Also assume the risk-free rate is 5 percent.

We examine how the value of the executive's option at the start of Year 1 (which proxies for his expected payoff from the option at the end of Year 3) varies when the executive plans to increase volatility over different intervals and under different circumstances before it is exercisable. We do not use the Black-Scholes formula because it assumes constant volatility. We instead simulate a binomial option pricing model, allowing 500 steps per year.

[22] Strictly speaking, options do not vest; they become exercisable. For brevity, we use the word "vest" to refer to options that change status from being unexercisable to exercisable.

First, assume that the executive does not plan to alter the firm's volatility, so that volatility is constant at 20 percent throughout the option's life. In this case, the value of the option, as given by the binomial model, is $10.46. This is the same value as would be given by Black-Scholes.

Next, suppose that the executive plans to increase volatility to 40 percent in the first year but then revert volatility back to 20 percent in the final two years. In this case, the value of the option is increased to $12.89. The intuition is that increasing volatility over any time interval, even temporarily, leads to more extreme prices in expectation when the option becomes exercisable, thus increasing the option's value.

Next, suppose the executive instead plans to increase volatility to 40 percent in Year 3 but leave it at 20 percent in the first two years. In this case, the value of the option is again $12.89. Thus, it is irrelevant whether volatility is increased immediately following the option grant or in the year before the option becomes exercisable. The value of the option is the same in either case.

Finally, suppose that the executive plans to increase volatility in Year 3 *only if* the option is close to the money at that time. In particular, assume that the executive will increase volatility to 40 percent only if the stock price at the beginning of Year 3 is within 10 percent of the strike price of $50. In this case, the value of the option is $11.36. Thus, only increasing volatility if the option is close to the money makes the option less valuable than increasing volatility unconditionally.

In fact, if the executive plans to only increase volatility in Year 3 when within 10 percent of the money, he would have to increase volatility to 74.5 percent to make the value of the option equal to the $12.89 it would be worth if he increased it to 40 percent unconditionally in Year 1 or Year 3.

These patterns are illustrated graphically in Figure A.1. Panels A and B show that the relationship between the value of the option and the volatility of the underlying is the same regardless of whether volatility is increased in Year 1 only or Year 3 only. Panel C shows that if volatility is only increased in Year 3 when the option is close to the money at the beginning of the year, the value of the option is less sensitive to the increase in volatility. The intuition is that the distribution of terminal prices is less dispersed if volatility is only increased when the option is close to the money at the start of Year 3. Thus, if the manager wants to increase volatility in Year 3 only when the option is close to the money, the manager would have to increase volatility by a much larger amount to achieve the same gains as increasing volatility unconditionally.

A similar intuition continues to hold if the example is embellished to make it more realistic. For example, we instead considered an American option with a 10-year maturity and three-year cliff vesting. We confirmed that the above intuition holds using the Hull and White (2004) model, which is a popular binomial-style model for valuing employee stock options of this kind that also accounts for employee termination and early exercise.

Thus, while it is true that there may be costs associated with increasing volatility after a grant, these costs cannot necessarily be mitigated by deferring the increase. If one defers, the only way to get the same benefit is either 1) to unconditionally increase volatility by the same amount or 2) to increase volatility by a much larger amount conditional on being close to the money before vesting. It is not clear that (2) is feasible or less costly. It may not be feasible because there may be technological limitations on how much an executive can increase volatility. It may not be less costly because it involves a larger increase in volatility. Moreover, the large increase would occur when the stock has underperformed (i.e., its price is still close to the at-the-money strike price from when the option was first granted). Executives are likely to have less job security at such times and be more constrained in their ability to manipulate volatility.

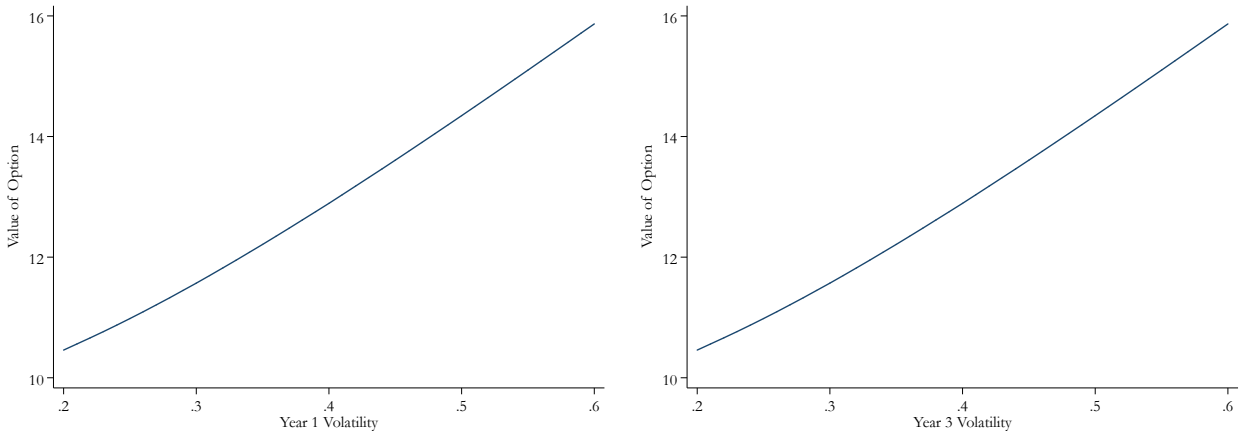
Overall, our simulations show that executives have incentives to increase volatility above its natural level in each year between option grant and exercise. For this reason, our main outcome of interest is the *change* in volatility relative to the level in the previous year. In other words, we don't necessarily expect that the level of volatility will be higher in the first year after the shock to option

grants than in subsequent years. However, we should see a larger change in volatility (relative to observed volatility in the previous year) at the start of a new cycle than in other years. This is because there is a larger change in option grants at the start of a new cycle than in other years (where there is no change). An analogous argument can be made regarding our second instrument.

Figure A.1
Increased Volatility with Vesting

This figure shows the relationship between the value of an option and the volatility of the underlying. The example illustrated is for a European call option with a strike price of $50 and a 3-year time to maturity. Because it is a European option, it can only be exercised at the end of the third year, not before or after. The risk-free rate is assumed to be 5%. The baseline level of volatility is assumed to be 20%. Panel A shows how the value of the option changes if volatility is increased to various levels in the first year only or the third year only. Panel B shows how the value of the option changes if volatility is increased in the third year only when the stock price is close to the money, i.e., within a 5%, 10%, or 20% band around the strike price at the beginning of the third year.

Panel A: Increased Volatility in Year 1 vs. Year 3



Panel B: Increased Volatility in Year 3 if Close to the Money

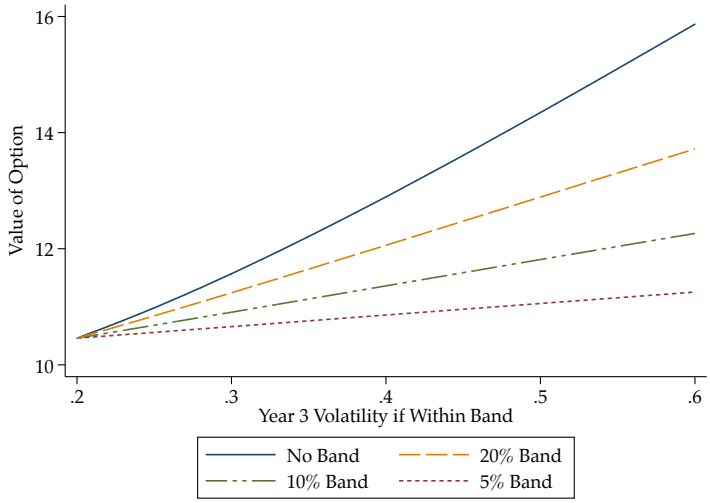


Table A.1
Summary Statistics

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	4.92	8.12
Consumer Durables	3.91	5.74	2.52
Manufacturing	19.92	20.22	11.99
Energy	6.64	5.19	3.79
Chemicals	3.12	4.37	3.52
Business Equipment	12.11	11.48	12.62
Telecommunications	1.56	1.64	3.16
Utilities	5.86	6.01	8.03
Shops	13.67	10.38	13.17
Health	7.81	7.65	8.03
Finance	12.50	14.21	12.08
Other	6.64	8.20	12.98
Total	100.00	100.00	100.00

Year: 2005	Fixed Number	Fixed Value	Other
	Percent	Percent	Percent
Consumer Non-Durables	4.84	5.11	5.35
Consumer Durables	3.49	3.24	1.88
Manufacturing	10.75	12.10	9.74
Energy	2.15	4.60	4.35
Chemicals	2.15	3.58	2.24
Business Equipment	19.35	15.50	16.56
Telecommunications	2.15	2.04	3.06
Utilities	1.08	3.07	3.93
Shops	11.02	10.05	9.65
Health	15.86	12.27	9.10
Finance	16.40	18.57	20.77
Other	10.75	9.88	13.36
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	440.35	1291.00	4504.74	322.48	852.32	3278.62
Sales (Millions)	385.96	1110.41	3596.84	412.00	1233.70	3276.91	339.43	808.47	2398.20
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	16.23	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)	204.98	286.08	439.58	201.33	287.51	412.05	185.31	275.00	416.00
Bonus (Thousands)	61.25	160.00	350.00	60.00	157.00	315.75	30.00	120.46	290.00
Number New Options	10.00	22.60	50.00	10.50	21.00	45.95	15.00	34.55	81.00
Number Prev Options	41.24	95.00	205.50	37.00	81.25	182.02	26.40	76.63	185.10
Value New Options (Thousands)	100.68	234.51	532.54	96.74	224.54	496.48	123.36	311.95	836.55
Value Prev Options (Thousands)	357.75	996.87	2545.50	305.19	861.44	2277.08	158.51	692.19	2178.96
Delta New Options	1.79	3.95	9.00	1.73	3.95	8.47	2.02	5.06	13.91
Delta Prev Options	6.67	17.92	43.64	6.09	15.83	38.85	2.87	11.90	36.56
Vega New Options	1.47	3.47	8.29	1.58	3.53	7.69	1.58	4.23	11.68
Vega Prev Options	3.01	7.85	19.04	2.89	7.62	17.33	1.21	5.20	14.81
Value Prev Options + Stock	438.55	1123.34	2760.02	364.00	1011.38	2624.63	209.99	835.95	2537.04
Delta Prev Options + Stock	7.29	18.95	47.82	6.58	17.02	42.63	3.60	13.25	39.36

Table A.1
(continued)

Year: 2005	Fixed Number			Fixed Value			Other		
	p25	p50	p75	p25	p50	p75	p25	p50	p75
Firm-Level:									
Assets (Millions)	368.53	1167.67	4203.90	512.98	1794.87	5773.00	289.10	893.34	3195.25
Sales (Millions)	236.20	830.41	2973.92	289.80	1089.68	4159.31	175.21	561.12	1880.92
Market to Book	1.24	1.75	2.42	1.25	1.69	2.34	1.17	1.54	2.35
Volatility (12 Months)	0.20	0.27	0.39	0.18	0.26	0.36	0.20	0.29	0.42
Volatility (120 Trading Days)	0.22	0.30	0.40	0.21	0.29	0.38	0.23	0.33	0.42
CAPX / PPE	0.13	0.23	0.39	0.13	0.20	0.37	0.13	0.22	0.42
Acquisitions (Millions)	0.00	0.00	25.08	0.00	0.00	37.99	0.00	0.00	13.33
Market Leverage	0.02	0.14	0.33	0.04	0.14	0.30	0.01	0.15	0.35
Book Leverage	0.04	0.30	0.52	0.11	0.32	0.53	0.03	0.29	0.54
Total Dividends (Millions)	0.00	0.00	29.69	0.00	5.92	62.96	0.00	0.00	20.92
Firm Return	-0.10	0.04	0.24	-0.10	0.05	0.26	-0.13	0.05	0.29
Return on Assets	0.06	0.13	0.19	0.06	0.14	0.20	0.04	0.12	0.20
Cash Flow / Assets	0.03	0.09	0.14	0.04	0.10	0.15	0.02	0.08	0.14
Executive-Level:									
Salary (Thousands)	255.38	351.08	520.13	274.41	383.62	568.75	236.56	327.55	488.70
Bonus (Thousands)	62.05	173.97	491.30	95.00	263.60	619.53	50.00	172.45	426.20
Number New Options	19.71	40.00	100.00	20.00	42.22	92.50	20.00	50.00	112.50
Number Prev Options	97.47	240.00	581.35	96.75	225.00	535.00	57.37	171.91	450.09
Value New Options (Thousands)	140.46	360.60	888.14	173.31	432.55	1027.27	167.23	419.10	1042.99
Value Prev Options (Thousands)	756.99	2236.59	6393.39	922.25	2705.70	7100.51	396.64	1652.87	5145.18
Delta New Options	2.37	5.90	16.07	3.04	7.56	19.49	2.73	7.16	18.16
Delta Prev Options	12.95	38.33	116.06	16.15	47.62	133.34	6.60	28.60	86.55
Vega New Options	2.50	6.12	17.85	3.29	8.04	22.12	2.64	7.22	19.13
Vega Prev Options	6.31	19.59	58.85	7.60	21.44	58.45	2.41	11.73	37.88
Value Prev Options + Stock	817.40	2496.64	7029.96	1030.45	2962.89	8041.02	427.76	1850.26	5628.46
Delta Prev Options + Stock	13.46	40.78	126.84	17.30	50.65	142.22	6.96	30.41	90.97

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 standard deviation 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

Table A.3
IV1: Volatility, Controls

Panel A shows IV estimation results, where the variable ΔLog Vega 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. Control variables include executive tenure, log of cash compensation (salary + bonus), log sales, log assets, sales growth, market to book, tangibility ratio, a dummy variable for whether the firm has rated debt, and the log delta and vega of previously granted (unexercised) options and stock holdings. All control variables are measured in the year prior to the year of the current option grant. 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	0.131***	0.144***	0.182***	0.195***
	(0.0259)	(0.0328)	(0.0346)	(0.0442)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
F-Stat (1st Stage)	76.82	50.02	76.59	49.65
Observations	36859	16142	36967	16181

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First	0.0103***	0.0120***	0.0143***	0.0161***
	(0.00177)	(0.00230)	(0.00231)	(0.00307)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
R^2	0.520	0.550	0.453	0.485
Observations	36859	16142	36967	16181

Table A.4
IV2: Volatility, Controls

Panel A shows IV estimation results, where ΔLog Vega is instrumented using FN × Ind Return as defined in Table 5. Observations are at the executive-year level. The sample is limited to executives who are on either fixed number or fixed value plans (excluding the first years of cycles). All other variables are as defined in Table 3. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the FN × Ind Return instrument. The main effects of interaction terms are included in all specifications but not shown. Control variables include executive tenure, log of cash compensation (salary + bonus), log sales, log assets, sales growth, market to book, tangibility ratio, a dummy variable for whether the firm has rated debt, and the log delta and vega of previously granted (unexercised) options and stock holdings. All control variables are measured in the year prior to the year of the current option grant. 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	0.0590**	0.0737**	0.0849***	0.112***
	(0.0265)	(0.0320)	(0.0328)	(0.0400)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
F-Stat (1st Stage)	197.6	115.8	198.5	117.2
Observations	23109	10048	23166	10067

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
FN × Ind Return	0.0279**	0.0373**	0.0402***	0.0568***
	(0.0125)	(0.0162)	(0.0155)	(0.0204)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	All	CEO/CFO	All	CEO/CFO
R^2	0.583	0.628	0.526	0.579
Observations	23109	10048	23166	10067