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

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1 Introduction

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

The use of stock options in executive compensation packages surged in the last 30 years. During the 1990s, stock options became the largest component of executive pay, and by the year 2000, options accounted for 49% of total compensation for CEOs of S&P 500 companies (Frydman and Jenter, 2010). Today, options continue to be prevalent, accounting for 25 percent of total compensation for these CEOs. Moreover, performance vesting shares, which have option-like payoff structures, have grown increasingly popular in the 2000s, representing over 30 percent of equity-linked pay (Bettis, Bizjak, Coles, and Kalpathy, 2012). Options can affect risk taking in at least two ways. Because options have convex payoffs, the expected compensation from options increases with volatility. After the recent financial crisis, many pointed to this effect to argue that options induced executives to take excessive risk. However, replacing a fixed component of compensation with options also increases an executive's exposure to the firm's volatility. This exposure effect pushes risk-averse executives to reduce their firm's volatility. We seek to measure the net effect of these two competing forces. The endogeneity of option pay complicates the task, making it difficult to determine the direction of any causal relation. We exploit quasi-exogenous variation in option compensation that results from institutional features of multi-year compensation cycles to resolve the endogeneity problem.

The common intuition that stock options cause executives to take more risk stems from the fact that the Black-Scholes value of an option increases in the volatility of the underlying stock (see, e.g., Haugen and Senbet, 1981; Smith Jr. and Watts, 1982; Smith and Stulz, 1985). However, for an undiversified and risk-averse executive, it is not necessarily utility maximizing to increase risk in response to option pay. For example, Ross (2004) shows that options (and convex fee schedules more generally) have an ambiguous effect on risk taking incentives because they can make an executive's wealth more sensitive to the underlying stock price. If the executive is risk-averse, this can then lead to what Ross refers to as a magnification effect, which can outweigh the conventional convexity effect. This effect has also been noted by Lambert, Larcker, and Verrecchia (1991), Carpenter (2000), and Lewellen (2006). Thus, it is theoretically unclear whether options should increase or decrease risk taking in practice.

There is a large empirical literature that explores the relationship between executive stock options and various measures of risk taking behavior. The evidence remains mixed. For example, Agrawal and Mandelker (1987) find that firms with higher stock and option ownership make more variance-increasing acquisitions. DeFusco, Johnson, and Zorn (1990) find that firms that approve stock option plans exhibit an increase in volatility.¹ Subsequent research has focused on the relation between a manager's vega (the sensitivity of the total Black-Scholes value of all unexercised options to volatility) and risk taking. Several papers find a small positive cross-sectional association between vega and leverage (Cohen, Hall, and Viceira, 2000) as well as stock return volatility (Guay, 1999; Cohen, Hall, and Viceira, 2000). As Guay (1999) notes, however, vega does not take risk aversion into account. To address this, Lewellen (2006) assumes that managers have a power utility function and measures the

¹For more work along these lines, see Saunders, Strock, and Travlos (1990); Mehran (1992); May (1995); Tufano (1996); Berger, Ofek, and Yermack (1997); Denis, Denis, and Sarin (1997); Esty (1997); Schrand and Unal (1998); Aggarwal and Samwick (1999); Core and Guay (1999); Knopf, Nam, and Thornton (2002)

sensitivity of the certainty equivalent of a manager's compensation package to volatility and leverage. She finds that the more a manager's certainty equivalent decreases with volatility, the more likely the manager is to issue equity rather than debt.

While these studies have grown increasingly sophisticated in terms of measuring the sensitivity of option value to changes in risk, the direction of causality is ambiguous. For example, it is easy to imagine that the long-duration investment projects of growth firms are volatile and these firms tend to compensate managers with stock options to manage the agency problems that often accompany such projects. Similarly, overconfident CEOs may prefer unusually risky projects and to be paid in options. Thus, omitted variables may bias simple cross-sectional estimates of the impact of option-like payoffs on volatility. Moreover, even within-firm or within-executive analysis suffers from dynamic versions of these concerns; periods in which a firm or executive chooses high option compensation may also be periods in which the firm or executive wishes to take high risk. Similarly, changes in compensation may be accompanied by unobservable changes in governance or strategy that directly affect firm strategy and risk taking.

A handful of recent studies attempt to address these endogeneity issues by examining firm behavior during periods surrounding changes to accounting rules that made options less advantageous. Chava and Purnanandam (2010) find that options increase risk taking while Hayes, Lemmon, and Qiu (2012) find that options do not affect risk taking. One possible explanation for these mixed results is that changes in accounting rules affected all firms simultaneously, so changes in firm policies may be attributed to the changes in options when they are in fact due to other changes in the business environment. For example, the period coinciding with the rule changes overlaps with a period of rapid growth in the use of performance vesting shares, which have many option-like features but are not technically options. Furthermore, the rule changes were discussed in advance and likely anticipated

by many firms. Using a different strategy, Gormley, Matsa, and Milbourn (2012) examine how executives that endogenously differ in their unexercised option holdings respond to an exogenous increase in firm risk that stems from the discovery of carcinogens that were used by the firm. The exogenous nature of the shock helps to rule out reverse causality, but again a firm or manager with more unexercised options may also have a different optimal or preferred response to increased risk. To identify a causal effect of option pay on risk taking, the ideal test would utilize exogenous variation in option pay.

In this paper, we exploit two distinct sources of variation in option grants induced by institutional features of multi-year compensation plans. As noted by Hall (1999), many firms award options according to plans in which executives receive a fixed number or fixed value of options.² These plans generally last two to five years, after which a new cycle begins. On a fixed number plan, an executive receives the same number of new at-the-money options each year within a cycle. On a fixed value plan, an executive receives the same value of new at-the-money options each year within a cycle. We find that multi-year grant plans are pervasive. Conditional on options being paid, more than 40 percent of executive-firm-years correspond to fixed number or fixed value plans.

Our first instrument for option compensation uses only executives on fixed value plans. In general, compensation drifts upward steadily over time. Under a fixed value plan, however, option compensation is held constant for several years within a cycle. To adjust for this, there tends to be a discrete increase in option compensation coinciding with the start of a new fixed value cycle. The timing of these discrete increases in options is staggered across executives. This allows us to use whether a given year is a new cycle start year for an executive as an instrument for increases in option compensation. We then examine whether

²This paper differs from Hall (1999) in that we explore the effect of options on risk taking by using multi-year grant cycles to instrument for exogenous variation in the value of new options granted. In contrast, Hall (1999) focuses on describing the institutional features of option grants and how each type of plan interacts with changes in firm performance.

the increases in options induced by these start years have an effect on risk taking behavior.

A potential concern with this instrument is that the length of fixed value cycles may be renegotiated mid-way through a cycle, perhaps in response to changes in the business environment. In particular, if plan cycles are terminated early during periods in which managers find it desirable to change risk for reasons unrelated to compensation, the exclusion restriction will be violated. To address this concern, we exploit the fact that firms tend to use repeated fixed value cycles of equal length. Rather than use actual cycle start years as our instrument, we use predicted cycle start years based on the length of a manager's previous cycles. For example, if a firm starts new fixed value cycles in 1990 and 1992, we predict it will start new fixed value cycles in 1994, 1996, and so on. A second potential concern is that years coinciding with the start of new fixed value cycles may be special in other ways. For example, cycle start years may coincide with periods of decreased turnover which may directly affect firm risk. Empirically, we show that this is not the case. However, to rule out other unobservable differences in cycle start years we use a separate instrument that is robust to these concerns.

Our second instrumental variables strategy does not rely on the timing of cycle start years. Rather, it focuses on variation in the value of options granted within fixed number and fixed value cycles. Our instrument exploits the fact that the Black-Scholes value of an at-the-money option increases proportionally with its strike price. As 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 thus 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 firm stock price movements are partially driven by market and industry shocks which

are beyond the control of the executive. Thus, our second instrument for the change in the value of options granted is the interaction between plan type and aggregate returns.

Given that the instrument is an interaction term, the exclusion restriction is somewhat subtle. While Hall (1999) argues that firms choose between fixed number and fixed value plan somewhat randomly, we do not assume that plan type is randomly assigned. That is, our identifying assumption is not that plan type is unrelated to the level of risk an executive would choose absent compensation effects. For example, it may be the case that firms with fixed-number plans tend to systematically differ from fixed value firms in ways that affect their optimal level of risk. Here the exclusion restriction requires the weaker assumption that fixed number firms do not differ from fixed value firms in how their (non-compensation-related) risk taking moves with aggregate returns. We provide evidence that supports this assumption through a placebo test that compares how firm risk taking 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. We find no differences in this case. In addition, our first instrumental variables strategy does not rely on this assumption.

As others have done before us, we use realized equity volatility as our primary measure of risk taking. We find a significant positive effect of option compensation on this measure of risk. In particular, a 10 percent increase in the value of new options granted leads to a 3-6 percent increase in realized volatility. We further find that the increase in equity volatility is driven by increases in leverage. That is, an increase in option compensation leads to increases in leverage ratios that are large enough to account for most of the increased volatility. We also find that options have a positive effect on investment, but results here are less robust and more subject to interpretation issues. In theory, investing in riskier projects may significantly contribute to firm risk. However, it is difficult to discern from accounting data whether investment actually represents investment in riskier projects. Therefore, we

present suggestive results that options increase overall investment but do not draw strong conclusions.

Additionally, we examine dividend payouts. Here, the theoretical prediction is unambiguous. *Ceteris paribus*, the dividend payment should reduce the firm's stock price. Most executive stock options are not dividend protected and therefore fall in value following dividend payouts. As a result, option compensation gives executives incentive to decrease dividends. Consistent with this intuition, we find that options lead to reductions in dividend payouts among firms that pay dividends. 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 examine the effect of options on firm performance. Overall, we find that the payment of options has little effect on subsequent firm returns, and if anything the relationship is negative. We also find that option compensation decreases accounting measures of performance such as ROA and cash flow to assets. However, these results are harder to interpret and may reflect increased investment or a shift toward long-term projects with higher future cash flows.

In supplementary analysis, we show that the effect of new option grants on executive behavior is larger in subsamples in which the value of new option grants is a large fraction of the total value of unexercised options held by the executive. This suggests that our estimates should be viewed as lower bounds. We measure the executive's change in behavior following a shock to a single year of new option grants. If boards increase executive option grants in all years, the changes in executive behavior are likely to be significantly larger.

The remainder of this paper is organized as follows. Section 2 discusses the data, including the definition of key variables as well as summary statistics. Section 3 discusses the empirical

methodology used. Section 4 discusses the results. Section 5 concludes.

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, most commonly used in the literature, 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. There is some overlap between the coverage of Execucomp and Equilar. When a firm-year is present in both datasets, we use Execucomp.

All three data sets are derived from firms' annual proxy statements and contain information regarding the total compensation paid to top executives in various forms during the fiscal year. We define an option grant as any block of option shares that are pooled under a single grant identifier in the data and that are granted in a single day. In some cases, executives receive more than one option grant during a fiscal year. Equilar and Execucomp have more detailed grant-level data with information on the date and amount of each option grant made during the fiscal year. This is important because it allows us to better identify executives on fixed number and fixed value plans in cases when 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 measure aggregate returns more precisely between consecutive cycle grants and volatility more precisely following a cycle grant. In 2006 firms were required to begin reporting the fair value of option compensation as part of a new reporting format. Before 2006 they were not required to do so; therefore, we compute the Black-Scholes value

of options grants ourselves throughout as well. Following 2006, firms were also required to report information on unexercised options held by executives at the end of each fiscal year. Equilar and Execucomp both collect these data.

The accounting data comes from Compustat. Execucomp is already linked to these data sources because both are owned by S&P. For the other sources, firms are matched using CUSIP and ticker via the CRSP historical names file and the CRSP-Compustat link file. Following standard practice, financial firms (SIC codes 6000-6999) and regulated utilities (4900-4999) are excluded from the sample when accounting-based outcomes are used. Market and firm return data come from CRSP as well as the Fama-French Data Library.

2.2 Detecting Cycles

Unfortunately firms are not required to disclose multi-year compensation plans. Therefore, few report them. Following Hall (1999), we detect these cycles out using the data. While there is measurement error involved in our procedure, this should not present a source of systematic bias.

2.2.1 Fixed Number

An executive is coded as being on a fixed number cycle in two consecutive years if he receives the exact same number of options in both years. If the executive has multiple option grants per fiscal year, he is also coded as being 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 top executive in the firm as well as another grant that is part of a fixed number plan specific to him. In this case, to ensure that the fixed number grant is significant relative to other option compensation, we require that over the fixed number cycle, the number of options in the fixed number grant

constitutes more than 50% of the total number of options granted.

2.2.2 Fixed Value

There are a few additional issues to consider when we try to 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. Our conversations with compensation consultants suggest that the most common alternative valuation used in practice is the face value, i.e., the number of options granted multiplied by the grant-date price of the underlying stock.³ Among the firms that value option grants using the Black-Scholes methodology, firms may make a variety of assumptions 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 options grants fixed, firms sometimes hold the value as a proportion of salary or salary plus bonus fixed.

Accordingly, an executive is coded as being on a fixed value cycle in two consecutive years if the value of options he receives (possibly as a proportion of salary or salary plus bonus) is within 3 percent of the previous year. Value is either computed as the Black-Scholes value, face value, or company self-reported value.⁴ We require that years within a fixed value cycle must be defined using the same valuation methodology throughout. Again, if multiple grants

³Note, holding face value constant is also 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%.

⁴The 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): $Se^{dT}N(Z) - Xe^{-rT}N(Z - T^{(1=2)})$, where $Z = [\ln(S/X) + T(r + \frac{d}{2})] / \sqrt{T}$. 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 total dividends paid over the previous fiscal year end divided by the stock price at the end of the previous fiscal year.

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.

2.3 Summary Statistics

Figure 1 shows the prevalence of multi-year plans over time. The area under the bottom curve represents the percent of executives that were on a fixed number plan, conditional on being paid options that year. The area between the top and bottom curves represents the percent of executives that were on a fixed value plan. The years 1991 and 1992 are missing due to lack of continuous data coverage (Execucomp starts covering executives in 1992, but we cannot detect if executives are on a multi-year plan if we cannot observe 1991 data for the same person). Overall, fixed value plans are more prevalent in the sample, representing 24 percent of executive-years in which options are paid compared to 17 percent for fixed number plans. The prevalence of both plans is fairly stable throughout the sample period, although fixed number plans have become less common in recent years, peaking at 22 percent in 2003 and then declining monotonically 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 Black-Scholes option valuation methodology. In the very recent years, there has been a decline in both types of plans possibly due to disclosure and benchmarking regulations which have led firms to adjust options annually. The recent decline in the popularity of multi-year grants is not a problem for the external validity of this study because we are not interested in multi-year grants per se; we merely use them to generate exogenous variation in option grants.

Table 1 shows the distribution of cycle length by plan type. The modal cycle length is 2

for both fixed number and fixed value plans. For executive-years on fixed number plans, 69 percent are part of a cycle with modal length 2. For executive-years on fixed value plans, 83 percent are part of a cycle of length 2. Cycles of length 3 or 4 are not uncommon, but few executive-years are part of cycles longer than 4 for either plan type.

Next, we explore the extent to which firms that use fixed number and fixed value plans differ in their observable characteristics. Panel A of Table 2 shows the industry distribution for firm-years, categorized by the CEO's plan type. Industries are categorized using the Fama-French 12 industry classification scheme. We find that multi-year cycles are distributed across many industries and that the industry distribution is similar across plan types. Panel B of Table 2 compares other firm characteristics across cycle types. Because there are likely to be time trends in these accounting 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. In general, fixed number and fixed value firms appear similar in terms of industry distribution, size, profitability, investment, leverage, and dividend policy. Overall Table 2 is consistent with Hall's claim that firms sort approximately randomly into fixed number or fixed value plans. Nevertheless, our analysis will not rely on this assumption. This is discussed further in Section 3.

3 Empirical Strategy

3.1 Instrumental Variables Strategy 1

Our first instrumental variables strategy uses only variation within the observations in which an executive is on a fixed value cycle. 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. We exploit the fact that, among executives on fixed number plans, the value of

options granted tends to follow a step function in which the value remains flat in years within a cycle with large following cycle termination or the start of new cycles. The reason is that compensation tends to drift upward over time, yet executives on fixed value plans cannot experience an upward drift in their option compensation within a cycle. As a result, they experience a discrete increase, on average, in the year following the completion of a cycle. Further, the timing of when cycles complete are staggered across executives because the starting year of cycles and cycle length varies across executives. For example, one executive may complete cycles in 1990, 1992, and 1994 while another executive completes cycles in 1989, 1992, and 1995. Thus, a potential instrument for changes in the value of options granted is an indicator for the year following the end of a fixed value cycle.

Panel A of Table 3 explores whether the first year after a fixed value cycle completes indeed predicts changes in option grants. Standard errors are clustered by firm to account for the fact that we observe multiple executives per firm. Our main independent variable is the first year indicator, equal to one if the year is the first year of a new fixed value cycle or the first year after a completed fixed value cycle.⁵ Columns 1 and 2 show that executives on fixed-value plans experience approximately a 6 percent larger increase in the Black-Scholes value of their option compensation in the first year of a cycle relative to other years. This is true for all top executives as well as for the subsample of CEOs and CFOs. Note that because cycle start years are staggered, it is possible to include year fixed effects in the regression. Columns 3-4 and 5-6 show that cycle start years are also associated with significant increases in the delta and vega of the compensation package, respectively.

One concern with using fixed value cycle first years as an instrument for changes in options is that cycle termination may partly result from renegotiation mid-way through a

⁵Cycles need not be consecutive (so a cycle first year need not follow a cycle termination year), due to endogenous renegotiation as discussed later in this section. However, both the start of a new cycle and the year following the end of a cycle are associated with large increases in option pay. This is to counteract the fact that option grants in previous or later years are constrained to be fixed in value.

cycle. For example, in good times, executives may seek to prematurely end fixed value cycles and receive a raise. In this case, first years may coincide with periods in which risk taking is expected to increase or decrease for reasons unrelated to the incentives provided by compensation. This, in turn, would lead to a violation of the exclusion restriction that is required for an instrument to be valid.

To address this concern, we instead instrument using an indicator for predicted first year, i.e. whether an executive-year observation should have been the cycle first year if everything had proceeded as normal, without renegotiation. We use the fact that executives tend to have repeated cycles of equal length. This allows us to use the length of an executive's previous cycle to predict the length of his next cycle absent endogenous renegotiation. Using these predicted cycle lengths, we are able predict if the executive-year under observation will be a cycle first year. Because these predicted first years indicators are made using only prior information, they will be unrelated to expected changes in risk taking⁶.

We use the following simple algorithm to predict first years. Let k be the length of the executive's last completed fixed value cycle. If there was no previous cycle, let $k = 2$, because this is the modal cycle length in the data as shown in Table 1. In year t , let n_t be the number of consecutive years, inclusive, in which the executive received the same value of options (within the aforementioned tolerance of 3%). We predict that year $t + 1$ will be a cycle first year if $n_t = k$. Note that these predictions only use information from previous years. We also exclude the first year of each executive's tenure from the later IV analysis because those years are likely to be special in other ways besides being the first year of a new cycle. 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, so we use the above methodology which is the simplest and most

⁶Strictly speaking, this assumes that firms and executives do not choose the previous cycle lengths in anticipation of risk taking conditions at the start of the cycle after next.

transparent.

To illustrate how this works in practice, Figure 2 shows two real examples of fixed value cycles taken from the data. The years that we predict to be cycle start years are indicated by a dotted vertical line. The example in Panel A shows 3 cycles each of length 2. In this case, we correctly predict the cycle start years in 2006 and 2008. The example in Panel B shows a cycle of length 2 followed by two cycles, each of length 3. In this case we correctly predict a cycle start year in 2000, then incorrectly predict a start year in 2002 due to the change in cycle length, then correctly predict a start year in 2006.

Using our predicted start years, we estimate the effect of changes in option compensation using an instrumental variables framework. Specifically we estimate first and second stage equations of the form:

$$(1) \quad O_{ijt} = \alpha_0 + \alpha_1 I_{ijt}^{\text{PredictedFirstYear}} + \alpha_2 t + v_j + \epsilon_{ijt}$$

$$(2) \quad Y_{ijt} = \beta_0 + \beta_1 O_{ijt} + \beta_2 t + v_j + \epsilon_{ijt}$$

where i indexes executives, j indexes firms, and t indexes firm fiscal years. The variable $I_{ijt}^{\text{PredictedFirstYear}}$ is an indicator for predicted start year, O_{ijt} is a measure of the value of the option grant, and Y_{ijt} is the outcome of interest, measured as annual changes for stock variables and levels for flow variables. Year fixed effects and firm fixed effects are represented by t and v_j , respectively. Standard errors are again clustered by firm to account for the fact that we observe multiple executives from the same firm.

3.1.1 Exogenously-Timed but Anticipated Increases in Option Compensation

In an ideal experimental setting, executives would receive random surprise changes in options compensation. Predicted start years are different in that they provide exogenously-timed, but anticipated increases in option compensation. This does not invalidate our analysis if

represents a bias against our findings that option grants lead to increased risk-taking.

Suppose that an executive knows that next year will be the start of a new fixed value cycle, so he expects to receive an exogenously-timed \$500K increase in the value of options granted. If the executive is able to change firm risk instantaneously, he would not have an incentive to increase risk until after the options are granted (if he increases risk before the options are granted, he will just receive fewer option shares, totaling to the same value). However, if the executive can only change risk slowly, he might wish to begin increasing risk prior to receiving the options so that he can continue to increase risk after the options are granted. This anticipation effect should be a bias against our results which find a positive effect of options on risk because we only measure the marginal increase in risk after the options are granted in predicted first years.

A related concern is that, if executives are able to change risk quickly, they may seek to manipulate risk temporarily to increase the real value of their next option grant. For example, suppose an executive expects that next year, he will receive options with a Black-Scholes value of \$1 million according to his fixed value plan. In addition, suppose that he knows that the Black-Scholes value will be calculated using the firm's equity volatility in the 90 days before the grant. In this situation, he has an incentive to temporarily depress volatility in the 90 days prior to the grant so that the estimated value per option share falls, such that more shares need to be awarded to total \$1 million in Black-Scholes value. Then, after the options are granted, he can restore volatility to its previous level and hold options worth more than \$1 million. Short run manipulation of volatility of this kind is not a problem for our methodology because we examine the one-year change in the 12 month volatility as our outcome. If the incentive to engage in short run risk manipulation is the same before each annual grant, then the risk manipulation in two adjacent years should net to zero when we calculate the change in 12 month volatility following the grant. If the incentive to engage

in short run manipulation is increasing with the size of the option grant, then it should be a bias against our findings that the change in 12 month volatility is greater following exogenously-timed increases in option pay. Further, we find similar results if we analyze the one-year change in 120 trading day volatility follow the option grant, which presumably is not affected by short run risk manipulation.

Finally, one may be concerned that the predicted cycle first years may be unusual in ways beyond just the increase in option compensation. For example it may be that turnover risk is lower during these years if predicted first years are also the first year of an employment agreement (for a detailed description of employment agreements, see Xu, 2011). In this case, executives may increase risk taking because they feel they are less likely to be terminated. In later sections, we provide direct evidence against the turnover hypothesis. However, we cannot directly rule out other unobservable difference in these years. For example, predicted cycle first years may tend to be the first year of new product cycles. Instead, we complement our analysis with a second instrumental variables strategy that does not use the timing of cycle start years.

3.2 Instrumental Variables Strategy 2

Our second instrumental variables strategy uses differences in the way that option compensation moves within a cycle for executives on fixed number and fixed value plans. Mechanically, the value of new option grants cannot change within a cycle for executives on a fixed value plan. In contrast, the value of new option grants within a fixed number cycle changes with the price of the underlying stock. This is because the Black-Scholes value of each share of an at-the-money option increases in proportion to the strike price. Thus, if a firm using a fixed number plan experiences an increase in its stock price, the total value of new options awarded to its executives increases as well.

This is illustrated via an example in Table A.3, adapted from Hall (1999). The example shows how option compensation would evolve for an executive 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 increase by 20 percent each year along with the stock price. Thus, it is clear that the value of new grants are more sensitive to stock price movements for executives on fixed number plans than for executives on fixed value plans. Of course, stock price movements are partially driven by market and industry shocks, which are beyond an executive's control. Thus, our second instrument for changes in option compensation is the interaction between plan type and aggregate returns.

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

$$(1) \quad \Delta O_{ijt} = \alpha_0 + \alpha_1 I_{ijt}^{FN} + \alpha_2 R_{jt} + \alpha_3 I_{ijt}^{FN} R_{jt} + \epsilon_t + v_j + \eta_{ijt}$$

$$(2) \quad Y_{ijt} = \beta_0 + \beta_1 I_{ijt}^{FN} + \beta_2 R_{jt} + \beta_3 \Delta O_{ijt} + \epsilon_t + v_j + \eta_{ijt}$$

where I_{ijt}^{FN} is an indicator equal to one if the executive is on a fixed number plan, and R_{jt} is the Fama-French (49) industry return over the 12 months prior to the grant date (Fama and French, 1997). The interaction term $I_{ijt}^{FN} R_{jt}$, is the excluded instrument. The sample is restricted to executives on a fixed value or fixed number plan so that we can compare executives whose compensation is mechanically sensitive to industry returns with those whose compensation is mechanically insensitive to industry returns.

Note that I_{ijt}^{FN} and R_{jt} are not excluded instruments, as they appear in the second stage regression as well. Thus, our identification does not require that plan type or aggregate

returns be unrelated to non-compensation-related risk taking. It may well be, for example, fixed number firms tend be of the type that 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 requires that the interaction term, $I_{ijt}^{FN} R_t$, only relates to risk taking, Y_{ijt} ; through its effect on compensation. In other words, we assume that fixed value and fixed number executives do not have different non-compensation induced responses to changes in aggregate returns. We support this assumption through a placebo test that compares how firm risk taking 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.

3.3 Other Empirical Considerations

Before looking at the results, we address five considerations that apply to the empirical strategies described above. First, both instruments directly affect changes in the value of new options granted. However, most options have a minimum vesting period of three years, i.e., they cannot be exercised until three years after the grant date. Thus, the typical executive holds previously granted options in addition to the new grant of options. This is not a problem for our methodology because our instruments affect one component of total options held and do not directly affect the other components, so the instruments should also generate exogenous variation in the total stock of options. While data on each executive's total value of unexercised options is unavailable prior to 2006, we can approximate these values using the fact that firms are required to report the total number of shares of unexercised exercisable and unexercised unexercisable options held by each executive at the end of each firm fiscal year. Our estimation procedure follows Core and Guay (2002). In unreported results, we

show that our instrument generates significant variation in the total value of an executive's stock of unexercised options. We also show in later sections that the effect of new options on risk taking is larger in subsamples in which the most recent option grant represents a large fraction of all options held. Importantly, this suggests that our results measure a lower bound. Holding the stock of unexercised options constant, an exogenous 10 percent increase in the value of a new option grant increases risk by 3 to 6 percent. If all options grants were to increase by 10 percent, the effect on risk would likely be larger.

A second related consideration is that, in expectation, the Black-Scholes value of executive options rises with any temporary or permanent increase in volatility during the period after the option is granted but before it is exercised (usually three years due to minimum vesting requirements). Because our instruments induce annual-level variation in the value of new options granted, we are constrained to look at the change in firm outcomes in the year immediately following the option grant. To the extent that our instrument also affects behavior two or more years after the grant date, our estimates are again likely to represent lower bounds.

A third consideration is that non-option based compensation may adjust to offset changes in the value of options granted. For example, we know that in years when the aggregate return is high, executives on fixed number cycles tend to experience increases in option grants while those of fixed value cycles do not gain. During these boom periods, boards may increase the non-option compensation (e.g. cash bonus) of fixed value executives so that their total pay remains comparable to the total pay of fixed number executives. In Section 4.4, we show that this effect does not seem to be significant in the data. Nevertheless, even if non-option based compensation adjusts to completely offset changes in the value of options granted, such that value of total compensation remains fixed, variation in the proportion of total pay that is awarded as options can still affect risk taking incentives.

A fourth consideration is that the Black-Scholes value, delta, and vega of new at-the-money options are all highly correlated and affected by our instruments. Therefore, while previous studies have looked at the relationship between vega and risk taking while controlling for Black-Scholes value and delta, such an approach makes less sense in our context. For brevity, we show in the next section that our instruments significantly affect the annual change in the value, delta, and vega of new options granted and then focus on Black-Scholes value as the dependent variable in the first stage of the two-stage IV estimates. However, using delta or vega as the first stage outcome yields similar results. To emphasize this point, we also present reduced-form estimates of 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.

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

4 Results

4.1 Instrumental Variables Strategy 1

We begin by instrumenting for the change in the value of new option grants using the indicator for whether each executive-year observation is predicted to be the first year of a new fixed value cycle. As described in Section 3, the sample is restricted to executives on fixed value cycles and we use predicted first year rather than actual first year to purge the estimation of endogenous renegotiation regarding the timing of cycle start-years.

Panel B of Table 3 shows that the instrument, the predicted first year dummy, is a strong predictor of changes in the Black-Scholes value of new options. Using the full sample of executives, predicted first year corresponds to a 7 percent increase in the value of new options, an 8 percent increase in the delta of new options, and a 6 percent increase in the vega of new options. If we restrict the sample to CEOs and CFOs, the results are very similar with slightly larger point estimates. The Black-Scholes value, delta and vega of new at-the-money options are all highly correlated. For brevity, we focus on the Black-Scholes value in the remainder of the analysis. However, what we estimate is the overall effect of an increase in the value, delta, and vega of at-the-money options granted on risk taking.

Our estimated effect is specific to at-the-money options. A firm can also grant an executive options with higher value by decreasing the strike price below the current stock price. This never happens in practice, so our estimates cannot speak to the effect of these types of increases in the value of options paid.

In Panel A of Table 4, we explore the effect of an increase in options on firm volatility. We measure volatility in two ways: the volatility of monthly returns in the 12 months following the grant date and the volatility of daily returns in the 120 trading days following the grant date (approximately half a year). Both are annualized. Because we use an instrument that

predicts changes in option value, we focus on annual changes in our volatility measure as the outcome.⁷ The top panel presents the second stage of the IV regression of the change in volatility on the change in the log Black-Scholes value of new option grants, as instrumented by the predicted first year dummy. The bottom panel presents the reduced-form regression of the change in volatility 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 imply that a 10 percent increase in the value of new options corresponds to a more than 0.02 unit increase in equity volatility relative to the median 0.3, or a 6.7 percent increase in volatility. We can also consider the direct impact of the increase in volatility on the executive's wealth. The median executive in our sample holds options with a vega of \$100K. For an increase in volatility of 0.02, this translates to an additional \$200K in expected wealth.

In the remainder of the analysis we explore possible channels that may drive the change in 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.

Panel B of Table 4 shows that an increase in options does indeed lead to significant increases in leverage. A 10 percent increase in the value of new options corresponds to an 0.007 unit increase in the debt to capital ratio. Similarly, a 10 percent increase in the value of new options corresponds to a 0.006 increase in the debt to asset ratio, a 6 percent increase relative to the median.

Next, we explore the effect of options on investment. These tests should be viewed as exploratory because it is not obvious how an increase in investment should affect firm

⁷In unreported results, we also find a significant positive effect of an increase in options on the level of volatility.

risk. Since we use exogenous variation in options rather than investment, we do not take any position on the relationship between investment and risk. Instead, we explore how option grants affect investment, and leave the question of whether this increase in investment contributed to the observed increase in volatility to future work. In Panel A of Table 5, we find that a 10 percent increase in options leads to a significant 1.7 percent increase in capital expenditures and a 3 percent increase in total investment (defined as the sum of capital expenditures, R&D, acquisitions, and advertising expenses).

Panel A of Table 5 also explores the effect of options on dividends. Column (3) shows that, among firms that already pay dividends, a 10 percent increase in options leads to a significant 1.7 percent decline in dividends. The effect of options on the infra-marginal decision to pay any dividends is also negative, although the magnitude of the effect is small and insignificant. Again, we do not take a firm stand on how dividend payments affect firm risk. Nevertheless, the analysis 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, while most equity holders should be indifferent to dividend policy, option holders gain from reducing dividend payouts. The results in Table 5 using the instrument also stand in stark contrast to the positive correlation between dividend growth and options as shown later in Table A.1. The OLS results are likely driven by the problem that firms that are doing well are likely to increase both dividends payouts and option payouts. This issue highlights the necessity of the instrumental variables strategy draw out and clarify the true effects of options on executive behavior.

Finally, Panel B of Table 5 shows that options lead to flat or negative changes in firm performance. Equity returns in the 12 months following the increase in option grants are flat, while measures of operating performance such as ROA and cash flow to assets are

significantly lower. However, we should not interpret the reduction in short term operating performance as 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.

Altogether, these tables show that an increase in options leads to an increase in firm volatility that is primarily driven by an increase in firm leverage. As discussed in detail in Section 3 this analysis comes with three major caveats. First, by using predicted first year as our instrument, we rely on exogenously timed but expected increases in option pay. Relative to random unexpected changes in options, this is a bias against our findings of a positive changes in volatility. Second, we focus on exogenously timed changes in the value of new option grants even though executives are likely to also be influenced by their total stock of unexercised options, which includes unvested options granted in previous years. Finally, we may be concerned that predicted first years will tend to coincide with turnover, product cycles, or major performance reviews. Empirically, we find that expected cycle termination is uncorrelated with turnover (see Section 4.4) 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 number cycles. Following the methodology described

in Section 3, the excluded instrument is the interaction between the *fixed number* indicator and the industry return. The specification allows for endogenous *fixed differences* between *fixed number* and *fixed value* executives and does not assume that plan type is randomly assigned.

In Table 6, we show that the instrument indeed reliably predicts changes in the Black-Scholes value, delta, and vega of new option grants. We find that, for a one standard deviation change in the industry return, executives on *fixed number* plans receive an additional 12 percent increase in option grants relative to executives on *fixed value* plans. Again we instrument for changes in Black-Scholes value in the remainder of our analysis.

We begin using this second source of variation by exploring the effect of an increase in options on changes in volatility, as measured by the 12 month volatility and the 120 trading day volatility in the period after the option grant date. As with our first type of variation, we present both the second stage of the IV specification and the reduced-form regression of our outcome of interest on our instrument and controls. For brevity, Table 7 reports results for the full sample of executives and clusters standard errors by firm to adjust for within-firm correlations. Using this second source of variation, 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 not significantly different from those in the earlier estimation. A 10 percent increase in the value of new options granted leads to a 0.007 increase in equity volatility, or a 2.3 percent increase relative to median volatility.

We again find that the primary mechanism driving the change in volatility is an increase in firm leverage. Columns (3) and (4) of Table 7 show that a 10 percent increase in the value of new options granted leads to an approximately 3 percent increase in both the debt to capital ratio and the debt to asset ratio.

Table 8 explores the effect of changes in options on investment, dividend policy, and firm

performance. The results are similar to those using the first source of variation, although magnitudes differ slightly. We find that an increase leads to a marginally significant positive increase in capital expenditures with noisily estimated effects for total investment. Dividend growth falls significantly, which is consistent with the view that executives may wish to lower dividends because many executive stock options are not dividend protected. Finally, a increase in options leads to lower returns and operating performance (in contrast to the results using the first instrument, here we estimate that the change in returns is marginally significant while the change in operating performance as measured by ROA and cash flows is noisily estimated). Overall, our second source of variation in option grants yields the same message as before. Increased options lead to an increase in volatility that is primarily driven by increases in leverage.

As described earlier, the validity of the second IV procedure rests upon the assumption that fixed number and fixed value firms do not have differential non-compensation related responses to changes in industry returns. If this assumption holds, then the observed changes in volatility and other firm outcomes must be due to the change in option grants induced by the differential sensitivity of fixed number plans to market movements. We support this assumption by using a placebo test that compares the responses of fixed number and fixed value firms to industry returns in years in which the firms do not award options according to any multi-year plan. This placebo test exploits the fact that both fixed number and fixed value cycles grew in popularity prior to the 2000s (due to the rise of options compensation more generally) and fell in popularity after 2005, which is likely due to peer benchmarking disclosure requirements that led to option grants being adjusted annually. We estimate the following regression:

$$Y_{ijt} = \alpha_0 + \alpha_1 I_{ijt}^{FN\ Placebo} + \alpha_2 R_{it} + \alpha_3 I_{ijt}^{FN\ Placebo} R_{it} + \epsilon_t + v_j + \eta_{ijt}$$

We restrict the sample to executives who are, in some other year, on a fixed number or fixed value cycle and we exclude those who have ever been on both types of cycles. IV_{it} is an indicator for whether the executive was at some earlier or later point on a fixed number cycle. A β_3 close to zero would support the assumption that fixed number and fixed value firms do not have different optimal responses to market movements.

Table 9 shows that, across a variety of outcome measures (change in option value, volatility, return, investment, leverage, and dividend policy), fixed number and fixed value firms 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 the point estimates are close to zero with small standard errors, suggesting that β_3 is a well-estimated zero effect. These results provide evidence of the differential responses of fixed number and fixed value firms to industry returns in the years when options are awarded according to these cycles are due to the differential sensitivity of their option compensation to these returns rather than other factors.

4.3 New vs. Existing Options

So 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. We suspect that the effect of new options grants on risk taking may be weaker if the executive already holds a large stock of unexercised options that were granted in the past. Options are typically granted with a three-year minimum vesting period, i.e., they cannot be exercised until three years after the grant date. While we do not have precise measures of the Black-Scholes value, delta, or vega of each executive's stock of unexercised options, we can approximate these values using the fact that firms are required to report the total number of shares of unexercised exercisable and unexercised

unexercisable options held by each executive at the end of each firm fiscal year. Our estimation procedure follows Core and Guay (2002). We find that on average, new grants account for one-fifth of the Black-Scholes value of all options held. Since new at-the-money options tend to have higher vega than options that are already in-the-money, new option grants account for a higher fraction of the vega of all unexercised options, approximately one-third.

In Table 10 we find that the effect of increases in new option grants on changes in volatility is two times larger if the ratio of the value of new options to existing options is in the upper half of the distribution. We find similar results when we divide the sample into terciles in terms of the ratio of the value of new options to unexercised options.

As expected, we find that new options are more likely to affect behavior when they significantly change an executive's total option holdings. These results also suggest that our estimates should be viewed as a lower bound. We measure the marginal change in behavior following a shock to a single year of new option grants. If boards increase option grants in all years, the changes in executive behavior are likely to be significantly larger.

4.4 Endogeneity

We exploit cycle-induced variation in option grants because we suspect that the correlation between firm outcomes and option grants may be driven by other unobserved factors. In Table A.1, we show the endogenous relationships between option grants and firm outcomes as estimated using OLS. The top panel includes firm fixed effects while the bottom panel excludes them⁸. The OLS estimation leads to estimates that are very different, often of the opposite sign, relative to those estimated using IV. Using OLS, an increase in option grants

⁸Because the outcomes are expressed in terms of changes or flows (investment is the change in capital stock), mean level differences across firms are already accounted for even in specifications without firm fixed effects. The addition of firm fixed effects controls for fixed differences in mean growth rates across firms.

is correlated with near zero changes in volatility and leverage, significant declines in firm returns, and significant increases in investment and dividends. 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 relatively lower returns in the year following the pay raise relative to the high returns in the previous year. Growth firms may tend to award more in options and engage in high levels of investment. Finally, firms that have 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.

Table A.2 explores endogenous renegotiation of the terms of multi-year cycles and compensation. Endogenous renegotiation, to the extent that it occurs, does not bias our results because we use predicted cycle status instead of actual cycle status as our first source of variation and because we allow endogenous choice of fixed number or fixed value plans in our second IV estimation. Nevertheless, we present supplementary results measuring the extent of endogenous renegotiation. In Panel A, we explore whether executives tend to switch between fixed number and fixed value plans (or depart from using any plan) depending on firm or industry returns. We find very little evidence of endogenous switching between cycle types. Even when the industry return is high, such that fixed value executives receiving less pay than fixed number executives, fixed value executives are not more likely to depart from their original cycle type. In Panel B we look at whether fixed value executives, in years when the industry return is high, tend to receive raises in their non-option compensation to compensate for the fact that their option compensation remains flat while other executives likely receive increases in options. We find a positive but insignificant effect. Moreover,

even if it were the case that firms using fixed value plans adjusted other compensation so that their executives' total compensation were equally sensitive to aggregate returns as fixed number executives, we would still expect risk taking to be less sensitive to aggregate returns for fixed value executives. This follows because cash compensation does not create incentives to take on risk. Finally, we test if the predicted termination of fixed value or fixed number cycles tends to coincide with executive turnover and find no evidence of such effects.

5 Conclusion

We explore the effect of executive option grants on risk taking using two sources of variation induced by the institutional features of multi-year grant cycles. First, the value of new options grants increases by a large discrete amount in years that are predicted to be the start of a new fixed value cycle. Second, fixed number executives receive option grants that are more sensitive to market movements than fixed value executives.

The two types of variation yield similar results. We find that an increase in option grants leads to a modest but significant increase in firm equity volatility. The majority of this increase in volatility is driven by increases in leverage. An increase in option grants also leads significantly lower dividend growth with mixed effects on investment and firm performance.

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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 were on a fixed-number plan, conditional on being paid options that year. The area between the top and bottom curves represents the percent of executives that were on a fixed-value plan. The years 1991 and 1992 are missing due to lack of data coverage.

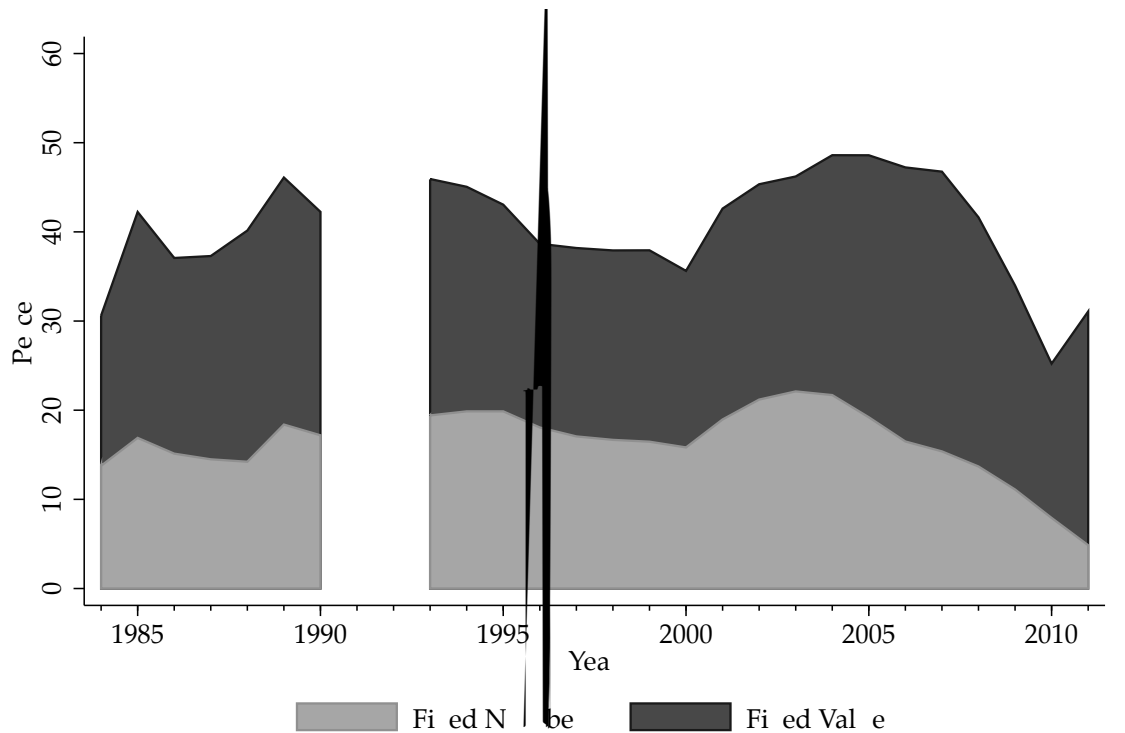
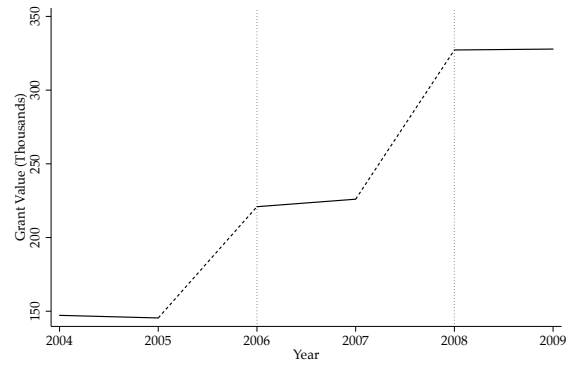


Figure 2

Real Examples of Fixed Value Cycles and Predictions

This figure represents two examples of fixed value cycles taken from the data. Years that we predict to be cycle start years are indicated by a dotted vertical line.

Panel A: Example 1



Panel B: Example 2

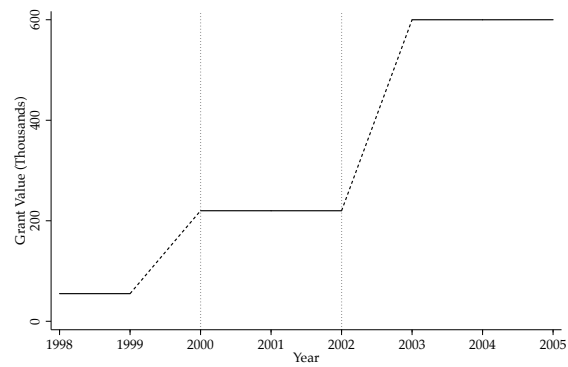


Table 1
Length of Cycles

This table shows the distribution of cycle length by cycles type as well as pooled.

	Fixed Number		Fixed Value		Total	
	Freq	Pct	Freq	Pct	Freq	Pct
2	12449	68.73	35809	83.16	48258	78.88
3	3586	19.80	4978	11.56	8564	14.00
4	1183	6.53	1407	3.27	2590	4.23
5	495	2.73	435	1.01	930	1.52
6	401	2.21	433	1.01	834	1.36
Total	18114	100.00	43062	100.00	61176	100.00

Table 2

Firm Characteristics

This table shows firm characteristics by cycle type. Panel A shows the industry distribution for firms-years, 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 characteristics across cycle types. Because there are likely to be time trends in these accounting variables, we take three cross sections of the data rather than pool all years.

Panel A: Industry Distribution

	Fixed Number		Fixed Value	
	Freq	Pct	Freq	Pct
Consumer NonDurables	386	6.05	592	6.22
Consumer Durables	199	3.12	331	3.48
Manufacturing	861	13.48	1,343	14.10
Oil, Gas, and Coal	231	3.62	406	4.26
Chemicals	195	3.05	407	4.27
Business Equipment	1,091	17.09	1,313	13.79
Communication	156	2.44	219	2.30
Utilities	229	3.59	429	4.50
Wholesale, Retail	726	11.37	983	10.32
Health	678	10.62	875	9.19
Finance	1,028	16.10	1,702	17.87
Other	605	9.48	924	9.70
Total	6,385	100.00	9,524	100.00

Panel B: Accounting Variables

Year=1995

	Fixed Number		Fixed Value	
	Mean	Median	Mean	Median
Assets	4440.84	1046.80	3952.22	1043.88
Sales	3167.94	1255.15	3079.98	1077.47
Market to Book	1.86	1.46	1.86	1.58
Firm Return	0.29	0.27	0.25	0.18
Options Compensation	799.77	410.49	723.06	442.52
CAPX / PPE	0.32	0.21	0.31	0.24
CAPX / Assets	0.09	0.07	0.10	0.08
Debt to Capital	0.37	0.37	0.36	0.36
Long-term Debt	903.71	194.30	736.12	180.10
Total Dividends	92.90	6.87	85.38	11.20
Dividend Dummy	0.62	1.00	0.73	1.00

Table 2
(continued)

Year=2000

	Fixed Number		Fixed Value	
	Mean	Median	Mean	Median
Assets	4714.24	900.11	5234.34	1308.48
Sales	3170.67	882.60	3817.95	1164.55
Market to Book	2.34	1.45	2.16	1.48
Firm Return	0.17	0.08	0.15	0.05
Options Compensation	3133.89	1064.21	2855.51	1220.67
CAPX / PPE	0.47	0.26	0.34	0.22
CAPX / Assets	0.08	0.05	0.08	0.06
Debt to Capital	0.36	0.34	0.36	0.37
Long-term Debt	986.67	198.44	1123.65	225.32
Total Dividends	110.66	0.00	88.01	2.82
Dividend Dummy	0.39	0.00	0.54	1.00

Year=2005

	Fixed Number		Fixed Value	
	Mean	Median	Mean	Median
Assets	4440.84	1046.80	3952.22	1043.88
Sales	3167.94	1255.15	3079.98	1077.47
Market to Book	1.86	1.46	1.86	1.58
Firm Return	0.29	0.27	0.25	0.18
Options Compensation	799.77	410.49	723.06	442.52
CAPX / PPE	0.32	0.21	0.31	0.24
CAPX / Assets	0.09	0.07	0.10	0.08
Debt to Capital	0.37	0.37	0.36	0.36
Long-term Debt	903.71	194.30	736.12	180.10
Total Dividends	92.90	6.87	85.38	11.20
Dividend Dummy	0.62	1.00	0.73	1.00

Table 3

Option Grants and Fixed Value First Years

Panel A of this table shows how option compensation changes in the first year of a fixed value cycle. Panel B shows how option compensation changes in predicted first years of a fixed value cycle. Observations are at the executive-year level. The sample is limited to executives on fixed value cycles. First year is an indicator variable equal to one if it is the first year of a fixed value cycle (or the year following the last year of a cycle). Predicted first year is an indicator variable equal to one if the year is predicted to be a first year based on the length of the previous cycle. The variable B-S Value equals the Black-Scholes value of options, Delta equals the change in Black-Scholes value associated with a 1 percent change in the price of the underlying, and Vega equals the change in Black-Scholes value associated with a .01 change in the annualized volatility of the underlying. Standard errors are clustered by firm to account for the fact that we observed multiple executives from the same firm.

Panel A: Real First Years

	Log B-S Value		Log Delta		Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
First Year	0.0596 [8.33]	0.0545 [5.72]	0.0518 [7.55]	0.0460 [5.05]	0.0381 [4.22]	0.0277 [2.34]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CEO/CFO Sample	No	Yes	No	Yes	No	Yes
N	37378	15509	36649	15214	36649	15214
R ²	0.107	0.0727	0.0930	0.0649	0.161	0.141

Panel B: Predicted First Years

	Log B-S Value		Log Delta		Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
Predicted First Year	0.0715 [7.92]	0.0760 [6.23]	0.0774 [8.85]	0.0824 [6.94]	0.0564 [5.34]	0.0579 [4.10]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CEO/CFO Sample	No	Yes	No	Yes	No	Yes
N	37378	15509	36649	15214	36649	15214
R ²	0.107	0.0743	0.0950	0.0680	0.162	0.143

Table 4

IV1 - Volatility & Leverage

Panel A shows the IV and reduced form results for volatility, where Log B-S Value is instrumented for by the Predicted First indicator, as defined in Table 3. Observations are at the executive-year level. The sample is limited to executives on fixed value cycles. We measure volatility in two ways: the annualized volatility of monthly returns in the 12 months following the grant date and the annualized volatility of daily returns in the 120 trading days following the grant date (approximately half of one year). Panel B shows the IV and reduced form results for leverage.

Panel A: Volatility				
	12 Month Volatility		120 TD Volatility	
	(1)	(2)	(3)	(4)
Log B-S Value	0.211 [5.10]	0.247 [4.38]	0.192 [5.00]	0.218 [4.34]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
CEO/CFO Sample	No	Yes	No	Yes
N	36558	14950	36562	14961
F-stat	65.87	40.12	66.04	41.07

	12 Month Volatility		120 TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First	0.0155 [6.41]	0.0181 [5.56]	0.0149 [6.72]	0.0173 [5.88]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
CEO/CFO Sample	No	Yes	No	Yes
N	41268	16542	41248	16543
R ²	0.257	0.225	0.267	0.231

Table 4
(Continued)

Panel B: Leverage				
	Debt/Capital		Debt/Assets	
	(1)	(2)	(3)	(4)
Log B-S Value	0.0719 [2.61]	0.0739 [2.10]	0.0580 [2.79]	0.0599 [2.29]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	30196	12476	30201	12477
F-stat	54.70	34.20	54.61	34.20
	Debt/Capital		Debt/Assets	
	(1)	(2)	(3)	(4)
Predicted First	0.00548 [2.95]	0.00528 [2.08]	0.00452 [3.33]	0.00425 [2.31]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	34408	13920	34413	13922
R ²	0.106	0.0620	0.0986	0.0550

Table 5

IV1 - Investment, Dividends, and Firm Performance

Panel A shows the IV and reduced form results for investment and dividends, where Log B-S Value is instrumented for by the Predicted First indicator, as defined in Table 3. Observations are at the executive-year level. The sample is limited to executives on fixed value cycles. Investment is defined as capital expenditures or as total investment, which is the sum of capital expenditures, R&D, acquisitions, and advertising expenses. Panel B shows the IV and reduced form results for performance, which is measured using the stock return in the 12 months following the grant date, the return on assets and the cash flow-to-assets ratio.

Panel A: Investments and Dividends

	Log CapX	Log Total Inv	Log Dividends	Dividend Payer
	(1)	(2)	(3)	(4)
Log B-S Value	0.171 [1.80]	0.295 [2.26]	-0.171 [-2.23]	-0.0113 [-0.31]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	29707	29996	16774	30304
F-stat	54.76	57.27	48.78	55.21

	Log CapX	Log Total Inv	Log Dividends	Dividend Payer
	(1)	(2)	(3)	(4)
Predicted First	0.0161 [2.28]	0.0275 [2.93]	-0.0130 [-2.12]	-0.000850 [-0.33]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	33855	34234	18773	34533
R ²	0.924	0.856	0.261	0.0872

Table 5
(Continued)

Panel B: Firm Performance

	12 Month Return	ROA	Cash Flow/Assets
	(1)	(2)	(3)
Log B-S Value	-0.0910 [-1.01]	-0.0535 [-3.14]	-0.0548 [-2.81]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
CEO/CFO Sample	No	No	No
N	36763	30148	27623
F-stat	63.56	54.78	40.36
	12 Month Return	ROA	Cash Flow/Assets
	(1)	(2)	(3)
Predicted First	-0.00470 [-0.72]	-0.00335 [-3.05]	-0.00292 [-2.63]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
CEO/CFO Sample	No	No	No
N	41822	34404	31453
R ²	0.250	0.667	0.689

Table 6

IV2 - Differential Sensitivity to Industry Returns

This table shows the differential sensitivity of the compensation of fixed number and fixed value executives to industry returns. Observations are at the executive-year level. The sample is limited to executives that are either on fixed number or fixed value plans (not in their first year). The variable FN is an indicator equal to one if the executive is on a fixed number plan. Industry returns are defined the Fama-French (49) industry return of the executives' firm in the 12 months preceding the option grant associated with the cycle. Other variables are defined as in Table 3. Main effects are included in the regressions but not shown. Standard errors are clustered by firm.

		Log B-S Value	Log Delta	Log Vega
		(1)	(2)	(3)
FN	Ind Return	0.482 [14.11]	0.512 [15.50]	0.452 [8.33]
Year FE		Yes	Yes	Yes
Firm FE		Yes	Yes	Yes
N		23352	22860	22860
R ²		0.249	0.233	0.292

Table 7

IV2 - Volatility and Leverage

This table shows the IV and reduced form results for volatility and leverage, where Log B-S Value is instrumented for by FN Ind Return as defined in Table 6. Observations are at the executive-year level. The sample is limited to executives that are either on fixed number of fixed value plans (not in their first year). All other variables are as defined in Table 4.

	12 Month Vol	120 TD Vol	Debt/Capital	Debt/Assets
	(1)	(2)	(3)	(4)
Log B-S Value	0.0672 [2.04]	0.0657 [2.05]	0.0609 [3.12]	0.0311 [2.19]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	22592	22565	18784	18788
F-stat	192.2	192.3	188.3	188.1
	12 Month Vol	120 TD Vol	Debt/Capital	Debt/Assets
	(1)	(2)	(3)	(4)
FN x Ind Return	0.0317 [2.02]	0.0307 [2.04]	0.0304 [3.14]	0.0159 [2.23]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	23144	23114	19262	19266
R ²	0.323	0.339	0.181	0.188

Table 8

IV2 - Investment, Dividends and Performance

This table shows the IV and reduced form results for Investment, Dividends and Performance, where Log B-S Value is instrumented for by FN Ind Return as defined in Table 6. Observations are at the executive-year level. The sample is limited to executives that are either on fixed number of fixed value plans (not in their first year). All other variables are as defined in Table 5.

Panel A: Investment and Dividends

	Log CapX	Log Total Inv	Log Dividends	Dividend Payer
	(1)	(2)	(3)	(4)
Log B-S Value	0.184 [1.72]	-0.00325 [-0.03]	-0.239 [-2.00]	-0.0484 [-1.63]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	18492	18673	10023	18855
F-stat	185.1	184.2	59.28	186.4
	Log CapX	Log Total Inv	Log Dividends	Dividend Payer
	(1)	(2)	(3)	(4)
FN x Ind Return	0.0877 [1.64]	-0.00718 [-0.12]	-0.0860 [-2.16]	-0.0253 [-1.73]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	18965	19153	10211	19331
R ²	0.922	0.855	0.329	0.167

Table 8
(Continued)

Panel B: Firm Performance			
	12 Month Ret	ROA	Cash Flow/Assets
	(1)	(2)	(3)
Log B-S Value	-0.175 [-1.93]	-0.00926 [-0.70]	-0.0136 [-0.94]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N			

Table 9
Placebo Test

This table shows the reduced form results for outcomes from Tables 6-8 using a placebo sample of executive-years that were not on a fixed number or fixed value plan, but were associated with firms that at some point used (or would use in the future) one (but not both) of those types of plans. FN Placebo is an indicator variable equal to one if the firm at some point used (or would use in the future) a fixed number plan, but was not currently.

	Log B-S Value	12 Month Volatility	12 Month Return
	(1)	(2)	(3)
FN Placebo x Ind Return	-0.0497 [-0.70]	0.0110 [0.72]	-0.0261 [-0.61]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	14006	19400	22617
R ²	0.0917	0.278	0.240

	Log Total Inv	Debt/Capital	Log Dividends
	(1)	(2)	(3)
FN Placebo x Ind Return	0.00733 [0.16]	0.00104 [0.11]	-0.0247 [-0.62]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	19005	16686	8149
R ²	0.842	0.136	0.320

Table 10

New vs. Existing Options

This table shows that the effect of new option grants on firm volatility is greater in cases in which the value of new at-the-money option grants is a large fraction of the value of the total stock of unexercised options held by the executive. We re-estimate the reduced form specifications from Tables 4 and 7, interacting our instruments with indicator variables for whether the observation corresponds to the top half or top two terciles in terms of the value of new options as a fraction of the value of all unexercised options. For brevity, we present the coefficients for the main interaction terms of interest. All direct and two-way interaction terms are included as controls.

	120 TD Volatility			
	(1)	(2)	(3)	(4)
Pred 1st Yr	0.0377 [1.91]	0.0492 [1.91]		
Pred 1st Yr x Top Half Fraction B-S Value	-0.0572 [-2.08]			
Pred 1st Yr x Middle Tercile Fraction B-S Value		-0.0830 [-2.57]		
Pred 1st Yr x Top Tercile Fraction B-S Value		-0.0314 [-0.87]		
FN x Ind Return			0.0140 [0.14]	-0.0155 [-0.14]
FN x Ind Ret x Top Half Fraction B-S Value			0.0833 [0.90]	
FN x Ind Ret x Middle Tercile Fraction B-S Value				0.00601 [0.05]
FN x Ind Ret x Top Tercile Fraction B-S Value				0.229 [1.76]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	732	732	457	457
R ²	0.281	0.281	0.299	0.298

Table A.1

OLS Endogenous Correlations

This table shows the OLS results of regressing Log B-S Value on various outcomes in the full sample of executives. Panel A includes firm fixed effects, while the bottom panel includes them.

Panel A: Firm Fixed Effects					
	12M Vol	12M Return	Log Total Inv	Debt/Capital	Log Dividends
	(1)	(2)	(3)	(4)	(5)
Log B-S Value	0.00306 [1.68]	-0.0302 [-6.17]	0.0224 [3.56]	-0.00574 [-4.86]	0.0274 [5.59]
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	65572	65940	54477	54964	28388
R ²	0.245	0.219	0.846	0.0873	0.232
Panel B: No Firm Fixed Effects					
	12M Vol	12M Return	Log Total Inv	Debt/Capital	Log Dividends
	(1)	(2)	(3)	(4)	(5)
Log B-S Value	-0.00277 [-5.60]	-0.0184 [-8.30]	0.608 [33.83]	0.000141 [0.29]	0.0221 [7.98]
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No
N	79886	88423	72430	66667	33854
R ²	0.212	0.122	0.248	0.0168	0.0482

Table A.2

Endogenous Renegotiation

In Panel A, we explore whether executives tend to switch between fixed value plans (or depart from using any plan) depending on firm or industry returns. In Panel B we look at whether fixed value executives, in years when the industry return is high, tend to receive raises in their non-option compensation to compensate for the fact that their option compensation remains flat while other executives likely receive increases in options.

Panel A: Switching of Cycle Type and Returns

	FV to Non-Cycle (1)	FV to FN (2)	FN to Non-Cycle (3)	FN to FV (4)
Firm Return	0.00568 [0.58]	-0.00211 [-1.02]	0.00963 [0.74]	0.00710 [1.47]
Industry Return	-0.00397 [-0.18]	0.00531 [0.94]	-0.00375 [-0.13]	0.00621 [0.50]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	22772	22772	12102	12102
R ²	0.000150	0.0561	0.0411	0.0322

Panel B: Adjustment of Other Compensation for Fixed Value Executives

	Log (Salary + Bonus) (1)	Log Non-Option Comp (2)	Turnover Next Year (3)
FV x Ind Return	0.0414 [1.62]	0.0356 [0.86]	
Predicted Last Year			0.000254 [1.11]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	23127	23378	32168
R ²	0.192	0.0808	0.236

Table A.3

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

This is an example adapted from Hall (1999). It shows how the value and number of options granted move with a firm's stock price with a fixed number and fixed value plan.

Plan	Stock price		
	Year 1 Grant	Year 2 Grant	Year 3 Grant
	100	120	144