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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 firms 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 help 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, because they can make an executive's wealth more sensitive to the underlying stock price, options (and convex compensation schedules more generally) have an ambiguous effect on risk taking incentives. 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 has also been noted by Lambert, Larcker, and Verrecchia (1991), Carpenter (2000), and Lewellen (2006). Further, stock options may not have any effect on executive behavior if executives are already well monitored or if executives hedge their exposure to option compensation risk by engaging in side bets (Garvey and Milbourn, 2003). 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. However, 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).

[1]For 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)

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 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 periods surrounding changes to accounting rules that made options less advantageous. These studies deliver mixed results: Chava and Purnanandam (2010) find that options increase risk taking, while Hayes, Lemmon, and Qiu (2012) find that options do not affect risk taking. Moreover, 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 to risk helps to rule out reverse causality and allows the authors to explore a related question: how does compensation structure change in response to changes in the firm's risk environment? However, to identify a causal effect of option pay on risk taking, the ideal test would utilize exogenous variation in option-pay rather than in the risk environment.

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. 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 options each year within a cycle. On a fixed value plan, an executive receives the same *value* of options each year within a cycle. We find that multi-year grant plans are pervasive. More than 40 percent of executive-firm-years are on fixed number or fixed value plans, conditional on options being paid.

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, on average, there tends to be a discrete increase in option compensation coinciding with the start of a new fixed value cycle. Further, the timing of when new fixed value cycles start is staggered across executives and firms. This allows us to use an indicator variable for whether a given year is a new cycle start year as an instrument for increases in option compensation.

We then examine whether 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. For example, 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 an indicator variable for predicted cycle start years based on the length of a manager's previous cycles. For example, if a manager had fixed value cycles starting in 1990 and 1992, we would predict cycle start years in 1994, 1996, and so on. A second potential concern is that our instrument delivers exogenously-timed but anticipated changes in option pay. In Section 3 we describe in detail why this does not explain our findings and if anything should dampen our results. A third 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 also affect risk taking incentives. 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 stock price movements are partially driven by market and industry shocks which are beyond the control of an 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 our second instrument is an interaction term, the identifying assumption in this case is somewhat subtle. While Hall (1999) suggests that firms more or less choose between fixed number and fixed value plans arbitrarily,we do not assume that plan type is unrelated to the level of risk an executive would choose absent compensation effects. For example, fixed number firms may systematically differ from fixed value firms in unobservable 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 terms of 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 largely by increases in leverage. 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. All else equal, dividend payments should reduce a 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.

Overall, our estimates should be viewed as lower bound for the true effect of a moderate increase in options on executive risk taking.[2] We measure changes in behavior following

[2]Note that we measure lower bounds for the risk response to *moderate* changes (~ 10%) in the value of new option grants. On average, executives respond by increasing firm equity risk. In theory, 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 useful for policy makers and boards who are considering moderate changes to existing options packages. We find that, on average, executives lie in a region of their utility function in which moderate increases in options lead to increased risk taking.

shocks to a single year of new option grants. However, most executives also hold previously granted unexercised options. If boards increase executive option grants in all years, the changes in executive behavior are likely to be larger. Consistent with this idea, we find that the effect of new option grants on volatility is greater in subsamples where the value of new option grants is high relative to the total value of unexercised options held by the executive. We also find that the effect of options on risk taking is greater for firms in the financial and high tech sectors, where executives may have greater ability to manipulate risk beyond changing leverage. Finally, our methodology uses annual variation in option grants, which constrains us to study annual changes in behavior. We may underestimate the potential for options to distort long-term corporate strategy as well as incentives to manipulate firm outcomes shortly before option exercise.

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. 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. 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. Before 2006 they were not required to do so; therefore, we compute the Black-Scholes value of option grants ourselves throughout our sample period as well. Following 2006, firms were also required to start reporting information on unexercised options held by executives at the end of each fiscal year. Equilar and Execucomp both collect these data.

The accounting data come 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 (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

Firms are not required to disclose multi-year compensation cycles, and therefore, few report them. Conversations with management at top compensation consulting firms as well as analysts at Equilar, a leading provider of executive compensation data, reveal that use of multi-year cycles is a common norm rather than a formal contract. Following Hall (1999), we back out 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 in a 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, adjusting for stock splits.

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 option grants fixed, firms sometimes hold the value as a proportion of salary or salary plus bonus fixed.

[3]Note, 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%.

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 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.

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. Overall, fixed value plans are more prevalent in the sample, representing 24 percent of executive-years in which options are paid compared to 18 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 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

[4]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.

to the rising acceptance of the 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 plans is not problematic for the external validity of this study because we are not interested in multi-year plans 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 value plans 92 percent are part of a cycle of length 2. The prevalence of 2-cycles may partly be an artifact of our cycle detection methodology, particularly for fixed value. However, our conversations with compensation consultants indicate that 2-cycles are indeed common. Presumably we detect fixed number plans with more accuracy, as fewer assumptions need to be made in doing so. Looking at the distribution of cycle length for these plans we see that 2-cycles still account for 67 percent of executive-years. Again to the extent that there is measurement error in our cycle detection process it should not present a source of bias within the instrumental variables framework.

Next, we explore the extent to which firms that use fixed number and fixed value plans 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 2 presents the year 2000, while the years 1995 and 2005 are presented in the Appendix. 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. Thus, there is no reason to expect our results to be driven by differences in which industries use each type of plan. Panel B of Table 2 compares other firm and executive characteristics across cycle types. In general, fixed number and fixed value firms appear similar in terms of market to book,volatility, investment, leverage, and profitability. Fixed number firms tend to be larger in terms of assets and sales. 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 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 value 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 increases following cycle termination or the start of a new cycle. 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 may complete 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 indicators for 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 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 option 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 a cycle first year if everything had proceeded as normal, without renegotiation. To do this, we use the fact that executives tend to have repeated cycles of equal length. Indeed, conditional on being on a fixed value cycle, the length of the cycle is equal to that of the previous cycle in 90% of cases. This

[5]Cycles 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.

allows us to use the length of an executive's previous cycle to predict the length of his next cycle absent 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 are made using only prior information, they will be unrelated to expected changes in risk taking.[6]

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 $n_t \geq k$. 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 transparent.

To illustrate how this works in practice, Figure 2 shows two examples of real 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 of length 2. In this case, we correctly predict the cycle first years in 2006 and 2008. The example in Panel B shows a cycle of length 2 followed by a 2 cycles of length 3. In this case we correctly predict a cycle first year in 2000, then incorrectly predict a first year in 2002 due to the change in cycle length, then correctly predict a first year in 2006. Incorrect predictions do not bias our

[6]Strictly 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.

results – in fact, they purge the instrument of bias from potential endogenous renegotiation. Rather, incorrect predictions reduce the power of the first stage of our IV estimation.

Using our predicted first 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:

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

$$(2) \quad Y_{ijt} = \delta_0 + \delta_1 \widehat{\triangle O_{ijt}} + \gamma_t + v_j + \mu_{ijt}$$

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

Note, we do not use the actual change in option grants during predicted first years as our instrument, because the size of that raise may be endogenous to firm unobservables. Instead, we use the fact that the indicator for predicted first year corresponds to pay raises on average and is staggered across executives. In addition, we do not need to assume that firms randomly choose between cycles of length 2 or length 3 (or longer). Instead, we use the fact that, even among executives who only receive options according to cycles of length 2, predicted cycle start years will be staggered (with some executives receiving raises in even years and others in odd years). In unreported results, we restrict our sample to executives with cycles of only length 2 and find similar results.

There remain a few potential concerns with this strategy. First, predicted first years provide exogenously timed, but potentially anticipated increases in option compensation. However, even if the increase were fully anticipated, executives would not have an incentive to change risk until after the increase occurred, assuming that risk could be changed instantaneously . If executives could only change risk slowly, then they might wish to begin doing so prior to receiving the increase in options, but if anything, this would make us less likely to find year-to-year changes in risk coinciding with (predicted) cycle first years.

A related concern is that, if executives were able to change risk quickly, they may seek to manipulate it temporarily to increase the real value of their next option grant. For example, suppose an executive knew that next year, he would receive options with a Black-Scholes value of $1 million according to his fixed value plan. In addition, suppose he knew that the Black-Scholes value would be calculated using the firm's equity volatility in the 90 days before the grant. In this case, the executive would have an incentive to temporarily depress volatility in the 90 days prior to the grant so that the estimated value per option would decline, such that a greater number of options would need to be awarded to total $1 million in Black-Scholes value. Then, after the grant, the executive could 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 annual 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 annual change in 12 month volatility. If the incentive to engage in short run manipulation is increasing with the size of the option grant, then it should be a bias against our findings that the annual change in 12 month volatility is greater following exogenously-timed increases in option pay. Further, we find similar results if we analyze the change in volatility based on the first 120 trading days follow the option grant, which presumably is less affected by short run risk manipulation as it is farther removed from the next option grant.

Finally, one may be concerned that predicted cycle first years are unusual in ways beyond just the increase in option compensation that accompanies them. 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. We provide direct evidence against the turnover risk 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.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 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:

$$(1) \quad \triangle O_{ijt} = \beta_0 + \beta_1 I_{ijt}^{FN} + \beta_2 R_{jt} + \beta_3 I_{ijt}^{FN} R_{jt} + \gamma_t + v_j + \epsilon_{ijt}$$

$$(2) \quad Y_{ijt} = \delta_0 + \delta_1 I_{ijt}^{FN} + \delta_2 R_{jt} + \delta_3 \widehat{\triangle O_{ijt}} + \gamma_t + v_j + \mu_{ijt}$$

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 coefficient, δ_3, is the effect of an increase in new option grants on our outcome of interest, Y_{ijt}. The sample is restricted to executives on a fixed value or fixed number plan 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.

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 strategy does not require that plan type or aggregate returns be unrelated to non-compensation-related 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 requires that the interaction term, $I^{FN}_{ijt}R_t$, 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 other important considerations that apply to both strategies described above. First, both instruments directly affect changes in the value of *new* options granted. However, few options vest in less than 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 have no direct effect on 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 unexercised exercisable and unexercised unexercisable options held by each executive at the end of each 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 portfolio of unexercised options. Importantly, this suggests that our results measure a lower bound. All else equal, an exogenous 10 percent increase in the value of a new option grant increases risk by 3 to 6 percent. If all option grants were to increase by 10 percent, the effect on risk would likely be larger.

A second consideration relates to the fact that non-option based compensation may adjust to offset changes in the value of options granted. For example, we know that in years when aggregate returns are 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. However, even if non-option based compensation adjusts to completely offset changes in the value of options granted, such that total compensation remains fixed, variation in the proportion of total pay that is awarded as options would still affect risk taking incentives.

A third 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 on behavior.

Finally, note that we instrument for annual changes rather than levels in the value of new options granted. Fixed value plans tend to resemble a step function, so predicted first years do not necessarily correspond to higher option *levels* than other years if compensation is increasing over time. Instead predicted first years correspond to above-average annual *changes* in options. If levels of outcomes are approximately linear functions of levels of options, then exogenous changes in options should affect annual changes in the level of outcomes. The same intuition applies even if there is also mean reversion in the outcome variables. Thus, for our main dependent variable, we use the annual change in volatility. For other firm outcomes, we use the annual 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 a given year 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 indicator, is a strong predictor of changes in the value of new options. Using the full sample of executives, predicted first year corresponds to a 7 percent increase in the Black-Scholes 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. All estimates are highly significant, and the F-statistics for the instrument all greatly exceed 10 (the rule of thumb threshold for concerns relating to weak instruments). The Black-Scholes value, delta and vega of new at-the-money options are all highly correlated. We focus on Black-Scholes value in the remainder of the analysis as it is the best measure of the magnitude of the option grant. However, what we estimate is the mean overall effect of an increase in the value of at-the-money options granted on risk taking. It is important to note that our estimated effect is specific to at-the-money options. A firm could also grant an executive options with higher value by decreasing the strike price below the current stock price, although in practice this does not occur. In unreported results we find that our instrument, the predicted first year dummy, indeed strongly predicts true fixed value first years in the data, with a t-statistic exceeding 100.

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 of 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.

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

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 asset ratio, a 6 percent increase relative to the median. We can also express outcomes in terms of the change in the log of the equity to assets ratio in order to estimate the percentage of the change in volatility that can be explained by changes in leverage. According to Column (3), a 10 percent increase in the value of new options is associated with a 2.6 percent decline in the equity to asset ratio, which in turn implies approximately a 2.6 percent increase in equity volatility. Thus, the increase in leverage accounts for nearly 40 percent (2.6/6.7) of the increase in volatility that was previously estimated for the median firm.[8] In the last two columns, we look at raw levels of debt instead of debt scaled by assets. We again find an increase in leverage, suggesting that our estimates reflect active leverage management instead of changes in the denominator of these leverage ratios.

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. Since

[8] This approximation is made by observing that

$$\begin{aligned} r^A &= r^E \left(\frac{E}{A}\right) + r^D \left(\frac{D}{A}\right) \\ \Rightarrow \sigma^A &= \sigma^E \left(\frac{E}{A}\right) + \sigma^D \left(\frac{D}{A}\right) \\ \Rightarrow \ln(\sigma^E) &= \ln(\sigma^A) - \ln(\frac{E}{A}) \end{aligned}$$

where the last line follow from the previous line assuming debt is approximately risk free ($\sigma^D = 0$). Thus a $X\%$ decline in $\frac{E}{A}$ approximately leads to an $X\%$ increase in σ^E.

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 1.9 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 1.8 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.3. 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 importance of the instrumental variables strategy to estimate 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 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.

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 the indicator for 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 finding of a positive change 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 whole portfolio of unexercised options, including unexercised options granted in previous years. We explore the interaction between new and old options in Section 4.3. Finally, one may be concerned that predicted first years will tend to coincide with turnover, product cycles, or major performance reviews. Empirically, we find that expected cycle termination is uncorrelated with turnover (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 value cycles. Following the methodology described in Section 3, the excluded instrument is the interaction between the fixed number indicator and the industry return. Again, this specification does not rely on the assumption that plan type is exogenous with respect to risk taking.

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 before, 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 an 0.007 increase in equity volatility, or a 2.3 percent increase relative to median volatility.

We again find that a major mechanism driving the change in volatility is an increase in firm leverage. Columns (3) and (4) of Table 7 show that a 10 percent increase in the value of new options granted leads to an approximately 6 percent increase in the debt to asset ratio. The coefficient on the log equity to asset ratio suggests that again about 40% (.9/2.3) of the increase in equity volatility is due to the increase in leverage. Again, we also find significant changes in raw debt as well, suggesting the results reflect active debt management.

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 instrument, 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 is 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 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:

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

restricting the sample to executives who are, in some other year, on a fixed number or fixed value cycle but are not currently. We exclude those who have ever been on both types of cycles. The variable $I_{ijt}^{FN\,Placebo}$ 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 placebo results support the view that 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 incentives from option compensation rather than other factors.

4.3 Heterogeneity

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 as well as by industry.

We suspect that the 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. 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 portfolio of unexercised options, we can roughly approximate these values using the fact that firms are required to report the total number of unexercised exerciseable and unexercised unexerciseable options held by each executive at the end of each 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 previously granted options that tend to be 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 pool together the reduced form specifications from Tables 4 and 7, including both instruments in a single estimating equation. In Columns (1) and (3) we interact our two instruments with an indicator equal to one if the observation corresponds to the top half of the distribution in terms of the value of new options as a fraction of the value of all unexercised options. The joint p-value of the coefficients on the two interaction terms is reported below. In Column (3), we find strong evidence that the effect of our instruments on volatility is stronger when the new options represent a large fraction of the unexercised options held. The effect of our first instrument (Pred 1st Yr) is almost twice as large in this case, and the the effect of our second instrument (FN x Ind Return) is not statistically significant in the bottom half of the distribution, whereas it is highly significant in the top half. In unreported results also we find that the magnitude of the coefficients on both instruments increase monotonically in each tercile of the distribution. The coefficients on these interaction terms are also positive when we use 12 month volatility in Column (1), however, they are not jointly significant. This may be because this is a noisier measure of volatility, as it is based on only 12 data points, and because it includes the several months immediately preceding the next option grant, which may be subject to short-run volatility manipulation as described in Section 3.1.

These results also suggest that our baseline 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 were to increase option grants in all years, the changes in executive behavior would likely be significantly larger.

In Columns (2) and (4) we interact the two instruments with an indicator equal to one if the firm is in the financial or high-tech sectors, where executives may have greater ability to manipulate risk beyond changing leverage. For example, executives in the financial sector can manipulate asset risk by pursuing riskier financial investments or by reducing risk management. Similarly, given that the high-tech sector has very short product cycles, executives in this sector may be able to manipulate risk by pursuing riskier product development. We find that these interaction terms are jointly significant with p-values of 0.015 and 0.027, respectively. This provides supporting evidence that the effect of new options on risk taking is indeed greater in these sectors.

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.3, we show the endogenous relationships between option grants and firm outcomes as estimated using OLS. The sample is restricted to executives on fixed number and fixed value plans. The top panel includes firm fixed effects while the bottom panel excludes them.[9] The OLS procedure leads to estimates that are very different, often of the opposite

[9]Because 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

sign, relative to those from the IV procedure. Using OLS, an increase in option grants is correlated with significant decreases in firm returns and leverage, 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 lower returns in the year following the pay raise relative to the high returns in the previous year. Growth firms may tend to award more in options and engage in high levels of investment. Finally, firms that have have done well may tend to increase both dividends and option grants, resulting in a positive correlation between the two. This stands in sharp contrast to the IV results, which find a negative causal relationship between options and dividends, as predicted by the fact that most executive options are not dividend protected and decline in value following dividend payments.

Table A.4 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 in our first IV estimation 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 receive lower option pay increases than fixed number executives, fixed value executives are not more likely to depart from their 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

effects. The addition of firm fixed effects controls for fixed differences in mean growth rates across firms.

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 non-option compensation so that their executives' total compensation matched that of similar executives not on fixed value plans, we would still expect risk taking to be less sensitive to aggregate returns for fixed value executives. This follows because cash compensation does not alter risk taking incentives in the way that option compensation does. 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.

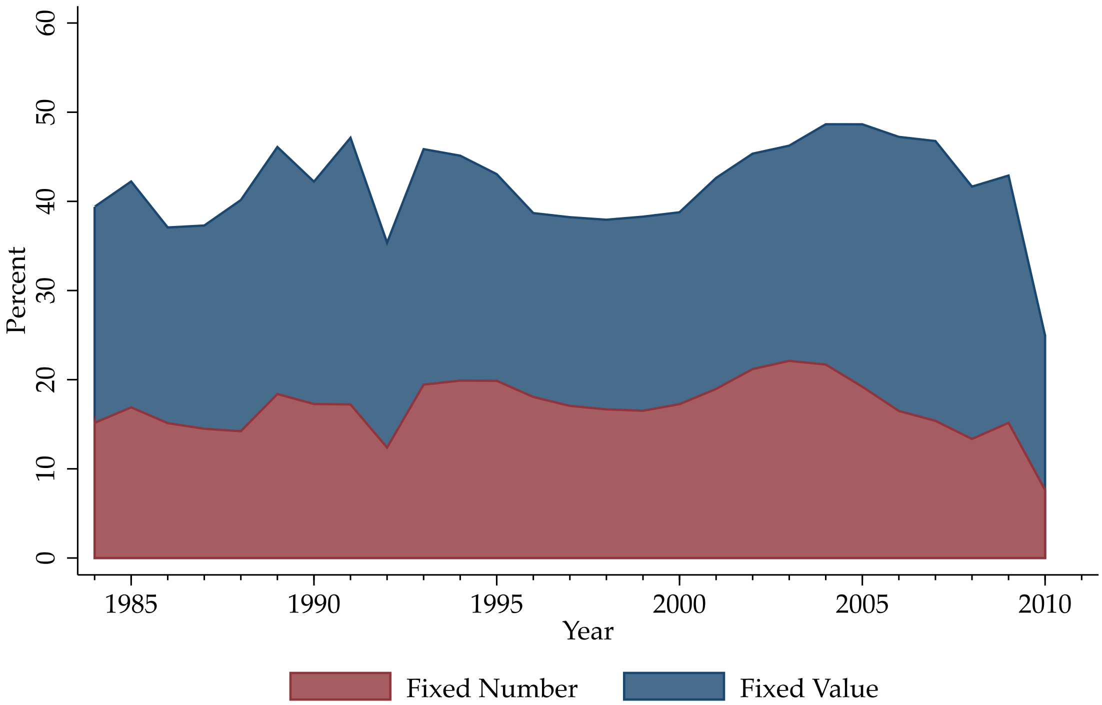
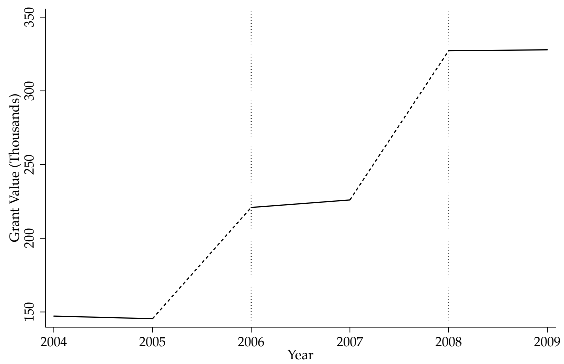


Figure 2
Real Examples of Fixed Value Cycles and Predictions

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

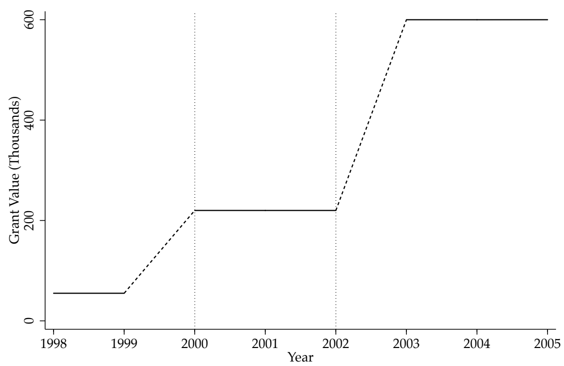


Table 1
Length of Cycles

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

	Fixed Number		Fixed Value	
	Freq	Pct	Freq	Pct
2	20,514	66.87	39,988	92.61
3	6,186	20.17	2,760	6.39
4	2,284	7.45	332	0.77
5	900	2.93	45	0.10
≥ 6	792	2.58	56	0.13
Total	30,676	100.00	43,181	100.00

Table 2

Firm/Executive Characteristics

This table shows firm and executive characteristics by cycle type. 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 percentile of the distributions. Because there are likely to be time trends in these variables, we show only summary statistics from fiscal year 2000, rather than pool all years. Fiscal years 1995 and 2005 are shown in the Appendix Table A.1.

Panel A: Industry Distribution

	Fixed Number Percent	Fixed Value Percent
Consumer Non-Durables	6.89	6.25
Consumer Durables	2.48	3.71
Manufacturing	12.40	14.06
Energy	4.13	3.91
Chemicals	2.48	3.32
Business Equipment	17.91	15.23
Telecommunications	3.31	2.34
Utilities	4.96	5.66
Shops	9.92	10.35
Health	8.26	6.05
Finance	15.43	18.75
Other	11.85	10.35
Total	100.00	100.00

Table 2
(continued)

Panel B: Other Characteristics

	Fixed Number			Fixed Value		
	p25	p50	p75	p25	p50	p75
Firm-Level:						
Assets	358.74	1,089.20	3,127.47	539.84	1,479.26	6,089.73
Sales	247.87	802.57	2,096.00	340.68	1,110.34	4,276.08
Market to Book	1.08	1.35	2.54	1.08	1.40	2.36
Volatility (12 Months)	0.35	0.50	0.84	0.32	0.43	0.65
Volatility (120 Trading Days)	0.40	0.53	0.88	0.40	0.50	0.69
CAPX / PPE	0.15	0.26	0.51	0.13	0.22	0.42
Acquisitions	0.00	0.00	46.80	0.00	0.00	33.57
Debt to Book	0.14	0.37	0.59	0.16	0.38	0.58
Total Dividends	0.00	0.00	15.86	0.00	6.09	57.38
Dividend Dummy	0.00	0.00	1.00	0.00	1.00	1.00
Firm Return	-0.28	0.08	0.43	-0.25	0.11	0.46
Return on Assets	0.01	0.04	0.10	0.01	0.05	0.10
Cash Flow / Assets	0.06	0.10	0.17	0.06	0.11	0.17
CEO-Level:						
Salary	480.00	707.34	100,000.00	449.46	700.00	1,004.11
Bonus	138.00	560.56	3,000.00	200.00	567.78	1,400.00
Options (B-S Value)	324.84	978.21	3,602.28	442.24	1,218.32	3,375.66
Age	50.00	55.00	59.00	49.00	55.00	59.00

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

	Δ Log B-S Value		Δ Log Delta		Δ Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
First Year	0.0595***	0.0547***	0.0522***	0.0472***	0.0385***	0.0292**
	[8.38]	[5.84]	[7.67]	[5.26]	[4.29]	[2.50]
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	37578	15709	36849	15414	36849	15414
R^2	0.106	0.0721	0.0928	0.0647	0.161	0.142

Panel B: Predicted First Years

	Δ Log B-S Value		Δ Log Delta		Δ Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
Predicted First Year	0.0710***	0.0751***	0.0770***	0.0814***	0.0563***	0.0578***
	[7.89]	[6.19]	[8.85]	[6.92]	[5.37]	[4.14]
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	37578	15709	36849	15414	36849	15414
R^2	0.107	0.0736	0.0948	0.0676	0.162	0.143

Table 4
IV1 - Volatility & Leverage

Panel A shows the IV and reduced form results for volatility, where ΔLog 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. Lev Ratio represents the debt to asset ratio, E/A represents the equity to asset ratio, Debt represents total debt (short-term plus long-term).

Panel A: Volatility

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Δ Log B-S Value	0.211***	0.243***	0.193***	0.219***
	[5.11]	[4.38]	[5.04]	[4.38]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
CEO/CFO Sample	No	Yes	No	Yes
N	36755	15148	36759	15159
F-stat	65.66	39.73	65.82	40.64

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First	0.0155***	0.0179***	0.0149***	0.0173***
	[6.47]	[5.64]	[6.81]	[6.02]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
CEO/CFO Sample	No	Yes	No	Yes
N	41480	16749	41460	16750
R^2	0.257	0.226	0.267	0.232

Table 4
(Continued)

Panel B: Leverage

	Δ Lev Ratio		Δ Log E/A		Δ Log Debt	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log B-S Value	0.0744***	0.0802**	-0.263***	-0.310***	0.365*	0.165
	[2.67]	[2.22]	[-3.29]	[-2.65]	[1.76]	[0.66]
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	30336	12617	29606	12277	26777	11119
F-stat	53.75	32.53	49.92	28.60	53.76	35.76

	Δ Lev Ratio		Δ Log E/A		Δ Log Debt	
	(1)	(2)	(3)	(4)	(5)	(6)
Predicted First	0.00557***	0.00562**	-0.0190***	-0.0223***	0.0308**	0.0116
	[3.03]	[2.25]	[-3.99]	[-3.18]	[2.00]	[0.56]
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	34557	14064	33700	13686	30442	12390
R^2	0.106	0.0610	0.144	0.128	0.112	0.0539

Table 5
IV1 - Investment, Dividends, and Firm Performance

Panel A shows the IV and reduced form results for investment and dividends, where ΔLog 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	Δ Log Dividends	Δ Dividend Payer
	(1)	(2)	(3)	(4)
Δ Log B-S Value	0.189**	0.318**	-0.178**	-0.00888
	[1.96]	[2.40]	[-2.30]	[-0.24]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	29845	30135	16900	30444
F-stat	53.79	56.29	48.04	54.26

	Log CapX	Log Total Inv	Δ Log Dividends	Δ Dividend Payer
	(1)	(2)	(3)	(4)
Predicted First	0.0175**	0.0291***	-0.0133**	-0.000687
	[2.49]	[3.11]	[-2.21]	[-0.27]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	34004	34385	18902	34682
R^2	0.923	0.856	0.261	0.0871

Table 5
(Continued)

Panel B: Firm Performance

	12 Month Return	ROA	Cash Flow/Assets
	(1)	(2)	(3)
Δ Log B-S Value	-0.0963	-0.0538***	-0.0557***
	[-1.08]	[-3.13]	[-2.83]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
CEO/CFO Sample	36960	30288	27753
N	63.33	53.64	39.51

	12 Month Return	ROA	Cash Flow/Assets
	(1)	(2)	(3)
Predicted First	-0.00526	-0.00335***	-0.00296***
	[-0.82]	[-3.07]	[-2.68]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
CEO/CFO Sample	42038	34556	31592
N	0.251	0.666	0.688

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 of 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 as 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.

	Δ Log B-S Value	Δ Log Delta	Δ Log Vega
	(1)	(2)	(3)
FN × Ind Return	0.482***	0.511***	0.452***
	[14.21]	[15.57]	[8.38]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	23459	22967	22967
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 ΔLog 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 define in Table 4.

	Δ 12 Month Vol	Δ 120 TD Vol	Δ Lev Ratio	Δ Log E/A	Δ Log Debt
	(1)	(2)	(3)	(4)	(5)
Δ Log B-S Value	0.0683**	0.0662**	0.0608***	-0.0905**	0.438**
	[2.09]	[2.09]	[3.14]	[-2.31]	[2.40]
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	22697	22670	18866	18359	16503
F-stat	195.0	195.1	190.9	183.2	165.4

	Δ 12 Month Vol	Δ 120 TD Vol	Δ Lev Ratio	Δ Log E/A	Δ Log Debt
	(1)	(2)	(3)	(4)	(5)
FN x Ind Return	0.0322**	0.0309**	0.0305***	-0.0442**	0.224**
	[2.07]	[2.07]	[3.16]	[-2.26]	[2.37]
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	23252	23222	19345	18845	16917
R^2	0.323	0.339	0.180	0.225	0.237

Table 8

IV2 - Investment, Dividends and Performance

This table shows the IV and reduced form results for investment, dividends and performance, where ΔLog 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 define in Table 5.

Panel A: Investment and Dividends

	Log CapX	Log Total Inv	Δ Log Dividends	Δ Dividend Payer
	(1)	(2)	(3)	(4)
Δ Log B-S Value	0.182*	-0.00446	-0.237**	-0.0475
	[1.70]	[-0.04]	[-2.02]	[-1.61]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	18572	18753	10096	18937
F-stat	187.5	186.5	61.34	188.9

	Log CapX	Log Total Inv	Δ Log Dividends	Δ Dividend Payer
	(1)	(2)	(3)	(4)
FN x Ind Return	0.0872	-0.00768	-0.0864**	-0.0250*
	[1.64]	[-0.13]	[-2.18]	[-1.71]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	19047	19235	10285	19414
R^2	0.922	0.855	0.328	0.167

Table 8
(Continued)

Panel B: Firm Performance

	12 Month Ret	ROA	Cash Flow/Assets
	(1)	(2)	(3)
Δ Log B-S Value	-0.174*	-0.00961	-0.0133
	[-1.92]	[-0.73]	[-0.92]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	22814	18857	17247
F-stat	199.4	187.1	173.5

	12 Month Ret	ROA	Cash Flow/Assets
	(1)	(2)	(3)
FN x Ind Return	-0.0820*	-0.00622	-0.00840
	[-1.85]	[-0.95]	[-1.16]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	23412	19335	17694
R^2	0.297	0.715	0.731

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 of the the firm at some point used (or would use in the future) a fixed number plan, but was not currently.

	Δ Log B-S Value	Δ 12 Month Volatility	12 Month Return
	(1)	(2)	(3)
FN Placebo x Ind Return	-0.0289	0.00882	-0.0281
	[-0.41]	[0.58]	[-0.67]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	14283	19778	23037
R^2	0.0899	0.279	0.241

	Log Total Inv	Δ Lev Ratio	Δ Log Dividends
	(1)	(2)	(3)
FN Placebo x Ind Return	0.00356	0.00117	-0.0202
	[0.08]	[0.12]	[-0.50]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	19295	16951	8392
R^2	0.841	0.136	0.317

Table 10
Heterogeneity

This table pools together the reduced form specifications from Tables 4 and 7, interacting our two instruments with two different indicator variables. In Columns (1) and (3) the instruments are interacted with an indicator equal to one if the observation corresponds to the top half of the distribution in terms of the value of new options as a fraction of the value of all unexercised options, estimated as in Core and Guay (2002). In Columns (2) and (4) the instruments are interacted with an indicator equal to one if the firm is in the financial or high-tech sector. The joint p-value of the two interaction terms is reported below.For brevity, we present the coefficients for the main interaction terms of interest. All direct and two-way interaction terms are included as controls.

	Δ 12M Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Pred 1st Yr	0.0177***	0.0144***	0.0130***	0.0117***
	[5.59]	[5.05]	[4.33]	[4.31]
FN x Ind Return	0.0120	0.000477	-0.0201	0.0230
	[0.65]	[0.02]	[-1.06]	[1.19]
Pred 1st Yr x Top Half Fraction B-S Value	0.00110		0.00890**	
	[0.23]		[2.00]	
FN x Ind Ret x Top Half Fraction B-S Value	0.0332		0.0973***	
	[1.21]		[3.37]	
Pred 1st Yr x Finance or High Tech Sector		0.0109**		0.0131***
		[1.99]		[2.64]
FN x Ind Ret x Finance or High Tech Sector		0.0600**		0.00911
		[1.98]		[0.30]
P-value Joint Test	0.463	0.0152	0.000341	0.0268
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	42296	45708	42291	45687
R^2	0.257	0.258	0.273	0.274

Table A.1
Firm/Executive Characteristics

This table shows firm characteristics by cycle type. 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 characteristics across cycle types. Because there are likely to be time trends in these variables, we show only summary statistics from fiscal year 1995 and 2005, rather than pool all years.

Panel A: Industry Distribution

Year=1995

	Fixed Number Percent	Fixed Value Percent
Consumer Non-Durables	5.88	4.94
Consumer Durables	4.41	5.45
Manufacturing	19.49	19.48
Energy	6.25	5.19
Chemicals	2.94	4.42
Business Equipment	12.87	11.69
Telecommunications	1.47	1.82
Utilities	5.51	5.71
Shops	13.60	11.17
Health	8.09	7.01
Finance	13.60	15.06
Other	5.88	8.05
Total	100.00	100.00

Year=2005

	Fixed Number Percent	Fixed Value Percent
Consumer Non-Durables	4.57	5.03
Consumer Durables	3.49	3.35
Manufacturing	10.22	11.89
Energy	2.15	4.36
Chemicals	2.15	3.52
Business Equipment	19.89	15.75
Telecommunications	2.15	1.84
Utilities	1.08	3.02
Shops	11.29	9.88
Health	15.86	12.06
Finance	16.94	19.43
Other	10.22	9.88
Total	100.00	100.00

Table A.1
(continued)

Panel B: Other Characteristics

Year=1995

	Fixed Number			Fixed Value		
	p25	p50	p75	p25	p50	p75
Firm-Level:						
Assets	456.25	1,276.55	4,273.03	462.65	1,314.85	4,372.40
Sales	435.01	1,257.46	4,316.85	451.22	1,297.40	3,275.36
Market to Book	1.14	1.41	1.99	1.12	1.50	2.06
Volatility (12 Months)	0.18	0.25	0.35	0.17	0.25	0.34
Volatility (120 Trading Days)	0.22	0.29	0.39	0.21	0.28	0.40
CAPX / PPE	0.15	0.21	0.36	0.16	0.23	0.36
Acquisitions	0.00	0.00	24.10	0.00	0.00	15.60
Debt to Book	0.20	0.37	0.54	0.24	0.39	0.54
Total Dividends	0.00	7.97	46.58	0.85	15.67	63.54
Dividend Dummy	0.00	1.00	1.00	1.00	1.00	1.00
Firm Return	0.04	0.27	0.47	0.03	0.20	0.43
Return on Assets	0.02	0.04	0.09	0.02	0.05	0.09
Cash Flow / Assets	0.08	0.10	0.15	0.07	0.12	0.16
CEO-Level:						
Salary	372.69	511.25	722.08	365.00	509.55	682.38
Bonus	104.15	300.00	722.64	145.00	310.50	620.00
Options (B-S Value)	192.06	415.00	1,041.85	230.29	436.83	830.81
Age	52.00	57.00	62.00	51.00	55.00	60.00

Table A.1
(continued)

Year=2005

	Fixed Number			Fixed Value		
	p25	p50	p75	p25	p50	p75
Firm-Level:						
Assets	272.17	1,043.25	3,382.14	521.79	1,833.38	6,038.89
Sales	209.38	868.79	2,513.89	281.85	1,130.93	4,388.00
Market to Book	1.21	1.77	2.70	1.25	1.70	2.36
Volatility (12 Months)	0.22	0.28	0.43	0.18	0.25	0.35
Volatility (120 Trading Days)	0.23	0.30	0.41	0.21	0.28	0.38
CAPX / PPE	0.13	0.24	0.39	0.13	0.20	0.36
Acquisitions	0.00	0.00	14.34	0.00	0.00	39.23
Debt to Book	0.02	0.20	0.47	0.11	0.33	0.53
Total Dividends	0.00	0.00	17.47	0.00	6.50	64.28
Dividend Dummy	0.00	0.00	1.00	0.00	1.00	1.00
Firm Return	-0.14	0.04	0.28	-0.10	0.05	0.26
Return on Assets	0.01	0.05	0.09	0.01	0.06	0.10
Cash Flow / Assets	0.03	0.09	0.15	0.03	0.10	0.15
CEO-Level:						
Salary	571.78	851.67	265,673.00	650.00	941.67	300,000.00
Bonus	126.60	773.06	6,400.00	477.50	1,246.64	33,642.50
Options (B-S Value)	437.39	964.89	2,063.34	436.75	1,084.32	2,309.96
Age	50.00	56.00	60.00	50.00	55.00	59.00

Table A.2

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 firms stock price with a fixed number and fixed value plan.

		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

Note: The annual standard devation is assumed to be 32 percent, the risk-free rate is 6 percent, the dividend rate is 3 percent and the maturity is 10 years

Table A.3
OLS Endogenous Correlations

This table shows the OLS results of regressing Δ Log B-S Value on various outcomes. The sample is limited to executives on fixed number or fixed value plans. Panel A includes firm fixed effects, while the bottom panel includes them.

Panel A: Firm Fixed Effects

	Δ 12M Vol	12M Return	Log Total Inv	Δ Lev Ratio	Δ Log Dividends
	(1)	(2)	(3)	(4)	(5)
Δ Log B-S Value	0.00659***	-0.0359***	0.0400***	-0.00777***	0.0384***
	[2.64]	[-5.01]	[4.16]	[-4.35]	[5.29]
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	41168	41386	33902	34127	18574
R^2	0.262	0.253	0.855	0.119	0.263

Panel B: No Firm Fixed Effects

	Δ 12M Vol	12M Return	Log Total Inv	Δ Lev Ratio	Δ Log Dividends
	(1)	(2)	(3)	(4)	(5)
Δ Log B-S Value	0.00535**	-0.0113*	0.126***	-0.00928***	0.0387***
	[2.27]	[-1.65]	[6.54]	[-5.49]	[5.15]
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No
N	41168	41386	33902	34127	18574
R^2	0.217	0.132	0.0197	0.0198	0.0439

Table A.4

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. 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	FV to FN	FN to Non-Cycle	FN to FV
	(1)	(2)	(3)	(4)
Firm Return	0.00530	-0.00212	0.00950	0.00702
	[0.54]	[-1.02]	[0.73]	[1.46]
Industry Return	-0.00438	0.00537	-0.00642	0.00698
	[-0.20]	[0.95]	[-0.22]	[0.57]
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N	22842	22842	12113	12113
R^2	-0.00244	0.0553	0.0394	0.0312

Panel B: Adjustment of Other Compensation for Fixed Value Executives

	Δ Log (Salary + Bonus)	Δ Log Non-Option Comp	Turnover Next Year
	(1)	(2)	(3)
FV x Ind Return	0.0401	0.0349	
	[1.58]	[0.85]	
Predicted Last Year			0.000251
			[1.10]
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
N	23236	23486	32351
R^2	0.191	0.0809	0.237