NBER WORKING PAPER SERIES

HOW DO QUASI-RANDOM OPTION GRANTS AFFECT CEO RISK-TAKING?

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Working Paper 23091
http://www.nber.org/papers/w23091

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
January 2017

Kelly Shue is with the University of Chicago, Booth School of Business and NBER; Richard Townsend is with the Univseriity of Califonia San Diego, Rady School of Management. We are grateful to Michael Roberts (the editor), the associate editor, two anonymous referees, Marianne Bertrand, Ing-Haw Cheng, Ken French, Ed Glaeser, Todd Gormley, Ben Iverson (discussant), Steve Kaplan, Jonathan Lewellen, Katharina Lewellen, Borja Larrain (discussant), David Matsa (discussant), David Metzger (discussant), Toby Moskowitz, Enrichetta Ravina (discussant), Canice Prendergast, Amit Seru, and Wei Wang (discussant) for helpful suggestions. We thank seminar participants at the AFA, BYU, CICF Conference, Depaul, Duke, Gerzensee ESSFM, Harvard, HKUST Finance Symposium, McGill Todai Conference, Finance UC Chile, Helsinki, IDC Herzliya Finance Conference, NBER Corporate Finance and Personnel Meetings, SEC, Simon Fraser University, Stanford, Stockholm School of Economics, University of Amsterdam, UC Berkeley, UCLA, and Wharton for helpful comments. We thank David Yermack for his generosity in sharing data. We thank Matt Turner at Pearl Meyer, Don Delves at the Delves Group, and Stephen O'Byrne at Shareholder Value Advisors for helping us understand the intricacies of executive stock option plans. Menaka Hampole provided excellent research assistance. We acknowledge financial support from the Initiative on Global Markets. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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January 2017
JEL No. G3,J3,M12,M52

ABSTRACT

We examine how an increase in stock option grants affects CEO risk-taking. The overall net effect of option grants is theoretically ambiguous for risk-averse CEOs. To overcome the endogeneity of option grants, we exploit institutional features of multi-year compensation plans, which generate two distinct types of variation in the timing of when large increases in new at-the-money options are granted. We find that, given average grant levels during our sample period, a 10 percent increase in new options granted leads to a 2.8–4.2 percent increase in equity volatility. This increase in risk is driven largely by increased leverage.

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Performance-sensitive pay for executives has surged over the last 30 years. Much of this surge has been in the form of stock options, which became the largest component of executive compensation in the 1990s, accounting for approximately 50% of the total compensation of S&P 500 CEOs by the end of the decade. Following changes in the accounting treatment of options in 2005, the use of options declined. Still, options remain one of the major components of CEO pay, accounting for over 20% of total pay (Murphy, 2013). Moreover, Bettis et al. (2012) find that many firms substituted from option grants toward performance-vesting stock grants, which have option-like payoffs. Similar to an option, a performance vesting share provides zero payoff below some performance threshold and increasing payoffs above the threshold. As of 2008 nearly 40% of equity awards had performance vesting rather than simple time vesting. Given the prevalence of options and option-like compensation, it is important to understand if and how these forms of pay affect CEO decision-making. In particular, there is a long-standing and important question dating back to Jensen and Meckling (1976) of whether options influence CEO risk-taking behavior.

The idea that stock options create incentives for risk-taking is rooted in the convexity of their payoffs: if the underlying stock price rises above the strike price, the option holder earns the difference, but if the stock price drops below the strike price, the option holder does not lose the difference. However, in addition to this "convexity effect," Ross (2004) shows that there is also a countervailing "magnification effect" associated with options. The magnification effect is driven by the fact that options increase the sensitivity of an executive's wealth to the underlying stock price, which may lead a risk-averse executive to want to decrease risk. In practice, it is also possible that options have no effect on risk-taking if executives are sufficiently well monitored or if they are able to hedge their option holdings (Garvey and Milbourn, 2003). Thus, the overall effect of option compensation on risk-taking is ultimately an empirical question.[1]

Estimating the effect of options on risk-taking is difficult due to endogeneity issues. The main challenge is that a third omitted factor could affect both options and risk-taking. For example, if value-maximizing firms believe that options increase risk-taking, they may increase option pay exactly when the benefits to risk-taking are greater (i.e., when there are more risky positive NPV projects). In this case, risk-taking may increase due to option pay or because the CEO is responding to the firm's need for more risk-taking. While much of the previous research on options and risk-taking has been correlational in nature, two recent studies by Chava and Purnanandam (2010) and Hayes et al. (2012) attempt to address these endogeneity issues by examining how executive risk-taking changed when option use declined following the change in the accounting treatment of options in 2005. However, firms that decreased option compensation after the reform also tended to increase stock compensation at the same time. As a result, this experiment is not ideal for isolating the total net effect of options—for risk-averse managers, stock compensation affects risk-taking incentives as well. Further, Bettis et al. (2012) point out that the confounding effect of the increase in stock compensation is exacerbated by the fact that much of the stock compensation was in the form of performance-vesting shares, which have option-like convex payoffs. Finally, the regulatory change affected all firms simultaneously, meaning there is no control group available to estimate the counterfactual change in risk-taking that would have occurred over the same time period absent the reform.[2]

To identify a causal effect of options on risk-taking, the ideal test would utilize exogenous variation in option pay that is staggered across firms over time. In this paper, we exploit a natural experiment that delivers such variation. Our identification strategy builds on Hall's (1999) observation that firms often award options according to multi-year plans. Two types of plans are commonly used: fixed-number and fixed-value.[3] On a fixed-number plan, an executive receives the same number of options each year within a cycle. On a fixed-value plan, an executive receives the same value of options each year within a cycle. Cycles are generally short, lasting about two years, after which a new cycle typically begins.

Multi-year plans give us two distinct sources of variation in the timing of option pay increases. Our first instrumental variables strategy uses only CEOs on fixed-value plans. For these executives, option compensation tends to follow an increasing step function. During a fixed-value cycle, the value of options granted is held constant. At the beginning of a new cycle, there is a discrete increase in the value of option grants, on average. The timing of when these steps occur is staggered across firms. These staggered steps motivate our first instrument: an indicator variable for whether each CEO-year is *predicted* to be the first year of a new fixed-value cycle. We use predicted cycle first years instead of *actual* cycle first years as our instrument because the timing of when new cycles actually begin may be endogenously renegotiated between the manager and the board. For example, a manager may negotiate to prematurely start a new cycle for some unobserved reason that also directly relates to the firm's risk. Instead, we use a predicted first year indicator, which corresponds to when new cycles would likely have started if renegotiation had not taken place. Our predictions exploit the fact that firms tend to use repeated cycles of equal length. We use the length of a manager's previous cycle to predict when the next cycle will begin. Thus, predictions are based only on past information. For example, if a manager had cycles starting in 1990 and 1992, we would predict that a new cycle would start in 1994. Assuming that firms do not set the length of the current cycle in anticipation of risk-taking conditions at the start of future cycles, the predicted first year instrument should purge the estimation of bias from renegotiation.

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

Our conversations with leading compensation consultants suggest that multi-year plans are used to minimize contracting costs, as option compensation only has to be set once every few years. Hall

(1999) argues that firms sort into the two types of plans somewhat arbitrarily, saying, "Boards seem to substitute one plan for another without much analysis or understanding of their differences." Consistent with this, Shue and Townsend (2017) find suggestive evidence that boards granted fixed-number options due to contracting frictions and lack of sophistication regarding option valuation, rather than because such plans implemented an optimal contract. Nonetheless, we do not assume here that firms choose randomly between plans. We also do not assume that the average level of options paid during a multi-year plan cycle is exogenous. Among firms using multi-year plans, it remains true that those with high option compensation may be those where risk was going to be high (or low) in any case, for reasons unrelated to option compensation. Moreover, even if some boards choose multi-year plans in an unsophisticated way, this does not imply that all panel variation in option compensation is exogenous to risk-taking. Instead, what we argue is that multi-year plans generate random variation in the *timing* of when large compensation increases occur.

For our first instrument, we use fixed-value firms, for which option grants can increase only at regularly pre-scheduled intervals (when new cycles start). For example, consider a fixed-value firm on regular three-year cycles. Other time-varying factors may drive trends in risk for this firm. However, these trends are unlikely to coincide exactly with the timing of when new cycles are scheduled to start. If we observe that the risk of this firm increases significantly more at the start of a new cycle than in other years, we attribute these increases in risk to the coinciding increases in option compensation.[4]

For our second instrument, we focus on fixed-number firms. The value of options granted in any particular year varies with aggregate returns within a fixed-number cycle. This means that the timing of increases in option pay within a cycle will be random in the sense that the increases are partly driven by industry shocks that are beyond the control of the firm and are largely unpredictable. To account for the possibility that aggregate returns can directly affect risk, we use fixed-value firms as a control group because their option compensation must remain fixed despite changes in aggregate returns. Thus, our identifying assumption is that fixed-number and fixed-value firms do not differ in their response to aggregate returns for reasons other than the differential sensitivity of their option compensation. Fixed-number firms may systematically differ from fixed-value firms, but we assume they do not differ in how their non-compensation-related risk-taking moves with aggregate returns. We perform a number of tests in the paper that support this assumption.

Our two instrumental variables strategies yield similar results across a range of firm outcomes. Given that the two strategies rely on completely different identifying assumptions, they help cross-validate one another. Overall, we find a significant positive effect of option compensation on risk-taking, as measured by realized equity volatility. Given average grant levels during our sample period, a 10 percent increase in new options granted leads to a 2.8–4.2 percent increase in volatility. We find that the increase in risk is driven largely by increases in leverage, which mechanically increases the volatility of equity. In addition, we find that option pay has a modest and generally insignificant effect on investment. In supplementary tests, we also find that the effect of new option grants on volatility is greater in subsamples where the value of new option grants is high relative to the total value of the unexercised options held by the CEO. Finally, we find suggestive evidence that the effect of options on risk-taking is greater for firms in the financial and high-tech sectors, where executives may have greater ability to affect risk beyond changing leverage.

Our paper contributes to the option compensation literature by addressing the endogeneity of option grants through a natural experiment that offers exogenous variation in option pay that is staggered across firms over time. Relative to the existing literature, we also focus on a somewhat different research question. The bulk of the literature has focused on estimating the association between the "vega" of a CEO's personal portfolio and subsequent risk-taking behavior, where vega is defined as the sensitivity of the value of the CEO's option holdings to changes in the firm's equity volatility. Most of these studies also control for the "delta" of a CEO's personal portfolio, where delta is defined as the sensitivity of the value of the CEO's stock and option holdings to changes in the firm's stock price. Cohen et al. (2000), Coles et al. (2006), Chava and Purnanandam (2010), Liu and Mauer (2011), Armstrong and Vashishtha (2012), and Hayes et al. (2012) all examine the association between vega and risk-taking, mostly finding a positive relation. However, the effect of vega on risk-taking is theoretically unambiguous, as vega is a measure of convexity. Using Ross's terminology, increases in vega should increase the convexity effect, leading to weakly more risk-taking, all else equal. Thus, the existing empirical literature has focused on quantifying the magnitude of the convexity effect.

Our paper focuses instead on estimating the theoretically ambiguous overall net effect of option compensation, which includes the combination of the convexity effect (operating through vega) and the magnification effect (operating through delta). Almost all firms in our sample grant options that are at-the-money. When "more" at-the-money options are granted, the Black–Scholes value, delta, and vega of option compensation all increase simultaneously. We investigate how such a simultaneous increase in these three dimensions of option compensation affects risk-taking. Knowing the total net effect of increases in at-the-money option grants on risk-taking is important, as most compensation committees and policymakers do not contemplate awarding option compensation in such a way that only delta or vega is changed in isolation. Rather, the relevant decision is typically whether to grant an executive more or less at-the-money options.[5]

The rest of the paper proceeds as follows. Section I discusses the data and construction of key variables. Section II discusses our two instrumental variables strategies in more detail. Section III presents our results. Section IV concludes.

I. Data

A. Sources

We use data from Execucomp from 1992 to 2010, which covers executive compensation for the CEO and other top executives in S&P 1500 firms. We limit attention only to CEOs in our analysis. For option compensation, we use individual grant-level data, which allows us to better identify CEOs on fixed-number and fixed-value plans in cases where a CEO receives multiple grants in a fiscal year. We also require data on the date of each option grant to measure aggregate returns between consecutive grants and equity volatility following a grant. Exact grant date information is available after 2006. Prior to 2006, firms were only required to report the expiration date of an option grant. In those years, we follow the literature (e.g., Aboody and Kasznik, 2000) and infer the grant date from the expiration date under the assumption that expiration dates occur on grant date anniversaries.

In 2006, firms were required to begin reporting the grant date value of their option compensation, which we use in our analysis. For years prior to 2006, we use Execucomp's computed Black–Scholes value. In 2006, firms were also required to begin reporting detailed information on the portfolio of unexercised options held by executives at the end of each fiscal year. We use this information to compute the value, delta, and vega of unexercised options held by each executive. Prior to 2006, we follow the procedure of Core and Guay (2002) to estimate these values.

Accounting data come from Compustat. Following standard practice, financial firms (6000-6999) and regulated utilities (4800-4999) are excluded from the sample when accounting-based outcomes are used. However, these firms are included in the sample when non-accounting-based outcomes such as volatility are used. Industry and firm return data come from the Center for Research in Security Prices (CRSP) and the Fama–French Data Library.

B. Detecting Cycles

Firms are not required to disclose intended schedules for multi-year compensation cycles, and therefore, few report them. Following Hall (1999), we instead back out these cycles using the data. Ideally, we would use the firm's planned cycle structure in our IV analysis. Inferring the cycle structure from realized option grants necessarily introduces measurement error. In particular, we infer planned cycles with error if the firm did not intend to adopt a multi-year plan but awarded the same number or value of options across consecutive years for potentially endogenous reasons. We will also infer planned cycles with error if the firm departs from a pre-planned schedule for potentially endogenous reasons. As will be discussed in later sections, our methodology is robust to both of these types of errors. In our IV framework, measurement error will reduce the precision of our estimates but not lead to bias.

B.1. Fixed-Number

We infer an executive to be on a fixed-number cycle in two consecutive years if the executive receives the exact same number of options in both years, adjusting for stock splits. When executives receive multiple grants per year we only compare the largest grant. This is done because an executive may receive one grant as part of a long-term incentive plan that is common among all executives in the firm as well as another grant that is part of a fixed-number plan. To ensure that the fixed-number grants are significant relative to other option grants, we require that the number of options in the fixed-number grants constitute more than 50 percent of the total number of options granted over the years of the cycle. Our results are not sensitive to these assumptions. In most cases, executives receive a single option grant and limiting our analysis to this subsample yields qualitatively similar results.

B.2. Fixed-Value

We consider an executive to be on a fixed-value cycle in two consecutive years if the value of the options the executive receives is within three percent of the previous year. Value is computed as either Black–Scholes value or "face value" (i.e., the number of options granted multiplied by the grant-date price of the underlying stock). While face value has little theoretical relation to the value of an option grant, it is common for fixed-value firms to target face value during our sample period.[6] Naturally, we require that a fixed-value cycle be defined using the same valuation method (Black–Scholes value or face value) in all years. We allow for a three percent tolerance because firms often grant options in round lots, and so value is not exactly fixed, even by their own internal valuation methodology. In addition, the Black–Scholes value we use may be computed using somewhat different assumptions, regarding volatility and other parameters, than those used by the firm. Again, if multiple grants are awarded per year, then the largest grants are compared and can form the basis of a fixed-value cycle as long as they are significant relative to other options granted, using the same criteria as before.

C. *Measuring Risk*

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

D. *Summary Statistics*

Panel A of Table I shows the distribution of cycle length by plan type. The modal cycle length is two years for both fixed-number and fixed-value plans.[7] Conversations with compensation consultants indicate that two-year cycles are indeed common.

Next, we explore the extent to which firms that use fixed-number, fixed-value, or neither plan differ in their observable characteristics. Because there are likely to be time trends in these variables and the relative prevalence of the two types of plans has changed over time, we examine three cross-sections of the data rather than pool all years together. Table I presents the year 2000, while 1995 and 2005 are presented in the Internet Appendix. Panel B of Table I shows the industry distribution for firm-years, categorized by the CEO's plan type. We find that multi-year cycles are distributed across many industries and that the industry distribution is approximately similar across plan types, with fixed-number cycles being more prevalent in the business equipment and health industries. Panel C of Table I compares other firm and CEO characteristics across plan types. In general, fixed-number and fixed-value firms appear similar in terms of market-to-book, volatility, investment, leverage, and profitability. In terms of assets and sales, fixed-value firms tend to be larger than fixed-number firms, which are in turn larger than firms using neither type of plan. Overall, we find that firms do not differ sharply across the three categories, consistent with Hall's claim that firms sort approximately randomly into these plans. Nevertheless, as will be discussed in Section II, our analysis will never assume that firms choose randomly between fixed-number and fixed-value plans.

Over our sample period, we find that multi-year plans are quite prevalent. Specifically, 18 percent of CEO-years with option compensation are associated with a fixed-number plan and 11.5 percent are associated with a fixed-value plan. Moreover, these numbers are likely low because we are fairly conservative in how we define these plans. For example, rather than holding the raw value of option grants fixed within a fixed-value cycle, firms often hold value fixed as a proportion of salary or salary plus bonus. If we included these types of cycles, we would find a greater prevalence of multi-year plans.[8]

II. Empirical Strategy

We introduce two instruments that provide exogenous variation in the timing of when large increases in the amount of new at-the-money options are granted. We begin by noting that the Black–Scholes value, delta (the change in the Black–Scholes value of a grant associated with a one percent change in the underlying), and vega (the change in the Black–Scholes value of a grant associated with a 0.01 unit change in the volatility of the underlying) of new at-the-money option grants are all highly correlated and affected by our instruments. An exogenous increase in new option grants implies that all three values increase together. We do not attempt to identify the effect of each of these on risk-taking, holding the others constant. Instead, we measure the overall effect of an increase in option pay, when delta and vega increase simultaneously. As discussed by Ross (2004), this overall effect is theoretically ambiguous, as it includes the countervailing convexity and magnification effects of options. Moreover, the overall effect of options is important because most compensation committees and policymakers do not contemplate awarding option compensation in such a way that only delta or vega is changed in isolation. Rather, the relevant decision is typically whether to grant an executive more or less at-the-money options.

Therefore, we instrument for Black–Scholes value in our two-stage least squares estimates because this is a simple summary measure of the magnitude of a grant. However, instrumenting for delta or vega yields similar results. To emphasize this point, we also present reduced-form estimates of our outcomes regressed directly on our excluded instruments and controls, with the understanding that the coefficient on the excluded instrument represents a general effect of higher option value and associated higher delta and vega.[9]

A. *Instrumental Variables Strategy 1*

Our first instrumental variables strategy uses only observations corresponding to fixed-value plans. Thus, it is not subject to the concern that fixed-value firms may be different from fixed-number firms due to the fact that plans are endogenously chosen. Instead, we use the staggered timing of predicted increases in option grants within the fixed-value sample to estimate the effect of options on risk-taking. Thus, we are able to estimate a causal effect within the fixed-value sample.

To help fix ideas, Figure 1 illustrates three real examples of fixed-value cycles taken from the data. From these examples, two patterns emerge that are true more generally. First, option compensation tends to follow an increasing step function for executives on fixed-value plans. This is because compensation tends to drift upward over time, yet executives on fixed-value plans cannot experience an upward drift within a cycle. As a result, they experience a discrete increase, on average, in the year following the completion of a cycle. Second, executives tend to have repeated cycles of equal length that are staggered across executives. For example, the executive in Panel A completes cycles in 2006, 2008, and 2010, while the executive in Panel B completes cycles in 2007, 2009, and

2011. While these two stylized facts do not hold in all cases—as can also be seen in Figure 1—our identification strategy only requires that they hold true on average.

Panel A of Table II confirms that the increasing step function pattern holds true on average. We regress the *change* in log option compensation on an indicator variable equal to one in the first year following the end of a fixed-value cycle. The first year indicator is equal to one for any first year following a completed cycle, even if that observation does not represent the start of a new cycle. This is because option pay tends to jump substantially after being fixed for two or more years, even if the firm chooses to discontinue fixed-value plans in the future. Accordingly, the sample is limited to years that are part of fixed-value cycles as well as years that immediately follow a completed fixed-value cycle.

Because the first year indicator is staggered across firms and CEOs, we can control for year fixed effects in these and all future regressions. We also control for time-varying firm characteristics measured in the year prior to the current option grant: CEO tenure, log of cash compensation (salary + bonus), log sales, log assets, sales growth, market-to-book, tangibility ratio, and a dummy variable for whether the firm has rated debt. To account for changes in the incentives from previously granted equity compensation, we control for the change in the log delta and vega of the CEO's previously granted (unexercised) option and stock holdings.

We find that the first year indicator corresponds to a 15 percent larger increase in the Black–Scholes value of new option grants than in other years. Consistent with the fact that the Black–Scholes value, delta, and vega of new at-the-money options move together, we find that the first year indicator is also associated with a 16–18 percent larger increase in the delta and vega of new option compensation.

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

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

We use the following simple prediction algorithm. To determine whether an observation for CEO i in year $t+1$ is predicted to be the first year of a new fixed-value cycle, we only use data for the CEO from years t and earlier. 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 I. At the start of year t, let n_t be the number of consecutive years, inclusive, in which the executive received the same value of options (within the aforementioned tolerance of three percent). We predict that year $t+1$ will be a first year if $n_t \geq k$. Finally, we also exclude the first year of each executive's tenure from the analysis because those years are likely to be special in other ways besides being the first year of a new cycle (Pan, Wang, and Weisbach, 2013).

To illustrate how this works in practice, the dotted vertical lines in Figure 1 indicate years that we predict to be cycle first years. Panels A and B both show three cycles of length 2. In these cases, we correctly predict all of the cycle first years (e.g., for Panel A, these occur in 2006, 2008, and 2010). The example in Panel C shows a cycle of length 2 followed by two cycles of length 3. In this case, we correctly predict a cycle first year in 2000, incorrectly predict a first year in 2002 due to the change in cycle length, and then correctly predict a first year in 2003 and 2006. Incorrect predictions reduce the power of the first stage of our IV estimation, but do not bias our results. In fact, they purge the instrument of potential bias arising from endogenous renegotiation.

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

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

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

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

where i indexes CEOs, j indexes firms, and t indexes years. The variable $I_{ijt}^{PredictedFirstYear}$ is the indicator for predicted first year.

The variable $\triangle O_{ijt}$ is a measure of the change in value of the option grant, and $\triangle Y_{ijt}$ are the outcome variables measured as annual changes for stock variables and levels for flow variables. We measure options and firm outcomes in terms of annual changes because our instrument delivers exogenously timed increases in option grants. As such, we do not necessarily expect risk-taking to be higher in the first year of a fixed-value cycle than in subsequent years, but we do expect a larger change in risk-taking in the first year of a new cycle than in continuation years. Because we estimate our regressions in terms of annual changes, we do not include firm fixed effects. However, we do control for year fixed effects, represented by γ_t, and allow standard errors to be clustered by firm.

The main coefficient of interest, δ_1, represents the effect of an increase in options on outcomes

$\triangle Y_{ijt}$.[10] Importantly, in the second stage, we do not regress firm outcomes on the actual change in option compensation that a CEO experienced at the start of a new cycle, as the size of that change may be related to unobservables that affect risk-taking. Instead, we use the fact that the indicator for predicted first year corresponds to increases in option pay *on average* and is staggered across firms. Our analysis essentially compares average changes in risk-taking in years when the indicator is equal to one to years in which the indicator is equal to zero.

One might be concerned that predicted first years provide exogenously timed but potentially *anticipated* increases in option compensation. However, this is not an issue for our empirical strategy. To see this, first suppose that a manager could change risk instantaneously. He would have no incentive to increase risk prior to an anticipated increase in the value of his option compensation next period. The anticipated option grant would not provide any extra incentive to increase risk today. However, if a manager could only adjust risk slowly, he might wish to begin doing so prior to receiving the increase in options. Yet, if anything, this would bias us against finding larger increases in risk during predicted first years than in other years.[11]

B. Instrumental Variables Strategy 2

Our second instrumental variables strategy uses only observations corresponding to fixed-value and fixed-number plans. Specifically, we exploit differences in the way that option compensation moves *within* a cycle for executives on these two types of plans. The value of new option grants remains approximately fixed within a cycle for executives on fixed-value plans. In contrast, the value, delta, and vega 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.

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

Black–Scholes formula reduces to:

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

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

This is illustrated via an example in Table III, adapted from Hall (1999). The example shows how option compensation would evolve for an executive at the same firm if he were on a fixed-value or fixed-number plan. The executive is paid 28,128 options valued at $1 million under both plans in Year 1. The firm's stock price then increases by 20 percent in each of the next two years. Under a fixed-value plan, the firm grants the executive fewer options each year to keep the value of those options constant at $1 million. Under a fixed-number plan, the firm continues to grant the executive 28,128 options each year, and as a result, the value of those options increases by 20 percent each year along with the stock price. This illustrates how the value of new grants is more sensitive to stock price movements for executives on fixed-number plans than for executives on fixed-value plans.

Of course, movements in each firm's stock price are partially driven by industry shocks. These shocks are beyond a CEO's control and are also difficult to predict, even by sophisticated agents.[12] Thus, our second instrument for changes in option compensation is the interaction between plan type and industry returns.

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

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

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

where I^{FN}_{ijt} is an indicator equal to one if the executive is on a fixed-number plan, and R_{kt} is the Fama–French (49) industry return over the 12 months prior to the grant date. The interaction term, $I^{FN}_{ijt} R_{kt}$, is the excluded instrument. The coefficient, δ_3, is the effect of an increase in new option grants on our outcome of interest, $\triangle Y_{ijt}$, measured again as annual changes for stock variables and levels for flow variables.

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

The exclusion restriction instead requires that the interaction term, $I^{FN}_{ijt} R_{kt}$, only relates to risk-taking, $\triangle Y_{ijt}$, through its effect on compensation. In other words, we assume that fixed-value and fixed-number executives do not have different non-compensation induced responses to changes in aggregate returns. We examine whether there is support for this assumption in the data through a number of additional tests, which are presented in Section III.B. In addition, our first instrumental variables strategy does not require this assumption.

Finally, the sample is restricted to CEOs on fixed-number or fixed-value cycles, as we wish our identification to be based on the comparison of executives whose compensation is mechanically sensitive to industry returns with those whose compensation is mechanically insensitive to industry returns. We also exclude observations corresponding to the first years of cycles because our first stage outcome is the annual change in option compensation. In the first year of a new cycle, the change in option compensation relative to the previous year is not necessarily more sensitive to returns for fixed-number executives—in the first year, fixed-number (value) executives do not receive the same number (value) of options as in the previous year, while in later years they do. Another consequence of restricting our second IV sample to cycle continuation years (excluding the first year) is that we are identifying off of variation induced by industry returns *within* cycles, so this second methodology is robust to the potential concern that the predicted start of cycles may be correlated with other unobserved cycles of activity within the firm.

III. Results

A. Instrumental Variables Strategy 1

We begin by using the predicted first year indicator as an instrument for changes in option grants. We use predicted first years rather than actual first years to purge the estimation of bias from endogenous renegotiation. In unreported results, we find that the predicted first year indicator indeed strongly predicts true fixed-value first years in the data, with a t-statistic exceeding 100.

Panel B of Table II shows that the predicted first year indicator is strongly correlated with changes in the Black–Scholes value, delta, and vega of new options granted. Predicted first years corresponds to an approximate 15 percent increase in the Black–Scholes value, delta, and vega of new at-the-money options granted.

The results remain very similar in the even-numbered columns in which we add control variables for time-varying CEO and firm characteristics measured in the year prior to the grant: CEO tenure, log of cash compensation (salary + bonus), log sales, log assets, sales growth, market-to-book, tangibility ratio, and a dummy variable for whether the firm has rated debt. To account for changes in the incentives tied to the CEO's previous equity-related grants, we also control for the change in the log delta and vega of previously granted (unexercised) option and stock holdings. This same set of control variables is included in all future tables. All estimates are highly significant, with F-statistics greatly exceeding 10, the rule of thumb threshold for concerns relating to weak instruments (Staiger and Stock, 1997).

In Table IV, we use our first instrument to explore the effect of an increase in option pay on the annual change in equity volatility, our primary measure of risk-taking. We measure volatility in two ways: the volatility of daily returns in the 12 months following the grant date and the volatility of daily returns in the first 120 trading days following the grant date. The latter measure is designed to be less sensitive to potential manipulation of volatility immediately prior to a fixed-value option grant. Both measures are annualized.

The top panel presents the IV estimates from regressing the change in volatility on the change in the log Black–Scholes value of new option grants, as instrumented by the predicted first year indicator. The bottom panel presents the reduced-form estimates from regressing the change in volatility directly on the instrument and other controls. With and without additional control variables, we find that an increase in options leads to an increase in equity volatility. The results in Column 4 imply that a 10 percent increase in new options corresponds to approximately a 0.011 unit increase in equity volatility relative to the median of 0.29 for our sample period, or a 3.8 percent increase in volatility.

It is further reassuring that our main coefficient of interest remains very similar in magnitude once we control for time-varying CEO and firm characteristics in the even-numbered columns (control variables are included in both the first and second stages of all IV specifications). If our instruments are valid, they should be orthogonal to time-varying firm and CEO characteristics, leading to similar main coefficients after the inclusion of additional control variables.

Next, we explore possible channels that may drive this change in equity volatility. One prime candidate is leverage. Basic capital structure theory implies that, holding the assets and real activity of the firm constant, an increase in leverage will mechanically lead to an increase in equity volatility. Columns 1–3 of Table V show that an increase in option compensation does indeed lead to significant increases in leverage. Specifically, Column 1 implies that a 10 percent increase in the value of new options granted corresponds to a 0.010 unit increase in the market leverage ratio (total debt scaled by total market value of assets). One issue with using market leverage ratios, however, is that these ratios may reflect changes in the market price of equity rather than active debt management. To address this possibility, we use the simple change in log total book leverage in Column 2 and an indicator for positive net debt issuance in Column 3. The results imply that a 10 percent increase in the value of new options granted corresponds to a 1.3 percent increase in book leverage and a 3.5 percent greater probability of positive net issuance. Thus, the increase in leverage appears to be due to active debt management.

We can also estimate the proportion of the increase in equity volatility that can be explained by the increase in leverage. In unreported results, we find that a 10 percent increase in the value of new options is associated with a statistically significant 1.75 percent decline in the equity to assets ratio, which in turn implies approximately a 1.75 percent increase in equity volatility.[13] Thus, the increase in leverage accounts for nearly half ($1.75/3.8 = 46\%$) of the increase in volatility.

Next, we explore the effect of options on investment. These tests should be viewed as exploratory because it is not clear how an increase in investment affects firm risk. While it may seem intuitive that investment increases risk, some argue that certain forms of investment, such as capital expenditures, decrease risk (Coles et al., 2006). Therefore, we examine the effect of option compensation on investment, leaving open the question of whether this contributes to the increase in volatility. In Columns 4–5 of Table 4, we find that greater option compensation corresponds to lower capital expenditures but higher total investment (defined as the sum of capital expenditures, R&D, acquisitions, and advertising expenses), although neither estimate is significantly different from zero. In unreported tests, we also explore how options affect R&D, diversifying acquisitions, and non-diversifying acquisitions separately and find positive, albeit noisily estimated, effects.

We also perform further tests to ensure the validity of our first instrument. A potential concern with this instrument is that predicted first years may tend to coincide with other cycles within firms. For example, firms may set multi-year plans to expire right before the introduction of new products. Alternatively, they may time major performance reviews to occur in the last year of a multi-year plan, leading to decreased risk of CEO turnover during predicted first years. In such cases, increases in risk may coincide with predicted first years for reasons other than the increases in option compensation that tend to occur during these years.

In order to examine whether the increases in risk are tied to the increases in option compensation, we decompose our instrument into two distinct variables: predicted first years where option pay increases and predicted first years where option pay (weakly) decreases. This test exploits the fact that option grants can both increase or decrease at the start of new fixed-value cycles (although they increase on average). If our results are driven by non-compensation cycles that coincide with fixed-value cycles, we should see increases in risk during both types of predicted first years. On the other hand, if our results are driven by option compensation, we should only see increases in risk during predicted first years in which option pay actually increases. The results are reported in Panel A of Table VI. As can be seen, predicted first years in which option compensation increases are associated with significant increases in risk-taking. In contrast, predicted first years in which option compensation decreases have no significant changes in risk-taking.

In addition, we examine directly whether fixed-value cycles appear to be correlated with other firm cycles that might relate to risk. To examine product cycles, we merge our data with the CapitalIQ key developments database, which provides structured summaries of material news and events that may affect the market value of a firm's securities. Among other things, CapitalIQ tracks announcements related to a firm's products. This allows us to explore whether predicted first years tend to be ones in which major product announcements occur. The results are reported in the first column of Panel B in Table VI. We find no significant change in the number of product announcements that occur during predicted first years. In unreported results, we also find no change in the probability of any product announcement. Similarly, in Column 2, we find no significant change in the number (or probability) of business expansion events. Thus, it does not seem that fixed-value cycles are correlated with product or expansion cycles. We also look at whether predicted first years tend to coincide with debt maturity cycles. Were this the case, it might explain our finding that leverage increases more during predicted first years than in other years. However, in Column 3, we find no significant change in the percent of debt that is maturing in predicted first years relative to other years for fixed-value firms.[14] Finally, in Column 4 we also examine whether predicted first years tend to be years with lower CEO turnover. One might expect this to be the case if major performance reviews tend to occur in the last year of a cycle. However, we again find no significant difference. This last finding is also consistent with our conversations with compensation consultants suggesting that performance reviews are typically performed annually instead of at cycle termination.[15]

B. 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 II.B, the excluded instrument is the interaction between the fixed-number indicator and industry returns. In Table VII, we show that the instrument significantly predicts changes in the Black–Scholes value, delta, and vega of new option grants. For a one standard deviation change in the industry return, CEOs on fixed-number plans receive an additional 14 percent increase in the value of option grants relative to CEOs on fixed-value plans. Again, we instrument for changes in Black–Scholes value in the remainder of our analysis. Thus, our estimates represent the overall net effect of higher option pay, which includes the convexity effect (which operates through vega) and the magnification effect (which operates through delta).

As with our first instrument, we begin by exploring the effect of an increase in option pay on changes in volatility. We again find that an increase in the value of new option grants leads to an increase in equity volatility. The results in Columns 2 and 4, which include the full set of control variables, imply that a 10 percent increase in the value of new options granted leads to a 0.0086–0.0144 increase in equity volatility, or a 2.79–4.24 percent increase relative to median volatility in the sample.

We again find that a major mechanism driving the change in volatility is an increase in firm leverage. Columns 1–3 of Table IX show that a 10 percent increase in the value of new options granted leads to a .0072 unit increase in the market leverage ratio, a 1.3 percent increase in total in book leverage, and a 3.1 percent increase in the probability of positive net issuance. We estimate that 45 percent of the increase in equity volatility is due to increased leverage. Thus, the results once again suggest that leverage is actively increased in response to increases in option compensation. In

Columns 4 and 5, we also explore the effect of changes in option compensation on investment. We find that an increase in options leads to greater capital expenditures and total investment. However, as with our first instrument, the coefficients are noisily estimated and insignificant.

Our second instrumental variables strategy requires the assumption that fixed-number and fixed-value CEOs do not have differential non-compensation-related responses to industry returns. If this assumption holds, then the differential sensitivity of firm outcomes to industry returns for fixed-number firms must be due to the differential sensitivity of their option compensation. Note that our first IV strategy already offers a validity check, showing that our results are not dependent on this assumption. The first instrument only uses data for fixed-value CEOs and does not require assumptions about how CEOs would react to industry returns in the absence of differences in compensation. Using the first instrument, we estimate similar results across a range of firm outcomes.

To further ensure that the results we find using our second instrument are not driven by differential sensitivity of fixed-number and fixed-value firms to industry returns, we perform several tests. As noted in section II.B, I_{ijt}^{FN} (the fixed-number indicator) and R_{kt} (industry returns) are not excluded instruments, as they appear in the second-stage regression as well. Thus, our identification strategy allows for the possibility that plan-type or aggregate returns directly relate to risk-taking. However, we would also like to account for the possibility that the effect of industry returns on changes in volatility may be non-linear. If this is the case, controlling for R_{kt} linearly will not be sufficient to break the correlation between our instrument and the error term. To address this possibility, we include not only R_{kt} as a control variable but a 5-degree polynomial of R_{kt}. In this case, our excluded instruments become the interaction between this 5 degree polynomial and I_{ijt}^{FN}. The non-excluded instruments include the direct effects of I_{ijt}^{FN} and the 5-degree polynomial of R_{kt}. The results with and without other controls are reported in Columns 1–2 and 4–5 of Table X. As can be seen, our estimates change very little with the inclusion of non-linear controls.

Finally, we allow all of our firm and CEO control variables to interact with the 5-degree polynomial of R_{kt} as well. To the extent that fixed-number and fixed-value firms differ in observable ways that lead to differential sensitivity of their risk to industry returns, controlling for the interaction between these observable characteristics and industry returns should lead our estimated effect to become insignificant. For example, if smaller firms are more likely to be on fixed-number plans than on fixed-value plans and smaller firms have stronger risk responses to industry returns for other reasons, controlling for the interaction between firm size and industry returns should account for this. However, we find little change in our estimated effect when we control for interactions between a large set of firm and CEO characteristics and the industry return polynomial in Columns 3 and 6. Of course, we cannot control for unobservable firm differences. Nevertheless, the stability of our coefficients after the inclusion of many additional observable control variables suggests that unobservable selection is also likely to be low (Oster, 2013). Overall, these results provide evidence that our baseline findings are not driven by non-linearity or observable differences between fixed-number and fixed-value firms.

It remains possible that there are unobservable differences between fixed-number and fixed-value firms that drive our baseline findings. To help rule out this possibility, we perform a placebo test that compares the responses of fixed-number and fixed-value firms to industry returns during years in which their CEO is not on a fixed-number or fixed-value plan. Among other things, this test exploits the fact that multi-year plans grew in popularity in the early 1990s and declined in popularity in the mid 2000s, possibly due to the introduction of peer benchmarking requirements that required firms to justify their level of executive pay annually. We estimate the following regression:

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

restricting the sample to CEOs who are not currently on a cycle but were on a fixed-number or fixed-value cycle in some other year. The variable $I_{ijt}^{FN\,Placebo}$ is an indicator for whether the CEO was on a fixed-number cycle in some other year. A β_3 close to zero would provide evidence that fixed-number and fixed-value CEOs respond similarly to market movements absent compensation effects. Panel A of Table XI shows that, across all the previously examined outcomes, fixed-number and fixed-value CEOs react similarly to changes in industry returns in years in which the CEO is not awarded options according to either type of multi-year plan. It is further reassuring that the point estimates are close to zero with small standard errors, suggesting that β_3 is a well-estimated zero effect.

One may still be concerned that firms discontinue the use of fixed-number plans exactly when they anticipate or desire a decline in the responsiveness of their firm's risk to industry returns. To address this final possibility, we exploit the implementation of FAS123r in 2005, which led many firms to discontinue their use of options altogether. Prior to FAS123r, at-the-money option compensation did not have to be recognized as an expense on a firm's income statement. Such options thus represented a way to compensate employees without affecting current reported earnings. After FAS123r went into effect, firms had to begin expensing the grant date value of at-the-money options using Black–Scholes (or a related model). We identify firms that discontinued the use of options entirely, for all top executives, in the year that FAS123r went into effect (or the year before or after) to be option "discontinuers."[16] For these firms, it is even less likely that they stopped using multi-year plans due to anticipated changes in the responsiveness of their firm's risk to industry returns. Rather, these firms likely stopped using multi-year plans—and options altogether—due to accounting considerations. Accordingly, in Panel B of Table XI, we repeat our placebo analysis, limiting the sample to these option discontinuers in the post-FAS123r period. Now the sample consists of CEOs that were on either a fixed-number or fixed-value plan prior to the discontinuation of options, and the variable $I_{ijt}^{FN\,Placebo}$ is an indicator for whether the CEO was on a fixed-number plan. Again, the interaction between $I_{ijt}^{FN\,Placebo}$ and industry returns is insignificant. Thus, fixed-number and fixed-value CEOs who stopped receiving options due to FAS123r subsequently respond similarly to changes in industry returns. Again, these results suggest that fixed-number and fixed-value firms are not inherently different in their response to industry returns.

C. Heterogeneity

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

We hypothesize based on Ross (2004) that the marginal effect of new option grants on risk-taking may be weaker if the executive already holds a sizable portfolio of unexercised options that were granted in the past. In Panel A of Table XII, we re-estimate our baseline reduced form specifications from Column 4 of Tables IV and VIII, Panel B. Specifically, the change in volatility is regressed on Pred First Year in Columns 1–2 and on FN $\times$ Ind Return in Columns 3–4.[17] For each observation, we calculate the ratio of the value of new options to all unexercised options at the grant date. Using this ratio, we divide the sample into terciles. For both instruments, we find that the effect of new options on risk-taking is 3–5 times greater when new options are a higher fraction of all options held. The p-values in the bottom row show that the differences between the first and third terciles are also statistically significant.

In Panel B, we instead split the sample based on whether the firm belongs to the financial or high-tech sectors. Executives in these sectors may have relatively greater ability to affect risk beyond merely changing leverage. For example, many allege that the recent rise of complicated derivative products combined with deregulation allowed executives in the financial sector to be particularly sensitive to risk-taking incentives. Similarly, the high-tech sector is characterized by high rates of innovation and high information asymmetry, which may allow executives to increase risk by pursuing riskier product development. We test these theories by comparing the effect of options on risk-taking within the finance and high-tech sectors with the effect in other industries. Using both instruments, we find that the effect of options on risk-taking is approximately twice as large in the finance and high-tech sectors than in other sectors. The large differences in magnitudes support the hypothesis that executives in the finance and high-tech sectors respond more strongly to changes in options. However, we caution that these results are only suggestive. After splitting the sample, we lack statistical power to establish significant *differences* between finance/high-tech executives and other executives (p-values for the test of differences between coefficients are in the range of 0.3).

D. Discussion and Robustness

Our analysis shows that, *all else equal*, an increase in option compensation leads to an increase in firm volatility that is driven in part by increases in leverage. However, aggregate equity volatility and leverage among large public firms remained approximately stable over our sample period. In light of these aggregate trends, our results suggest that other factors are needed to explain broad changes in volatility and leverage. For example, it is possible that other factors pushed executives to reduce risk, but the rise in options dampened their response.

We also note that, so far, we have explored the overall net effect of an increase in options on executive behavior. Options can affect behavior through various channels. One such channel, which we have not focused on much, is the wealth effect. An increase in options, like an increase in cash compensation, increases a CEO's wealth, which in turn could lead to an increase in risk tolerance (Becker, 2006). Columns 1 and 2 of Panel A of Table XIII present suggestive evidence that the wealth channel does not drive our results. In particular, we control for the change in log total compensation (the sum of the grant date values of salary, bonus, restricted stock, options, and other compensation) and continue to find a strong effect of options on risk-taking behavior. Of course, current total compensation may only be a small fraction of a manager's wealth. However, all of our regressions are in terms of changes, and a manager's total compensation is a reasonable proxy for the *change* in his wealth relative to the previous year. Overall, the results suggest that the composition of pay matters. Risk-taking increases when options as a fraction of total pay increase.

Next, we explore the possibility that other types of compensation, such as salary, bonus, and restricted stock, move with our shocks to option grants. In Columns 3 and 4 of Panel A of Table XIII, we find similar results for the effect of options on risk-taking after controlling separately for changes in all other components of compensation: salary, bonus, restricted stock, and any other compensation. In unreported tests, we also find that salary and bonus do not increase with our instruments. Likewise, we find that our instruments do not predict changes in restricted stock. Moreover, when we limit the sample to years in which no restricted stock is awarded in Columns 5 and 6, we find similar results.

Panel B of Table XIII presents additional robustness checks. Columns 1 and 2 show that our results cannot be explained by the potentially endogenous choice of cycle length. In particular, our first set of IV results using the predicted first year instrument are instead driven by the staggering of when first years occur among CEOs on cycles of equal length. We re-estimate our two IV specifications after restricting the sample to CEOs on two-year cycles and find similar results. Next, we show that our results are insensitive to assumptions regarding the treatment of CEOs who receive more than one option grant per year. As noted previously, an executive may receive one grant as part of a firm-wide long-term incentive plan as well as another grant that is part of a fixed-value or number plan. Columns 3 and 4 show that limiting our analysis to the subsample of CEO-years with a single grant yields similar results.

Finally, we show that our results are unlikely to be driven by endogeneity in the exact choice of date for each option grant. Previous evidence on backdating by Lie (2005) has shown that CEOs may have strategically chosen grant dates ex-post to maximize the value of their option grants. However, for backdating to explain our results, not only would firms have to backdate strategically in a way that coincides with increases in volatility, they would have to engage in more backdating when our instruments predict increases in option compensation. For our first instrument, this would mean that backdating would have to coincide with predicted first years and thus occur on a somewhat regular schedule. For our second instrument, this would mean that backdating would have to be more sensitive to industry returns for fixed-number firms than for fixed-value firms.

In Columns 5 and 6, we provide direct empirical evidence that our results are robust to backdating concerns. We substitute grant dates in the data for the current option grant with grant dates based on the previous year's grant. For example, if an executive had reported grants on March 1, 2007 and May 5, 2008, then we would use March 1, 2008 as the grant date for the latter grant. This approximates when the 2008 options would have been granted, absent strategic backdating. We form realized volatility measures based on these alternative grant dates and find similar results with both instruments. Thus, our results are very unlikely to be driven by endogenous choice of grant dates.

Another potential concern is that after FAS123r becomes effective, we may not be able to control fully for changes in a CEO's incentives from non-option compensation. As mentioned earlier, after FAS123r, many firms began using performance-vesting shares. These types of shares typically have convex payoffs as well, but Execucomp does provide sufficient data to compute their vegas (Bettis et al., 2012). To make sure that our results are not somehow driven by performance-vesting shares, in Internet Appendix Table IAII, we re-estimate our baseline specifications, limiting the sample to the pre-FAS123r period (i.e., fiscal years starting prior to June 15, 2005). This leads to similar results.

In general, option compensation also vests over time. Like the prior literature, we ignore the issue of why firms attach time-vesting schedules to at-the-money options, which is beyond the scope of this study. Nonetheless, one may wonder why managers would increase volatility in the year following a shock to option grants since the options may not fully vest for several years. However, 42 percent of a typical option grant does vest within a one-year window (Cadman et al., 2012). In addition, after an option is granted, increasing volatility over any time interval prior to exercise will increase its value by leading to more extreme prices in expectation.[18]

Finally, given that many firms do not use fixed-number or fixed-value plans, one may wonder about the extent to which our results generalize to firms outside of our samples. That is, even if we are estimating a true causal effect for firms using multi-year plans, it may be the case that the causal effect for other firms would differ. We have no ex-ante reason to believe that the effect for other firms would be different, but it is also difficult to rule out this possibility entirely. In order to provide suggestive evidence on the generalizability of our results, we match each observation in the no-plan sample with its nearest neighbor in the main estimation samples using propensity score matching. We match on the firm-level controls from our baseline specification, along with industry and year. We then re-estimate our baseline IV specification, this time weighting observations by the number of no-plan observations that matched to them. This weighting scheme makes our estimation sample similar to the no-plan sample based on observable characteristics. For example, observations with characteristics that are out of line with those in the no-plan sample will receive zero weight in the new IV estimates. The results are presented in Internet Appendix Table IAIII. Panel A shows that the weighting scheme makes the our first IV sample comparable to the no-plan sample. Panel B shows the IV results with the weighting scheme. We see that the weighted results are similar to our baseline results, and if anything the magnitudes are slightly larger with the weights. Panels C and D repeat the same exercise using the second instrument. Again, the magnitudes are similar to before but slightly larger. Therefore, this analysis suggests no reason to believe that our results would not generalize beyond the multi-year plan samples.

IV. Conclusion

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

We find that, on average, executives lie in a region of their utility function in which increases in option pay—in the range of 10–15% generally observed in our data—lead to increases in firm equity volatility. A significant portion of the increase in volatility is driven by increases in leverage. Returning to the theory, we know that the effect of options on risk-taking may be non-monotonic. Very large option grants that are awarded to risk-averse and undiversified executives may lead to reduced risk-taking. Nevertheless, our estimates should be informative for boards and policymakers who are interested in the effects of moderate changes to existing convex compensation packages.

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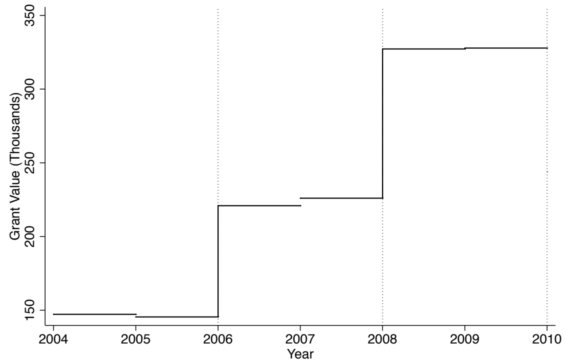
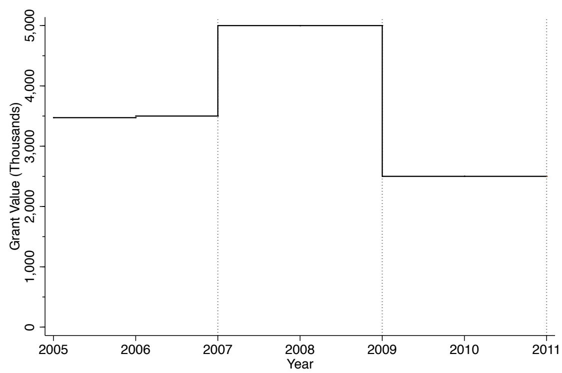
Panel A: Nucor



Panel B: US Bancorp



Panel C: Thomas & Betts

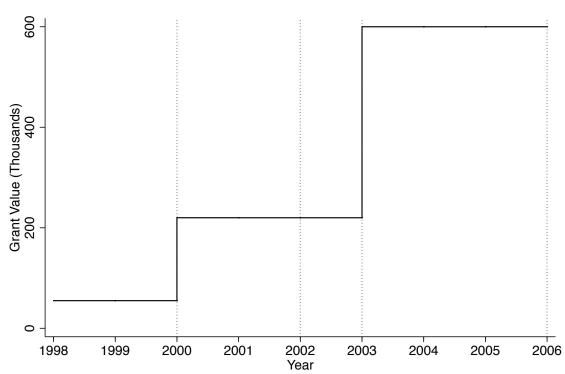


Figure 1. Real Examples of Fixed-Value Cycles and Predictions. This figure represents three examples of fixed-value cycles taken from the data. Years that we predict to be cycle first years are indicated by dotted vertical lines.

Table I
Summary Statistics

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

Panel A: Length of Cycles

	Fixed Number		Fixed Value	
	Freq	Percent	Freq	Percent
2	2755	61.14	2666	89.55
3	1034	22.95	279	9.37
4	387	8.59	18	0.60
≥ 5	330	7.32	14	0.47
Total	4506	100.00	2977	100.00

Panel B: Industry Distribution

Year: 2000	Fixed Number		Fixed Value		Other	
	Freq	Percent	Freq	Percent	Freq	Percent
Consumer Non-Durables	17	6.30	8	5.56	74	6.35
Consumer Durables	9	3.33	7	4.86	29	2.49
Manufacturing	36	13.33	29	20.14	116	9.96
Energy	10	3.70	6	4.17	52	4.46
Chemicals	10	3.70	5	3.47	31	2.66
Business Equipment	45	16.67	15	10.42	265	22.75
Telecommunications	8	2.96	3	2.08	36	3.09
Utilities	15	5.56	10	6.94	56	4.81
Shops	28	10.37	21	14.58	146	12.53
Health	25	9.26	7	4.86	85	7.30
Finance	33	12.22	21	14.58	148	12.70
Other	34	12.59	12	8.33	127	10.90
Total	270	100.00	144	100.00	1165	100.00

Table I
(Continued)

Panel C: Other Characteristics

Year: 2000	Fixed Number			Fixed Value			Other		
	p25	p50	p75	p25	p50	p75	p25	p50	p75
Firm-Level:									
Assets (Millions)	563.47	1545.80	6393.29	750.29	2051.62	6300.42	444.27	1383.17	5036.34
Sales (Millions)	476.09	1202.65	3211.77	640.98	1495.26	3994.24	372.11	1095.55	3708.53
Market to Book	1.09	1.42	2.35	1.09	1.37	2.38	1.12	1.54	2.53
Volatility (12 Months)	0.35	0.46	0.71	0.31	0.44	0.58	0.37	0.51	0.76
Volatility (120 Trading Days)	0.38	0.50	0.72	0.39	0.49	0.59	0.43	0.55	0.79
CAPX / PPE	0.14	0.23	0.42	0.13	0.23	0.38	0.15	0.26	0.51
Acquisitions (Millions)	0.00	1.01	48.53	0.00	0.00	39.97	0.00	0.66	38.30
Market Leverage	0.06	0.22	0.44	0.06	0.21	0.43	0.03	0.17	0.39
Book Leverage	0.19	0.41	0.57	0.19	0.42	0.57	0.10	0.37	0.55
Total Dividends (Millions)	0.00	3.05	37.73	0.00	11.45	62.06	0.00	0.00	30.41
Return on Assets	0.10	0.15	0.22	0.10	0.15	0.23	0.08	0.15	0.24
Cash Flow / Assets	0.07	0.10	0.17	0.08	0.10	0.16	0.06	0.11	0.18
CEO-Level:									
Salary (Thousands)	395.00	567.12	834.00	400.00	627.60	833.65	321.16	500.00	754.65
Bonus (Thousands)	184.00	440.00	940.83	182.91	416.25	835.04	79.47	315.00	800.00
Number New Options	50.00	105.00	225.00	50.00	117.17	250.00	80.00	180.00	438.75
Number Prev Options	228.29	475.00	1022.50	207.56	433.43	975.96	128.75	375.00	855.00
Value New Options (Thousands)	572.43	1278.20	3536.43	652.58	1187.33	3036.36	802.31	2417.57	6631.91
Value Prev Options (Thousands)	1870.69	5624.94	17512.38	1763.59	4556.15	13947.44	788.34	4129.09	13884.27
Delta New Options	7.91	18.16	52.67	8.82	17.66	45.51	10.91	31.04	83.41
Delta Prev Options	30.02	84.67	242.06	32.72	73.15	211.23	12.29	61.22	192.95
Vega New Options	5.86	13.80	38.56	7.39	14.14	38.60	6.87	19.97	51.48
Vega Prev Options	17.54	43.61	125.54	23.52	43.15	122.39	6.17	24.82	74.79
Value Prev Options + Stock	2150.92	6519.80	20821.43	2185.94	5197.37	14455.02	949.50	4752.08	14484.78
Delta Prev Options + Stock	31.66	89.07	258.88	34.33	75.80	228.73	14.59	67.40	197.01
Observations	270			144			1165		

Table II

IV1: First Stage

Panel A of this table shows how option compensation changes in fixed-value cycle first years. Panel B shows how option compensation changes in fixed-value cycle *predicted* first years. Observations are at the CEO-year level. The sample is limited to CEOs who are currently on fixed-value cycles or were in the previous year. First Year is an indicator variable equal to one in the year following the final year of a cycle. Predicted First Year is an indicator variable equal to one if the year is predicted to be a cycle first year based on the length of the previous cycle (see Section 3.1 for a detailed discussion of our prediction methodology). The variable B-S Value equals the Black–Scholes value of new option compensation, Delta equals the change in the Black–Scholes value of new option compensation associated with a one percent change in the price of the underlying, and Vega equals the change in the Black–Scholes value of new option compensation associated with a 0.01 change in the annualized volatility of the underlying. Control variables include CEO tenure, log of cash compensation (salary + bonus), log sales, log assets, sales growth, market to book, tangibility ratio, and a dummy variable for whether the firm has rated debt. These control variables are measured in the year prior to the year of the current option grant. In addition, controls for the change in the log delta and vega of previously granted (unexercised) option and stock holdings are included. That is, we calculate the log of total outstanding delta and vega as of the current grant date (excluding the current grant) and subtract from this the log of total outstanding delta and vega following the previous year's grant. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Real First Years

	Δ Log B-S Value		Δ Log Delta		Δ Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
First Year	0.149***	0.152***	0.165***	0.176***	0.157***	0.171***
	(0.0193)	(0.0192)	(0.0265)	(0.0256)	(0.0312)	(0.0302)
Controls	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.084	0.097	0.032	0.088	0.105	0.153
Observations	3,692	3,692	3,692	3,692	3,692	3,692

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	0.141***	0.144***	0.152***	0.165***	0.146***	0.161***
	(0.0185)	(0.0185)	(0.0255)	(0.0244)	(0.0296)	(0.0286)
Controls	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.082	0.095	0.031	0.087	0.104	0.152
Observations	3,692	3,692	3,692	3,692	3,692	3,692

Table III

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

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

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

Table IV
IV1: Volatility

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

Panel A: IV Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Δ Log B-S Value	0.115***	0.110***	0.108***	0.0978***
	(0.0390)	(0.0380)	(0.0378)	(0.0368)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	58.29	60.76	58.29	60.76
Observations	3,692	3,692	3,692	3,692

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First	0.0163***	0.0158***	0.0152***	0.0141***
	(0.00515)	(0.00518)	(0.00500)	(0.00504)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
R^2	0.300	0.302	0.279	0.284
Observations	3,692	3,692	3,692	3,692

Table V

IV1: Other Outcomes

Panel A shows IV estimation results, where the variable ΔLog B-S Value is instrumented using the Predicted First Year indicator, as defined in Table II. Observations are at the CEO-year level. The sample is limited to CEOs who are currently on fixed-value cycles or were in the previous year. Financial firms (SIC 6000-6999) and regulated utilities (SIC 4800-4999) are excluded. The variable Lev Ratio represents market leverage, which is defined as total debt divided by the market value of assets. The variable Log Debt represents the log of total debt. The variable Debt $\uparrow$ is an indicator equal to one if net debt issuance is positive, i.e., if total debt increased relative to the previous year. The variable Capx represents capital expenditures, and Tot Inv represents total investment, i.e., the sum of capital expenditures, R&D, acquisitions, and advertising expenses. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the Predicted First Year instrument. Control variables are those listed in Table II. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IV Estimation

	Leverage			Investment	
	(1) Δ Lev Ratio	(2) Δ Log Debt	(3) Debt $\uparrow$	(4) Log Capx	(5) Log Tot Inv
Δ Log B-S Value	0.0964**	0.129**	0.350**	-0.131	0.121
	(0.0484)	(0.0643)	(0.161)	(0.184)	(0.215)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	40.22	40.22	40.22	40.22	40.22
Observations	2,815	2,815	2,815	2,815	2,815

Panel B: Reduced Form Estimation

	Leverage			Investment	
	(1) Δ Lev Ratio	(2) Δ Log Debt	(3) Debt $\uparrow$	(4) Log Capx	(5) Log Tot Inv
Predicted First	0.0123**	0.0165**	0.0447**	-0.0167	0.0155
	(0.00572)	(0.00798)	(0.0195)	(0.0235)	(0.0278)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R^2	0.11	0.13	0.09	0.82	0.78
Observations	2,815	2,815	2,815	2,815	2,815

Table VI
IV1: Validity Tests

Panel A reproduces the reduced form specification from Panel B of Table IV, decomposing the predicted first year indicator into two variables. Predicted First (ΔBS Val > 0) is equal to one if the observation corresponds to a predicted first year in which the value of the CEOs option pay has increased. Predicted First (ΔBS Val ≤ 0) is equal to one if the observation corresponds to a predicted first year in which the value of the CEO's option pay has weakly decreased. Panel B again reproduces the reduced form specification from Panel B of Table IV, with different dependent variables. The variable Product represents the number of major product announcements the firm made during the year according to the CapitalIQ key developments database. The variable Expansion represents the number of business expansion announcements the firm made during the year. We consider CapitalIQ's coverage of a particular firm to begin with the firm's first recorded announcement of any type. The variable % Debt Maturing represents the percent of the firm's debt that was maturing in the year. We compute the percent of debt maturing in a year using the ratio of current liabilities to current liabilities plus long-term debt, as of the previous fiscal year end. Financial firms (SIC 6000-6999) and regulated utilities (SIC 4800-4999) are excluded when this dependent variable is used. The variable CEO turnover is an indicator equal to one if it is the last year the CEO is at the firm (except if it was also the last year the firm was in the data). Control variables are those listed in Table II. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Reduced Form Estimation with Partitioned Intrument

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First (Δ BS Val > 0)	0.0198***	0.0193***	0.0204***	0.0211***
	(0.00662)	(0.00672)	(0.00641)	(0.00640)
Predicted First (Δ BS Val ≤ 0)	0.00992	0.00864	0.00713	0.00278
	(0.00782)	(0.00791)	(0.00781)	(0.00792)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
R^2	0.305	0.311	0.282	0.294
Observations	3,692	3,692	3,692	3,692

Panel B: Other Firm Cycles

	(1) Δ Product	(2) Δ Expansion	(3) Δ % Debt Maturing	(4) CEO Turnover
Predicted First	-0.00944	-0.159	0.000670	0.00578
	(0.125)	(0.158)	(0.00514)	(0.00874)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R^2	0.025	0.022	0.031	0.028
Observations	3,478	3,478	2,795	2,950

Table VII
IV2: First Stage

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

	Δ Log B-S Value		Δ Log Delta		Δ Log Vega	
	(1)	(2)	(3)	(4)	(5)	(6)
FN × Ind Return	0.559***	0.548***	0.630***	0.599***	0.546***	0.510***
	(0.0473)	(0.0474)	(0.0878)	(0.0892)	(0.114)	(0.108)
Controls	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.182	0.261	0.082	0.281	0.153	0.286
Observations	3,535	3,535	3,535	3,535	3,535	3,535

Table VIII
IV2: Volatility

Panel A shows IV estimation results, where ΔLog B-S Value is instrumented using FN $\times$ Ind Return, as defined in Table VII. Observations are at the CEO-year level. The sample is limited to CEOs who are on either fixed-number or fixed-value plans (excluding the first years of cycles). All other variables are as defined in Table IV. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the FN $\times$ Ind Return instrument. The main effects of interaction terms are included in all specifications but not shown. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IV Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
Δ Log B-S Value	0.0832**	0.0864**	0.138***	0.144***
	(0.0424)	(0.0432)	(0.0457)	(0.0466)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	140.04	133.82	140.04	133.82
Observations	3,535	3,535	3,535	3,535

Panel B: Reduced Form Estimation

	Δ 12 Month Volatility		Δ 120 TD Volatility	
	(1)	(2)	(3)	(4)
FN $\times$ Ind Return	0.0465**	0.0474**	0.0773***	0.0788***
	(0.0200)	(0.0201)	(0.0251)	(0.0251)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
R^2	0.260	0.263	0.257	0.261
Observations	3,535	3,535	3,535	3,535

Table IX

IV2: Other Outcomes

Panel A shows IV estimation results, where ΔLog B-S Value is instrumented using FN × Ind Return, as defined in Table VII. Observations are at the CEO-year level. The sample is limited to CEOs who are on either fixed-number or fixed-value plans (excluding the first years of cycles). Financial firms (SIC 6000-6999) and regulated utilities (SIC 4800-4999) are excluded. All other variables are as defined in Table V. Panel B shows the results of the reduced form estimation in which these outcomes are regressed directly on the FN × Ind Return instrument. The main effects of interaction terms are included in all specifications but not shown. Control variables are those listed in Table II. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IV Estimation

	Leverage			Investment	
	(1) Δ Lev Ratio	(2) Δ Log Debt	(3) Debt ↑	(4) Log Capx	(5) Log Tot Inv
Δ Log B-S Value	0.0717**	0.129**	0.310**	0.307	0.196
	(0.0315)	(0.0626)	(0.135)	(0.191)	(0.235)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	122.83	122.83	122.83	122.83	122.83
Observations	2,930	2,930	2,930	2,930	2,930

Panel B: Reduced Form Estimation

	Leverage			Investment	
	(1) Δ Lev Ratio	(2) Δ Log Debt	(3) Debt ↑	(4) Log Capx	(5) Log Tot Inv
FN × Ind Return	0.0427**	0.0718**	0.173**	0.171	0.109
	(0.0179)	(0.0342)	(0.0741)	(0.106)	(0.132)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R^2	0.20	0.13	0.08	0.84	0.75
Observations	2,930	2,930	2,930	2,930	2,930

Table X

Interacted Controls and Non-Linearity

This table re-estimates Table VIII with additional controls. First, rather than controlling linearly for the main effect of industry returns, we include a 5-degree polynomial of industry returns. Second, in Columns (3) and (6) we allow that 5-degree polynomial to interact with all of the control variables listed in Table II in order to control for the fact that firms with different observable characteristics might respond differently (and non-linearly) to industry returns. Across all specifications with a 5-degree polynomial of industry returns, the excluded instrument is the interaction between the 5-degree polynomial of industry returns and the fixed-number plan indicator variable. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Δ 12 Month Volatility			Δ 120 TD Volatility		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log B-S Value	0.0866**	0.0882**	0.107***	0.127***	0.128***	0.0917**
	(0.0415)	(0.0425)	(0.0414)	(0.0427)	(0.0439)	(0.0447)
Controls	No	Yes	Yes	No	Yes	Yes
Poly. Ind. Returns	Yes	Yes	Yes	Yes	Yes	Yes
Controls $\times$ Poly. Ind. Returns	No	No	Yes	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat (1st Stage)	41.58	37.77	37.60	41.58	37.77	37.60
Observations	3,535	3,535	3,535	3,535	3,535	3,535

Table XI
IV2: Placebo Test

Panel A of this table shows reduced form regression results where the outcomes in Table IX are regressed directly on the FN Placebo × Ind. Return placebo instrument. The placebo sample is restricted to CEOs receiving option pay who are not currently on a cycle, but were on a fixed-number or fixed-value cycle in some other year (in the past or future). FN Placebo is an indicator variable equal to one if the CEO was on a fixed-number cycle in some other year. The main effects of the interaction terms are included in all specifications but not shown. In panel Panel B of this table, the placebo sample is instead limited to firms that discontinued granting options altogether (for all top executives) in the fiscal year before, the fiscal year of, or the fiscal year after FAS123r went into effect for the firm (FAS123r applied to the first fiscal year starting after June 15, 2005). The sample only includes the post-FAS123r period. In this case, FN Placebo is an indicator variable equal to one if the CEO was on a fixed-number cycle prior to the firm ceasing the use of option pay and zero if the CEO was on a fixed-value cycle. Control variables are those listed in Table II. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Placebo Test

	Volatility		Leverage			Investment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δ 12 Month Vol	Δ 120 TD Vol	Δ Lev Ratio	Δ Log Debt	Debt ↑	Log Capx	Log Tot Inv
FN Placebo × Ind Return	-0.000489	-0.0164	-0.00615	0.0118	0.0198	-0.0493	0.0284
	(0.0175)	(0.0164)	(0.0149)	(0.0240)	(0.0499)	(0.0708)	(0.0913)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.29	0.29	0.12	0.13	0.08	0.85	0.75
Observations	6,098	6,098	5,647	5,647	5,647	5,647	5,647

Panel B: Placebo Test with firms that discontinued options sourrounding FAS123r

	Volatility		Leverage			Investment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δ 12 Month Vol	Δ 120 TD Vol	Δ Lev Ratio	Δ Log Debt	Debt ↑	Log Capx	Log Tot Inv
FN Placebo × Ind Return	-0.0271	-0.0192	0.0196	-0.0652	-0.167	-0.0464	-0.210
	(0.0493)	(0.0461)	(0.0389)	(0.0560)	(0.175)	(0.237)	(0.209)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.55	0.67	0.18	0.15	0.14	0.82	0.74
Observations	657	657	551	551	551	551	551

Table XII

Heterogeneity

This table re-estimates the baseline reduced form specifications from Column 3 of Panel B of Tables IV and VIII. The change in 120 trading day volatility is regressed on Pred First Year in the left-most columns and on FN × Ind Return in the right-most columns. In Panel A, the sample is split into terciles based upon the ratio of the value of new options to all unexercised options as of the grant date of the new options. The High columns are the top tercile sample, and the Low columns are the bottom tercile sample. The P-value row represents the P-value of the test that the coefficients in the High and Low column are equal. In Panel B, the sample is split based upon whether the company is in the finance (SIC 6000-6999) or high-tech sectors ("high-tech" in Fama–French 5-Industry Classification). Control variables are those listed in Table II. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Percent New Options

DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year		Instrument = FN × Ind Return	
	(1) Low	(2) High	(3) Low	(4) High
Instrument	0.0124*	0.0313***	0.0291	0.156***
	(0.00738)	(0.00906)	(0.0456)	(0.0402)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
P-value	0.044	0.044	0.004	0.004
R^2	0.359	0.321	0.313	0.283
Observations	1,230	1,229	1,178	1,177

Panel B: Finance and High Tech

DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year		Instrument = FN × Ind Return	
	(1) No Fin/Tech	(2) Fin/Tech	(3) No Fin/Tech	(4) Fin/Tech
Instrument	0.0108*	0.0201**	0.0536*	0.0955**
	(0.00617)	(0.00869)	(0.0319)	(0.0413)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
P-value	0.336	0.336	0.272	0.272
R^2	0.269	0.338	0.254	0.299
Observations	2,524	1,168	2,364	1,171

Table XIII
Robustness

This table re-estimates the baseline IV specifications from Column 3 of Panel A of Tables IV and VIII . In both panels, Δ Log B-S Value is instrumented using the Predicted First Year indicator in odd-numbered columns and FN $\times$ Ind Return in even-numbered columns. In the first two columns of Panel A, controls for the annual change in log total compensation (the sum of the grant date values of salary, bonus, restricted stock grants, option grants, and all other compensation) are added to the baseline IV specifications. In Columns 3 and 4, separate controls for the annual changes in the grant date values of the logarithms of salary, bonus, restricted stock grants, and all other compensation are added to the baseline IV specifications. In Columns 5 and 6, the sample is restricted to CEO-years in which no restricted stock is granted. In the first two columns of Panel B, the sample is restricted to two-year cycles. In Columns 3 and 4, the sample is restricted to the set of CEO-years in which only one option grant is awarded. In Columns 5 and 6, rather than computing volatility following the reported grant date, we compute volatility following a predicted grant date (using the same month and day of the previous year's reported grant date). The main effects of interaction terms are included in all specifications but not shown. Control variables are those listed in Table II. Standard errors are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Other Compensation

DEP VAR: Δ 120 TD VOL	Total Comp Control		Component Controls		No Restricted Stock	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log B-S Value	0.171***	0.189***	0.0937***	0.143***	0.0715**	0.149***
	(0.0618)	(0.0587)	(0.0354)	(0.0463)	(0.0348)	(0.0513)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrument	IV1	IV2	IV1	IV2	IV1	IV2
F-Stat (1st Stage)	33.35	98.00	65.08	135.98	49.03	103.85
Observations	3,692	3,535	3,692	3,535	2,212	2,425

Panel B: Additional Tests

DEP VAR: Δ 120 TD VOL	2-Yr Cycles		1 Grant/Yr		Predicted Grant Date	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log B-S Value	0.0991***	0.150***	0.115***	0.110**	0.104**	0.165***
	(0.0356)	(0.0552)	(0.0404)	(0.0488)	(0.0461)	(0.0549)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrument	IV1	IV2	IV1	IV2	IV1	IV2
F-Stat (1st Stage)	58.61	69.69	52.70	126.27	52.51	156.75
Observations	3,326	2,147	3,271	3,008	3,468	3,228

END NOTES

[1]The magnification effect has also been noted by Lambert et al. (1991), Carpenter (2000), Hall and Murphy (2002a), and Lewellen (2006), among others. Options may also have other ambiguous implications for risk. For example, options increase in value with firm performance, and managers may increase or decrease firm risk in the pursuit of stronger firm performance. In addition, option compensation increases wealth, which may alter risk tolerance.

[2]Using a different strategy, Gormley et al. (2013) examine responses to an exogenous increase in firm litigation risk. The exogenous nature of the shock helps rule out reverse causality and allows the authors to explore an important related question: how does a change in risk affect option compensation? However, to identify a causal effect of options on risk-taking, the ideal test would utilize exogenous variation in option pay rather than in the risk environment.

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

[4]Note that we do not regress firm outcomes on the actual change in option compensation that a CEO experienced at the start of a new cycle, as the size of that change may be related to unobservables that affect risk-taking. Instead, we use the fact that the indicator for predicted first year corresponds to increases in option pay on average and is staggered across firms. Our analysis essentially compares average changes in risk-taking in years when the indicator is equal to one to years in which the indicator is equal to zero.

[5]There is indirect evidence from several papers that the overall net effect of options on risk-taking may in fact be negative. For example, Lewellen (2006) calibrates a model of the volatility cost of debt for managers, assuming they are risk averse and have power utility functions. She finds that for a range of empirically relevant parameters, higher option ownership tends to increase, not decrease, the volatility cost of debt. In a similar vein, Bettis et al. (2005) find that executives exercise their options earlier when volatility increases, which in a model calibration suggests that

subjective option values may actually decrease with volatility.

[6]See “Raising the Stakes: A Look at Current Stock Option Granting Practices,” 1998, Towers Perrin CompScan Report. In addition, note that holding “face value” constant is equivalent to holding “potential realizable value” constant, where “potential realizable value” is the value of the option at expiration, assuming a constant rate of appreciation of the underlying stock, e.g., five percent. Prior to 2006, firms were required to report either Black–Scholes value or potential realizable value in their proxy statements.

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

[8]See “Raising the Stakes: A Look at Current Stock Option Granting Practices,” 1998, Towers Perrin CompScan Report. Our IV strategy is still applicable for fixed salary proportion cycles because executives still tend to receive small increases in option pay within cycles and larger jumps in option pay at the start of a new cycle, so option grants still tend to follow an upward step function. We find similar results if we include these grants in our fixed-value sample.

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

[10]Note that we do not need to further adjust our standard errors to account for the fact that our

instrument is a "predicted" variable. In contrast to generated regressors in OLS, generated instruments in IV do not require standard errors to be adjusted (Wooldridge, 2002). Also, our predicted first year instrument is not what would typically be considered a generated instrument/regressor. It does not come out of a pre-first-stage regression model estimated with error. In other words, we are not estimating a 3-Stage Least Squares (3SLS) specification in which the predicted first year dummy is used to instrument for the true first year dummy, which is then used to instrument for changes in option compensation. Instead, we use a standard 2SLS IV specification. The true first year dummy does not factor directly into this 2SLS IV estimation. Instead, the first stage directly instruments for the change in option compensation using the predicted first year dummy. That is, our instrument literally is whether an observation is a *predicted* first year.

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

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

[13]This approximation is made by observing that

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

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

[14]We compute the percent of debt maturing in a year using the ratio of current liabilities to current liabilities plus long term debt, as of the previous fiscal year end. Results are similar if we instead use the ratio of long term debt due in one year to total long term debt.

[15]In Internet Appendix Table IAIV, we also perform a placebo test where we repeat our analysis using stock compensation, limiting the sample to observations without option compensation. We find no change in volatility associated with predicted first years of stock fixed-value cycles. However, our sample in this case is quite small. Therefore, we may lack power to estimate an effect, even if one were present.

[16]FAS123r applied to a firm's first fiscal year starting after June 15, 2005

[17]Instead of showing the IV specifications, Table XII shows the reduced form in which the outcome is regressed directly on the instrument and controls. This is done so that we can more easily report p-values, which test whether coefficients are different across columns. We arrive at these p-values by, for example, estimating Columns 1–2 in Panel A within a single OLS regression, where standard errors are clustered by firm.

[18]Also note that our main outcome of interest is the change in volatility relative to the level in the previous year. In other words, we do not necessarily expect that the level of volatility will be higher in the first year after a shock to option grants than in subsequent years. However, we should see a larger change in volatility (relative to observed volatility in the previous year) when there is a large shock to new options granted.