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How Do Quasi-Random Option Grants Affect CEO Risk-Taking?

KELLY SHUE and RICHARD R. TOWNSEND*

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 multiyear 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% increase in new options granted leads to a 2.8% to 4.2% increase in equity volatility. This increase in risk is driven largely by increased leverage.

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 a major component of CEO pay, accounting for over 20% of total pay (Murphy (2013)). Moreover, Bettis et al. (2012) find that many firms have 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

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40% of equity awards were characterized by performance vesting rather than simple time vesting. Given the prevalence of options and option-like compensation, it is important to understand the extent to which 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 options can have a countervailing “magnification effect.” 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 an 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 net present value (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 existing research on options and risk-taking has been correlational in nature, two recent studies by Chava and Purnanandam (2010) and Hayes, Lemmon, and Qiu (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. Furthermore, 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, so there is no control group available to estimate the counterfactual change

¹ The magnification effect has also been noted by Lambert, Larcker, and Verrecchia (1991), Carpenter (2000), Hall and Murphy (2002), 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.

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 multiyear plans. Two types of plans are commonly used: fixed-number and fixed-value.³ Under a fixed-number plan, an executive receives the same number of options each year within a cycle. Under 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.

Multiyear plans give us two distinct sources of variation in the timing of option pay increases. Our first instrumental variables (IV) 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. Predictions are therefore 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 IV strategy does not use the timing of cycle first years, but rather 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 Hall (1999)

² Using a different strategy, Gormley, Matsa, and Milbourn (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 multiyear grant cycles in detail, but does not use them as an instrument to explore the effect of options on managerial behavior.

notes, 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 driven in part 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 multiyear plans are used to minimize contracting costs, as option compensation only has to be set once every few years. Hall (1999, p. 97) argues that firms sort into the two types of plans somewhat arbitrarily, observing that "Boards seem to substitute one plan for another without much analysis or understanding of their differences." Consistent with this view, Shue and Townsend (2017) find suggestive evidence that boards granted fixed-number options due to contracting frictions and a 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 multiyear plan cycle is exogenous. Among firms using multiyear plans, 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 multiyear plans in an unsophisticated way, it does not necessarily follow that all panel variation in option compensation is exogenous to risk-taking. Instead, we argue that multiyear 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 prescheduled intervals (i.e., 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 the risk of this firm increases significantly more at the start of a new cycle than in other years, these increases in risk are likely due 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 driven

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 the 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.

in part 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 differ systematically from fixed-value firms, but we assume that they do not differ in how their noncompensation-related risk-taking moves with aggregate returns. We find support for this assumption in a number of tests.

Our two IV 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. Based on average grant levels during our sample period, a 10% increase in new options granted leads to a 2.8% to 4.2% 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 in which 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 focuses 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, Hall, and Viceira (2000), Coles, Daniel, and Naveen (2006), Chava and Purnanandam (2010), Liu and Mauer (2011), Armstrong and Vashishtha (2012), and Hayes, Lemmon, and Qiu (2012) all examine the association between vega and risk-taking, and mostly find a positive relation. However, the effect of vega on risk-taking is theoretically unambiguous, as vega is a measure of convexity. Using Ross’s (2004) terminology, increases in vega should increase the convexity effect, leading to weakly more risk-taking, all else equal. Thus, the existing empirical literature focuses 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 both 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 the simultaneous increase in these three dimensions of option compensation affects risk-taking. Understanding the total net effect of increases in at-the-money option grants on risk-taking is important, as most compensation committees and policy makers 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 the construction of our key variables. Section II discusses our two IV strategies. 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 to CEOs in our analysis. For option compensation, we use individual grant-level data, which allow us to better identify CEOs on fixed-number and fixed-value plans in cases in which 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.

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, Bizjak, and Lemmon (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.

Accounting data come from Compustat. Following standard practice, financial firms (6000 to 6999) and regulated utilities (4800 to 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 multiyear compensation cycles, and therefore few report them. Following Hall (1999), we 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 multiyear plan but awarded the same number or value of options across consecutive years for potentially endogenous reasons. We also infer planned cycles with error if the firm departs from a preplanned schedule for potentially endogenous reasons. As we discuss below, our methodology is robust to both of these types of errors. In our IV framework, measurement error reduces the precision of our estimates but does 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 compare only the largest grant as 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% 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 that the executive receives is within 3% 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 3% tolerance because firms often grant options in round lots, in which case 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 parameters other 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, Hall, and Viceira (2000), Armstrong and Vashishtha (2012), Hayes, Lemmon, and Qiu (2012), Gormley, Matsa, and Milbourn (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. 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%. Another possibility would be to use cash flow volatility. However, as we discuss shortly, our methodology is constrained to looking 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

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, for example, 5%. 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 be due in part to relatively more measurement error in the process

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 Internet Appendix.

Panel A: Length of Cycles					
Fixed Number			Fixed Value		
Freq	Percent		Freq	Percent	
2	2,755	61.14	2,666	89.55	
3	1,034	22.95	279	9.37	
4	387	8.59	18	0.60	
5+	330	7.32	14	0.47	
Total	4,506	100.00	2,977	100.00	
Panel B: Industry Distribution					
Fixed Number			Fixed Value		
Freq	Percent		Freq	Percent	Other
Year: 2000					
Consumer Nondurables	17	6.30	8	5.56	6.35
Consumer Durables	9	3.33	7	4.86	2.49
Manufacturing	36	13.33	29	20.14	9.96
Energy	10	3.70	6	4.17	4.46
Chemicals	10	3.70	5	3.47	2.66
Business Equipment	45	16.67	15	10.42	22.75
Telecommunications	8	2.96	3	2.08	3.09
Utilities	15	5.56	10	6.94	4.81
Shops	28	10.37	21	14.58	12.53
Health	25	9.26	7	4.86	7.30
Finance	33	12.22	21	14.58	12.70
Other	34	12.59	12	8.33	10.90
Total	270	100.00	144	100.00	100.00

(Continued)

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	1,545.80	6,393.29	750.29	2,051.62	6,300.42	444.27	1,383.17	5,036.34	
Sales (Millions)	476.09	1,202.65	3,211.77	640.98	1,495.26	3,994.24	372.11	1,095.55	3,708.53	
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	1,022.50	207.56	433.43	975.96	128.75	375.00	855.00	
Value New Options (Thousands)	572.43	1,278.20	3,536.43	652.58	1,187.33	3,036.36	802.31	2,417.57	6,631.91	
Value Prev Options (Thousands)	1,870.69	5,624.94	17,512.38	1,763.59	4,556.15	13,947.44	788.34	4,129.09	13,884.27	
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	2,150.92	6,519.80	20,821.43	2,185.94	5,197.37	14,455.02	949.50	4,752.08	14,484.78	
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			1,165			

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 results for year 2000, while results for 1995 and 2005 are presented in the Internet Appendix.⁸ Panel B of Table I shows the industry distribution for firm-years, categorized by the CEO's plan type. We find that multiyear 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 (1999) claim that firms sort approximately randomly into these plans. Nevertheless, as we discuss in Section II, our analysis does not assume that firms choose randomly between fixed-number and fixed-value plans.

Over our sample period, we find that multiyear plans are quite prevalent. Specifically, 18% of CEO-years with option compensation are associated with a fixed-number plan and 11.5% are associated with a fixed-value plan. 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 multiyear plans.⁹

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 1% change in the underlying), and vega (the change in the Black-Scholes value of a grant associated with a

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 The Internet Appendix may be found in the online version of this article.

⁹ 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.

0.01 unit change in the volatility of the underlying) of new at-the-money option grants are 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 policy makers do not contemplate awarding option compensation in such a way that only delta or only vega is changed in isolation. Rather, the relevant decision is typically whether to grant an executive more or less at-the-money options.

Accordingly, we instrument for Black-Scholes value in our two-stage least squares (2SLS) 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.¹⁰

A. IV Strategy 1

Our first IV 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. We are therefore 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

¹⁰ 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/nonlinear transformations of the endogenous variable being instrumented.

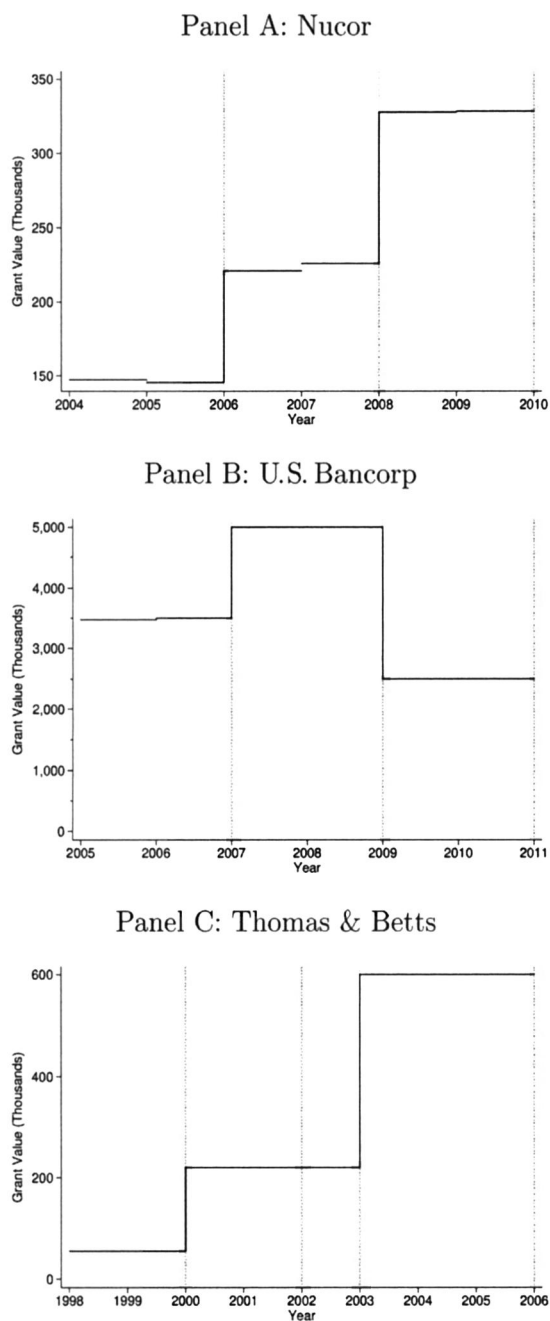


Figure 1. Real examples of fixed-value cycles and predictions. This figure shows three examples of fixed-value cycles taken from the data. Years that we predict to be cycle first years are indicated by dotted vertical lines.

Table 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 II.A 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 1% 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 appear in parentheses and 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*** (0.0193)	0.152*** (0.0192)	0.165*** (0.0265)	0.176*** (0.0256)	0.157*** (0.0312)	0.171*** (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*** (0.0185)	0.144*** (0.0185)	0.152*** (0.0255)	0.165*** (0.0244)	0.146*** (0.0296)	0.161*** (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

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 on average.

Panel A of Table II confirms that the increasing step function pattern holds 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% 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% to 18% 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 midway 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% 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 3%). 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 (2015)).

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 two. 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 two followed by two cycles of length three. 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 design choice allows us to purge the estimates of potential bias that would arise if actual cycle status is correlated with current conditions. Indeed, 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, 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 estimate the effect of changes in option compensation in an IV framework. Specifically, we estimate first- and second-stage equations of the form

$$\Delta O_{ijt} = \beta_0 + \beta_1 I_{ijt}^{\text{PredictedFirstYear}} + \gamma_t + \text{controls} + \epsilon_{ijt}, \quad (\text{First stage})$$

$$\Delta Y_{ijt} = \delta_0 + \delta_1 \widehat{\Delta O_{ijt}} + \gamma_t + \text{controls} + \mu_{ijt}, \quad (\text{Second stage})$$

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

The variable ΔO_{ijt} denotes the change in the value of the option grant, and ΔY_{ijt} are the outcome variables, which are 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, which are 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 ΔY_{ijt} .¹¹ 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 the 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, and 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. This would work against finding larger increases in risk during predicted first years than in other years.¹²

B. IV Strategy 2

Our second IV 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

¹¹ 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 three-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.

¹² 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.

Table III
Sensitivity of New Grants to Stock Price: Fixed Value versus 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%, the risk-free rate is 6%, the dividend rate is 3%, and the maturity is 10 years.

Plan		Stock Price		
		Year 1 Grant 100	Year 2 Grant 120	Year 3 Grant 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	\$1,000,000	\$1,000,000
	Number of Options	28,128	28,128	28,128

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 present 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.

Table III illustrates this via an example adapted from Hall (1999). The example compares how option compensation would evolve for an executive on a fixed-value versus 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% 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% 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 driven in part by industry shocks. These shocks are beyond a CEO's control and are difficult to predict, even by sophisticated agents.¹³ 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

$$\Delta O_{ijt} = \beta_0 + \beta_1 I_{ijt}^{FN} + \beta_2 R_{kt} + \beta_3 I_{ijt}^{FN} R_{kt} + \gamma_t + controls + \epsilon_{ijt}, \quad (\text{First stage})$$

$$\Delta Y_{ijt} = \delta_0 + \delta_1 I_{ijt}^{FN} + \delta_2 R_{kt} + \delta_3 \widehat{\Delta O_{ijt}} + \gamma_t + controls + \mu_{ijt}, \quad (\text{Second stage})$$

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

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

The exclusion restriction instead requires that the interaction term, $I_{ijt}^{FN} R_{kt}$, only relates to risk-taking, ΔY_{ijt} , through its effect on compensation. In other words, we assume that fixed-value and fixed-number executives do not have different noncompensation-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 IV 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 a 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

¹³ 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 5% of the variation in yearly industry returns.

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. IV 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. We find that the predicted first-year indicator does indeed strongly predict 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 correspond to an approximate 15% 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 related 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

Table IV
IV1: Volatility

Panel A of this table shows instrumental variable estimation results, where the variable $\Delta \text{Log } B\text{-}S \text{ Value}$ 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 appear in parentheses and are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Instrumental Variable Estimation				
	Δ 12-Month Volatility		Δ 120-TD Volatility	
	(1)	(2)	(3)	(4)
$\Delta \text{ Log } B\text{-}S \text{ Value}$	0.115*** (0.0390)	0.110*** (0.0380)	0.108*** (0.0378)	0.0978*** (0.0368)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
<i>F</i> -stat. (First 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)
<i>Predicted First Year</i>	0.0163*** (0.00515)	0.0158*** (0.00518)	0.0152*** (0.00500)	0.0141*** (0.00504)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
<i>R</i> ²	0.300	0.302	0.279	0.284
Observations	3,692	3,692	3,692	3,692

results in Column (4) imply that a 10% increase in new options corresponds to an approximately 0.011 unit increase in equity volatility relative to the median of 0.29 for our sample period, or a 3.8% 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

Table V
IV1: Other Outcomes

Panel A shows instrumental variable estimation results, where the variable $\Delta \text{Log } B\text{-}S \text{ Value}$ 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 to 6999) and regulated utilities (SIC 4800 to 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, that is, if total debt increased relative to the previous year. The variable *Capx* represents capital expenditures, and *Tot Inv* represents total investment, that is, 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 appear in parentheses and are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Instrumental Variable Estimation					
	Leverage			Investment	
	(1) $\Delta \text{ Lev Ratio}$	(2) $\Delta \text{ Log Debt}$	(3) $\text{Debt } \uparrow$	(4) Log Capx	(5) Log Tot Inv
$\Delta \text{ Log } B\text{-}S \text{ Value}$	0.0964** (0.0484)	0.129** (0.0643)	0.350** (0.161)	-0.131 (0.184)	0.121 (0.215)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
F-stat (First 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) $\Delta \text{ Lev Ratio}$	(2) $\Delta \text{ Log Debt}$	(3) $\text{Debt } \uparrow$	(4) Log Capx	(5) Log Tot Inv
<i>Predicted First Year</i>	0.0123** (0.00572)	0.0165** (0.00798)	0.0447** (0.0195)	-0.0167 (0.0235)	0.0155 (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

increase in leverage will mechanically lead to an increase in equity volatility. Columns (1) to (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% 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% increase in the value of new options granted corresponds to a 1.3% increase in book leverage and a 3.5% 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. We find that a 10% increase in the value of new options is associated with a statistically significant 1.75% decline in the equity to assets ratio, which in turn implies an approximately 1.75% increase in equity volatility.¹⁴ 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 should affect firm risk. While it may seem intuitive that investment increases risk, some argue that certain forms of investment, such as capital expenditures, decrease risk (Coles, Daniel, and Naveen (2006)). We therefore examine the effect of option compensation on investment, leaving open the question of whether this contributes to the increase in volatility. In Columns (4) and (5) of Table IV, 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. We also explore how options affect R&D, diversifying acquisitions, and nondiversifying acquisitions separately and find positive, albeit noisily estimated, effects.

We perform additional 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 multiyear 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 multiyear 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.

¹⁴ This approximation is made by observing that

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

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 .

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 increase or decrease at the start of new fixed-value cycles (although they increase on average). If our results are driven by noncompensation cycles that coincide with fixed-value cycles, we should see increases in risk during both types of predicted first years, whereas if our results are driven by option compensation, we should see increases in risk only 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 directly examine 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 years 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. 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 matures in predicted first years relative to other years for fixed-value firms.¹⁵ Finally, in Column (4), we 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.¹⁶

¹⁵ 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.

¹⁶ 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 is quite small, and hence, we may lack power to estimate an effect, even if one were present.

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 Year* ($\Delta BS\ Val > 0$) is equal to one if the observation corresponds to a predicted first year in which the value of the CEO's option pay has increased. *Predicted First Year* ($\Delta BS\ Val \leq 0$) 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 to 6999) and regulated utilities (SIC 4800 to 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 appear in parentheses and are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Reduced-Form Estimation with Partitioned Instrument				
	Δ 12-Month Volatility		Δ 120-TD Volatility	
	(1)	(2)	(3)	(4)
Predicted First Year ($\Delta BS\ Val > 0$)	0.0198*** (0.00662)	0.0193*** (0.00672)	0.0204*** (0.00641)	0.0211*** (0.00640)
Predicted First Year ($\Delta BS\ Val \leq 0$)	0.00992 (0.00782)	0.00864 (0.00791)	0.00713 (0.00781)	0.00278 (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 Year	-0.00944 (0.125)	-0.159 (0.158)	0.000670 (0.00514)	0.00578 (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

B. IV 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.

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 appear in parentheses and 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*** (0.0473)	0.548*** (0.0474)	0.630*** (0.0878)	0.599*** (0.0892)	0.546*** (0.114)	0.510*** (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

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% increase in the value of option grants relative to CEOs on fixed-value plans. Again, we instrument for changes in the Black-Scholes value in the remainder of our analysis. Our estimates thus 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 in Table VIII 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% increase in the value of new options granted leads to a 0.0086 to 0.0144 increase in equity volatility, or a 2.79% to 4.24% 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) to (3) of Table IX show that a 10% increase in the value of new options granted leads to a 0.0072 unit increase in the market leverage ratio, a 1.3% increase in total book leverage, and a 3.1% increase in the probability of positive net issuance. We estimate that 45% of the increase in equity volatility is due to increased leverage. Thus, the results 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

Table VIII
IV2: Volatility

Panel A shows IV estimation results, where $\Delta \text{Log } B\text{-}S \text{ Value}$ is instrumented using $FN \times \text{Ind Return}$, 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 $FN \times \text{Ind Return}$ instrument. The main effects of interaction terms are included in all specifications but not shown. Standard errors appear in parentheses and are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Instrumental Variable Estimation				
	Δ 12-Month Volatility		Δ 120-TD Volatility	
	(1)	(2)	(3)	(4)
$\Delta \text{ Log } B\text{-}S \text{ Value}$	0.0832** (0.0424)	0.0864** (0.0432)	0.138*** (0.0457)	0.144*** (0.0466)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
<i>F</i> -stat (First 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)
$FN \times \text{Ind Return}$	0.0465** (0.0200)	0.0474** (0.0201)	0.0773*** (0.0251)	0.0788*** (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

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 IV strategy requires the assumption that fixed-number and fixed-value CEOs do not have differential noncompensation-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 uses data only 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

Table IX
IV2: Other Outcomes

Panel A shows instrumental variable estimation results, where $\Delta \text{Log } B\text{-}S \text{ Value}$ is instrumented using $FN \times \text{Ind Return}$, 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 to 6999) and regulated utilities (SIC 4800 to 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 $FN \times \text{Ind Return}$ 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 appear in parentheses and are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Instrumental Variable Estimation					
	Leverage			Investment	
	(1) $\Delta \text{ Lev Ratio}$	(2) $\Delta \text{ Log Debt}$	(3) $\text{Debt } \uparrow$	(4) Log Capx	(5) Log Tot Inv
$\Delta \text{ Log } B\text{-}S \text{ Value}$	0.0717** (0.0315)	0.129** (0.0626)	0.310** (0.135)	0.307 (0.191)	0.196 (0.235)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
$F\text{-stat (First Stage)}$	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) $\Delta \text{ Lev Ratio}$	(2) $\Delta \text{ Log Debt}$	(3) $\text{Debt } \uparrow$	(4) Log Capx	(5) Log Tot Inv
$FN \times \text{Ind Return}$	0.0427** (0.0179)	0.0718** (0.0342)	0.173** (0.0741)	0.171 (0.106)	0.109 (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

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 nonlinear. 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 five-degree polynomial of R_{kt} . In this case, our excluded instruments become the interaction between this five-degree polynomial and I_{ijt}^{FN} . The nonexcluded instruments include the direct effects of I_{ijt}^{FN} and the

Table X
Interacted Controls and Nonlinearity

This table reestimates Table VIII with additional controls. First, rather than controlling linearly for the main effect of industry returns, we include a five-degree polynomial of industry returns. Second, in Columns (3) and (6) we allow the five-degree polynomial to interact with all of the control variables listed in Table II to control for the fact that firms with different observable characteristics might respond differently (and nonlinearly) to industry returns. Across all specifications with a five-degree polynomial of industry returns, the excluded instrument is the interaction between the five-degree polynomial of industry returns and the fixed-number plan indicator variable. Standard errors appear in parentheses and 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** (0.0415)	0.0882** (0.0425)	0.107*** (0.0414)	0.127*** (0.0427)	0.128*** (0.0439)	0.0917** (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 (First 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

five-degree polynomial of R_{kt} . The results with and without other controls are reported in Table X, Columns (1) and (2) and Columns (4) and (5), respectively. As can be seen, our estimates change very little with the inclusion of nonlinear controls.

Finally, we allow all of our firm and CEO control variables to interact with the five-degree polynomial of R_{kt} . 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 if 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 (2016)). Overall, these results provide evidence that our baseline findings are not driven by nonlinearity or observable differences between fixed-number and fixed-value firms.

It remains possible that unobservable differences between fixed-number and fixed-value firms 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 multiyear 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 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 multiyear 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 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 expense 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) as option "discontinuers."¹⁷ For these firms, it is even less likely that they stopped using multiyear plans due to anticipated changes in the responsiveness of their firm's risk to industry returns. Rather, these firms likely stopped using multiyear 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 who were on either a fixed-number or fixed-value plan prior to the discontinuation of options, and the variable $I_{ijt}^{FN\ Placebo}$ indicates 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. These results suggest

¹⁷ FAS123r applied to a firm's first fiscal year starting after June 15, 2005.

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* \times *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 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 appear in parentheses and 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 \uparrow	Log Capx	Log Tot Inv
FN Placebo \times Ind Return	-0.000489 (0.0175)	-0.0164 (0.0164)	-0.00615 (0.0149)	0.0118 (0.0240)	0.0198 (0.0499)	-0.0493 (0.0708)	0.0284 (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 Surrounding 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 \uparrow	Log Capx	Log Tot Inv
FN Placebo \times Ind Return	-0.0271 (0.0493)	-0.0192 (0.0461)	0.0196 (0.0389)	-0.0652 (0.0560)	-0.167 (0.175)	-0.0464 (0.237)	-0.210 (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

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

Table XII
Heterogeneity

This table reestimates the baseline reduced-form specifications from Column (3) of Tables IV and VIII, Panel B. The change in 120-trading day volatility is regressed on *Pred First Year* in the leftmost columns and on $FN \times Ind\ Return$ in the rightmost 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 to 6999) or high-tech sectors ("high-tech" in Fama-French five-industry classification). Control variables are those listed in Table II. Standard errors appear in parentheses and are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Percent New Options				
DEP VAR: Δ 120 TD VOL	Instrument = Pred First Year		Instrument = FN \times Ind Return	
	(1) Low	(2) High	(3) Low	(4) High
Instrument	0.0124* (0.00738)	0.0313*** (0.00906)	0.0291 (0.0456)	0.156*** (0.0402)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
<i>p</i> -value	0.044	0.044	0.004	0.004
<i>R</i> 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 \times Ind Return	
	(1) No Fin/Tech	(2) Fin/Tech	(3) No Fin/Tech	(4) Fin/Tech
Instrument	0.0108* (0.00617)	0.0201** (0.00869)	0.0536* (0.0319)	0.0955** (0.0413)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
<i>p</i> -value	0.336	0.336	0.272	0.272
<i>R</i> ²	0.269	0.338	0.254	0.299
Observations	2,524	1,168	2,364	1,171

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 reestimate 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) and (2) and on $FN \times Ind\ Return$ in

Columns (3) and (4).¹⁸ 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 three to five 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

¹⁸ 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) and (2) in Panel A within a single OLS regression, where standard errors are clustered by firm.

Table XIII
Robustness

This table reestimates the baseline instrumental variable specifications from Column (3) of Tables IV and VIII, Panel A. In both panels, $\Delta \text{Log B-S Value}$ is instrumented using the *Predicted First Year* indicator in odd-numbered columns and $FN \times \text{Ind Return}$ 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 instrumental variable 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 instrumental variable 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 appear in parentheses and are clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Other Compensation						
	Total Comp Control		Component Controls		No Restricted Stock	
DEP VAR: Δ 120 TD VOL	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \text{Log B-S Value}$	0.171*** (0.0618)	0.189*** (0.0587)	0.0937*** (0.0354)	0.143*** (0.0463)	0.0715** (0.0348)	0.149*** (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 (First 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						
	2-Yr Cycles		1 Grant/Yr		Predicted Grant Date	
DEP VAR: Δ 120 TD VOL	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \text{Log B-S Value}$	0.0991*** (0.0356)	0.150*** (0.0552)	0.115*** (0.0404)	0.110** (0.0488)	0.104** (0.0461)	0.165*** (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 (First 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

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 be only 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. 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. When we limit the sample to years in which no restricted stock is awarded in Columns (5) and (6), we continue to 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 driven instead by the staggering of when first years occur among CEOs on cycles of equal length. We reestimate 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) shows 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, but they would also 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 unlikely to be driven by the endogenous choice of grant dates.

Another potential concern is that, in the post FAS123r period, we may not be able to control fully for changes in a CEO's incentives from nonoption 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 ensure that our results are not somehow driven by performance-vesting shares, in Internet Appendix Table IAII, we reestimate our baseline specifications after limiting the sample to the pre-FAS123r period (i.e., fiscal years starting prior to June 15, 2005). We obtain similar results.

In general, option compensation also vests over time. Like 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% of a typical option grant does vest within a one-year window (Cadman, Rusticus, and Sunder (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.¹⁹

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 multiyear 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. 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 reestimate 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 IVIII. Panel A shows that the weighting scheme makes the 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

¹⁹ 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.

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. This analysis suggests there is no reason to believe that our results would not generalize beyond the multiyear 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 multiyear 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% to 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 nonmonotonic. 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 policy makers who are interested in the effects of moderate changes to existing convex compensation packages.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.