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We study the relation between corporate governance and opportunistic option grant manipulation. Our methodology for studying grant manipulation focuses on how grant date prices rank within the price distribution of the grant month. Investigating the incidence of "lucky grants" -- defined as grants given at the lowest price of the month – we estimate that about 1150 lucky grants resulted from manipulation and that 12% of firms provided one or more lucky grant due to manipulation during the period 1996-2005. Examining the circumstances and consequences of lucky grants we find:

- Lucky grants were more likely when the company did not have a majority of independent directors on the board and/or the CEO had longer tenure -- factors that are both associated with increased influence of the CEO on pay-setting and board decision-making.
- Lucky grants were more likely to occur when the potential payoffs from such luck were high; indeed, even for the same CEO, grants were more likely to be lucky when granted in months in which the potential payoffs from manipulation were relatively higher.
- Luck was persistent: a CEO's chance of getting a lucky grant increases when a preceding grant was lucky as well.
- In contrast to impressions produced by cases coming under scrutiny thus far, grant manipulation has not been primarily concentrated in new economy firms but rather has been widespread throughout the economy, with a significant incidence of manipulation in each of the economy's 12 (Fama-French) industries.
- We find no evidence that gains from manipulated option grants served as a substitute for compensation paid through other sources; indeed, total reported compensation from such sources in firms providing lucky grants was higher.
- We estimate the average gain to CEOs from grants that were backdated to the lowest price of the month to exceed 20% of the reported value of the grant and to increase the CEO's total reported compensation for the year by more than 10%.
- About 1,000 (43%) of the lucky grants were "super-lucky," having been given at the lowest price not only of the month but also of the quarter, and we estimate that about 62% of them were manipulated.
- We identify certain pools of grants with an especially high probability of manipulation. For example, we identify a pool of 600 grants out of which 88% are estimated to be manipulated.

Key words: Executive compensation, corporate governance, options, backdating, spring loading, inside information, CEO, independent directors.
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I. INTRODUCTION

The opportunistic timing of executive stock option grants, via backdating or other forms of manipulation, has attracted a great deal of attention. The Senate Banking and Finance committees held hearings on the subject, and the SEC and a small army of private law firms hired by companies are investigating past grant practices. More than 120 companies have thus far come under scrutiny, dozens of executives and directors have been forced to resign, and dozens of companies announced that they will have to restate their past financial statements.[1]

Despite the substantial attention devoted to the subject our understanding of the circumstances and factors that produced such manipulation in some companies but not in others remains incomplete. In this paper we seek to shed light on this by identifying the relation between grant date manipulation and the characteristics of the granting firm, the receiving CEO, and the grant itself. Our tests identify a link between grant date manipulation and factors associated with higher influence of the CEO over directors, such as lack of board independence and long CEO tenure. We also find that backdated grants were not provided as a substitute for other forms of compensation but rather conferred extra benefits on executives already receiving higher pay relative to their peers. Manipulating the grant date was also more likely when the economic gain from it was higher; indeed, even for the same firm or CEO, grant manipulation was more likely to occur in month in which stock price volatility made manipulation more profitable.

Prior work by financial economists on option timing has focused on the abnormality of returns prior to or after the grant date (e.g., Yermack, 1997; Lie, 2005; Heron and Lie, 2006a; Narayanan and Seyhun, 2006b). Our approach focuses on the

[1] The WSJ maintains an "Options Scorecard" at www.wsj.com with an updated list of all the companies that have come under scrutiny in connection with backdating issues, and it counted more than 120 such companies as of Nov. 12, 2006. For an account of the large scale of investigations of past grants conducted by companies with the help of hired outside professionals, see James Bandler and Kara Scannell, "In Options Probes, Private Law Firms Play Crucial Role," Wall Street Journal, October 28, 2006.

ranking of a grant date's price in the distribution of prices during the month of the grant. We show that the grant date manipulation resulted in an abnormal fraction of the grants being given on days where the stock price was at the lowest level of the month. Much (though not all) of our analysis focuses on "lucky" grants – grants given at the lowest price of the month. We estimate that, during 1996-2005, about 12% of our sample firms provided one or more lucky grants whose timing was the result of manipulation. We show that lucky grants provide a useful tool for studying the opportunistic timing of option grants including, in particular, identifying the firm and CEO characteristics associated with manipulation and deriving estimates of the incidence and gains from manipulation.

The universe of grants we study contains all the at-the-money, unscheduled grants awarded to public companies' CEOs during the decade of 1996-2005. We find a clear monotonic relation between how a trading day ranked within the price distribution of the month and the likelihood that the day happened to be a grant date. Compared to a random assignment, a day was most likely to be chosen if its stock price was at the lowest level, second most likely to be chosen if its price was at the second-lowest level, and third most likely to be chosen if its price was at the third lowest level. Similarly, dates with a stock price at the highest level of the month were most likely to be avoided as grant dates, followed in turn by dates with the second-highest price and then dates with the third-highest price.

Compared with a random assignment of grant dates, the excess incidence of grants is concentrated at the lowest price of the month, that is, in the form of lucky grants. We estimate that about 1150 lucky grants (roughly half of all lucky grants in our sample) owe their status to opportunistic timing rather than mere luck. This opportunistic timing was spread over a significant number of CEOs and firms. We estimate that about 850 CEOs (about 10% of all CEOs) and about 720 firms (about 12% of all firms) received or provided manipulated lucky grants. In addition, about 550 additional grants at the second-lowest or third-lowest price of the month owe their status to manipulation.

We provide evidence that backdating, and not merely "spring-loading" based on the use of inside information, has been a major driver of the higher-than-random incidence of lucky grants. Opportunistic timing based on spring-loading is commonly viewed as raising less severe concerns that one based on backdating. Spring-loading is unlikely to enable differentiating between two stock prices that are very close together. However, we find that a day with the lowest price of the month was substantially more likely to be selected as a grant date than a day with the second lowest level even when the difference between the two price levels is less than one percent. Of course, if an option is backdated when the whole distribution of stock prices is known, one could choose to take advantage even of such small differences in prices.

We then turn to examine the characteristics of firms, CEOs, and grant circumstances that were correlated with lucky grants. We find that the occurrence of lucky grants was correlated with factors that are associated with increased influence of the CEO on the company's internal pay-setting and decision-making processes. Lucky grants were more likely to occur when the company did not have a majority of independent directors on the board. Furthermore, lucky grants were more likely to occur when the CEO had longer tenure. The contribution of increased tenure to a higher likelihood of getting a lucky grant was especially significant for CEOs hired from outside the firm; these CEOs started with a lower likelihood of lucky grants, but as their tenure increased, their incidences of lucky grants has increased at a faster pace, narrowing the difference in lucky grants incidence between them and CEOs hired from the inside. These findings are consistent with the view that grant date manipulation reflects governance problems.

Consistent with the view that manipulated timing reflects an economic decision, we find that lucky grants were more likely when the potential payoffs from manipulation are relatively high. Indeed, not only were lucky grants more common in companies with a volatile stock price but also, for a given CEO with more than one grant, the likelihood of an individual grant being lucky increased when the gap between the lowest and the median price in the month of the grant was higher.

Looking at the patterns over a CEO's service, we further find that luck has been persistent. The odds of a CEOs' grant being lucky were significantly higher when a preceding grant to the CEO was lucky as well.

We also test the conjecture put forward by various observers that backdating was rationally used by firms as a tax-advantaged substitute for other forms of compensation.[2] This view would predict that, all else constant, firms awarding lucky grants should tend to provide lower CEO compensation from other sources. We find, however, that CEOs benefiting from lucky grants received a significantly higher total compensation from other sources, not a lower one.

The cases that have come under scrutiny thus far have led to a widespread impression that grant manipulation has been largely or at least primarily concentrated in new economy firms. While we find that the odds of lucky grants have been somewhat higher in new economy firms, grant manipulation has been widespread in economy firms, and more than 80% of manipulated lucky grants have been given in such firms. Looking beyond the new economy /old economy dichotomy, we find a significant incidence of grant manipulation in each of the economy's twelve industries (using the Fama-French classification). Indeed, controlling for firm and grant financial characteristics, there is no statistically significant correlation between the odds of lucky grants and most industry classifications.

While much of our analysis focuses on grants awarded at monthly lows, we also extend our analysis to investigate manipulation within broader time period. We find that about 1,000 lucky grants (43% of all lucky grants) were "super-lucky," defined as grants awarded at the lowest price of the calendar quarter. We estimate that about 62% of all super-lucky grants owe their status to manipulation. We also estimate that about 11% of firms and about 7% of CEOs were involved in the awarding or receiving of super-lucky grants that were manipulated.

By identifying factors that have made grants more likely to be lucky even though they would not have had such an effect under random selection, our analysis allows us to identify certain pools of grants that are associated with a substantially higher incidence of

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[2] This possibility was raised, for example, by Wall Street Journal columnist Holman Jenkins jr. and by a Wall Street Journal editorial. See Jenkins, "Business World: The 'Backdating' Witch hunt," Wall Street Journal, June 21, 2006; "Backdating to the Future," October 12, 2006. The possibility that backdating has been partly driven by section 162(m) of the Tax Code, which limited to $1 million the deduction that companies may take for the nonperformance compensation paid to any given executive, was one of the reasons leading the Senate Finance Committee to schedule hearings on backdating and the tax treatment of executive pay.

manipulation. For example, we identify a pool of 600 grants in which 88% of the grants are estimated to have been manipulated.

Finally, we also derive an estimate of the gains to CEOs from backdating. It has been suggested that the value of backdating to CEOs has been rather limited (see, e.g., Walker (2006)). Our (conservative) estimates indicate that these gains were rather significant with an estimated average gain to CEOs from lucky grants that were manipulated exceeding 20% of the reported value of the grant and increasing the CEO's total reported compensation for the year by more than 10%.

The literature on the timing of option grants begins with the seminal work by Yermack (1997), showing that stock prices exhibit negative abnormal returns prior to a grant date and positive abnormal return afterwards. While Yermack attributes this pattern to the use of private inside information, Aboody and Kasznik (2000), and Chauvin and Shenoy (2001) suggest that it was partly due to manipulation of firms' information disclosures. The celebrated paper by Lie (2005) puts forward backdating as an important cause of the abnormal stock returns preceding and following grant dates. Heron and Lie (2006a), Narayanan and Seyhun (2006a), and Collins, Gong, and Li (2005) study how the patterns of pre-and post-grant returns were influenced by the adoption of SOX, which imposed a two-day filing requirement on firms making option grants, thus confirming the existence of backdating. Narayanan and Seyhun (2006b) find support in pre- and post-grant returns for the use of two different types of mis-dating techniques. Heron and Lie (2006b) use return patterns to show that a significant fraction of grants had their timing manipulated and to explore the correlation between the return patterns and firm characteristics.

We contribute to the literature in several ways. First, we establish a correlation between grant manipulation and governance, showing that timing was correlated with the lack of a majority of independent directors on the board. Second, we identify a connection between timing and CEOs' characteristics, showing that the likelihood of timing increased with CEO tenure. Third, we show that, over time, a given CEO was more likely to receive a lucky grant when the payoffs from such a lucky grant were higher. Fourth, we identify the persistence of CEO luck. Fifth, we show that lucky grants have been associated with higher total compensation to the CEO through all reported sources.

We also contribute to the literature by providing an alternative approach for studying option timing, one that is based on the ranking of the grant price within the price distribution of the month rather than one that is based on a comparison of pre- and post-abnormal returns. In particular, we show how grants at the bottom of the price distribution of the grant month – and especially lucky grants – can provide a useful tool for identifying links between timing opportunism and the characteristics of firms and CEOs. We are also able to use lucky grants to derive estimates for the incidence of and payoff from grant manipulation.

Our paper also contributes to the literature on the potential benefits of independent directors. Empirical work has not found a robust relationship between the presence of independent directors and firm value (see Bhagat and Black, 1999, 2002). There is evidence, whoever, that a majority of independent directors on the board has a significant impact on certain specific areas of corporate behavior (e.g., Byrd and Hickman, 1992; Shivdasani, 1993; Brickley, Coles, and Terry, 1994; Cotter, Shivdasani, and Zenner, 1997; Dann, Del Guercio, and Partch 2003; Gillette, Noe, and Rebello, 2003; Weisbach (1987)). We contribute to this literature by showing that the lack of a majority of independent directors is correlated with manipulated timing of option grants. This finding is consistent with recent work suggesting that independent directors might have an impact on executive compensation decisions (e.g., Core, Holthausen, and Larcker, 1999; Chhaochharia and Grinstein, 2006) and the incidences of fraud (e.g., Beasely, 1996, 2000; Dechow, Sloan and Sweeny, 1996).

In addition, our analysis contributes to understanding the significance of length of time a CEO has served in this position. Core, Holthausen, and Larcker (1999) and Cyert, Kang, and Kumar (2002) find that the CEO is more likely to get a high pay as well as a golden parachute when more of the outside directors have been appointed under the current CEO.

The remainder of our analysis is organized as follows. Section II describes our data and provides summary statistics. Section III examines the extent to which the incidence of lucky grants has been affected by opportunistic timing, as well as the extent to which such opportunistic timing has partly resulted from backdating rather than the use of private information. Section IV investigates the relation between option timing and governance arrangements, firm characteristics, CEO characteristics, and the payoffs from getting a lucky grant. Section V analyzes how the incidence of lucky grants varied across the economy's different industries. Section VI estimates the gains to CEOs from lucky grants. Section VII investigates whether firms providing lucky grants tended to pay CEOs less via other forms of compensation. Section VIII extends our analysis to examine grants whose grant price was lowest not only in the grant month but also in the calendar quarter. Section IX concludes.

II. PRICE RANKS: SIGNIFICANCE, DATA, AND SUMMARY STATISTICS

A. Detecting Option Grant Date Anomalies

The literature on the opportunistic timing of option grants (starting with Yermack (1997)) – and the more recent literature on backdating (Lie (2005), Heron and Lie (2005a, b), and Narayanan and Seyhun (2006a, b)) -- have focused on post- and pre-grant stock returns as their tool for detecting and investigating abnormal patterns. In particular, to detect patterns that could be the result of backdating, this research examined whether post-grant returns tended to be positive, whether pre-grant returns tended to be negative, and whether post-grant returns tended to exceed pre-grant returns. Post- and pre-grant returns have then been the tool used by this research to investigate the variables correlated with grant manipulation as well as to estimate the incidence of such manipulation.[3]

We use an alternative approach to investigate abnormal patterns by focusing on the rank of the price on the day of the option grant relative to the distribution of the prices of the month. Consider a grant that was provided in a given month, and suppose that the relevant decision-makers inside the firm reported the grant after the month and were willing to retroactively select a date with a favorable low stock price. In this case, one

[3] Heron and Lie (2006b) observe that grant dates are more likely to rank low rather than high in the distribution of prices, and Narayanan and Seyhun (2006b) show the existence of such tendency in post-SOX grants that are reported late, but these studies use pre- and post-grant returns as their main tool of analysis.

would expect the grant to have been reported as given at the lowest price of the month or, if the decision-makers wanted to err on the side of caution, at some other price at the bottom of the month's price distribution (e.g., the second-lowest or third-lowest price). Our strategy is therefore to examine whether at-the-money grants given at stock prices at the bottom of the price distribution were abnormally frequent. As a benchmark we compute the expected probability of the grant being given on a certain day of the month, based on the assumption that the grant date is chosen without regards to the price distribution.[4]

Looking at price ranks can be useful in zeroing in on instances of manipulation via backdating. Suppose that a company reported that it provided a grant in the middle of a month with an exercise price equal to the $100 price on the grant date. Suppose also that the price on the first day of the month (and prior to it) was $111, that the price at the last day of the month (and subsequently) was $110, and that the stock price was $90 in all other days of the month. In this case, the grant was preceded by a -10% stock return and followed by a +10% stock return. While these post- and pre-grant returns could reflect timing based on the use of inside information, a look at price ranks suggests that this grant is unlikely to have been backdated; the grant was awarded at the third-highest price of the month, and the grant's designers could have easily, and in the event of backdating would have likely, placed it on a day with a more favorable exercise price juts prior or just after the officially reported date.

Consider also a hypothetical case in which the stock price was relatively flat during the month, with the stock price equal to $101 in all days except for one day in which the price was $100, and suppose that the grant was reported to have been awarded on the date with the $100 stock price. In this case, the pre- and post-grant return patterns of -1% and +1% respectively, are consistent with timing but far from remarkable. A look at price ranks, however, indicates that the grant was awarded at the lowest price of the month, with the most favorable timing during the month that was at all possible.

[4] Although we refer to the benchmark as one of "random selection" of grant dates, this is not meant to involve a strictly random assignment but rather one in which grant dates are selected on the basis of factors that are independent of price rank consideration..

Price rankings thus provide a potentially useful method to detect abnormally favorable grant practices and correlating such practices to relevant variables. In this first comprehensive examination of grant practices based on price ranks, we use the grant month as the examined period for much of our analysis. That is, our inquiry focuses on how grant prices ranked within the price distribution of the grant month. This inquiry focuses, as it were, on investigating backdating instances in which the "look-back" period spanned a calendar month. [5]

While our choice of period enables us to focus on the backdating instances that were likely of greatest economic significance for CEOs and shareholders, our analysis does not and is not designed to capture fully instances of backdating based on small look-back periods. Narayanan and Seyhun (2006b) demonstrate that, especially during the post-SOX period, there have been likely numerous instances in which grants were mis-dated by just a few days, often by just one or two days. Thus, we should caution the reader that our analysis investigates an important subset of backdating practices, not all of them, and that the estimates we derive for manipulated grants in this subset are not estimates of the total number of manipulated grants.

B. The Data

We construct our dataset from Thomson Financial's insider trading database, which includes all insiders' filings of equity transactions in forms 3, 4, 5 and 144 between the years 1996-2006. In the course of constructing this dataset we use procedures similar to those used by Heron and Lie (2006a, b) and Narayanan and Seyhun (2006b). Our dataset includes observations with a cleanse indicator of R ("data verified through the cleansing process), H ("cleansed with a very high level of confidence"), or C ("a record added to nonderivative table or derivative table in order to correspond with a record on the opposing table"). We restrict our sample to transactions that occurred before 12/31/2005 (so that data about stock prices during the grant month is available in the 2005 CRSP database). We further require stock returns to be available for the entire month of the grant date. Finally, we include grants to the CEO, President, or Chairman of

[5] Later in the paper we show some robustness tests using the calendar quarter instead of the month.

the Board to address the possibility that CEOs sometimes identify themselves as Chairman or President in their SEC filings.

We eliminate any duplicate grants that occur on a given grant date so that there is only one grant for a given date and company combination. After eliminating multiple grants, our sample consists of 41,397 grants. From this sample we eliminate grants that are scheduled, which might be less likely to have been manipulated. A grant is defined as a scheduled grant if the CEO received a grant on the same date plus/minus one day in the preceding year. We also eliminate grants which were given in months where the firm had an ex-date of a dividend; to the extent that firms schedule grants after a dividend's x-date, the grant price might fall below the stock prices preceding the x-date even in the absence of any backdating or spring loading.

Finally, we check whether the strike price of the grant is close enough to the closing price of the grant date, or to the closing price of a day before or a day after the grant. A close enough price is defined as a price that is within 1% of the strike price. The date with the closest closing price to the strike price is then defined as the effective grant date.[6] The dataset constructed along the above lines contains about 19,000 grants in about 6000 firms.

C. Summary Statistics

Table 1 shows the distribution of the grants depending on the grant day price-rank during the calendar month of the grant.

The last two columns show the percentage of grants whose grant price was below and above the median price of the month. 56% of the grants in our sample were given at a strike price below the median price, compared with only 38% that were given at a price above the median (6% of the grants are given exactly at the median price): a difference of 18%. We also see that the asymmetry of the distribution was greater when the grant was given before the adoption of SOX than afterwards.

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[6] Consistent with Heron and Lie (2006a), we are also able to allocate the strike prices of about half of the grants in the sample. Heron and Lie discuss in detail the possible reasons for deviation from the strike price.

Table 1 also displays the changes over time in the incidence of grants below and above the median. The asymmetry of the distribution peaks in 2001, with a 27% difference between the below-median and above-medina groups, and then declines sharply after SOX.

Table 1 also provides statistics about the percentage of grants at given price ranks. Overall, we observe a clear monotonic relation between the rank of the price in a month and the percentage of grants given at that level. For the full sample, the frequency of grants is the highest at the lowest price of the month (12%), second-highest at the second-lowest price of the month (9%), third-highest at the third-lowest price level (8%), and so forth. Conversely, the frequency of grants is lowest at the highest price level (4%), second-lowest at the second-highest level (5%), and so forth.

We find that much of the "action" is at the top and bottom parts of the price-rank distribution with a large difference between the incidences of grants at the lowest and highest prices of the month. In fact, 12% of grants were given at the lowest price of the month but only 4% were given at the highest price of the month, with the difference being even bigger (15% vs. 4%) prior to the adoption of SOX. Needless to say, such a difference would not be expected if grant dates were randomly selected. The difference between the second-lowest and the second-highest groups is smaller but still substantial – 9% vs. 5%. And the differences continue to narrow as one moves further away from the extremes of the price distribution.

Our sample contains many CEOs who received more than one grant, as well as many firms that awarded grants to two or more CEOs during the considered period.[7] Thus, one might wonder whether the grants producing the asymmetry displayed in Table 1 are concentrated in a relatively small number of CEOs and firms. To get a sense whether this is the case, Table 2 displays the distribution of grant prices across CEOs and firms.

Table 2, panel A shows that 45% of CEOs had at least one grant at one of the three lowest prices of the month, but only 26% had at least one grant at one of the three

[7] In our sample, 4510 CEOs received one grant, 1874 received two grants, 1050 received three grants, and 1386 received four or more grants. Also, 3510 firms in our sample have one CEO, 1560 have two CEOs, and 697 firms have three or more CEOs (Table 2).

highest prices of the month. Similarly, while 22% of CEOs had at least one grant at the lowest price of the month, only 9% had at least one grant at the highest price of the month. These figures suggest that the asymmetry in the incidence of grants at the bottom and top of the price distribution is not due to a small number of CEOs.

Table 2, panel B similarly shows that the asymmetry is not due to a small number of firms that among them manipulated a large number of grants. While 58% of firms gave one or more grant at one of the three lowest prices of the month, only 35% of firms gave one or more grant at one of the three highest prices. Furthermore, 30% of firms gave one or more grant at the lowest price of the month, compared with 12% that gave one or more grant at the highest price of the month.

Table 3 shows univariate statistics on the differences between grants that were lucky and other grants (panel A) and differences in the incidence of lucky grants among different groups of grants (panel B). The Table indicates that lucky grants were more frequent (at 1% significance):

- in months in which the difference between the lowest and the median price of the month was higher;
- before SOX was adopted;
- in firms with below-median size;
- in new economy firms;
- among grants provided to CEOs with longer tenure and/or ownership stake exceeding 5%;
- in companies without a majority of independent directors on the board;
- in companies without an independent compensation committee; and
- when a preceding grant to the CEO was lucky.

We shall discuss these relations in greater detail below when we run multivariate regressions.

III. CEOs' Luck

A. Mere Luck?

To evaluate whether and how the selection of days to serve as grant dates deviated from random, we run the following logit regression over all the days in each of the months in which a grant is given:

$$Is\_Grant_{it} = a0 + a1 * Dummy\_Three\_lowest\_prices_{it} + a2 * Dummy\_Three\_highest\_prices_{it} + e_{it} \quad (1)$$

where Is_Grant_{it} is a dummy variable which equals one if at date t firm i granted options to the CEO, and zero otherwise. $Dummy\_Three\_lowest\_prices_{it}$ is a dummy variable which equals one if the price at date t was one of the three lowest prices of the month, and $Dummy\_Three\_highest\_prices_{it}$ is a dummy variable which equals one if the price at date t was one of the three highest prices of the month and zero otherwise. We cluster the errors by CEOs. The clustering corrects for correlations in the error terms $\{e_{it}\}$ across grants that are given to the same CEO. Table 4, column 1 shows the results of the logit regression (1). The coefficient of the $Dummy\_Three\_lowest\_prices_{it}$ variable is 0.531 and the coefficient of the $Dummy\_Three\_highest\_prices_{it}$ is -0.179. Both coefficients are statistically different from zero at the 1% level. Thus, for any given trading day that was a potential candidate for selection as grant date, having a stock price that is one of the three lowest prices of the month makes that day more likely to be selected as a grant date, and having a day with a price that is one of the three highest prices of the month makes that day less likely to be selected as a grant date. In a logit regression, the coefficients are the log of the odds that a date will be chosen as a grant date. Thus, relative to the default of a day that is not among the three lowest or three highest, a day with a price among the three lowest prices of the month will have odds that are exp(0.531) = 1.70 times larger (that is, 70% higher) to be selected as a grant date, and a day with a price among the three highest will have odds that are exp(-0.179) = 0.88 times smaller (that is, 12% lower) to be chosen as a grant date.

Because SOX required reporting option grants within two days after the grant is given, grant timing manipulation can be expected to be less prominent after SOX (Heron and Lie (2006a), Narayanan and Seyhun (2006a), Collins, Gong, and Li (2005)). As Heron and Lie (2006a) and Narayanan and Seyhun (2006b) show, however, more than 20% of companies did not comply with the two-day filing requirement during the post-SOX period, and SOX therefore could not eliminate manipulation altogether. To take the difference between the pre- and post-SOX periods into account, we re-run the regression (1) interacting the explanatory variables with dummies for whether the grant was given before SOX or after SOX.

We present the results in column 2 of Table 4. The coefficient of the $\text{Dummy\_Three\_lowest\_prices}_{it}$ variable is 0.585 for the pre-SOX period and 0.406 for the post-SOX period. Again, both coefficients are statistically significantly different from zero at the 1% level. Thus, the results indicate that SOX did not bring an end to the higher-than-random selection of days at the bottom of the distribution. A test of a difference between the two coefficients, however, indicates that the pre-sox coefficient is higher than the post-sox coefficient. This result is consistent with SOX reducing the incidences of grant manipulation.

Also, column 2 indicates that the coefficient of the $\text{Dummy\_Three\_highest\_prices}_{it}$ variable is -0.238 for the pre-SOX period and -0.064 for the post-SOX period. The former coefficient is statistically different from zero at the 1% level, the latter only at the 10% level, and a test of the difference between the two coefficients indicates that the pre-SOX coefficient is lower than the post-SOX coefficient. Thus, the results are consistent with SOX reducing but not eliminating the moving of grant dates away from the three highest prices of the month.

B. The Monotonic Relation between Price Rank and Likelihood of Granting Options

Having lumped together the three lowest price levels, as well as the three highest price levels, we now explore how levels within each group differ. Specifically, we run the following regression:

$$Is\_Grant_{it} = a0 + a1 * Dummy\_lowest\_price_{it} + a2 * Dummy\_2^{nd}\ lowest\_price_{it} + \ldots + a4 * Dummy\_4^{th}\ lowest\ price_{it} + b4 * Dummy\_4^{th}\ highest\_price_{it} + \ldots + b1 * highest\_price_{it} + e_{it} \quad (2)$$

We again cluster the errors by CEOs. The clustering corrects for correlations in the error terms $\{e_{it}\}$ across grants that are given to the same CEO. We present the results in Table 5.

The results in Table 5 column 1 show a monotonic relation between the likelihood of getting a grant on a particular date and the rank of the price on that date. We form a series of t-tests of differences between adjacent coefficients and reject the null of no differences. The results are also economically significant. For example, the coefficient on the $Dummy\_lowest\_price_{it}$ is 0.885, implying that if the date has the lowest price of the month, the odds of giving a grant on that date increase by a factor of exp(0.885) = 2.4 (or by 140%). Conversely, the coefficient of the highest price is -0.211, implying that if the date has the highest price of the month, the odds of giving a grant on that date decreases by a factor of exp (-0.211) = 0.81 (or by 19%).

Column 2 shows the results where each of the coefficients in (2) is interacted with a dummy variable for whether the grant was given before or after SOX. Consistent with the results in Table 2, dates at the bottom of the distribution were each more likely to be selected before SOX than after SOX, though each of them still remained after SOX more likely to occur than under random assignment. Moreover, both before SOX and after SOX, the likelihood of selection went down monotonically from the highest to the lowest price of the month price of the month.

D. Estimating the Percentage of Manipulated Grants

Having seen that the lowest three prices have been selected more often than under random assignment, and that days with the three highest prices have been selected less often, we now turn to estimate the number of grants that have been manipulated in one direction or another. For every price rank included in Table 1, we calculate the expected number of grants with that price rank if grants were randomly assigned over the trading days during the grant month.[8] This estimation is done by calculating for each individual grant, assuming random assignment, the probability of being granted at the specific price rank, and then aggregating these probabilities across all grants. Because of the large number of grants involved, a random assignment is highly unlikely to deviate significantly from the expected number we calculate.

The difference between the actual number of grants in any price rank and the expected number provides our estimate for the number of grants whose timing was manipulated. This estimation method is conservative because it assumes that, for each price rank, all manipulation was done in one direction, either into or out of that price rank. For example, we find that the number of grants given at the third lowest price of the month exceeds the expected number by 112 grants. This assumes that the manipulation only takes the form of moving the 112 grants from higher price ranks to this category. However, if some grants were moved from the third-lowest-price category to the lowest- or second-lowest, then more than 112 grants had to move to the third-lowest category from higher ranks, and thus more than 112 grants of those reported with the third-lowest-price had to be manipulated.

Table 6 shows our estimation results. We estimate that over the full sample period of 1996-2005, 1163 lucky grants – about 50% of all lucky grants – were manipulated. The percentage of lucky grants that were lucky due to manipulation was about 55% before SOX and 35% afterwards. Relative to the total number of grants, about 6% of the total grants were manipulated to occur at the lowest price of the month.

We find a somewhat smaller but still substantial fraction of manipulated grants among grants given at the second- and third-lowest prices of the month. For grants with the second-lowest (third-lowest) price of the month, we estimate that about 23% (11%) are manipulated. Our estimates of the fraction of manipulated grants in these categories are about the same before SOX and after SOX. Overall, we estimate that, during 1996-2005, there were about 1700 grants that were placed in one of the three lowest prices due

[8] The scenario of random assignment also assumes that, after the day is randomly selected, the distribution of prices among the month's different days is not manipulated or affected by the choice of grant date. The probability of a day being the lowest price day is computed by the ratio of the number of days in the grant month that have the lowest price to the number of trading days in that firm’s stock during the grant month.

to manipulation. Such grants comprised 9% of all the grants awarded during this period (11.4% before SOX). [9]

Once we move to price ranks above the third-lowest price, we find that the number of actual grants does not significantly exceed the estimated number under random assignment even for the fourth-lowest and fifth-lowest price categories. Thus, the aggregate number of actual grants in these categories does not provide evidence of significant manipulation. Note, however, that the aggregate number of grants in these categories could be the product of a significant number of grants moving to these categories from higher price ranks and a roughly similar number moving from these categories to lower price ranks.

Finally, with respect to grants given at the highest prices of the month, the actual number of grants is significantly below the estimated number under random assignment. For each of the highest-, second-highest, and third-highest categories, the actual number of grants was lower by more than 30% relative to the estimated number under random assignment. Of course, in the event of opportunistic timing, these categories are more valuable to avoid and costly to move into.

Table 6 also gives a sense of the magnitude of the discount in exercise price that manipulation could produce. For the category of lucky grants, the grant price was on average 12% lower than the median price of the month.

Table 7 provides our estimates of the number of CEOs that received, and the number of firms that provided manipulated grants. Again, our estimation methodology is to calculate the difference between actual numbers and the ones expected under random assignment. The table indicates that the number of CEOs with one or more lucky grants (1931) exceeds the number estimated under random assignment by about 850. The estimated number of CEOs receiving one or more lucky grants due to manipulation comprises about 10% of all CEOs in our sample. With respect to firms, the number of

[9] Our estimate for manipulated grants at one of the lowest three prices of the months is consistent with the higher figure estimated by Heron and Lie (2006b) for the total number of manipulated grants. As discussed in Section II, we do not attempt to capture "small-scale" backdating in which grants were mis-dated by a small number of days, whereas the Heron-Lie methodology which is based on comparison of pre- and post-grant returns attempts to capture such instances of manipulation as well.

those providing one or more lucky grants exceeds the estimated number under random assignment by about 720. This figure implies that about 12% of all firms in our sample provided one or more lucky grants due to manipulation.

D. Backdating or Spring-Loading?

Deviations from patterns expected under random assignment might be not only due to backdating but also due to spring-loading based on private information (e.g., Yermack, 1997). Having found that many lucky grants owe their presence in this category to manipulation, we turn to examine the possibility that such manipulation was largely driven by spring loading rather than backdating. To examine this possibility, we conduct three tests of the hypothesis that the excess incidence of lucky grants was largely due to spring loading.[10]

The first test we conduct focuses on grants awarded in months in which the difference between the lowest and second-lowest prices of the month was very small. In such cases, it is implausible that insiders would view one price level as reflecting significant under-valuation but not the other. Accordingly, under the spring loading hypothesis, one would not expect a significant difference in the odds of selecting as a grant date the lowest versus the second-lowest price day. In contrast, in the event of backdating selecting the best price available in retrospect, such a difference can still be expected even when the lowest and second-lowest prices are very little apart.

We therefore pick from our database only grants given in a month in which the difference between the lowest and second-lowest prices is less than 1%. About half of the grants (9684 grants) fall into this category. We then run the following regression:

$$\text{Is\_Grant}_{it} = a0 + a1 * \text{Dummy\_lowest\_price}_{it} + a2 * \text{Dummy\_second\_lowest\_price}_{it} \qquad (3)$$

[10] Our tests complement the work of Lie (2005), Heron and Lie (2006a), and Narayanan and Seyhun (2006b) who show that backdating has been a major driver of the abnormal patterns of pre- and post-grant returns. Because our focus below is on a subset of manipulated grants – those resulting in grants at the lowest price of the month – we seek to confirm that backdating has played a substantial role in this important subset of manipulated grants.

Table 8 column 1 shows the results of regression (3). The coefficients a1 and a2 are both positive and statistically significant. However, the coefficient a1 is significantly larger than the coefficient a2. The a1 coefficient is 0.582 and the a2 coefficient is 0.381. Therefore, the odds that the grant is given at the lowest price of the month are exp(0.582)=1.72 times higher than they are given on other days, while the odds that the grant is given at the second-lowest price of the month are only exp(0.381)=1.46 times higher. The difference between the coefficients is significantly different from zero at the 1% level. This result is inconsistent with the view that the excess incidence of lucky grants is solely the product of spring loading.

Table 8 column 2 shows the result of a version of regression (3) where the sample consists of only the days of the month in which the price is the lowest or the second lowest, and the regression has only the lowest-price dummy variable. Absent any backdating, we should expect an even distribution between grants that are given at the lowest price of the month and grants that are given at the second-lowest price of the month, since we should not expect managers to target exactly the lowest price of the month with information releases. Therefore, the coefficient of the lowest price dummy should equal zero. However, the coefficient is 0.1661 and is statistically significantly different from zero. Thus, the odds of picking the lowest day of the month are exp(0.1661)=1.18 times higher than the odds of picking the second lowest price of the month.

Our second test for whether the excess of lucky grants was driven by spring loading is based on when the company reported the grant to the SEC.[11] Under the spring loading hypothesis, grant dates are chosen on the basis of the favorable private information that insiders had at the time of the grant. Thus, under this hypothesis, the odds of a lucky grant are not expected to depend on how long after the grant date reporting occurred. In contrast, if grant dates are manipulated to backdate the grant at the lowest price, then reporting the grant in the following month only, or at least later in that month, facilitates the selection of the lowest price of the month as the grant price.

[11] Heron and Lie (2006a) and Narayanan and Seyhun (2006b)) analyze how the pre- and post-grant returns accompanying grants have been influenced by when the company reported the grant.

To distinguish between grants that are reported close to the grant date and grants that are filed later, we introduce two dummy variables: Reported_same_month which equals one if the filing with the SEC occurs in the same month as the grant and zero otherwise; And Reported_next_month which equals one if the filing date is in the month following the grant month or later. About 33% of the grants in our sample were filed in the same month as the grant month. (80% of those after SOX and 6% of those preceding SOX.) We then run the following regression:

$$\text{Is\_Grant}_{it} = a0 + a1 * \text{Dummy\_lowest\_price}_{it} * \text{Reported\_same\_month} + a2 * \text{Dummy\_lowest\_price}_{it} * \text{Reported\_next\_month} + + e_{it} \qquad (4)$$

Under the spring loading hypothesis, the filing month should be irrelevant. Accordingly, the hypothesis predicts that we should see no differences between the coefficients a1 and a2.

We show the results in Table 9. The coefficient of a2 is larger than the coefficient of a1 by 0.406, and a t-test rejects the null that the two coefficients are the same. The odds of a lucky grant that is reported in the same month is exp(0.557) =1.74 times as high as those reported on other dates, and the odds of a lucky grant reported in the next month only is exp(0.963)=2.62 as high.

Our third test of the spring-loading hypothesis is based on the idea that insiders are likely to have private firm-specific information but unlikely to have such information about the future direction of the stock market (Lie, 2005). Table 10 shows the results of a regression where the dependent variable is a dummy variable equal to one if the firm gave a grant on that day and zero for all other days of the month. Only months with grants are included in the regression. The independent variable of interest is the return of the stock from the grant date until the end of the month. In column 1 the explanatory variable is the raw return, and in column 2 we decompose the return into the market return component (using the CRSP value-weighted return), and the idiosyncratic component.

The results in Columns 1 – 2 suggest that both the idiosyncratic component of the return and the market component of the return are positively related to the likelihood that a grant will be given on the specific date. Both components are significant at the 1% significance level. This result reinforces the conclusion that backdating has played a significant role in producing the higher-than-expected incidence of lucky grants.

IV. The Determinants of Luck

We have identified a significant presence of manipulated grants among grants given at one of the lowest three prices of the month – and especially among lucky grants given at monthly lows. Because a large fraction of lucky grants owe their status to manipulation, lucky grants provide a useful tool for studying the factors likely to be associated with manipulation, and we now turn to pursue this inquiry.

A. Grant Circumstances and Firm Characteristics

We begin our inquiry with factors for which the necessary data is in Thomson and CRSP and thus enables us to conduct tests based on our grant dataset as a whole. We run the following regression of whether a grant was lucky on various explanatory variables:

$$\text{Lucky}_{it} = [\text{FIRM CHARACTERISTICS}_{it}] + [\text{GRANT CHARACTERISTICS}_{it}] + \text{SOX}_{it} + e_{it} \quad (5)$$

In some of the specifications, we include firm and CEO fixed effects. When these are not included, we cluster the errors by CEOs to correct for potential correlations across the likelihood of lucky grants among the same CEOs.

The first firm characteristic we use in the regression is size. Smaller firms might have less outside scrutiny and less visibility, making grant manipulation less likely to be detected by outsiders. Our variable for size is the natural log of relative market capitalization – defined as the ratio of the market capitalization of the firm at the grant date divided by the median market capitalization of all firms that gave a grant during that year.

We also use a variable that classifies firms into new and old economy firms following the definition in Murphy (2003). Option grants practices can develop and spread within industries in new economy firms as industry insiders (and even their advisers) are more likely to network with each other than with those in old economy firms. And the fact that most of the backdating cases identified thus far have been of new economy firms, such manipulation is now viewed by many as likely to have been concentrated among such firms.

We also include in the regression the percentage difference between the lowest and median price of the grant month (in log). This variable is used as a proxy for the potential payoffs from turning a grant actually given on another day into a lucky grant. If manipulation were an economic decision determined by payoffs, then a higher propensity of grants should be lucky in months with high lowest-to-median price differences. Alternatively, if the manipulation is a standard practice in some clusters of firms without attempting to maximize insider utility, then we should not find a significant association. We also use a decomposition of the lowest-median difference into the market component and the firm-specific component.

Since SOX imposes stricter reporting requirements, we use a dummy variable equal to one if the grant was given post-SOX to control for the change in reporting requirements.

Finally, even under random selection of dates, a grant would be more likely to be lucky when more trading dates in the month had a price equal to the lowest price level of the month. Also, even when there is only one day with this price level, the probability that it would be selected is lower when the month has more trading days. We therefore add an additional control equal to the ratio of the number of days in the month of the grant with closing prices equal to the lowest price of the month to the number of trading days in the firm's stock in the grant month.

Table 11 displays our results. Column 1 is a pooled regression. In terms of firm characteristics, we find that the lucky grants are more common (with 1% significance) in firms whose relative size is smaller. Lucky grants are also more common (again, with 1% significance) in new economy firms.

In terms of grant circumstances, grants were more likely to be lucky (at 1% significance) before SOX. Furthermore, consistent with the possibility that the incidence of manipulation was responsive to its potential payoffs, grants were more likely to be lucky (again, at 1% significance) when the difference between the lowest and the median price level was large. This result is consistent with Heron and Lie (2006b), who find that firms with higher volatility of stock returns are more likely to time their grants. As we shall see, however, our result is driven not only by differences in volatility across firms but also by differences over time for any given CEO and firm in the potential payoffs from backdating.

Column 2 displays the results of a regression in which we decompose the difference between the lowest and median price of the grant month into its market and firm-specific components. Consistent with the results in our third test of backdating vs. spring-loading in the preceding section, both components matter. The coefficient of each one of them is positive and significant.

Columns 4-7 of Table 11 show the results of fixed effect regressions. Columns 4-5 are like columns 1-2 but with firm fixed effects, and columns 6-7 correspond to 1-2 but with CEO fixed effects. These regressions enable us to control for all (stable) characteristics of firms and CEOs that we do not have in our regression. The results indicate that, even after controlling for CEO and firm fixed effects, the coefficient of the lowest-median difference remains positive and significant. These results indicate that our findings in columns 1-2 are not all due to cross-sectional differences, i.e., differences between firm types such as high-volatility and low-volatility firms. For any given firm that gives multiple grants over time, grants are more likely to be lucky in months in which the difference between the lowest and the median price is relatively large. Similarly, for any given CEO who receives multiple grants over time, lucky grants are more likely in months that have a higher lowest-median difference. Consistent with the results in column 2, columns 5 and 7 indicate that both the market component and the firm-specific component of the lowest-median price difference explain incidences of lucky grants.

B. CEO Characteristics

In this section, our main interest is in factors associated with the influence and power that the CEO has over the directors. To the extent that backdating was merely the product of a rational business decision by the firm to provide non-performance compensation in this form, the incidence of lucky grants should not be expected to correlate with such factors. However, if backdating was produced by agency problems and governance failures, then lucky grants can be expected to correlate with such factors.

For the analysis in this section, we rely on CEO variables that are available in the ExecuComp dataset, which restricts our sample to about 6000 observations.

We begin with the regression specification in (4) and we add to it CEO characteristics in two steps. Our results are presented in the first three columns of Table 12. Column 1 of the table presents the results of a regression that begins with the benchmark specification of Table 11 and adds to it two CEO characteristics. The first is tenure of the CEO in the firm (in logs). The longer the tenure, the more influence the CEO is likely to have on directors and internal pay practices (e.g., Core, Holthausen, and Larcker 1999; Cyert, Kang, and Kumar, 2002; Harford and Lie, 2006). As column 1 indicates, the coefficient of this variable is positive and significant at 1% level, consistent with the view that a longer tenure makes grants more likely to be lucky.

The second variable, Outsider, is a dummy variable for whether the CEO came to the position from outside the company. An outside CEO is expected, at least initially, to have less influence on the directors and the firm's internal pay process and be more cautious about using practices that might be regarded as aggressive. The coefficient on Outsider in Column 1's regression is indeed negative but it is not statistically significant from zero.

Since the influence on directors and internal processes of a, say, CEO with a five-year tenure, is unlikely to depend significantly on whether the CEO was initially hired from the outside or inside, we investigate the possible significance of Outsider in a second way. In the regression reported in Column 2, we replace tenure with two different variables – tenure for CEOs hired from the outside and tenure for CEOs hired from the inside. Our conjecture is that tenure is more critical for obtaining familiarity with and influence over the firm's internal processes for CEOs hired from the outside. Consistent with this view, we find that the coefficient of tenure is higher for outside CEOs than for inside CEOs in a large and statistically significant way. The coefficient of tenure for insiders is still positive and significant at 5%, indicating that tenure still matters for inside CEOs, albeit less than for outside CEOs.

Our next step is to see whether the CEO's ownership stake is correlated with the CEO's chances of getting lucky grants. The relation between executive ownership and various aspects of firm performance and behavior has been explored in the past in different contexts (for example, McConnell, and Servaes, 1995; Morck, Shleifer and Vishny, 1988; Holderness and Sheehan, 1988). To explore this issue, we introduce two dummy variables. One for CEOs with more than 5% but less than 25% stock ownership, and one for CEOs with more than 25%. Column 3 displays the results of the logit regression including these variables. We find that grants to CEOs with stakes between 5% and 25% are more likely (with 5% significance) to be lucky. This result should be treated with caution because the coefficient remains positive but loses its significance in subsequent regressions in which director independence is added.

C. Director Independence

The final variables we add are taken from the IRRC dataset and measure director independence. Because our interest is in examining whether lucky grants are more likely in the presence of factors associated with greater influence of the CEO on directors, such variables are naturally relevant.

Director independence has been viewed as an instrument of improving board oversight in general and oversight over executive compensation in particular. Case law has therefore long encouraged, and recent stock exchanges require, independent compensation committees. Similarly, having a majority of independent directors on the board has been long considered a good governance practice and has been recently mandated by stock exchange requirements. While formal independence requirements are not sufficient to eliminate CEO influence on directors (Bebchuk and Fried, 2004), they reduce it.

We therefore add to our regressions dummy variables for whether the board has an independent compensation committee and for whether the board has a majority of independent directors. Our results are displayed in the last three columns of Table 12. Column 4 of Table 12 uses only the independent committee dummy; column 5 uses only the independent board dummy; and column 6 uses both.

The coefficient on the independent board dummy is negative and significant at the 1% level both when the independent compensation committee dummy is and is not included. This result indicates a correlation between lucky grants and boards that lack a majority of independent directors. The size of the coefficient implies that having a majority of independent directors on the board reduces the odds of a lucky grant by 1-exp(-0.411)=33%. Thus, grant manipulation might be one of the contexts in which having a majority of independent directors makes a difference.[12]

In contrast, the coefficient on the independent compensation committee dummy is not significant either with or without the board independence dummy. It might be that lack of majority of independent directors on the board as a whole weakens the position of outside directors and thus undermines their effectiveness even when they serve on independent committees. This view is partly reflected in the exchanges' decisions to require a majority of independent directors on the board as a whole and not only the independence of key committees such as audit, nomination, and compensation.

Finally, we should note that the significance of tenure does not go away (or even substantially change in magnitude) when director independence variables are added. This result is consistent with the widespread view that the presence or absence of a majority of formally independent directors on the board does not fully determine the extent to which the board is truly independent of the CEO in making compensation and oversight decisions.[13]

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[12] As noted in the introduction, while research has not found a link between board independence and better corporate performance in general, it has identified some specific types of decisions for which such independence matters (e.g., Cotter, Shivdasani and Zenner (1997), Chhaochharia and Grinstein (2006)).

[13] In addition to director independence, there might well be other characteristics of serving directors which might be relevant to the odds of lucky grants and which our analysis does not identify. In particular, in a current study that complements our work, Bizjak, Lemmon, and Whitby (2006) show a link between the spread of option backdating and interlocking directors.

D. Serial Luck

The preceding subsections have identified a number of variables that are correlated with lucky grants. Undoubtedly, there are CEO and firm traits that could affect the incidence of lucky grants but were not included. Indeed, characteristics such as aspects of the CEO's personality and the firm's compensation staff might be difficult or impossible for researchers to observe. However, to the extent that such traits exist, one would expect luck to be "serial" or "persistent". That is, controlling for all the variables thus far used, one would still expect a grant to be more likely to be lucky if a preceding grant was lucky. Such persistence would not be expected, of course, under random selection.

To examine the existence and magnitude of such persistence, we re-run the regressions in tables 12 and 13, but this time add two dummy variables. One dummy variable is equal to one when the CEO has a preceding grant in our dataset and it was lucky. The other dummy variable is equal to one if the CEO has a preceding grant in our dataset and it was <u>not</u> lucky. (Our default is therefore executive grants that were not preceded in our dataset by another grant to the same executive.)

Table 13 displays the results of all the key regressions with the two dummy variables added to them. In all the regressions, the coefficient of the previous lucky dummy is positive and significant at the 1% level. The coefficient is on the order of 0.6, which implies that having a preceding grant that was lucky increases the odds of a CEO's current grant being lucky by 82% (relative to a CEO for whom we have no information whether the previous executive grant was lucky). In contrast, the coefficient on the dummy for having a preceding grant that was not lucky (which lumps together all other price ranks, including preceding grants at the second-lowest price of the month) is negative but not statistically significant in all the regressions. Thus, our results indicate that there are additional factors beyond those identified by us that make lucky grants more likely even though those factors are not expected to influence the probability of luck under random selection. Identifying such additional factors might be a worthwhile task for future research.

E. Zeroing in on Manipulated Grants

The results in this section identify several variables that are associated with an increased likelihood of a grant being lucky even though these variables cannot be expected to have such an effect in the absence of manipulation. Thus, pools of grants with these characteristics can be expected to have more incidents of lucky grants with a higher fraction of them owing their status to manipulation. That is, our results enable us to identify pools of lucky grants that are especially likely to have been manipulated.

Table 14 displays results with respect to classes of grants that according to our analysis can be expected to display a high probability of being unexpectedly lucky. We form such classes by focusing on three key variables: 1) The highest volatility quartile (decile), with volatility defined as before as the difference between the lowest and the median price in the grant's month. 2) A dummy variable that equals one if the CEO received a previous grant that was lucky. 3) Lack of independent board, a variable which is only available for a subset of firms with IRRC data available.

The first column of Table 14 shows the number of observations per class, the second column displays the actual number of lucky grants, and the third column shows the ratio of actual lucky grants to total number of observations. The fourth and fifth columns display our estimate of the percentage of lucky grants in the pool of grants that were manipulated and of the number of manipulated grants in the pool. Panel A shows statistics for the pre-SOX period, and panel B for the post-SOX period.

Pre-SOX, in our sample as a whole, 55% of all lucky grants were unexpectedly granted at the lowest price and thus can be estimated to have been manipulated. However, in the class of grants with the highest volatility quartile (decile) this fraction increases to 71% (76%). In the class of pre-SOX grants that were preceded by lucky grants, we estimate that 74% of the lucky grants owed their status to manipulation. Finally, in the class of pre-SOX grants by companies which lacked a majority of independent directors, 65% of the lucky granted are estimated to have been manipulated.[14] When we put together the three variables to create a pool of grants that have at least one of the above

[14] In Table 14, the number of pre-SOX lucky grants by companies without a majority of independent directors (and the subset of manipulated lucky grants y such companies) is small relative to the size of the other classes reported because we have data about director independence only for the limited subset of companies for which IRRC data is available.

three variables (high volatility, preceding lucky grant, or lack of board independence), we obtain a pool of about 4,000 grants in which 19% of the grants (761 grants) were lucky with 69% of the lucky grants (524 grants) estimated to have been manipulated.

We next look at the pool of grants that have at least two of the variables we focus on. The pool of lucky grants defined in this way has an especially high fraction of unexpected, manipulated grants. Indeed, when being in the highest volatility decile is used for defining the pool, we obtain a pool of lucky grants out of which 83% are expected to have been manipulated.

Finally, panel B displays the results of such an analysis for post-SOX grants. Because the incidence of lucky grants is lower post-SOX, the pools of post-SOX lucky grants are smaller. However, the results enable us to identify also post-SOX pools with a high incidence of lucky grants (relative to the post-SOX sample as a whole) and thus pools of lucky grants in which the fraction of manipulated grants is high.

V. LUCK AROUND THE ECONOMY

A. Luck in and out of the New Economy

Because most of the backdating cases that have thus far been uncovered involve new economy firms, there is a widespread impression that grant date manipulation was concentrated in the new economy sector (see, e.g., Walker 2006). While the regressions reported earlier indicate that a new economy classification increases the likelihood of a grant being lucky, they do not indicate that such classification is a critical factor. Other factors -- such as the difference between the month's lowest and median price, the CEO's tenure, and director independence – also have explanatory power over whether a grant is lucky.

Looking at the regression results, however, we cannot rule out the possibility that manipulated grants are concentrated in new economy firms. It might be that firms with large differences between the grant month's lowest and median prices are mainly new economy firms and thus most of the manipulated grants involve new economy firms. We explore this question by replicating our estimation of the incidence of manipulated grants but do so for the set of new and old economy firms separately, following the classification into new versus old economy firm of Murphy (2003).

The results are shown in Table 15, and they indicate that grant manipulation was not practice largely limited to the new economy. While the incidence of manipulation is somewhat smaller among old economy (48% of actual grants) firms than among new economy firms (59%), most of the manipulated grants did take place in old economy firms. In particular, out of the estimated 1163 manipulated grants for the sample as a whole (see Table 6), we estimate that 944 (more than 80%) involve grants given by old economy firms.

Similarly, the incidence of firms providing one or manipulated grants somewhat smaller among old economy firms (12%) than it is among new economy firms (16%); and the incidence of CEOs receiving one or more manipulated lucky grants is smaller among old economy CEOs (9%) than it is among new economy CEOs (12%). Again, however, given the much larger number of old economy firms, more than 80% of the firms providing and the CEOs receiving manipulated lucky grants were from the old economy. Taken together, these finding indicate that grant manipulation has not been largely or even primarily limited to new economy firms

Given the above findings, the question that naturally arises is why the firms that have thus far come under scrutiny have been disproportionately new economy firms. Our conjecture is that, while the use of manipulated grants has not been concentrated in new economy firms, such firms might have been especially aggressive in their use of such manipulation. The companies that have become the focus of investigation thus far are the ones in which the abnormal returns before and after the grant date were especially salient and pronounced. Thus, to the extent that old economy firms were somewhat as likely to provide grants at the lowest price of the month, but considerably less likely than new economy firms to give grants at, say, the lowest price of the year, such a difference could have led to backdating by new economy firms being disproportionately noticed by regulators and the media in their initial search for backdating firms.

B. Luck by Industry

We now turn to look beyond the new/old economy division how manipulated lucky grants vary across the economy's industries. The thousands of old economy firms that are publicly traded span, of course, diverse industries, and even new-economy firms span three different industries using the twelve Fama-French Industry definition (Business Equipment, Telecom, and Shops).[15] In this section we analyze the propensity of manipulated grants across the twelve Fama-French Industries.[16]

Table 15 shows the results of our analysis. The table is ordered by the estimated fraction of manipulated grants in the industry. As the Table displays, we find a significant incidence of manipulated lucky grants in each of the economy's industries. The highest fraction of lucky grants that owe their status to manipulation is in the Business Equipment and Telecom industries (59% and 57% respectively), and the lowest fraction is in the Finance, Consumer Non-Durables, and Manufacturing industries (32%, 34%, and 35% respectively). Notably, the fraction of lucky grants that owe their status to manipulation is larger or equal to 50% in seven of the economy's twelve industries (including, importantly, several industries where no new economy firms are included).

Table 15 also shows how the twelve industries vary in terms of the incidence of firms and CEOs providing and receiving manipulated grants. Again, we find a significant incidence of such firms and CEOs in each of the twelve industries. The incidence of CEOs receiving manipulated lucky grants is lowest in the Utilities, Consumer Non-Durables, and Finance industries (4%, 5%, and 5% respectively) and highest in the Business Equipment industry (13%). The incidence of firms providing manipulated grants is at its lowest level (6%) in the Finance industry and at its highest level (17%) in the Business Equipment industry.

[15] They also span four different one-digit SIC code classifications (3, 4, 5, and 7).

[16] The industry definitions are obtained from Ken French's website. We also conducted an analysis of the propensity of lucky grants across industries classified on the basis of one-digit SIC codes, and we similarly found a significant presence of manipulated lucky grants in all industries which made significant use of option grants (that is, all industries other than agriculture and public administration).

The variation across industries that we identify is not necessarily all due to "industry effects", say, industry "norms" or "culture."[17] Industry classification might well be correlated with factors such as stock price volatility and firm size that we have found to be correlated with lucky grants. Thus, to investigate the extent to which the variation across industries is due to such factors rather than "pure" industry effects, we re-run regression (5) adding industry dummy variables using the energy industry as the default group. The last column of Table 15 shows the coefficients on the industry dummies in this regression.

We find that, once we control for stock price volatility (proxied in (5) by the difference between the lowest and median price of the grant month) and firm size, only the Finance industry has a probability of a lucky grant that is different to a statistically significant degree from the Energy industry. Pair-wise F-tests further suggest that the last four industries (Manufacturing, Utilities, Consumer Non-Durables, and Finance) all display a significantly different coefficient from the first industry (Business Equipment). However, such tests fail to find statistically significant differences in any other pair-wise comparisons of industries and, in particular, among any two of the first eight industries.[18]

VI. GAINS FROM LUCK

A natural question to ask is whether the gains to CEOs from manipulated timing have been material. Some observers suggested that, notwithstanding the substantial attention accorded to backdating and whatever legal and ethical issues they might involve, the amounts executives directly gained from backdating were quite limited relative to the regular compensation paid to CEOs.[19] In this section we try to assess the

[17] Fleischer (2006) argues that differences in corporate culture and compliance norms were likely a key determinant why some firms but not other engaged in grant manipulation.

[18] For the sub-sample where we also have governance data, we also ran a regression (not shown) similar to that in Table 12 except that we added the industry dummies. Again, we found that, after controlling for CEO and firm governance characteristics, some industry differences remain but that most industries are not statistically distinguishable in terms of lucky grant odds.

[19] See, e.g., Walker (2006). He goes on to suggest that while the direct effect of the backdating (taking the number of options granted as given) on the value of the grant was small, CEOs might have gained because the backdating of fixed-value grants resulted in a larger number of options

gains to CEOs from timing manipulation. The estimates we derive indicate that the payoffs from timing manipulation were hardly insignificant.

Surely, we do not know for certain which grants had their timing manipulated, but we have been able to identify a pool of grants where a large fraction of grants was likely manipulated. In particular, we have shown that a large fraction of the grants given in one of the three lowest prices, especially the lowest price, had their timing manipulated. So it is worth estimating the potential gain that a CEO would have derived from having a grant placed in this pool opportunistically, assuming the grant was indeed manipulated.[20]

Table 16 reports such estimates. We first calculate the value of each grant in the considered pool assuming it was indeed granted on the date reported using the parameters given in the Thomson database for the grant date, maturity date, strike price, and the number of options granted.[21] Assuming the grant was manipulated, we then compute and show the average ratios of three benchmark estimates of the value that the grant in fact had to the receiving CEO relative to the estimated option value at the grant date. One comparison benchmark is the value the option had assuming it was in fact granted not on the reported date but on a date in the grant month in which the price was equal to the month's median price. The second comparison benchmark is the expected value that the grant had assuming it was granted not on the reported date but on a randomly selected day during the grant month (that is, assuming it was given on any of these days with the same probability).[22] The third benchmark comparison is to the value that the CEO's

being given. For a similar view expressed by the media, see Gary Rivlin & Eric Dash, "Silicon Valley Firms Scrutinized on Stock option Policies," NY Times, Jul. 22, 2006.

[20] While Narayanan, Schipani, and Seyhun (2006) and Walker (2006) calculate the potential gains to executives in some of the cases that have come under scrutiny thus far, we attempt below to estimate the gains to executives in the whole pool of manipulated lucky grants.

[21] In order to calculate Black-Scholes values, we use the 3-month T-bill rate as the risk free rate, and as a proxy for volatility we use the standard deviation of daily returns in the year prior to the grant. Grants with less than 30 days of stock returns in the previous year are excluded.

[22] This is computed as the average over Black-Scholes option values in the grant month, where the daily option values are based on the strike price of the actual grant but the stock price being the price of the particular day of the month. All other parameters are held constant.

option had at the end of the grant month.[23] We report ratios of the benchmarks to the actual grant value in the first three rows and the number of observations below.

We do these calculations separately for grants in the three pools of lucky grants: Grants at the lowest price of the month, grants at the second-lowest price, and grants at the third-lowest price. These results are displayed in the first three columns of Table 16. We find that the three methods yield very similar results. Lucky grants that were manipulated had a value to the executive that was 20%-21% higher than the grant-date value calculated under the assumption of truthful reporting. By the end of the grant month, manipulated lucky grants had a value that was 21% higher than their value assuming truthful reporting. Assuming that the actual grant date had a median price or that the grant date was randomly selected, the value of the grant to the CEO was 20% higher than its value under accurate reporting.

Our estimates for the gains from manipulations, placing grants in the second-lowest and third-lowest price categories, also yields significant (though naturally lower) figures. We estimate the typical gain to a CEO from a grant in the second-lowest price category that was manipulated to be 12%-14%. For grants in the third-lowest category, our estimate for the typical gain is 9%-11% of the value of the grants assuming accurate reporting of the grant date.

One aspect of our findings makes the above estimates significantly conservative. In two of our estimation methods, we assume that manipulated grants were moved to their reported date from a date with the median price or from a randomly selected date. Our earlier results (see Table 6 on the estimated incidence of manipulation in each price rank category) indicate that, other things equal, a manipulated grant at the bottom of the price distribution was more likely to have been moved from a date with a price at the high part of the month's price distribution than from a date in the middle part of this distribution. To assess the potential significance of this source of under-valuation, the three last columns of Table 16 provide estimates of the gain to a CEO from moving the reported date of a grant given in a day with one of the highest three prices of the month from the actual date to a date with the month's median price, to a randomly selected date,

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[23] This is computed using the strike price of the actual grant and the stock price at the last trading day of the month.

or to the month's last trading day. These gains turn out to be significant, yielding 6%-10% of the grant's value assuming accurate reporting, depending on the scenario and method used. Thus, because we do not attempt to add gains of this type to our calculation of the gains from manipulated grants at the bottom of the price distribution, our estimates are conservative and likely understate the level of gains.

There is another aspect of our findings that makes our estimates of percentage under-reporting quite conservative. Not knowing which grants in the lucky grants pool were manipulated, we assume that the manipulated grants in this pool are similar in characteristics to the other (non-manipulated) grants in the pool. However, our results suggest that manipulation might have been more likely to occur when the difference between the lowest and the median price was high, which operates to increase the percentage appreciation in grant value due to backdating to the month's lowest price.

The next three rows, labeled dollar underreporting, translate the typical gains from a manipulated grant into dollar terms (expressed in 2005 dollars using the CPI index). For lucky grants that were manipulated, our estimate of the dollar gain to the CEO ranges (depending on the method) from 1.4 to 1.7 million dollars. With respect to this estimate, we should caution that a smaller firm size and smaller grant value of manipulated grants vs. other grants in the pool of lucky grants works in the direction of over-estimation.

The final three rows present an estimate of the ratio of a CEO's gain from a manipulated grant in one of three prices at the bottom of the distribution to the total compensation of the CEO. Total compensation is from ExecuComp (the tdc1 variable) and hence reduces the sample to those companies for which we have data from ExecuComp. To derive this estimate, we take the Black-Scholes value of the options reported by ExecuComp, and use our methods for estimating the percentage of this value that the CEO gained assuming the grant was manipulated. We then estimate the average ratio of such (unreported) gains to total reported compensation and get estimates of 9%-10%. Recalling the factors that made our estimation method for the percentage by which manipulated grants were under-reported conservative, we believe that these estimates are conservative and likely significantly under-stated. Thus, our estimates enable rejecting the possibility raised by some that backdating gains might have been insignificant relative to CEOs' regular compensation.

VII. REPORTED COMPENSATION AND GAINS FROM LUCKY GRANTS

We next explore the total reported compensation that was awarded to CEOs who got grants which present a substantial likelihood of having been manipulated. In particular, we seek to examine the view that backdating has been used by firms as a tax-efficient form of non-performance pay. On this view, backdating gains could have been an attractive form of compensation because they enabled providing non-performance pay that was not subject to the $1 million limitation of tax deductibility under section 162(m) of the Internal Revenue Code. That is, (un-reported) backdating gains are conjectured to have been used by some companies as a tax-efficient substitute for other forms of compensation. A finding that firms using this form of substitute compensation have paid lower compensation through other sources (relative to peer companies) would be consistent with this view. Below we therefore test whether, controlling for standard controls, total reported compensation was lower for CEOs that were the recipients of lucky grants.

Table 17 presents regression results for the sub-sample of CEOs for whom data is available in ExecuComp. The dependent variable is the natural logarithm of total compensation (tdc1) from ExecuComp. In columns 1 and 2, the independent variables of interest are a dummy for lucky equal to one if the grant was given at the lowest price of the month. In columns 3 and 4, the independent variable of interest is called relative luck and is defined as the gain from luck in the event the grant is lucky (which is thus zero when the grant is not lucky) divided by total reported compensation.[24]

We control for other known determinants of the level of compensation, namely: the standard deviation of the daily stock returns in the year prior to the fiscal year where the grant was given; the log of the book value of assets; the firm's return on assets;

[24] The results displayed in the table use our first method of estimating gains from luck (see Table 16), which assumes that manipulated lucky grants were in fact given in a day with a price equal to the month's median. Using the other methods, all the regressions in the table yield similar results to those displayed.

industry-adjusted Tobin's Q; the firm's leverage; and the firm's stock returns in the year of compensation and (separately) the prior year; and a dummy for whether the firm is a new economy firm. In the regressions reported in columns 2 and 4, CEO age and tenure are from ExecuComp and are added as additional controls for CEO characteristics from the second regression onwards.

We cluster the errors by CEOs. As before, this enables us to correct for potential correlations across the levels of compensation among the same CEOs.

The coefficient on lucky is positive and significant (at the 5% level), enabling us to reject the prediction that firms granting options at the lowest price of the month paid lower compensation through other sources. The conclusion is similar using relative gains from luck, whose coefficient is also positive and significant at the 10% level; the higher a CEO's gain (if any) from receiving a lucky grant, the higher the CEO's total compensation from other, reported sources. We thus fail to find evidence that supports the view that gains from grant manipulation were provided to CEOs as a substitute for, and reduced the total amount of, other forms of compensation.

We also conduct another test to examine whether firms paying one million dollar of salary or more were more likely to provide lucky grants. For such firms, the $1 million dollar limitation on tax deductibility of nonperformance compensation is a binding constraint. Thus, to the extent that backdating was in part motivated by a desire to provide nonperformance pay without going beyond the 162(m) limitation, one would expect lucky grants to be used more in such firms. We test for the presence of such a correlation. However, we find no significant correlation between lucky grants and a dummy equal to one if the salary is $1 million or above (coefficient of -0.001 with a p-value of 0.92) controlling for the same firm and CEO characteristics as reported in Table 17. Similarly, we do not find a higher likelihood of lucky grants for firms paying a salary between $950 thousand and $1 million. Thus, this test also does not provide support for the view that backdating could be explained as an attempt to provide nonperformance compensation in a tax-efficient way that avoided section 162(m) penalties.

What explains the identified positive correlation between abnormal compensation and lucky grants? One possible explanation is that CEOs with greater influence over their own pay have been more likely both to obtain manipulated lucky grants and to receive higher compensation from other sources. Another (not mutually exclusive) explanation is that the payment of higher compensation than in peer companies provided greater motivation to shifting some compensation to forms of compensation below the radar screen (including through manipulated grants). More work needs to be done to disentangle these and other possible explanations. For now, we can just reject the conjecture that grant manipulation was used by otherwise similar firms with the shareholder-regarding aim of providing non-performance pay in a tax-efficient way.

VIII. SUPER-LUCKY GRANTS

We have thus far focused on grants awarded at the lowest price of the grant month. To the extent that opportunistic timing has been due to backdating, however, it is possible that firms mis-dated grants within a period longer than a calendar month. In this section we examine lucky grants that were "super-lucky," defined as having a grant price at the lowest price of the calendar quarter in which the grant was reported to have been awarded.

Table 18, Panel A displays statistics concerning the incidence of super-lucky grants. In the overall sample period, we find 992 grants (5.2% of all grants) that were super-lucky. Out of the set of lucky grants (see Table 6), 42% were super-lucky (992 out of 2329 grants). Furthermore, comparing the number of actual super-lucky grants with the estimated number of such grants, we estimate that about 610 super-lucky grants (62% of all such grants) were manipulated.

As in the case of manipulated lucky grants, we find that most of the manipulated super-lucky grants were awarded by old economy firms. As Table 18 indicates, we estimate that about 470 super-lucky grants in old economy firms (59% of all super-lucky grants in such firms) were manipulated. A similar estimation for new economy firms indicates that about 71% of the super-lucky grants awarded by them were manipulated. Thus, while new economy firms display a somewhat higher propensity of manipulation, old economy firms do not fall far behind, and, given their larger proportion in the population, are associated with most of the manipulated super-lucky grants.

Table 18, Panel B shows statistics by CEO and firm, similar to the ones shown in Table 7 for lucky grants. As the table indicates, we estimate that about 7% of CEOs received one or more super-lucky grants due to manipulation. Similarly, about 11% of firms provided at least one super-lucky grant due to manipulation. In untabulated results, we find the pattern to be very similar in both old and new economy firms. For example, in old economy firms, 9.2% (6%) of the CEOs got a lucky (super lucky) grant, and 11.8% (10%) of the firms got such a grant.

Given that there is an increased fraction of manipulated grants among super lucky grants, we also display classes of grants formed by volatility (quartile and decile), whether the preceding grant was lucky, and whether the board is independent. These results are shown in Table 19 for pre-SOX grants. Using super-lucky grants, we can identify groups of grants that consist of manipulated grants with a very high probability. For example, among the 423 super-lucky grants by firms in the highest volatility quartile, we estimate that 88% have been manipulated. An 87% proportion of manipulated grants is estimated to exist in the pool of super-lucky grants by firms without a majority of independent directors. An even higher fraction of manipulated grants – 92% – is estimated to exist among the 189 super-lucky grants that were preceded by a lucky grant. If we put together in one pool all the super-lucky grants that have one or more of the three above characteristics – volatility in the highest quartile, a preceding lucky grant, or a lack of independent board – we obtain a pool of 600 grants out of which 88% is estimated to have been manipulated.

IX. CONCLUSION

We have investigated in this paper the opportunistic timing of options during the period 1996-2005, focusing on the ranking of grant-date prices within the price distribution of the grant month. Opportunistic timing increases the incidence of grants at the bottom of the price distribution – and especially at the lowest price of the month. The price-rank of a grant, and in particular whether the grant was given at the lowest price of the month, provide a useful tool for investigating the incidence, causes, and consequences of grant manipulation.

Using this proxy we are able to identify various factors that contribute to the manipulation of some grants but not others. In particular, we identify a link between manipulation and governance. Lucky grants are more likely to occur when the firm lacks a majority of independent directors and when the CEO has longer tenure, both factors associated with greater CEO influence on the company's pay-setting and governance processes. Relatedly, we find that CEOs receiving lucky grants also receive total compensation from other sources that is higher relative to peer firms, thus finding no evidence that extra gains from grant timing manipulation was used by firms as a substitute for other compensation forms.

We also document a link between timing manipulation and the potential payoffs from it. Not only is manipulation more common in firms with higher stock price volatility, but it is also more likely to occur, for a given CEO and firm, in months in which the potential gain from it is higher relative to other times. Our analysis also highlights the existence of serial luck. Luck is persistent with CEOs more likely being lucky in their next grant when their prior grant was lucky. This finding indicates that, beyond the factors we identify, there might be other systematic factors that underlay timing manipulation and future research might seek to identify them.

Finally, we have used the incidence of grants at different price-ranks to derive estimates of the incidence and gains from manipulation. Among other things, we find substantial fractions of lucky grants being manipulated, as well as significant fractions of all CEOs and firms receiving or granting manipulated lucky grants. By providing estimates of the substantial incidence of lucky grants, firms, and CEOs in old economy firms, our analysis dispels the impression that grant manipulation is concentrated in new economy firms. Furthermore, we find that the gains to CEOs receiving manipulated lucky grants are material relative to the no-manipulated grant value and their annual compensation from other sources, and we provide an estimate for these gains.

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TABLE 1: DISTRIBUTION OF GRANT-DAY PRICES BY RANK

The table shows the distribution of grant-date prices relative to prices during the month in which the options are granted. The sample consists of 19036 option grants to insiders between 1996-2005, and is taken from Thomson Financial's insider-transaction database. Lucky are grants whose grant-date price is the lowest price of the month in which the options were granted. Grants that are denoted 2nd lowest – 5th lowest and Highest – 5th highest are grants whose strike price is the 2nd – 5th lowest price of the month in which the options are granted and grants whose strike price is the highest – 5th highest in the month in which the options are granted. Grants Below median and Grants Above median are grants whose strike price is lower and higher than the median price of the month in which the options were granted respectively. Grants Before SOX are ones whose grant date is before September 1, 2002, and grants after SOX are ones whose grant date is on or after September 1, 2002.

Year	Total number of grants	Lowest (Lucky)	2nd lowest	3rd lowest	4th lowest	5th lowest	5th highest	4th highest	3rd highest	2nd highest	Highest	Below median	Above median	Difference
1996	1348	16%	13%	12%	10%	9%	8%	9%	8%	7%	4%	54%	34%	20%
1997	1672	16%	10%	9%	10%	8%	8%	8%	8%	7%	5%	55%	35%	20%
1998	1692	13%	10%	9%	8%	9%	6%	7%	6%	5%	5%	55%	36%	19%
1999	1584	15%	10%	8%	8%	7%	7%	6%	5%	5%	4%	56%	36%	20%
2000	1879	15%	10%	8%	8%	7%	5%	7%	6%	5%	4%	58%	35%	23%
2001	2326	15%	9%	8%	8%	7%	5%	4%	4%	4%	4%	61%	34%	27%
2002	2062	10%	7%	7%	6%	6%	5%	4%	3%	4%	4%	57%	38%	19%
2003	2164	9%	7%	7%	6%	6%	4%	5%	4%	4%	4%	55%	38%	17%
2004	2219	8%	8%	6%	5%	5%	5%	5%	5%	4%	5%	53%	43%	10%
2005	2090	8%	6%	6%	5%	6%	6%	5%	4%	5%	5%	51%	45%	6%
Before SOX	11998	15%	10%	9%	8%	7%	6%	6%	6%	5%	4%	57%	35%	22%
After SOX	7038	8%	7%	6%	5%	6%	5%	5%	4%	4%	5%	53%	42%	11%
Overall	19036	12%	9%	8%	7%	7%	6%	6%	5%	5%	4%	56%	38%	18%

TABLE 2: DISTRIBUTION OF GRANT-DAY RANK BY CEO AND FIRM

The table shows the distribution of grant-date prices relative to prices during the month in which the options are granted. Panel A shows the percentage of CEOs that had at least one grant whose grant-date price was at the three lowest, the three highest, the lowest, or the highest price of the month, respectively. The distribution is shown separately for CEOs that received one grant in our sample, two grants, three grants etc. Panel B shows the distribution separately for firms that granted one grant, two grants, three grants, etc. The sample consists of 19036 option grants to insiders between 1996-2005, and is taken from Thomson Financial's insider-transaction database. Grants Before SOX are ones whose grant date is before September 1, 2002, and grants after SOX are ones whose grant date is on or after September 1, 2002.

PANEL A: BY CEO

Grants per CEO	Number of CEOs	Percent of CEOs that had at least one grant at the three lowest prices of the month	Percent of CEOs that had at least one grant at the three highest prices of the month	Difference	Percent of CEOs that had at least one grant at the lowest prices of the month	Percent of CEOs that had at least one grant at the highest price of the month	Difference
1	4510	32%	16%	16%	14%	5%	9%
2	1874	47%	28%	19%	20%	9%	11%
3	1050	58%	34%	24%	29%	11%	18%
4	549	70%	42%	28%	39%	15%	24%
Larger than 4	837	81%	55%	26%	51%	23%	28%
Total	8820	45%	26%	19%	22%	9%	13%

PANEL B: BY FIRM

Grants per Firm	Number of Firms	Percent of Firms that had at least one grant at the three lowest prices of the month	Percent of Firms that had at least one grant at the three highest prices of the month	Difference	Percent of Firms that had at least one grant at the lowest prices of the month	Percent of Firms that had at least one grant at the highest price of the month	Difference
1	1880	36%	17%	19%	16%	4%	8%
2	1106	52%	29%	23%	23%	9%	14%
3	860	60%	36%	24%	30%	12%	18%
4	569	69%	48%	21%	37%	15%	22%
Larger than 4	1404	84%	57%	27%	52%	25%	27%
Total	5819	58%	35%	23%	30%	12%	18%

TABLE 3: FIRM, CEO, AND GRANT CHARACTERISTICS

Panel A compares averages of financial and governance characteristics of grants whose strike price was the lowest during the month (Lucky) and other grants (Not Lucky). Panel B displays the incidence of Lucky grants among various categories of grants. Option grant information is from the Thomson Financials insider trading database. Stock return and market cap data is taken from the CRSP database. Other financial and governance characteristics are taken from the IRRC and the ExecuComp database. Market capitalization is the market value of equity, calculated at the end of the month in which the option was granted. Relative size is the market cap of equity divided by the median market cap of firms in the sample for that year. New Economy are firms that belong to a new economy industry, as defined in Murphy (2003). Grants Before SOX are ones whose grant date is before September 1, 2002, and grants after SOX are ones whose grant date is on or after September 1, 2002. CEO from outside is a dummy variable which equals one if the CEO was not employed by the firm before getting into the CEO position. Independent Board dummy equals one if the majority of the directors on the board are independent. Previously lucky is a dummy variable which equals one if the grant that the CEO received before the current grant was a lucky grant. A firm might have more than one observation if its CEO received more than one grant in the sample.

PANEL A: DIFFERENCE IN FIRM AND CEO CHARACTERISTICS

	Lucky	Not Lucky	Difference
Relative size (market cap divided by median cap for the year)	5.32	8.69	***
New Economy	17%	14%	***
Difference between the median and lowest price in the grant month	13.1%	10.1%	***
After SOX	25%	39%	***
CEO from outside	18%	17%	
CEO Tenure (in years)	7.55	6.41	***
CEO fractional equity ownership	3%	2%	***
Independent Board dummy	77%	85%	***
Independent compensation committee dummy	95%	97%	
Preceding grant lucky	9.5%	5.5%	***
Observations	2359	16660	

PANEL B: DIFFERENCE IN LIKELIHOOD OF LUCKY GRANTS

Variable	% Lucky		Difference	Variable
Company size below median	14%	11%	***	Company size above median
New Economy	15%	12%	***	Not new economy
High (top quartile) difference between lowest and median price	17%	10%	***	Low (bottom quartile) difference between lowest and median price
Before SOX	15%	8%	***	After SOX
CEO from Inside	10%	11%		CEO from Outside
Tenure >=5	11%	9%	***	Tenure <5
CEO fractional equity ownership ≥5%	16%	9%	***	CEO fractional equity ownership <5%
No majority of independent directors	14%	9%	***	Majority of independent directors
Non-independent compensation committee	13%	9%	*	Independent compensation committee
Preceding grant lucky	15%	9%	***	Preceding grant not lucky

TABLE 4: LIKELIHOOD OF A DAY BEING SELECTED AS A GRANT DATE

For each firm that granted options, the sample consists of all dates during the month where the option was granted. The dependent variable is a dummy variable which equals one if the firm granted an option on that particular date and zero otherwise. Grants Before SOX are ones whose grant date is before September 1, 2002, and grants after SOX are ones whose grant date is on or after September 1, 2002. Three lowest prices of the month and Three highest prices are dummy variables which equal one if the grant-date price was one of the three lowest prices of the month and three highest prices of the month, respectively, and zero otherwise. The coefficients shown are from a logit regression. *, **, *** represents significance at the 10%, 5%, and 1% level, respectively. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level.

Intercept	-3.072	***	-3.072	***
	(0.010)		(0.010)	
Three Lowest Prices of the Month	0.531	***		
	(0.017)			
Three Highest Prices of the Month	-0.179	***		
	(0.022)			
Three Lowest * Before SOX			0.585	***
			(0.019)	
Three Lowest * After SOX			0.406	***
			(0.028)	
Three Highest * Before SOX			-0.238	***
			(0.026)	
Three Highest * After SOX			-0.064	***
			(0.034)	
Observations	391844		391844	

TABLE 5: PRECISE RANK AND THE LIKELIHOOD
OF SELECTION AS A GRANT DATE

The regression is similar to the regression in Table 4, except that the independent variables are dummies for whether the price on the grant date was the lowest, 2nd lowest, 3rd lowest, etc. *, **, *** represents significance at the 10%, 5%, and 1% level, respectively. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level.

Intercept	-3.1092	***
	(0.009)	
Lucky (lowest)	0.8849	***
	(0.028)	
2nd lowest	0.4261	***
	(0.029)	
3rd lowest	0.2737	***
	(0.030)	
4th lowest	0.169	***
	(0.031)	
5th lowest	0.1457	***
	(0.031)	
5th highest	-0.1332	***
	(0.032)	
4rd highest	-0.0884	***
	(0.032)	
3rd highest	-0.1838	***
	(0.034)	
2nd highest	-0.189	***
	(0.036)	
Highest	-0.211	***
	(0.039)	
Observations	391844	

Intercept	-3.1105	***
	(0.010)	
Lucky (lowest) * Before Sox	1.0035	***
	(0.031)	
2nd lowest *Before Sox	0.4639	***
	(0.032)	
3rd lowest *Before Sox	0.2786	***
	(0.034)	
4th lowest *Before Sox	0.2112	***
	(0.035)	
5th lowest *Before Sox	0.1435	***
	(0.036)	
5th highest *Before Sox	-0.1882	***
	(0.039)	
4rd highest *Before Sox	-0.1302	***
	(0.038)	
3rd highest *Before Sox	-0.2048	***
	(0.040)	
2nd highest *Before Sox	-0.2213	***
	(0.042)	
Highest*Before Sox	-0.3395	***
	(0.049)	
Lucky (lowest) * After Sox	0.6092	***
	(0.046)	
2nd lowest *After Sox	0.3499	***
	(0.050)	
3rd lowest *After Sox	0.2769	***
	(0.051)	
4th lowest *After Sox	0.088	
	(0.053)	
5th lowest *After Sox	0.1714	***
	(0.053)	
5th highest *After Sox	-0.0245	
	(0.056)	
4rd highest *After Sox	-0.004	
	(0.057)	
3rd highest *After Sox	-0.1393	**
	(0.062)	
2nd highest *After Sox	-0.1207	**
	(0.060)	
Highest * After Sox	0.0103	
	(0.058)	
Observations	391844	

TABLE 6: ESTIMATING THE INCIDENCE OF MANIPULATED GRANTS

The table shows an estimate of the number of grant-date prices that should fall on the lowest price of the month, second lowest, third lowest etc, if the grant date was randomly selected. We estimate the probability of observing a grant on a particular price-rank day be counting the number of days in the month where the price is at a given price-rank and divide it by the total number of trading days of the stock in that month. The table compares the estimate to the actual number of grants that fall into these ranks. We also show the average ratio of the exercise price to the median stock price in the month. Grants Before SOX are ones whose grant date is before September 1, 2002, and grants after SOX are ones whose grant date is on or after September 1, 2002. The sample consists of 19036 option grants between 1996-2005.

	Lucky (lowest)	2nd lowest	3rd lowest	Three lowest	4th&5th lowest	Other	4th&5th highest	Three highest	3rd highest	2nd highest	Highest
Before SOX (11998)											
Actual Number of Grants	1741	1177	1031	3949	1864	2875	1524	1786	682	619	485
Expected Number of Grants	785	883	919	2587	1835	3251	1841	2483	904	834	745
Actual-Expected	956	294	112	1362	29	-376	-318	-697	-222	-215	-260
(Actual-Expected)/Expected	121.80%	33.30%	12.20%	52.65%	1.60%	-11.60%	-17.30%	-28.07%	-24.60%	-25.80%	-34.90%
(Actual-Expected)/Actual	54.90%	25.00%	10.90%	34.49%	1.60%	-13.10%	-20.90%	-39.03%	-32.50%	-34.80%	-53.70%
(Actual-Expected)/Total	8.00%	2.40%	0.90%	11.35%	0.20%	-3.10%	-2.60%	-5.81%	-1.80%	-1.80%	-2.20%
Exercise Price/Median Stock Price	0.87	0.92	0.94	0.90	0.96	1	1.03	1.09	1.07	1.09	1.12
After SOX (7038)											
Actual Number of Grants	588	482	452	1522	794	3088	697	937	304	300	333
Expected Number of Grants	381	393	400	1174	803	3113	801	1148	391	384	373
Actual-Expected	207	89	52	348	-9	-25	-104	-211	-87	-84	-40
(Actual-Expected)/Expected	54.30%	22.60%	13.00%	29.64%	-1.10%	-0.80%	-13.00%	-18.38%	-22.30%	-21.90%	-10.70%
(Actual-Expected)/Actual	35.30%	18.50%	11.60%	22.86%	-1.20%	-0.80%	-15.00%	-22.52%	-28.70%	-27.90%	-11.90%
(Actual-Expected)/Total	2.90%	1.30%	0.70%	4.94%	-0.10%	-0.40%	-1.50%	-3.00%	-1.20%	-1.20%	-0.60%
Exercise Price/Median Stock Price	0.91	0.93	0.95	0.93	0.96	1	1.04	1.07	1.05	1.07	1.08
Overall (19036)											
Actual Number of Grants	2329	1659	1483	5471	2658	5963	2221	2723	986	919	818
Expected Number of Grants	1166	1276	1318	3760	2638	6364	2643	3631	1295	1218	1118
Actual-Expected	1163	383	165	1711	20	-401	-422	-908	-309	-299	-300
(Actual-Expected)/Expected	99.70%	30.00%	12.50%	45.51%	0.80%	-6.30%	-16.00%	-25.01%	-23.90%	-24.50%	-26.80%
(Actual-Expected)/Actual	49.90%	23.10%	11.10%	31.27%	0.80%	-6.70%	-19.00%	-33.35%	-31.30%	-32.50%	-36.70%
(Actual-Expected)/Total	6.10%	2.00%	0.90%	8.99%	0.10%	-2.10%	-2.20%	-4.77%	-1.60%	-1.60%	-1.60%
Exercise Price/Median Stock Price	0.88	0.92	0.94	0.91	0.96	1	1.04	1.08	1.06	1.08	1.1

TABLE 7: ESTIMATING THE INCIDENCE OF CEOS AND FIRMS ASSOCIATED WITH MANIPULATED GRANTS

The table shows the number of CEOs in Panel A (Firms in Panel B) with one to five-and-more grants in the sample. The third column shows the number of CEOs who receive at least one grant at the lowest price of the month. The forth column shows the expected number of CEOs who receive at least one grant at the lowest price of the month. This number is computed in the following way: For CEOs with only one grant, it is the product of 4510 (CEOs with only one grant) and the probability of observing the lowest price in the month. This probability is equal to the number of days where the price was the lowest price of the month divided by the total number of trading days in that month. For CEOs with more than one grant, the expected number of CEOs that receive at least one grant at the lowest price is equal to one minus the probability of having each grant not being lucky. This is one minus the product of the probabilities that each individual grant is at the lowest price. A similar calculation is used to estimate the expected number of firms that give at least one grant at the lowest price of the month.

Distribution of Grants By CEO

# Grants	CEOs	Actual # CEOs At Lowest	Expected # CEOs at Lowest	Actual - Expected	(Actual - Expected) /Expected	(Actual - Expected) / Actual	(Actual - Expected) / Total
1	4510	614	306	308	101%	50%	6.8%
2	1874	381	228	153	67%	40%	8.2%
3	1050	301	176	125	71%	42%	11.9%
4	549	212	114	98	86%	46%	17.9%
5>	837	423	258	165	64%	39%	19.7%
All	8820	1931	1082	849	78%	44%	9.6%

Distribution of Grants By Firm

# Grants	Firms	Actual # Firms At Lowest	Expected # Firms at Lowest	Actual - Expected	(Actual - Expected) /Expected	(Actual - Expected) / Actual	(Actual - Expected) / Total
1	1880	296	138	158	114%	53%	8.4%
2	1106	254	149	105	70%	41%	9.5%
3	860	262	152	110	72%	42%	12.8%
4	569	212	125	87	70%	41%	15.3%
5>	1404	729	467	262	56%	36%	18.7%
All	5819	1753	1031	722	70%	41%	12.4%

TABLE 8: SPRINGLOADING VS.BACKDATING – TEST BASED ON MONTHS WITH SMALL DIFFERENCES BETWEEN LOWEST AND SECOND-LOWEST PRICES

The table shows logit-regression results where the dependent variable is a dummy for whether the CEO was granted options on a particular date. The sample consists only of months in which the difference between the lowest price and the second-lowest price is less than 1%. The sample for the first-column regression consists of all dates during the month where the option was granted. The sample for the second-column regression consists only of the dates in which the lowest price or the second lowest price of the month prevails. Dummy – lowest and Dummy – second lowest equal one if the price is the lowest price of the month and second-lowest price of the month and zero otherwise. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level.

Intercept	-3.051	***	-2.6902	***
	(0.015)		(0.042)	
Lowest price of the month	0.582	***	0.1661	***
	(0.047)		(0.057)	
Second-lowest price of the month	0.381	***		
	(0.050)			
Observations	119026		19368	

TABLE 9: BACKDATING VS. SPRINGLOADING – TEST BASED ON TIME OF REPORTING

For each firm that granted options, the sample consists of all dates during the month where the option was granted. The dependent variable is a dummy variable which equals one if the firm granted an option on that particular date and zero otherwise. We interact each ranking dummy variable with a dummy variable for whether the filing month with the SEC is the same as the reporting month. The coefficients are from a logit regression. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level.

Intercept	-3.049	***
	(0.008)	
Lucky*Reported same month	0.557	***
	(0.045)	
Lucky*Reported next month	0.963	***
	(0.026)	
Observations	391844	

TABLE 10: SPRING-LOADING VS. BACKDATING –
TEST BASED ON MARKET-WIDE MOVEMENTS

For each granted option, the sample consists of all dates during the month in which the option was granted. The dependent variable is a dummy variable which equals one if the firm granted an option on that particular date and zero otherwise. Return from grant date to end of month is the natural log of the gross stock return from the date under consideration until the end of month. Market return from grant date to end of month is the natural log of the gross market return from the date under consideration until the end of the month. The market return is the CRSP value weighted return. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level.

Intercept	-2.986 ***	-3.020 ***
	(0.007)	(0.008)
Return from grant date to end of month	0.614 ***	
	(0.034)	
Market return from grant date to end of month		1.747 ***
		(0.242)
Firm-specific return from grant date to end of month		0.585 ***
		(0.036)
Observations	391844	391844

TABLE 11: THE DETERMINANTS OF BEING LUCK – A FIRST LOOK

The table shows the logit regression results where the dependent variable is a dummy variable for whether the grant was given at the date where the lowest price of the month prevailed and zero otherwise. Relative size is the natural log of the ratio between the market cap of the firm at the end of the year and the median market cap of the firms in the sample for that year. Difference between median and lowest price is the natural log of the gross return to shareholders from the lowest price of the month in which the options were granted to the median price of that month. Market component of the median price – lowest price difference is the market return from the minimum-price day to the median-price day. Firm-specific component of the median price – lowest price difference is the total minus the market return from the minimum-price day to the median-price day. New Economy firms are firms with SIC codes as defined in Murphy (2003). Grants Before SOX are ones whose grant date is before September 1, 2002, and grants after SOX are ones whose grant date is on or after September 1, 2002.We also control for the fraction of days in the month that have the lowest price (not shown). Due to data availability, the sample is reduced to 18543 observations. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level (except for the fixed effect regressions). *, **, *** indicate significance at the 10%, 5%, and 1% levels respectively.

	Whole sample		Firm fixed effect		Executive fixed effect	
Intercept	-2.363 ***	-2.375 ***				
	0.048	0.049				
Relative size	-0.051 ***	-0.054 ***	0.156 ***	0.160 ***	0.162 ***	0.154 ***
	(0.012)	(0.012)	(0.046)	(0.053)	(0.053)	(0.046)
Difference between median and lowest price	1.190 ***		0.890 ***		0.808 ***	
	(0.171)		(0.250)		(0.269)	
Market component of the median price – lowest price difference		2.338 ***		1.832 *		1.707 *
		(0.753)		(1.107)		(0.959)
Firm-specific component of the median price – lowest price difference		1.147 ***		0.771 ***		0.854 ***
		(0.171)		(0.267)		(0.251)
New Economy	0.197 ***	0.193 ***				
	(0.063)	(0.063)				
SOX	-0.507 ***	-0.504 ***	-0.548 ***	-0.565 ***	-0.568 ***	-0.5458 ***
	(0.052)	(0.052)	(0.071)	(0.084)	(0.084)	(0.071)
Observations	18543	18543	18543	18543	18543	18543

TABLE 12: GOVERNANCE AND THE DETERMINANTS OF LUCK

The table shows logit regression results where the dependent variable is a dummy variable for whether the grant was given at the date where the lowest price of the month prevailed and zero otherwise. The sample consists of 6001 executive grant dates from Thomson financials with governance information at the firm level from the IRRC and the ExecuComp databases. Dummy CEO Outsider equals one if the executive was not employed in the firm before becoming an executive and zero otherwise. Independent compensation committee dummy equals one if the compensation committee consists only of independent directors and 0 otherwise. Independent board dummy equals one if the board has a majority of independent directors and zero otherwise. This variable is available only from 1998 onwards. The definition of the rest of the variables appears in Table 11. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Intercept	-2.963	***	-2.871	***	-2.868	***	-3.152	***	-2.673	***	-3.058	***
	(0.167)		(0.171)		(0.170)		(0.334)		(0.216)		(0.334)	
Relative size	-0.066	**	-0.065	**	-0.057	*	0.005		-0.050		0.006	
	(0.029)		(0.029)		(0.029)		(0.035)		(0.033)		(0.035)	
NewEconomy	0.089		0.079		0.088		0.104		0.129		0.101	
	(0.134)		(0.134)		(0.134)		(0.162)		(0.153)		(0.162)	
SOX	-0.303	***	-0.297	***	-0.275	***	-0.249	**	-0.219	*	-0.202	*
	(0.101)		(0.102)		(0.102)		(0.118)		(0.114)		(0.119)	
Market component of the median price - lowest price difference	3.890	***	3.941	***	3.952	***	5.507	***	4.476	***	5.443	***
	(1.377)		(1.379)		(1.380)		(1.585)		(1.520)		(1.584)	
Firm-specific component of the median price -lowest price difference	1.663	***	1.675	***	1.653	***	2.455	***	1.437	***	2.468	***
	(0.390)		(0.387)		(0.387)		(0.464)		(0.409)		(0.464)	
Dummy CEO Outsider	-0.042		-0.687	**	-0.620	**	-0.377		-0.462		-0.369	
	(0.115)		(0.294)		(0.293)		(0.350)		(0.320)		(0.347)	
Tenure	0.187	***										
	(0.050)											
Tenue * Insider dummy			0.122	**	0.092		0.148	**	0.111	*	0.148	**
			(0.057)		(0.057)		(0.071)		(0.064)		(0.071)	
Tenure * Outsider dummy			0.434	***	0.357	***	0.380	***	0.360	***	0.375	***
			(0.114)		(0.117)		(0.139)		(0.126)		(0.138)	
CEO Ownership >5% and <25% dummy					0.350	**	0.154		0.157		0.093	
					(0.145)		(0.191)		(0.170)		(0.192)	
CEO Ownership >25% dummy					0.394		-0.359		0.136		-0.511	
					(0.333)		(0.540)		(0.388)		(0.546)	
Independent compensation committee dummy							-0.393				-0.140	
							(0.256)				(0.272)	
Independent board dummy									-0.404	***	-0.411	***
									(0.122)		(0.145)	
Observations	6001		6001		6001		4284		4284		4284	

TABLE 13: SERIAL LUCK

The table shows the logit regression results where the dependent variable is a dummy variable for whether the grant was given at the date where the lowest price of the month prevailed and zero otherwise. The dummy variable Previous Lucky (Unlucky) equals one if the executive had a previous grant which was granted on the date with the lowest (not the lowest) price of the month, and zero otherwise. The definition of the rest of the variables appears in Tables 11 and 12. The numbers in parentheses are the estimated standard errors of the coefficients, adjusted for clustering at the executive level. *, **, *** indicate significance at the 10%, 5%, and 1% levels respectively.

Intercept	-2.376	***	-2.387	***	-2.888	***	-3.105	***
	(0.052)		(0.053)		(0.175)		(0.338)	
Relative size	-0.050	***	-0.052	***	-0.061	**	0.011	
	(0.012)		(0.012)		(0.029)		(0.035)	
NewEconomy	0.184	***	0.181	***	0.059		0.083	
	(0.063)		(0.063)		(0.134)		(0.162)	
SOX	-0.497	***	-0.494	***	-0.289	***	-0.193	
	(0.053)		(0.053)		(0.103)		(0.120)	
Difference between median and lowest price	1.156							
	(0.170)							
Market component of the median price - lowest price difference			2.216	***	3.836	***	5.398	***
			(0.755)		(1.382)		(1.586)	
Firm-specific component of the median price -lowest price difference			1.117	***	1.663	***	2.482	***
			(0.170)		(0.388)		(0.464)	
Dummy CEO Outsider					-0.675	**	-0.369	
					(0.294)		(0.347)	
Tenue * Insider dummy					0.116	**	0.148	**
					(0.057)		(0.071)	
Tenure * Outsider dummy					0.419	***	0.367	***
					(0.114)		(0.138)	
CEO Ownership >5% and <25% dummy							0.074	
							(0.193)	
CEO Ownership >25% dummy							-0.472	
							(0.546)	
Independent compensation committee dummy							-0.134	
							(0.272)	
Majority of Independent directors dummy							-0.409	***
							(0.145)	
Previous Lucky	0.580	***	0.577	***	0.650	***	0.508	***
	(0.083)		0.0831		(0.155)		(0.190)	
Previous Unlucky	-0.070		-0.071		-0.086		-0.061	
	(0.051)		(0.051)		(0.095)		(0.115)	
Observations	18543		18543		6001		4284	

TABLE 14: ZEROING IN ON MANIPULATED GRANTS

This table consists of two panels that display the number of grants at the lowest price of the month conditional on a number of variables. Those variables are: 1) Highest volatility quartile (decile). Volatility is defined as the distance between the lowest and median price in a month. 2) Preceding grant was lucky. This dummy variable is equal to one if the CEO's previous grant was at the lowest price. 3) Not independent Board. This dummy is equal to one if the board is not majority independent. The variable is only available for a subset of firms with IRRC data available. For each of these variables, we further show statistics on subsamples depending on whether the firm is an old or new economy firm, where new economy firms are defined if they operate in one of the following four digit SIC codes: 3570, 3571, 3572, 3576, 3577, 3661, 3674, 4812, 4813, 5045,5961, 7370, 7371, 7372, 7373, and on relative size. Relative size is the natural log of the ratio between the market capitalization of the firm at the end of the year and the median market capitalization of the firms in the sample for that year. The sample is split at the median relative size by year. For the sub-sample of grants with available IRRC data, we split the sample conditional on IRRC data being available. Panel A shows statistics for the pre-SOX period, panel B for the post-SOX period.

Panel A: Pre-SOX	Observ-ations	Actual Number	Actual/ Obser-vations	(Actual-Expected) /Actual	Unexpected Number
All	11998	1741	15%	55%	956
Highest volatility quartile	2969	589	20%	71%	421
Highest volatility decile	1187	279	24%	76%	211
Preceding grant was lucky	765	189	25%	74%	140
Not independent board (IRRC subset only)	713	99	14%	60%	59
At least one of: Highest volatility quartile, Preceding grant was lucky, Not independent Board	4064	761	19%	69%	524
At least two of: Highest volatility quartile, Preceding grant was lucky, Not independent Board	375	111	30%	82%	91
At least two of: Highest volatility decile, Preceding grant was lucky, Not independent Board	183	59	32%	83%	49

TABLE 14 (continued)

Panel B: Post SOX	Obser-vations	Actual Number	Actual/ Obser-vations	(Actual-Expected) /Actual	Unexpected Number
All	7038	588	8%	35%	207
Highest volatility quartile	1780	197	11%	53%	105
Highest volatility decile	709	101	14%	64%	65
Preceding grant was lucky	486	53	11%	51%	27
Not independent board (IRRC subset only)	136	19	14%	65%	12
At least one of: Highest volatility quartile, Preceding grant was lucky, Not independent Board	2250	250	11%	53%	133
At least two of: Highest volatility quartile, Preceding grant was lucky, Not independent Board	152	19	0.125	57%	11
At least two of: Highest volatility decile, Preceding grant was lucky, Not independent Board	71	13	18%	73%	9

Table 15: LUCK IN THE ECONOMY

The table shows statistics by industries. Industries are defined as the 12 Fama-French industries. For each industry the table reports, the number of firms, the number of grants and the number of grants at the lowest price of the month, the fraction of grants at the lowest price that are unexpected, the fraction of CEOs that unexpectedly received a grant at the lowest price of the month, and the fraction of firms that unexpectedly granted options at the lowest price of the month. The estimated number of grants is based on the fraction of days where the price is at the lowest price of the month relative to the total number of trading days in the month. For CEOs with only one grant, it is the product of the number of CEOs with only one grant and the probability of observing the lowest price in the month. This probability is equal to the number of days where the price was the lowest price of the month divided by the total number of trading days in that month. For CEOs with more than one grant, the expected number of CEOs that receive at least one grant at the lowest price is equal to one minus the probability of having each grant not being lucky. The latter is one minus the product of the probabilities that each individual grant is at the lowest price of the month. The same methodology is used to calculate the expected number of firms that give at least one grant in a month. New (Old) economy firms are defined as firms operating (not operating) in one of the following four digit SIC codes: 3570, 3571, 3572, 3576, 3577, 3661, 3674, 4812, 4813, 5045, 5961, 7370, 7371, 7372, 7373. New economy firms are part of the Business Equipment, Telecom and Shops industries. The last column contains the regression coefficients on the industry dummy. The holdout industry is the Energy industry and the old economy, respectively. The regression run corresponds to the first regression in Table 11 where the new economy dummy is replaced by the Fama-French industry dummies. ***, *, indicate significance at the 1% and 10% level.

Economy Sectors	#Firms in Industry	#Grants in Industry	%Grants at Lowest	(Actual-Estimated) /Actual	%CEO with Manipulated Grants	%Firm with Manipulated Grants	Regression Coef
New economy	794	2581	14%	59%	12%	16%	0.197 ***
Old economy	5025	16455	12%	48%	9%	12%	
12 Fama-French Industries							
Business Equipment: Computers, Software, and Electronic Equipment	1300	4477	15%	59%	13%	17%	0.226
Telecom: Telephone and Television Transmission	199	524	13%	57%	9%	10%	0.123
Other: Mines, Constr, BldMt, Trans, Hotels, Bus Serv, Entertainment	786	2433	13%	55%	12%	16%	0.141
Chem: Chemicals and Allied Products	82	304	13%	53%	12%	14%	0.154
Health: Healthcare, Medical Equipment, and Drugs	657	2437	13%	53%	12%	14%	0.119
Energy: Oil, Gas, and Coal Extraction and Products	175	570	12%	50%	11%	14%	
Shops: Wholesale, Retail, and Some Services (Laundries, Repair Shops)	593	1923	12%	50%	10%	13%	0.065
Consumer Durables: Cars, TV's, Furniture, Household Appliances	118	395	11%	46%	9%	12%	0.017
Manufacturing: Machinery, Trucks, Planes, Off Furn, Paper, Com Printing	491	1729	11%	41%	6%	9%	-0.129
Utilities	100	298	9%	35%	4%	7%	-0.383
Consumer NonDurables: Food, Tobacco, Textiles, Apparel, Leather, Toys	229	780	10%	34%	5%	7%	-0.176
Money: Finance	1089	3166	9%	32%	5%	6%	-0.268*

TABLE 16: THE PAYOFFS OF BEING LUCKY

The table reports averages of different variables capturing the value of the options granted relative to various benchmarks by price-rank. The Black-Scholes value of the option grant is computed using information from Thomson about the grant date, maturity date, and strike price. The risk free rate is the three month T-bill rate. The volatility is estimated based on daily stock returns in the year prior to the grant month. Observations with fewer than 30 return observations were excluded. There are three benchmark Black-Scholes values: First, the value of an option with the strike price of the grant but the stock price is the median price of the month. Second, an expected option price. This is computed as the average over Black-Scholes option values in the grant month, where the daily option values are based on the strike price of the actual grant but the stock price being the price of the day in the month. All other parameters are held constant. Third, the value of the option on the last day of the month. This is computed using the strike price of the actual grant and the stock price at the last trading day of the month. We report ratios of the benchmarks to the actual grant value in the first three rows and the number of observations below. The following three rows, labeled dollar underreporting, show average dollar values of the difference between the benchmark and the actual grant value. The dollar values reported are expressed in 2005 dollars using the CPI index. The following three rows present the ratio of the dollar underreporting (not inflation adjusted) relative to the total compensation of the CEO. Total compensation is from ExecuComp (tdc1) and hence reduces the sample size. The dollar underreporting is calculated as the ratio of benchmark to grant value (presented in the first three rows) minus one, times the Black-Scholes value of the options reported by ExecuComp.

	Lucky (lowest)	2nd lowest	3rd Lowest	3rd highest	2nd highest	Highest
Ratio of option value at median price relative to option value at grant price	1.1962	1.1195	1.0913	0.9366	0.9179	0.8917
Ratio of expected option value relative to option value at grant price	1.2001	1.1282	1.0939	0.9387	0.9189	0.892
Ratio of option value at last day of month relative to option value at grant price	1.2136	1.1435	1.1117	0.9543	0.9236	0.9011
Observations	2230	1591	1400	917	877	783
Dollar underreporting (estimate based on option value at median price)	$1,395,375	$547,457	$404,311	$(199,885)	$(222,513)	$(289,438)
Dollar underreporting (estimate based on expected option value)	$1,380,259	$546,191	$441,007	$(204,700)	$(212,609)	$(245,424)
Dollar underreporting (estimate based on option value at last price)	$1,719,200	$725,892	$718,949	$(155,826)	$(219,464)	$(265,564)
Observations	2230	1591	1400	917	877	783
Dollar underreporting/total compensation (median price estimate)	9.91%	5.78%	4.87%	-3.00%	-3.57%	-4.89%
Dollar underreporting/total compensation (expected price estimate)	9.69%	6.22%	4.81%	-2.90%	-3.48%	-4.87%
Dollar underreporting/total compensation (last price estimate)	9.07%	6.40%	4.94%	-2.08%	-3.09%	-5.22%
Observations	656	505	453	333	305	324

TABLE 17: TOTAL REPORTED COMPENSATION AND LUCKY GRANTS

The sample consists of option grants to CEOs who are listed in ExecuComp as the CEO for the year. The dependent variable is the natural log of total compensation (tdc1) from ExecuComp. The independent variables are: A dummy for lucky equal to one if the grant was on the lowest day of the month. The ratio of implied underreported option value to total compensation (relative luck), where the implied underreporting is calculated as the ratio of the benchmark to grant value minus one, times the Black-Scholes value of the options reported by ExecuComp. The benchmark value is computed as the value of an option with the strike price of the grant but the stock price is the median price of the month. All other parameters are held constant. The standard deviation of the daily stock returns in the year prior to the fiscal year where the grant was given. The log of the book value of assets. The return on assets (ROA) computed as net income divided by book value of assets. Industry-adjusted Tobin's Q where the industry adjustment was made at the 2-digit SIC level. Leverage is the ratio of book value of debt divided by book value of assets. Firm return t is the cumulative stock return in the year of the grant (t) and the year prior to the grant (t-1). New Economy is a dummy equal to one for industries in the following 4-digit SIC: 3570, 3571, 3572, 3576, 3577, 3661, 3674, 4812, 4813, 5045,5961, 7370, 7371, 7372, 7373. CEO age and tenure are from ExecuComp. We report coefficients and t-statistics (in brackets) of OLS regressions with year and industry dummies (at the 2-digit SIC level). The year and industry dummies are not shown. Errors are clustered at the firm level and t-statistics are based on robust standard errors. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable		ln(Total Compensation)		
	(1)	(2)	(3)	(4)
Lucky	0.078	0.075		
	(2.21)**	(2.13)**		
Relative gain from luck			0.242	0.238
			(1.92)*	(1.86)*
Standard deviation of daily stock returns	7.017	7.013	7.515	7.458
	(4.55)***	(4.42)***	(4.62)***	(4.43)***
Log book value	0.486	0.485	0.488	0.488
	(39.61)***	(38.33)***	(38.61)***	(37.38)***
ROA	0.224	0.170	0.184	0.125
	(1.34)	(0.98)	(1.08)	(0.72)
Ind-adj Tobin's Q	0.099	0.099	0.107	0.107
	(8.72)***	(8.61)***	(8.85)***	(8.75)***
Leverage	-0.263	-0.273	-0.313	-0.325
	(2.80)***	(2.88)***	(3.38)***	(3.51)***
Firm return t	0.041	0.042	0.025	0.022
	(1.28)	(1.29)	(0.76)	(0.67)
Firm return t-1	0.179	0.171	0.177	0.171
	(6.38)***	(5.97)***	(6.21)***	(5.85)***
New Economy	0.223	0.227	0.230	0.234
	(3.66)***	(3.67)***	(3.61)***	(3.58)***
Tenure		0.011		0.012
		(2.24)*		(2.22)**
Tenure^2		-0.000		-0.000
		(1.58)		(1.83)
CEO age<50		-0.009		0.012
		(0.22)		(0.30)
CEO age >65		-0.242		-0.183
		(2.84)***		(2.11)**
CEO age missing		-0.015		-0.007
		(0.41)		(0.19)
Constant	5.995	3.481	6.023	3.578
	(39.74)***	(24.37)***	(38.41)***	(25.07)***
Observations	4547	4325	4276	4058
R-squared	0.54	0.55	0.54	0.55

TABLE 18: ESTIMATING THE INCIDENCE OF MANIPULATED SUPER-LUCKY GRANTS

The table reports the actual and expected number of grants. The expected number of grants is computed as the number of days with a certain price rank in a quarter where a grant was given divided by the number of trading days in that quarter where the stock actually traded. The reported number is the sum of this ratio by rank. Exercise Price/Median Stock Price is the average of the ratio of the exercise price of the option in a given rank to the median stock price in the quarter of the grant. The sample consists of 19017 option grants to insiders between 1996-2005, and is taken from Thomson Financial's insider-transaction database. The sample size is reduced because we require at least one trading day in each of the months of the quarter. Grants Before SOX and Grants After Sox are grants whose strike date is before and on or after September 1st, 2002 respectively. Quarters are defined by calendar time. Old economy firms are defined as firms not operating in one of the following four digit SIC codes: 3570, 3571, 3572, 3576, 3577, 3661, 3674, 4812, 4813, 5045,5961, 7370, 7371, 7372, 7373. Panel B shows the number of CEOs with one to five-and-more grants in the sample. The forth column shows the expected number of CEOs who receive at least one grant at the lowest price of the quarter. This number is computed in the following way: For CEOs with only one grant, it the product of 4510 (CEOs with only one grant) and the probability of observing the lowest price in the quarter. This probability is equal to the number of days where the price was the lowest price of the quarter divided by the total number of trading days in that quarter. For CEOs with more than one grant, the expected number of CEOs that receive at least one grant at the lowest price is equal to one minus the probability of having each grant not being lucky. The latter is one minus the product of the probabilities that each individual grant is at the lowest price of the quarter. The same methodology is used to calculate the expected number of firms that give at least one grant in a quarter.

Panel A: Super-Lucky Grants Distribution

	Before SOX			After SOX			Overall			Overall, Old Economy		
Total Number of Grants	11987			7030			19017			16037		
Price rank of grant date in the price distribution of the grant month:	Lowest	2nd lowest	3rd lowest	Lowest	2nd lowest	3rd lowest	Lowest	2nd lowest	3rd Lowest	Lowest	2nd lowest	3rd lowest
Actual Number of Grants	792	529	477	200	170	166	992	699	643	793	582	547
Actual/Total grants	6.6%	4.4%	4.0%	2.8%	2.4%	2.4%	5.2%	3.7%	3.4%	4.9%	3.6%	3.4%
Expected Number of Grants	253	256	254	126	126	125	379	382	378	322	324	321
Actual-Expected	539	273	223	74	44	41	613	317	265	471	258	226
(Actual-Expected)/Actual	68%	52%	47%	37%	26%	25%	62%	45%	41%	59%	44%	41%
Exercise Price/Median Stock Price	0.77	0.83	0.85	0.84	0.85	0.88	0.79	0.83	0.86	0.81	0.84	0.87

Panel B: Distribution of Grants by CEO and Firm

# Grants	CEOs	Actual # CEOs at Lowest Price	Expected # of Lucky Grants	Actual – Expected	(Actual - Expected) / Expected	(Actual - Expected) / Actual	(Actual - Expected) / Total
1	4510	280	102	178	175%	64%	4%
2	1874	179	74	105	142%	59%	6%
3	1050	157	59	98	167%	63%	9%
4	549	101	39	62	161%	62%	11%
>4	837	261	92	169	185%	65%	20%
All	8820	978	365	614	168%	63%	7%
	Firms	Actual # Firms					
1	1880	138	47	91	193%	66%	4.8%
2	1106	137	49	88	178%	64%	7.9%
3	860	135	50	85	169%	63%	9.9%
4	569	95	42	52	124%	55%	9.2%
>4	1404	474	170	305	180%	64%	21.7%
All	5819	979	359	620	173%	63%	10.7%

TABLE 19: SUPER-LUCKY GRANTS WITH HIGH LIKLIHOOD OF MANIPULATION

This table displays the number of grants at the lowest price of the quarter conditional on a number of variables. Those variables are: 1) Highest volatility quartile (decile). Volatility is defined as the distance between the lowest and the median price in a quarter. 2) Preceding grant was lucky. This dummy variable is equal to one if the CEO’s previous grant was at the lowest price of the month. 3) Not independent Board. This dummy is equal to one if the board is not majority independent. The variable is only available for a subset of firms with IRRC data available. We show statistics on the pre-SOX sample only. We also compute the expected number of grants given the number of days in a quarter where the price was the lowest price of the month. The unexpected number of grants is the difference between the actual and expected number of grants. The quarter is defined as the calendar quarter.

Pre-SOX	Observations	Actual Number at Lowest Price of Quarter	Actual/ Observations	(Actual-Expected) /Actual	Unexpected Number
Highest volatility quartile	2911	423	15%	88%	372
Highest volatility decile	1160	171	15%	88%	151
Preceding grant lucky	765	189	25%	92%	174
No independent board	713	99	14%	87%	86
At least one of: Highest volatility quartile, Preceding grant was lucky, No independent board	3861	600	16%	88%	529