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CEO compensation contagion: Evidence from an exogenous shock ☆

Frederick L. Bereskin [a,*], David C. Cicero [b]

[a] Lerner College of Business & Economics, University of Delaware, Newark, DE 19716, USA
[b] College of Business Administration, University of Tennessee, Knoxville, TN 37996, USA

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ABSTRACT

We examine how Chief Executive Officer (CEO) compensation increased at a subset of firms in response to a governance shock that affected compensation levels at other firms in the economy. We first show that Delaware-incorporated firms with staggered boards and no outside blockholders increased CEO compensation following the mid-1990s Delaware legal cases that strengthened their ability to resist hostile takeovers. Consistent with the Gabaix and Landier (2008) contagion hypothesis, non-Delaware firms subsequently increased CEO compensation when the rulings affected a substantial number of firms in their industries. We further show how these legal developments contributed significantly to the rapid increase in CEO compensation in the late 1990s.

1. Introduction

Executive compensation is one of the most controversial topics in financial economics. Average compensation for CEOs of Standard and Poors (S&P) 500 firms increased from just under $1 million in 1970 to over $14 million in 2000 (Jensen, Murphy, and Wruck, 2004). Much of this increase was concentrated in the 1990s, when average CEO compensation more than quadrupled. This striking pattern motivated a great deal of public discussion, academic study, and political action.[1]

Many, such as Bebchuk and Fried (2003, 2005), assert that the level of compensation rose indefensibly in this time period, and argue that agency problems and entrenchment of management are to blame. Other researchers provide analysis that suggests that the increase in CEO pay could largely reflect efficient outcomes in the labor market (Gabaix and Landier, 2008). In this paper, we consider two potentially important determinants of CEO compensation during the 1990s. The first issue is whether and how firms changed compensation practices in response to a shift in the legal

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* Corresponding author. Tel.: +1 302 831 4680/+1 865 974 716; fax: +1 302 831 3061.
E-mail addresses: bereskin@udel.edu (F.L. Bereskin), dcicero1@utk.edu (D.C. Cicero).

[1] See Jensen, Murphy, and Wruck (2004) for documentation of compensation trends during this period and a thorough discussion of the social and political environment and responses. More recent history has witnessed a leveling off of CEO compensation. According to Kaplan (2011), average CEO compensation peaked in 2000 and fell significantly to around $8 million as of 2009, with the largest declines coinciding with the two recessions in the decade. This pattern highlights the importance of understanding the economics of compensation practices in the U.S. in the 1990s.

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environment in which they operated. We test in this more recent setting the hypothesis of Bertrand and Mullainathan (1999a) that CEOs of firms without strong principals respond to increased takeover protection by skimming additional compensation. The next question we consider is whether there is compensation "contagion" as hypothesized by Gabaix and Landier (2008). Specifically, we consider whether firms not directly affected by the changing legal environment nonetheless changed CEO compensation in response to the actions of the directly affected firms. We find strong results on both of these fronts, and argue that together these forces amount to important determinants of the overall increase in CEO compensation during this time period.

We first examine the level of compensation at firms that experienced a shock to their governance environment. In 1995, the Delaware Supreme Court strengthened the ability of a target company's management to "just say no" to a hostile takeover threat.[2] As a result, the rules for resisting hostile takeovers were altered in an unexpected manner, and Delaware firms found themselves newly insulated from the corporate control market.[3] We find that CEOs at a subset of Delaware firms in which the managers likely gained the most security from the new laws extracted private benefits in the form of higher compensation. We estimate that the decision led abnormal CEO compensation to increase by approximately $694,000 (32.9%) at the firms in which managers gained the most security, which includes Delaware-incorporated firms with staggered boards that do not have a large outside blockholder.

Consistent with compensation contagion, we find that the increased compensation spilled over to other firms who likely compete with the entrenched Delaware firms for the same executive talent. In particular, we find a significant increase in the level of compensation at firms that are not incorporated in Delaware, yet operate in industries with a substantial number of firms that were directly affected by the legal changes. Our results suggest that annual CEO compensation at these indirectly affected firms increased by approximately $616,000 (34.1%) compared to a sample of unaffected firms. The compensation contagion is observed with a lag, consistent with firms responding after their competitors' compensation details are disclosed. In addition, we find that the higher compensation at competing firms persists. Consequently, this study provides evidence of how an exogenous shock to compensation for a subset of firms spreads to other firms in the economy.

Our results are related to the work on compensation peer groups. That literature shows that boards choose similar firms as benchmarks when determining how to compensate their executives (Bizjak, Lemmon, and Naveen, 2008). There is also evidence that some firms choose comparison groups strategically to justify higher compensation, although this practice may be declining with new disclosure rules (Faulkender and Yang, 2010; Bizjak, Lemmon, and Nguyen, 2011). To the extent that firms choose compensation peers efficiently, this practice should facilitate the spread of compensation shocks through the economy. If firms bias their peer groups to justify higher compensation, then a shock to compensation at a subset of firms could provide a boost to an already increasing trend. Nonetheless, the propagation of a compensation shock through the economy represents a determinant of the level of compensation that is distinct from what would be observed through the abuse of peer group analysis alone. We are not familiar with any other empirical work on this specific question of compensation contagion due to competitive forces.

In the next part of our analysis, we consider the extent of the impact of the Delaware legal changes on CEO compensation generally during this period. We start with regression specifications that include common determinants of CEO compensation across a broad cross-section of firms, but without accounting for the overall growth in average firm size. These specifications are consistent with others in the literature prior to the work of Gabaix and Landier (2008). We find that measures of otherwise *unexplained* compensation in these regressions are about 31% higher when we do not control for the impact of the Delaware legal changes compared to estimates from regressions that do capture this effect. This indicates that even in this limited framework, the Delaware legal developments are found to be an important determinant of the market-wide level of CEO compensation.

Finally, we consider how this shock relates to an equilibrium interpretation of CEO compensation. Gabaix and Landier (2008) greatly impacted the discussion of CEO compensation by solving a powerful competitive equilibrium model and presenting supporting empirics showing it could explain the sizeable observed compensation increases during this period.[4] In this part of our analysis, we first show that incorporating an important

[2] See the Appendix for a more thorough discussion of the relevant legal rulings.

[3] In particular, the firms that gained the most security from hostile takeovers were those with "staggered" or "classified" boards. A staggered board is one in which only a fraction of directors (typically one-third) is up for election in any year. A board that wishes to rebuff a takeover can adopt a poison pill (or shareholders' rights plan) that would automatically dilute an acquiror's ownership upon an effective takeover bid. This combination continues to provide a strong defense to hostile takeovers, as evidenced by the Delaware Chancery Court's recent decision to uphold Airgas Inc.'s use of a poison pill to fend off a hostile tender offer by Air Products and Chemicals, Inc. (*Air Products and Chemicals, Inc., v. Airgas, Inc.* (Chancery Court of Delaware, February 16, 2011)). Approximately 60% of public companies had staggered boards as of 1998, so the impact of the takeover defenses afforded under the new case law could be great. See Bebchuk, Coates, and Subramanian (2002) for an extensive analysis of the anti-takeover effect of staggered boards in conjunction with poison pills, and their strong evidence that hostile takeovers were greatly curtailed as a result of these legal developments.

[4] Gabaix and Landier (2008) present a general equilibrium model of CEO compensation in a framework in which compensation determines the match of particular CEOs to particular firms. Given CEOs with differing abilities, their model predicts that compensation will be a function of both the size of a CEO's firm and the average firm size in the economy. According to their "dual scaling equation," under the common assumption of constant returns to scale, when compensation is regressed onto firm size and average firm size, the coefficients of these two variables should sum to one.

additional data filter into empirical tests of the Gabaix and Landier "dual scaling equation" (their main theoretical result) weakens the fit of their baseline model to the data.[5] We then modify their empirical model to account for the Delaware legal changes. In this better-specified regression, we confirm that the Delaware legal changes were an important determinant of compensation, and find that the remaining increase in compensation indeed follows the fundamental relationships predicted by the competitive equilibrium model.

In summary, our paper provides evidence of compensation skimming by CEOs who became more entrenched as a result of landmark mid-1990s Delaware corporate takeover case law. CEOs of firms who compete with these newly insulated firms for executive talent subsequently enjoyed higher compensation too, consistent with compensation contagion driven by competitive forces. We show that the Delaware legal change is a major determinant of the overall compensation increases during this time period. Our results support a competitive equilibrium model of compensation augmented to account for this shock. Finally, we suggest that contagion of other exogenous shocks could help explain longer-term trends in CEO compensation. For example, other innovations in corporate takeover law throughout the two decades preceding our period of study may help explain the escalation of pay prior to the 1990s.

The remainder of this paper continues as follows. In Section 2 we discuss the related literature and develop the hypotheses that we examine in our paper. Section 3 continues by discussing our data and empirical approach. In Section 4 we discuss our primary results. Finally, we provide concluding remarks in Section 5.

2. Related literature and hypothesis development

2.1. The effect of Delaware anti-takeover rulings on compensation

The first part of our analysis focuses on whether a shock to Delaware corporate takeover law in the mid-1990s affected the manner in which firms compensate their CEOs.[6] Specifically, we examine the effects of innovations in Delaware case law around 1995 that significantly increased firms' ability to resist hostile takeovers. Low (2009) provides a discussion of the noteworthy Delaware cases that led to this exogenous shock in the corporate control environment, and we discuss the main cases in more detail in the Appendix. These legal developments made clear that boards of takeover targets would have wide discretion to maintain takeover defenses, including poison pills, even in the face of strong shareholder support for a takeover. As a result, it is likely that the only way a hostile bidder could acquire a defiant Delaware company would be to gain control of the board. Management of firms with staggered boards therefore experienced the greatest gains in security against takeover bids, since it would take two election cycles for shareholders in favor of a takeover to control the majority of board seats and rescind a poison pill.[7]

Since the outcome of these cases was uncertain, numerous researchers have treated the change as exogenous, and test whether these cases impacted corporate practices of Delaware firms with staggered boards. Bebchuk, Coates, and Subramanian (2002) find that between 1996 and 2000, no hostile bid won control of a firm that had an "effective" staggered board, defined as a staggered board that is designed to preclude circumvention. Subramanian (2004) shows that the positive value effect of Delaware incorporation disappears after the court rulings. Bebchuk and Cohen (2005) find that the value of firms with staggered boards tends to be lower after 1995. Rauh (2006) finds reduced employee ownership of employer stock in defined-contribution plans following the rulings, consistent with an entrenchment hypothesis. Finally, Yun (2009) shows that Delaware firms hold more cash after the rulings, but that this effect is smaller in firms with strong internal governance.

Previous literature discusses competing theories for how compensation practices would be modified in response to changes in takeover pressure. A hypothesis based on optimal contracting predicts that CEO compensation should not increase—and could decrease—given the decline in the risk of takeover (Brickley and James, 1987; Hubbard and Palia, 1995; Bertrand and Mullainathan, 1999a). In contrast, an entrenchment-based hypothesis predicts that CEOs with greater negotiating power will become more entrenched and will extract higher compensation from their firms (Bertrand and Mullainathan, 2001; Bebchuk, Fried, and Walker, 2002; Bebchuk and Fried, 2005; Kuhnen and Zwiebel, 2008; Cheng and Indjejikian, 2009).

Prior studies examine these two hypotheses in other contexts. DeAngelo and Rice (1983) focus on corporate charter bylaw amendments that make transfer of control more difficult, but fail to find a statistically significant relation to compensation practices. Agrawal and Knoeber (1998) find that in 1987, CEO pay was higher when the threat of takeover was higher. They find that the increase was concentrated in firms whose executives were not

[5] The data restriction we implement, and which was apparently not used in Gabaix and Landier (2008), is to restrict the sample to firms that report December fiscal year-ends. This restriction causes the measurement period for firm characteristics taken from annual reports to coincide in time and minimizes the level of noise in the regressions. Their conclusions were based in part on the inability to reject a null hypothesis, and we find that this is weaker in a sample limited to firms with December fiscal year-ends.

[6] Some other examples of studies in the governance literature that use natural experiments to proxy for exogenous shocks include Cheng, Nagar, and Rajan (2005), Rauh (2006), Giroud and Mueller (2010), Huang and Zhao (2010), and Becker and Stromberg (2012).

[7] A "poison pill" is formally known as a shareholder rights plan, and provides for dilution of the ownership interest of a shareholder once it crosses a certain ownership percentage threshold. Poison pills can take varying forms but with the same general objective. A typical pill is triggered when any shareholder crosses a threshold (say, 20% ownership) and would allow all other shareholders to purchase new shares in the company at a discount to market value, thus causing the owner's stake to be devalued and its voting rights diluted. A typical "staggered board," or classified board, provides for directors to serve three-year terms, and one-third of directors to be elected each year.

entitled to "golden parachutes" in the event of a takeover, and argue that the additional compensation makes up for the expected loss of future wages. In apparent contrast, Borokhovich, Brunarski, and Parrino (1997) find that in the 1980s, CEOs of companies that adopt anti-takeover charter amendments receive higher salaries and more valuable stock option grants, suggesting that an entrenchment effect dominates in these cases. However, the results of Borokhovich, Brunarski, and Parrino (1997) could be suspect because the decision to adopt anti-takeover provisions could have been driven by originally high levels of managerial entrenchment.

Bertrand and Mullainathan (1999a) provide perhaps the clearest evidence on this issue. They analyze changes in compensation around implementation of state anti-takeover laws in the 1980s (generally termed "second-generation anti-takeover legislation"), and find that firms react differently depending on the presence of a principal with a strong negotiating position.[8] CEO compensation rose the most at firms with no outside blockholders, suggesting that the impact of an unexpected change in the governance environment depends on firms' existing governance arrangements.[9] Therefore, when examining the impact of the new Delaware law on compensation, we control for the presence of an outside blockholder, and expect to find compensation skimming in the absence of a strong principal.[10]

Our focus is on those firms most likely to see an increase in managerial entrenchment following the legal rulings: firms incorporated in Delaware with classified boards. Since after the rulings a hostile takeover essentially required taking over the board of directors, managers at firms with classified boards were most insulated from takeover since it would take at least two rounds of director elections to secure a majority (Bebchuk, Coates, and Subramanian, 2002; Bebchuk and Cohen, 2005).[11]

2.2. Contagion of the exogenous shock

The second part of our analysis examines whether changes to CEO compensation at a subset of firms has an impact on other firms as well. In other words, we examine whether increases in CEO compensation are "contagious."

Gabaix and Landier (2008) present a competitive equilibrium model of executive compensation, in which CEOs are matched to firms and compensated based on their ability to increase firm value. CEOs are assigned to firms based on their marginal productivity, which is a function of skill and firm size. In equilibrium, the level of a CEO's compensation is determined by the CEO's personal ability, the size of the firm, and the size of other firms in the economy (which determines the CEO's outside opportunities). Consistent with their theory, Gabaix and Landier (2008) demonstrate empirically that increases in average firm-size is a possible explanation for the majority of the controversial run-up in CEO compensation between 1980 and 2003. However, some researchers question the power of this perspective to explain compensation trends in the U.S. (Bebchuk and Fried, 2003; Gordon and Dew-Becker, 2007; Cremers and Grinstein, 2010; Frydman and Saks, 2010).

Of most importance for our purposes, Gabaix and Landier's (2008) model predicts a "contagion effect" in CEO compensation that arises from competition. In their model, CEO compensation is partly a function of outside opportunities. Accordingly, if a subset of firms increases CEO compensation, there would be a subsequent increase in equilibrium wages at firms who compete for the same executive talent. In their calibrated model, Gabaix and Landier find that even if only 10% of firms attempt to pay twice as much as their competitors, equilibrium compensation of *all* CEOs would double. They note that this contagion effect could be an alternative explanation for the recent significant increase in average CEO compensation.[12]

Based on this intuition, we examine whether increased compensation at firms that become more entrenched following the legal rulings has an impact on compensation at firms that are not directly affected by the rulings. Specifically, we test whether firms that are least affected by the new Delaware case law (which we consider to be non-Delaware, non-staggered board firms), nonetheless subsequently increase CEO compensation when they compete in industries in which a significant fraction of firms were directly affected.[13] To our knowledge, this is the first paper to examine empirically this question of compensation contagion.

[8] "First-generation" anti-takeover legislation was introduced with 1968's Williams Act, followed by further regulations adopted by states in the 1970s. After these laws were deemed unconstitutional by the Supreme Court in 1982 (due to jurisdiction-related issues for states' laws), states adopted "second-generation" laws. The Supreme Court decided in 1987 that these laws are constitutional.

[9] We take no position on whether or not firms' governance arrangements were "optimal" prior to legal changes. However, we expect firms that choose different governance structures to react differently to unexpected disturbances. In the scenario presented here, it is reasonable that firms that choose to operate without a strong outside principal would be particularly vulnerable to the agency costs arising from a shift in the CEO's bargaining position.

[10] This expectation is also consistent with Cyert, Kang, and Kumar (2002) and Core, Holthausen, and Larcker (1999), who note that the presence of an outside blockholder is associated with lower levels of CEO entrenchment and compensation.

[11] Poison pills can generally be introduced freely by management (Daines and Klausner, 2001). We follow Daines and Klausner (2001) and do not consider whether sample firms actually have poison pills in place, since management can easily adopt a pill at its discretion. In contrast, a staggered board is either adopted in the corporate charter or its bylaws, making their status difficult to change. Bebchuk and Cohen (2005) note that approximately 10% of firms' staggered boards are established in firms' bylaws, and would require shareholder approval to amend. Staggered boards established in the firm's charter cannot be amended by shareholders without board initiative.

[12] This effect is not symmetric—Gabaix and Landier also note that if 10% of firms choose to pay their CEOs half as much as their competitors, total CEO compensation decreases by only 9%.

[13] To construct the strongest tests of compensation contagion possible, we focus on the firms that should compete the most aggressively for the same executives, i.e., those in the same industry. Gabaix and Landier (2008) predict a market-wide increase in compensation, but we expect firms from other industries to respond with an additional time lag as the compensation shock begins to affect a broader cross-section of the economy. See the discussion of Table 4 for evidence of eventual contagion of the compensation shock to the overall economy.

3. Data and empirical methods

The first part of our analysis focuses on the level of compensation at Delaware staggered-board firms versus other firms. Due to endogeneity concerns, we employ a differences-in-differences methodology that is used in previous literature (Bertrand and Mullainathan, 1999a,b, 2003, Yun, 2009; Low, 2009). The general form of the differences-in-differences regression is

$$y_{i,t} = \alpha_t + \beta_i + \gamma X_{i,t-1} + \delta_1 Del_i \times ClassBoard_i + \delta_2 \times Del_i \times After_t + \delta_3 \times Del_i \times ClassBoard_i \times After_t + \epsilon_{i,t}, \quad (1)$$

where i indicates firm; t indicates year; α captures the year effect; β captures firm fixed-effects; $X_{i,t-1}$ is a vector of firm-specific control variables (we lag the control variables by one year); Del_i is a dummy indicating incorporation in Delaware as of 1994; $ClassBoard_i$ indicates the firm has a classified board as of 1994; and $After_t$ indicates a year following the 1995 legal rulings in Delaware. Because year and firm fixed-effects are included in our regressions, we do not include *After*, *Del*, and *ClassBoard*. The coefficient of interest is δ_3, which indicates whether there is a change in compensation for Delaware-incorporated firms with staggered boards around the legal changes that is different from the changes observed at other firms.

The dependent variable of interest is the log of the total level of compensation. Specifically, we use the log of TDC1 from Compustat which includes salary, bonus, other annual compensation, the total value of restricted stock granted in the year, the total value of stock options granted (using Black-Scholes), long-term incentive payouts, and all other compensation.

Data on firm characteristics are taken from Compustat, executive compensation data come from Execucomp, and we extract staggered board and other governance data from RiskMetrics. We use the Thomson-Reuters Institutional Holdings (13F) Database to identify whether or not a firm had an outside equity blockholder who owned 5% or more of the outstanding shares. For our differences-in-differences tests, we require that a firm be in existence in 1994 (the year before the legal changes) and that we can determine whether the firm had a staggered board in 1994. We exclude any firm that changes its staggered-board status during our time period. We exclude firms that do not report a December fiscal year-end, to make our compensation and firm characteristic data directly comparable across firms; this is especially important given the nature of the exogenous shock, and how its contagion effect occurs with a lag.[14] These data restrictions cause many firms to be dropped from our analysis, and therefore our sample size is not large. The final sample for our main differences-in-differences tests includes 589 firms and 2,617 firm-years.

For tests focused on the contagion of compensation practices to other firms, we allow the addition of new firms into our sample over time. This is because we are concerned only with the contemporaneous spillover effects, and therefore do not have the same endogeneity concerns that were present in the first set of tests. Our full data set for these tests includes 204 firms and 926 firm-years, and falls into three groups: (1) firms that are not incorporated in Delaware, do not have staggered boards, and in which *less than* 10% of the firms in the two-digit Standard Industrial Classification (SIC) industry-group are Delaware-incorporated with staggered boards and no blockholder ("Not affected"); (2) firms that are not in Delaware, do not have staggered boards, and in which *at least* 10% of the firms in the two-digit SIC industry-group are Delaware-incorporated with a staggered board and no blockholder ("Affected ind"); and (3) Delaware-incorporated firms with a staggered board and no blockholder ("Affected firm").[15]

In addition to year and firm fixed-effects, we include a number of other control variables in our regressions, based on factors other researchers have found to affect compensation. Controls include the following:

1. *LogTotalFirmValue*—The log of total firm value as defined in Gabaix and Landier (2008), which includes the market value of equity and the book value of debt.
2. *InstiOwn%*—The fraction of institutional ownership of the outstanding common shares.
3. *Leverage*—The ratio of debt to total value of the firm.
4. *Q*—Tobin's *Q*, defined as the market value of the firm divided by replacement value of assets.
5. *LogTenure*—The log of one plus the number of years the CEO has been in his position.
6. Δ*LogShareholderWealth*—The change in the log of firm value in the year the compensation is paid.
7. *ROA*—Income divided by total assets.

In addition, we control for two aspects of an executive's pay-for-performance sensitivity. The sensitivity of a CEO's total wealth to performance as of the beginning of the year is captured by the *ScaledWPS* measure provided in Edmans, Gabaix, and Landier (2009).[16] The effect of new awards and options is captured by PPS_{Grant}, defined as the change in the dollar value of the executive's stock and stock option compensation received in that year associated with a one thousand dollar change in firm value. PPS_{Grant} is calculated as

$$PPS_{Grant} = 1{,}000 \times \frac{\text{Shares awarded} + \Delta \times \text{Shares represented by options awarded}}{\text{Total shares outstanding}}, \quad (2)$$

where Δ is the option delta. For our main tests we present regressions both with and without variables controlling for CEOs' wealth- and pay-for-performance sensitivities (PPS).

[14] Our results are robust to including firms with non-December fiscal year-ends.

[15] The median percentage of directly affected firms across industries for our sample period is 10%, which is the reason that we take 10% as our cutoff point for whether an industry has relatively many "affected" firms.

[16] We thank Alex Edmans for providing the scaled wealth-performance sensitivity (WPS) measures on his Web site.

Table 1
Sample statistics.

This table provides the number of observations, mean, and median values for Delaware and non-Delaware firms. *ClassBoard* is a dummy variable equal to one if the firm had a classified board in 1994. *Compensation* is total compensation. *ScaledWPS* is the scaled wealth-performance sensitivity from Edmans, Gabaix, and Landier (2009) divided by 100. PPS_{Grant} is pay-for-performance sensitivity derived from option and stock grants in that year. *TotalFirmValue* is the firm-value as calculated in Gabaix and Landier (2008) (the sum of the value of debt and equity). *InstiOwn%* is the percentage of equity owned by institutional investors. *Leverage* is firm leverage (using the market-value of equity). *Q* is the market value of the firm divided by the replacement value of its assets. *Tenure* is the CEO's tenure. *ROA* is the return on assets. Δ*ShareholderWealth* is the change in the market value of equity compared to the prior year. Sample values are for years in either 1992–1994 or 1997–1999. *, **, And *** denote significant differences from zero at the 10%, 5%, and 1% levels, respectively for *t*-tests (Wilcoxon tests) of the differences in means (medians) between Delaware and non-Delaware firms.

Panel A: Firms with no blockholder

	Non-Delaware firms			Delaware firms		
	N	Mean	Median	N	Mean	Median
ClassBoard (0/1)	594	0.59		551	0.57	
Compensation ($million)	594	3.45	1.61	551	4.52***	2.65***
ScaledWPS	594	0.20	0.05	551	0.30**	0.07***
PPS_{Grant}	594	0.98	0.20	551	1.42*	0.33***
TotalFirmValue ($million)	594	24,348	5,958	551	24,455	6,781
InstiOwn%	594	0.24	0.21	551	0.26	0.19
Leverage	594	0.21	0.21	551	0.21	0.19
Q	594	1.65	1.28	551	1.84***	1.45***
Tenure (years)	594	5.96	4.00	551	7.52***	5.00**
ROA	594	0.04	0.04	551	0.04	0.04
Δ*ShareholderWealth* ($million)	594	854	71	551	1,188	109

Panel B: Firms with blockholder

	Non-Delaware firms			Delaware firms		
	N	Mean	Median	N	Mean	Median
ClassBoard (0/1)	693	0.59		779	0.54*	
Compensation ($million)	693	3.02	1.86	779	3.88***	2.17***
ScaledWPS	693	0.24	0.06	779	0.28	0.07*
PPS_{Grant}	693	1.34	0.59	779	1.86***	0.62
TotalFirmValue ($million)	693	12,955	3,098	779	12,448	3,821*
InstiOwn%	693	0.59	0.60	779	0.63***	0.64***
Leverage	693	0.19	0.18	779	0.18	0.16*
Q	693	1.78	1.37	779	1.76	1.45**
Tenure (years)	693	6.67	4.00	779	7.23	5.00
ROA	693	0.04	0.04	779	0.04	0.04
Δ*ShareholderWealth* ($million)	693	86	50	779	184	51

4. Results

4.1. Changes in CEO compensation at firms directly affected by the Delaware legal changes

In our first set of tests, we evaluate changes in CEO compensation at firms that were directly impacted by the 1995 Delaware legal changes. For these tests we have constructed two samples of firms, separated by whether there was an outside equity blockholder present in the year before the compensation was paid. Table 1 provides summary statistics for these samples. Panel A compares Delaware and non-Delaware firms with no outside blockholder, and Panel B does the same for firms with a blockholder. The incidence of classified boards is similar across both Delaware and non-Delaware firms in both samples, at about 60% of firms. Total compensation, scaled wealth-performance sensitivity, and pay-for-performance sensitivity are higher among Delaware firms for both the sample with blockholders and the sample without blockholders. These differences highlight the importance of our use of a differences-in-differences methodology. In Panel A, the overall level of institutional ownership is low, which is not surprising given that there is no outside blockholder for this group. The tenure of CEOs at Delaware-incorporated firms also tends to be somewhat longer in Panel A. This could be related to the higher value of Tobin's *Q* for the Delaware firms, but is also consistent with greater entrenchment of managers at Delaware firms in the absence of an influential shareholder. In contrast, CEO tenure is similar across firms in Panel B. The values for all other control variables are typical, and are not different across our Delaware and non-Delaware firms in either sample.

In Table 2, we evaluate whether firms that are more likely to become entrenched pay higher levels of compensation following the legal rulings. We examine firm-years from 1992 through 1999. To implement the cleanest tests possible, we exclude firm-years in 1995 and 1996.[17] Following Bertrand and Mullainathan (1999a), who show

[17] *Unitrin* was decided by the Delaware Supreme Court on January 11, 1995, and could have impacted compensation arrangements of some firms during 1995. The U.S. District Court decided the *Moore* case on December 4, 1995. Consequently, it is unclear how many firms would have responded to the ruling when setting compensation arrangements for 1996.

Table 2
Effect of Delaware anti-takeover rulings on CEO compensation.

This table examines the determinants of CEO compensation prior to and following the legal rulings in 1995. The dependent variable is the natural log of total compensation. *DelawareAfter* is a dummy variable equal to one if the firm was incorporated in Delaware in 1994, and the year is 1997 or later. *ClassBoardAfter* is a dummy variable equal to one if the firm had a classified board in 1994, and the year is 1997 or later. *DelClassBoardAfter* is a dummy variable equal to one if the firm was incorporated in Delaware in 1994, had a classified board in 1994 and the year is 1997 or later. *ScaledWPS* is the scaled wealth-performance sensitivity from Edmans, Gabaix, and Landier (2009) divided by 100. PPS_{Grant} is pay-for-performance sensitivity derived from option and stock grants in that year. *LogTotalFirmValue* is the natural log of firm-value as calculated in Gabaix and Landier (2008) (the sum of the value of debt and equity). *InstiOwn%* is the percentage of equity owned by institutional investors. *Leverage* is firm leverage (using the market-value of equity). *Q* is the market value of the firm divided by the replacement value of its assets. *LogTenure* is the natural log of one plus the CEO's tenure. *ROA* is the return on assets. Δ*LogShareholderWealth* is the change in the natural log of the market value of equity compared to the prior year. Δ*LogShareholderWealthNegative* is a dummy variable equal to one if Δ*LogShareholderWealth* is negative. *Year*1993, *Year*1994, *Year*1997, *Year*1998, and *Year*1999 are indicator variables if the year is 1993, 1994, 1997, 1998, or 1999, respectively. Sample compensation values are for years in either 1992–1994 or 1997–1999. Firm and year fixed-effects are included. Robust firm-clustered *t*-statistics are provided in parentheses below the coefficient value. *, **, And *** denote significant differences from zero at the 10%, 5%, and 1% levels, respectively.

	Firms with no blockholder			Firms with blockholder		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
DelawareAfter	−0.257**	−0.251**	−0.250**	0.128	0.083	0.106
	(−2.480)	(−2.200)	(−2.240)	(1.439)	(0.828)	(1.035)
ClassBoardAfter	−0.056	−0.055	−0.061	0.085	0.010	0.055
	(−0.570)	(−0.543)	(−0.589)	(1.117)	(0.117)	(0.622)
DelClassBoardAfter	0.284**	0.275*	0.273*	−0.181	−0.046	−0.087
	(2.103)	(1.801)	(1.801)	(−1.577)	(−0.351)	(−0.649)
ScaledWPS	−0.173*	−0.231*		−0.168***	−0.230***	
	(−1.796)	(−1.888)		(−7.500)	(−6.272)	
PPS_{Grant}	0.117***			0.121***		
	(8.362)			(15.223)		
LogTotalFirmValue	0.428***	0.389***	0.353***	0.381***	0.237***	0.251***
	(6.699)	(4.679)	(4.455)	(5.330)	(3.151)	(3.026)
InstiOwn%	−0.314*	−0.315	−0.332	0.041	0.134	0.055
	(−1.696)	(−1.504)	(−1.617)	(0.232)	(0.562)	(0.204)
Leverage	−0.083	−0.313	−0.257	−0.396	−0.374	−0.495
	(−0.321)	(−0.985)	(−0.811)	(−1.630)	(−1.243)	(−1.590)
Q	0.015	0.027	0.025	0.081**	0.079*	0.007
	(0.417)	(0.478)	(0.417)	(2.283)	(1.961)	(0.109)
LogTenure	0.065**	0.048	0.033	0.020	−0.031	−0.066*
	(2.006)	(1.288)	(0.888)	(0.898)	(−1.005)	(−1.894)
ROA	0.331	0.091	0.070	0.613**	0.595*	0.699**
	(1.238)	(0.307)	(0.235)	(2.497)	(1.902)	(2.063)
Δ*LogShareholderWealth*	0.014	0.004	0.003	0.010	0.001	−0.002
	(0.637)	(0.144)	(0.115)	(0.565)	(0.035)	(−0.114)
Δ*LogShareholderWealthNegative*	−0.140***	−0.148***	−0.135***	−0.186***	−0.181***	−0.170***
	(−4.430)	(−4.232)	(−3.906)	(−7.115)	(−5.845)	(−5.384)
Δ*LogShareholderWealth*	−0.017	−0.005	−0.004	−0.013	−0.004	−0.001
×Δ*LogShareholderWealthNegative*	(−0.755)	(−0.159)	(−0.137)	(−0.670)	(−0.240)	(−0.084)
Year1993	0.074	0.096	0.117*	0.101*	0.109*	0.114*
	(1.433)	(1.612)	(1.928)	(1.699)	(1.715)	(1.772)
Year1994	0.225***	0.236***	0.261***	0.226***	0.219***	0.231***
	(4.558)	(4.090)	(4.505)	(3.881)	(3.444)	(3.541)
Year1997	0.457***	0.546***	0.581***	0.357***	0.492***	0.474***
	(4.791)	(5.688)	(6.144)	(3.890)	(4.990)	(4.656)
Year1998	0.571***	0.645***	0.682***	0.359***	0.546***	0.525***
	(5.421)	(6.338)	(6.854)	(3.824)	(5.380)	(5.029)
Year1999	0.756***	0.862***	0.912***	0.557***	0.778***	0.786***
	(7.246)	(7.974)	(8.577)	(5.709)	(7.327)	(7.232)
Constant	4.530***	5.595***	6.320***	5.609***	8.929***	8.799***
	(3.226)	(3.083)	(3.631)	(3.846)	(5.782)	(5.321)
N	1,145	1,145	1,145	1,472	1,472	1,472
R^2	52.5%	37.8%	36.6%	64.6%	40.0%	34.1%
Firm fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes

that the effects of the rulings are most likely to be concentrated among firms with less powerful external monitors, we separate our sample between firms without blockholders (the first three columns of Table 2) and firms with blockholders (the last three columns).

The first three columns of Table 2 show that CEOs at firms where managerial entrenchment likely increased the most (firms without blockholders) receive higher compensation following the rulings. This conclusion is drawn from the coefficients associated with *DelClassBoardAfter*, which is an indicator variable equal to one for firm-years in 1997 or later for firms incorporated in Delaware that had a staggered board. This coefficient in the first specification is 0.284, indicating an approximate $694,000 (32.9%) average increase in compensation at these firms after the legal changes.[18] The other coefficients in the regression are reasonable. Firm size and CEO tenure are associated with significantly higher levels of compensation. There is a positive relationship between the pay-for-performance sensitivity and level of current compensation, indicating that pay packages that increase incentives more are larger. However, there is a negative relationship between current compensation and the pre-existing wealth-performance sensitivity (*ScaledWPS*), suggesting that CEOs whose wealth will already change more with firm value do not need as much new compensation. The second column shows that our results are robust to omitting PPS_{Grant} from our regression, and the third column shows that our results are robust to omitting *ScaledWPS* as well, although there is a decrease in R^2 and an increase in standard errors causing significance to drop to the 10% level.

DelawareAfter is significantly negative in these three regressions, demonstrating smaller increases in compensation at other Delaware firms relative to non-Delaware firms in the late 1990s. This could reflect the fact that managers of Delaware firms without classified boards became relatively less entrenched after the ruling. This would be the case if there is similar demand for acquisition targets after the legal rulings, but it becoming less feasible to acquire Delaware firms with classified boards. The positive trend of the year fixed-effects suggests that compensation not associated with our control variables is consistently increasing, and in consistently larger intervals. The coefficients associated with the other variables of interest must be interpreted relative to this increasing trend. Moreover, untabulated results show that otherwise unexplained compensation at Delaware firms was higher than compensation at non-Delaware firms in the early 1990s. Therefore, our results are consistent with larger overall increases in unexplained compensation in the late 1990s, with a narrowing of the gap between Delaware firms without classified boards and non-Delaware firms. In contrast, Delaware firms with classified boards experienced larger increases in compensation during this period.[19]

The last three columns of Table 2 examine the effect of the legal changes on firms with blockholders. This regression indicates that *DelClassBoardAfter* is negative and insignificant, while both *DelawareAfter* and *ClassBoardAfter* are positive and insignificant. Although they are insignificant, these results point to the possibility that firms with blockholders efficiently reshape CEO compensation to reflect shifts in the risk of a contest for corporate control. Holding the demand for takeover targets constant, the increase in protection for Delaware-incorporated firms with classified boards will be associated with a relative decline in protection at other similar firms, including Delaware-incorporated firms without classified boards and firms incorporated outside of Delaware that have classified boards. The lower level of compensation at Delaware-incorporated firms with classified boards is consistent with efficient contracting that no longer compensates these managers for the risk of takeover, whereas the higher compensation associated with the other coefficients could reflect additional compensation to account for the increased takeover risk.

In Table 3, we extend our tests to control for firm-specific governance variables. We examine the effects of two governance characteristics often associated with CEO entrenchment: CEO equity ownership (Morck, Shleifer, and Vishny, 1988) and the percentage of insiders serving on the board of directors. We only study firms without blockholders, since these are the firms found to increase their compensation after the legal rulings.[20] The regressions in columns 1 and 2 control for CEO ownership, first with an indicator variable for the top quartile of CEO ownership (*CEOOwnDum*) and then with a continuous variable for the level of CEO ownership (*%CEOOwn*). We find that the value associated with *DelClassBoardAfter* is not reflected among firms with low levels of CEO ownership. For example, in both regressions we find an insignificant value associated with *DelClassBoardAfter* and a significant positive coefficient on the interaction terms $CEOOwnDum \times DelClassBoardAfter$ (in column 1) and $\%CEOOwn \times DelClassBoardAfter$ (in column 2).

The results are similar when controlling for the percentage of insiders serving on the board of directors (column 3). This regression includes a continuous variable (*%Insider*) that indicates the percentage of the board represented by insider directors. Although *DelClassBoard* equals zero for firms with relatively independent boards, the interaction term $\%Insider \times DelClassBoardAfter$ indicates that the effect of the legal rulings is concentrated in Delaware-firms with classified boards and relatively low levels of board independence.[21]

In this section, we have presented evidence of CEO compensation increases at firms where managers became more entrenched following the judicial rulings in

[18] The marginal effect is estimated as the average change in total compensation when *DelClassBoardAfter* is set equal to one compared to the value if we set *DelClassBoardAfter* equal to zero.

[19] In unreported tests, we analyze whether the increases in CEO compensation are driven by either the fixed or incentive component of the compensation packages, and find that neither component is disproportionately increased. This result goes against an argument that firms can increase the incentive component of entrenched managers' pay so that they will more likely accept a hostile takeover offer that would cause their restricted stock and options to vest early.

[20] Compared to our tests in Table 2, our sample size declines slightly since not all firms have data for CEO ownership or board representation.

[21] In unreported robustness tests, we consider the sensitivity of our results to the 1994 changes to the corporate tax deductibility of CEO compensation. Internal Revenue Service (IRS) Code section 162(m), which went into effect in 1994, limits the deductibility of non-performance-based cash compensation to $1 million per year. There is some evidence that this change caused firms to increase performance-based pay relative to fixed compensation, although the overall evidence on this question is weak (Perry and Zenner, 2001; Rose and Wolfram, 2000, 2002). It is possible that firms more affected by the rule change responded by increasing the overall value of compensation to offset the additional risk borne by the CEOs. We therefore conduct our main tests on separate subsamples of firms that either were or were not constrained by the new tax law and find that our results are unaffected.

Table 3
Firm-specific governance and the effect of Delaware anti-takeover rulings on CEO compensation—incremental effect of governance variables.

This table examines the determinants of CEO compensation prior to and following the legal rulings in 1995 for firms without blockholders. The dependent variable is the natural log of total compensation. We add additional explanatory variables to model 1 in Table 2, and for brevity, are not reporting the coefficients of the following explanatory variables used in Table 2: *ScaledWPS*, PPS_{Grant}, *LogTotalFirmValue*, *InstiOwn%*, *Leverage*, *Q*, *LogTenure*, *ROA*, *ΔLogShareholderWealth*, *ΔLogShareholderWealthNegative*, and *ΔLogShareholderWealth* × *ΔLogShareholderWealthNegative*. In column 1 we add an indicator variable that is equal to one if CEO equity ownership as a percentage of total equity is in the top quartile (*CEOOwnDum*) and zero otherwise; in column 2 we add a continuous variable for the level of CEO equity ownership as a percentage of total equity (*%CEOOwn*); in column 3 we add a variable for the percentage of the board composed of insider directors (*%Insider*). We interact *CEOOwnDum*, *%CEOOwn*, and *%Insider* with *DelawareAfter*, *ClassBoardAfter*, and *DelClassBoardAfter*. Sample compensation values are for years in either 1992–1994 or 1997–1999. Firm and year fixed-effects are included. Robust firm-clustered *t*-statistics are provided in parentheses below the coefficient value. *, **, and *** denote significant differences from zero at the 10%, 5%, and 1% levels, respectively.

Variable	(1)	(2)	(3)
DelawareAfter	−0.130	−0.141	0.067
	(−0.758)	(−0.766)	(0.476)
ClassBoardAfter	−0.102	−0.178	0.100
	(−0.496)	(−0.924)	(0.633)
DelClassBoardAfter	0.216	0.212	0.027
	(0.860)	(0.847)	(0.144)
CEOOwnDum	0.107		
	(0.779)		
CEOOwnDum ×*DelawareAfter*	−0.307*		
	(−1.891)		
CEOOwnDum ×*ClassBoardAfter*	−0.417**		
	(−2.379)		
CEOOwnDum ×*DelClassBoardAfter*	0.612**		
	(2.104)		
%CEOOwn		0.074***	
		(3.015)	
%CEOOwn ×*DelawareAfter*		−0.480	
		(−1.550)	
%CEOOwn ×*ClassBoardAfter*		−0.165**	
		(−2.597)	
%CEOOwn ×*DelClassBoardAfter*		0.650**	
		(1.979)	
%Insider			0.589**
			(2.222)
%Insider ×*DelawareAfter*			−0.818**
			(−2.565)
%Insider ×*ClassBoardAfter*			−0.694**
			(−2.136)
%Insider ×*DelClassBoardAfter*			1.051**
			(2.531)
Constant	6.331***	6.938***	3.887**
	(2.673)	(2.879)	(2.590)
N	414	414	635
R^2	65.0%	65.4%	62.8%
Firm and year fixed-effects	Yes	Yes	Yes

Delaware. This pattern is evident among firms with staggered boards and no outside blockholders, particularly when the CEO has a larger ownership stake and when the board is less independent. In the following section, we examine whether other firms are affected by this exogenous shock to their competitors.

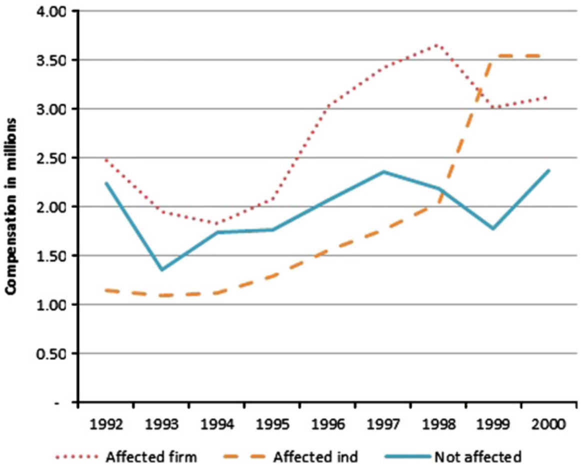

Fig. 1. Compensation across affected groups, 1992–2000. This figure presents the median compensation across three groups of firms: (1) Delaware-incorporated firms with a staggered board and no blockholder (“Affected firm”); (2) firms that are not incorporated in Delaware, do not have staggered boards, and in which *at least*10% of the firms in the two-digit SIC industry-group are Delaware-incorporated with a staggered board and no blockholder (“Affected ind”); and (3) firms that are not incorporated in Delaware, do not have staggered boards, and in which *less than*10% of the firms in the two-digit SIC industry-group are Delaware-incorporated with staggered boards and no blockholder (“Not affected”).

4.2. The contagion of CEO compensation levels

As discussed above, we hypothesize that changes in CEO compensation at a subset of firms will affect how other competing firms compensate their chief executives. We draw on Gabaix and Landier (2008) to support this hypothesis with arguments about CEOs’ outside opportunities.

We begin this part of our analysis by separating firms into three groups, and comparing their CEOs’ compensation. In Fig. 1, we show the median compensation during the 1990s for these groups: (1) firms that are incorporated in Delaware, have staggered boards, and no blockholders (“Affected firm” group); (2) firms that are not incorporated in Delaware, do not have staggered boards, and in which less than 10% of firms in the two-digit SIC industry-group are “Affected” firms (“Not affected industry” group); and (3) firms that are not incorporated in Delaware, do not have staggered boards, but in which more than 10% of other firms in the two-digit SIC industry-group are in the Affected firms group (“Affected industry” group).[22] Since our objective is to isolate those firms clearly not directly affected by the Delaware legal changes, we exclude from this analysis firms that have a classified board and are incorporated outside of Delaware, and those that do not have a classified board but are incorporated in Delaware. Firms with these characteristics can be affected directly, albeit less drastically, by the legal changes.

[22] We remind the reader that the median percentage of directly affected firms across industries for our sample period is 10%, and we have grouped non-Delaware firms accordingly.

Consistent with our previous analysis, Fig. 1 shows that average CEO compensation increases sharply in the Affected firm group from 1994 through 1997, the period surrounding the Delaware rulings. CEOs of firms in the Affected industry group also receive higher compensation, with the increases occurring at a lag compared to the Affected firms. Consistent with compensation contagion, the increases in this group occur with an approximate two- or three-year delay compared to Affected firms. This would be expected if firms compare their CEOs' compensation to that of their peers, since it would take one year to observe public reporting of the higher level of compensation and likely another year to incorporate it into their CEOs' compensation contracts. Finally, the figure shows that firms in the Not affected industry group actually have no notable increase in compensation in the late 1990s. This is a surprising result given the general run-up in compensation during this period, and highlights the dramatic impact of the Delaware legal rulings.

While Fig. 1 is instructive, we consider whether a similar pattern is still evident when we control for the other possible determinants of compensation. We estimate the following regression, and compare the individual year effects from a regression that only includes one set of year dummies to the baseline and incremental year

Table 4

Compensation-regression year fixed-effects—effect of controlling for affected firms and affected industries.

This table presents the value of group-specific year-effects from regressions for compensation that: (1) does not incorporate whether firms or their industries are affected by the 1995 Delaware legal rulings, or (2) controls for whether firms are affected by the legal rulings in Delaware in 1995 or are in industries with a majority of firms that were affected:

$$Comp_{i,t} = ScaledWPS_{i,t} + PPS_{Grant-i,t} + LogTotalFirmValue_{i,t-1} + InstiOwn\%_{i,t-1} + Lev_{i,t-1} + Q_{i,t-1} + LogTenure_{i,t-1} + ROA_{i,t} + \Delta LogShareholderWealth_{i,t} + \Delta LogShareholderWealthNegative_{i,t} + \Delta LogShareholderWealth_{i,t} \times \Delta LogShareholderWealthNegative_{i,t} + \sum_{t=1992}^{2000} YearDum_t. \quad (4)$$

$$Comp_{i,t} = ScaledWPS_{i,t} + PPS_{Grant-i,t} + LogTotalFirmValue_{i,t-1} + InstiOwn\%_{i,t-1} + Lev_{i,t-1} + Q_{i,t-1} + LogTenure_{i,t-1} + ROA_{i,t} + \Delta LogShareholderWealth_{i,t} + \Delta LogShareholderWealthNegative_{i,t} + \Delta LogShareholderWealth_{i,t} \times \Delta LogShareholderWealthNegative_{i,t} + \sum_{t=1992}^{2000} YearDum_t + \sum_{t=1992}^{2000} YearDum_t \times AffectedInd_{i,t} + \sum_{t=1992}^{2000} YearDum_t \times AffectedFirm_{i,t}. \quad (5)$$

Comp is the natural log of compensation. The left panel provides the year fixed-effect ($YearDum_t$) from Model (1), the right panel provides the year fixed-effects and their interactions with *AffectedInd* and *AffectedFirm* from Model (2). $AffectedInd_{i,t}$ is an indicator variable equal to one if the firm is: (1) Not incorporated in Delaware and does not have a staggered board, and (2) more than 10% of its two-digit SIC industry-group is Delaware-incorporated with staggered boards and no blockholder. $AffectedFirm_{i,t}$ is an indicator variable equal to one if the firm is Delaware-incorporated with a staggered board and no blockholder. Base case applies to any record for which both $AffectedInd_{i,t}$ and $AffectedFirm_{i,t}$ are equal to zero. All other dependent variables are as defined in Table 2. Standard errors are clustered by firm. *, **, and *** denote significant differences from zero at the 10%, 5%, and 1% levels, respectively.

Model (1) *YearDum*	
1992	0
1993	0.073
1994	0.169***
1995	0.137*
1996	0.299***
1997	0.320***
1998	0.417***
1999	0.618***
2000	0.588***
N	1,673
R^2	65.8%

Model (2) *YearDum ×*	1 (Base case)	*AffectedInd*	*AffectedFirm*
1992	0	−0.279	−0.01
1993	0.113	−0.360***	−0.048
1994	0.142	−0.103	−0.123
1995	0.048	−0.05	−0.011
1996	0.196	−0.128	0.118
1997	0.217	−0.123	0.122
1998	0.159	0.018	0.381***
1999	0.317	0.288***	0.280**
2000	0.379*	0.158	0.171
N	1,673		
R^2	67.0%		

dummy interactions from the full regression:

$$\begin{aligned} Comp_{i,t} = {} & ScaledWPS_{i,t} + PPS_{Grant-i,t} \\ & + LogTotalFirmValue_{i,t-1} + InstiOwn\%_{i,t-1} \\ & + Lev_{i,t-1} + Q_{i,t-1} + LogTenure_{i,t-1} + ROA_{i,t-1} \\ & + \Delta LogShareholderWealth_{i,t} \\ & + \Delta LogShareholderWealthNegative_{i,t} \\ & + \Delta LogShareholderWealth_{i,t} \\ & \times \Delta LogShareholderWealthNegative_{i,t} \\ & + \sum_{t=1992}^{2000} YearDum_t + \sum_{t=1992}^{2000} YearDum_t \\ & \times AffectedInd_{i,t} + \sum_{t=1992}^{2000} YearDum_t \\ & \times AffectedFirm_{i,t}, \end{aligned} \tag{3}$$

where the control variables are defined as in earlier tables. Additionally, *AffectedInd* is an indicator variable equal to one if the firm is: (1) not incorporated in Delaware and does not have a staggered board, and (2) more than 10% of its two-digit SIC industry-group is Delaware-incorporated with staggered boards and no blockholder. *AffectedFirm* is an indicator variable equal to one if the firm is Delaware-incorporated with a staggered board and no blockholder. We include a longer time period of observations than before in this regression to show the persistence of the changes in CEO compensation across the 1990s.

Table 4 and Fig. 2 compare the year fixed-effects in Eq. (3) above with a regression that does not interact the year dummies with *AffectedInd* or *AffectedFirm*. The table shows that in Model 4 without incremental year interaction terms, there is an increasing trend in the otherwise unexplained compensation over this period that is highly significant. By comparison, Model 5 includes the incremental year dummy interactions. The baseline year dummies in Model 5 are now generally insignificant, and the incremental dummies' values are as expected. Panel A of Fig. 2 contrasts the baseline year dummies from Model 4 with those of Model 5, showing the decline in unexplained compensation when one accounts for the Delaware legal changes. Moreover, the incremental year dummies in Panel B show a pattern consistent with compensation contagion. The year dummies interacted with *AffectedFirm* become positive and significant first, in 1998, followed by the interactions with *AffectedInd* in 1999. It is interesting that as of 2000, although both of these incremental dummies remain positive, they are insignificant but the baseline dummy has turned significant and positive. This pattern suggests that higher compensation spread first from the firms directly affected by the Delaware legal changes to those who compete with them directly for talent, and subsequently to other firms across the economy.

We explore compensation contagion in a differences-in-differences framework in Panel A of Table 5, with a regression analysis that contrasts compensation levels before and after the rulings. Unlike in Section 4.1, here we omit 1997 from the "after" period since the compensation for many of the affected firms might not have been made public as of 1997 and therefore, industry peers might not yet have responded to these changes. We now also include 1995 in the before-period sample since we are not concerned about these firms adjusting their compensation in immediate response to the rulings since they were not directly affected.[23] We allow for various post-ruling windows to make sure that our results are not sensitive to the definition of the after-period.

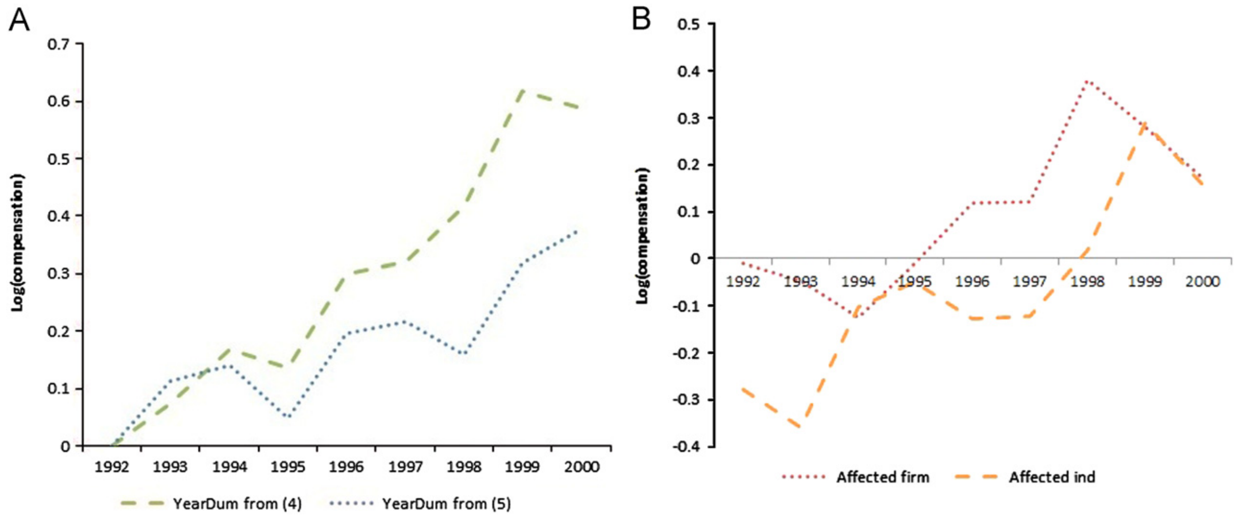

Fig. 2. Compensation-regression year fixed-effects across affected groups, 1992–2000. This figure presents the value of year effects from the regressions from Table 4 for three groups of firms: (a) Delaware-incorporated firms with a staggered board and no blockholder ("Affected firm"); (b) firms that are not incorporated in Delaware, do not have staggered boards, and in which *at least*10% of the firms in the two-digit SIC industry-group are Delaware-incorporated with a staggered board and no blockholder ("Affected ind"); and (c) firms that are not incorporated in Delaware, do not have staggered boards, and in which *less than*10% of the firms in the two-digit SIC industry-group are Delaware-incorporated with staggered boards and no blockholder ("Not affected"). The regressions correspond to Models 4 and 5 in Table 4. Panel A: YearDum from Model 4 and Model 5 in Table 4. Panel B: *YearDum* × *AffectedFirm* and *YearDum* × *AffectedInd* from Model 5 in Table 4.

Table 5
CEO compensation contagion.

This table examines the determinants of CEO compensation prior to and following the legal rulings in 1995 for firms without staggered boards that are not incorporated in Delaware. The dependent variable is the natural log of total compensation. *AffectedInd* is an indicator variable equal to one if at least 10% of firms in the two-digit SIC industry-group have staggered boards, no blockholder, and are incorporated in Delaware. *AffectedIndAfter* is equal to one if *AffectedInd* equals 1 and the year is 1998 or later. *ScaledWPS* is the scaled wealth-performance sensitivity from Edmans, Gabaix, and Landier (2009) divided by 100. PPS_{Grant} is pay-for-performance sensitivity derived from option and stock grants in that year. *LogTotalFirmValue* is the natural log of firm-value as calculated in Gabaix and Landier (2008) (the sum of the value of debt and equity). *InstiOwn%* is the percentage of equity owned by institutional investors. *Leverage* is firm leverage (using the market-value of equity). *Q* is the market value of the firm divided by the replacement value of its assets. *LogTenure* is the natural log of one plus the CEO's tenure. *ROA* is the return on assets. *ΔLogShareholderWealth* is the change in the market value of equity compared to the prior year. *ΔLogShareholderWealthNegative* is a dummy variable equal to one if *ΔLogShareholderWealth* is negative. In Panel A, we provide the results of regressions for various ranges of years. In Panel B, we include year-specific interaction terms to show whether the effect of contagion is concentrated in a particular year. Year fixed-effects are included. Robust firm-clustered *t*-statistics are provided in parentheses below the coefficient value. *, **, and *** denote significant differences from zero at the 10%, 5%, and 1% levels, respectively.

Panel A: Contagion tests

Variable	1992–1995, 1998–2000	1993–1995, 1998–2000	1992–1995, 1999–2000	1993–1995, 1999–2000
AffectedInd	−0.111	−0.101	−0.114	−0.105
	(−1.337)	(−1.269)	(−1.372)	(−1.314)
AffectedIndAfter	0.294***	0.282***	0.337***	0.329***
	(2.811)	(2.788)	(2.837)	(2.802)
ScaledWPS	−0.145***	−0.146***	−0.135***	−0.136***
	(−3.248)	(−3.239)	(−3.650)	(−3.625)
PPS_{Grant}	0.132***	0.128***	0.130***	0.126***
	(7.994)	(8.111)	(6.700)	(6.938)
LogTotalFirmValue	0.446***	0.448***	0.444***	0.446***
	(21.736)	(22.051)	(21.127)	(21.538)
InstiOwn%	0.652***	0.641***	0.638***	0.625***
	(4.968)	(4.835)	(4.685)	(4.565)
Leverage	−0.461**	−0.483**	−0.421**	−0.442**
	(−2.301)	(−2.424)	(−1.974)	(−2.106)
Q	0.059	0.056	0.047	0.042
	(1.471)	(1.399)	(1.136)	(1.035)
LogTenure	0.066*	0.063*	0.072**	0.069*
	(1.973)	(1.883)	(2.027)	(1.926)
ROA	0.370	0.323	0.352	0.295
	(0.748)	(0.678)	(0.670)	(0.587)
ΔLogShareholderWealth	0.030	0.030	0.106**	0.106**
	(1.015)	(1.031)	(2.190)	(2.191)
ΔLogShareholderWealthNegative	−0.214***	−0.215***	−0.195***	−0.198***
	(−4.872)	(−4.542)	(−4.123)	(−3.795)
ΔLogShareholderWealth	−0.041	−0.042	−0.120**	−0.121**
× *ΔLogShareholderWealthNegative*	(−1.358)	(−1.375)	(−2.457)	(−2.460)
Constant	3.887***	3.916***	3.921***	4.287***
	(7.998)	(8.287)	(8.046)	(9.151)
N	800	753	683	636
R^2	71.6%	72.3%	71.7%	72.5%
Year fixed-effects	Yes	Yes	Yes	Yes

Panel B: Contagion effect with separate year-effects

Variable	1992–1995, 1998–2000	1993–1995, 1998–2000	1992–1995, 1999–2000	1993–1995, 1999–2000
AffectedInd	−0.114	−0.105	−0.114	−0.106
	(−1.372)	(−1.305)	(−1.376)	(−1.318)
AffectedInd	0.199*	0.183*		
×*year1998*	(1.721)	(1.665)		
AffectedInd	0.391***	0.379***	0.370***	0.358***
× year1999	(2.888)	(2.825)	(2.741)	(2.681)
AffectedInd	0.286**	0.280*	0.299**	0.295**
× year2000	(1.974)	(1.952)	(2.090)	(2.078)
ScaledWPS	−0.147***	−0.148***	−0.136***	−0.137***
	(−3.258)	(−3.249)	(−3.641)	(−3.614)
PPS_{Grant}	0.131***	0.128***	0.130***	0.126***
	(8.019)	(8.133)	(6.732)	(6.975)
LogTotalFirmValue	0.445***	0.447***	0.444***	0.446***
	(21.507)	(21.762)	(20.981)	(21.334)
InstiOwn%	0.642***	0.630***	0.637***	0.624***
	(4.910)	(4.773)	(4.666)	(4.546)
Leverage	−0.443**	−0.463**	−0.418*	−0.440**

Table 5 (*continued*)

	(−2.224)	(−2.340)	(−1.962)	(−2.095)
Q	0.060	0.056	0.048	0.043
	(1.508)	(1.434)	(1.169)	(1.065)
LogTenure	0.066*	0.063*	0.072**	0.069*
	(1.967)	(1.879)	(2.020)	(1.920)
ROA	0.388	0.342	0.359	0.302
	(0.789)	(0.722)	(0.682)	(0.599)
ΔLogShareholderWealth	0.026	0.027	0.104**	0.105**
	(0.911)	(0.923)	(2.197)	(2.199)
ΔLogShareholderWealthNegative	−0.217***	−0.219***	−0.195***	−0.198***
	(−4.935)	(−4.598)	(−4.114)	(−3.786)
ΔLogShareholderWealth	−0.038	−0.039	−0.119**	−0.120**
× *ΔLogShareholderWealthNegative*	(−1.275)	(−1.290)	(−2.467)	(−2.471)
Constant	3.916***	3.947***	3.929***	4.280***
	(8.003)	(8.277)	(8.013)	(9.190)
N	800	753	683	636
R^2	71.6%	72.3%	71.7%	72.5%
Year fixed-effects	Yes	Yes	Yes	Yes

Additionally, because the tests in this section deal with contemporaneous spillover effects to firms not directly affected by the legal changes, we do not view it as necessary to restrict the sample to those firms that were present in 1995.

We continue to find evidence of compensation contagion in this regression framework. As demonstrated in Panel A of Table 5 by the significant positive coefficient on *AffectedIndAfter*, there are otherwise unexplained increases in compensation at non-Delaware firms in industries with a significant concentration of directly affected firms. Based on the average values of the variables in the first specification, the coefficient implies an approximate $616,000 (34.1%) increase in compensation at Affected industry firms, compared to the firms in the Not affected industry group. Panel B shows that the compensation increases at firms in the Affected industry group are not confined to a particular year. The interaction of *AffectedInd* with dummy variables for 1998, 1999, and 2000 are consistently significant. This suggests that a shock to compensation at a subset of firms can result in a broad and persistent change in compensation levels. The other coefficients and R^2 are comparable to the values in Panel A.

In untabulated robustness tests, we run the regressions in Table 5 with a sample limited to single-segment firms. If focused firms compete more directly for the same CEOs, then the compensation increase would be especially prominent among those firms that operate in only one industry. Suggesting this is the case, we find that the coefficient on *AffectedIndAfter* is, on average, 39% higher across all specifications compared to its value in Table 5. We confirm, however, that our results continue to hold for multi-segment firms, albeit to a lesser extent.

4.3. *The general level of CEO compensation in the late 1990s*

Given the strong evidence of compensation contagion during the 1990s, it is reasonable to examine how much of the compensation run-up during this period can be attributed to this phenomenon. In Table 6, we contrast the coefficients associated with two different regressions explaining the level of compensation, albeit without including variables to account for a Gabaix and Landier (2008) equilibrium. In the first regression, we regress the level of compensation against common control variables only. In the second regression, we also account for the Delaware legal changes by controlling for whether a firm is in either the Affected firm or Affected industry group. We are interested in how the variable *After* changes across these two regressions, since its value could be interpreted as the amount of the unexplained change in compensation during the latter part of the 1990s. We show that this coefficient is approximately 31% higher (0.331 compared to 0.252) when we do not control for the impact of the Delaware legal rulings and its contagion. In terms of the marginal effect on compensation, the coefficient on *After* reflects an average abnormal increase of $741,000 (39.2%) in compensation in the first regression, compared to only $570,000 (28.6%) in the second regression, assuming that the other variables are evaluated at their averages. The coefficient of 0.13 associated with *AffectedAfter* in the second regression indicates a marginal increase in compensation related to the Delaware legal changes of $313,000 (13.9%) when the other regression variables are evaluated at their averages.

Since we are comparing coefficients across regressions with different explanatory variables, we cannot draw specific conclusions about the impact of the Delaware legal changes on the other variables. However, Table 6 presents evidence that the firms directly or indirectly affected by the legal changes raised their compensation significantly more than other firms during this time period (from the second regression). Therefore, a

[23] Our results are robust to various constraints on the period examined, such as including observations from 1997.

Table 6
Compensation increases in the 1990s, with an exogenous shock and its contagion.

This table examines the determinants of CEO compensation, both in general and when controlling for firms/industries that are directly affected by the anti-takeover court rulings. The dependent variable is the natural log of total compensation. *ScaledWPS* is the scaled wealth-performance sensitivity from Edmans, Gabaix, and Landier (2009) divided by 100. PPS_{Grant} is pay-for-performance sensitivity derived from option and stock grants in that year. *LogTotalFirmValue* is the natural log of firm-value as calculated in Gabaix and Landier (2008) (the sum of the value of debt and equity). *InstiOwn%* is the percentage of equity owned by institutional investors. *Leverage* is firm leverage (using the market-value of equity). *Q* is the market value of the firm divided by the replacement value of its assets. *LogTenure* is the natural log of one plus the CEO's tenure. *ROA* is the return on assets. *ΔLogShareholderWealth* is the change in the market value of equity compared to the prior year. *ΔLogShareholderWealthNegative* is a dummy variable equal to one if *ΔLogShareholderWealth* is negative. *After* is an indicator variable equal to one if the year is 1996 or later. *Affected* is an indicator variable equal to one if either (1) the firm is Delaware-incorporated with a classified board and no blockholder, or (2) at least 10% of firms in the industry are Delaware-incorporated with a classified board and no blockholder. Robust firm-clustered *t*-statistics are provided in parentheses below the coefficient value. *, **, and *** denote significant differences from zero at the 10%, 5%, and 1% levels, respectively.

Variable	(1)	(2)
ScaledWPS	−0.171***	−0.171***
	(−9.664)	(−9.760)
PPS_{Grant}	0.138***	0.137***
	(24.439)	(24.516)
LogTotalFirmValue	0.449***	0.448***
	(48.744)	(48.822)
InstiOwn%	0.404***	0.399***
	(7.323)	(7.253)
Leverage	−0.080	−0.062
	(−0.916)	(−0.724)
Q	0.124***	0.124***
	(10.093)	(9.925)
LogTenure	0.064***	0.063***
	(4.240)	(4.203)
ROA	0.332***	0.345***
	(2.599)	(2.701)
ΔLogShareholderWealth	0.003	0.002
	(0.290)	(0.218)
ΔLogShareholderWealthNegative	−0.186***	−0.185***
	(−11.532)	(−11.478)
ΔLogShareholderWealth × *ΔLogShareholderWealthNegative*	−0.005	−0.004
	(−0.539)	(−0.457)
After	0.331***	0.252***
	(17.732)	(8.612)
Affected		−0.098***
		(−2.758)
AffectedAfter		0.130***
		(3.602)
Constant	3.838***	3.922***
	(18.510)	(18.864)
N	6,167	6,167
R^2	66.1%	66.1%

researcher who controls for this effect will observe a sizeable reduction in the level of otherwise unexplained compensation during this period.

Next, we incorporate the contagion of compensation increases associated with the Delaware legal changes into the equilibrium model of Gabaix and Landier (2008). Our results are useful for understanding increases in CEO compensation during this time period that their model does not explain fully. CEO compensation in the S&P 500 in the late 1990s actually outpaced the growth of firm size, which is inconsistent with their baseline equilibrium argument.[24] Our analysis provides an explanation for the remaining increase in CEO compensation during this period.

To conduct this analysis, we modify Gabaix and Landier's (2008) key cross-sectional test of their "dual scaling equation" (they present these results in their Table 2, p. 68). Their main cross-sectional prediction is that, under an assumption of constant returns to scale, when the log of CEO compensation is regressed onto the logs of firm size and typical firm size in the economy (they use the 250th largest firm in each year), the coefficients on these two variables sum to one. In their Table 2, they show that when using data from 1992–2004, they are unable to reject this hypothesis across multiple specifications controlling for various fixed-effects and/or governance quality. However, although Gabaix and Landier have modeled a contagion effect formally, they do not test for its existence in the data.

In Table 7, we implement similar tests to Gabaix and Landier (2008) over the same time period they study, and contrast the results of regressions with and without the addition of the *Affected* and *AffectedAfter* variables. To be approximately comparable with the Gabaix and Landier regressions that include only the top 1,000 or 500 largest firms in each year, we conduct separate regressions for three different data sets. These include a full sample (Panel A), and two smaller samples that remove firms below $1 billion (Panel B) and below $3 billion (Panel C) market value, respectively. Similar to Gabaix and Landier, we also report results with specifications that include no fixed-effects, industry fixed-effects, and firm fixed-effects.

Our results strongly support the model of Gabaix and Landier (2008), modified to include the impact of the Delaware legal changes. Interestingly, and in contrast to Gabaix and Landier, we are unable to support the hypothesis that the sum of the coefficients on *TotalFirmValue* and *TotalFirmValue*250 equal one in regressions that do not control for the impact of the Delaware legal changes (regressions 1, 3, and 5 of each panel). However, this hypothesis is supported as driving baseline increases in compensation when we also control for the impact of the legal changes. In each panel, the coefficient on *TotalFirmValue* is stable across specifications regardless of the inclusion of the additional controls. However, the coefficient on *TotalFirmValue*250 tends to be higher than

[24] This can be seen in Gabaix and Landier's (2008) Fig. 1, where the index of Jensen, Murphy, and Wruck (2004) plots CEO compensation at the S&P 500. Their figure also plots the Frydman and Saks (2010) index of the average compensation levels for the top three executives at 50 of the very largest firms over this same time period, and this index tracks more closely with average market value. It is possible that these largest firms are not subject to the same entrenchment shocks associated with increased insulation from the corporate control market. Alternatively, their managers could also have been bound by a public outrage constraint on additional compensation since they likely already defined the very top end of the distribution; according to Execucomp, in 2000, CEOs of the 50 largest S&P 500 firms received average compensation of $37 million compared with an average of $13 million at the next 450 largest firms.

Table 7
Test of Gabaix and Landier (2008) dual-scaling equation, with an exogenous shock and its contagion.

This table models compensation as in Table 2 from Gabaix and Landier (2008), from 1992–2004 (as in their paper). The dependent variable is the natural log of total compensation. *LogTotalFirmValue* is the natural log of firm-value as calculated in Gabaix and Landier (2008) (the sum of the value of debt and equity). *LogTotalFirmValue250* is the natural log of firm-value as calculated in Gabaix and Landier (2008) of the 250th largest firm. *Affected* is an indicator variable equal to one if either (1) the firm is Delaware-incorporated with a classified board and no blockholder, or (2) at least 10% of firms in the industry are Delaware-incorporated with a classified board and no blockholder. *AffectedAfter* is an indicator variable equal to one if the year is 1996 or later and *Affected* equals 1, and zero otherwise. The second-last and last lines in each panel are the test of *LogTotalFirmValue*+*LogTotalFirmValue250*=1 and the associated p-value, respectively. Panel A includes all firms, Panel B limits the sample to firms with firm-value greater than or equal to \$1 billion, and Panel C limits the sample to firms with firm-value greater than or equal to \$3 billion. Robust firm-clustered t-statistics are provided in parentheses below the coefficient value. *, **, and *** denote significant differences from zero at the 10%, 5%, and 1% levels, respectively.

Panel A: All firms	(1)	(2)	(3)	(4)	(5)	(6)
LogTotalFirmValue	0.384***	0.383***	0.453***	0.453***	0.309***	0.316***
	(33.559)	(33.519)	(40.544)	(40.563)	(11.029)	(11.329)
LogTotalFirmValue250	0.717***	0.617***	0.629***	0.541***	0.786***	0.716***
	(22.184)	(15.805)	(21.116)	(14.684)	(21.260)	(16.739)
Affected		−0.150***		−0.152***		−0.130***
		(−3.754)		(−4.408)		(−4.041)
AffectedAfter		0.169***		0.143***		0.084***
		(4.634)		(4.375)		(2.826)
N	9,033	9,033	9,033	9,033	9,033	9,033
N(firms)	1,339	1,339	1,339	1,339	1,339	1,339
R^2	40.3%	40.4%	50.1%	50.2%	76.0%	76.0%
Fixed-effects	None	None	Industry	Industry	Firm	Firm
Test of GL (2008) hypothesis: *LogTotalFirmValue*+ *LogTotalFirmValue*250−1	0.101***	0.000	0.082***	−0.006	0.095***	0.032
(p-Value)	(0.003)	(0.988)	(0.009)	(0.854)	(0.003)	(0.423)

Panel B: Total firm value ≥\$1 billion	(1)	(2)	(3)	(4)	(5)	(6)
LogTotalFirmValue	0.362***	0.360***	0.434***	0.434***	0.311***	0.317***
	(22.840)	(22.831)	(29.290)	(29.331)	(8.877)	(9.095)
LogTotalFirmValue250	0.765***	0.673***	0.669***	0.589***	0.814***	0.755***
	(21.223)	(15.350)	(20.024)	(14.140)	(18.940)	(15.220)
Affected		−0.121***		−0.130***		−0.106***
		(−2.741)		(−3.448)		(−2.983)
AffectedAfter		0.162***		0.132***		0.070**
		(4.018)		(3.652)		(2.126)
N	7,352	7,352	7,352	7,352	7,352	7,352
N(firms)	1,100	1,100	1,100	1,100	1,100	1,100
R^2	31.9%	32.1%	44.6%	44.7%	73.1%	73.1%
Fixed-effects	None	None	Industry	Industry	Firm	Firm
Test of GL (2008) hypothesis: *LogTotalFirmValue*+ *LogTotalFirmValue*250−1	0.127***	0.033	0.103***	0.023	0.125***	0.072*
(p-Value)	(0.001)	(0.449)	(0.002)	(0.584)	(0.000)	(0.094)

Panel C: Total firm value ≥\$3 billion	(1)	(2)	(3)	(4)	(5)	(6)
LogTotalFirmValue	0.356***	0.353***	0.419***	0.420***	0.288***	0.299***
	(15.838)	(15.743)	(19.666)	(19.696)	(6.996)	(7.266)
LogTotalFirmValue250	0.835***	0.737***	0.736***	0.642***	0.852***	0.774***
	(19.358)	(14.147)	(18.464)	(12.939)	(16.192)	(12.438)
Affected		−0.121**		−0.153***		−0.121***
		(−2.309)		(−3.424)		(−2.850)
AffectedAfter		0.174***		0.154***		0.098**
		(3.584)		(3.453)		(2.447)
N	5,265	5,265	5,265	5,265	5,265	5,265
N(firms)	754	754	754	754	754	754
R^2	28.0%	28.2%	43.2%	43.3%	71.4%	71.5%
Fixed-effects	None	None	Industry	Industry	Firm	Firm
Test of GL (2008) hypothesis: *LogTotalFirmValue*+ *LogTotalFirmValue*250−1	0.191***	0.09*	0.155***	0.062	0.14***	0.073
(p-Value)	(0.000)	(0.081)	(0.000)	(0.196)	(0.001)	(0.152)

estimated by Gabaix and Landier when we do not control for the legal changes, but it drops to comparable values in regressions that do include these controls. Without the additional controls, we find that the two firm-size coefficients sum to between a low of 1.082 and a high of 1.191, and each sum is significantly different from 1.0 at the 1% level. Moreover, the coefficients associated with *Affected* and *AffectedAfter* demonstrate an increase in compensation at these firms following the legal rulings. These results are consistent with the average level of compensation and average firm size being at least partially spuriously correlated during this period, if one does not account for a contagion effect. However, when incorporating a contagion effect driven by the changes in Delaware law, the unified Gabaix and Landier (2008) model fits the data nicely.

We have examined why we are unable to replicate the baseline Gabaix and Landier (2008) cross-sectional results without accounting for the Delaware legal changes. It appears to result from differences in the selection criteria for sample firms. We exclude firms that did not report a December fiscal year-end, to ensure that we evaluate compensation paid over the same time periods and control for firm characteristics measured at the same point in time. From our reading of Gabaix and Landier, it does not appear that they implemented this restriction. We speculate that by relaxing this requirement, one could fail to reject the hypothesis that the coefficients on *TotalFirmValue* and *TotalFirmValue*250 sum to one due to excess noise in the data, leading to larger standard errors. Indeed, in unreported results, we run all of the regressions in this section on a sample not restricted based on fiscal year-end month, and are similarly unable to reject their dual scaling equation in any of those regressions.

5. Conclusion

We examine the effect on CEO compensation of changes in Delaware case law in 1995 that gave executives greater ability to defend against hostile takeover bids. We begin by considering the responses of firms directly affected by the new law, and find that firms with staggered boards and without large shareholders increase CEO compensation, likely driven by increased managerial entrenchment. These results are consistent with those found by researchers examining the impact of the second-generation anti-takeover laws of the 1980s, and provide evidence that the 1995 Delaware legal changes represent an important development in the takeover market.

We then demonstrate that firms that compete in the same industries with entrenched Delaware-incorporated firms also significantly increase the level of their CEOs' compensation after the legal changes, consistent with the "contagion effect" modeled by Gabaix and Landier (2008). We show further that compensation contagion could be responsible for a substantial portion of the general run-up in compensation during this period. Finally, we demonstrate that when one accounts for compensation contagion, the model of Gabaix and Landier can explain the remainder of the escalation in compensation levels.

While we have examined only one shock to CEO compensation, there have been many other changes to the legal environment that U.S. firms operate in that could have produced similar outcomes. Examples include the first- and second-generation state anti-takeover laws of the 1980s. To the extent that our results can be generalized to these other instances is an open empirical question, the answer to which could contribute significantly to our understanding of increases in CEO compensation over a longer time period.

Appendix A. Relevant anti-takeover court rulings

In this Appendix we provide a brief overview of the important legal cases related to the use of poison pills in conjunction with a staggered board.

The original case considering the poison pill defense was *Moran v. Household International* (1985), in which the Delaware Supreme Court upheld the target's ability to use a pill to avoid an unwanted takeover, and indicated that the court would apply the "business judgement rule" when reviewing the legality of the board's actions in this context. The court in *Moran* confirmed that the enhanced scrutiny standard of *Unocal Corp. v. Mesa Petroleum* (1985) would apply to these situations, under which Delaware corporations could only rebuff a takeover overture if it was determined to present a threat to the company, and they could only defend the company with a "proportional" response. The extent of a target board's discretion was brought into question with the *City Capital Associates v. Interco Inc.* (1988) case, in which the Delaware Chancery Court forced a target of a hostile takeover to remove a poison pill because it was not a response proportionate to the threat levied by the bid. The *Interco* case brought uncertainty to potential acquisition targets, as highlighted by the fact that following the ruling, Martin Lipton (the Wachtell, Lipton, Rosen & Katz lawyer credited with having invented the poison pill) sent a memorandum to his Delaware-incorporated clients suggesting they may need to incorporate elsewhere to avoid takeovers (Gordon, 1991).

The next important installment in this line of cases was *Paramount v. Time* (1989). Here, the Delaware Supreme Court indicated that a target board's use of takeover defenses must be in the range of reasonableness, and that a reasonable justification could be to protect corporate policies or plans even though it meant forgoing a significant monetary gain for shareholders if a deal was completed. In addition, the Paramount court criticized the *Interco* opinion as not having correctly applied the *Unocal* standard.

The most important cases for our analysis are the Delaware Supreme Court's decision in *Unitrin v. American General Corp.* (1995), and the following federal case of *Moore Corp. Ltd. v. Wallace Computer Services* (1995). In *Unitrin* (1995), the Delaware Supreme Court bolstered the strength of the "just say no" defense when they took a management-friendly stance that expanded the definition of a "threat" to the company, and made it clear that establishing that a response was "proportional" would not be difficult. The full extent of a target boards' discretion was made clear by a federal district court in *Wallace Computer Services* (1995). In that case, Wallace Computer successfully used a poison pill to defend itself against a tender offer by Moore even though it was supported by 75% of Wallace Computer shareholders. This case was widely viewed as establishing that incumbent management has extremely wide discretion to "just say no" to a takeover under Delaware law. Beyond this time, a hostile bidder would have to replace a majority of the target's board of directors through proxy contests to repeal a poison pill and complete an acquisition. If a target firm has a staggered board, that process takes a minimum of two election cycles, giving incumbent management an extremely strong deterrent to a corporate control threat. The potency of this defense was further illustrated in the late 1990s by the boards of Pennzoil and Circon. Each of these companies used poison pills in conjunction with classified boards to successfully defend against hostile takeovers even though the potential acquirors offered sizeable premiums and the combinations were supported by large majorities of shareholders.

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