Journal of Financial Economics 126 (2017) 422–444

Contents lists available at ScienceDirect

Journal of Financial Economics

journal homepage: www.elsevier.com/locate/jfec

Staggered boards and long-term firm value, revisited☆,☆☆

K.J. Martijn Cremers[a,*], Lubomir P. Litov[b], Simone M. Sepe[c,d]

[a] University of Notre Dame, 264 Mendoza College of Business, Notre Dame, IN 46556, United States
[b] Price College of Business, University of Oklahoma, Norman, OK 73069, United States
[c] University of Arizona James E. Rogers College of Law 1201 E. Speedway Tucson, Arizona 85721, United States
[d] Institute for Advanced Study in Toulouse, Fondation Jean-Jacques Laffont, Toulouse School of Economics, 21 Allée de Brienne, 31015 Toulouse Cedex 6, France

ARTICLE INFO

Article history:
Received 9 March 2016
Revised 13 October 2016
Accepted 24 October 2016
Available online 18 August 2017

JEL classification:
G32
G34
K22

Keywords:
Staggered board
Firm value
Stakeholder relationships
Innovation

ABSTRACT

This paper revisits the staggered board debate focusing on the long-term association of firm value with changes in board structure. We find no evidence that staggered board changes are negatively related to firm value. However, we find a positive relation for firms engaged in innovation and where stakeholder relationships matter more. This suggests that staggered boards promote value creation for some firms by committing the firm to undertaking long-term projects and bonding it to the relationship-specific investments of its stakeholders. Our results are robust to matching procedures and an exogenous change in Massachusetts corporate law that mandated staggered boards.

1. Introduction

Whether staggered (or classified) boards are a desirable governance arrangement for publicly traded firms is the subject of a long-standing debate. Unlike a unitary board, where all directors stand for reelection each year, in a

☆ The results in the Online Appendix are available at http://ssrn.com/abstract=2866677.

☆☆ We thank seminar participants at Columbia University, Arizona State University, Emory University, Economic University of Vienna, Harvard University, Northwestern University, Toulouse School of Economics, University of Arizona, Brigham Young University, University of Southern California, University of Iowa, University of Nebraska, University of Kansas, University of Oklahoma, Texas Tech University, University of Virginia, University of TexasSan Antonio, University of Illinois at Urbana-Champaign, as well as participants at the 2014 American Law and Economics Association Meeting, the 2014 IDC Herzlyia Conference at Tel Aviv University, the 2014 Ackerman Conference on Corporate Governance at Bar-Ilan University, the 2014 University of Delaware Corporate Governance Symposium, the 2014 European Summer Symposium in Economic Theory, the 2014 Financial Management Association meeting, the 2014 Society for Empirical Legal Studies meeting at the University of California in Berkeley, the 2015 Midwestern Finance Association meeting, the 2015 Finance Cavalcade, the 2015 Western Finance Association meeting, and the 2016 American Finance Association meeting. For useful comments and discussions, we thank Yakov Amihud, Thomas Bates, Stefan Bechtold, Bernard Black, Patrick Bolton, Jeff Coles, Jacque Cremer, Vicente Cunat, Harry DeAngelo, Isil Erel, Dirk Jenter, Olubumni Faleye, Chitru Fernando, Allen Ferrell, Jesse Fried, Oliver Hart, Gerard Hertig, Gregg Jarrell, Kose John, Dalida Kadyrzhanova, Louis Kaplow, Jonathan Karpoff, Reinier Kraakmann, Chris Lamoureaux, Saura Masconale, William Megginson, Alan Schwartz, Laura Starks, Neal Stoughton, Oren Sussman, Michael Roberts, Jean Tirole, Lucy White, and Joseph Zechner. We also thank Lucian Bebchuk for providing us with access to his data on charter-based and bylaws-based staggered board classifications, Dirk Jenter for providing access to his CEO turnover data, and David Larcker for providing access to the data errors for staggered boards in the IRRC volumes documented in Larcker, Reiss, and Xiao (2015).

* Corresponding author.
E-mail addresses: mcremers@nd.edu (K.J.M. Cremers), litov@ou.edu (L.P. Litov), sms234@email.arizona.edu (S.M. Sepe).

http://dx.doi.org/10.1016/j.jfineco.2017.08.003

staggered board directors are typically grouped into three different classes serving staggered three-year terms, with only one class of directors standing for reelection each year. As this requires challengers to win at least two election cycles to replace a majority of the board and, hence, to endure a costly delay before gaining voting control, a staggered board protects directors from market discipline.

Conventional wisdom holds that such protection offered by a staggered board may operate to entrench directors and managers, thereby encouraging shirking, empire building, and/or private benefits extraction (Manne, 1965; Jensen, 1988, 1993). Consistent with this view, several empirical studies show that in the cross-section, firms with a staggered board tend to have lower firm value as measured by Tobin's *Q* (Bebchuk and Cohen, 2005; Faleye, 2007; Bebchuk, Cohen and Ferrell, 2009).

A contrasting view is that the adoption of staggered boards could potentially contribute to firm value, in two ways. First, a staggered board could help mitigate managers' incentives to overinvest in short-term projects if investors are uninformed or myopic (Stein, 1988, 1989). Second, a staggered board could serve to bond a manager's commitment to the relationship-specific investments made by the firm's stakeholders, reducing the likelihood that the firm's business strategy is changed via a takeover and thus lowering the risk that takeovers will impose costs on stakeholders (Knoeber, 1986; Laffont and Tirole, 1988; Shleifer and Summers, 1988). Consistent with this "bonding hypothesis," more recent studies report evidence that firms may benefit from a staggered board if they have strong stakeholder relationships (Cen, Dasgupta and Sen, 2016; Johnson, Karpoff and Yi, 2015, 2016).

Motivated in part by this more recent evidence, we revisit the staggered board debate using a longer sample period (1978–2015) than the existing empirical literature and with a new focus on the long-term association of firm value with changes in board structure (i.e., decisions to adopt or remove a staggered board). Our main findings are as follows. We find no evidence that staggered boards have a strong or persistently negative association with firm value. Rather, in more innovative firms, or where stakeholder investments are more relevant (e.g., with a large customer or in a strategic alliance), adopting (removing) a staggered board is associated with an increase (decrease) in long-term firm value. For example, the adoption (removal) of a staggered board is associated with an increase (decrease) in firm value, as proxied by Tobin's *Q*, of 5.3% for firms with a large customer, and has an insignificant association for firms without a large customer. Further, our results are driven by the second half of our time period (i.e., the 1996–2015 sub-period) and generally insignificant in the first half (i.e., the 1978–1995 sub-period). Overall, these results suggest that staggered boards have heterogeneous effects across firms and time, providing no support for the entrenchment view but also making it difficult to draw any one-size-fits-all inference about the relation between staggered boards and firm value.

Our results are robust to various methodologies, including regressions of changes in Tobin's Q on changes in board structure and a stock portfolio return approach. However, a primary challenge in interpreting the empirical relation between changes in board structure and long-term firm value is that staggered boards, like other corporate arrangements, are not randomly assigned (Adams, Hermalin and Weisbach, 2010). We try to mitigate these endogeneity concerns in the following four ways.

First, we consider three selection mechanisms related to: (1) the market for corporate control, where anticipated future takeovers affect valuations; (2) entrenchment, where changes in manager-board relationships provide explanatory power for changes in board structure: and (3) reverse causality, where ex ante low (high) firm valuations provide explanatory power for the adoption (removal) of a staggered board. We find no evidence for a selection bias related to merger and acquisition (M&A) activity or entrenchment, but show that firms with lower value are substantially more likely to adopt a staggered board. Reverse causality thus helps provide explanatory power for the sign reversal between our time series results and the cross-sectional results in most of the literature. While the value of firms staggering up tends to increase afterwards, this increase is insufficient to fully erode the value difference with other firms in the same industry, generating the negative cross-sectional association between firm value and staggered boards. Conversely, we find no significant association between lagged firm value and the decision to de-stagger.

Second, we incorporate possible selection effects through the creation of multiple matched samples based on different matching procedures. In each matched sample, each firm with a changing board structure in a given year is matched to a firm with the same ex ante board structure and similar observable characteristics that relate to board structure, but which did not change its board structure in that year. The matched samples confirm the positive (negative) relation between the adoption (removal) of a staggered board and firm value.

Third, we employ the dynamic generalized method of moments (GMM) estimator proposed by Arellano and Bover (1995) and Blundell and Bond (1998). As explained by Wintoki, Linck and Netter, (2012), this methodology estimates a simultaneous system in which firm value, board structure, and other key corporate characteristics are all endogenous and dynamically interrelated. As this method relies on strong assumptions, these results are presented as a robustness check rather than as showing causality. Using a system where we can reject that the instruments are weak and that accounts for unobservable heterogeneity using firm fixed effects, the dynamic GMM results also show a positive (negative) relation between adopting (removing) a staggered board and firm value.

Fourth, we conduct a long-term event study exploiting plausibly exogenous variation in board structure due to changes in Massachusetts corporate law. In 1990, Massachusetts made staggered boards "quasi-mandatory" by requiring firms incorporated in the state to adopt a staggered board by default and making it difficult to opt out of this requirement. We compare the value of Massachusetts firms in the few years before and after this legal change in a matched sample of firms, where the control firms are incorporated outside of Massachusetts but have a similar size, are in the same industry, and have the same board structure as the Massachusetts firms. After the legal change, the value of the Massachusetts firms increased more than the value of their control firms, but with no difference between Massachusetts firms with and without a staggered board prior to the law change. Therefore, while these results do not provide direct evidence that mandating the adoption of a staggered board increases firm value, they are also clearly inconsistent with the hypothesis that having a staggered board in place lowers firm value.

These results notwithstanding, we acknowledge that our attempts to mitigate the endogenous choice of a staggered board have significant limitations. As an alternative to resolving endogeneity, we examine two economic channels through which a staggered board could be associated with an increase in long-term firm value, i.e., the myopic market hypothesis and the bonding hypothesis. As outlined above, under the myopic markets hypothesis, a staggered board helps shield management from the pressure of myopic investors. According to the bonding hypothesis, a staggered board provides an efficient commitment device towards the firm-specific investments of a firm's stakeholders, such as top employees, large customers, suppliers, and strategic alliance partners.

Our results suggest that the role of staggered boards differs across firms in a way that both economic channels could play a role, although overall the results seem more consistent with the bonding hypothesis. We find that the adoption (removal) of a staggered board has a positive (negative) association with firm value among firms with stronger stakeholder relationships, such as firms with large customers, productive employees, and in strategic alliances. We similarly find that the adoption (removal) of staggered boards has a more positive (negative) association with firm value among firms whose projects require longer-term investments and are likely harder to value by outside investors, such as firms with more investments in innovation and intangibles. Conversely, using proxies that other research has linked to myopic investment behavior (Bushee, 1998; Cremers, Pareek and Sautner, 2016), we find no evidence that the association between staggered boards and firm value is stronger for firms with more short-term institutional investors.

Nonetheless, the decline in firm value after de-staggering remains challenging to interpret, under either channel. De-staggering may be due to outside shareholder pressure, to which firms with worse prospects could be more likely to give in. As a result, the subsequent decline in value could reflect selection rather than causality. Alternatively, the bonding channel suggests that firms that agree to de-stagger their board signal that the firm expects fewer rents (i.e., lower future surpluses) from its long-term stakeholder relationships, which would explain the subsequent decline in firm value.

Further, while our paper focuses on examining the long-term effects of staggered boards on firm value rather than the question of *why* firms adopt or remove a staggered board, the above results suggest at least a partial answer to that question. On the one hand, some firms appear to stagger up to secure the benefits arising from long-term investments and stronger shareholder bonding (Johnson et al., 2015, 2016). On the other hand, shareholder pressure may have been the cause of increased de-staggering during the 2000s, with this pressure being at least in part motivated by the belief that staggered boards lower firm value (Subramanian, 2014).

In conclusion, our paper makes two main contributions to the literature. First, it provides an improvement over the identification strategies used in prior studies, as most of these studies examined the relation between staggered boards and long-term firm value by relying exclusively on cross-sectional results (Bebchuk and Cohen, 2005; Faleye, 2007). Compared to these studies, we employ a more comprehensive sample, firm fixed effects in pooled panels, regressions of changes in value on changes in board structure, several different matching procedures, dynamic GMM estimation, and exogenous variation in Massachusetts corporate law.

Second, we show that changes in firm value around changes in board structure are inconsistent with the entrenchment view of staggered boards. Rather, our results point to a heterogeneous relationship between board structure and performance, suggesting that for different subsets of firms, staggered boards could contribute to firm value by preventing inefficient takeovers and/or serving to bond a firm's commitment to the firm's long-term stakeholders. Consistent with a large body of literature (see Adams et al., 2010, for a review; as well as Barry and Hatfield, 2012; Ahn and Shrestha, 2013; and Ge, Tanlu and Zhang, 2016), the heterogeneous relation of board structure with performance thus indicates that a one-size-fits-all view of board structure is not supported by the data.

2. Data and descriptive statistics

2.1. Data

Our data come from several sources, with the main data sample covering the time period 1978–2015. Data availability varies with the different sources. We obtain data for the key independent variable of our study, *Staggered board*, from two main sources, covering a total number of 3,076 firms. For the time period 1990–2015, as in prior studies on the value impact of staggered boards (Bebchuk and Cohen, 2005; Faleye, 2007; Masulis, Wang and Xie, 2007), we use the corporate governance data set maintained by RiskMetrics, which acquired the Investor Responsibility Research Center (IRRC).[1] For the time period 1978–1989, we use data from Cremers and Ferrell (2014), who hand-collected information on firm-level corporate governance provisions for these years, including information on

[1] During the period 1990–2006, IRRC published volumes in the following years: 1990, 1993, 1995, 1998, 2000, 2002, 2004, and 2006. We hand-checked the data on staggered boards in all missing years in the 1994–2006 time period using proxy statements from the Security and Exchange Committee's 'EDGAR' website, in order to capture the precise timing of changes in board structure. We start our hand checks in 1994 because electronic records on the SEC's website are only available since that year. Furthermore, Larcker et al. generously shared the data errors for staggered boards in the IRRC volumes documented in Larcker, Reiss, and Xiao (2015). Incorporating these errors results in changing 115 observations, and eliminating 11 removals and seven adoptions of staggered boards, while introducing eight other removals and 14 other adoptions of staggered boards. These changes do not significantly affect our results.

the same provisions tracked by the IRRC for the period 1990–2015 and, in particular, staggered boards.

Since our main focus is on the value relevance of staggered boards, the main dependent variable in our analysis is firm value. Consistent with many prior studies investigating the relation between governance arrangements and firm value (Demsetz and Lehn, 1985; Morck, Shleifer and Vishny, 1988; Lang and Stultz, 1994; Yermack, 1996; Daines, 2001; Gompers, Ishii and Metrick, 2003), we measure firm value using Tobin's Q (Q), defined as the ratio of market-to-book value of assets (Fama and French, 1992) from Compustat data. As an additional measure of changes to firm value, we use the stock returns surrounding changes in board structure, obtaining stock return data from the Center for Research in Security Prices (CRSP) database (see Section 3.2). In our analysis of (in) voluntary Chief Executive Officer (CEO) turnover (see Section 3.3.2), we employ CEO turnover data from Jenter and Kanaan (2015) over the time period 1993–2001 for all ExecuComp firms.

We provide brief definitions of all the controls and the interaction variables in Table 1. Our default controls are Ln*(Assets)*, *Delaware incorporation*, return on assets (*ROA*), *CAPX/Assets*, research and development expenditures over total sales (*R&D/ Sales*), and *Industry M&A volume*. The last control is used to verify the existence of an anticipation effect of future takeover activity (Edmans, Goldstein and Jiang, 2012). Our extended set of controls also includes *G-Index* and *Insider ownership*, to replicate more closely Bebchuk and Cohen (2005). *G-Index* is a composite of 24 provisions that measures the strength of shareholders rights, with a higher score indicating weaker shareholder rights. In computing *G-Index*, we remove *Staggered board* (as in Bebchuk and Cohen, 2005) and *Poison pill*, as we separately include these two provisions. We obtain *G-Index* data from Cremers and Ferrell (2014) for 1978–1989 and the RiskMetrics data set (formerly IRRC) for 1990–2006.[2] *Insider ownership* data reduce our sample size, as Compact Disclosure (data source until 2006) primarily covers NYSE and Amex firms before 1995.

Table 2 presents the descriptive statistics of all the variables.[3] Averaged across all firm-year observations, about 52% of firms have a staggered board. The average Q in our sample is 1.61 with a standard deviation of 0.89.[4]

2.2. Staggering and de-staggering

Fig. 1 presents the annual percentage of firms with a staggered board in our sample from 1978 to 2015. We observe substantial time variation. This period is characterized by a slow trend of staggering up, which rapidly accelerates in 1984 to 1992. In the 1992–2006 period, there is a fairly stable ratio of firms with a staggered board, hovering at around 60%. After 2006, the ratio of firms with a staggered board steadily declines, until reaching about 35% in 2015.

Fig. 2 shows the percentage of firms with a staggered board over time. We demonstrate the dynamics of staggering up and staggering down within six different cohorts of firms through time, where no new firms enter each cohort subsequently, while firms drop out of the cohorts due to M&A, privatizations, bankruptcies, and other de-listing events. The six cohorts of firms are as follows: (1) firms with a staggered board in 1978, (2) firms without a staggered board in 1978, (3) firms with a staggered board in 1990, (4) firms without a staggered board in 1990, (5) firms with a staggered board in 2000, and (6) firms without a staggered board in 2000.

Among the 195 firms in our sample with a staggered board in 1978, only a few de-staggered before 2005, as nearly 93% remain staggered in 2004 (out of the firms still in the sample). Starting in 2005, a large number of firms in this cohort de-staggered, with only about 36% of the surviving 42 firms remaining staggered in 2015. Conversely, among the 684 firms without a staggered board in 1978, almost half staggered-up by 1989 and 55% had a staggered board in 2004, after which many firms in this cohort de-staggered, leaving only 24% of the cohort sample (146 firms) that survives with a staggered board in 2015. Comparing the 1990 and 2000 cohorts to the 1978 cohort, we observe analogous trends. In particular, among the firms with a staggered board in 1990 and 2000, almost all remained staggered until 2005 and began to increasingly de-stagger afterward. We note that when a firm staggers up, it typically takes a while before it decides to de-stagger. Lastly, in the 1995–2002 time period that has been the focus of most prior studies on staggered boards (Bebchuk and Cohen, 2005; Faleye, 2007; Bebchuk et al., 2009), there is little time variation in board structure.

3. Results

3.1. Staggered boards and firm value: cross-sectional and time series results

In this section, we consider both the cross-sectional and time series association between staggered boards and long-term firm value. In particular, for the time series association, we show results for both pooled panel Q regressions with firm fixed effects and changes-in-Q regressions on changes in board structure. For all tables, we show *t*-statistics based on robust standard errors clustered by firm. The motivation for employing standard errors clustered by firm is to incorporate the correlation of regression residuals across time for a given firm, which is particularly important for variables with little time variation (Petersen, 2009). In Table 3 only, we also provide the *t*-statistic based on robust standard errors that are not clustered, where *t*-statistics are considerably smaller when we cluster by firm.

Table 3 presents results for the pooled panel Q regression with year and four-digit Standard Industrial Classification (SIC) industry fixed effects in the regression for columns 1–3 and year and firm fixed effects in the regres-

[2] As a caveat, Larcker, Reiss, and Xiao (2015) document some coding errors in the G-Index provisions. We use the G-Index primarily to more closely replicate past literature using the uncorrected G-Index data, and only incorporate coding corrections from Larcker, Reiss, and Xiao (2015) pertaining to the staggered board.

[3] Pairwise correlations are provided in Online Appendix Table A.1.

[4] The averages of the control variables are similar across the sample of firms with and without staggered boards (see Online Appendix Table A.2).

Table 1
Definitions.

Dependent variables	Definition
Tobin's $Q_{[t]}$	Market value of assets (total assets – book equity + market equity) divided by book value of assets. Calculation follows Fama and French (1992). Source of data is Compustat annual data file.
Monthly returns on long (short) portfolio "6m12"	Monthly return of a portfolio created by stocks that stagger up (down) their boards. Portfolio is created by including all stocks of firms that have (de)staggered their board for 12 months, starting six months before the fiscal year-end of the year in which the company has reported its board being (de-)staggered for the first time. Returns are either equally or value weighted.
Monthly returns on long (short) portfolio "12m12"	Analogous to "6m12," but now including all stocks of firms that have (de)staggered their board for 12 months, starting 12 months before the fiscal year-end of the year in which the company has reported its board being (de)staggered.
Monthly returns on long (short) portfolio "12m24"	Analogous to "12m12," but now including all stocks of firms that have (de)staggered their board for 24 months.
(Forced) CEO turnover$_{[t]}$	Defined as one if there is an (in) voluntary CEO departure in the Jenter and Kanaan (2015) data file; zero otherwise. Data are available for 1993–2001.
Independent variables	
CAPX/Assets$_{[t]}$	Capital Expenditure$_{[t]}$/Total Assets$_{[t]}$.
Delaware incorporation$_{[t]}$	Indicator variable if the company is incorporated in Delaware.
Excess returns$_{[t]}$	Annual returns for each firm at the fiscal year end date net of market return for the same period. Data for stock returns are from CRSP. Data for market returns are from Ken French's data library. This variable is then winsorized at 2.5% in each tail of its distribution.
G-Index (minus staggered board)$_{[t]}$	Sum of 23 (i.e., 24 when excluding staggered board) governance provision indicators in the corporate charter or bylaws introduced by Gompers et al. (2003).
Insider ownership$_{[t]}$	The insider ownership in year *t* is the percentage of shares owned by insiders from all shares. Collected from Compact Disclosure for 1986–2006. We supplement these data with ownership by the top five officers of the firm from ExecuComp for 2007–2015.
Ln(Age) $_{[t]}$	Natural logarithm of firm age, calculated as the difference in year *t* and the first year the company appeared in the CRSP database.
Ln(Assets) $_{[t]}$	Natural logarithm of total book assets in year *t*.
Industry M&A volume$_{[t]}$	The ratio of mergers and acquisitions' dollar volume in SDC to the total market capitalization from CRSP per Fama-French 49 industries. We only consider ordinary stocks and excludes American depositary receipts (ADRs) and real estate investment trusts (REITs). We only include SDC transactions that are completed and where buyer achieves control of the target.
Poison pill$_{[t]}$	Antitakeover provision obtained from the Cremers and Ferrell (2014) database for 1978–1989, and from RiskMetrics for 1990–2015.
R&D/ Sales$_{[t]}$	R&D$_{[t]}$/ Sales$_{[t]}$.
ROA$_{[t]}$	Earnings before interest, taxes, depreciation and amortization over total assets (EBITDA$_{[t]}$/Total assets$_{[t]}$).
Staggered board$_{[t]}$	Indicator variable equal to one (zero otherwise) if the board is staggered in year *t*. Data are obtain from Cremers and Ferrell (2014) for 1978–1989, and from Risk Metrics, SharkRepellent.net, hand collection, and incorporating the data corrections from Larcker et al., (2015) for 1990–2015.
Δ *Staggered board*$_{[t\text{-}1,\ t]}$	Indicator variable equal to one if the board adopted a staggered board in year *t* and equal to minus-one if the board removed a staggered board in year *t*, and equal to zero otherwise.
Staggered board adopted$_{[t\text{-}1,\ t]}$	Indicator variable equal to one if the board adopted a staggered board in year *t*, and equal to zero otherwise.
Staggered board removed$_{[t\text{-}1,\ t]}$	Indicator variable equal to one if the board removed a staggered board in year *t*, and equal to zero otherwise.
System GMM instruments	
S_{SB}/S_{ALL}	Percentage of total sales in an industry-year by firms with a staggered board. Industry is defined as three-digit SIC code.
S_{CON}/S_{ALL}	Percentage of total sales in an industry-year by conglomerate firms. Industry is defined as three-digit SIC code.
$S_{STAGGER}/S_{ALL}$	Percentage of total sales in an industry-year by firms that staggered their board in the past two years. Industry is defined as three-digit SIC code.
$S_{DESTAGGER}/S_{ALL}$	Percentage of total sales in an industry-year by firms that repealed their staggered board in the past two years. Industry is defined as three-digit SIC code.
Interacted variables	
Institutional holding duration$_{[t]}$	Average length of time (in quarters) that the firm's institutional owners of its stock have owned the equities in their 13F holdings reports from Thomson Reuters (available through Wharton Research Data Services), weighted by the institution's ownership in the firm's stock.
Percent transient institutions$_{[t]}$	Percentage of the stock's institutional ownership that is 'transient' as classified in Bushee (1998) using data available from his website. Transient institutions have relatively high portfolio turnover and are relatively well diversified.
Firm sales$_{[t]}$	Ln(Sales) in year *t*.
Ln[(Total assets$_{[t]}$ - Net PP&E$_{[t]}$)/Total assets$_{[t]}$].	
Ranked patent citation count$_{[t]}$	Annually ranked patent citation count. Data are available for 1978–2003. Citations are calculated following Hall, Jaffe and Trajtenberg (2002). Source is the NBER U.S. Patent Citations data file. We divide the ranked patent citation count by 1000.

(*continued on next page*)

Table 1 (*continued*)

Research quotient$_{[t]}$	A firm-specific output elasticity of R&D, representing the percentage change in revenues for 1% change in R&D, as introduced by Knott (2008). Source of data for 1978–2010 is WRDS and from Anne-Marie Knott's website for 2011–2015.
Contract specificity	Industry-level fraction of the inputs that are sold on an organized exchange in the Nunn (2007) data file, available for 1997 only, see http://scholar.harvard.edu/nunn/pages/data-0.
Labor productivity	Data on output per hour from the Bureau of Labor Statistics, see http://www.bls.gov/bls/productivity.htm. Available for 400 selected industries in manufacturing, mining, utilities, wholesale and retail trade, and services.
Large customer	Indicator variable equal to one if there is at least one customer accounting for at least 10% of the consolidated sales of the firm in that fiscal year. The source of the data is the Compustat Customer Segments database. Data are available since 1985.
Strategic alliance	Indicator variable equal to one if the firm is in a strategic alliance. We only include strategic alliances with up to three partners. Source of data is SDC Strategic Alliances. Data are available since 1985.

Table 2
Descriptive statistics for main dependent and independent variables.

This table presents sample descriptive statistics for the main dependent and independent variables, as well as the interacted variables. All continuous variables are winsorized at 2.5% in both tails. See Table 1 for variable definitions.

Dependent variables:	Mean	Median	St. dev.	Min	Max	Obs.
Q $_{[t]}$	1.613	1.307	0.889	0.728	4.729	34,476
Δ Q$_{[t-1,\ t]}$	0.003	0.012	0.449	−1.836	1.580	31,134
Independent variables:	Mean	Median	St. dev.	Min	Max	Obs.
CAPX/Assets$_{[t]}$	0.059	0.047	0.046	0	0.198	34,476
Delaware incorporation$_{[t]}$	0.558	1	0.497	0	1	34,476
G-Index (minus stagg. board)$_{[t]}$	7.680	8	3.200	1	18	23,525
Insider ownership$_{[t]}$	0.070	0.03	0.100	0	1	21,216
Ln(Age) $_{[t]}$	2.870	3	0.980	0	4.450	27,754
Ln(Assets) $_{[t]}$	7.351	7.238	1.578	4.584	11.171	34,476
Industry M&A volume$_{[t]}$	0.028	0.012	0.049	0	0.359	34,476
R&D/Sales$_{[t]}$	0.029	0	0.056	0	0.231	34,476
ROA$_{[t]}$	0.139	0.135	0.079	−0.041	0.324	34,476
Staggered board$_{[t]}$	0.524	1	0.499	0	1	34,476
Interacted variables:	Mean	Median	St. dev.	Min	Max	Obs.
Institutional holding duration	6.846	6.818	0.990	2.226	10.445	28,298
Percent transient institutions	0.222	0.196	0.138	0	0.923	28,298
Ln(Intangible / Total assets$_{[t]}$*)*	−0.487	−0.323	0.483	−3.876	0	34,229
Ranked Patent citation count$_{[t]}$	0.385	0.385	0	0.385	0.385	34,476
Firm sales$_{[t]}$	7.270	7.193	1.527	−2.937	13.089	34,460
Research quotient$_{[t]}$	0.116	0.112	0.049	−0.015	0.275	14,847
Contract specificity$_{[t]}$	0.913	0.967	0.131	0.352	0.998	10,478
Labor productivity$_{[t]}$	1.472	1.104	0.615	0.825	2.904	27,715
Large customer$_{[t]}$	0.333	0	0.471	0	1	29,107
Strategic alliance$_{[t]}$	0.214	0	0.410	0	1	27,782

sion for columns 4–6 for the full time period (1978–2015), as well as the first and second half of the sample (i.e., the 1978–1995 and 1996–2015 sub-periods, respectively). Separating these sub-periods is particularly interesting due to the many changes in corporate governance, ownership, and takeover markets that occurred during these periods, which likely affected the role of staggered boards.

In column 1 using our full 1978–2015 sample, the cross-sectional association of *Staggered board* and *Q* is negative and both statistically and economically significant, suggesting that firms with a staggered board have a firm value that is 2.1% (= −0.034/1.61) lower than firms without a staggered board. We thus find a negative cross-sectional association like Bebchuk and Cohen (2005), but one with a considerably lower economic magnitude. We find similarly negative coefficient estimates of *Staggered board* in our two sub-periods, but the negative coefficient is only statistically significant (at 5% confidence level) for the 1978–1995 period and becomes insignificant for the 1996–2015 period.

We consider the time series evidence in column 4 in Table 3 by using the same pooled panel Tobin's *Q* regressions as for column 1 but now with firm rather than industry fixed effects. Including firm fixed effects is equivalent to removing the time-invariant component in *Q*, *Staggered board*, and all controls, reducing the potential bias resulting from omitted time-invariant variables at the firm level. Once we include firm fixed effects, we are essentially comparing the average firm value before versus after a change in board structure. We find a statistically significant positive time series association between *Staggered board* and *Q*.[5] For the full sample in column 4, the adoption (removal)

[5] The firm fixed effects results in Table 3 exploit only the "within firm" variation using firm fixed effects. As a robustness check, Online Appendix Table A.3 reports the "between firm" coefficients of *Staggered board*, only exploiting cross-sectional variation and ignoring time series changes in board structure within firms. The "between firm" coefficients are similar to the results in Table 3, confirming that those pooled panel regression

Fig. 1. Percentage of firms with a staggered board. The figure shows the percentage of firms with a staggered board in our 1978–2015 sample period. Excluded from the sample are firms that have dual class shares. See Table 2 for sample descriptive characteristics.

of a staggered board is associated with an increase (decrease) in *Q* of 3.2% (=0.051/1.61), with a *t*-statistic of 2.06 using standard errors clustered by firm.[6]

The positive long-term time series association between staggered boards and *Q* is driven by the second half of the sample. As shown in column 5 in Table 3, the coefficient of *Staggered board* is positive but insignificant for the 1978–1995 period, while column 6 shows a coefficient of 0.098 with a *t*-statistic of 2.73 when we use clustering by firm for the 1996–2015 period. The economic magnitude of the latter effect seems meaningful, as this coefficient indicates that the adoption (removal) of a staggered board is associated with an increase (decrease) in *Q* of 5.5% (i.e., dividing the coefficient of 0.098 by the average *Q* in the sample, which is 1.77).

Many possible explanations exist for these changes in the association between firm value and board structure across time. In general, these results suggest that the staggered board structure became increasingly relevant over time. In particular, Cremers and Ferrell (2014) show that board control, and more generally shareholder rights, became more important after the judicial validation of the poison pill by the Delaware Supreme Court in 1985. This is because with a poison pill in place, or the ability to adopt a poison pill on short notice referred to as a "shadow pill" or a pill "on the shelf," see Coates (2000), hostile takeovers became more difficult, as gaining board control and having the pill removed became the only route available to a prospective bidder.[7] Further, it is also likely that

results with industry fixed effects essentially capture cross-sectional variation. Online Appendix Table A.3 also shows the variation decomposition of *Q*, indicating that the cross-sectional (between firm) variation in *Q* is considerably larger than its time series variation.

[6] Online Appendix Table A.4 presents results for the time period used in Bebchuk and Cohen (2005), 1995–2002, with their additional controls. Overall, the cross-sectional results are consistent with Bebchuk and Cohen (2005), although they report stronger economic and statistical significance (though using robust standard errors that do not seem to be clustered), and using two-digit rather than four-digit SIC codes as we do. Results with two-digit industry SIC codes are similar in magnitude to those presented in Bebchuk and Cohen (2005) (see Online Appendix Table A.5). Our results, however, remain the same using either three-digit SIC codes or the Fama-French 49 industry groups.

[7] Consistent with this, in Online Appendix Table A.4 and using firm fixed effects, we find that *Staggered Board* has an insignificant coefficient for 1978–1985 (column (4)), while it has a positive and significant coefficient for 1986–2015 (column (5)). However, many other changes over the 1980s and 1990s may have played a role next to or with the poison pill, such as the emergence of institutional investors and increasingly

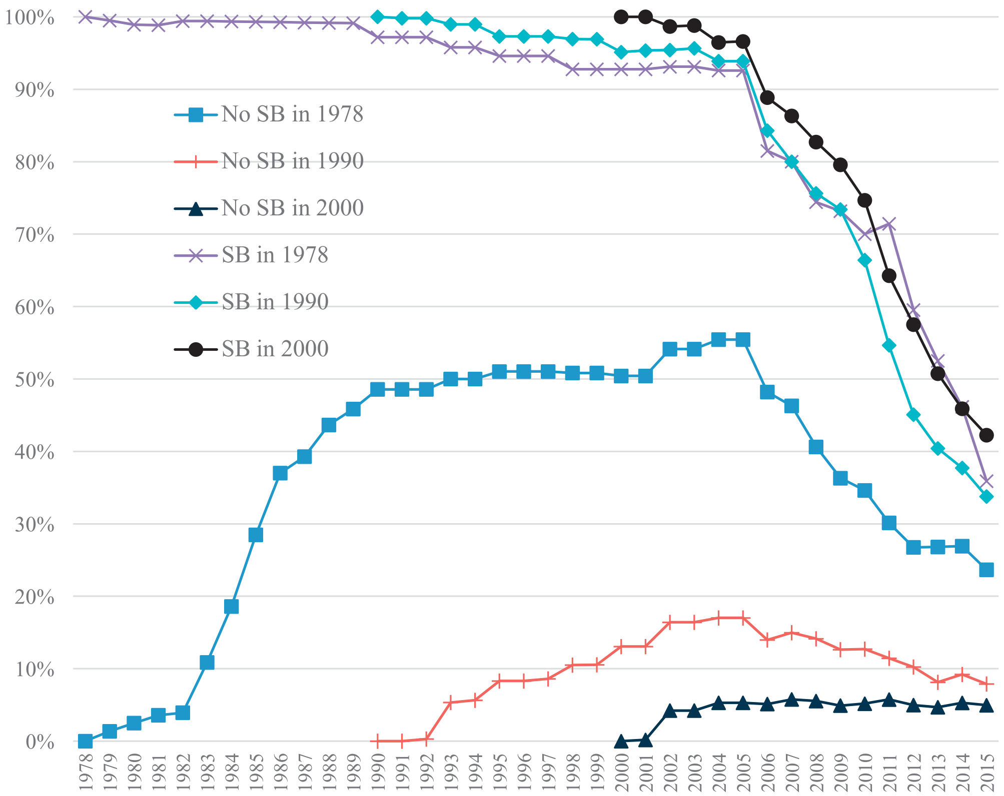

Fig. 2. Cohort analysis for staggering up and de-staggering. The figure shows the percentage of firms with a staggered board each year for six cohorts of firms: (1) firms with a staggered board in 1978 (SB in 1978), (2) firms without a staggered board in 1978 (No SB in 1978), (3) firms with a staggered board in 1990 (SB in 1990), (4) firms without a staggered board in 1990 (No SB in 1990), (5) firms with a staggered board in 2000 (SB in 2000), and (6) firms without a staggered board in 2000 (No SB in 2000). The figure shows the annual percentage with a staggered board *within* each cohort as a percentage of those firms that remain in our sample that year until the last year in our data, 2015.

Table 3
Firm value and staggered boards.

This table presents annual pooled panel Q regressions on *Staggered board* with industry (columns 1–3) or firm (columns 4–6) fixed effects. All specifications include year dummies and the following control variables: *Staggered board*$_{[t-1]}$, Ln(*Assets*)$_{[t-1]}$, *Delaware incorporation*$_{[t-1]}$, *ROA*$_{[t-1]}$, *CAPX/Assets*$_{[t-1]}$, *R&D/Sales*$_{[t-1]}$, and *Industry M&A volume*$_{[t-1]}$. The analysis includes the following sub-periods: 1978–2015, 1978–1995, and 1996–2015. All variables are defined in Table 1. *T*-statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively. For the key independent variable, *Staggered board*$_{[t-1]}$, we also show the *t*-statistics based on robust standard errors that are not clustered, reported in brackets.

Dep. variable: $Q_{[t]}$						
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Period:	1978–2015	1978–1995	1996–2015	1978–2015	1978–1995	1996–2015
Fixed Effects:	Industry + year fixed effects			Firm + year fixed effects		
Staggered board$_{[t-1]}$	−0.034**	−0.046**	−0.036	0.051**	0.023	0.098***
(firm cluster)	(2.04)	(2.39)	(1.61)	*(2.06)*	(0.73)	(2.73)
[no cluster]	*[4.34]*	*[4.65]*	*[3.18]*	*[4.32]*	*[1.58]*	*[5.16]*
Ln (Assets)$_{[t-1]}$	−0.031***	−0.033***	−0.031***	−0.222***	−0.104***	−0.352***
	(4.35)	(3.83)	(3.34)	(13.14)	(4.33)	(15.08)
Delaware incorporation$_{[t-1]}$ *INcorporaIncoincorporation*$_{[t-1]}$	0.02	0.023	0.014			
	(1.07)	(1.09)	(0.55)			
ROA$_{[t-1]}$	5.338***	4.043***	5.825***	3.189***	2.039***	2.81***
	(34.17)	(21.51)	(30.11)	(21.62)	(12.53)	(15.72)
CAPX/Assets$_{[t-1]}$	−0.376	−0.493*	0.315	0.057	0.102	0.299
	(1.68)	(1.83)	(1.00)	(0.34)	(0.58)	(1.22)
R&D/ Sales$_{[t-1]}$	4.168***	4.168***	4.323***	1.274**	2.302*	−0.288
	(11.88)	(5.98)	(11.13)	(2.46)	(1.92)	(0.50)
Industry M&A volume$_{[t-1]}$	−0.231***	0.117	−0.32***	−0.235***	−0.047	−0.252***
	(3.03)	(1.11)	(3.46)	(3.40)	(0.47)	(3.03)
# of firms in regression	3,076	1,581	2,415	3,076	1,581	2,415
N	34,476	13,022	21,454	34,476	13,022	21,454
Adj. R^2	0.510	0.542	0.495	0.710	0.758	0.727

investors became more aware over time of the availability of shadow pills, which could similarly explain why the deterrence effect of staggered boards gradually increased (Coates, 2000).[8]

Next, we investigate the time series dimension of the association between firm value and staggered boards by regressing changes in Q on changes in *Staggered board*. We calculate the change in firm value at the end of the fiscal year when the board change occurred to the firm value one, two, and three years later. The results in Panel A of Table 4 confirm that firm value, as proxied by Q, increases following the adoption of a staggered board and decreases following a decision to de-stagger. The coefficients on the change in *Staggered board* in columns 1–3 show that the increase (decrease) in value after staggering up (down) occurs gradually, rather than all in the first year. This suggests that market participants need some time to fully learn and process the changed prospects of the firm that occur after the change in board structure. Columns 4–6 show that the changes in board structure are not associated with changes in Q in the first half of the sample (i.e., 1978–1995), while columns 7–9 show a strongly positive association in the second half of the sample (i.e., 1996–2015), consistent with the pooled panel regressions with firm fixed effects in Table 3.

Given the trends in staggering up and down over our full time period (i.e., 1978–2015), it is a useful robustness check to distinguish between adoptions versus removals of staggered boards. The dummy variable *Staggered board adopted* equals one in the year that the firm adopts a staggered board and zero otherwise. The dummy variable *Staggered board removed* equals one in the year that the firm removes a staggered board and zero otherwise. The results are presented in Panel B of Table 4. As shown in column 3 for the change in Q after three years and using the full time period, the coefficients on adoptions and removals of staggered boards are of similar magnitude but opposing signs, with a coefficient for *Staggered board adopted* of 0.112 (t-stat. =3.50) and *Staggered board removed* of −0.142 (t-stat. =2.86). As before, these results are driven by the 1996–2015 time period (columns 7–9) and are insignificant in the 1978–1996 time period (columns 4–6).

3.2. Portfolio approach

As a robustness check to our Q analysis, we conduct a long-term stock return event study around changes in board structure that provides complementary evidence to existing short-term event studies, which tend to provide mixed and statistically fairly weak results (Jarrell and Poulsen, 1987; Mahoney and Mahoney, 1993; Faleye, 2007; Guo, Kruse and Nohel, 2008, 2014). Following prior studies (Gompers et al., 2003; Bebchuk et al., 2009; Cremers and Ferrell, 2014), Table 5 reports the abnormal returns of equally weighted portfolios buying (selling) stocks of firms around the time they stagger up (down).[9] We consider portfolios that use staggered board information at the time this was public information, as well as portfolios that are constructed with perfect foresight of subsequent changes in board structure, and various lengths of time to hold the stocks.

Overall, the results of our long-term event study are consistent with our results using Q, although the abnormal return estimates are quite noisy due to the limited number of stocks in each portfolio (on average 13–23 stocks, depending on how long we keep stocks in the portfolio). In the long portfolio, the stocks of firms are purchased before they stagger up and tend to exhibit positive abnormal performance that is statistically significant using the market model, insignificant using the three-factor model, and marginally significant using the four-factor model. In the short portfolio, the stocks of firms are purchased before they de-stagger and generally have negative alphas that are consistently statistically insignificant. The long–short portfolio consistently has positive abnormal returns with a statistical significance that again depends on the factor model and portfolio construction that we use.

Combined with our change-in-Q regressions in Tables 3 and 4, these results suggest that investors learn about prospective changes to board structure over time, rather than primarily at the proxy filing date or the annual meeting date. This apparent gradual learning also makes it more difficult to interpret short-term event studies of abnormal returns concerning staggered boards around the proxy filing or the annual meeting dates.

3.3. Three specific selection mechanisms

As board structure is an endogenous choice (Adams et al., 2010), a crucial challenge affecting our results is that endogeneity can lead to selection effects, where firms with particular characteristics are more likely to adopt or repeal a staggered board. This includes the possibility that firm performance drives changes in corporate governance rather than the other way around, or both could be related to factors that are omitted or difficult to observe, such as disruptive innovations or complex competition dynamics. While employing changes in firm value or using firm fixed effects helps to ameliorate the endogeneity bias, these methodologies leave open the possibility of dynamic endogeneity (Wintoki et al., 2012; Hoechle, Schmid, Walter, and Yermack, 2012). In order to mitigate these concerns, in this section we examine three different selection mechanisms: (1) the market for corporate control; (2) the possibility of entrenchment or board capture; and (3) reverse causality. In Sections 3.4–3.6, we employ several different matching

independent boards after the reform of NYSE listing standards and the introduction of the Sarbanes-Oxley Act. A more detailed investigation of these changes and their relevance for staggered boards falls outside the scope of this paper.

[8] We also checked whether the association between changes in board structure and firm value are strongly affected by the technology sector valuation increase and decrease in 1999–2001, or by the recent financial crisis. We also find that our results are robust to removing 1999–2001 or 2007–2009 from the sample (or 2005–2007 when we consider changes in Q), as well as to adding higher-order fixed effects where we interact four-digit SIC codes and year fixed effects in a specification with firm fixed effects (see Online Appendix Table A.6).

[9] Results for value-weighted portfolios are reported in Panel A of Online Appendix Table A.7, and further robustness checks are provided in Panel B of Online Appendix Table A.7.

Table 4
Changes in firm value and changes in staggered boards.

Panels A and B presents pooled panel first difference regressions with the dependent variable being the change in Q from t to $t+1$ in columns 1, 4 and 7 (i.e., $\Delta\ Q_{[t,\ t+1]}$), the change in Q from t to $t+2$ in columns 2, 5 and 8 (i.e., $\Delta\ Q_{[t,\ t+2]}$), and the change in Q from t to $t+3$ in columns 3, 6 and 9 (i.e., $\Delta\ Q_{[t,\ t+3]}$). The dependent variables have been demeaned with their annual cross-sectional averages. As independent variables, we include the following: Δ *Staggered board*$_{[t-1,t]}$ (in Panel A), Δ Ln*(Assets)*$_{[t-1,t]}$, Δ *ROA*$_{[t-1,t]}$, Δ *CAPX/Assets*$_{[t-1,t]}$, Δ *R&D/Sales*$_{[t-1,t]}$, and Δ *Industry M&A volume*$_{[t-1,t]}$. In Panel B, *Staggered board removed*$_{[t-1,t]}$ and *Staggered board adopted*$_{[t-1,t]}$ are used instead of Δ *Staggered board*$_{[t-1,t]}$. Sample period is 1978-2015 in columns 1–3, 1978–1995 in columns 4–6, and 1996–2015 in columns 7-9. Standard errors are clustered at the firm level. Results are robust to an adjustment to the standard errors for autocorrelation as in Newey-West (where the adjustment includes up to six lags). *T*-statistics (in their absolute value) are based on robust standard errors and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively. All control variables are defined in Table 1. Included but not shown are industry fixed effects using the Fama-French 49 industry definitions. Our sample for column 1 includes 386 cases of staggering up and 309 cases of staggering down.

Panel A:

Dep. variable:	$\Delta\ Q_{[t,\ t+1]}$	$\Delta\ Q_{[t,\ t+2]}$	$\Delta\ Q_{[t,\ t+3]}$	$\Delta\ Q_{[t,\ t+1]}$	$\Delta\ Q_{[t,\ t+2]}$	$\Delta\ Q_{[t,\ t+3]}$	$\Delta\ Q_{[t,\ t+1]}$	$\Delta\ Q_{[t,\ t+2]}$	$\Delta\ Q_{[t,\ t+3]}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	1978 – 2015			1978 – 1995			1996 – 2015		
Δ *Staggered board*$_{[t-1,\ t]}$	0.0201	0.0767***	0.123***	−0.00167	−0.0178	−0.0185	0.00138	0.0728*	0.135***
	(1.36)	(3.01)	(4.26)	(0.09)	(0.57)	(0.55)	(0.06)	(1.82)	(2.91)
Δ Ln*(Assets)*$_{[t-1,\ t]}$	−0.303***	−0.545***	−0.594***	−0.144***	−0.268***	−0.316***	−0.366***	−0.666***	−0.733***
	(14.65)	(17.33)	(15.82)	(5.86)	(7.81)	(7.25)	(13.96)	(16.47)	(14.79)
Δ *ROA*$_{[t-1,\ t]}$	−0.373***	−0.844***	−1.289***	−0.143	−0.202	−0.451***	−0.474***	−1.206***	−1.857***
	(4.44)	(8.58)	(10.83)	(1.59)	(1.57)	(3.30)	(3.98)	(8.51)	(10.33)
Δ *CAPX/Assets*$_{[t-1,\ t]}$	−0.902***	−0.930***	−1.200***	−0.393***	−0.487***	−0.718***	−1.404***	−1.559***	−2.027***
	(6.47)	(5.35)	(6.65)	(2.98)	(3.59)	(4.38)	(6.14)	(4.98)	(6.32)
Δ *R&D/ Sales*$_{[t-1,\ t]}$	0.674	0.374	0.167	1.898	1.159	1.815	0.459	0.0226	−0.387
	(1.61)	(0.74)	(0.28)	(1.57)	(0.86)	(1.20)	(0.99)	(0.04)	(0.59)
Δ *Industry M&A volume*$_{[t-1,t]}$	−0.0503	−0.107***	−0.161***	0.0934*	−0.107*	0.0427	−0.0708**	−0.104**	−0.195***
	(1.59)	(2.87)	(3.99)	(1.78)	(1.87)	(0.71)	(2.00)	(2.47)	(4.11)
N	31,049	27,017	24,146	11,435	11,014	10,638	19,614	16,003	13,508
Adjusted R^2	0.017	0.031	0.032	0.009	0.012	0.013	0.021	0.041	0.047

Panel B:

Dep. Variable:	$\Delta\ Q_{[t,\ t+1]}$	$\Delta\ Q_{[t,\ t+2]}$	$\Delta\ Q_{[t,\ t+3]}$	$\Delta\ Q_{[t,\ t+1]}$	$\Delta\ Q_{[t,\ t+2]}$	$\Delta\ Q_{[t,\ t+3]}$	$\Delta\ Q_{[t,\ t+1]}$	$\Delta\ Q_{[t,\ t+2]}$	$\Delta\ Q_{[t,\ t+3]}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	1978 – 2015			1978 – 1995			1996 – 2015		
Staggered board removed$_{[t-1,\ t]}$	−0.012	−0.087**	−0.142***	0.033	0.171	0.127	0.002	−0.068*	−0.105**
	(0.53)	(2.22)	(2.86)	(0.34)	(1.02)	(0.96)	(0.10)	(1.73)	(2.04)
Staggered board adopted$_{[t-1,\ t]}$	0.029	0.070**	0.112***	0.001	−0.005	−0.010	0.019	0.089	0.207***
	(1.55)	(2.23)	(3.50)	(0.05)	(0.16)	(0.28)	(0.45)	(0.98)	(2.82)
Δ Ln*(Assets)*$_{[t-1,\ t]}$	−0.303***	−0.545***	−0.594***	−0.144***	−0.267***	−0.316***	−0.366***	-0.666***	−0.732***
	(14.64)	(17.34)	(15.82)	(5.85)	(7.79)	(7.24)	(13.95)	(16.47)	(14.76)
Δ *ROA*$_{[t-1,\ t]}$	−0.374***	−0.844***	−1.288***	−0.144	−0.204	−0.453***	−0.474***	−1.206***	−1.856***
	(4.44)	(8.58)	(10.83)	(1.60)	(1.59)	(3.31)	(3.98)	(8.51)	(10.32)
Δ *CAPX/Assets*$_{[t-1,\ t]}$	−0.902***	−0.930***	−1.200***	−0.393***	−0.489***	−0.719***	−1.404***	−1.559***	−2.027***
	(6.48)	(5.35)	(6.64)	(2.98)	(3.60)	(4.39)	(6.14)	(4.98)	(6.32)
Δ *R&D/ Sales*$_{[t-1,\ t]}$	0.674	0.374	0.167	1.898	1.159	1.816	0.459	0.023	−0.383
	(1.61)	(0.74)	(0.28)	(1.57)	(0.86)	(1.20)	(0.99)	(0.04)	(0.59)
Δ *Industry M&A volume*$_{[t-1,t]}$	−0.050	−0.107***	−0.161***	0.093*	−0.107*	0.043	−0.071**	−0.104**	−0.195***
	(1.59)	(2.87)	(3.99)	(1.78)	(1.87)	(0.71)	(2.00)	(2.47)	(4.12)
N	31,049	27,017	24,146	11,435	11,014	10,638	19,614	16,003	13,508
Adjusted R^2	0.017	0.031	0.032	0.009	0.012	0.013	0.021	0.041	0.047

procedures, dynamic GMM estimation, and exogenous variation in Massachusetts corporate law.

3.3.1. The market for corporate control

Staggered boards could relate to firm value through takeovers, as positive (negative) changes in firm value after adopting (removing) a staggered board may be partly due to an anticipation effect of future takeovers that may or may not materialize (Song and Walkling, 2000; Cremers, Nair and John, 2008; Edmans et al., 2012). In particular, boards could opportunistically use takeover anticipation to convince shareholders to adopt a staggered board in order to negotiate from a stronger position with potential acquirers, which could provide explanatory power for the subsequent increase in firm value. Reversely, the decrease in firm value upon de-staggering could be due to an anticipated takeover not occurring.

However, Q regressions that include interactions with different proxies for M&A intensity provide no evidence consistent with this possibility (see Online Appendix Table A.8). These results are consistent with Edmans et al. (2012), who find that the “trigger effect” of future takeover

Table 5
Portfolio analysis.
This table presents abnormal returns of equally weighted monthly portfolios of firms that have staggered up (in the long portfolio) and firms that have de-staggered (in the short portfolio) around board staggering and de-staggering events in our sample of firms during the time period 1978–2015. The long (short) portfolios are composed as follows. For portfolio *6m12* (*12m12*), we include all stocks of firms that have (de)staggered their boards starting six (12) months before the fiscal year-end of the year in which the firm has reported its board being (de)staggered for the first time, and hold these stocks for 12 months. Portfolio *12m24* is constructed like *12m12* portfolio except that stocks are held for 24 months. We use three models: the four-factor Carhart (1997) model (i.e., momentum, high minus low book-to-market (HML), small minus big (SMB), and market return), the three-factor Fama-French model (i.e., HML, SMB, and market return), and the market model (i.e., including only the market return). For each model, we present the returns to the long portfolio, short portfolio, and long minus short portfolio. *T*-statistics (in their absolute value) are based on robust standard errors and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively. The number of stocks in the long and short portfolios is averaged across all months.

	Four-factor model			Three-factor model			Market factor model		
Portfolio *"6m12"*	Long	Short	Long–Short	Long	Short	Long–Short	Long	Short	Long–Short
Alpha (monthly)	0.449*	0.145	0.361	0.415	0.063	0.41	0.652**	0.175	0.442
	(1.68)	(0.56)	(0.87)	(1.53)	(0.25)	(1.00)	(2.26)	(0.66)	(1.08)
Average # firms	*12*	*19.1*	–	*12*	*19.1*	–	*12*	*19.1*	–
N	355	250	231	355	250	231	355	250	231
Adj. R^2	0.562	0.641	0.019	0.562	0.626	0.017	0.491	0.583	0.004
	Four-factor model			Three-factor model			Market factor model		
Portfolio *"12m12"*	Long	Short	Long–Short	Long	Short	Long–Short	Long	Short	Long–Short
Alpha (monthly)	0.581*	−0.184	1.086**	0.455	−0.296	1.136**	0.598**	−0.175	1.082**
	(1.79)	(0.77)	(2.12)	(1.39)	(1.25)	(2.32)	(1.97)	(0.72)	(2.40)
Average # firms	*12*	*18.9*	–	*12*	*18.9*	–	*12*	*18.9*	–
N	353	256	235	353	256	235	353	256	235
Adj. R^2	0.448	0.632	0.006	0.443	0.628	0.009	0.402	0.598	0.007
	Four-factor model			Three-factor model			Market factor model		
Portfolio *"12m24"*	Long	Short	Long–Short	Long	Short	Long–Short	Long	Short	Long–Short
Alpha (monthly)	0.30*	0.045	0.273	0.202	−0.053	0.259	0.424**	0.035	0.311
	(1.69)	(0.23)	(0.98)	(1.12)	(0.29)	(0.96)	(2.18)	(0.18)	(1.18)
Average # firms	*21.5*	*28.6*	–	*21.5*	*28.6*	–	*21.5*	*28.6*	–
N	438	397	391	438	397	391	438	397	391
Adj. R^2	0.627	0.639	0.006	0.621	0.634	0.008	0.553	0.608	0.001

activity seems to dominate the "anticipation effect," meaning that low firm value tends to attract more takeover activity rather than (the expectation of) more takeover activity resulting in higher firm value. These results are also consistent with Bates, Becher and Lemmon (2008), who find that firms with a staggered board have similar takeover outcomes as firms with unitary boards. For example, they show that firms with a staggered board, once targeted, have both a similar likelihood to be acquired and similar bid announcement returns, concluding that "the economic effect of bid deterrence on the value of the firm is quite small. Overall, the evidence is inconsistent with the conventional wisdom that board classification is an anti-takeover device that facilitates managerial entrenchment (p. 656)."

3.3.2. Private information of (entrenched?) boards

A further selection mechanism consistent with our results is that firms could be more likely to stagger up (down) if the board has good (bad) private information that is not yet incorporated into the stock price. Staggering up in the case of anticipated long-term value increases may thus involve a potential trade-off between protecting the higher long-term value from being expropriated in a short-term takeover and the potential cost of entrenching future directors. De-staggering in the case of anticipated bad news may facilitate an acquisition before the bad news is fully realized or may increase shareholder power to remove directors in the near term.

By construction, this selection mechanism is hard to (dis)prove, as it involves private information. However, the resulting changes in board structure seem inconsistent with the interests of currently entrenched insiders who aim to prioritize their own interests over shareholder interests. If directors want to adopt a staggered board against shareholder interests, then the right circumstances to do so would seem to be either when directors have private information of future bad news or right after (rather than

Table 6
Firm value and staggered boards: reverse causality tests.

This table presents regression results for the adoption (columns 1–3) and removal (columns 4–6) of a staggered board as a function of the valuation of the firm (as captured by $Q_{[t-1]}$) plus other characteristics. The 1978–2015 sample for columns 4–6 (1–3) includes all firms that do (not) have a staggered board up until (and including) the year in which they remove (adopt) the staggered board if there is any such change, and are dropped from the sample afterwards. We use the Cox proportional hazard model (see Greene, 2000) and report the marginal effects using robust standard errors clustered at firm level (after standardizing the continuous variables to have zero mean and unit variance). The model includes the following control variables: $Q_{[t-1]}$, Ln(*Assets*)$_{[t-1]}$, *Delaware incorporation*$_{[t-1]}$, *ROA*$_{[t-1]}$, *CAPX/Assets*$_{[t-1]}$, *R&D/ Sales*$_{[t-1]}$, and *Industry M&A volume*$_{[t-1]}$. *T*-statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively. All variables are defined in Table 1.

Dep. variable:	Cox models Pr (Stagger in period t)			Cox models Pr (De-stagger in period t)		
	1978–2015	1978–1995	1996–2015	1978–2015	1978–1995	1996–2015
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Q_{[t-1]}$	−0.844***	−0.347***	−0.196	−0.12	0.246	−0.127
	(7.59)	(4.54)	(1.00)	(1.48)	(1.16)	(1.55)
Ln(Assets) $_{[t-1]}$	0.106*	0.054	0.122	0.769***	0.343	0.806***
	(1.70)	(0.89)	(0.97)	(9.99)	(0.88)	(12.17)
Delaware incorporation$_{[t-1]}$	−0.24***	0.022	−0.625***	0.005	-0.083	0.063
	(2.17)	(0.18)	(2.52)	(0.04)	(0.15)	(0.56)
ROA$_{[t-1]}$	0.36***	0.227***	−0.07	0.10	−0.658	0.142
	(4.80)	(2.77)	(0.36)	(1.16)	(1.51)	(1.80)
CAPX/Assets$_{[t-1]}$	0.122***	0.156***	−0.061	0.073	0.245	0.021
	(2.66)	(2.69)	(0.51)	(1.53)	(1.36)	(0.43)
R&D/ Sales$_{[t-1]}$	−0.083	−0.077	−0.196	0.015	−0.539	0.077
	(1.18)	(1.36)	(1.14)	(0.22)	(1.62)	(1.11)
Industry M&A volume$_{[t-1]}$	0.009	0.051	0.175*	−0.155	0.065	−0.177
	(0.14)	(1.03)	(1.75)	(1.59)	(0.32)	(1.66)
# of firms in regression	1,683	1,003	1,035	1,513	607	1,476
N	15,661	7,376	8,406	14,587	4,275	12,338
Pseudo R^2	0.038	0.012	0.024	0.038	0.043	0.042

before) good news is released that increases the stock price. Indeed, if directors are entrenched and only willing to give up staggered boards under strong shareholder pressure, we would expect boards to be more likely to give in to such pressure after, rather than before, the realization of privately anticipated bad news, in the hope that the bad news will not materialize.

Setting aside the question of whether selection based on private information is consistent with the entrenchment view of staggered boards, we empirically verify that our results do not seem driven by more entrenched boards as proxied by a high *G-Index* score or a poison pill (see Online Appendix Table A.8). Further, using CEO turnover data from Jenter and Kanaan (2015), we show in Online Appendix Table A.9 that having a staggered board is unrelated to the likelihood to fire the CEO, which again seems inconsistent with staggered boards promoting entrenchment.

3.3.3. Reverse causality

Another possible self-selection mechanism is reverse causality. In this case, having a relatively low firm value induces firms to adopt a staggered board (rather than a staggered board causing a low firm value), thereby providing explanatory power for the cross-sectional result that firms with staggered boards tend to have low firm values. Table 6 presents the results of Cox proportional hazard model (Greene, 2000, 2004) regressions explaining the adoption (removal) of a staggered board, including in the sample all firms that do not (do) have a staggered board until they adopt (remove) a staggered board.[10] In each specification, all variables are standardized to have zero mean and unit variance so that the presented marginal likelihoods can directly indicate the economic magnitude of a standard deviation change in the independent variable.

As shown in column 1 in Table 6, the coefficient of lagged *Q* indicates that a standard deviation increase in firm value is associated with a decrease of 84% in the probability of staggering up (*t*-stat. =7.59). This suggests that the choice of staggering up is primarily made by firms with a relatively low valuation before they adopt a staggered board, and helps reconcile the negative cross-sectional association to our evidence of a positive time series association. Indeed, the financial value of firms that staggered up in our sample increased after staggering up, but is insufficient to erode the pre-existing value difference with the other firms in their industry. Results using a random probit model are similar for the decision to adopt a staggered board for the full time period, although with reduced economic magnitude.[11] Concerning de-staggering

[10] Online Appendix Table A.10 shows the analogous results using probit models, where the marginal likelihood of lagged Q is negative but only significant in the period 1978–1995.

[11] Our reverse causality conjecture is confirmed when we add the one-year lagged Q as an additional control to the pooled panel regressions with industry fixed effects regressions of Table 3, presented in Online Appendix Table A.4 in column 6. If reverse causality affects the cross-sectional results, we expect the identified negative impact to become con-

decisions, columns 3 and 4 of Table 6 show statistically insignificant results, indicating that firm value does not reliably predict de-staggering, which is inconsistent with the reverse causality argument.

3.4. Changes in Q using matched samples

Motivated by the results in Section 3.3, we next incorporate possible selection effects through the creation of a matched sample for all firm-years that have either staggered up or staggered down (i.e., the treatment firms), where the control firms in the matched sample do not change their staggered board structure in the year that the treatment firms change their board structure (the event year). In creating various matched samples, our aim is to select control firms with similar firm- and industry-level characteristics as the treatment firms, in order to increase the likelihood that unobserved endogeneity similarly affects both groups.

We consider four different methods to assign control firms to the treatment firms: (1) propensity score matching using the probit models for staggering and de-staggering decisions, respectively; (2) propensity score matching using the Cox proportional hazard model of columns 1 and 4 of Table 6 for staggering and de-staggering decisions, respectively; (3) nearest-neighbor matching (Abadie and Imbens, 2006) using the main characteristics that are significantly related to staggering and de-staggering decisions in Table 6, namely, lagged *Q*, profitability, capital expenditures, and R&D; and (4) radius matching, where treated firms are matched to one or more control firms having a propensity score within a predefined radius (Dehejia and Wahba, 2002).[12] In all of these methods, for each treatment firm changing its board structure, we choose a control firm in the same Fama-French 49 industry group, although results are similar if we do not require industries to be the same.

For the various matched samples, we rerun the firm fixed effects pooled panel regressions we used for column 1 of Table 4 (reporting the results in Panel A of Table 7) and the change-in-*Q* regressions for Table 5 (reporting the results in Panel B of Table 7). For the pooled panel regressions, we only use observations for the two years preceding and the two years following the event year in which the treatment firms change their board structure. While doing so limits the number of observations, it also decreases the likelihood of other changes rendering treatment and control firms less comparable.[13] Further, the treatment and control firms have similar characteristics, as none of the variables have economically or statistically significant differences (even at the 10% level) across the two groups in any of the matched samples.

The matched sample results in Table 7 are quite similar to the full sample results in Tables 4 and 5. As shown in Panel A, the coefficient of *Staggered board* from the pooled panel regressions with firm fixed effects and the standard controls is statistically significant at the 5% level for all four matching methods. Further, the results in Panel B again confirm our main result that the financial value of firms that adopt (remove) a staggered board goes up (down), as the coefficient on Δ*Staggered board* remains positive and statistically significant across all specifications and matching procedures.

3.5. System GMM estimator

Another possibility is that changes in board structure might reflect a dynamic endogeneity. We consider this by applying the dynamic system GMM estimator proposed by Arellano and Bover (1995) and Blundell and Bond (1998) and implemented in a corporate finance setting by Wintoki et al. (2012), among others. This methodology estimates a system in which firm value, board structure, and other key corporate characteristics are all jointly endogenous and dynamically interrelated. Indeed, our previous probit and Cox regression results indicate that staggering up decisions are strongly related to past performance and other firm- and industry-level characteristics. As a result, it is useful to show that our results are robust to implementing a methodology that explicitly incorporates dynamic and simultaneous endogeneity, as well as unobservable heterogeneity.

The disadvantages of the GMM approach are that results may be sensitive to the choice of instruments, and, more importantly, that weak instruments could bias the results. We try to mitigate these concerns by using a variety of specifications with different sets of instruments for which statistical tests reject that the particular set of instruments is weak. Another limitation is that this methodology effectively considers only the variation in board structure that is related to variation in observable characteristics (whereas many changes in board structure may be driven by variables that are hard to observe), such that we present these results mainly as a robustness check rather than as showing causality.

Our implementation closely follows Hoechle et al. (2012), who apply the dynamic system GMM estimator to model the diversification discount. First, we choose the set of endogenous variables to include: (i) firm value (as proxied by *Q*, and its first and second lag); (ii) having a staggered board; (iii) profitability (as proxied by *ROA*); (iv) capital expenditures (*CAPX/Assets*); and (v) research and development expenditures (*R&D/Sales*), where the last three variables are included because they help predict changes to board structure (see Table 6). Each endogenous variable is modeled in the level equation as depending on (i) the first difference of the fourth lag of the variable at hand and the other endogenous variables (which within the system are "predetermined" and therefore constitute valid instruments), (ii) the standard pre-determined controls (namely, year fixed effects, *Delaware incorporation*,

siderably weaker once we control for lagged firm value. Consistent with this, the cross-sectional association of *Staggered board* and firm value becomes insignificant once we control for lagged firm value.

[12] We use the probit models in columns 1 and 4 in Online Appendix Table A.10. Following Guo and Masulis (2015), we use a radius (or caliper) of 0.15. Our results are robust to an alternative radius of 0.10. A benefit of the radius matching is that it allows for the use of additional control firm matches.

[13] Results are similar if we use the three years preceding and following the event year, or if we use one year preceding and following the event year, or if we simply use all available observations.

Table 7
Firm value and staggered boards: firm fixed effects in matched samples.
The results in Panel A are from an annual pooled panel Q regression on *Staggered board* with firm and year dummies and control variables, as in Table 3. We use different matching procedures for the sample for each column. The sample includes treated firms (i.e., firm that either stagger up or de-stagger) and their matched peers, in a sample starting at t-2 and ending $t+2$ years around the event year. Panel B presents pooled panel first difference regressions with the dependent variable being the change in Q from t to t+ 1 in Column 1 (i.e., $\Delta Q_{[t,\ t+1]}$), the change in Q from t to t+ 2 in Column 2 (i.e., $\Delta Q_{[t,\ t+2]}$), etc. The dependent variables have been demeaned with their annual cross-sectional averages. We use matched samples that consist of treatment firms that change their board structure in year t and control firms that do not change their board structure in year t (and have the same board structure before the change). We consider four different methods to construct matched samples. In both samples, we include the following control variables: *Staggered board*$_{[t-1]}$, Ln *(Assets)*$_{[t-1]}$, *Delaware incorporation*$_{[t-1]}$, *ROA*$_{[t-1]}$, *CAPX/Assets*$_{[t-1]}$, *R&D/ Sales*$_{[t-1]}$, and *Industry M&A volume*$_{[t-1]}$. The time period is 1978–2015. All variables are defined in Table 1. Included but not shown are industry fixed effects using the Fama-French 49 industry definitions. T-statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively.

Panel A:

Dep. variable: $Q_{[t]}$

Variables	(1)	(2)	(3)	(4)
Matching procedure:	Propensity score based on probit	Propensity score based on Cox	Nearest-neighbor matching	Radius matching
Staggered board$_{[t-1]}$	0.067**	0.066**	0.045**	0.079**
(firm cluster)	(2.98)	(2.44)	(2.09)	(3.13)
Ln (Assets)$_{[t-1]}$	−0.175***	−0.202	−0.192***	−0.227***
	(4.67)	(3.26)	(5.27)	(4.88)
ROA$_{[t-1]}$	1.690***	1.858	1.851***	2.296***
	(5.97)	(4.64)	(6.39)	(7.65)
CAPX/Assets$_{[t-1]}$	0.176	0.060	0.095	0.250
	(0.47)	(0.11)	(0.28)	(0.75)
R&D/ Sales$_{[t-1]}$	0.357	−0.250	−0.383	1.268
	(0.34)	(0.20)	(0.45)	(0.80)
Industry M&A volume$_{[t-1]}$	−0.016	0.102	−0.110	−0.035
	(0.13)	(0.52)	(0.75)	(0.19)
N	5,845	3,164	5,923	6,081
Adjusted R^2	0.794	0.771	0.786	0.750

Panel B:

Matching method	Dep. variable:	$\Delta Q_{[t,\ t+1]}$	$\Delta Q_{[t,\ t+2]}$	$\Delta Q_{[t,\ t+3]}$
		(1)	(2)	(3)
Propensity score based on probit likelihood				
	Δ *Staggered board*$_{[t-1,\ t]}$	0.029**	0.041**	0.057**
		(2.16)	(2.25)	(2.32)
Propensity score based on Cox proportional hazard				
	Δ *Staggered board*$_{[t-1,\ t]}$	0.025*	0.039**	0.052**
		(1.89)	(2.13)	(2.10)
Nearest-neighbor match				
	Δ *Staggered board*$_{[t-1,\ t]}$	0.033**	0.044**	0.057**
		(2.46)	(2.39)	(2.30)
Radius matching using propensity score based on probit likelihood				
	Δ *Staggered board*$_{[t-1,\ t]}$	0.025**	0.040**	0.053**
		(1.95)	(2.19)	(2.11)

Ln*(Assets),* and *Industry M&A*), plus (iii) a set of exogenous instruments, described in more detail below and in Table 1.

Second, we take the first difference (in the differenced equation of the GMM system) of all endogenous variables in order to control for unobserved heterogeneity and reduce potential bias from omitted variables. We instrument such endogenous variables with the fourth lag of the variable at hand and the other endogenous variables, as well as the first differences of the pre-determined controls (*Delaware incorporation*, Ln*(Assets)*, and *Industry M&A*), plus the first differences of the exogenous instruments described below.

We use up to four exogenous industry-level instruments (plus their squared values) in our various specifications, each of which relate to changes in board structure and plausibly only affect firm value through changes in board structure. These instruments include the percentage of total sales in the firm's industry for firms that (1) have a staggered board, (2) staggered their board in the past two years, (3) de-staggered their board in the past two years, and (4) are conglomerates. The first three instruments ex-

Table 8
Firm value and staggered boards in a dynamic GMM framework.
This table presents results for annual pooled system GMM Q regressions on *Staggered board* and control variables, allowing for two lags of the dependent variable. In this system GMM estimator, we assume that all control variables are endogenous, except for *Delaware incorporation*$_{[t-1]}$, Ln*(Assets)*$_{[t-1]}$, and *Industry M&A volume*$_{[t-1]}$. The data sample is 1978–2015. The instruments used in the system GMM estimation are listed in Online Appendix Table A.8, which also presents the first stage results. The AR(1) and AR(2) are tests for first-order and second-order serial correlation in the first-differenced residuals, under the null of no serial correlation. The Hansen test of over identification is under the null that all instruments are valid. The Diff-in-Hansen test of exogeneity is under the null that instruments used for the equations in levels are exogenous (Roodman, 2009). All variables are defined in Table 1. *T*-statistics (in their absolute value) are based on robust standard errors and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively.

Dep. variable: $Q_{[t]}$				
Variables	(1)	(2)	(3)	(4)
$Q_{[t-1]}$	1.048***	1.049***	1.029***	1.031***
	(4.36)	(4.50)	(4.25)	(4.47)
$Q_{[t-2]}$	−0.134	−0.141	−0.129	−0.133
	(0.87)	(0.94)	(0.84)	(0.91)
Staggered board$_{[t-1]}$	0.041***	0.036***	0.052***	0.048***
	(3.04)	(2.69)	(2.99)	(2.78)
Ln (Assets)$_{[t-1]}$	0.006	0.005	0.005	0.005
	(0.97)	(0.84)	(0.79)	(0.78)
Delaware incorporation$_{[t-1]}$	−0.021**	−0.018*	−0.018*	−0.016*
	(2.13)	(1.90)	(1.77)	(1.66)
ROA$_{[t-1]}$	−0.523	−0.424	−0.436	−0.366
	(0.84)	(0.68)	(0.70)	(0.59)
CAPX/Assets$_{[t-1]}$	−0.176	−0.192	−0.03	−0.117
	(0.18)	(0.21)	(0.03)	(0.13)
R&D/ Sales$_{[t-1]}$	2.001***	1.877**	1.949***	1.877**
	(2.60)	(2.44)	(2.61)	(2.51)
Industry M&A volume$_{[t-1]}$	−0.211**	−0.212**	−0.209**	−0.209**
	(2.14)	(2.17)	(2.14)	(2.19)
N	25,644	25,644	25,644	25,644
Number of firms	2,581	2,581	2,581	2,581
F-stat (*p*-value)	363.783 (0.0)	369.265 (0.0)	8872.137 (0.0)	8696.713 (0.0)
p-value for AR(1)	0.003	0.002	0.003	0.002
p-value for AR(2)	0.309	0.28	0.332	0.299
Hansen *J* (*p*-value)	6.64 (0.156)	11.03 (0.087)	10.83 (0.119)	10.78 (0.095)
Hansen *J* test diff. *p*-value	--	0.345	-	0.281

ploit the empirical finding that changes in board structure tend to cluster across industries and time. The fourth instrument is motivated by the observation that conglomerate firms are less likely to stagger up because their larger size means that they are less likely to be takeover targets (Cremers et al., 2008).

The coefficients of the GMM system's equation for time variation in firm value are presented in Table 8. [14] While lagged Q is strongly statistically significant, twice-lagged Q is insignificant, which is consistent with twice-lagged endogenous variables being used as valid instruments. Most importantly for our purposes, the coefficients of *Staggered board* across the various specifications are consistently positive, similar in magnitude to those reported in Tables 3 and 4, and strongly statistically significant. If our instruments are valid, this indicates that our main result is robust to incorporating simultaneous dynamic endogeneity and unobservable heterogeneity.

3.6. Changes in the corporate law of Massachusetts

On April 17, 1990, Massachusetts introduced new staggered board rules in Massachusetts State Bill HB 5640. Under the new rules, Massachusetts-incorporated firms are required to have a staggered board, which can only be removed (starting after January 1992) with a board vote or, alternatively, the vote of a qualified supermajority of shareholders.[15] This legislative change produced two relevant effects. First, all firms incorporated in Massachusetts had a staggered board for the mandatory initial period of 18 months. Second, after January 1992, the provision of a staggered board became a "sticky default" or quasi-mandatory rule (Ayres, 2012) for Massachusetts firms, as switching to a unitary board regime was made considerably more difficult by the new rules.[16]

[14] Online Appendix Table A.11 shows the first stage results in Panels A and B, and diagnostic test results in Panel C. Panels D and E show the correlation matrix of the instruments in the level and differenced equations, respectively.

[15] Mass. Ann. Law Sch. 156B, Section 50A (Law Co-op 1996), see Subramanian (2002, p. 1859).

[16] Daines, Li, and Wang (2016) also consider the association between staggered boards and firm value in the context of the 1990 Massachusetts law change, finding results consistent with the inference that staggered boards do not, in general, decrease firm value.

Our analysis focuses on how the value of firms incorporated in Massachusetts changed before versus after this law went into effect, relative to similar firms not incorporated in Massachusetts. We construct a sample of Massachusetts firms with the following procedure. We first identify Massachusetts firms from the January 1989 Compact Disclosure disc and hand-collect data from SEC form DEF 14A, coding their board structure in the year before the new rules went into effect. We then construct a matched sample of Massachusetts firms and control firms from our main data set that are not incorporated in Massachusetts.

A significant limitation is that the number of firms incorporated in Massachusetts is fairly small. For our basic matched sample, we require that the relative difference in Q and log assets between the Massachusetts firm and the control firm is minimized and at most 30% (above or below), as well as that both firms have an identical board structure (i.e., unitary or staggered board). Under these constraints, we obtain a basic sample of 79 firms incorporated in Massachusetts, out of which 55 have a unitary board and 24 have a staggered board. For robustness, we also use a more restrictive matched sample, for which we require that the relative difference in Q and log assets between the Massachusetts and control firms is at most 20% (above or below) and that both the Massachusetts firms and control firms have no missing data in the five-year period (1988–1992) surrounding the new Massachusetts rules (so that we can estimate the change in value over this period more accurately). This restricted matched sample consists of 68 Massachusetts firms, of which 44 have a unitary board and 24 have a staggered board.[17]

The results for pooled panel Q regressions using the basic matched sample are presented in columns 1–4 of Table 9, and using the restricted matched sample in columns 5–8. We regress Tobin's Q on a dummy indicating whether the firm is incorporated in Massachusetts (*MA incorporated*), the interaction of *MA incorporated* with a dummy indicating the period after the law change (i.e., for 1991 and 1992, *Post 1990*), a set of standard controls, as well as year and industry fixed effects using four-digit SIC codes and present the results in column 1. The coefficient of the interaction *MA incorporated* x *Post 1990* equals 0.191 (t-stat. = 1.98). This result indicates that firms that were incorporated in Massachusetts (and thus had a quasi-mandatory staggered board in 1991 and 1992) had a larger firm value than their control firms after the law change. As the average Q in this sample equals 1.53, the coefficient suggests that firms incorporated in Massachusetts increased about 10% in value in the two years following the implementation of the law, relative to their control firms.

[17] Online Appendix Table A.15, Panel C, shows the number of firms at various stages of the sample construction, for both samples. Panel D shows descriptive statistics for the restricted matched sample, which are very similar for the basic matched sample. As shown in Panel E, the Massachusetts firms have economically similar Tobin's Q, size, and profitability as their controls before the change in Massachusetts' corporate law. Statistically, the differences are significant for *CAPX/Assets* and *R&D/Assets*, but these differences seem economically minor, while the difference for Q is statistically significant for the basic matched sample, and insignificant for the restricted matched sample.

We add a triple interaction of *MA incorporated* x *Post 1990* with a dummy indicating that the firm had a staggered board in 1989 (*SB in 1989*). This triple interaction has an insignificant coefficient in column 2, which shows that the law change has a similarly positive association with firm value for firms in Massachusetts with and without a staggered board prior to the new law. This result does not support the notion that forcing a switch to staggered boards generally increases firm value. Rather, it raises the possibility that Massachusetts firms in general—both with and without a staggered board prior to the law—were affected by some contemporaneous effect after 1989 that did not affect the control firms. Therefore, on the one hand, the results in column 2 strongly contradict the view that staggered boards are detrimental to firm value or that having a staggered board is generally a sign of an entrenched board. On the other hand, it does not provide strong evidence that mandating staggered boards on average helps increase firm value.

Next, we employ firm rather than industry fixed effects. The results in columns 3 and 4 remain quite similar, suggesting that they are not driven by unobserved firm characteristics that do not vary much during 1988–1992. For example, the coefficient on *MA incorporated* x *Post 1990* in column 3 equals 0.200 (t-stat. =2.19), which indicates that Massachusetts firms with a mandatory staggered board in 1991 and 1992 increased more in value than their control firms. Finally, the results using the restricted matched sample in columns 5–8 are similar to those for the basic matched sample.

3.7. Other evidence on causality from the literature

In this section, we consider four closely related studies, each addressing the endogeneity problem in the association between firm value and staggered boards in a different way. Overall, the results of these studies are mixed, as two of them report evidence suggesting that staggered boards improve firm value, while the other two indicate the opposite.

Larcker, Ormazabal, and Taylor (2011, p. 432) consider the stock market reaction to 18 events concerning corporate governance regulations in 2007–2009, which they argue collectively "represent an exogenous shock to equilibrium governance practices." They show a negative stock return around proposal announcements for proxy access reform, including a proposal to eliminate staggered boards, observing that their results are "inconsistent with the market viewing the elimination of staggered boards as value increasing. If anything, the results suggest the opposite. The elimination of the option to have a staggered board is value decreasing" (p. 433).

Cunat, Gine and Guadalupe (2012) consider both short-term abnormal returns and long-term changes in performance in a study focusing on close votes on shareholder-sponsored proposals, describing the passing of such proposals as "akin to an independent random event (it is 'locally' exogenous) and therefore uncorrelated with firm characteristics" (p. 1944). Among other results, they show significant long-term performance improvements after shareholders narrowly vote in favor of shareholder-

Table 9
Firm value and staggered boards – Massachusetts incorporated firms.

This table presents results for annual pooled panel Q regressions relating firm value to the enactment of the Massachusetts staggered board law (i.e., Massachusetts State Bill HB5640) in 1990. We include firms incorporated in the state of Massachusetts and their matched control firms of non-Massachusetts incorporated firms in our main database selected as the closest match based on Q, logarithm of asset size, and board structure (staggered or unitary board) as of 1988 and 1989. *MA incorporated* takes the value of one if the company is incorporated in the state of Massachusetts (and zero otherwise), and *Post 1990*, equals one if the observation is after the passage of the Massachusetts staggered board bill, and zero otherwise. *SB in 1989* is an indicator variable that equals one if the firm had a staggered board in 1989. All specifications include time fixed effects, while the regressions for columns 1–2 and 5–6 include industry fixed effects using four-digit SIC code industry groups, and columns 3–4 and 7–8 include firm fixed effects. All columns also include control variables Ln $(Assets)_{[t-1]}$, $ROA_{[t-1]}$, $CAPX/Assets_{[t-1]}$, $R\&D/\ Sales_{[t-1]}$, and $Industry\ M\&A\ volume_{[t-1]}$. The sample includes two years before and two years after the year of the law's passage, i.e., 1988–1992. We use the basic matched sample for columns 1–4, and the restricted matched sample with more stringent matching criteria for columns 5–8. All variables are defined in Table 1. *T*-statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively.

Dep. variable: $Q_{[t]}$								
	Basic (unbalanced) matched sample				Restricted (balanced) matched sample			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MA incorporated x *Post 1990*	0.191**	0.204*	0.200**	0.218*	0.150*	0.150*	0.17**	0.175*
	(1.98)	(1.72)	(2.19)	(1.84)	(1.92)	(1.68)	(2.12)	(1.77)
MA incorporated	−0.064	−0.088			0.130	0.114		
	(0.37)	(0.42)			(0.69)	(0.52)		
SB in 1989 x *MA incorporated* x *Post 1990*		−0.045		−0.056		−0.001		−0.014
		(0.31)		(0.37)		(0.01)		(0.10)
SB in 1989 x *MA incorporated*		0.074				0.045		
		(0.31)				(0.17)		
SB in 1989 x *Post 1990*		−0.047				−0.015		
		(0.27)				(0.08)		
$Ln(Assets)_{[t-1]}$	−0.039	−0.04	−0.015	−0.013	−0.021	−0.021	0.034	0.036
	(0.70)	(0.71)	(0.06)	(0.06)	(0.36)	(0.36)	(0.17)	(0.19)
$ROA_{[t-1]}$	2.011***	2.013***	0.416	0.445	2.344***	2.35***	1.179	1.188
	(3.06)	(3.16)	(0.54)	(0.57)	(3.63)	(3.70)	(1.52)	(1.5)
$CAPX/Assets_{[t-1]}$	0.371	0.344	1.944	1.948	−0.988	−1.024	0.823	0.825
	(0.35)	(0.32)	(1.64)	(1.65)	(1.11)	(1.15)	(0.96)	(0.95)
$R\&D/\ Sales_{[t-1]}$	2.305	2.319	0.303	0.404	4.548	4.478	2.019	2.057
	(0.78)	(0.82)	(0.12)	(0.15)	(1.47)	(1.55)	(1.03)	(1.04)
$Industry\ M\&A\ volume_{[t-1]}$	−0.338	−0.332	−0.184	−0.181	−0.019	−0.015	0.016	0.014
	(0.86)	(0.86)	(0.53)	(0.52)	(0.06)	(0.05)	(0.06)	(0.05)
N	552	552	555	555	452	452	452	452
Adjusted R^2	0.609	0.607	0.816	0.816	0.642	0.639	0.848	0.847
Fixed effects	Year, Industry	Year, Industry	Year, Firm	Year, Firm	Year, Industry	Year, Industry	Year, Firm	Year, Firm

sponsored proposals to reduce antitakeover provisions, including proposals to remove a staggered board. However, they do not separate proposals concerning staggered board removals from those pertaining to the removal of other antitakeover provisions.

In Online Appendix Table A.12, we replicate the main Q results of Cunat et al. (2012), and show that once we separate out proposals to remove different antitakeover measures, we find no statistically significant evidence of increases in Q following narrow shareholder approval of proposals to remove staggered boards. Further, results using close votes on shareholder-sponsored proposals need to be interpreted with caution, for three reasons: (1) these results are arguably driven by relatively few observations,[18] (2) corporations where these votes are close may be different from other firms and this may introduce selection effects when considering the relevance of these results for firms more generally, and (3) shareholder-sponsored proposals are generally not binding, since dismantling a staggered board commonly requires both shareholder and board approval. As a result, the market reaction after close votes of shareholder-sponsored proposals likely reflects market learning about a combination of factors, including the potential repeal of a staggered board, existence of shareholder pressure, and disagreement among shareholders.

Cohen and Wang (2013) study two systematic events related to staggered boards that involved Air Products' attempt to acquire Airgas in 2010. The first is an October 2010 ruling by the Delaware Chancery Court, which temporarily weakened the insulation force of staggered boards. The second is a subsequent ruling by the Delaware Supreme Court in November 2010, which reversed the Chancery Court's decision. Combining the two-day abnormal returns of these events, Cohen and Wang (2013) report evidence (statistically significant at 10%) that is consistent with staggered boards causing a lower firm value. However, while we can replicate the main results as reported in their paper, our replications in Online Appendix Table A.13 suggest that these results are not robust to either en-

[18] Defining "close votes" as shareholder votes of 49–51%, there are 31 close votes for proposals to remove a staggered board (out of 445 total) and 45 for proposals to remove other antitakeover provisions (out of 881 total).

larging the sample by adding more data through hand collection or using different industry fixed effects.[19]

Lastly, Johnson et al. (2015) follow Coates (2001), who argued that the identity of an initial public offering (IPO)'s law firm helps to explain variation in takeover defenses. Relatedly, Johnson et al. employed the identity of an IPO firm's law firm as an instrument for shareholder rights provisions in corporate charters and bylaws, hand-collecting this information for a large set of IPO firms for 1997–2005. Using the IPO law firm instrument, they find that firm value at the IPO is positively related to having a staggered board for firms with stronger stakeholder relationships. While this particular instrument seems applicable only to IPO firms rather than the more mature firms in our sample, it is noteworthy that using a different set of firms and employing a novel instrument yields cross-sectional results that are consistent with our time series results. This is also consistent with Johnson et al. (2016), who find that staggered boards have a positive effect on firm value in young (rather than just IPO) firms with important stakeholder relationships.

3.8. The channels through which staggered boards relate to firm value

An alternative to resolving the endogeneity of board structure is to investigate two channels through which staggered boards could be associated with long-term firm value. First, Stein (1988, 1989) hypothesizes that markets may be myopic, such that shareholders with a short horizon may mistakenly seek to replace management or agree to a suboptimal takeover, especially in firms with significant asymmetric information. In response to this market pressure from short-term investors, management may develop incentives towards myopic investment behavior. Staggered boards could help prevent this market inefficiency by reducing market pressure.

Second, Knoeber (1986), Shleifer and Summers (1988), and Johnson et al. (2015, 2016) consider the bonding hypothesis, which pertains to firms with important relationships with stakeholders such as top employees and large customers. As these stakeholders make firm-specific investments in their relationship with the firm, they risk expropriation if the firm is taken over. In this case, staggered boards could efficiently serve as a commitment device towards more stable stakeholder relationships, lowering the cost of contracting with these stakeholders, and facilitating investments in value-creating projects (see Arlen, 2007; and Cremers, Nair and Peyer, 2008, for further discussion).

In order to test these hypotheses, we present results using four sets of proxies, although the related channels are non-exclusive and difficult to separate empirically.[20] In particular, the first two sets of proxies, namely, for (1) the prevalence of long-term innovation and (2) firms with assets with more asymmetric information or greater complexity, are meant to test both hypotheses simultaneously. The third and fourth sets of proxies, namely, for (3) the likelihood that the firm is subject to market pressure from short-term investors and (4) the importance of particular stakeholder relationships, are chosen to separate the myopic and bonding hypotheses.

At the same time, we acknowledge that the decline in firm value after de-staggering remains challenging to interpret, under either channel. Under the myopic market hypothesis, de-staggering may be due to pressure from outside, myopic investors. Firms with worse prospects could be more likely to give in, such that the subsequent decline in value would then reflect selection rather than causality. Under the bonding channel, it is similarly possible that firms that agree to de-stagger their board signal that the firm expects lower future surpluses from its stakeholder relationships, which would explain the decline in firm value after de-staggering (rather than the de-staggering itself causing the decrease in firm value).

3.8.1. Research and development investments

Under the myopic markets hypothesis, the pressure on management towards myopic investment decisions seems most important for firms with projects that require long-term investments, such as investments in R&D (Bushee, 1998; Chan, Lakonishok and Sougiannis, 2001; Eberhart, Maxwell and Siddique, 2004). As a result, under the myopic markets hypothesis, one would expect firms with significant R&D investments to benefit more from a staggered board. At the same time, the commitment device a staggered board provides toward the firm's stakeholders under the bonding hypothesis could also matter more for such firms. As innovation often requires firm-specific investments by top employees, suppliers, customers, or strategic alliance partners, a staggered board could be useful to prevent the ex post expropriation of the stakeholders' firm-specific investments in firms more engaged in R&D investments. Therefore, our three proxies for the importance of R&D are useful to test both hypotheses: (1) *R&D/Sales*, the intensity of corporate expenditures on research and development activities; (2) *R&D quotient*, a measure of the firm-specific output elasticity of R&D proposed by Knott (2008); and (3) *Ranked patent citation count* from the National Bureau of Economic Research (NBER) U.S. Patent Citations data file, measuring the relative importance of the firm's innovative research assets.

[19] The sensitivity of these results to changing the sample construction and industry fixed effects is perhaps unexpected, as the sample in Cohen and Wang (2013) is fairly small, with 77 treatment and 62 control firms, and considering the noise associated with two-day abnormal returns. Amihud and Stoyanov (2017) find that the Cohen and Wang (2013) results become insignificant after they exclude from the sample stocks with a price below $1, market capitalization below $10 million or firms that are delisted and traded only over-the-counter. On the other hand, Cohen and Wang's (2017) response counters that their results, especially using expanded samples, are robust to the variations in sample and methodology considered by Amihud and Stoyanov (2017).

[20] On the one hand, firms in which specific investments by stakeholders are more important may also be subject to more pressure from myopic investors. For example, stakeholder relationships are more likely to be characterized by asymmetric information, such that investors may be relatively less informed at firms with stronger stakeholder relationships. On the other hand, longer-term projects that are subject to pressure from myopic investors are more likely to require firm-specific investments by stakeholders.

Table 10
Firm value and staggered boards: interactions with investments in innovation.

This table presents the results of a time series analysis as in Table 3 that includes interactions with variables that capture investments in innovation. We include the following control variables in the regression: Ln *(Assets)*$_{[t-1]}$, *Delaware incorporation*$_{[t-1]}$, *ROA*$_{[t-1]}$, *CAPX/Assets*$_{[t-1]}$, and *R&D/Sales*$_{[t-1]}$, and *Industry M&A volume*$_{[t-1]}$, which we do not show for brevity (unless a variable is being interacted with *Staggered board*$_{[t-1]}$). The interacted variables include the following: *R&D/ Sales*$_{[t-1]}$, *Ranked patent citation count*$_{[t-1]}$, and *Research quotient*$_{[t-1]}$. The sample period is 1978–2015, except columns 3 and 4, which sample period is 1978–2007. Estimation is using pooled panel *Tobin's Q*$_{[t]}$ regressions. We include year and firm fixed effects. All interaction and control variables are defined in Table 1. *T*-statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively.

Dep. Variable: $Q_{[t]}$						
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Staggered board$_{[t-1]}$	0.112**	0.051**	0.044	0.041	0.045	0.046
	(2.39)	(2.06)	(1.14)	(1.07)	(0.98)	(1.00)
R&D/ Sales$_{[t-1]}$	0.644	1.274**				
	(1.00)	(2.46)				
Ranked patent citation count$_{[t-1]}$			0.016	0.127**		
			(0.18)	(2.02)		
Research quotient$_{[t-1]}$					0.236	0.601***
					(0.92)	(2.79)
R&D/ Sales$_{t-1}$ * *Staggered board*$_{[t-1]}$	1.202*					
	(1.85)					
Ranked patent citation count$_{[t-1]}$ * *Staggered board*$_{[t-1]}$			0.204*			
			(1.81)			
Research quotient$_{[t-1]}$* *Staggered board*$_{[t-1]}$					0.849**	
					(2.13)	
N	34,476	34,476	17,778	17,778	14,847	14,847
Adjusted R^2	0.713	0.713	0.716	0.716	0.707	0.706

The results for interactions of the *Staggered board* dummy with each of these three proxies in pooled panel Q regressions with year and firm fixed effects plus our standard controls are presented in Table 10. Consistent with both the myopic markets and the bonding channels, these results show that changes in staggered board structures are more strongly related to firm value for firms that are more engaged in research and development, have more patent citations, or have (arguably) more complex operations. The interaction with *R&D/Sales* (column 1 of Table 10) has a positive, statistically, and economically significant coefficient. Firms whose *R&D/Sales* is one standard deviation higher than the mean experience a 4.2% (= 1.202*0.056/1.6) higher level of Q after staggering up relative to firms whose *R&D/Sales* is at the mean.[21] Similarly, firms that have a one standard deviation higher *Ranked patent citation count* have a 2.9% (= 0.204*0.23/1.6) higher Q if they stagger up compared to firms with mean (i.e., very low) patent counts (column 3). Further, the *Research quotient* and *Staggered board* interaction has a positive coefficient of 0.849 with a *t*-stat of 2.13 (column 5), which indicates that firms whose *Research quotient* is a standard deviation higher (than the mean) have a 2.6% (= 0.849*0.049/1.6) higher increase in Q after staggering up, compared to firms with an average *Research quotient*.

[21] We obtain this estimation by multiplying the coefficient of the interacting variable (i.e., 1.202) by the standard deviation of *R&D/Sales* (0.056), divided by the average Q (1.6) in the sample for column 1.

3.8.2. Informational complexity

The second set of proxies that we use to verify the two channels through which staggered boards could be associated with long-term firm value consists of two "catch-all" measures of the complexity of firm operations and asymmetric information (Core, Holthausen and Larcker, 1999; Duru, Wang and Zhao, 2013). These measures include *Firm size* (the log of total revenue) and Ln(*Intangibles assets/Total assets*), where the latter is calculated as the log of one minus the ratio of the book value of plant, property, and equipment (PP&E) over total assets. While both proxies could indicate firms that are more likely to be subject to market pressure from short-term investors, such proxies could also indicate firms where stakeholder relationships are more important. Nevertheless, in general, these proxies are harder to interpret and less directly related to both hypotheses than other proxies we use, and so we mainly show these additional tests as a robustness check.

The results in columns 1–4 of Table 11 indicate that both firm size and intangibility capture an aspect of the heterogeneity in the association between staggered boards and firm value and are broadly supportive of both the myopic markets hypothesis and the bonding hypothesis. For larger firms and firms with more intangible assets, adoptions (removals) of staggered boards are associated with larger increases (decreases) in firm value. Firms with Ln(*Intangible assets/Total assets*) that is one standard deviation higher than the mean present a 3% (= 0.10*0.48/1.6) higher Q if they stagger up relative to firms with average intangible assets (column 1). Firms with *Firm sales* that is one standard deviation higher than the average tend to

Table 11
Firm value and staggered boards: interactions with operational complexity and institutional horizon.

This table presents the results of a time series analysis as in Table 3 that includes interactions with variables that capture operational complexity and the presence of short-term institutional investors. We include the following control variables in the regression: Ln*(Assets)*$_{[t-1]}$, *Delaware incorporation*$_{[t-1]}$, *ROA*$_{[t-1]}$, *CAPX/Assets*$_{[t-1]}$, and *R&D/Sales*$_{[t-1]}$, and *Industry M&A volume*$_{[t-1]}$, which we do not show for brevity. The interacted variables include the following: *Intangible assets/ Total assets*$_{[t]}$, *Firm size*$_{[t]}$, *Institutional holding duration*$_{[t-1]}$, and *Percent transient institutions*$_{[t-1]}$. The sample period is 1978–2015 for columns 1–4 and 1982–2015 for columns 5–8 due to data availability. Estimation is using pooled panel *Tobin's Q*$_{[t]}$ regressions. We include year and firm fixed effects. All interaction and control variables are defined in Table 1. *T*-statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively.

Dep. variable: $Q_{[t]}$								
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Staggered board$_{[t-1]}$	0.064**	0.052**	0.028	0.049**	-0.183	0.093**	0.104***	0.086**
	(2.38)	(2.08)	(1.14)	(1.97)	(−1.29)	(2.37)	(2.61)	(2.34)
Ln(Intangible assets/ Total assets$_{[t-1]}$*)*	0.088**	0.136***						
	(2.38)	(4.32)						
Firm sales$_{[t-1]}$			−0.218***	−0.201***				
			(11.67)	(12.07)				
Institutional holding duration$_{[t-1]}$					−0.045***	−0.024**		
					(3.12)	(2.13)		
Percent transient institutions$_{[t-1]}$							0.468***	0.420***
							(5.91)	(7.41)
Ln(Intangible assets/Total assets$_{[t-1]}$*)* * *Staggered board*$_{[t-1]}$	0.102***							
	(2.77)							
Firm sales$_{[t-1]}$ * *Staggered board*$_{[t-1]}$			0.035**					
			(2.43)					
Institutional holding duration$_{[t-1]}$ * *Staggered board*$_{[t-1]}$					0.040**			
					(2.07)			
Percent transient institutions$_{[t-1]}$ * *Staggered board*$_{[t-1]}$							−0.087	
							(0.96)	
N	34,229	34,229	34,460	34,460	28,298	28,298	29,305	29,305
Adjusted R^2	0.713	0.712	0.711	0.710	0.694	0.693	0.694	0.694

have a 3.3% (= 0.035*1.5/1.6) higher *Q* if they stagger up relative to firms with average size sales (column 3).

3.8.3. Short-term investors

In order to separately test the myopic market hypothesis, we use two proxies that capture the presence of short-term institutional investors, adopting variables that have been related to market pressure on management towards myopic investment decisions. First, *Institutional holding duration* measures the average holding duration (across their entire, aggregate U.S. public equity holdings) for the institutions currently owning the stock, weighted by the amount each institution owns. This proxy was introduced in Cremers and Pareek (2015) and employs SEC Form 13F institutional holdings data from Thomson Reuters. Second, *Percent transient institutions* measures the percentage of institutional ownership that can be classified as transient according to Bushee (1998), i.e., institutions that are both well-diversified and have high turnover.[22]

Using these proxies, Bushee (1998) and Cremers et al. (2016) find that the presence of transient and short-term institutional investors, respectively, is associated with myopic investment behavior, such as temporary cuts in R&D. Accordingly, we use both proxies to test the hypothesis that staggered boards can mitigate market pressure towards myopic investment behavior, examining whether adoptions (removals) of staggered boards are more strongly related to increases (decreases) in value for firms with more short-term or transient institutional investors.

We interact the *Staggered board* dummy with *Institutional holding duration* and *Percent transient Institutions* in pooled panel *Q* regressions with year and firm fixed effects plus our standard controls, with the results reported in columns 5–8 of Table 11, respectively. The interaction of *Staggered board* with *Institutional holding duration* is positive and significant, such that changes in staggered board structure are more strongly related to firm value for firms that have longer-term institutional owners. The interaction coefficient of 0.040 suggests that firms whose *Institutional holding duration* is one standard deviation higher than the mean experience a 2.2% (= 0.04*0.99/1.8) higher level of *Q* after staggering up relative to firms whose *Institutional holding duration* is at the mean.[23] Further, the interaction of *Staggered board* with *Percent transient institutions* is neg-

[22] The 13F data start in 1980, but we require at least two years of holdings reports before calculating an institution's holding duration over the last five years or the percentage of institutional ownership that is transient. The classifications for transient firms are available on Bushee's website. Both variables are winsorized at 1%, which is especially important for *Percent transient institutions*, which is calculated as the ratio of the percentage of stocks held by transient institutions over the percentage of stocks held by institutional investors. As shown in Table 2, the average *Institutional holding duration* equals 6.8 quarters (with fat tails on both sides of its distribution), and the average *Percent transient institutions* equals 22%.

[23] We obtain this estimation by multiplying the coefficient of the interacting variable (i.e., 0.04) by the standard deviation of *Institutional Holding Duration* (0.99), divided by the average *Q* (1.8) in the sample for column 1.

ative and statistically insignificant, showing that there is no evidence that staggered boards matter more for firms with more transient institutions.[24] Therefore, the evidence on the myopic markets hypothesis is mixed, as the results for firms with more R&D investments and more informational complexity are consistent with the hypothesis, while the results on short-term investors are not.

3.8.4. Firm-specific stakeholder relationships

In order to specifically test the bonding hypothesis, we lastly consider four different proxies that aim to capture the importance of stakeholder relationships. The first two proxies are at the firm level and time varying, and are both adopted following Johnson et al. (2015). First, *Large customer* is a proxy for the importance of customers in creating financial value, measured as a dummy that equals one if the firm has at least one customer accounting for 10% or more of its sales according to the historic Compustat Segment tapes (available for 1985–2015). Second, *Strategic alliance* measures whether the corporation has a long-term partnership with another business (Bodnaruk, Massa and Simonov, 2013), and is an indicator variable equal to one if the firm participates in one, and zero otherwise, obtained from the Thomson Reuters SDC M&A database for 1978–2015. We only include strategic alliances with up to three partners (representing 97% of all strategic alliances). Since data prior to 1985 are sparse, we start the *Strategic alliance* sample in 1985.

The third and fourth proxies, *Labor productivity* and *Contract specificity*, are at the industry level with limited and no time variation, respectively. The use of industry-level proxies is a useful complement to firm-level proxies, where selection effects may be more severe. *Labor productivity* comes from the U.S. Bureau of Labor Statistics (using the four-digit SIC code level data) and is available for only a subset of firms' industries. This proxy aims to measure which industries have a higher marginal product of labor and, hence, more firm-specific investments by the employees. *Contract specificity* is borrowed from Nunn (2007), and measures the fraction of inputs (i.e., products and services) that are not sold on an organized exchange or reference priced in a trade publication, and for which the market thus appears less complete.[25] Industries with more contract specificity require more firm-specific investments and commitment by suppliers.

Table 12 presents the results of interacting the *Staggered board* dummy with each of these four stakeholder relationship proxies in pooled panel *Q* regressions with year and firm fixed effects plus our standard controls. These results support the bonding hypothesis, as generally the positive association between staggered boards and firm value is stronger or only apparent for firms where stakeholder relationships appear to be more important.

The interaction between *Staggered board* and *Large customer* has a positive and both statistically and economically significant coefficient equal to 5.3% (0.083/1.6; *t*-stat. =2.80, see column 1). This indicates that decisions to adopt or remove a staggered board are on average associated with substantially larger increases and decreases, respectively, in firm value for firms with a large customer.

The coefficient in column 3 from the *Staggered board* interaction with *Strategic alliance* is 3.4% (i.e., 0.055/1.6; *t*-stat. =1.89, statistically significant at 10%) and shows that the positive association between staggered boards and firm value is nearly twice as strong for firms engaged in a strategic alliance than for firms without such long-term engagement. Next, the results in column 5 show that the interaction between *Staggered board* and *Labor productivity* is also positive and both strongly statistically and economically significant. Economically, the coefficient implies that if a firm is in an industry where *Labor productivity* is a standard deviation above the average, the adoption of a staggered board is associated with a 3.8% greater increase in firm value.[26]

Lastly, the results in column 7 show that the *Staggered board* interaction with *Contract specificity* also has a positive and statistically significant coefficient, which implies that if a firm is in an industry whose *Contract specificity* is a standard deviation above the average, the adoption of a staggered board is associated with a 3.3% (0.41*0.13/1.6, *t*-stat. of 1.93) higher increase in firm value compared to firms in industries with average *Contract specificity*.[27]

3.8.5. Massachusetts firms

In Online Appendix Table A.15, we show the analogous results for all our proxies for the importance of investments in long-term projects such as R&D (in Panel A) and firm-specific relationships with employees, customers, suppliers, and in strategic alliances (in Panel B) for the restricted matched sample of Massachusetts and matched non-Massachusetts firms in 1988–1992 (see Section 3.6). The results are generally consistent with the results in the full sample. In particular, Massachusetts firms that have higher R&D expenses, more patent citation counts, or have strategic alliances increased more in value following the Massachusetts law change that rendered staggered boards quasi-mandatory. The results for the other proxies are sta-

---

[24] We also control for the percentage of institutional ownership and report the results in columns 5–8 of Table 11. The results are similar if we only use firms with at least 50% institutional ownership or the percentage of total transient institutional ownership rather than the percentage of institutional ownership that is transient. We find that both proxies are unrelated to adoptions or removals of staggered boards, and that changes in board structure are unrelated to changes in either proxy (see Online Appendix Table A.14, Panels C and D).

[25] Data for *Contract specificity* are made available at http://scholar.harvard.edu/nunn/pages/data-0 for 1997 and for about a quarter of the industries in our sample (and is set as missing otherwise).

[26] The economic significance of the interacted impact of *Labor productivity* and *Staggered board* on *Q* is calculated by dividing the coefficient of 0.10, times the standard deviation of *Labor productivity* of 0.61, by the sample average *Q* during 1978–2015 of 1.6. Results using the basic, balanced sample are quite similar.

[27] The results in Online Appendix Table A.14, Panels A and B suggest that the nine different proxies for the importance of investments in long-term projects and important stakeholder relationships generally have insignificant, inconsistent or weak associations with the decision to adopt (in Panel A) or remove (in Panel B) a staggered board. This confirms that having a low firm value is what matters most for the decision to adopt a staggered board. Nonetheless, once a firm has staggered up, firms with more long-term investments and stronger stakeholder relationships benefit the most from the decision to stagger up.

Table 12
Firm value and *staggered boards*: interactions of *Staggered board* with proxies for stakeholder commitment.

This table presents results for pooled panel *Q* regressions with firm and year fixed effects that includes interactions with variables that proxy for stakeholder commitment. We include the following control variables in the regression: *Ln (Assets)*$_{[t-1]}$, *Delaware incorporation*$_{[t-1]}$, *ROA*$_{[t-1]}$, *CAPX/Assets*$_{[t-1]}$, *R&D/Sales*$_{[t-1]}$, and *Industry M&A volume*$_{[t-1]}$ which we do not show for brevity (unless a variable is being interacted with *Staggered board*$_{[t-1]}$). The interacted variables include the following: *Large customer*$_{[t-1]}$, *Strategic alliance*$_{[t-1]}$, *Labor productivity*$_{[t-1]}$, and *Contract specificity*$_{[t-1]}$. The sample period is 1985–2015 for columns 1–4 and 1997–2015 for columns 5 and 6. All variables are defined in Table 1. *T*-statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by ***, **, and *, respectively.

Dep. variable: $Q_{[t]}$								
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Staggered board$_{[t-1]}$	0.039	0.066**	0.058*	0.072**	-0.078	0.076**	-0.287*	0.091*
	(1.14)	(2.12)	(1.87)	(2.19)	(1.45)	(2.33)	(1.70)	(1.81)
Large customer$_{[t-1]}$	−0.093***	−0.046***						
	(3.65)	(2.77)						
Strategic alliance$_{[t-1]}$			−0.031	−0.001				
			(1.51)	(0.05)				
Labor productivity$_{[t-1]}$					−0.209***	−0.161***		
					(7.56)	(6.85)		
Contract specificity$_{[t-1]}$							−0.971	−1.006
							(1.49)	(1.55)
Large customer$_{[t-1]}$ * *Staggered board*$_{[t-1]}$	0.083***							
	(2.80)							
Strategic alliance$_{[t-1]}$* *Staggered board*$_{[t-1]}$			0.055*					
			(1.89)					
Labor productivity$_{[t-1]}$ * *Staggered board*$_{[t-1]}$					0.099***			
					(3.74)			
Contract specificity$_{[t-1]}$ * *Staggered board*$_{[t-1]}$							0.411*	
							(1.93)	
Table 3 controls included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	29,107	29,107	27,782	27,782	27,715	27,715	10,478	10,478
Adjusted R^2	0.713	0.713	0.713	0.713	0.714	0.713	0.664	0.664

tistically insignificant, which may be due to relatively little cross-sectional dispersion in this fairly small sample.

4. Conclusion

In this paper, we revisit the association between staggered boards and long-term firm value. Using a panel data set for 1978–2015, we focus on long-term changes in firm value around the adoption (removal) of a staggered board. We find no support for the entrenchment view that staggered boards encourage shirking, empire building, or private benefits extraction, as we uncover no evidence that changes in board structure have a strong or persistently negative association with changes in firm value. Rather, we find that firm value increases (decreases) after firms adopt (remove) a staggered board for certain subsets of firms, namely, those engaged in long-term projects, with important stakeholder relationships, or that are more difficult to value.

These results are most consistent with the bonding hypothesis that staggered boards can serve as an efficient commitment device towards the firm's stakeholders. Laffont and Tirole (1988), Knoeber (1986), Shleifer and Summers (1988), and Johnson et al. (2015, 2016) examine the bonding hypothesis and suggest that takeover defenses such as the staggered board can lower the cost of contracting with the firm's stakeholders and facilitate investments in value-creating long-term projects.

Overall, our results are also consistent with the literature that the relation between board structure and firm value is nuanced and heterogeneous. Our results thus challenge the "one-size-fits-all" policy that favors the annual election of directors—and which is currently supported by many proxy advisory firms and other shareholder advocates—as inconsistent with the empirical evidence on staggered boards.

References

Abadie, A., Imbens, G., 2006. Large sample properties of matching estimators for average treatment effects. Econometrica 74, 235–267.
Adams, R., Hermalin, B.E., Weisbach, M.S., 2010. The role of boards of directors in corporate governance. A conceptual framework and survey. Journal of Economic Literature 48 (1), 58–107.
Ahn, S., Shrestha, K., 2013. The differential effects of classified boards on firm value. Journal of Banking and Finance 37, 3993–4013.
Amihud, Y., Stoyanov, S., 2017. Do staggered boards harm shareholders? Journal of Financial Economics 123, 432–439.
Arellano, M., Bover, O., 1995. Another look at the instrumental variable estimation of error-component models. Journal of Econometrics 68, 29–51.
Arlen, J., 2007. Regulating post-bid embedded defenses: lessons from Oracle versus PeopleSoft. Harvard Negotiation Law Review 12 (1), 71–102.
Ayres, I., 2012. Regulating opt-out: an economic theory of altering rules. Yale Law Journal 121, 2032–2405.
Barry, J.M., Hatfield, J.W., 2012. Pills and partisans: understanding takeover defenses. University of Pennsylvania Law Review 160, 633–713.
Bates, T., Becher, D., Lemmon, M., 2008. Board classification and managerial entrenchment: evidence from the market for corporate control. Journal of Financial Economics 87, 656–677.
Bebchuk, L.A., Cohen, A., 2005. The costs of entrenched boards. Journal of Financial Economics 78, 409–433.
Bebchuk, L.A., Cohen, A., Ferrell, A., 2009. What matters in corporate governance? Review of Financial Studies 22, 783–827.
Blundell, R., Bond, S., 1998. Initial conditions and moment restrictions in dynamic panel data models. Journal of Econometrics 87, 115–143.

Bodnaruk, A., Massa, M., Simonov, A., 2013. Alliances and corporate governance. Journal of Financial Economics 107, 671–693.
Bushee, B., 1998. The influence of institutional investors on myopic R&D investment behavior. The Accounting Review 73, 305–333.
Carhart, M.M., 1997. On persistence in mutual fund performance. Journal of Finance 52, 57–82.
Cen, L., Dasgupta, S., Sen, R., 2016. Discipline or disruption? Stakeholder relationships and the effect of takeover threat. Management Science 62, 2820–2841.
Chan, L.K.C., Lakonishok, J., Sougiannis, T., 2001. The stock market valuation of research and development expenditures. Journal of Finance 56 (6), 2431–2456.
Coates, J.C., 2000. Takeover defenses in the shadow of the pill: a critique of the scientific evidence. Texas Law Review 79 (2), 271–382.
Coates, J.C., 2001. Explaining variation in takeover defenses: blame the lawyers. California Law Review 89, 1301–1421.
Cohen, A., Wang, C., 2013. How do staggered boards affect shareholder value? Evidence from a natural experiment. Journal of Financial Economics 110, 627–641.
Cohen, A., Wang, C., 2017. Staggered boards and shareholder value: a reply to Amihud and Stoyanov. Journal of Financial Economics (in press).
Core, J.E., Holthausen, R.W., Larcker, D.F., 1999. Corporate governance, chief executive officer compensation and firm performance. Journal of Financial Economics 51, 371–406.
Cremers, K.J.M., Ferrell, A., 2014. Thirty years of shareholder rights and firm valuation. Journal of Finance 69 (3), 1167–1196.
Cremers, K.J.M., Nair, V., John, K., 2008. Takeovers and the cross-section of returns. Review of Financial Studies 22, 1409–1445.
Cremers, K.J.M., Nair, V., Peyer, U., 2008. Takeover defenses and competition: the role of stakeholders. Journal of Empirical Legal Studies 5, 791–818.
Cremers, K.J.M., Pareek, A., 2015. Short-term trading and stock return anomalies: momentum, reversal and share issuance. Review of Finance 19 (4), 1649–1701.
Cremers, K.J.M., Pareek, A., Sautner, Z., 2016. Short-term investors, long-term investments, and firm value. University of Notre Dame, Rutgers University and Frankfurt School of Finance and Management Unpublished working paper.
Cunat, V., Gine, M., Guadalupe, M., 2012. The vote is cast: the effect of corporate governance on shareholder value. Journal of Finance 67, 1943–1977.
Daines, R.M., 2001. Does Delaware law improve firm value? Journal of Financial Economics 62, 525–558.
Daines, R.M., Li, S.X., Wang, C., 2016. Can staggered boards improve value? Evidence from the Massachusetts natural experiment. Stanford Law School and Graduate School of Business, University of Southern California and Harvard Business School Unpublished working paper.
Dehejia, R., Wahba, S., 2002. Propensity score-matching for non-experimental causal studies. Review of Economics and Statistics 84, 151–161.
Demsetz, H., Lehn, K., 1985. The structure of corporate ownership: causes and consequences. Journal of Political Economy 93, 1155–1177.
Duru, A., Wang, D., Zhao, Y., 2013. Staggered boards, corporate opacity and firm value. Journal of Banking and Finance 37, 341–360.
Eberhart, A.C., Maxwell, W.F., Siddique, A.R., 2004. An examination of long-term abnormal stock returns and operating performance following R&D increases. Journal of Finance 59, 623–650.
Edmans, A., Goldstein, I., Jiang, W., 2012. The real effects of financial markets: the impact of prices on takeovers. Journal of Finance 67, 933–971.
Faleye, O., 2007. Classified boards, firm value, and managerial entrenchment. Journal of Financial Economics 83, 501–529.
Fama, E., French, K., 1992. The cross-section of expected stock returns. Journal of Finance 47, 427–465.
Ge, W., Tanlu, L., Zhang, J.L., 2016. What are the consequences of board destaggering? Review of Accounting Studies 21, 808–858.
Gompers, P., Ishii, J., Metrick, A., 2003. Corporate governance and equity prices. Quarterly Journal of Economics 118, 107–155.
Greene, W., 2000. Econometric Analysis, Fourth Ed. Prentice Hall, Upper Saddle River, NJ.
Greene, W., 2004. The behavior of the fixed effects estimator in nonlinear models. The Econometrics Journal 7, 98–119.
Guo, R.J., Kruse, T.A., Nohel, T., 2008. Undoing the powerful anti-takeover force of staggered boards. Journal of Corporate Finance 14, 274–288.
Guo, R.J., Kruse, T.A., Nohel, T., 2014. Activism and the shift to annual director elections. Journal of Accounting and Finance 14, 83–103.
Guo, L., Masulis, R., 2015. Board structure and monitoring: new evidence from CEO turnovers. Review of Financial Studies 28, 2770–2811.
Hall, B.H., Jaffe, A.B., Trajtenberg, M., 2002. The NBER patent citation data file: lessons, insights and methodological tools. In: Jaffe, A., Trajtenberg, M. (Eds.), Patents, Citations and Innovations. The MIT Press, Cambridge, MA, pp. 403–460.
Hoechle, D., Schmid, M., Walter, I., Yermack, D., 2012. How much of the diversification can be explained by poor corporate governance. Journal of Financial Economics 103, 41–60.
Jarrell, G.E., Poulsen, A.B., 1987. Shark repellents and stock prices: the effects of antitakeover amendments since 1980. Journal of Financial Economics 19, 127–168.
Jensen, M.C., 1988. Takeovers: their causes and consequences. Journal of Economic Perspectives 2, 21–48.
Jensen, M.C., 1993. The modern industrial revolution, exit, and the failure of internal control systems. Journal of Finance 48, 831–880.
Jenter, D., Kanaan, F., 2015. CEO turnover and relative performance evaluation. Journal of Finance 70 (5), 2155–2184.
Johnson, W., Karpoff, J., Yi, S., 2015. The bonding hypothesis of takeover defenses: evidence from IPO firms. Journal of Financial Economics 117, 307–332.
Johnson, W., Karpoff, J., Yi, S., 2016. The lifecycle effects of firm takeover defenses, Working paper.
Knoeber, C.R., 1986. Golden parachutes, shark repellents, and hostile tender offers. American Economic Review 76, 155–167.
Knott, A.M., 2008. R&D/returns causality: absorptive capacity or organizational IQ. Management Science 54 (12), 2054–2067.
Laffont, J., Tirole, J., 1988. Repeated auctions of incentive contracts, investment and bidding parity, with an application to takeovers. RAND Journal of Economics 19, 516–537.
Lang, L., Stulz, R., 1994. Tobin's Q, corporate diversification, and firm performance. Journal of Political Economy 102, 1248–1280.
Larcker, D., Ormazabal, G., Taylor, D.J., 2011. The market reaction to corporate governance regulation. Journal of Financial Economics 101, 431–448.
Larcker, D., Reiss, P.C., Xiao, Y., 2015. Corporate governance data and measures revisited. Stanford University Graduate School of Business Unpublished working paper.
Mahoney, J.M., Mahoney, J.T., 1993. An empirical investigation of the effect of corporate charter antitakeover amendments on stockholder wealth. Strategic Management Journal 14, 17–32.
Manne, H., 1965. Mergers and the market for corporate control. Journal of Political Economy 75, 110–118.
Masulis, R., Wang, C., Xie, F., 2007. Corporate governance and acquirer returns. Journal of Finance 62, 1851–1859.
Morck, R., Shleifer, A., Vishny, R., 1988. Management ownership and market valuation: an empirical analysis. Journal of Financial Economics 20, 293–315.
Nunn, N., 2007. Relationship-specificity, incomplete contracts, and the pattern of trade. Quarterly Journal of Economics 122, 569–600.
Petersen, M.A., 2009. Estimating standard errors in finance panel data sets: comparing approaches. Review of Financial Studies 22, 435–480.
Roodman, D., 2009. How to do xtabond2: an introduction to difference and system GMM in Stata. The Stata Journal 9, 86–136.
Shleifer, A., Summers, L., 1988. Breach of trust in hostile takeovers. In: Auerbach, A.J. (Ed.), Corporate Takeovers: Causes and Consequences. University of Chicago Press, Chicago, IL, pp. 33–68.
Song, M., Walkling, R., 2000. Abnormal returns to rivals of acquisition targets: a test of the "acquisition probability hypothesis.". Journal of Financial Economics 55, 143–171.
Stein, J., 1988. Takeover threats and managerial myopia. Journal of Political Economy 96 (1), 61–80.
Stein, J.C., 1989. Efficient capital markets, inefficient firms: a model of myopic corporate behavior. The Quarterly Journal of Economics 104, 655–669.
Subramanian, G., 2002. The influence of antitakeover statutes on incorporation choice: evidence of the “race” debate and antitakeover overreaching. University of Pennsylvania Law Review 150 (6), 1795–1873.
Subramanian, G., 2014. Delaware's choice. Delaware Journal of Corporate Law 39, 1–45.
Yermack, D., 1996. Higher market valuation of companies with a small board of directors. Journal of Financial Economics 40, 185–211.
Wintoki, M., Linck, J., Netter, J., 2012. Endogeneity and the dynamics of internal corporate governance. Journal of Financial Economics 105, 581–606.