Can Staggered Boards Improve Value? Evidence from the Massachusetts Natural Experiment

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Abstract

We study the effect of staggered boards on long-run firm value, using a natural experiment: a 1990 law that imposed a staggered board on all firms incorporated in Massachusetts. We find a significant and positive average increase in Tobin's Q among the Massachusetts treated firms, suggesting that staggered boards can be beneficial for early-life-cycle firms, which exhibit greater information asymmetries between insiders and investors. These results are validated using a larger sample of firms from the Investor Responsibility Research Center. In exploring possible channels for these effects, we find that the effects are stronger among innovating Massachusetts firms, particularly those facing greater Wall Street scrutiny. The evidence is consistent with staggered boards improving managers' incentives to make long-term investments.

Keywords: Staggered board; entrenchment; long-termism; Tobin's Q
JEL Classifications: G14, G32, K22

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Working Paper 16-105

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Working Paper 16-105

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1 Introduction

No corporate-governance topic has been more heavily debated in recent years than the effect of staggered (or "classified") boards (SBs). Staggered boards are controversial because they enable directors to resist shareholder attempts to change control of the firm. When a board of directors is staggered, only one-third of the directors are up for re-election in any given year (as is also true of the U.S. Senate). Thus, even if all shareholders want to immediately replace all of the incumbent directors with new directors, they can only oust one-third of the board each year. It thus takes at least two annual meetings for insurgents to win control of a board. Such delays are costly for insurgents, and staggered boards have become the most important source of variation in regulating firms' exposure to the market for corporate control.

Supporters of staggered boards argue that the insulation from shareholder intervention allows directors sufficient time to learn and thus to make better investment and operating decisions. Directors may rationally avoid making potentially valuable investments if they can be ousted (or if the firm can be taken over) before the value of these investments becomes apparent (Stein, 1988, 1989). Because staggered boards delay changes in control and protect the firm from takeovers in the short run (and before the value of some investments is realized), managers can focus on creating long-run value and avoid inefficient short-termism. A staggered board may also improve the firm's bargaining power in the event of a takeover bid: protected by a staggered board, managers can credibly refuse an opportunistic takeover offer; managers might also use this power to elicit a higher offer for shareholders (DeAngelo and Rice, 1983). The insulation provided by staggered boards may also lead to greater real authority for managers, thereby increasing their initiative or their incentive to acquire new information (Aghion and Tirole, 1997).

Opponents argue, by contrast, that staggered boards harm shareholders by insulating directors and managers from the beneficial disciplinary forces of shareholder control, leading to such agency problems as shirking or empire building (a position known as the "entrenchment view") (Manne, 1965). They also argue that staggered boards can be used by self-interested directors and managers to block acquisition attempts (Easterbrook and Fischel, 1981) and may thus deter bids that would benefit shareholders (Grossman and Hart, 1980).

With plausible theoretical arguments on both sides of the debate, the value of staggered boards remains an empirical question. Much of the empirical research over the past decade has supported the entrenchment view. Bebchuk and Cohen (2005) document a strong and negative association between staggered boards and firm value, measured by Tobin's Q. A number of papers support this view; staggered boards are associated with lower market valuation (Guo, Kruse, and Nohel, 2008; Cohen and Wang, 2013), smaller gains to shareholders in completed takeovers (Bebchuk, Coates, and Subramanian, 2002a,b), worse acquisition decisions (Masulis, Wang, and Xie, 2007), and more lax board monitoring (Faleye, 2007).

Consistent with this body of evidence, institutional investors increasingly oppose staggered boards. The Council of Institutional Investors, major institutional investors (e.g., American Funds, BlackRock, CalPERS, Fidelity, TIAA-CREF, and Vanguard), and the two leading proxy advisors, ISS and Glass Lewis, have all adopted voting policies opposing staggered boards. Shareholder activists often press management to abolish the practice of staggered boards and investors typically vote to eliminate them when given the chance: shareholder proposals to de-stagger boards have won more than 80% of votes cast in recent years. Thus the number of Standard & Poor 500 (S&P 500) companies with staggered boards has declined by 80%, from 300 in the year 2000 to 60 in 2013.[1]

[1]According to data collected by the Harvard Shareholder Rights project. See http://srp.law.harvard.edu/index.shtml.

But the debate continues. Supporters of staggered boards mount vigorous defenses,[2] and as of mid-2014 over half of the 3000+ publicly traded companies tracked by FactSet Research Systems' Shark Repellent database still maintained a staggered board structure. From 2000 to 2014, moreover, an increasing proportion of IPO companies went public with a staggered board structure: whereas 44% of IPO firms in 2000 had staggered boards (Daines, 2001), 80% of the IPO firms in 2014 had this structure (WilmerHale, 2015).

The persistence of this debate in spite of empirical evidence stems, in our view, from two shortcomings of the empirical research. First, the research on staggered boards is almost entirely correlational rather than causal. It is therefore possible that the negative correlation between staggered boards and firm value reflects selection rather than causation. Second, relatively little work has been devoted to understanding the possibly heterogeneous effects of staggered boards: that is, staggered boards may be beneficial for some firms even if they are on average harmful.

We contribute to the longstanding debate on staggered boards by providing stronger empirical evidence on the causal effect of staggered boards on firm value. Our identification strategy is based on a policy shock in Massachusetts (MA), where a state law adopted in 1990 (House Bill 5556) compelled the adoption of staggered boards. We construct a quasi-experiment by comparing the value of treated firms (firms that gained a staggered board because of the legislation) to the value of similar control firms from 1984 to 2004.

We also contribute to the literature by providing evidence on the heterogeneous effects of staggered boards. Most of the studies in this area have relied on datasets that cover the largest and most mature public firms (e.g., Bebchuk, Cohen, and Ferrell, 2009; Masulis

[2]See, for example, Wachtell Lipton Rosen & Katz, *Harvard Shareholder Rights Project is Wrong*, March 23, 2012, http://blogs.law.harvard.edu/corpgov/2012/03/23/harvards-shareholder-rights-project-is-wrong/.

et al., 2007; Cremers, Litov, and Sepe, 2016); by contrast, affected MA firms tend to be substantially smaller, younger, and less profitable, and to face a greater degree of information asymmetry. Our study therefore provides evidence on the causal impact of staggered boards on those firms in earlier stages of their life cycles.

Using a difference-in-differences (DID) design, estimates suggest that MA firms forced to adopt staggered boards saw an increase in Tobin's Q of 15.9% over the next 15 years. We find similar effects at the medians. These main results support the view that staggered boards are beneficial to early-life-cycle firms that face greater information asymmetries.

These findings appear robust. Notably, our empirical tests support the validity of the parallel-trends assumption, on which the average treatment effect is identified. In particular, we find no evidence of differential trends in firm value prior to 1990. We also test for the possibility of differential economic trends in MA, by comparing MA firms that already had a staggered board before the law was passed (and were thus unaffected by the law) to their non-MA matched controls. We find no significant effects in Q using this alternative sample, suggesting that our main findings are not attributable to differential trends in the economic environment in MA.

We also find external validation for the hypothesis that staggered boards are beneficial to early-life-cycle firms that face greater information asymmetries by examining the widely-used and larger dataset of the Investor Responsibility Research Center (IRRC). Estimating the traditional cross-sectional Q regressions in this literature (Gompers, Ishii, and Metrick, 2003; Bebchuk and Cohen, 2005; Bebchuk et al., 2009; Bebchuk, Cohen, and Wang, 2013), we find that though the association between Tobin's Q and staggered boards is indeed negative for larger and mature firms, consistent with Bebchuk and Cohen (2005), the association is positive and significant for early-life-cycle firms whose investors face a relatively high degree of information asymmetry.

Our explorations of possible mechanisms suggest that staggered boards allow managers to focus on long-run value. First, in subsample analyses, we find firm-value improvement to be concentrated among innovating firms—young firms or those that invest in R&D. These effects are strongest among innovating firms covered by sell-side analysts and thus particularly subject to Wall Street pressures. Second, this increase in firm value is explained in part by firms' greater willingness to invest for the long run and to innovate. The legislation led to a significant increase in R&D and capital expenditures at firms that were covered by analysts and either innovating or R&D-intensive. Relatively young firms were also more likely to secure patents after they were required to adopt staggered boards. In contrast to the entrenchment view, we do not find evidence that the legislation led to a statistically significant decline in accounting profitability or firm leverage.

The results documented in this paper suggest that the greater insulation afforded by staggered boards is valuable to an important subset of firms, and are consistent with the empirical observation that a large proportion of IPO firms—who are typically younger and face greater information asymmetries—adopt staggered boards. However, we note that our study is unable to resolve the ongoing debate on the effect of staggered boards among the largest and most mature public firms, as our research setting does not provide causal evidence that staggered boards are helpful for the typical large public firm.

The remainder of the paper proceeds as follows. Section 2 explains why staggered boards matter and why they regulate exposure to the market for corporate control. Section 3 examines prior research on staggered boards. Sections 4 details the Massachusetts legislation imposing staggered boards on public firms. Section 5 presents our empirical findings. Finally, Section 6 concludes.

2 Why Staggered Boards Matter

A company has either a unitary or a staggered board structure. Members of unitary boards all stand for election at each annual shareholder meeting; directors on staggered boards, by contrast, belong to separate classes—typically three—whose terms are staggered. Because shareholders vote on only one class of directors (one-third of the board) each year, a change in control requires an insurgent to win a majority of shareholder votes in at least two consecutive annual meetings.

To understand why the staggered board is the most effective commonly-used defense against takeovers, and why it is therefore a focus of debate, one must first understand the poison pill. Though justly famous, the poison pill is a potent device only at firms with staggered boards. Its main effect is to ensure that changes of control occur via elections rather than the sale of shares (Gilson and Schwartz, 2001).

A poison pill is created when a board allows some shareholders to purchase a great deal of newly-issued stock very cheaply in the event that anyone buys a block of shares (typically 10–20%) without managers' prior approval. If the pill is triggered, ownership stake of the bidder is drastically diluted; in the limit, the bidder's initial stake becomes worthless, thus making an acquisition impossibly expensive for unapproved buyers. Thus, no acquirer has ever intentionally triggered a poison pill and, as long as the pill is in place, it is an insurmountable defense against takeover.[3]

Importantly, all public firms either have poison pills or can speedily adopt one whenever necessary, even after an unsolicited bid is announced.[4] Thus a hostile bid can succeed only

[3]In December, 2008 Versata Enterprises triggered Selectica's NOL poison pill. However, this was not part of a takeover contest, but related to a commercial dispute, and did not involve a traditional poison pill designed to deter hostile bids. The Selectica pill was instead designed to protect an NOL asset whose value depended on whether there had been a change of ownership.

[4]See, for example, the famous Unitrin case, in which the Delaware Supreme Court upheld a poison pill

if it can defeat a poison pill. Because pills can only be canceled by the board of directors, a bidder must either persuade incumbent directors to eliminate the pill or instead wage a proxy fight to oust incumbent directors and elect new directors who can quickly remove the poison pill and allow the takeover to proceed. Note that such a new board can also quickly remove any other defenses subject to the board's discretion, such as control-share, fair-price, business-combination, or super-majority provisions (Daines and Klausner, 2001). These other discretionary defenses thus impose no marginal cost, given that a bidder must always replace the board in order to eliminate a poison pill.

In short, because directors can adopt a poison pill at any time, incumbents must be voted out as part of every hostile takeover. The pill makes elections critical: a hostile bidder must place an attractive offer on the table and persuade shareholders to replace incumbents with a slate of directors willing to reconsider the offer and pull the pill. A staggered board lengthens the time necessary to change control of the board and this delay is costly for the bidder, who incurs up-front search and bidding costs. Incumbent managers retain control of the target firm in the interim and may sabotage the bidder's plans by seeking another buyer, selling valued assets, or pursuing incompatible strategies. Consistent with this scenario, Bebchuk et al. (2002b) find that firms with staggered boards are significantly less likely to be taken over.[5]

Thus, when it is easier to remove incumbent directors in a proxy fight—that is, when a company has a unitary board—the company and its managers will be more exposed to the

adopted after a tender offer was initiated.

[5]Under a unitary board structure, incumbent directors and their defenses can be quickly removed—often within four–six weeks. If the shareholders have the power to vote by written consent, such an election can be held in three–four weeks. Otherwise, bidders must distribute and collect proxies, which takes roughly six weeks. Elections can be held at any time during the year if shareholders can either call a special meeting or vote by written consent. If they can do neither, insurgents must await an annual meeting. If a board is staggered, shareholders may not call interim elections or remove incumbent directors except for extreme cases, such as instances of theft, fraud, or gross inefficiency and incompetence (Balotti and Finkelstein, 2008).

market for corporate control. When it is difficult to remove incumbent directors—when the board is staggered—managers will be insulated.

3 Prior Research on Staggered Boards

Considerable recent research has examined how managerial behavior and firm value are affected by governance devices that protect managers from the market for corporate control (e.g., Bertrand and Mullainathan, 2003; Gompers et al., 2003; Bebchuk et al., 2009; Cremers et al., 2016; Bebchuk et al., 2013; Atanassov, 2013). A centerpiece of this research, and a subject of intense ongoing debate, is the value of staggered boards.[6]

Much of the empirical work on this topic appears to support the entrenchment view. Bebchuk and Cohen (2005) documents that staggered boards are associated with lower firm valuations, as measured by Tobin's Q. Consistent with the entrenchment view, Masulis et al. (2007) finds that staggered-board firms tend to make value-decreasing acquisitions; Faleye (2007) finds that staggered boards are associated with lower CEO pay-performance sensitivity and lower CEO performance-turnover sensitivity. Bates, Becher, and Lemmon (2008) finds that staggered boards are associated with higher takeover premiums but lower takeover likelihood; consistent with earlier work, they also document a negative association with firm valuation. Finally, the event studies of Daines (2004) and Cohen and Wang (2013) provide evidence that investors view staggered boards as reducing shareholder value.[7]

[6] A large body of literature have examined the effect on various firm outcomes of insulation from the market for corporate control via state anti-takeover statutes (e.g., Garvey and Hanka, 1999; Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010; Atanassov, 2013). Much of this work has been puzzling to legal academics and corporate lawyers, who argue that these statutes are irrelevant in the presence of poison pills (Catan and Kahan, 2014).

[7] The evidence of Daines (2004), which studies the market reactions to the passage of the Massachusetts legislation examined in this paper, suggests that markets were inefficient with respect to the value-implications of staggered boards in 1990. Consistent with this view, Bebchuk et al. (2013) shows that

Despite this evidence, debate continues to rage—in part, we believe, due to the limitations of existing research evidence. First, with the exception of Daines (2004) and Cohen and Wang (2013), nearly all empirical research on staggered boards is correlational and lacks a clean strategy for identifying causal effects. Second, much of this research has focused on the average effects for the relatively larger and more mature firms that are covered by the Investor Responsibility Research Center (IRRC), and relatively little is known about the heterogeneous effects of staggered boards.[8]

Recent papers challenging the entrenchment view have fueled the further debate. Most notably, the recent work of Cremers et al. (2016) has challenged the well-known cross-sectional results of Bebchuk and Cohen (2005). They find that when firm-fixed effects are introduced into the empirical tests of Bebchuk and Cohen (2005), the association between staggered boards and firm value becomes positive and significant. Their results suggest that de-staggering boards is associated with a decline in Tobin's Q of 6.3%, and the authors argue that the cross-sectional association between firm value and staggered boards reflects the greater tendency of low-value firms to adopt such governance structures (rather than a tendency for staggered boards to cause low value). Though the authors acknowledge a lack of direct causal evidence, they argue that these findings "support the view that staggered boards help to commit shareholders and boards to longer horizons and challenge the manage-

markets learned gradually about the value-implications of insulating governance devices during the decade of the 1990s. The event study of Cohen and Wang (2013) relies on two 2010 Delaware court rulings that affect the strength of staggered boards for a subset of Delaware-incorporated firms. However, their effects are local to a subsample of Delaware firms, which are in general different, e.g., larger in size and higher Q (Daines, 2001), from non-Delaware firms.

[8]Recent work by Ahn and Shrestha (2013) and Duru, Wang, and Zhao (2013) examines heterogeneous effects. The former finds that staggered boards are positively associated with Tobin's Q in firms with low monitoring costs and greater advising needs, whereas the latter finds that the negative impact of staggered boards on firm valuation and accounting performance declines as a firm's opacity increases. Relatedly, Bhojraj, Sengupta, and Zhang (2014) focus their analyses on the G-Index and the E-Index, which measure the degree of insulation provided by firms' governance mechanisms, and argue that innovative firms benefit from such insulation. However, these studies are association-based.

rial entrenchment interpretation that staggered boards are not beneficial to shareholders."[9]

Though this study cannot adjudicate the debate on the causal effect of staggered boards on firm value at the larger and more mature firms covered by Bebchuk and Cohen (2005) and Cremers et al. (2016), we contribute to this body of literature by leveraging a quasi-experimental setting in Massachusetts, described in the next Section. In doing so, we provide causal evidence of the effect of staggered boards on long-run firm value. In particular, because our results apply to the set of affected Massachusetts firms that are early in their life cycles and that face greater information asymmetry, our findings speak to the heterogeneous effects of staggered boards.

4 The Massachusetts Legislation

A large British industrial firm, BTR P.L.C., made a hostile tender offer for the shares of Norton Company, a Massachusetts manufacturer of sandpaper, industrial abrasives, and ceramics, on March 16, 1990. The offer was good news for Norton shareholders: BTR's $75 all-cash offer represented a 50% premium over the share price one month earlier and was well above its 52-week high of $60. Because Norton was protected by a poison pill, BTR also launched a proxy fight to remove Norton's incumbent directors and install its own nominees, who could then (if they chose) dismantle Norton's defenses to consummate the takeover.

Norton's managers and employees, and Massachusetts legislators, were less enthusiastic. Employees and local politicians were mobilized on the grounds that a takeover would prompt

[9] A similar study of the consequences of de-staggering, Ge, Tanlu, and Zhang (2016), finds similar results. In addition to Tobin's Q, the paper finds that de-staggerings are accompanied by declining ROA and R&D investments; they thus challenge the view that destaggered boards are generally optimal and value-increasing. A recent draft of Cremers et al. (2016) finds that MA-incorporated firms experienced higher Tobin's Qs after the 1990 legislation, but does not examine the mechanism or the legislation's heterogeneous effects or the various robustness tests discussed in this paper.

layoffs and reduce the firm's charitable giving. The opposition quickly took on a nationalistic flavor. The *Boston Globe* denounced "a surprise dawn attack on one of the oldest manufacturing concerns in Massachusetts" (*Boston Globe*, March 17, 1990). The *New York Times* reported that Massachusetts Governor Michael Dukakis "compared BTR's tender offer to the British invasion of America during the revolutionary war, explaining that it was 'another attempt by a foreign power to interfere with our ability to shape our own [destiny]'" (*New York Times*, May 27, 1990: 11). Other politicians decried this "second British invasion" and joined Dukakis in vowing to protect the "good, solid Massachusetts company" from being "victimized" or "devoured" by the "the foreign acquiror" (UPI, March 19; *Boston Globe*, April 9). Norton employees even burned the Union Jack at demonstrations outside local government offices (Reuters, April 12: 46); others sang "God Bless America."

Massachusetts politicians also expressed "mounting concern" about foreign takeovers of "critically positioned US companies." (*Financial Times*, April 20: 40) Because Norton also made ceramic parts used in the aerospace industry, they argued, the firm's independence was important to the national security of the U.S.; they petitioned the federal government on national-security grounds to stop the impending takeover.

Facing the prospect of incumbent board members' ouster at the impending annual meeting, Norton managers sought help from the state legislature. With the aid of Wachtell, Lipton, Rosen & Katz, the law firm that had invented the poison pill, Norton managers and their allies proposed a bill that imposed a staggered board on all Massachusetts firms. A staggered board would prevent BTR from gaining a majority of the board seats in the next election, and would give managers additional time to seek alternatives. The bill, MA House Bill 5556, provided that a board, once staggered, could opt out of that structure at its discretion. But that option offered shareholders little advantage. Once protected by a staggered board, directors would have incentives to retain the protection. Moreover, a board's decision to opt out would not be credible because it was reversible: a board that voted to opt out of the staggered board was always free to opt back in later on, even after receiving a hostile bid (as one firm in our sample did). In other words, the implication of the legislation is that firms incorporated in MA are explicitly or implicitly protected by a staggered board, much like the implicit protection by a poison pill enjoyed by companies, irrespective of whether a pill is explicitly in place.

The new law changed the balance of power between shareholders and managers of MA firms. Shareholders were not allowed to vote on the board's initial decision about whether to opt out of the bill's coverage. Moreover, although shareholders could eventually vote to opt out, they were not allowed to do so for two years; even then, they would need a super-majority vote. (We could not find any firms whose shareholders succeeded at opting out.) The measure was decried by institutional investors as "an unprecedented assault on the most fundamental right of shareholders, the right to elect a board to represent their interests" (UPI, April 17). Some commentators even questioned whether the legislation was constitutional (Bainbridge, 1992).

The bill nevertheless was rushed through committees with remarkable speed, in spite of warnings from "New York" investors, as the *Boston Globe* put it, that they would invest in firms in other states if the law passed (*Boston Globe*, April 9). On April 17, in an emergency session attended by only "a handful of representatives," the bill was passed by both the House and Senate (*New York Times*, May 27, 1990: 11). Norton managers had thus secured via lobbying that they could not have won in a shareholder vote.

The next day, in the presence of cheering Norton employees, Governor Dukakis signed the bill and praised the firm's victory in a second "War of Independence" (Reuters, April 19). At the signing ceremony, "Norton chairman John Nelson, who was occasionally close to tears, said he was grateful for the bill because Norton and other state companies will no longer 'be vulnerable to the one-two punch of a simultaneous last-minute tender offer and proxy fight"' (*Boston Globe*, April 19: 49). Less than two weeks after winning the war of independence against foreign powers, Norton managers agreed to an acquisition at a higher price by the French conglomerate Compagnie de Saint-Gobain. (The French apparently posed a less serious threat to national security, and thus had once again helped Massachusetts repel another British invasion.)

This legislation exogenously imposed a staggered board on MA-incorporated firms with unitary (or annually-elected) boards. The next section describes our use of these events as a quasi-experiment, comparing the value of treated firms (MA-incorporated firms without staggered boards prior to the legislation) to that of control firms (non-MA-incorporated firms without staggered boards prior to the legislation), to study the impact of a staggered board.

5 Empirical Results

5.1 Sample Selection and Research Design

To investigate the long-run impact of staggered boards, our main empirical analyses examine the average effect of the legislation on the value of affected firms, i.e., MA-incorporated firms whose boards were staggered due to the state law (treatment firms). To estimate such an effect, we match the affected firms with a set of similar non-MA-incorporated firms without staggered boards (control firms). Our identification strategy relies on the assumption that the choice of where to incorporate—in Massachusetts versus elsewhere—on the part of similar firms in the same industry is unrelated to the effect of staggered boards on firm value and performance.[10]

We first identify a broad set of potential treatment firms by hand collecting MA-incorporated firms with valid observations in the CRSP-Compustat Merged (CCM) database around the date of the legislation. Specifically, we looked for firms with an annual filings both before and after the legislation, and exclude firms that already signed merger agreements or REITs due to their unique governance structure. We require proxies to be available for 1989 or 1990, obtained from either Lexis Nexis or Compact Disclosure, to determine whether a given firm had a staggered board prior to the legislation. This initial hand collection resulted in a potential treatment sample of 67 MA-incorporated firms that did not have staggered boards prior to April of 1990. From this sample, we eliminated 8 firms that have reincorporated since 1990 or for which the most recent incorporation information is unavailable, and we eliminated 1 firm with missing values for total assets, firm age, or book-to-market multiple in 1990. Our final sample consists of 58 treatment firms, for which we obtain all available financial data from CCM from 1984 to 2004.[11] We manually verified that the firms were affected by the legislation, in particular we were unable to find any firms in our sample whose shareholders opted out of the legislation.[12]

We follow similar steps above to identify a set of potential non-MA-incorporated non-staggered control firms: we require them to have valid observations in CCM around the

[10] GICS industry groupings have been shown to explain the cross-sectional variation in stock returns, financial ratios, and valuation multiples better than traditional industry classifications, like the SIC and NAICS codes (Bhojraj, Lee, and Oler, 2003).

[11] Applying these filters to the MA non-treated firms (i.e., those with staggered boards) results in a final sample of 32.

[12] We did find firms whose *boards* opted out of the legislation, but such firms continue to be considered as treated since the boards can opt back in at their discretion. As explained in the prior section, MA-incorporated firms whose shareholders did not opt out of the legislation are either explicitly or implicitly protected by a staggered board, much like the implicit protection afforded by a poison pill, irrespective of whether a pill is explicitly in place. Indeed, we found at least one firm (TCC) whose board originally opted out of staggering, but later opted in when faced with a takeover attempt.

date of legislation, to have proxies available for 1989 or 1990, and to have a valid state of incorporation. We filter out firms with staggered boards in 1990 and firms incorporated in Delaware, whose unique legal environment might lead to a different selection of firms to incorporate there.[13]

From this pool we construct a matched control sample by matching, for each treatment firm, the closest (in Mahalanobis distance) two firms within the same 2-digit Global Industry Classification (GICS2) industry in terms of the following firm characteristics: pre-1990 mean total assets, pre-1990 mean book-to-market ratio, and firm age as of 1990.[14] The resulting control sample consists of 116 non-MA-incorporated non-staggered firms, for which all available financial data are obtained from CCM for the years 1984 to 2004.

5.2 Summary Statistics

Table 1 reports summary statistics on the characteristics—size, age, Tobin's Q, performance, leverage, information asymmetry,[15] and investments—of treated firms and their matched controls during the pre-treatment period, 1984–1990. Columns 1 and 2 report the mean control and treatment firm values respectively; the differences and t-statistics are reported in columns 3 and 4. The treated and matched control firms are statistically indistinguishable from each other at the mean for each of the background characteristics examined. Most notably, the treated and matched control firms are virtually identical in their mean Tobin's Q (1.586 for the matched controls and 1.605 for the treated firms).[16]

[13]In general, firms incorporate either in their home state or in Delaware, and firms selecting Delaware tend to be significantly larger and more likely to engage in M&A transactions (Daines, 2001).

[14]Our main findings are qualitatively similar when matching to the closest GICS2 peer.

[15]We use the Amihud illiquidity ratio as a measure of information asymmetry. This measure is computed over the first three months of 1990 for those firms with at least 2 positive and 2 negative return dates and with at least 10 total valid return observations.

[16]In untabulated results, we also find that the median values for each of these firm characteristics between the control and treated firms are statistically indistinguishable from each other; again, Tobin's Q is virtually

Column 5 reports the percentile ranks relative to the population of firms included in the Investor Responsibility Research Center (IRRC) dataset, the set of firms on which much of the prior work on staggered boards and governance has been based (e.g., Gompers et al., 2003; Bebchuk and Cohen, 2005; Masulis et al., 2007; Bebchuk et al., 2009, 2013; Cremers et al., 2016; Bhojraj et al., 2014). Relative to the IRRC sample of firms in 1990, the average treated firm in our sample is comparatively small and young, faces greater information asymmetry, and is less profitable in terms of ROE and ROA. The average firm has total assets approximately equivalent to the 30^{th} percentile of the IRRC sample, faces information asymmetry greater than 99.8% of the IRRC sample, and is older than only 23% of the IRRC firms. Thus the treatment effects estimated in this study pertain to firms earlier in their life cycles and facing greater information asymmetry than the larger and more mature firms covered by the IRRC.

5.3 The Effect of the Massachusetts Legislation on Tobin's Q

Following prior literature, our primary analyses focus on the impact of staggered boards on Tobin's Q (e.g., Gompers et al., 2003; Bebchuk and Cohen, 2005; Cremers et al., 2016). Figure 1 compares the rolling-three-year averages in the mean Tobin's Q of firms that were affected by the legislation (Treat) and their matched control firms (Control). Consistent with the comparison of pre-period background characteristics in Table 1, Figure 1 shows that our matched control firms capture the pre-period trends of treatment firms in Tobin's Q: the two groups exhibit nearly identical patterns, lending confidence to the (implicit) parallel-trends assumption necessary for inference. After the imposition of staggered boards on MA-incorporated firms in 1990, however, treatment firms exhibit higher mean Tobin's Q

identical among the two groups at the median (1.26 for both the treatment firms and their matched control firms).

values than control firms between 1990 and 2004.

Moving to multivariate regression analysis, Table 2 reports our baseline estimates of the average treatment effects on the MA-treated firms using difference-in-differences (DID) specifications. Column 1 reports a basic specification from pooled OLS regressions of *tobin's q* on a treatment indicator (Treat), a post-legislation indicator (Post), and an interaction of the two variables (Treat x Post). We note that neither the Treat nor the Post variables are significantly different from 0 at the 10% level, suggesting that the treated and control firms do not differ significantly from one another in *tobin's q* pre-treatment, consistent with Table 1, and that there is not a significant post-treatment trend in *tobin's q* among the control firms. We focus on the interaction term, the DID estimator, from columns 1–3, which suggests that the MA treated firms experienced a 16% improvement in Tobin's Q due to the imposition of staggered boards.

In untabulated results, we also investigate the treatment effect on median Tobin's Q, by estimating the DID specifications of Table 2 using median regressions (Koenker and Bassett, 1978). We obtain coefficients on the interaction term—interpreted as the treatment-control difference in the differences between pre- and post-legislation *median* Tobin's Q—that are similar to those of Table 2 both in terms of statistical significance and economic magnitudes. For example, based on the specifications of column 3 and 4, we find that the MA legislation led to an increase in the median of log Tobin's Q by 0.16 and the median of Tobin's Q by 0.22.

We note that the magnitudes of the effects on Tobin's Q that we document are comparable to those of Bebchuk and Cohen (2005), who find, within the sample of IRRC firms, that firms with staggered boards are, all else equal, associated with Tobin's Q levels that are on average 0.21 lower than those of firms without staggered boards. Our OLS and median regression DID estimates suggest that, for the sample of MA treated firms, staggered boards led to an improvement in value of slightly greater magnitudes (0.32 at the mean and 0.22 at the median) but have the opposite sign.

Overall, our main results suggest that among Massachusetts treated firms—early-life-cycle firms that face considerable information asymmetry—the imposition of staggered boards increased firm value. These findings support the argument that, among such firms, staggered boards allow managers to focus on long-run strategy and investments, whose value may not be clear to outsiders.

5.4 Robustness Tests

This section examines the robustness of the main results and inferences reported above. We provide empirical assessments of the internal validity of our findings above and external validation of the conclusions we draw from the Massachusetts quasi-experiment.

5.4.1 Addressing Variations in the Treatment Window

We first assess the stability in the treatment effect on Tobin's Q by considering alternative treatment windows. After passage of the MA legislation, it may have taken some time for firms to adjust their behavior and for market valuations to respond. Furthermore, the legitimacy of poison pills, and thus the antitakeover force of staggered boards, were being cemented in the late 1980s to mid 1990s, with the *Paramount v. Time* decision in 1989 and the *Unitrin v. American General* decision in 1995.

Table 3 compares the baseline DID estimates that use all 1984–2004 data, reported in column 1, to specifications in which we account for different "adjustment periods" by removing the interim years. In columns 2, 3, and 4, we exclude 1990, 1990–1991, and 1990–

1994 data, respectively, from our DID estimation.

We make two observations from these results. First, our main treatment-effect estimates in column 1 are not driven by the years immediately after the adoption of staggered boards, and are relatively stable over time. Second, the DID point estimates increase as our exclusion window expands, consistent with the effect of staggering on firm value being greater over a longer-run horizon.

5.4.2 Assessing Parallel Trends in Q Prior to Treatment

We also assess the likelihood of the implicit assumption of parallel trends between treatment and control firms in Tobin's Q, which is central to identifying the average treatment effect. Though we cannot fully test this assumption because counter-factual outcomes after the policy change are unobservable, we can test for parallel trends in the pre-treatment period between the treated MA-incorporated firms and their matched controls to assess the validity of the quasi-experimental design (Angrist and Pischke, 2008; Lechner, 2011). Differential trends in the pre-treatment period would be inconsistent with the assumption of parallel trends post-treatment.

Table 4, columns 1 and 2, test for differential pre-treatment trends in Q between the treatment firms and and the control firms by including in the main specification (column 1) an additional interaction term between Treat and an indicator for the several years prior to the 1990 legislation. Column 1 uses an indicator for 1989 and 1990, whereas column 2 uses an indicator for the four years from 1987 to 1990. In each case, the interaction term is not statistically significant at conventional levels, suggesting that there are no differential trends in Tobin's Q between treatment and control firms leading up to 1990. These statistical findings are consistent with Figure 1, which shows that our matched control firms exhibit similar average trends in Q prior to 1990.

5.4.3 Addressing Economic Conditions for Massachusetts Firms

Although the treatment and control firms were similar before the legislation, in terms of both the means in background characteristics and the trends in Q, it is still possible that treatment firms became more valuable because of favorable economic conditions for MA firms and not because staggered boards were imposed. To address this possibility, we estimate our main specifications on the sample of MA firms that were not affected by the MA law (i.e., MA firms that already had staggered boards before the 1990 legislation) and on their matched control firms (i.e., non-MA firms staggered in 1990). If the unaffected MA firms also became relatively more valuable over time, this would suggest that the main effects we document above arise from economic conditions (i.e., differential economic trends) for MA firms rather than from the MA legislation.

Table 4, columns 3 and 4, report the results of our tests using *tobin's q* and Tobin's Q, respectively. The DID coefficients of 0.0011 and 0.0422 are not only statistically insignificant, but they are also economically insignificant relative to the Table 2 estimates of 0.1586 and 0.3164. These results suggest that the main results of Table 2 are driven by the imposition of staggered boards and not by economic conditions in MA.

5.4.4 External Validation Using IRRC

To further validate our main findings, we examine the hypothesis that staggered boards could be beneficial for early-life-cycle firms whose investors face greater information asymmetry by using an alternative sample of firms from the IRRC dataset. The advantage of the IRRC is that it offers a much broader sample of firms over time, providing an opportunity to validate our conclusions externally and to test more directly the possible heterogeneous effects of staggered boards.[17] The disadvantage of the IRRC is that, unlike our MA quasi-experimental setting, the variation captured in the data is unlikely to be driven by exogenous shocks. Thus we rely on the traditional pooled cross-sectional regression approaches in the governance literature (Gompers, Ishii, and Metrick, 2010) and include a battery of firm-level controls that could explain both Q and the presence of staggered boards (Bebchuk et al., 2009, 2013): an index of other provisions in the G-Index (Gompers et al., 2003), log of total assets, log of company age, an indicator for Delaware incorporation, percent shares owned by insiders, square of insider ownership, return on assets, capital expenditure to total assets ratio, and R&D to sales ratio.

Table 5, column 1, replicates the main findings of Bebchuk and Cohen (2005), using the sample of IRRC firms from 1990 to 2007 following Bebchuk et al. (2013).[18] We regress Tobin's Q on an indicator for staggered boards (SB), and include firm controls, time-fixed effects, and industry-fixed effects.[19] On average, we find a negative and significant association between Tobin's Q and staggered boards among this sample of relatively large and mature firms.

Having replicated the traditional findings, we proceed to examine whether a subsample of firms in the IRRC that are earlier in their life cycles and whose investors face a relatively high degree of information asymmetry exhibit the same cross-sectional associations. We define as Early-Life-Cycle/High-Asymmetry those firms whose age is less than 6 years old

[17]Each volume of the IRRC dataset covers 1,400 to 2,000 firms. In addition to those firms that belong to the S&P500, firms considered to be important by the IRRC are also covered.

[18]Our construction of the annual cross sections of governance data follows Bebchuk et al. (2013) (See Section 2.1 of their paper.) We also follow them in using IRRC data up to 2007 and in excluding the newer RiskMetrics data because the latter data is not comparable.

[19]We use SIC2 industry codes following Bebchuk et al. (2009), but our findings are similar using the GICS2 industry sectors employed in prior tests.

(the median age of our MA-incorporated firms), whose market capitalization lies in the lower quartile of the cross-sectional distribution, and whose information asymmetry (proxied by the Amihud illiquidity ratio) lies in the upper quartile of the cross-sectional distribution.

Table 5, column 2, estimates the specification of column 1, but includes an indicator for Early-Life-Cycle/High-Asymmetry and an interaction between SB and Early-Life-Cycle/High-Asymmetry. We also include in our set of firm controls an additional interaction term between the index of other provisions in the G-Index with Early-Life-Cycle/High-Asymmetry. The main coefficient on SB in this regression suggests that, among the more mature firms, or larger, or lower-information-asymmetry firms, the association between Tobin's Q remains negative and statistically significant at the 5% level. However, among the set of early-life-cycle firms that face a relatively high degree of information asymmetry, we find a significant positive association between SB and Tobin's Q. Indeed, among such firms the association is 0.2234 (0.3226–0.0992), which is statistically significant at the 10% level, as reported in the last row of the table. For comparability to our main results, Table 5, column 3, repeats the estimation of column 2 but uses *tobin's q* as the dependent variable. These estimates suggest that SBs are associated with 10.69% higher Q among the Early-Life-Cycle/High-Asymmetry firms, whereas SBs are associated with 3.09% lower Q among larger and more mature firms.

To summarize, these results provide external validation of results and conclusions from the MA quasi-experiment. They further suggest that staggered boards have differential effects that relate to firms' life cycles.

5.5 Exploring Possible Mechanisms

This subsection investigates possible channels by which firm value is improved.

5.5.1 Effect on Innovating Firms

Supporters of staggered boards argue that they can encourage innovation or investment, particularly at firms that require a long horizon to execute their strategy and whose outside investors are likely to be less informed about the firm's value. At such firms, a staggered board might allow managers to invest in valuable projects whose value becomes clear to outsiders only in the long run and where success may require tolerance for early failures (Manso, 2011). We therefore examine the differential impact of the MA legislation on a subset of innovating firms, which we define as young firms or firms investing in research and development.

Table 6, column 1, reports the expanded OLS specification in Table 2, column 3—with time- and industry-fixed effects as well as firm controls—for the subsample of innovating treatment firms (with age below the 50^{th} percentile of the 1990 CCM population and with positive R&D expense in 1990) and their matched controls.[20] We find that the baseline positive effects of staggered boards on Tobin's Q are concentrated in the innovating firms, who experienced a 19.7% increase in firm value following the MA legislation. In contrast, we find a negative but statistically insignificant DID coefficient for the subsample of non-innovating firms (column 2).

Column 3 further investigates the subsample of innovating firms covered by analysts. The insulation that staggered boards provide may be more beneficial to firms subject to the pressures and earnings expectations of Wall Street analysts, which critics assert can lead to managerial myopia (e.g., Bhojraj, Hribar, Picconi, and McInnis, 2009; Terry, 2015). Our analysis suggests that the benefits of staggering are strongest at the set of innovating firms that are covered by analysts; such firms experienced a 22.6% increase in Tobin's Q.

[20] In 1990, 32% of the CCM sample reported a positive R&D expense.

In contrast, we find no significant effect on the subset of non-innovating or non-covered treatment firms (column 4).[21]

Overall, our findings are consistent with the idea that staggered boards are valuable for, and therefore much more common among, IPO firms than among mature firms. However, our research setting and results do not speak to the causal effect of staggered boards for those firms later in their life cycles.

5.5.2 Long-Run Investments

We next examine how firms' investments in capital expenditures and research and development were affected by the MA legislation. Table 7 reports DID estimates for *CAPEX* and *R&D* using the entire sample (columns 1 and 3), the subsample of innovating and covered firms (columns 2 and 4), and the subsample of R&D intensive firms (those with R&D expenditure in the top 20 percentile of the CCM population in 1990) and covered firms (columns 3 and 6). Note that we replace missing values in Capital Expenditure and R&D with zeros; in all regression specifications, an indicator variable for missing values in the dependent variable is included.

We find that the MA legislation led to a significant increase in capital expenditures and R&D investments among the subset of innovating or R&D-intensive firms that were most susceptible to the pressures of Wall Street. Regressions of *CAPEX* suggest that the MA legislation led to a 24% increase in capital expenditures among innovating and covered firms, an effect both economically significant and also statistically significant at the 5%

[21] Complementing these findings, in untabulated results we calculate the returns to a zero-investment portfolio that goes long on an equal-weighted portfolio of the MA-incorporated firms that were most affected by the legislation and short on an equal-weighted portfolio of their control firms. Portfolios are rebalanced monthly, and dollar amounts invested in firms that drop out of the sample are reinvested equally in the remaining firms in the portfolio. Consistent with our results on Q, we find that an investment in innovating and covered firms produces a 1,234% return by the end of 2004.

level. Similarly, we report a 22% increase in capital expenditures among R&D intensive and covered firms, a statistically significant effect at the 5% level. Our findings on R&D are less strong statistically; among innovating and covered firms, we find a point estimate of an 8% increase in R&D expenditure, but it is not distinguishable from 0 at the 10% level; among R&D-intensive and covered firms, however, we document a 40% increase in R&D expenditure that is significant at the 5% level.

We also analyze the effect of the legislation on patent generation.[22] Table 8 reports DID estimates for *Patents* using the entire sample (column 1), the subsample of innovating and covered firms (column 2), the subsample of young firms (column 3), and the subsample of young and covered firms (column 4). We show that the MA legislation led to a significant increase in patent generation, in particular among the subset of young firms that face greater market pressure. Columns 3 suggests that the MA legislation led to a 20% increase in *Patents*, among young firms, an effect both economically significant and also statistically significant at the 10% level. These effects are particularly strong for young and covered firms, which saw a 45% increase in *Patents*, an effect that is statistically significant at the 5% level.

Overall, these results suggest that the value increase attributable to staggering can be explained, at least in part, by firms' greater willingness to make investments in growth and innovation. Firms that relied on innovation and that faced analyst pressure experienced significant growth in capital investments, R&D expenditures, and patents. These findings are consistent with the claim that, for firms facing unusually high information asymmetries, Wall Street's scrutiny, and short-run earnings targets, staggered boards afford management

[22]Our patent data are from the Thomson Innovation database, which provides international patent coverage. We collected information on all the U.S. patents that our treatment firms and their matched control firms had applied for between January 1, 1984, and December 31, 2004, that were ultimately granted. The significant gap between 2004 and 2015 alleviates the "truncation problem" encountered by empirical studies that use patent data, namely the inclusion of fewer patent applications towards the end of the sample period due to the time lag between application and approval.

valuable stability and a longer-run horizon for investments (Graham, Harvey, and Rajgopal, 2005; He and Tian, 2013). However, our results do not rule out other explanations for the increase in firm value due to the MA legislation, such as that staggered boards improve value by providing greater independence to outside directors (Ganor, 2014).

We complete this analysis by examining the effect of staggering on operating performance and leverage. Prior studies suggest that insulating governance mechanisms, such as staggered boards, are associated with worse operating performance (e.g., Gompers et al., 2003; Bebchuk et al., 2013). Table 9, columns 1–4, report DID estimates on *roe* and *roa*. In contrast to the earlier papers, we do not find staggered boards to have a significantly negative effect on operating performance. In particular, we find economically meaningful point estimates among the subsample of innovating and covered firms: a 6% increase in gross ROE and a 4% increase in gross ROA. Although the estimates for the most affected firms are marginally significant (at the 15% level), none of the specifications obtains a DID estimate that is statistically distinguishable from zero at the conventional levels.

Finally, in Table 9, columns 5 and 6 we examine the effect on leverage. As above, we find an economically significant positive point estimate for innovating and covered firms—an increase of 4%—but do not find statistical significance. Drawing on the totality of the results presented in the paper, we can conclude at minimum that, for early-life-cycle firms that face a relatively higher degree of information asymmetry, staggered boards do not destroy value by leading managers to live the quiet life, or to take advantage of the job security that is afforded to them, at the expense of shareholders.

6 Conclusion

Staggered boards remain a topic of debate and controversy in corporate governance, fueled in part by the conflicting results of academic research on their effects. Prior research is also hampered by a lack of causal identification and has not fucsed on the heterogeneous effects of staggered boards.

This study exploits a quasi-experimental setting produced by a 1990 law requiring all Massachusetts-incorporated firms to adopt staggered boards. Our evidence suggests that staggered boards can be beneficial for early-life-cycle firms whose investors face a relatively high degree of information asymmetry, resulting in greater firm value in the long run. We find external validation to this hypothesis using the conventional IRRC data. Our evidence also suggests that this value increase is at least in part explained by firms' greater willingness to invest in growth and innovation.

These findings contribute to the academic literature and to the corporate-governance debate by providing plausible causal identification that also allows for examination of staggered boards' heterogeneous effects. We caution, however, that our findings should not be interpreted to suggest that staggered boards unambiguously improve firm value. In particular, our work does not suggest that staggered boards are beneficial to larger and more mature firms (Bebchuk and Cohen, 2005; Cremers et al., 2016).

A staggered board may be valuable at a particular stage in a firm's life cycle. This is consistent with a number of other findings in the corporate governance literature. Very young firms, and those facing severe information asymmetries, are typically funded by venture capital or other private investors (Chan, 1983) and are therefore insulated from the market for corporate control. Investors do not object to staggered boards at this early stage (Daines and Klausner, 2001), and firms typically go public with staggered boards. Some IPO firms even adopt more extreme protection from takeovers (e.g., dual-class shares), though these protections typically phase out as the firm matures and its founders leave the business. Generally speaking, it is only when public firms have matured that investors oppose staggered boards and prefer to rely instead on the market for corporate control. Our evidence suggests that, consistent with these patterns, staggered boards (and insulation from shareholder intervention) are useful to early-life-cycle firms that face more severe information asymmetries, and therefore suggest that they might usefully be paired with sunset provisions that phase out these powerful insulating forces as firms mature.

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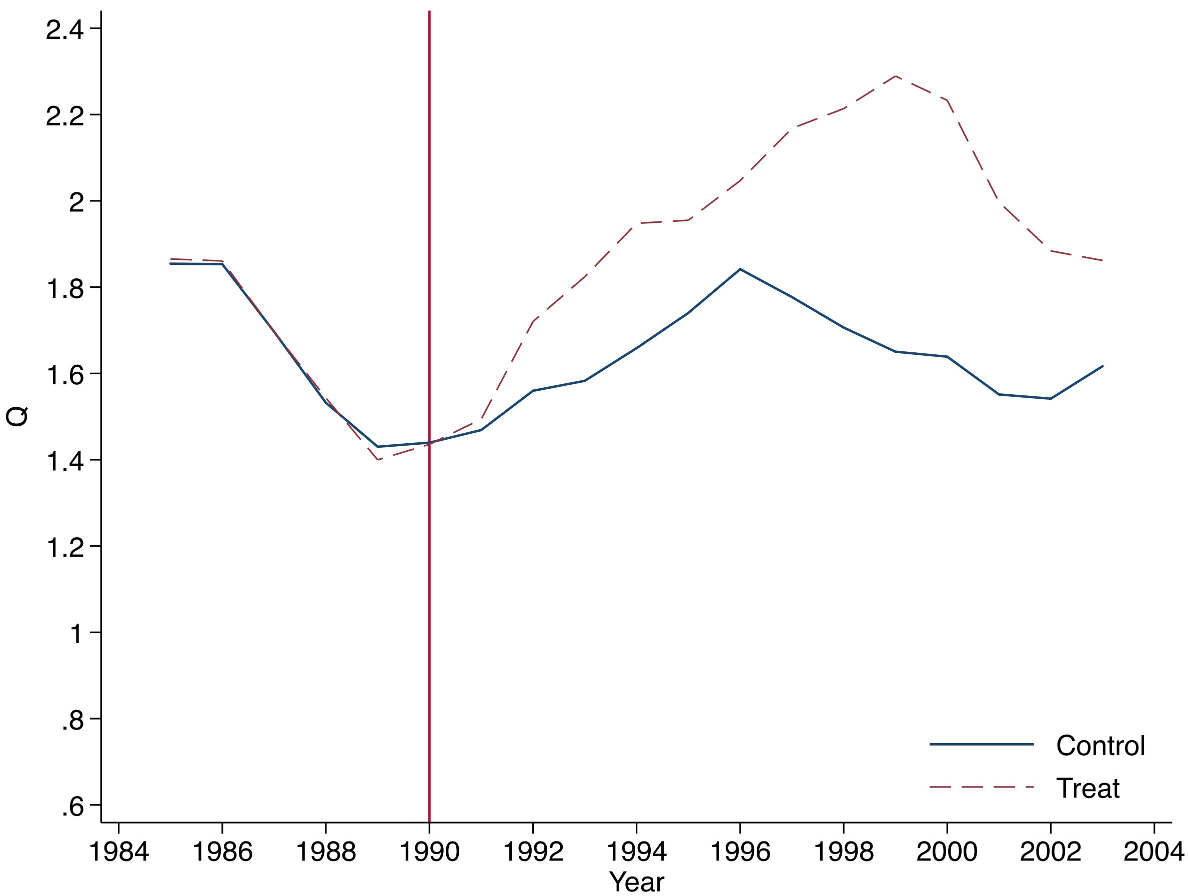

Fig. 1. Tobin's Q between Treatment and Control Firms
This figure compares the rolling-three-year averages in the mean annual Tobin's Q of firms affected by the legislation (Treat) and their matched control firms (Control). The red line indicates the year of the Massachusetts legislation, 1990.

Table A1.

Description of Variables

This table presents definitions of variables used in our regressions. Our financial and corporate data are obtained from the CRSP-Compustat Merged database; Compustat variable names appear in square brackets below; CRSP variable names appear in parentheses. Patent data are from the Thomson Innovation database. We replace the missing values for depreciation, R&D expense, capital expenditure, and number of patent citations with zeros. All dependent variables are truncated at the 1% level.

Variable	Description	Calculation
Dependent Variables		
Tobin's Q		(total assets [at] + price [prcc_c] × commonshare [csho] − equity [ceq] − deferred taxes [txdb]) / assets [at]
tobin's q	Natural Logarithm of Tobin's Q	
ROE	Return on Equity	(operating income before depreciation [oibdp] − depreciation [dp]) / total common equity [ceq]
roe	Natural Logarithm of (1+ ROE)	
ROA	Return on Assets	(operating income before depreciation [oibdp] − depreciation [dp]) / total assets [at]
roa	Natural Logarithm of (1+ ROA)	
Leverage		liabilities [lt] / total assets [at]
leverage	Natural Logarithm of Leverage	
R&D	Research and Development Expense	[xrd]
R&D	Natural Logarithm of (1+R&D)	
CAPEX	Capital Expenditure	[capx]
CAPEX	Natural Logarithm of (1+CAPEX)	
Patents	Number of patents applied for by the firm that were eventually granted	
Patents	Natural Logarithm of (1+Patents)	
Matching and Control Variables		
Book to Market		(equity [ceq] + deferred taxes and investment credit [txditc]) / market cap [prcc_f × csho]
Assets	Total Assets	[at]
assets	Natural Logarithm of Assets	
Age	Firm Age (in years)	Number of years since first observed PERMNO on CRSP
age	Natural Logarithm of Firm Age	
Indicator Variables		
Post	Post-legislation indicator	equals 1 if the fiscal year end occurred after 1990.
Treat	Treatment indicator	equals 1 if the firm was MA-incorporated without a staggered board prior to 1990
Other Variables		
Info Asymmetry	Amihud illiquidity ratio	Daily average of 1000000×|(ret)| / |(prc)|×(vol) from January 1 to March 30 of 1990

Table 1.

Summary Statistics on Matched Sample

This table compares pre-period (from 1984 to 1990) means of the characteristics of control firms (non-MA-incorporated firms without staggered boards in 1990), reported in column (1); the treated firms (MA-incorporated firms without staggered boards in 1990), reported in column (2); their differences, reported in column (3); and the *t*-statistics associated with the differences in means, reported in column (4). *t*-statistics are computed based on cluster-robust standard errors, clustered by firm. Column (5) reports the percentages of firms in the Investor Responsibility Research Center (IRRC) database in 1990 with values lower than the treated sample's pre-period mean.

	(1)	(2)	(3)	(4)	(5)
Firm Characteristics	Control	Treated	Δ	T	Pctile IRRC
Total Assets	328.694	479.682	150.988	0.728	29.6%
Firm Age	10.280	10.006	-0.274	-0.005	22.9%
Tobin's Q	1.586	1.605	0.019	0.155	73.4%
Return on Equity	0.101	0.132	0.031	0.879	25.8%
Return on Assets	0.053	0.061	0.008	0.525	26.2%
Leverage	0.472	0.444	-0.028	-0.850	18.7%
Info Asymmetry	12.395	8.443	-3.952	-0.900	99.8%
R&D Expense	4.309	6.705	2.396	1.589	25.4%
Capital Expenditure	16.939	22.777	5.838	0.694	28.4%

Table 2.

Average Treatment Effect on Tobin's Q

This table reports OLS results of regressing *tobin's q* (columns 1–3) and Tobin's Q (column 4) on a treatment indicator (Treat), a post-legislation indicator (Post), an interaction of the two variables (Treat x Post), and other controls. Columns 1–4 vary depending on whether year and industry fixed effects or firm-level controls are included. The Post indicator is absorbed by time fixed effects and is not reported in such specifications. Columns 3 and 4 include *assets* and *age* as firm controls. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)
Treat x Post	0.1605**	0.1623**	0.1586**	0.3164**
	(0.066)	(0.067)	(0.064)	(0.150)
Treat	-0.0390	-0.0408	-0.0439	-0.0707
	(0.061)	(0.063)	(0.053)	(0.118)
Post	-0.0439			
	(0.057)			
Time FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Firm Controls	No	No	Yes	Yes
Observations	2,552	2,552	2,552	2,552
Adj R^2	0.0090	0.0385	0.2056	0.1553

Table 3.
Stability in Treatment Effect

This table reports OLS results from regressing *tobin's q* on a treatment indicator (Treat), an interaction with a post-legislation indicator (Treat x Post), time- and industry-fixed effects, and *assets* and *age* as firm-level controls. Column 2 excludes 1991, column 3 excludes 1991–1993, and column 4 excludes 1991–1995. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	(1) All Years	(2) Exclude 1990	(3) Exclude 1990-1991	(4) Exclude 1990-1994
Treat x Post	0.1586**	0.1546**	0.1618**	0.1677**
	(0.064)	(0.069)	(0.073)	(0.081)
Treat	-0.0439	-0.0397	-0.0378	-0.0355
	(0.053)	(0.056)	(0.056)	(0.056)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	2,552	2,388	2,231	1,785
Adj R^2	0.2056	0.2047	0.2042	0.2126

Table 4.
Testing the Parallel-Trends Assumption

Columns 1 and 2 of this table report OLS results from regressing *tobin's q* on a treatment indicator (Treat), an interaction with a post-legislation indicator (Treat x Post), time- and industry-fixed effects, and *assets* and *age* as firm-level control, as in column 3 of Table 2. An additional interaction term of Treat with an time indicator for the period 1989–1990 and 1987–1990 are included in columns 1 and 2 to test for differential pre-treatment trends in *tobin's q*.

Columns 3 and 4 of this table report the results of OLS regressions using *tobin's q* and Tobin's Q as dependent variables. The treatment firms are the MA-incorporated firms that already had staggered boards prior to the MA legislation; the control firms are comparable non-MA firms that already had staggered boards prior to the MA legislation. *assets*, *age*, and time- and industry-fixed effects are included as controls. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	Pre-Treatment Trends		Already-Staggered Firms	
	(1)	(2)	(3)	(4)
Treat x Post	0.1470*	0.1760*	0.0011	0.0422
	(0.084)	(0.094)	(0.075)	(0.206)
Treat	-0.0322	-0.0613	-0.0093	0.0261
	(0.068)	(0.081)	(0.061)	(0.123)
Treat x $\mathbb{I}$[1989–1990]	-0.0347			
	(0.067)			
Treat x $\mathbb{I}$[1987–1990]		0.0269		
		(0.076)		
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	2,552	2,552	1,354	1,354
Adj R^2	0.2057	0.2057	0.3925	0.3091

Table 5.
External Validity: IRRC Sample

This table reports pooled OLS regression results of Tobin's Q (columns 1 and 2) and *tobin's q* (column 3) on an indicator for staggered board (SB), an indicator for high information-asymmetry firms (Early-Life-Cycle/High-Asymmetry), and an interaction term (SB x Early-Life-Cycle/High-Asymmetry), firm controls, and time- and industry-fixed effects. Early-Life-Cycle/High-Asymmetry firms are those whose ages are below 6 years, whose market capitalization is in the bottom quartile, and whose Info Asymmetry is in the top quartile of the cross-sectional distribution. Following Bebchuk and Cohen (2005) and Bebchuk et al. (2013), firm controls include an index of other provisions in the G-Index (Gompers et al., 2003), log of total assets, log of company age, an indicator for Delaware incorporation, the percentage of shares owned by insiders, square of inside ownership, return on assets, capital-expenditure-to-total-assets ratio, and R&D-to-sales ratio. An additional interaction term between an index of other provisions in the G-Index and Early-Life-Cycle/High-Asymmetry is also included. The last row reports, for the specifications in columns 2 and 3, the *p*-value of the F-statistic that tests the null hypothesis that the staggered-board coefficient for Early-Life-Cycle/High-Asymmetry firms is 0. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	(1) Tobin's Q	(2) Tobin's Q	(3) *tobin's q*
SB	-0.0933**	-0.0992**	-0.0309**
	(0.039)	(0.041)	(0.013)
Early-Life-Cycle/High-Asymmetry		-0.3401	-0.1028
		(0.229)	(0.097)
SB x Early-Life-Cycle/High-Asymmetry		0.3226**	0.1378**
		(0.132)	(0.059)
Time FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Observations	22,389	22,389	22,389
Adj R^2	0.3399	0.3412	0.4632
F-Test *p*-value	NA	.0658	.0607

Table 6.

Treatment Effect on *tobin's q* for Subsamples

This table shows the results of OLS regressions with *tobin's q* as the dependent variable. Column 1 shows the result for the subsample of innovating treatment firms and their matched control firms; column 2 shows the result for the subsample of non-innovating treatment firms and their matched control firms; column 3 shows the result for the subsample of innovating and covered treatment firms, along with their matched control firms; column 4 shows the result for the subsample of non-innovating and non-covered treatment firms, along with their matched control firms. Innovating treatment firms are those with positive R&D expense or those that are young, i.e., whose age (in 1990) is below the median of the CRSP-Compustat Merged database universe; non-innovating treatment firms are those that are not young and are not incurring R&D expenses. Covered firms are those firms with analyst coverage in at least one of the four quarters prior to the legislation; non-covered firms are those firms without analyst coverage in at least one of the four quarters prior to the legislation. *assets* and *age* are included as firm controls. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)
	Innovating	Non-Innovating	Innovating and Covered	Non-Innovating or Non-Covered
Treat x Post	0.1973***	-0.0218	0.2264**	0.1018
	(0.076)	(0.052)	(0.102)	(0.073)
Treat	-0.0699	0.1165**	-0.0823	-0.0007
	(0.061)	(0.056)	(0.072)	(0.061)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	2,158	394	1,127	1,425
Adj R^2	0.1663	0.4394	0.2198	0.2139

Table 7.

Investments

This table reports the results of OLS regressions using *CAPEX* (columns 1–3) and *R&D* (columns 4–6) as the dependent variables. Column 1 and 4 report the results using the full sample of treatment and matched control firms. Columns 2 and 5 reports results using the subsample of innovating and covered treatment firms and their matched control firms. Column 3 and 6 reports results using the subsample of R&D-intensive and covered treatment firms and their matched control firms. Innovating firms are those with positive R&D expense or those that are young, i.e., whose age (as of 1990) is below the median of the CRSP-Compustat Merged database universe. R&D-intensive firms are those whose R&D expense lies in the top 80^{th} percentile in the 1989 fiscal year. Covered firms are those firms with analyst coverage in at least one of the four quarters prior to the legislation. *assets* and *age* are included as firm controls. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		CAPEX			*R&D*	
	All	Innovating and Covered	R&D-Intensive and Covered	All	Innovating and Covered	R&D-Intensive and Covered
Treat x Post	0.0542	0.2376**	0.2221**	-0.0282	0.0843	0.3970**
	(0.077)	(0.094)	(0.109)	(0.085)	(0.122)	(0.170)
Treat	0.0964	0.0814	0.0812	-0.1050	-0.0171	0.2709
	(0.068)	(0.110)	(0.135)	(0.101)	(0.211)	(0.223)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,569	1,131	713	2,561	1,103	685
Adj R^2	0.8603	0.8555	0.8761	0.6383	0.6218	0.7352

Table 8.
Patents

This table reports the results of OLS regressions using *Patents* as the dependent variables. Column 1 reports the results using the full sample of treatment and matched control firms. Column 2 reports results using the subsample of innovating and covered treatment firms and their matched control firms. Column 3 reports results using the subsample of young treatment firms and their matched control firms. Column 4 report results using the subsample of young and covered firms and their matched control firms. Innovating firms are those with positive R&D expense or those that are young, i.e., whose age (as of 1990) is below the median of the CRSP-Compustat Merged database universe. Covered firms are those firms with analyst coverage in at least one of the four quarters prior to the legislation. *assets* and *age* are included as firm controls. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	(1) All	(2) Innovating and Covered	(3) Young	(4) Young and Covered
Treat x Post	0.0380	0.1672	0.2028*	0.4492**
	(0.089)	(0.179)	(0.117)	(0.198)
Treat	0.1516*	0.2709	0.0997	0.0080
	(0.082)	(0.188)	(0.086)	(0.210)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	2,548	1,080	1,226	571
Adj R^2	0.1637	0.2491	0.1493	0.2682

Table 9.

Operating Performance and Leverage

This table reports the results of OLS regressions with *roe* (columns 1 and 2), *roa* (columns 3 and 4), and *leverage* (columns 5 and 6) as the dependent variables. Columns 1, 3, and 5 report the results using the full sample of treatment and matched control firms. Columns 2, 4, and 6 report the results for the subsample of innovating and covered treatment firms and their matched control firms. Innovating firms are those with positive R&D expense or those that are young, i.e., whose age (in 1990) is below the median of the CRSP-Compustat Merged database universe. Covered firms are those firms with analyst coverage in at least one of the four quarters prior to the legislation. *assets* and *age* are included as firm controls. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year levels and reported in parentheses. Significance levels are indicated by $^{+}$, *, **, *** for 15%, 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	roe		*roa*		*leverage*	
	All	Innovating and Covered	All	Innovating and Covered	All	Innovating and Covered
Treat x Post	0.0222	0.0605^{+}	0.0048	0.0371^{+}	0.0219	0.0435
	(0.032)	(0.039)	(0.018)	(0.025)	(0.065)	(0.093)
Treat	-0.0044	-0.0355	0.0017	-0.0212	-0.0097	-0.0275
	(0.023)	(0.039)	(0.013)	(0.024)	(0.072)	(0.105)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,487	1,092	2,544	1,113	2,577	1,135
Adj R^2	0.1482	0.1807	0.1430	0.1977	0.1799	0.0955

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