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Abstract

Staggered boards (SBs) are one of the most potent common entrenchment devices, and their value effects are considerably debated. We study SBs' effects on firm value, managerial behavior, and investor composition using a quasi-experimental setting: a 1990 law that imposed an SB on all Massachusetts-incorporated firms. The law led to an increase in Tobin's Q, investment in CAPEX and R&D, patents, higher-quality patented innovations, and resulted in higher profitability. These effects are concentrated in innovating firms, especially those facing greater Wall Street scrutiny. An increase in institutional and dedicated investors also accompanied the imposition of SBs, facilitating a longer-term orientation. The evidence suggests SBs can benefit early-life-cycle firms facing high information asymmetries by allowing their managers to focus on long-term investments and innovations.

Keywords: Staggered board; Entrenchment; Life-cycle; Tobin's Q; Investments; Innovation; Profitability; Institutional investors; Investor composition

JEL Classifications: G18, G34, K22

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1. Introduction

Staggered (or "classified") boards (SBs) are one of the most important—some argue the *only* meaningful—common defenses against hostile takeovers since the invention of the poison pill. SBs allow incumbent directors to resist the will of even a majority of shareholders and prevent control changes for several years (Daines and Klausner 2001; Bebchuk et al. 2002b; Klausner 2013; Catan and Kahan 2016). No other common defense provides this level of near-bulletproof protection (Klausner 2018).[1] As Delaware's former Vice-Chancellor Chandler noted, no bidder "has ever successfully...gain[ed] control of a classified board" over the opposition of incumbent management.[2]

Because SBs protect incumbent management teams from shareholder pressure, the impact of SBs is one of the most hotly debated corporate governance topics in recent years. This paper leverages a quasi-experimental setting to study the long-term causal effects of SBs on firm value and how SBs might change managerial behavior or investor composition.

Shareholder activists press management to abolish SBs, and investors typically vote to eliminate them when given a chance. 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 SBs. Shareholder proposals to de-stagger boards have won more than 80% of votes cast in recent years. As a result, the number of Standard & Poor's 500 (S&P 500) companies with an SB has declined by 82% during 2000–2020, from 300 to 54.

Opponents argue that SBs harm shareholders by insulating directors and managers from the disciplinary forces of shareholder control, which leads to agency problems such as shirking and empire building (a position known as "the entrenchment view") (Manne 1965). Self-interested managers can also use SBs to block acquisition attempts that benefit shareholders (Grossman and Hart 1980; Easterbrook and Fischel 1981). A body of empirical research generally supports this view. For example, Bebchuk and Cohen (2005) document a strong and negative association between SBs and firm value, as measured by Tobin's Q. SBs are associated with lower shareholder value (Guo et al. 2008; Cohen and Wang 2013, 2017), smaller gains to shareholders in completed

[1] The exception is dual-class shares, which can provide nearly complete insulation from shareholder pressure. However, they are relatively rare (Gompers et al. 2010).

[2] See *Airgas, Inc. v. Air Products and Chemicals, Inc.*, Delaware Court of Chancery opinion (decided October 8, 2010).

takeovers (Bebchuk et al. 2002a,b), worse acquisition decisions (Masulis et al. 2007), and weaker board monitoring (Faleye 2007).

By contrast, supporters of SBs argue that directors and managers can make better decisions when protected from shareholder oversight and external threats of removal. Directors have better information about the firm's prospects than shareholders, and directors may rationally avoid making valuable investments if they can be ousted (or if the firm can be taken over) before the value of those investments becomes apparent to shareholders (Stein 1988, 1989). Because an SB protects the firm from takeovers in the short run, managers protected by an SB can focus on creating long-run value and avoid inefficient short-termism when the value of investments is not apparent to or well understood by outsiders. Supporters also argue that SBs improve the firm's bargaining power in the event of a takeover bid: protected by an SB, managers can credibly refuse a bid and bargain for more (DeAngelo and Rice 1983). Finally, the insulation provided by SBs may also lead to greater real authority for managers, thus increasing their initiative or incentive to acquire new information (Aghion and Tirole 1997). Consistent with these arguments, recent empirical analyses suggest that SBs may improve firm value (Ge et al. 2016; Cremers et al. 2017).

Given plausible theoretical arguments on both sides of the debate, the impact of SBs remains an open empirical question. In our view, the intense academic and public policy debate about SBs persists in part because current empirical research suffers from three shortcomings. First, research on SBs is almost entirely correlational rather than causal. Therefore, the negative correlation between SBs and firm value may reflect selection rather than causation. Although recent research has begun to address causation (e.g., Cohen and Wang 2013, 2017; Cremers et al. 2017), causal inference in prior research on SBs remains problematic and contested (Catan and Klausner 2017; Amihud et al. 2018). Second, there is limited evidence about the mechanisms through which SBs might affect firm value (e.g., SBs' impact on managerial or shareholder behavior). Third, even if SBs are on average harmful, they may be beneficial for some firms. Indeed, we believe that a more important question for the theoretical debate (and perhaps an equally important question for the policy debate) is not whether SBs are good or bad on average, but rather *for which firms* SBs could be beneficial and *why*.

In this paper, we contribute new and nuanced causal evidence on the effects of SBs. Our identification strategy stems from a policy shock in Massachusetts (MA), where a state law adopted in 1990 exogenously imposed SBs on MA-incorporated firms. This law offers a unique setting to study the long-term effects of an SB.[3] We study the period from 1984 to 1997, and construct a difference-in-differences (DID) design by comparing treated firms to their matched controls—non-MA-incorporated firms that are similar to the matched treatment firm in terms of pre-period total assets, book-to-market ratio, and firm age, and identical to the matched treatment firm in terms of industry sector and SB status as of 1990.

We document three main sets of findings. First, firms that became protected by SBs saw an average increase in Tobin's Q of 14.3% over the next seven years. These findings are robust: They hold when we use alternative measures of firm value (e.g., the book to market ratio or the new measure of Total Q proposed by Peters and Taylor (2017)), control for various time-varying firm or industry-level effects, or include lagged Q or firm-fixed effects. These findings also hold in various pre- and post-treatment windows. We also provide evidence for the validity of the empirical design, particularly the parallel-trends assumption. We show that our main results are not due to contemporaneous trends that differentially impact MA firms.

Second, our results suggest that SBs could benefit early-life-cycle firms that face more severe information asymmetries. We provide out-of-sample validation of this idea by estimating the traditional cross-sectional Q regressions from the literature (Gompers et al. 2003; Bebchuk and Cohen 2005; Bebchuk et al. 2009, 2013) using the Investor Responsibility Research Center (IRRC) data, covering the largest publicly traded firms between 1990 to 2007. We find that the association between Tobin's Q and SBs is indeed positive and significant for early-life-cycle firms facing high levels of asymmetry. However, we also find that the association is negative and significant for mature firms or firms that face lower information asymmetries, consistent with SBs' heterogeneous effects.

Third, we provide evidence on the potential channels through which SBs could improve firm value. We find that managers behaved differently when protected from shareholder scrutiny: after the legislation, managers invested more in capital expenditures and R&D, secured more patents,

[3] Cohen and Wang (2013, 2017) examine the impact of an SB in a quasi-experimental setting. However, their setting does not allow a direct examination of SBs' long-run effects on firm value or the causal mechanisms. They examine two 2010 Delaware court rulings that affect the strength of SBs at a subset of Delaware-incorporated firms: The first court ruling weakened the strength of SBs for some firms, and the second ruling overturned the initial one. Because these court rulings occurred within two months of each other, this setting is natural for event studies (i.e., studies of differential stock reactions to the rulings). However, it does not allow for analyses of the long-term effects of SBs on firm value or managerial behavior (such as a change in investments, innovation, or profitability). Cremers et al. (2017) also utilizes the Massachusetts setting in one of its tests. As we explain below, our study differs in significant ways and provides several novel findings that shed light on the effects of SBs.

produced higher quality innovations (measured by citation-weighted patents, the economic value of patents, or the originality of the patents), and their firms were more profitable. Moreover, in subsample analyses, we find that all of these changes are concentrated at firms that are young or that invest in R&D intensively ("innovating" firms), particularly those innovating firms covered by sell-side analysts and thus particularly subject to Wall Street pressures. Thus, firms that had been subject to the most external scrutiny saw the most improvement. Finally, we show that treated firms experienced a significant increase in the fraction of shares held by institutional investors and "dedicated" institutional shareholders (e.g., shareholders with large and stable ownership blocks, Bushee 2004), who are relatively more patient and who could alleviate myopic pressures on managers (Bushee 1998).

Together, we provide new evidence for *why* and *for whom* SBs could improve firm value. When firms are young, and when investors are less informed about the long-term value of investment projects, SBs allow managers to focus on long-run value by reducing market pressures. These findings are consistent with the empirical observation that a large proportion of IPO firms—which tend to be younger and face greater information asymmetries—go public with SB structures.

Our work contributes to the growing body of work examining the causal effect of SBs. Our causal evidence is timely given recent criticisms about the robustness of prior association studies. For example, Bebchuk and Cohen (2005) find a negative cross-sectional relation between Tobin's Q and SBs. However, Ge et al. (2016) and Cremers et al. (2017) show that this negative association is not robust to the inclusion of firm-fixed effects or estimation in changes. On the other hand, Catan and Klausner (2017) and Amihud et al. (2018) point out that this suggestion that SBs improve firm value relies on identifying the effect of SBs from firms' decisions to de-stagger their boards in the 2000s, which is confounded by factors associated with de-staggering decisions such as the deteriorating trends in Tobin's Q among large firms.

Most closely related to our study is Cremers et al. (2017), which also utilizes the MA legislation as one of its additional analyses on the relation between SBs and firm value. Our results differ in that we provide evidence on the potential mechanisms through which SBs could affect firm value by directly examining how the legislation affected managerial behavior, profitability, and investor composition. Moreover, we find an increase in firm value in treated firms, while Cremers et al. (2017) find an effect in MA-incorporated firms on average but not among the treated firms in particular. Finally, strengthening the credibility of our research design, we test for factors that may undermine the inferences made in Cremers et al. (2017) (e.g., Catan and Klausner 2017; Amihud et al. 2018). In particular, we validate the parallel-trends assumption by showing that: our treatment and matched control samples exhibit covariate balance; there were no differential pre-period trends between treatment and matched control firms; the high growth trends in the 1990s in the information and technology sector do not confound our results; and there was no contemporaneous value increase in the unaffected MA firms relative to their matched controls.

Finally, we contribute to the emerging body of evidence on the heterogeneous effects of SBs. Ge et al. (2016) suggests that SBs are more valuable to firms with advisory needs, whereas Cremers et al. (2017) suggests that SBs benefit firms engaged in innovation and where stakeholder relationships matter more. Using a relatively clean setting, we provide causal evidence that SBs are valuable for an important subset of firms—young firms that face a relatively high degree of information asymmetry. We also provide new evidence about the channels through which SBs may improve value for these firms. Although the study's MA setting does not cleanly identify the causal effect of SBs for the largest mature public firms, our reduced-form cross-sectional regression analyses suggest that SBs are, on average, harmful for these firms, consistent with the event-study evidence of Cohen and Wang (2017).[4]

2. Legal and Institutional Background

This section explains why SBs have become powerful anti-takeover devices and describes the legislation that required firms incorporated in MA to adopt an SB.

Staggered Boards

A staggered board is a board in which directors are divided into classes (typically three), and shareholders vote on one class of directors (1/3 of the board) each year. Board turnover is slow in such a firm. The alternative is to have a board where directors can all be replaced quickly,

[4] By studying the market reactions to two Delaware court rulings that impacted the protective force of SBs, Cohen and Wang (2013) shows that market participants perceive SBs as being detrimental to firm value. Cohen and Wang (2017) further shows that these findings are concentrated in the set of firms with large market capitalizations (e.g., above $1 billion).

as they can in a "unitary" or annually elected board.[5] This distinction in election rules matters for corporate governance and takeovers because it determines how quickly shareholders can oust directors who oppose their will. In an SB, change can be slow: shareholders can replace only 1/3 of the board in the first election, and it takes a second election (and at least another year) for shareholders to control a board majority.

SBs allow incumbent directors to resist shareholders for years if they choose. SBs draw much of their special anti-takeover power from the *poison pill*, which gives the board power to deter unwanted bids. A poison pill provides that if anyone buys a block of shares (typically 10–20%) without the target board's prior approval, other shareholders have the right to purchase a great deal of newly issued stock very cheaply. This dilutes the bidder's ownership stake and renders an unapproved acquisition prohibitively expensive to him. The prospect of this dilution can be a potent defense: no acquirer has ever intentionally triggered a poison pill (Klausner 2018).[6]

However, a pill is powerful only as long as directors are willing to wield its powers. If shareholders vote to oust resisting directors, a newly elected board can pull the pill (or dismantle any other takeover defense) and allow the acquisition to proceed. In an annually elected board, this can happen in a matter of weeks. With an SB, this can take years.

SBs are a powerful takeover defense because they delay board turnover necessary to effectuate takeovers in the presence of the poison pill. This delay is expensive to bidders, who incur up-front search and bidding costs, but will not be able to effectuate the merger until they capture a majority of the board—which is at least another year away. Incumbent managers will 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.

Thus, SBs and poison pills are symbiotic. By itself, the poison pill is not a particularly effective takeover deterrent since a hostile bidder can vote out resisting board members and replace them with new directors who will disarm the poison pill. The poison pill cannot survive the election;

[5] Under a unitary board structure, incumbent directors can be quickly removed—often within four to six weeks. If shareholders can either call a special meeting or vote by written consent, they can oust directors any time during the year. Otherwise, they must wait until the next annual meeting, but can then replace the entire board. By contrast, if a board is staggered, state law prevents shareholders from removing an incumbent director before the end of her term, at least absent proof of theft, fraud, or gross inefficiency and incompetence (Balotti and Finkelstein 2008). Thus, it is substantially more difficult for shareholders of SB firms to oust directors or gain control of the board.

[6] In December 2008, Versata Enterprises triggered Selectica's net operating loss (NOL) poison pill. This move was not part of a takeover contest but rather a commercial dispute. The Selectica pill was designed to protect a NOL asset whose value depended on whether there had been a change of ownership, not to deter hostile bids.

for this reason, a poison pill is poisonous primarily for firms with SBs. Conversely, an SB is not a particularly potent takeover deterrent by itself since a hostile bidder could establish control by acquiring a significant block (e.g., a majority) of shares. However, the pill prevents this by blocking insurgents from accumulating a block large enough to control the board.

Nevertheless, because all boards can freely and quickly adopt a pill without shareholder approval (and thus are implicitly protected by a "shadow" poison pill), what is more important is whether a firm has an SB.[7] For these reasons, since the poison pill, SBs are widely regarded as the most important—and some argue the *only* meaningful—common defense against hostile takeovers, as SBs have provided nearly bulletproof protection against hostile bids (Klausner 2018).

We note that the powerful combination of a (shadow) pill and an SB renders other takeover defenses relatively trivial. Because pills can be quickly adopted by all firms, all bidders must be prepared to vote out the board to remove a pill. If the bidders succeed in replacing the board, the newly elected and bidder-friendly board can quickly eliminate not only the pill, but any other takeover defenses, such as control-share, fair-price, business-combination, and super-majority provisions. These other discretionary defenses thus impose no marginal cost to the bidder (Daines and Klausner 2001).[8] Consistent with this view, we believe that, in the U.S. legal-institutional environment after 1985, SBs provide the best setting for studying the effects of managerial entrenchment.

It is also worth noting that SBs' protections extend beyond hostile takeovers. Ultimately, shareholders have influence because they can elect (and oust) the board. SBs reduce or delay this threat. Although hostile takeovers have declined significantly since the 1980s, SBs continue to protect incumbent managers from shareholder activism. For example, Coffee and Palia (2016) identifies the decline of SBs as one of the main factors behind the rise of shareholder activism

[7] In 1985, the influential Delaware Supreme Court upheld the right of a board to adopt poison pill plans without prior shareholder approval and the right to use these shareholder rights plans to deter unwanted takeover bids. See *Moran v. Household Int'l., Inc., 500 A.2d 1346 (Del.1985)*. Because of Delaware law's influence over other jurisdictions, it was understood that poison pills would be effectively valid for firms incorporated in other states. After the *Moran* decision, most states explicitly validated the use of poison pills, and no states have invalidated their use. See Coates IV (2000) and Catan and Kahan (2016) for a discussion of the "shadow" poison pill protecting all public firms. A board is free to adopt a pill even after receiving a bid. See, for example, the *Unitrin, Inc. v. American General Corp* case, *651 A.2d 1361 (Del.1995)*.

[8] For these reasons, legal scholars have called into question the large body of literature in economics and adjacent fields (e.g., Garvey and Hanka 1999; Bertrand and Mullainathan 2003; Giroud and Mueller 2010; Armstrong et al. 2012; Atanassov 2013) that exploits state-level anti-takeover statutes to study the effects of managerial entrenchment. For example, Klausner (2013) and Catan and Kahan (2016) argue that these statutes are not economically important and that prior findings likely result from misspecified econometric models. Notwithstanding these theoretical arguments, whether and which state-level statutes offer protective powers remain an open question for which recent empirical analyses provide mixed evidence (Cain et al. 2017; Karpoff and Wittry 2018).

in large public companies: the absence of SBs increases the threat of proxy fights and managerial replacement. Consistent with these arguments, Shin (2016) documents that SBs lower the likelihood of being targeted by shareholder activists by 13%. By limiting shareholders' ability to replace board members, SBs make activists' threats less credible. Thus, the protections afforded by SBs remain relevant today.

The Massachusetts Legislation

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

Norton's managers and employees, and Massachusetts legislators, were less enthusiastic. Norton managers mobilized employees and local politicians with claims that a takeover could prompt layoffs and cuts in R&D spending or reduce the firm's charitable giving. The opposition even 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 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 U.S. national security; they petitioned the federal government, on national-security grounds, to stop the impending takeover.

Fearing the prospect that shareholders would oust incumbent directors at the upcoming 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's managers and their allies proposed legislation (MA House Bill 5556) that would impose SBs on all Massachusetts firms. An SB would prevent BTR from gaining a majority of the board seats in the next election and give managers additional time to seek alternatives. Because the proposed law would change the balance of power between shareholders and managers at MA-incorporated firms, it 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).

Despite warnings from so-called "New York" investors that they would invest in firms in other states if the law passed (*Boston Globe*, April 9), the bill was rushed through committees with remarkable speed. On April 17, in an emergency session attended by only "a handful of representatives," the bill was passed by both the House and the Senate (*New York Times*, May 27, 1990: 11). Norton managers had thus secured, via lobbying, a takeover defense that shareholders would not have granted. 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 a war of independence against a foreign power, 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 once again helped Massachusetts repel a British invasion.

This legislation exogenously imposed SBs on MA-incorporated firms with unitary boards. The legislation technically allowed a board to opt out of the protection. However, that decision was easily reversible: a board that voted to opt out of an SB was always free to opt back in later on, even after receiving a hostile bid (as one firm in our sample did).[9] The legislation also prevented shareholders from effectively opting out. No shareholder vote was allowed for two years and even then required a super-majority approval for an opt out. Moreover, the shareholder vote itself could be easily avoided by directors who wanted to: a board could simply decide to opt out on their own, thus retaining their right to opt back in. Indeed, we found no firms whose *shareholders* succeeded at opting out. Thus, after the legislation, directors of MA firms were either explicitly protected by an SB or had the option of adopting one whenever needed.[10]

The MA law provides a unique setting to study the long-term effects of SBs, something other quasi-experimental settings leveraged by SB studies have been unable to do (e.g., Cohen and Wang 2013, 2017).[11]

3. Empirical Results

This section details the research design and sample selection. It then describes the main results on Tobin's Q and the analyses of potential mechanisms, in terms of changes in managerial and shareholder behavior, through which SBs can impact firm value.

Sample Selection and Research Design

Our primary empirical analyses examine the legislation's long-term effect on the value of affected ("treated") firms, i.e., MA-incorporated firms whose boards were staggered due to the state law. To estimate such an effect, we match the affected firms with a set of similar non-MA-incorporated firms without SBs ("control" firms).

We first identified 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 legislation. Specifically, we looked for firms with annual filings before and after the legislation. We

[9]The board of one firm in our sample (TCC) initially opted out of the legislation, but later opted back in when faced with a takeover attempt. For a more recent example, see https://dealbook.nytimes.com/2010/11/10/behind-sanofis-letter-to-genzyme/.

[10]An MA firm's shareholders do not have the ability to easily amend its SB status on their initiative in one election, as they would in the case of a bylaw-based SB. Thus, the statutory SBs impose a mandatory two-election delay on change of control transactions similar to that imposed by SBs found in a charter.

[11]Kim (2015) studies market reactions to the mandated adoption of SBs in Indiana (2009), Oklahoma (2010), and Iowa (2011). In principle, these states provide alternative settings in which we could study long-run effects as well. However, as Oklahoma reversed its decision in 2012, and there are a very small number of affected firms in Indiana (18) and Iowa (4), we expect these settings to be less powerful.

excluded firms that had already signed merger agreements and REITs due to their unique governance structure. We required proxies to be available for 1989 or 1990, obtained from either Lexis Nexis or Compact Disclosure, to determine whether a given firm had an SB before the legislation. This initial hand collection resulted in a potential treatment sample of 67 MA-incorporated firms that did not have SBs before April 1990. From this sample, we eliminated five firms that had re-incorporated by 1997 (the end of our sample period) or for which the most recent incorporation information is unavailable.[12] we also eliminated one firm with missing values for total assets. Finally, we eliminated four firms that had dual-class shares, resulting in a final sample of 57 treatment firms, for which we obtained from CCM all available financial data for the period 1984-1997.

We followed similar steps to identify a set of potential non-MA-incorporated non-staggered control firms: we required them to have valid observations in CCM around the legislation, to have proxies available for 1989 or 1990, and to have a valid state of incorporation. We filtered out firms with SBs in 1990 and firms incorporated in Delaware, whose unique legal environment might prompt a different selection of firms to incorporate there. For example, Daines (2001) shows that firms generally incorporate either in their home region or in Delaware, and firms selecting Delaware tend to be significantly larger and more likely to engage in M&A transactions. Thus, identification strategy relies on the assumption that a firm's decision to incorporate in the home region (typically predetermined years earlier) is unrelated to the effect of an SB.

From this pool, we matched each treatment firm to the two firms within its 2-digit Global Industry Classification (GICS2) industry sector that are closest (in Mahalanobis distance) 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. The resulting control sample consists of 114 matches, i.e., non-MA-incorporated non-staggered firms, for which we obtained from CCM all available financial data for the years 1984 to 1997. This constitutes the primary sample for our analysis. For our robustness tests, we also identified a set of 33 MA-incorporated non-treated firms (i.e., non-REIT firms that adopted SBs or had dual-class shares before 1990). We matched each of these firms to the two closest control firms, following the same procedure as before.

Matching to similar non-MA control firms offers several significant advantages compared to, for example, using already-staggered firms as controls. By matching to firms within the same industry,

[12] Our results are robust to the inclusion of the firms that re-incorporated away from Massachusetts.

we can better control for industry trends. Exact industry matching is also important because the industry distribution of the affected MA-incorporated firms is notably different from that of the unaffected MA-incorporated firms. For example, whereas about 50% of the affected MA firms are in the information and technology (IT) sector (see Table 1 panel A for the industry distribution of the treatment firms), only 24% of the already-staggered MA firms come from IT. In contrast, a relatively higher percentage of the already-staggered firms come from the utilities sector (21% vs. 7%). Another advantage of our matching strategy is that it enables us to construct a larger sample of controls to ensure greater statistical power.

We verified manually that the treatment firms were affected by the legislation and found no firms in our sample whose *shareholders* opted out of the legislation. Among the treatment firms, 16 firms' *boards* opted out of the legislation (28% of total). As explained in the prior section, we consider these firms as treated because the legislation allowed their boards to opt back in at their discretion so that these firms were *implicitly* protected by SBs. We also checked the attrition rates of treatment firms in our sample. By the end of 1997, 9 firms (16% of total) were no longer publicly traded, either due to acquisitions (6) or de-listings (3). Our robustness tests will examine the impact of excluding opt-outs and the sensitivity to survivorship.

We also checked whether the matched control observations remained un-treated. For example, we did not find any firms whose treatment status changed because it re-incorporated to Massachusetts. This is not surprising: re-incorporation decisions require a shareholder vote. However, a non-MA firm can also adopt an SB subject to shareholders' approval through a vote. Thus, there is no marginal advantage for a firm to re-incorporate in Massachusetts *in order to* adopt an SB. If shareholders deem an SB beneficial, they can vote to approve such a change without changing the state of incorporation.[13] Also, among the matched control firms for which we were able to track down the history of SB status, only one adopted an SB by the end of our sample period. Removing this observation does not change our inferences.

[13]Relatedly, no control firms could have obtained an SB during our sample period due to their state of incorporation adopting similar legislation.

Summary Statistics

Table 1 reports summary statistics on the affected MA-incorporated firms and the matched control sample. Panel A reports the industry sector distribution of the treatment firms. As noted in the prior section, about 50% of our sample consists of firms in the IT sector. The next largest sectors are industrials, accounting for 18% of the sample, and consumer discretionary, accounting for 12% of the sample. Compared to the sample of firms covered in the 1990 volume of the Investor Responsibility Research Center (IRRC), the standard database used in corporate governance and SB research covering the largest publicly traded firms (e.g., Bebchuk and Cohen 2005; Masulis et al. 2007; Gompers et al. 2010; Bebchuk et al. 2009, 2013), our MA treatment sample is skewed towards the IT and health care sectors, has similar representation in the industrial and consumer discretionary sectors, and has relatively small representation in the other sectors (i.e., energy, materials, consumer staples, financials, and telecommunications).

Panel B reports a comparison of firm characteristics—size (total assets), age, book-to-market, Tobin's Q, ROA, leverage, a proxy for information asymmetry[14], and investments—between treated firms and their matched controls during the pre-treatment period, 1984–1990. Columns 1 and 2 report the mean values of control and treatment firms. Columns 3 and 5 report the mean differences and their t-statistics. Overall, the treated and matched control firms are statistically indistinguishable at the mean for each of the background characteristics examined. Most notably, the treated and matched control firms are virtually identical in their pre-period mean Tobin's Q (1.571 versus 1.624).

Relative to the IRRC sample of firms in 1990, the average treated firm in our sample is small and young, faces greater information asymmetry, and is less profitable in terms of ROA. Column 6 reports the percentile ranks of the treated-firm pre-period means relative to the distribution of IRRC firms. The average treated firm has total assets approximately equivalent to the 21^{st} percentile of the IRRC sample, faces information asymmetry greater than 99.8% of the IRRC sample, and is older than only 22% 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

[14] We use the Amihud illiquidity ratio as a measure of information asymmetry. This measure (*Info Asymmetry*) is the absolute value of the daily return-to-volume ratio and is computed over the first three months of 1990 for those firms with at least two positive and two negative return dates and with at least 10 total valid return observations.

more mature firms covered by the IRRC.

The Effect of the Massachusetts Legislation on Tobin's Q

Figure 1 compares the rolling-three-year averages of 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 the treated and our matched control firms exhibit very similar trends in Q before the legislation, lending credence to the parallel-trends assumption necessary for inference. However, after the 1990 legislation, treatment firms have higher Tobin's Q values than control firms, a difference that grows and stabilizes by the mid-1990s.

Moving to multivariate analysis, Table 2 reports our baseline OLS estimates of the average treatment effects on the treated firms using DID specifications. The dependent variable of interest is log Tobin's Q (*tobin's q*), which Amihud et al. (2017) argues is desirable due to the strong positive skewness in Tobin's Q. Moreover, this choice facilitates the interpretation of our estimated effects in terms of percent change in Q. Since we are exploiting a state-level legal change, we cluster standard errors at the state level to account for arbitrary time-series correlation within states (Bertrand et al. 2004) and at the year level to account for across-firm contemporaneous correlation. Our results are similar when clustered at the firm and year levels.

Finally, our main empirical tests focus on the 14 years surrounding the 1990 MA legislation: seven years in the pre-treatment period (1984–1990) and seven years in the post-treatment period (1991–1997). We analyze a seven-year post-legislation period for several reasons. First, we are interested in the long-run effects of SBs on firm value. Second, it may have taken some time for managers to adjust their behavior after the passage of the legislation and for market values to respond. Third, we wanted to avoid examining a window that ends at the height of the dot-com bubble (e.g., a ten-year post-legislation period), which may be correlated with the relationship between firm value and governance characteristics (Core et al. 2006). Given the concentration of IT sector firms in our sample, we conservatively chose 1997 as the end of our sample period in our main tests. Nevertheless, our robustness tests examine how the DID estimates are affected by different windows.

Column 1, Table 2, 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* $\times$ *Post*). We note that neither the coefficient on *Treat* nor *Post* differs significantly from 0 at the 10% level. Thus, treated and control firms do not differ significantly from one another in *tobin's q* during the pre-treatment period, consistent with Table 1, and that there is no significant post-treatment trend in *tobin's q* among the control firms. Therefore any effects we capture in the DID estimator—the coefficient on *Treat* $\times$ *Post*—must be driven by changes among the treated firms in the post period.

Columns 2, 3, and 4 incrementally include control variables in estimating more robust versions of the baseline DID specification. Column 2 includes linear controls for one-year-lagged firm characteristics, following Catan and Klausner (2017), to account for the possibility that our matching algorithm does not adequately capture the treatment-control sample differences that could explain the subsequent variation in Q. Our controls include firm age (*age*) and one-year-lagged values of sales (*sales*), assets (*assets*), roa (*roa*), leverage (*leverage*), R&D expense scaled by assets (*r&d-to-assets*), and capital expenditure scaled by assets (*capex-to-assets*). Column 3 incrementally includes time-fixed effects, thus dropping the post-period indicator (*Post*). Finally, column 4 includes GICS2 industry-fixed effects.

Most notably, the *Treat* $\times$ *Post* coefficient remains similar in magnitude and statistical significance across these specifications. By contrast, the adjusted R^2s are increasing from 0.83% in the baseline DID specification (column 1) to 21.31% in the full specification (column 4). As Oster (2019) suggests, these patterns alleviate potential concerns about omitted variable biases in our research design and lend credence to our quasi-experimental setting.[15] Interpreting the coefficient of our main specification (column 4), the imposition of SBs led to an on-average 14.3% improvement in Tobin's Q. This effect is not only economically significant but also statistically significant at the

[15] Prior studies have also included the following as control variables: the contemporaneous values of return on assets, capital expenditures, and investments in research and development. However, as noted by Catan and Klausner (2017), such measures are more appropriately regarded as outcome variables. Thus lagged values are more appropriate as controls and are included in our standard specifications. Nevertheless, our main results are robust to controlling for the contemporaneous values of these variables.

1% level.[16]

Overall, our main results suggest that among the average treated firm— early-life-cycle firm that faces considerable information asymmetry—the imposition of SBs increased firm value. One possibility is that, among such firms, SBs allow managers to focus on long-run strategy and certain investments whose value may not be apparent to outsiders.

Robustness Tests

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

Varying the Model Specification

Table 3 reports the results of several robustness tests by varying the primary model specification reported in Table 2, column 4. In panel A, Table 3, we begin by examining our main results' sensitivity to the quasi-experiment study window. In columns 1, 2, and 3 of the panel, we consider shorter windows around the legislation: 1989 to 1992, 1988 to 1993, and 1986 to 1995. In all of these cases, we continue to find a positive and significant treatment effect on *tobin's q*, with larger point estimates when we use longer windows.

That we obtain similar results using narrower windows suggests that our findings are not driven by differential sample attrition. For example, 84% of the treated MA firms remained by the end of 1997 compared to 75% for control firms. These attrition concerns are mitigated in the narrow window analyses, where attrition has minimal impact on the sample and results. Similarly, we find that estimating the primary Q regression using a sample that excludes those treated firms (and their matched controls) that were no longer publicly traded at the end of 1997 yields virtually identical results. Moreover, in unreported results, we consider a zero-investment strategy that goes long an

[16] Our evidence contrasts with the findings of Daines (1997), which finds that stock prices of affected MA firms reacted negatively to the passage of the MA legislation examined in this paper, a finding that we can validate using our treatment and control sample. We interpret these results as suggesting that investors gradually learned the value-implications of SBs during this period, when takeover law was changing rapidly and the value effects of anti-takeover devices were uncertain (Bebchuk et al. 2013). Moreover, while Cohen and Wang (2013) also finds that market participants view SBs as being value-decreasing using two 2010 quasi-experiments, these results do not conflict with ours. As Cohen and Wang (2017) shows, these event study results are driven by large market cap firms and not the smaller early life-cycle firms that constitute our sample.

equal-weighted portfolio of MA treatment firms from May 1990 and goes short an equal-weighted portfolio of matched control firms. This strategy incorporates delisting returns, and firms that drop out of the sample are reinvested in the market, using the CRSP value-weighted index returns. This strategy produces a cumulative return between May 1990 and December 1997 of 320% for the long portfolio and 281% for the short portfolio, netting a 38.67% return. These findings also suggest that differential sample attrition is unlikely to drive our findings.

Table 3, panel A, column 4 extends the sample period to include the 21 years from 1984 to 2004. To the extent that our documented effects are correlated with the dot-com bubble, extending the window to 2004 (i.e., after the burst of the bubble) might attenuate SBs' estimated treatment effects. However, compared to the results in Table 2, column 4, we find that extending our sample period to 2004 yields a treatment effect that remains statistically significant, with a slightly larger effect magnitude at 14.75%.

We also consider variations in how treatment timing is defined and its impact on our estimated effects. In column 5, we exclude 1990 and 1991 from our sample to allow for an "adjustment period" around the legal change and find that the treatment effect remains positive and statistically significant. In untabulated results, we also find that our results are qualitatively unchanged if we exclude the observations in 1990 or from 1989 to 1991, or define the post-period to begin in 1990 or 1992. In all cases, the treatment effect estimate remains positive and statistically significant.

Next, we turn to the issue of potential confounders. Our main results include several firm-level controls that could drive *tobin's q*, such as year-fixed effects (which account for the effects of firm-invariant cross-sectional-level omitted variables) and industry-fixed effects (which account for the effects of time-invariant industry-level omitted variables). Nevertheless, we provide additional tests to examine the robustness of our main results to various potential sources of omitted-variables bias.

In Table 3, panel B, we begin by examining the possibility that our findings could be attributable to biases arising from measurement errors in Tobin's Q. Peters and Taylor (2017) proposes an alternative measurement of Q that accounts for firms' investments in intangible capital, which are ignored by the standard measurements of Q but called for by the economic theory. We obtain this alternative measure from the Wharton Research Data Services and estimate our main specification (from Table 2 column 4) using *total q* as the dependent variable. In column 1, panel B, we estimate a statistically significant (at the 5% level), positive, and even larger treatment effect: an on-average

42% increase in Total Q. Although it is possible that these magnitudes could be driven by particular measurement peculiarities proposed by Peters and Taylor (2017), our main results do not appear to be driven by differences in intangible capital between MA-treated firms and their matched control firms.

In panel B, column 2, we instead use the log of the market-to-book multiple (*mtb*) as the dependent variable, a measure of shareholder valuation commonly used in the accounting literature. We find that the treated firms experienced an economically and statistically significant (at the 5% level) 19% increase in market-to-book. These results suggest that our main findings in Q are unlikely to be entirely attributable to an increase in debt holder value but are at least partly driven by an increase in shareholder value.

Next, we examine the possibility that other unobserved firm-level variables could confound our main findings. We account for the possibility that time-invariant firm-specific omitted variables could confound our results by using firm-fixed effects. In panel B, column 3, we find that the estimated treatment effect remains positive and statistically significant at the 1% level even after including firm-fixed effects. We also examine a specification that includes lagged *tobin's q* as a control to capture potential effects of time-varying omitted variables (Wooldridge 2010). Our estimated effects, reported in panel B, column 4, remain statistically significant at the 1% level.

Finally, in panel B, column 5, we examine the robustness of the main findings to exclude those MA firms whose boards opted out of the MA legislation. Excluding these firms (and their matched controls) does not impact our results: we estimate an effect of an 11% increase in Tobin's Q that is both economically and statistically significant (at the 5% level). Note that this test is done purely for descriptive purposes to understand the degree to which the main results are sensitive to these firms' inclusion. As we explained above, firms that opted out of the legislation continued to be implicitly protected by SBs and, as such, are treated by the MA legislation. Indeed, we also find a positive and significant effect when estimating our main specification using the opt-out firms and their matched controls.

Assessing the Parallel-Trends Assumption

Our research design assumes that, absent treatment, treated MA firms and their matched control firms would have followed similar trends. The assumption of parallel trends is central to identifying treatment effects in DID designs, and its violation could lead to misleading inferences. For example, Catan and Klausner (2017) argues that the differential trends in Tobin's Q between de-staggering firms and non-destaggering firms confound the findings and inferences of Ge et al. (2016) and Cremers et al. (2017) about the effects of de-staggering boards.

Technically, it is impossible to test the parallel-trends assumption directly: we do not observe counterfactual outcomes after the policy change. Nevertheless, we examine the validity of this assumption in three ways. First, we test for evidence of differential trends between treatment and control firms during the pre-treatment period (Angrist and Pischke 2008; Lechner 2011). Differential trends in the pre-treatment period would be inconsistent with the assumption of post-treatment parallel trends. Table 4, columns 1 and 2, test for differential pre-treatment trends in *tobin's q* between the treatment firms and the control firms by including in the main specification an additional interaction term between *Treat* and an indicator for the several years before the 1990 legislation. Column 1 uses an indicator for 1989 and 1990 ($\mathbb{I}[1989-1990]$), and column 2 uses an indicator for the four years from 1987 to 1990 ($\mathbb{I}[1987-1990]$). In each case, the interaction term is not statistically significant at the 10% level, 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 pre-period trends in Q.

Nevertheless, it is still possible that, absent treatment, MA firms could have experienced different trends *after* 1990. In columns 3 and 4, we examine a particular source of differential time trends that could have confounded our main results: MA firms, and the industries to which they belonged, could have especially benefited from the economic expansion that began in the mid-1990s. In column 3, we control for industry-level time trends by including industry-year-fixed effects. (Industry- and year-fixed effects are dropped from this regression because industry-year-fixed effects absorb them.) The estimated coefficient of 0.146 remains positive and statistically significant at the 1% level. Column 4 provides an alternative test. We match each affected MA firm to the two most closely matched control firms from the same GICS2 industry incorporated in California as of 1990. Our assumption is that, like MA-incorporated firms, CA-incorporated firms from the same industry were also well-positioned to benefit from economic expansion in the mid-1990s. In column 4, Table 4, we obtain a DID coefficient of 0.149, which is statistically significant at the 5% level. The results of columns 3 and 4 suggest that our main findings are unlikely to be driven by economic trends that primarily benefited MA-incorporated firms.

To further test for such a possibility, we utilize a holdout sample of MA firms that were not affected by the MA SB law (i.e., MA firms that already had SBs or had dual-class shares before the 1990 legislation). We perform a placebo test using these 33 MA unaffected firms and their two most closely matched control firms (i.e., non-MA firms with SBs in 1990 from the same GICS2 industry as the MA firm and closest to it in terms of pre-period mean total assets, pre-period mean book-to-market ratio, and firm age in 1990). If the unaffected MA firms became more valuable after 1990 relative to their matched controls, it would support the possibility that our main results are confounded by other (i.e., non-SB related) contemporaneous factors that affected MA-incorporated firms but not non-MA-incorporated firms.

Table 4, column 5, report our placebo tests results following our main specification. The DID coefficients of -0.0329 are statistically insignificant at the 10% level and economically insignificant relative to the baseline estimate of 0.1430 (Table 2 column 4). In column 6, we perform a variant of the placebo test using only those ten unaffected MA firms from the IT sector (i.e., with a two-digit GICS code of 45). Again, we find a statistically and economically insignificant coefficient of -0.0236. These results are consistent with our main findings being driven by the imposition of SBs and not by other concurrent factors that may have influenced MA-incorporated firms, such as other MA legal or economic developments.

Overall, the battery of tests reported in Tables 1, 3, and 4 provide evidence on the internal validity of our quasi-experiment design. These tests also draw an important distinction between our empirical analyses and Cremers et al. (2017), which also examines the same MA legislation as one of their additional tests about the relationship between SB and Tobin's Q. They find a positive effect for SB on MA-incorporated firms on average, but do not find a differential positive effect for the treated MA firms (as we do). An important difference between our research designs stems from the selection of control firms. Whereas Cremers et al. (2017) chose non-MA-incorporated control firms from their main dataset (i.e., the pool of larger firms from RiskMetrics) and matched them with MA-incorporated firms based on Tobin's Q and total assets, our control firms are drawn from the universe of all CCM firms that come from the same industry and are most similar in terms of book-to-market, total assets, and firm age. We believe our matching procedure is more robust due to our exact industry matching and our use of the broad sample of public firms to find better matches for the relatively small and young MA treated firms. The covariate balance between our treatment and matched control samples and our analyses validating the parallel-trends assumption supports the credibility of our empirical approach. On the other hand, Cremers et al. (2017) does not provide similar empirical support for its approach's validity.

External Validation Using IRRC

We now turn to examine the validity of the hypothesis that SBs could be beneficial for early-life-cycle firms whose investors face greater information asymmetry. We use 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: each volume of the IRRC dataset covers between 1,400 to 2,000 firms, including those firms belonging to the S&P1500 and other firms considered important by the IRRC. This significantly larger sample provides an opportunity to validate our conclusions externally and test more directly the possible heterogeneous effects of SBs. The disadvantage of the IRRC is that, unlike our quasi-experimental MA 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 et al. 2010) and include a battery of firm-level controls that could explain both Q and the presence of SBs (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, percentage of shares owned by insiders, square of insider ownership, return on assets, capital-expenditure-to-total-assets ratio, 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). Our construction of the annual cross-sections of governance data follows Bebchuk et al. (2013) (e.g., see Section 2.1 of their paper). We also follow them in using IRRC data up to 2007 and excluding the newer RiskMetrics data because the latter data are not comparable. We regress Tobin's Q on an indicator for staggered boards (SB), and include firm controls, time-fixed effects, and industry-fixed effects. On average, we find a negative and significant association between Tobin's Q and SB in 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 dataset 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 a *Early-Life-Cycle/High-Asymmetry* firm as one that is less than six years old (the median age of our MA-incorporated firms), whose market capitalization lies in the lowest quartile of the cross-sectional distribution, and whose information asymmetry (proxied by the Amihud illiquidity ratio) lies in the highest 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 as a control an additional interaction term between the index of other provisions in the G-Index and *Early-Life-Cycle/High-Asymmetry*. The *SB* coefficient in this regression suggests that among the more mature or larger firms, or that exhibit a lower degree of information asymmetry, the association between Tobin's Q and *SB* remains negative and statistically significant at the 5% level. However, for the set of early-life-cycle firms that face a relatively high degree of information asymmetry, we find a significant and 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 (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. In contrast, SBs are associated with 3.09% lower Q among larger and more mature firms. Thus, Table 5 not only provides external validation of our inferences from the MA quasi-experiment but also suggests that an SB's impact may be different for larger or more mature firms.

Exploring Possible Mechanisms

This subsection investigates potential channels through which SBs could have improved firm value. We examine the MA legislation's value effects for subsets of firms and its impact on managers' and shareholders' behaviors.

Investment and Innovation

Supporters of SBs argue that takeover defenses can encourage investment and innovation, particularly at firms whose strategies require a long time horizon to execute and whose outside investors are likely to be less informed about the firm's value. At such firms, an SB might allow managers to invest in projects whose value becomes apparent to outsiders only in the long run and whose eventual success may require tolerance for early failures (Manso 2011). Under this hypothesis, we can expect the MA legislation to be particularly beneficial to innovating firms, which we define as young firms or those that invest substantially in R&D.

Table 6, column 1, reports an expanded version of our primary Q regression for the subsample of innovating treatment firms (with below-median age in 1990 or an R&D-to-sales ratio in the top quintile of the 1990 CCM population) and their matched controls. We find that the baseline positive effects of SBs on Tobin's Q are concentrated in the innovating firms, which experienced a 17.7% increase in firm value following the MA legislation. In contrast, for the subsample of non-innovating firms (column 2), we find a DID coefficient close to zero in magnitude and statistically indistinguishable from zero.

We further investigate the subsample of innovating firms covered by sell-side analysts. We use analyst coverage to approximate the increased pressures from Wall Street that some public firms face, from which the insulation SBs provide may be particularly beneficial, for example, in mitigating the consequences of managerial myopia (Bhojraj et al. 2009; Terry 2015). Our analysis suggests that the benefits of SBs are most substantial at innovating firms covered by analysts: these firms experienced a 25.32% increase in Tobin's Q. In contrast, we find no significant effect on the subset of non-innovating or non-covered treatment firms (column 4).

Next, we directly examine how the MA legislation impacted firms' investment behavior. In particular, we focus on firms' long-run investments, as measured by capital expenditures (*CAPEX*) and research and development (*R&D*) expenditures, whose values require some time to realize. We define *investments* as the log of 1 plus *CAPEX* and *R&D* expenditures, where we replace missing values with zeros. Table 7 reports DID estimates on *investments* using the entire sample (column 1), the subsample of innovating firms (column 2), and the subsample of innovating-and-covered firms (column 3). In all specifications, we include indicators for missing values for *CAPEX* and

R&D.

We find that the MA legislation led to a significant increase in *investments*. For the whole sample, we estimate a 9.4% increase in total investments. In columns 2 and 3, we estimate a 13.2% increase among innovating firms and an 18.8% increase among innovating-and-covered firms. Both estimates are economically significant and statistically significant at the 1% level, consistent with these firms benefiting the most from SBs' protections.

In addition to examining ex-ante measures of innovation, we also analyze the effect of the legislation on patent generation, an ex-post measure of innovation. We utilize the Derwent Innovation (previously known as the Thomson Innovation) database and collected information on all U.S. patent applications that our treatment firms and their matched control firms had submitted between January 1, 1984, and December 31, 1997 that were ultimately granted. We note that the significant gap between 1997 and 2020 alleviates the "truncation problem" encountered by empirical studies that use patent data, namely the observation of fewer approved patent applications toward the end of the sample period due to the time lag between application and approval. We define *patents* in a given year as the log of 1 plus the total number of patent applications filed in that year that were eventually approved.

Table 7, columns 4-6, report DID estimates for *patents*. We estimate a 10.1%, 13%, and 23.1% increase in the number of patents for the full sample (column 4) and the subsamples of innovating (column 5) and innovating-and-covered firms (column 6). In all cases, the effects are statistically significant at the 1% level.

Innovation Quality

We also examine how the MA legislation impacted the quality of innovations among the affected firms. Prior research has shown that excessive market pressures could impact not only the level but also the quality of innovation (e.g., He and Tian 2013). To the extent that the protection afforded by SBs facilitated a greater tolerance for failures and more experimentation in firms, we could also expect improvements in innovation quality.

We consider three different measures to capture the quality of innovations. First, we consider citation-weighted patents, a measure of patents' scientific impact, computed as the number of patents weighted by the number of citations each patent received in subsequent years until 2015.

We also consider a measure developed by Kogan et al. (2017), which estimates the economic value of patents using market reactions to patent approval news.[17] Finally, we consider a frequently employed measure of patent originality (e.g., Jaffe and Trajtenberg 2002), calculated as one minus the Herfindahl index across the technology classes of the citations made by the patent to earlier patents. A patent with an originality score approaching zero suggests that the patent draws on a narrow array of patents, while a measure of one suggests it builds on a diverse array of patents. If a patent cites (i.e., is influenced by) patents from a diverse range of technology classes, it is considered more original or novel. We calculate this originality measure at the firm-year level by averaging the originality score of all patents filed by a firm in a given year.[18]

Table 8 reports DID estimates on the three different measures of innovation quality. Columns 1-3 report results that use as the dependent variable *citation-weighted patents*, or log of 1 plus the sum of citation-weighted patents for a firm in a given year. For the entire sample of firms (column 1), we estimate a 33.2% increase in citation-weighted patents after the MA legislation. Among the subsample of innovating firms (column 2) and innovating-and-covered firms (column 3), we estimate increases of 35.9% and 50.7%. In all cases, the estimated effects are statistically significant at the 1% level.

Columns 4-6 report results that use *patent value / market cap* as the dependent variable, or log of one plus the firm's value of patents in each year scaled by market capitalization. We interpret the ratio of patent value to market capitalization as the relative value of patents filed in a given year. For the entire sample of firms (column 4), we estimate a 3% increase in the relative value of patents after the MA legislation. Among the subsample of innovating firms (column 5) and innovating-and-covered firms (column 6), we estimate increases of 4.1% and 7.4%. In all cases, the estimated effects are statistically significant at the 1% level.

Columns 7-9 report results that use *patent originality* as the dependent variable, or log of one plus the average originality score of a firm's patents each year. For the entire sample of firms

[17] Kogan et al. (2017) estimates the economic value of patents approved in the 1926 to 2010 period, and shows that this measure is highly correlated with the scientific value of patents, firm growth, resource allocation, and total factor productivity.

[18] The patent economic value and patent originality measures are obtained or calculated from datasets posted by the authors of Kogan et al. (2017). Estimates of the economic value of patents can be obtained from https://github.com/KPSS2017/Technological-Innovation-Resource-Allocation-and-Growth-Extended-Data. The underlying patent-level dataset—which contains information about each patent's technology classes and subclasses as well as the patent number of each patent that it cites or cites it, from which we construct patent originality—can be obtained from https://paper.dropbox.com/doc/U.S.-Patent-Data-1926-2010-t5nuNWnTH1InM0gyxkizL.

and the subsample of innovating firms (columns 7 and 8), our estimates suggest that there was, on average, no statistically or economically meaningful change in patent originality after the MA legislation. However, among innovating-and-covered firms (column 9), we estimate an average increase of 2.2% in patent originality, which is statistically significant at the 5% level, consistent with greater experimentation in these firms resulting in more novel patents.

Effect on Return on Assets

Next, we examine the effect of the MA legislation on return on assets. To the extent that the SB changed managerial behavior in a way that improved the firms' future cash flows, we should find improvements in affected firms' return on assets.

Table 9, columns 1–3, report DID estimates on *roa* for the full sample, the subsample of innovating firms, and the subsample of innovating-and-covered firms. In each case, we obtain a positive and statistically significant (at the 10% level) coefficient. The coefficient for the innovating firms (0.0205) is larger than that for the full sample (0.0161). For the subsample of innovating-and-covered firms, which exhibit the largest proportional increase in Q, increases in investments and innovation, and innovation quality, we also find the greatest increase in *roa* (0.0335), statistically significant at the 1% level.

Investor Composition

Finally, we examine how the MA legislation affected investor composition. Specifically, we investigate the possibility that the imposition of SBs led to more patient investors, such as institutional investors or dedicated institutional investors, who hold large and stable stakes. For example, Bushee (1998) finds that more significant shareholdings by institutional investors reduce myopic pressures for managers.

For this analysis, we rely on Brian Bushee's classification of institutional investors into dedicated, transient, or quasi-indexing investors, based on their observed trading and investment behavior (e.g., Bushee and Noe 2000; Bushee 2001).[19] We merged the investor classification data with the Thomson Reuters Institutional (13F) Holdings database, and computed the total number of

[19]This data can be downloaded from Bushee's website: https://accounting-faculty.wharton.upenn.edu/bushee/.

shares of each public firm at the end of each calendar year held by institutional investors, dedicated institutional investors, and transient or quasi-indexing institutional investors.

Table 10 reports DID estimates on the proportion of shares held by institutional investors (column 1) and the proportion of the firm's institutional shareholder holdings held by dedicated (column 2) or other (column 3) institutional investors. Column 1 shows that the MA legislation increased the proportion of shares held by institutional investors by 4.3%. This effect is statistically significant at the 1% level. It is also economically large: relative to the pre-period mean (among the affected MA firms) institutional shareholding of 25.8%, our estimate translates to a 17% proportional increase.

Column 2, Table 10, shows that the MA legislation also increased the proportion of institutional shareholdings held by dedicated institutional investors by 1.1%. This effect is statistically significant at the 5% level and is economically large: relative to the pre-period median of 4.4%, our estimate translates to a 25% proportional increase. Finally, in column 3, Table 10, we do not find a statistically significant effect on the proportion of institutional shareholdings held by other (i.e., transient or quasi-indexing) institutional investors. However, the point estimate is negative and similar in magnitude to column 2.

These findings suggest that investor selection could be another way that SBs allow managers to focus on long-term investments and innovation. After the imposition of a governance feature that made it more difficult for activist investors to overturn board decisions, shareholders were more likely to be "patient." This shareholder base might enable managers to invest in risky projects whose values were less clear to outsiders.

Discussion

Several caveats apply to our study's findings. First, our results are based on a relatively small sample of MA treatment firms. However, we find evidence of external validity in a sample of IRRC firms. In this sample, SBs are associated with higher valuations for early-life-cycle firms with higher information asymmetry, but not among other (e.g., more mature) firms. Second, while we find that SBs improved firm value, it is possible that the law harmed some stakeholders. Our study does not focus on the overall welfare effects of the legislation.

One question raised by our paper's findings is why some MA firms did not adopt SBs voluntarily before 1990, as it was (*ex post*) in the best interest of shareholders and board members. We analyze this question in two parts. First, before the poison pill's development in the mid-1980s, the SB was unimportant; the election rule would not deter a bidder's ability to purchase a control block. Little turned on the adoption of a trivial governance device. Second, after SBs became an important protection given the development of the poison pill, it is possible that shareholders might have anticipated the value of SBs in their firms and agreed to approve the creation of an SB. While this is possible, takeover law was changing rapidly at this time and shareholders were generally skeptical of the merits of entrenching managers, as evidenced by many institutional investors' reactions to the MA law and the initial adverse market reactions around its adoption (Daines 1997). We assume that it took market participants time to learn the value implications of SBs (e.g., consistent with the learning hypothesis of Bebchuk et al. 2013). To be sure, our strategy for selecting control firms is designed to mitigate the possibility that a selection mechanism (for firms not adopting an SB by 1990) *per se* confounds our empirical analyses.

4. Conclusion

This paper provides causal and nuanced evidence about how SBs can improve firm value by exploiting a quasi-experimental setting—a MA law requiring all firms to adopt SBs. Specifically, we examine *which firms* were better off with SBs and *why*.

Our analysis suggests that SBs improve firm value at early-life-cycle firms where managers contend with a relatively high degree of information asymmetry. Managers at these firms were more likely to invest in growth and innovation once they were entrenched: they made more long-term investments, created more (and higher quality) innovations, and their firms were ultimately more profitable. These effects were particularly pronounced in firms that relied on innovation and faced scrutiny from Wall Street analysts. These firms also attracted shareholders that were more patient, consistent with the idea that SBs afford valuable stability and a longer-term investment horizon (Graham et al. 2005; He and Tian 2013).

Further, our analysis of IRRC data suggests that the value of SBs changes as firms mature and as the value of investments and business strategy become more evident to outsiders. These findings are consistent with SBs' adverse entrenchment effects beginning to dominate as information asymmetry becomes less significant; they rationalize why shareholders typically do not object to young firms going IPO with SBs but prefer the discipline of the market for corporate control for larger and more mature firms. Thus, our findings support the view that SBs at young firms may be usefully paired with sunset provisions that phase out these powerful insulating forces as firms mature.

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

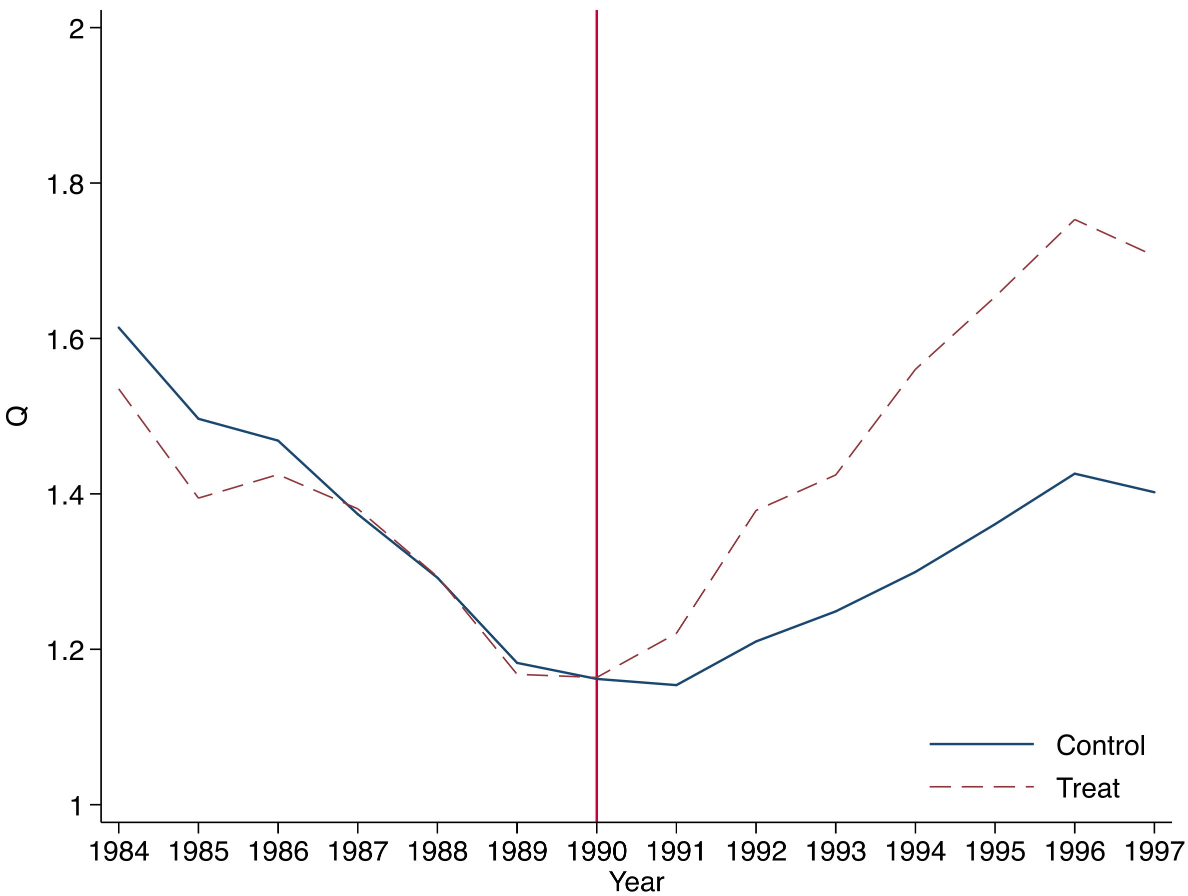

TABLE A1
Description of Variables

This table presents definitions of the main 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 Derwent (Thomson) Innovation database.

Variable	Description	Calculation
age	Natural Logarithm of Firm Age	ln(Number of years since first observed PERMNO on CRSP)
assets	Natural Logarithm of Total Assets	ln([at])
btm	Natural Logarithm of Book to Market Ratio	Natural Logarithm of (equity [ceq] + deferred taxes and investment credit [txditc]) / market cap [prcc_f × csho]
capex-to-assets	Natural Logarithm of the ratio of CAPEX to Total Assets	ln([capx]/[at])
citation-weighted patents	Natural Logarithm of (1 + the sum of patents weighted by the number of citations each patent received in subsequent years until 2015)	
Info Asymmetry	Amihud illiquidity ratio	Daily average of $1000000 \times \lvert(\text{ret})\rvert$ / $\lvert(\text{prc})\rvert \times (\text{vol})$ from January 1 to March 30, 1990
investments	Natural Logarithm of (1+CAPEX and R& D)	ln(1+[capx]+[xrd])
leverage	Natural logarithm of Leverage	ln(liabilities [lt] / total assets [at])
mtb	Natural Logarithm of Market to Book Ratio	Natural Logarithm of market cap [prcc_f × csho] / (equity [ceq] + deferred taxes and investment credit [txditc])
patents	Natural Logarithm of (1+ Patents) where Patents is the number of patents applied for by the firm that were eventually granted	
patent value / market cap	Natural Logarithm (1 + the sum of the economic value of patents divided by the total market capitalization) Kogan et al. (2017)	
patent originality	Natural Logarithm of the average value of (1 - the Herfindahl index across the technology classes of the citations made by the patent to earlier patents)	
Post	Post-legislation indicator	equals 1 if the fiscal year end occurred after 1990
r&d-to-assets	Natural logarithm of the ratio of R&D to Total Assets	ln([xrd]/[at])
roa	Natural Logarithm of (1+ ROA) where ROA is the Return on Assets	ln(1+((operating income before depreciation [oibdp] − depreciation [dp]) / total assets [at]))
sales	Natural Logarithm of Sales	ln(sale)
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*	
total q	Natural Logarithm of *Total Q* as defined in Peters and Taylor (2017)	
Treat	Treatment indicator	equals 1 if the firm was MA-incorporated without a staggered board prior to 1990

TABLE 1
Summary statistics

Panel A of this table summarizes the industry distribution, in terms of the two-digit Global Industry Classification System (GICS) sector, of the treatment firms in the study. Column 1 reports the proportion of MA treatment firms belonging to each sector and column 2 reports the proportion of the 1990 volume of the Investor Responsibility Research Center (IRRC) database belonging to each sector. Panel B of this table compares the pre-period (1984–1990) means of the characteristics of control firms (non-MA-incorporated firms without staggered boards in 1990), reported in column 1; treated firms (MA-incorporated firms without staggered boards and dual-class shares in 1990), reported in column 2; their differences, reported in column 3; the total number of observations, reported in column 4; and the t-statistics associated with the differences in means, reported in column 5. t-statistics are computed based on cluster-robust standard errors, clustering at the firm level. Column 6 reports the percentages of firms in IRRC's 1990 volume with values lower than the treated sample's pre-period mean.

Panel A: Treatment firm industry distribution

GICS Sector (Code)	(1) % of Total	(2) IRRC %
Materials (15)	1.75	9.79
Industrials (20)	17.54	17.27
Consumer Discretionary (25)	12.28	13.20
Health Care (35)	10.53	5.36
Financials (40)	1.75	21.98
Information Technology (45)	49.12	7.29
Utilities (55)	7.02	12.93
Total	100.00	87.82

Panel B: Background characteristics compared to matched sample

Firm Characteristics	(1) Control	(2) Treated	(3) Δ	(4) N	(5) T	(6) Pctile IRRC
Total Assets	299.095	375.0966	75.872	928	0.426	21.0%
Firm Age	9.798	10.006	0.208	912	0.142	21.9%
Book to Market	0.742	0.747	0.004	896	0.074	44.7%
Tobin's Q	1.571	1.624	0.052	901	0.466	81.4%
Return on Assets	0.047	0.061	0.014	916	0.869	26.8%
Leverage	0.466	0.433	-0.033	924	-1.102	15.7%
Info Asymmetry	14.136	8.545	-5.591	888	-1.315	99.8%
R&D Expense	5.962	9.606	3.643	936	1.587	28.5%
Capital Expenditure	18.140	31.109	12.969	936	1.055	37.4%

TABLE 2
Average treatment effect on *tobin's q*

Column 1 of this table reports OLS results of regressing *tobin's q* on a treatment indicator (*Treat*), a post-legislation indicator (*Post*), and an interaction of the two variables (*Treat* × *Post*). Columns 2–4 include firm-level controls, and vary depending on whether year or industry-fixed effects are included. The *Post* indicator is absorbed by (not reported in specifications that include) time-fixed effects. All variables are defined in Table A1. The sample period consists of the years 1984 to 1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
Treat × *Post*	0.1410***	0.1472***	0.1427***	0.1430***
	(0.051)	(0.022)	(0.055)	(0.055)
Treat	-0.0145	-0.0214	-0.0173	-0.0181
	(0.052)	(0.026)	(0.045)	(0.047)
Post	-0.0344	0.0274		
	(0.045)	(0.052)		
age		-0.0682***	-0.0728***	-0.0570***
		(0.024)	(0.022)	(0.020)
Lag *sales*		0.0366	0.0321	0.0257
		(0.039)	(0.038)	(0.041)
Lag *assets*		-0.0588	-0.0579	-0.0522
		(0.037)	(0.036)	(0.039)
Lag *roa*		-0.0039	0.0034	0.0118
		(0.132)	(0.132)	(0.124)
Lag *leverage*		-0.0771***	-0.0622***	-0.0444**
		(0.018)	(0.020)	(0.022)
Lag *r&d-to-assets*		0.7853*	0.7264	0.6444
		(0.460)	(0.478)	(0.435)
Lag *capex-to-assets*		0.5525**	0.3927	0.4152
		(0.257)	(0.247)	(0.282)
Time FE	No	No	Yes	Yes
Industry FE	No	No	No	Yes
Observations	1,862	1,862	1,862	1,862
Adj R^2	0.0083	0.1430	0.1849	0.2131

TABLE 3
Robustness

This table reports variants of the baseline specification in Table 2, column 4. Panel A reports results estimated based on different sample windows. Columns 1–4 report estimates based on the following expanding windows: 1989-1992, 1988-1993, 1986-1995, and 1984-2004. Column 5 reports results that exclude data from 1990 and 1991 from the sample. Panel B considers other variants of the baseline estimation. Column 1 uses *total q* of Peters and Taylor (2017) as the dependent variable; column 2 uses *mtb* as the dependent variable; column 3 includes firm-fixed effects; column 4 includes lag *tobin's q* as an additional control; and column 5 excludes the treatment firms (and their controls) that opted out of the legislation. All variables are defined in Table A1. Unless otherwise specified, the sample period consists of the years 1984–1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

Panel A: Different windows

	(1)	(2)	(3)	(4)	(5)
	1989-1992	1988-1993	1986-1995	1984-2004	Exclude 1990-1991
Treat × *Post*	0.1044**	0.1209***	0.1319***	0.1475***	0.1503**
	(0.051)	(0.041)	(0.048)	(0.053)	(0.058)
Firm Controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	613	900	1,407	2,414	1,550
Adj R^2	0.1593	0.1680	0.2006	0.2017	0.2191

Panel B: Different estimators

	(1)	(2)	(3)	(4)	(5)
	total q	*mtb*	Firm FE	Lagged Dep Var	Exclude Opt-Outs
Treat × *Post*	0.4196***	0.1908**	0.1159***	0.0698***	0.1098**
	(0.094)	(0.080)	(0.039)	(0.026)	(0.053)
Firm Controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	No	No	Yes
Observations	1,650	1,850	1,862	1,829	1,346
Adj R^2	0.1478	0.2080	0.5652	0.6138	0.1418

TABLE 4
Testing the parallel-trends assumption

This table reports variants of the baseline specification in Table 2, column 4, to test the parallel trends assumption. Columns 1 and 2 report variants of the baseline regression that add an additional interaction term to test differential pre-period trends in *tobin's q*: column 1 includes an interaction between *Treat* and a time indicator for the 1989–1990 period ($Treat \times \mathbb{I}[1989-1990]$) and column 2 includes an interaction between *Treat* and a time indicator for the 1987–1990 period ($Treat \times \mathbb{I}[1987-1990]$). Column 3 reports a variant of the baseline specification that includes industry-year-fixed effects. Column 4 reports a variant of the baseline specification that uses as the control sample the closest two matches incorporated in California.

Columns 5 reports results from the baseline specification using a placebo sample of unaffected MA firms (MA-incorporated firms that already had staggered boards or dual-class shares prior to the MA legislation) and their matched controls (two most similar non-MA firms from the same industry that already had staggered boards or had dual-class shares prior to the MA legislation). Column 6 reports the results from the baseline specification but only using the subset of the placebo sample that belongs to the information and technology (IT) sector, those unaffected MA-incorporated firms and their matched controls with a two-digit GICS code of 45. All variables are defined in Table A1. The sample period in all specifications consists of the years 1984–1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Pre-Treatment Trends		Ind-Yr FE	CA Matches	Unaffected MA Firms	
$Treat \times Post$	0.1354**	0.1560**	0.1461***	0.1492**	-0.0329	-0.0236
	(0.059)	(0.061)	(0.056)	(0.075)	(0.047)	(0.091)
Treat	-0.0104	-0.0310	-0.0141	0.0487	0.0105	-0.0630
	(0.048)	(0.041)	(0.048)	(0.052)	(0.039)	(0.087)
$Treat \times \mathbb{I}[1989-1990]$	-0.0222					
	(0.044)					
$Treat \times \mathbb{I}[1987-1990]$		0.0200				
		(0.035)				
age	-0.0571***	-0.0569***	-0.0542**	-0.1162***	-0.2049***	-0.3758***
	(0.020)	(0.020)	(0.021)	(0.038)	(0.040)	(0.042)
Lag *sales*	0.0257	0.0257	0.0273	-0.0069	0.0748	0.0705
	(0.041)	(0.041)	(0.041)	(0.044)	(0.069)	(0.055)
Lag *assets*	-0.0522	-0.0522	-0.0549	0.0162	0.0180	0.0540
	(0.039)	(0.039)	(0.040)	(0.043)	(0.064)	(0.117)
Lag *roa*	0.0119	0.0120	0.0070	0.1196	-0.2070	-0.0652
	(0.124)	(0.124)	(0.128)	(0.143)	(0.292)	(0.337)
Lag *leverage*	-0.0443**	-0.0444**	-0.0413*	-0.0304	-0.1249	-0.1013
	(0.022)	(0.022)	(0.024)	(0.034)	(0.084)	(0.131)
Lag *r&d-to-assets*	0.6461	0.6434	0.6427	0.8150**	2.0352***	2.2235**
	(0.435)	(0.435)	(0.443)	(0.377)	(0.419)	(0.865)
Lag *capex-to-assets*	0.4119	0.4192	0.3756	0.0377	0.7408	0.3074
	(0.280)	(0.281)	(0.303)	(0.387)	(0.706)	(1.221)
Time FE	Yes	Yes	No	Yes	Yes	Yes
Industry FE	Yes	Yes	No	Yes	Yes	No
IT (GICS2=45) Only	No	No	No	No	No	Yes
Observations	1,862	1,862	1,862	1,769	1,207	349
Adj R^2	0.1998	0.1998	0.1979	0.2504	0.4212	0.3239

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 early-life-cycle/high-information-asymmetry firms (*Early-Life-Cycle/High-Asymmetry*), and an interaction term (*SB* × *Early-Life-Cycle/High-Asymmetry*), firm controls, and time- and industry-fixed effects. *Early-Life-Cycle/High-Asymmetry* firms are those less than 6 years old, 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 are 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 × *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	0.0658	0.0607

TABLE 6

Treatment effect on *tobin's q* for subsamples

This table reports variants of the specification in Table 2, column 4, based on subsamples. Column 1 (2) reports the result estimated using the subsample of innovating (non-innovating) treatment firms and their matched control firms; column 3 (4) reports the result estimated using the subsample of innovating-and-covered (non-innovating or non-covered) treatment firms and their matched control firms. Innovating firms are defined as those that are either young or R&D intensive. Young treatment firms are those firms whose age (in 1990) is below the median of the CRSP-Compustat Merged database universe; R&D intensive firms are those whose R&D-to-sales ratio (in 1990) is in the top quintile of the CRSP-Compustat Merged database universe. Covered treatment firms are those with analyst coverage in at least one of the four quarters prior to the legislation. All variables are defined in Table A1. The sample period in all specifications consists of the years 1984–1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) Innovating	(2) Non-Innovating	(3) Innovating and Covered	(4) Non-Innovating or Non-Covered
Treat × *Post*	0.1770**	0.0435	0.2532***	0.0583
	(0.072)	(0.050)	(0.090)	(0.045)
Treat	-0.0289	0.0292	0.0279	-0.0123
	(0.058)	(0.029)	(0.069)	(0.040)
age	-0.0443	-0.0229	-0.0078	-0.0529
	(0.028)	(0.062)	(0.048)	(0.040)
Lag *sales*	0.0739	-0.0113	-0.0218	0.0159
	(0.062)	(0.029)	(0.108)	(0.038)
Lag *assets*	-0.1111*	-0.0083	-0.0182	-0.0539
	(0.063)	(0.029)	(0.106)	(0.036)
Lag *roa*	-0.0195	0.3769	-0.1295	0.0720
	(0.133)	(0.236)	(0.322)	(0.111)
Lag *leverage*	-0.0506*	0.0137	-0.0580**	-0.0110
	(0.029)	(0.046)	(0.027)	(0.037)
Lag *r&d-to-assets*	0.6093	0.0950	1.3367**	0.5259
	(0.464)	(0.520)	(0.591)	(0.502)
Lag *capex-to-assets*	0.3248	0.9969**	-0.0743	1.0036***
	(0.308)	(0.478)	(0.374)	(0.289)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	1,435	427	818	1,044
Adj R^2	0.1881	0.3382	0.2334	0.2541

TABLE 7
Investments and innovation

This table reports the results of OLS regressions using *Investments* (columns 1–3) and *Patents* (columns 4–6) as the dependent variables. Columns 1 and 4 report the results estimated using the full sample of firms and their matched control firms; columns 2 and 5 report the results estimated using the subsample of innovating treatment firms and their matched control firms; columns 3 and 6 report the results using the subsample of innovating-and-covered treatment firms and their matched control firms. Innovating firms are defined as those that are either young or R&D-intensive. Young treatment firms are those whose age (in 1990) is below the median of the CRSP-Compustat Merged database universe; R&D intensive firms are those whose R&D-to-sales ratio (in 1990) is in the top quintile of the CRSP-Compustat Merged database universe. Covered treatment firms are those with analyst coverage in at least one of the four quarters prior to the legislation. All variables are defined in Table A1.The sample period consists of the years 1984–1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	investments			*patents*		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Innovating	Innovating and Covered	All	Innovating	Innovating and Covered
Treat × *Post*	0.094***	0.132***	0.188***	0.101***	0.130***	0.231***
	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.07)
Treat	0.020	0.033	0.014	0.185**	0.257**	0.271
	(0.04)	(0.05)	(0.10)	(0.08)	(0.10)	(0.19)
age	-0.051**	-0.038*	0.013	-0.019	0.078*	0.010
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
Lag *sales*	-0.002	0.032	0.020	0.085	-0.012	0.102
	(0.07)	(0.10)	(0.12)	(0.05)	(0.07)	(0.12)
Lag *assets*	0.776***	0.727***	0.766***	0.157***	0.295***	0.247
	(0.05)	(0.06)	(0.08)	(0.05)	(0.08)	(0.16)
Lag *roa*	0.284***	0.252**	0.362**	-0.154	-0.248**	-0.384
	(0.10)	(0.11)	(0.15)	(0.10)	(0.10)	(0.26)
Lag *leverage*	-0.060	-0.033	-0.041	-0.111***	-0.111**	-0.220**
	(0.04)	(0.04)	(0.08)	(0.04)	(0.06)	(0.11)
Lag *r&d-to-assets*	4.367***	4.102***	5.205***	1.250**	1.207**	1.465
	(0.27)	(0.38)	(0.69)	(0.53)	(0.60)	(1.13)
Lag *capex-to-assets*	5.176***	4.865***	4.778***	0.764**	0.673	1.527*
	(0.45)	(0.44)	(0.45)	(0.35)	(0.41)	(0.79)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,862	1,435	818	1,862	1,435	818
Adj R^2	0.9060	0.8896	0.9220	0.3354	0.3851	0.4489

TABLE 8
Innovation quality

This table reports the results of OLS regressions using measures of innovation quality as dependent variables: columns 1-3 use *Citation-Weighted Patents*, columns 4-6 use *Patent Value / Market Cap*, and column 7-9 use *Patent Originality* as the dependent variable. Column 1, 4, and 7 report the results estimated using the full sample of firms and their matched control firms. Columns 2, 5, and 8 report the results estimated using the subsample of innovating treatment firms and their matched control firms. Columns 3, 6, and 9 report the results using the subsample of innovating-and-covered treatment firms and their matched control firms. Innovating firms are defined as those that are either young or R&D-intensive. Young treatment firms are those whose age (in 1990) is below the median of the CRSP-Compustat Merged database universe; R&D intensive firms are those whose R&D-to-sales ratio (in 1990) is in the top quintile of the CRSP-Compustat Merged database universe. Covered treatment firms are those with analyst coverage in at least one of the four quarters prior to the legislation. All variables are defined in Table A1. The sample period in each specification consists of the years 1984–1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	citation-weighted patents			*patent value / market cap*			*patent originality*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Innovating	Innovating and Covered	All	Innovating	Innovating and Covered	All	Innovating	Innovating and Covered
Treat × *Post*	0.332***	0.359***	0.507***	0.030***	0.041***	0.074***	-0.002	-0.004	0.022**
	(0.08)	(0.10)	(0.11)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Treat	0.437***	0.547***	0.752**	-0.002	-0.005	-0.026	0.011	0.016	0.036**
	(0.17)	(0.20)	(0.34)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)
age	0.018	0.188*	0.073	-0.002	-0.003	-0.017	0.006	0.013**	0.019*
	(0.10)	(0.10)	(0.11)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)
Lag *sales*	0.031	-0.108	-0.028	0.005	-0.005	0.007	-0.006	-0.047***	-0.068***
	(0.08)	(0.11)	(0.24)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)
Lag *assets*	0.327***	0.541***	0.504	0.011**	0.025***	0.020	0.027***	0.073***	0.087***
	(0.11)	(0.18)	(0.32)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Lag *roa*	-0.240*	-0.400***	-0.402	0.002	-0.003	-0.018	-0.013	-0.001	-0.003
	(0.13)	(0.14)	(0.46)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)
Lag *leverage*	-0.192**	-0.214*	-0.305*	-0.002	0.001	-0.010	-0.013	-0.003	0.014
	(0.09)	(0.12)	(0.18)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)
Lag *r&d-to-assets*	2.063**	1.875*	2.578	0.114***	0.112***	0.118*	0.211*	0.270*	0.357***
	(0.95)	(1.09)	(1.98)	(0.04)	(0.04)	(0.07)	(0.13)	(0.16)	(0.13)
Lag *capex-to-assets*	1.668**	1.466*	2.818**	0.075**	0.071*	0.136	0.187*	0.110	0.023
	(0.67)	(0.85)	(1.28)	(0.03)	(0.04)	(0.10)	(0.10)	(0.09)	(0.13)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,862	1,435	818	1,862	1,435	818	1,862	1,435	818
Adj R^2	0.2922	0.3391	0.4115	0.1502	0.1697	0.2079	0.1749	0.1995	0.2735

TABLE 9
Return on assets

This table reports the results of OLS regressions of *roa* on *Treat*, an interaction of *Treat* and *Post*, firm- and industry-fixed effects, and firm-level controls. Column 1 reports the results estimated using the full sample of treatment firms and their matched control firms; column 2 reports the results estimated using the subsample of innovating treatment firms and their matched control firms; column 3 reports the results using the subsample of innovating-and-covered treatment firms and their matched control firms. Innovating firms are defined as those that are either young or R&D-intensive. Young treatment firms are those whose age (in 1990) is below the median of the CRSP-Compustat Merged database universe; R&D-intensive firms are those whose R&D-to-sales ratio (in 1990) is in the top quintile of the CRSP-Compustat Merged database universe. Covered treatment firms are those with analyst coverage in at least one of the four quarters prior to the legislation. All variables are defined in Table A1. The sample period in each specification consists of the years 1984–1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1) All	(2) Innovating	(3) Innovating and Covered
Treat × *Post*	0.0161*	0.0205*	0.0335***
	(0.009)	(0.011)	(0.012)
Treat	0.0014	-0.0024	0.0070
	(0.002)	(0.001)	(0.006)
age	0.0071	0.0097	0.0134
	(0.006)	(0.007)	(0.009)
Lag *sales*	0.0477***	0.0506***	0.0476
	(0.007)	(0.011)	(0.030)
Lag *assets*	-0.0385***	-0.0389***	-0.0462*
	(0.008)	(0.013)	(0.026)
Lag *roa*	0.3078***	0.2964***	0.4717***
	(0.054)	(0.057)	(0.092)
Lag *leverage*	-0.0019	-0.0028	0.0051
	(0.010)	(0.010)	(0.012)
Lag *r&d-to-assets*	-0.1305	-0.1010	-0.0634
	(0.086)	(0.100)	(0.161)
Lag *capex-to-assets*	0.1066*	0.1273**	0.0744
	(0.060)	(0.064)	(0.162)
Time FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	1,836	1,413	805
Adj R^2	0.3325	0.3244	0.4393

TABLE 10
Investor type

This table reports the results of OLS regressions of measures of investor composition on Treat, an interaction of *Treat* and *Post*, firm- and industry-fixed effects, and firm-level controls. Column 1 reports results that use the percentage of shares outstanding held by institutional investors. Column 2 reports results that use the percentage of institutional holdings held by dedicated investors. Column 3 reports results that use the percentage of institutional holdings held by transient or quasi-indexing investors. Institutional shareholding data is obtained from the Thomson Reuters Institutional (13F) Holdings database, and institutional investor classification is obtained from Brian Bushee's website (Bushee and Noe 2000; Bushee 2001). All variables are defined in Table A1. The sample period in each specification consists of the years 1984–1997. Standard errors are two-way-cluster robust, clustering at the state and year levels, and are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
	Institutional Investors	Dedicated Investors	Transient or Quasi-Indexing
Treat × *Post*	0.0432***	0.0111**	-0.0224
	(0.012)	(0.005)	(0.026)
Treat	-0.0171	-0.0097	0.0203
	(0.013)	(0.008)	(0.024)
age	0.0175	0.0054	-0.0019
	(0.011)	(0.008)	(0.011)
Lag *sales*	0.0431***	-0.0007	0.0074
	(0.008)	(0.006)	(0.016)
Lag *assets*	0.0499***	0.0111	0.0206
	(0.007)	(0.008)	(0.015)
Lag *roa*	0.0344*	-0.0705**	0.1360***
	(0.018)	(0.033)	(0.039)
Lag *leverage*	-0.0621***	0.0016	-0.0236**
	(0.011)	(0.006)	(0.010)
Lag *r&d-to-assets*	0.2588***	-0.0850	0.3038**
	(0.093)	(0.052)	(0.140)
Lag *capex-to-assets*	0.3312***	0.1546**	-0.0913
	(0.116)	(0.066)	(0.092)
Time FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	1,862	1,862	1,862
Adj R^2	0.6207	0.0680	0.0874

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