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

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Abstract

We study the effect of staggered boards on long-run firm value using a natural experiment: a 1990 law that imposed a staggered board on all firms incorporated in Massachusetts. We find a significant and positive average (and median) increase in Tobins Q for innovating firms, particularly those facing greater Wall Street scrutiny. This increase in value appears to come, at least in part, from increased investment in R&D and capital expenditures and from valuable patents. Our findings suggest that staggered boards can be beneficial when firms and investors face information asymmetries – when firms are young, innovating, and reliant on R&D.

Keywords: Staggered board, entrenchment, long-termism, Tobin's Q

JEL: G14, G32, K22

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1 Introduction

No corporate governance topic has been more heavily debated in recent years than the effect of staggered (or "classified") boards. Staggered boards are controversial because they allow directors to resist shareholder attempts to change control of the firm. When a board of directors is staggered, only a third of the directors are up for re-election each year (as with the US senate). Thus, even if all shareholders want to oust incumbent directors immediately and to install all new directors, they can only oust one third of the board each year. It takes at least two annual meetings for insurgents to win control of the board. This delay is costly to insurgents and staggered boards are now the most important source of variation in regulating the firm's exposure to the market for corporate control.

Supporters of staggered boards argue that this insulation from shareholders allows directors to make better investment and operating decisions. Directors may rationally avoid making valuable investments if they can be ousted (or the firm taken over) before the value of these investments is apparent (Stein, 1988, 1989). Because staggered boards delay changes in control and protect the firm from takeovers in the short-run (and before the value of the investments is realized), managers are able to focus on creating long-run value and to avoid inefficient short-termism. Staggered boards also improve the firm's bargaining power in the event of a takeover bid; protected by a staggered board, managers can credibly refuse an opportunistic takeover offer and might use this power to elicit a higher offer for target shareholders (DeAngelo and Rice, 1983).

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

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

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

However, the debate on staggered boards continues unabated. Supporters of staggered boards mount vigorous defenses[2] and, as of mid-2014, over half of the 3000+ publicly traded companies tracked by FactSet Research Systems' Shark Repellent database

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

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

still maintain a staggered board structure. Moreover, from 2000 to 2014, an increasing proportion of IPO companies have adopted staggered boards. Whereas 44% of IPO firms in 2000 had staggered boards (Daines and Klausner, 2001), this structure is found in 80% of the 2014 IPO firms (WilmerHale, 2015).

The continued debate, in our view, stems from two challenges facing empirical research in this area. First, the research on staggered boards is almost entirely correlational, rather than causal.[3,4] Second, relatively little work is devoted to understanding the potential heterogeneous effects of staggered boards; it may be that staggered boards are beneficial for some firms, even if they are on average harmful. The case for staggered boards, even on its own terms, would apply only for a subset of public firms.

We contribute to the long-standing debate on staggered boards by providing causal empirical evidence on their effect on firm value. We examine a policy shock in Massachusetts, in which a 1990 Massachusetts (MA) law compelled the adoption of staggered boards (House Bill 5556) and construct a quasi-experiment by comparing the value of treated firms (firms that gained a staggered board because of the legislation) to the value of similar control firms from 1984 to 2004.

We also contribute to this literature by providing evidence on the heterogeneous effects of staggered boards. Most of the studies in this area have relied on datasets covering the largest and most mature firms in the public market (e.g., Bebchuk, Cohen, and Ferrell, 2009; Masulis et al., 2007; Cremers et al., 2014); by contrast, affected MA firms tend to be substantially smaller, younger, less profitable, more reliant on R&D and more likely subject to asymmetrical information. Our study therefore provides evidence on the causal

[3] Exceptions include Daines (2004) and Cohen and Wang (2013), which are event studies that compare market responses to legislative changes and court rulings, respectively, between affected and unaffected firms.

[4] The correlational nature of these studies, and their robustness to omitted variable biases, have been highlighted in the recent work of Cremers, Litov, and Sepe (2014). These authors find that the negative association between staggered boards and Tobin's Q do not hold in the presence of firm-fixed effects; rather, they document a positive and significant association in such specifications.

impact of staggered boards on those firms in earlier stages of their life cycles.

We study the impact of the legislation on firm value using a difference-in-differences (DID) research design. Our DID estimates suggest that, among the affected firms, the MA legislation led to an average increase in Tobin's Q of 14% over the next 15 years. Similarly, we document an increase in the median Tobin's Q of 13%. In subsample analyses, we find that the value improvement is concentrated among innovating firms—young firms or firms who invest in R&D. In particular, these effects are strongest among innovating firms who are covered by analysts and particularly subject to Wall Street pressures. We confirm, through a triple-differencing design, that these effects are not driven by differential trends in MA-incorporated firms post-1990. Consistent with these findings, we document that investing in a portfolio of these most affected (innovating and covered) firms and shorting a portfolio of their matched control firms produces a cumulative return of 560%..

In exploring potential mechanisms, we show that the increase in firm value that we document is in part explained by firms' greater willingness to invest and innovate. We find that the legislation led to a significant increase in capital and R&D expenditures among firms that were covered by analysts and either innovating or R&D intensive. Relatively young firms were also more likely to secure valuable patents after they were required to adopt staggered boards. In contrast to the entrenchment view, we do not find evidence that the legislation led to a significant decline in accounting profitability or firm leverage.

Overall, our findings are consistent with the view that staggered boards can be valuable for young, innovating firms that rely on long-term investments whose value are unclear to shareholders and to Wall Street analysts. For these firms, the stability and protection provided by a staggered board enables managers to focus on long run value and to make valuable investments.[5] These results are consistent with the finding that a

[5] Bhojraj, Sengupta, and Zhang (2014) make similar arguments in a correlational study based on the IRRC dataset using the Governance Index of Gompers, Ishii, and Metrick (2003) and the Entrenchment

large proportion of IPO firms adopt staggered boards. Shareholders have relatively strong incentives to adopt valuable governance structures when their firm goes public. The fact that so many IPO firms adopt staggered boards suggests that the greater insulation they provide is valuable for at least an important subset of firms (Daines and Klausner, 2001). Our findings do not rule out alternative channels through which staggered boards may increase firm value, however, such as providing greater independence to outside directors who cannot be replaced even by a control shareholder (Ganor, 2014).

Our study is unable to resolve the current debate on the effect of staggered boards among the largest and most mature public firms. We do not have evidence that staggered boards are helpful for the typical large public firm.

Section 2 explains why staggered boards matter and why they regulate exposure to the market for corporate control. Section 3 examines prior research on staggered boards. Sections 4 and 5 detail the Massachusetts legislation imposing staggered boards on public firms and its impact on long-run firm value and performance. Section 6 concludes.

2 Why Staggered Boards Matter

Companies have either a unitary or a staggered board structure. Directors on unitary boards stand for election in each annual shareholder meeting. In contrast, directors sitting on staggered boards are classified into separate classes—typically three—serving staggered terms. Because, shareholders vote only for one class of directors (1/3 of the board) in each election, a change in control requires winning shareholder votes in at least two consecutive annual meetings.

To understand why the staggered board is the most powerful common defense against takeovers, and why it is therefore the focus of debate, it is necessary to understand the poison pill, from which staggered boards derive their powerful anti-takeover force. As

Index of Bebchuk et al. (2009).

explained below, however, the poison pill, while justly famous, is potent only in firms with a staggered board. The main effect of the poison pill is that changes of control now happen via elections, rather than via the sale of shares (Gilson and Schwartz, 2001).

A poison pill is created when managers grant shareholders the "right" to buy a great deal of additional stock very cheaply in the event that anyone buys a block of shares (typically 10-20%) without managers' prior approval. In the event the pill is triggered, the bidder's ownership stake is drastically diluted, making an acquisition impossibly expensive for unapproved buyers. Thus, no bidder has ever intentionally triggered a poison pill and, as long as the pill is in place, it is an insurmountable defense against takeover.

Moreover, essentially all firms either have poison pills or can speedily adopt one whenever necessary, even after a hostile bid is announced.[6] Thus, to succeed, every hostile bid must be able to defeat a poison pill. Because pills can only be removed by a board of directors, bidders must wage a proxy fight to oust incumbent directors and elect new directors who might quickly remove the poison pill and allow the takeover to proceed. Note that once the bidder controls the board of directors, it can also quickly remove any other defense that operates at the board's discretion (such as control share, fair price, business combination, or super-majority provisions). These other discretionary defenses thus now impose no additional marginal cost for hostile bidders, given that they must always replace the board in order to remove a potential pill.

In short, because directors can adopt a poison pill at any time, incumbents must be voted out as part of every hostile takeover. The pill makes elections critical: hostile bidders must place an attractive offer on the table and persuade shareholders to replace incumbents with a slate of directors willing to reconsider the offer and pull the pill. Staggered boards delay these elections necessary to change control of the board and

[6] See, for example, Unitrin, where the Delaware Supreme Court upheld a poison pill adopted after the commencement of a tender offer.

this delay is costly for bidders who incur up-front search and bidding costs. Incumbent managers retain control of the firm during the interim and may frustrate the bidder's plans by seeking another buyer, selling valued assets, or pursuing incompatible strategies. Consistent with this, Bebchuk et al. (2002b) find that firms with classified boards are significantly less likely to be taken over.

Thus, if it is easy to remove incumbent directors in a proxy fight (i.e. a unitary board), managers will be relatively less protected from the market for corporate control; where it is difficult to remove incumbent directors (i.e. a staggered board) managers will be relatively more insulated.

3 Prior Research on Staggered Boards

Over the last decade, a great deal of research has examined how managerial behavior and firm value are affected by governance devices that insulate managers from the disciplinary forces of corporate control affect (e.g., Bertrand and Mullainathan, 2003; Gompers et al., 2003; Bebchuk et al., 2009; Cremers et al., 2014; Bebchuk, Cohen, and Wang, 2013; Atanassov, 2013). A center piece of this research, and the source of an intense debate, has been the value of staggered boards.[7]

Much of the empirical work on this topic seems to support the entrenchment view. The influential work of Bebchuk and Cohen (2005) documents that staggered boards are associated with lower firm valuation as measured by Tobin's Q. Consistent with the entrenchment view, Masulis et al. (2007) find that staggered-board firms tend to make value-decreasing acquisitions and Faleye (2007) finds that staggered boards are

[7] A large body of literature has been written on the effect of insulation from the market for corporate control on various firm outcomes using state anti-takeover statutes (e.g., Garvey and Hanka, 1999; Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010; Armstrong, Balakrishnan, and Cohen, 2012; Atanassov, 2013; Sapra, Subramanian, and Subramanian, 2014). However, much of this work has been puzzling to legal academics and corporate lawyers, who view these statutes as irrelevant and object that many studies do not properly identify the takeover threat faced by firms (Catan and Kahan (2014)).

associated with lower CEO pay-performance sensitivity and lower CEO performance-turnover sensitivity. Bates, Becher, and Lemmon (2008) find that staggered boards are associated with higher takeover premiums but also lower takeover likelihood; consistent with earlier work, they also document a negative association with firm valuation. Finally, the event studies of Daines (2004) and Cohen and Wang (2013) provide evidence that investors *view* staggered boards as reducing firm value.[8]

Despite this evidence, the debate continues to rage in part, we believe, due to the limitations of existing research evidence. First, with the exception of Daines (2004) and Cohen and Wang (2013), nearly all of the research on staggered boards is correlational and lacks an identification strategy. Second, much of this research has focused on the average effects on relatively larger and more mature firms that are covered by the Investor Responsibility Research Center (IRRC), and relatively little is known about the heterogeneous effects of staggered boards.[9]

Recent papers challenging the entrenchment view have fueled the debate further. Most notably, the recent work of Cremers et al. (2014) has challenged the well-known cross-sectional results of Bebchuk and Cohen (2005). They find that when firm-fixed effects are introduced in the empirical tests of Bebchuk and Cohen (2005), or analyzing the data in the time series, the association between staggered boards and firm value becomes positive

[8]The evidence of Daines (2004), which studies the market reactions to the passage of the Massachusetts legislation examined in this paper, suggest that markets were inefficient with respect to the value-implications of staggered boards in 1990. Consistent with this view, Bebchuk et al. (2013) shows that that market learning about the value-implications of insulating governance devices took place gradually over the decade of the 1990s. The event study of Cohen and Wang (2013) relies on two 2010 Delaware court rulings that affect the strength of staggered boards for a subset of Delaware-incorporate firms. However, their effects are local to a subsample of Delaware firms, which are in general different in characteristics relative to non-Delaware firms, e.g., larger in size and higher Q (Daines and Klausner, 2001).

[9]Recent work of Ahn and Shrestha (2013) and Duru, Wang, and Zhao (2013) examine potential heterogeneous effects. The former finds that staggered boards are positively associated with Tobin's Q in firms with low monitoring cost and greater advising needs, whereas the latter finds that the negative impact of staggered boards on firm valuation and accounting performance declines as the firm's opacity increases. Relatedly, Bhojraj et al. (2014) focus their analyses on the G-Index and the E-Index, which measures the degree of insulation provided by firms' governance mechanisms, and argue that innovative firms benefit from such insulation.

and significant. Their results suggest that de-classifying boards are associated with a decline in Tobin's Q of 6.3%, and argue that the cross-sectional association between firm value and staggered boards reflects the greater tendency of low value firms to adopt such governance structures. Though the authors acknowledge a lack of direct causal evidence, they argue that these findings "support the view that staggered boards help to commit shareholders and boards to longer horizons and challenge the managerial entrenchment interpretation that staggered boards are not beneficial to shareholders."[10]

Though the present study cannot adjudicate the debate on the average causal effect of staggered boards among the larger and more mature firms covered by Bebchuk and Cohen (2005) and Cremers et al. (2014), we contribute to this body of literature by leveraging a quasi-experimental setting in Massachusetts, detailed in the next Section. In doing so, we provide causal evidence of staggered boards on long-run firm value, and because our results apply to the set of affected Massachusetts firms that are relatively young and small, our findings speak to the heterogeneous effects of staggered boards.

4 The Massachusetts Legislation

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

[10] A similar study examining consequences of de-staggering, Ge, Tanlu, and Zhang (2014), finds that while board structure and Tobin's Q do not change, accounting performance and R&D fall; they thus challenge the view that de-staggering always improves firm performance.

Norton managers, employees, and Massachusetts legislators were less enthusiastic. Employees and local politicians were mobilized on the grounds that a takeover would bring layoffs and a reduction in the firm's charitable giving. The opposition quickly took on a nationalistic flavor. The Boston Globe announced "a surprise dawn attack on one of the oldest manufacturing concerns in Massachusetts." (Boston Globe, March 17, 1990) 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; Page 11). Other local 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 in demonstrations outside local government offices (Reuters, April 12, p.46), while others sang "God Bless America."

Massachusetts politicians also expressed "mounting concern" over foreign takeovers of "critically positioned US companies." They argued that because Norton also made ceramic parts used in the aerospace industry, the firm's independence was important to the national security of the U.S. and petitioned the Federal government to stop the impending takeover to protect national security (Financial Times, April 20, page 40).

Faced with ouster in the impending election, and seeking a staggered board, Norton managers sought from the state legislature what they could not get from shareholders. A bill imposing a staggered board on all Massachusetts firms was drafted with the aid of Wachtell, Lipton, Rosen & Katz, the law firm that invented the poison pill. A staggered board would prevent BTR from gaining a majority of the board seats in the upcoming election and give managers additional time to seek alternatives. The bill, MA House Bill 5556, provided that a board, once classified, could opt out of the coverage at its discretion.

However, this option offered shareholders little protection because, once protected by a staggered board, directors would have incentives to retain the protection.[11] Moreover, a board's decision to opt-out was not credible because it was later reversible (presumably even after receiving a hostile bid).

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

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

The next day, before cheering Norton employees, Gov. Dukakis signed the bill and celebrated 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; p. 49). Less than two weeks after winning the war of independence against foreign powers, Norton managers agreed to an acquisition

[11] We were able to confirm only one firm whose board opted out—EMC Corp., whose officers and directors also owned 47% of the firm's outstanding stock.

by the French conglomerate Compagnie de Saint-Gobain (the French apparently posed less of a threat to national security).

This legislation thus exogenously imposed a staggered board on MA-incorporated firms with no existing classification structure. In the next Section, we use these events as a quasi-experiment, comparing the value of treated firms (MA-incorporated non-staggered firms) to the value of control firms (non-MA-incorporated non-staggered firms), to study the impact of a staggered board.

5 Empirical Results

5.1 Sample Selection and Research Design

To investigate the long-run impact of staggered boards, our main empirical analyses examine the average effect of the legislation on the value of affected firms, i.e., MA-incorporated firms with no staggered board prior to the MA legislation ("treatment firms"). To estimate such an effect, we match the affected firms with a set of similar non-MA-incorporated firms with no staggered board ("control firms"). Our identification strategy relies on the assumption that the choice of state of incorporation—MA versus non-MA—between similar firms in the same 2-digit Global Industry Classification System (GICS2) industry is unrelated to the effect of staggered boards on firm value and performance.[12]

We first identify a broad set of potential treatment firms by identifying MA-incorporated firms with valid observations in the CRSP-Compustat Merged (CCM) database around the date of legislation. Specifically, we look for firms with an annual filing before and after the legislation. We also require firms to have proxies available for 1989 or 1990, obtained from either Lexis Nexis or Compact Disclosure, in order to determine whether a

[12]GICS industry groupings has been shown to better explain the cross-sectional variation in stock returns, financial ratios, and valuation multiples (Bhojraj, Lee, and Oler, 2003).

firm had a staggered board prior to the legislation. Finally, we also eliminate firms that have already signed merger agreements as well as REITs, due to their unique governance structure. These filters result in a hand-collected list of 103 MA-incorporated firms at the time of the legislation, for which 66 did not have staggered boards and 37 did. Among the 66 potential treatment firms, we remove those that have reincorporated since 1990 or for which the most recent incorporation information is unavailable, dropping 8 non-staggered firms, those firms with missing values in total assets, firm age, research and development expense, or capital expenditures in 1990, dropping 2 firms. We also dropped 1 firm (EMC) that opted out of the MA legislation. All variables used in the study are defined in Table A1. Our final sample consists of 55 treatment firms, for whom we obtain all available financial data from CCM from 1984 to 2004.[13]

We follow similar steps to above to identify a set of potential non-MA-incorporated non-staggered control firms: we require them to have valid observations in CCM around the date of legislation, to have proxies available for 1989 or 1990, and to have a valid state of incorporation. We filter out firms with staggered boards in 1990 as well as any Delaware-incorporated firms, due to their unique corporate legal environment that might lead to a different selection of firms to incorporate there.[14]

From this pool we construct a matched control sample by matching, for each treatment firm, the closest (in Mahalanobis distance) two firms within the same 2-digit Global Industry Classification (GICS2) industry in terms of the following firm characteristics: pre-1990 mean total assets, pre-1990 mean book-to-market ratio, and firm age as of 1990.[15] Our resultant control sample contains 110 non-MA-incorporated non-staggered firms, for whom all available financial data are obtained from CCM from 1984 to 2004.

[13] Applying these filters to the MA non-treated (i.e., with staggered boards) firms results in a final sample of 29.

[14] Firms incorporate either in their home state or in Delaware and Delaware firms are larger and more likely to engage in M&A transactions(Daines and Klausner, 2001).

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

5.2 Summary Statistics

Table 1 reports summary statistics on the characteristics—in terms of size, age, Tobin's Q, performance, leverage, information asymmetry, and investments—of treated firms and their matched controls in 1990. Columns (1) and (2) report the mean control and treatment firm values, respectively, with the differences and t-statistics reported in columns (3) and (4). These results indicate that our treated and matched control firms are statistically indistinguishable from each other at the mean for each of the background characteristics examined. Notably, our treated and matched control firms are virtually identical in their mean Tobin's Q (1.340 for the matched controls and 1.420 for the treated firms). In unreported results, we also find that the median values in each of these firm characteristics between the control and treated firms are statistically indistinguishable from each other; again, Tobin's Q is virtually identical among the two groups at the median (1.07 for matched control firms and 1.05 for treated firms).

Column (5) reports the percentile rank represented by the treated firm average relative to the CCM population in 1990. For example, the treatment sample's mean total asset (age) of \$779 million (11.4 years) is larger than 79.9% (65.2%) of CCM firms.[16] More importantly for our study, column (6) reports the percentile rank relative to the population of firms included in the Investor Responsibility Research Center (IRRC), the set of firms on which much of the prior work on staggered boards and governance has been based (e.g., Gompers et al., 2003; Bebchuk and Cohen, 2005; Masulis et al., 2007; Bebchuk et al., 2009, 2013; Cremers et al., 2014; Bhojraj et al., 2014). Relative to the IRRC sample of firms in 1990, the "average" treated firm in our sample is comparatively small, young, faces greater information asymmetry, and less profitable in terms of ROE and ROA; the average firm has total assets that is approximately in the 36^{th} percentile of

[16]Note that the median firm in our sample has total assets (age) of \$43 Million (7 years), which is larger than 42% (55%) of the CCM firms in 1990.

the IRRC sample, faces information asymmetry greater than 99.8% of the IRRC sample, and is older than only 23% of the IRRC firms.[17] Thus, the treatment effects estimated in this study pertain to firms earlier in their life cycles relative to the larger and more mature firms covered by the IRRC.

5.3 Effect of MA Legislation on Tobin's Q

Following prior literature, our primary analyses focus on the impact of staggered boards on Tobin's Q (e.g., Gompers et al., 2003; Bebchuk and Cohen, 2005; Cremers et al., 2014). Table 2 reports our baseline estimates on the average treatment effects on the MA-treated firms using difference-in-differences (DID) specifications. Column (1) reports a basic specification from pooled OLS regressions of *tobin's q* on a treatment indicator ("Treat"), a post-legislation indicator ("Post"), and an interaction of the two variables ("Treat x Post"). We note that neither the "Treat" and "Post" variables are significantly different from 0 at the 10% level, suggesting that the treated and control firms are not significantly different in *tobin's q* pre-treatment, consistent with Table 1, and that there is not a significant post-treatment trend in *tobin's q* among the control firms. The interaction term, the DID estimator, from columns (1) to (3) suggest that the MA treated firms experienced a ∼14% improvement in Tobin's Q due to the imposition of staggered boards.

We complement the above results by investigating the treatment effect on median Tobin's Q. Although examining the treatment effects on the mean of the outcome distribution is standard, the effect on other parts of the outcome distribution is also important. In our view, policymakers and researchers should be interested in the effect of policy on the median firm in the distribution. Thus, in Table 3 estimates the difference-in-differences

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

specifications of the previous table, but using median regressions (Koenker and Bassett, 1978). The coefficient of interest—on the interaction term—can be interpreted as the treatment-control difference in the differences between pre- and post-legislation *median* Tobin's Q.

The results of Table 3 are largely consistent, both in terms of statistical and economic significance, with those of Table 2: the MA legislation increased the median Tobin's Q of treated firms relative to control firms over the same period. Based on the specifications of (3), we find that the MA legislation led to an increase in the median Tobin's Q by 13%. We note that the absolute magnitudes of the effects documented in the present study are comparable to those of Bebchuk and Cohen (2005), who find, among the sample of IRRC firms, that firms with staggered boards are associated with an on average 0.21 lower Tobin's Q. Column (4) of Tables 2 and 3 show that for the sample of MA treated firms, staggered boards led to an improvement in value of similar magnitudes (0.26 at the mean and 0.18 at the median) but in opposite directions.

Together, these results suggest that staggered boards can have positive effects on the value of certain firms. Our treatment firms tend to be younger and smaller than the typical IRRC firm. We further investigate the possibility of staggered boards' heterogeneous effects within our sample. In particular, we focus on the effect of staggered boards on innovating firms, for whom it may be particularly beneficial to have a staggered board. Innovating firms, which we define as young firms or firms investing in research and development, are expected to require a longer horizon to execute their strategy, for whom there is likely greater information asymmetry between insiders and outsiders, and for whom success may require tolerance for early failures (Manso, 2011). For such firms, staggered boards might be especially valuable if they allow managers to invest in valuable projects whose value is clear only in the long-run.

Column (1) of Table 4 reports the expanded OLS specification in column (4) of Table

2—with time and industry fixed effects as well as firm controls—for the subsample of innovating treatment firms (with age below the 50^{th} percentile in the 1990 CCM population and with positive R&D expense in 1990) and their matched controls.[18] We find that the baseline positive effect of staggered boards on Tobin's Q are concentrated in the innovating firms, who experienced a ∼18% increase in firm value from the MA legislation. In contrast, we find a negative but statistically insignificant DID coefficient for the subsample of non-innovating firms.

Column (3) further investigates the subsample of innovating firms that have analyst coverage. The insulation that staggered boards provide may be more beneficial among firms subject to the pressures and earnings expectations of Wall Street analysts, which critics claim can lead to myopic behavior (e.g., Bhojraj, Hribar, Picconi, and McInnis, 2009; Terry, 2015). Our analysis suggests that the benefits of staggering is strongest among the set of innovating and covered firms, who experienced a ∼22% increase in Tobin's Q. In contrast, we find no significant effect for the subset of non-innovating or non-covered treatment firms (Column (4)).

Complementing these findings, we show in Figure 1 the returns to a zero-investment portfolio that goes long an equal-weighted portfolio of MA-incorporated firms that were most affected by the legislation and short an equal-weighted portfolio of their control firms.[19] Consistent with our results on Q, we find that an investment in innovating and covered firms produces a 560% return by the end of 2004; similarly, an investment in young and covered firms or R&D and covered firms result in returns of 845% and 592%, respectively.

Overall, our findings are consistent with the fact that staggered boards are much more common among IPO firms than among mature firms. However, our research setting and

[18] 32% of the CCM sample in 1990 report a positive R&D expense.

[19] Portfolios are rebalanced monthly. Dollar amounts for firms that drop out of the sample are reinvested equally across the remaining firms in the portfolio.

results do not speak to the causal effect of staggered boards for those firms later in their life cycles.

5.4 Investment and Operating Performance

In this subsection we investigate the potential channels through which firm value is improved. We first examine how firms' investments in capital expenditures and research and development were affected by the MA legislation. Table 5 reports DID estimates for *CAPEX* and *R&D* using the entire sample (columns 1 and 3), the subsample of innovating and covered firms (columns 2 and 4), as well as the subsample of R&D intensive (i.e., R&D expenditure belonging to the top 80 percentile of the CCM population in 1990) and covered firms (columns 3 and 6). Note that we replace missing values in Capital Expenditure and R&D with zeros; in all regression specifications an indicator variable for missing values in the dependent variable are included.

We find that the MA legislation led to a significant increase in capital expenditures and R&D investments among the subset of innovating or R&D intensive firms who were most susceptible to the pressures of Wall Street. Regressions of *CAPEX* suggest that the MA legislation led to a ∼19% increase in capital expenditures among innovating and covered firms, an effect both economically significant and also statistically significant at the 5% level. Similarly, we report a ∼20% increase in capital expenditures among R&D intensive and covered firms, a statistically significant effect at the 10% level. Our findings on R&D are less strong statistically. Among innovative and covered firms we find a point estimate of a ∼3% increase in R&D expenditure, but it is not distinguishable from 0 at the 10% level. Among R&D intensive and covered firms, however, we document a ∼26% increase in R&D expenditure.

We also analyze the effect of the legislation on patent generation.[20] Table 6 re-

[20]Our patent data are from the Thomson Innovation database which provides the world's most comprehensive international patent coverage from as early as the 19^{th} century to the present day. We collected

ports DID estimates for *Patents* and *Citation-Weighted Patents* using the entire sample (columns 1 and 5), the subsample of innovating and covered firms (columns 2 and 6), the subsample of young firms (columns 3 and 7), and the subsample of young and covered firms (columns 4 and 8). We show that the MA legislation led to a significant increase in patent generation, in particular among the subset of young firms who face greater market pressure. Columns (3) and (7) suggest that the MA legislation led to a ∼26% and ∼47% increase in *Patents* and *Citation-Weighted Patents*, respectively, among young firms, an effect both economically significant and also statistically significant at the 10% level. These effects are particularly strong for young and covered firms, which saw a ∼54% and ∼98% increase in *Patents* and *Citation-Weighted Patents*, respectively, with both coefficients being statistically significant at the 5% level.

Overall these results suggest that the value improvement from staggering can be, at least in part, explained by firms' greater willingness to make investments in growth and innovation. Firms that relied on innovation and that faced analyst pressure experienced significant growth in capital investments, R&D expenditures, and patents. These findings are consistent with that claim that, for firms facing unusually high information asymmetries, Wall Street's scrutiny and short-run earnings targets, staggered boards afford management valuable stability and a longer-run horizon for investments (Graham, Harvey, and Rajgopal, 2005; He and Tian, 2013) However, our results do not rule out other potential channels explaining the increase in firm value due to the MA legislation, for example, staggered boards improve value by providing greater independence to outside directors (Ganor, 2014).

We also examine the effect of staggering on operating performance and leverage.

information on all the U.S. patents our treatment and their matched control firms had applied for between January 1, 1984 and December 31, 2004 that were ultimately granted. Since there is a significant gap between 2004 and 2015, the "truncation problem" challenging empirical studies using patent data (i.e., fewer patent applications towards the end of the sample period are included due to the time lag between application and approval) is alleviated in our setting.

Prior studies suggest that insulating governance mechanisms such as staggered boards are associated with worse operating performance (e.g., Gompers et al., 2003; Bebchuk et al., 2013). In Table 7 columns (1)-(4), we report DID estimates on *roe* and *roa*. In contrast, we do not find a significantly negative effect on operating performance from staggered boards in our sample. In particular, we find economically meaningful point estimates among the subsample of innovating and covered firms: a $\sim 5\%$ increase in gross ROE and $\sim$2% increase in gross ROA. However, none of the specifications obtains a DID estimate that is statistically distinguishable from zero at the conventional levels.

Finally, we also examine the effect on leverage in columns (5) and (6) of Table 7. As with the above, we find an economically significant positive point estimate for innovative and covered firms—an increase of $\sim$11%—but do not find statistical significance. Together with the results of Table 5, we do not find evidence that, among the relatively younger and smaller firms covered in our study, staggered boards lead mangers to lead the quiet life or abuse the job security afforded to them.

5.5 Robustness Tests

5.5.1 Addressing Differential Trends in MA versus Other States

We begin by examining the possibility that our findings of the MA legislation leading to improvements in firm value captures a differential economic trend experienced by MA firms post 1990. To do so, we utilize the sample of MA non-treated (i.e., staggered prior to the 1990 legislation) and their matched control firms (i.e., non-MA firms staggered in 1990) to take out any MA versus non-MA differences in Tobin's Q between the pre- and post-treatment periods.

In particular, we estimate a difference in the difference-in-differences design ("DDD") to test whether our primary results can be explained by a differential trend in MA. This "triple diff" design can be estimated in a regression on a series of group and period indicators and their interactions: a MA firm indicator ("MA firm"), an indicator for firms not having staggered boards prior to the MA legislation ("No Pre-1990 SB"), a post legislation indicator ("Post"), and all possible interactions. The coefficient of interest, on the triple interaction between "No Pre-1990 SB", "MA firm", and "Post" is the difference between the mean DID of MA non-staggered and non-MA non-staggered firms and the mean DID of MA staggered and non-MA staggered firms.

Table 8 reports our DDD estimates for the whole sample, the subsample of innovating firms, and the subsample of innovating and covered firms in columns (1), (2), and (3), respectively. Column (1) reports a DDD coefficient of 0.1806—a 18.06% improvement in Tobin's Q from staggering—that is statistically significant at the 10% level. The subsample analyses in columns (2) and (3) report effects of 21.10% and 25.20% for innovating and innovating and covered firms, respectively; however, we do not find statistical significance at the standard levels for these coefficients.

Because the triple interaction coefficient reflects the difference between two DID estimates, greater estimation noise, standard errors, and thus the general attenuation in statistical significance in the DDD specification are expected, particularly in the subsample analyses where sample sizes used to estimate averages are further limited. Our focus here, therefore, is mainly on the magnitude of the effects.

Across the board, the magnitude of the DDD-estimated effects are comparable to the DID-estimated effects of Tables 2 and 4. In fact, the magnitudes of the DDD estimates are slightly larger. Overall, the findings of Table 8 suggest that our main results estimated using DID are unlikely to be driven by differential trends in Tobin's Q experienced by MA-incorporated firms relative to non-MA-incorporated firms post-legislation.

5.5.2 Addressing Variations in the Treatment Window

We next assess the stability in the treatment effect on Tobin's Q by considering alternative treatment windows. After the MA legislation, it may have taken firms some time to adjust their behavior and for market valuations to respond. Furthermore, the legitimacy of poison pills, and thus the antitakover force of staggered boards, were being cemented in the early-to-mid 1990s, with the *Paramount v. Time* decision in 1990 and related cases.

In Table 9 we compare the baseline DID estimates that use all data from 1984 to 2004, reported in column (1), to specifications in which we account for different "adjustment periods" by removing the interim years. In columns (2), (3), and (4), we exclude 1990, 1990 to 1991, and 1990 to 1994 data, respectively, from our DID estimation. We make two observations from these results. First, our main treatment effect estimates in column (1) are not driven by the years immediately after the adoption of staggered boards, and are relatively stable over time. Second, the DID point estimates are in fact increasing as our exclusion window expands, consistent with the effect of staggering on firm value being greater over a longer-run horizon.

6 Conclusion

Staggered boards remain the center of debate and controversy in corporate governance. This debate is in part fueled by the conflicting results in academic research on the average effect of staggered boards. Prior research on staggered boards moreover is limited by the fact that it largely lacks causal identification and has not examined staggered boards' heterogeneous effects.

This study exploits a quasi-experimental setting stemming from a 1990 law requiring all Massachusetts-incorporated firms to adopt a staggered board. Our evidence suggests staggered boards can be beneficial for relatively young firms that rely on investments in growth and innovation, ultimately resulting in greater firm value in the long run. These findings contribute to the academic literature and the corporate governance debate by providing a research setting with plausible causal identification that allows an examination of the heterogeneous effects of staggered boards.

We caution that these findings should not be interpreted to suggest that staggered boards unambiguously improve firm value. Our work does not suggest that staggered boards are valuable for large and more mature firms (Bebchuk and Cohen, 2005; Cremers et al., 2014). However, we believe that it is important and useful to examine the effects of staggered boards on managerial behavior and firm value over the life cycle of a firm.

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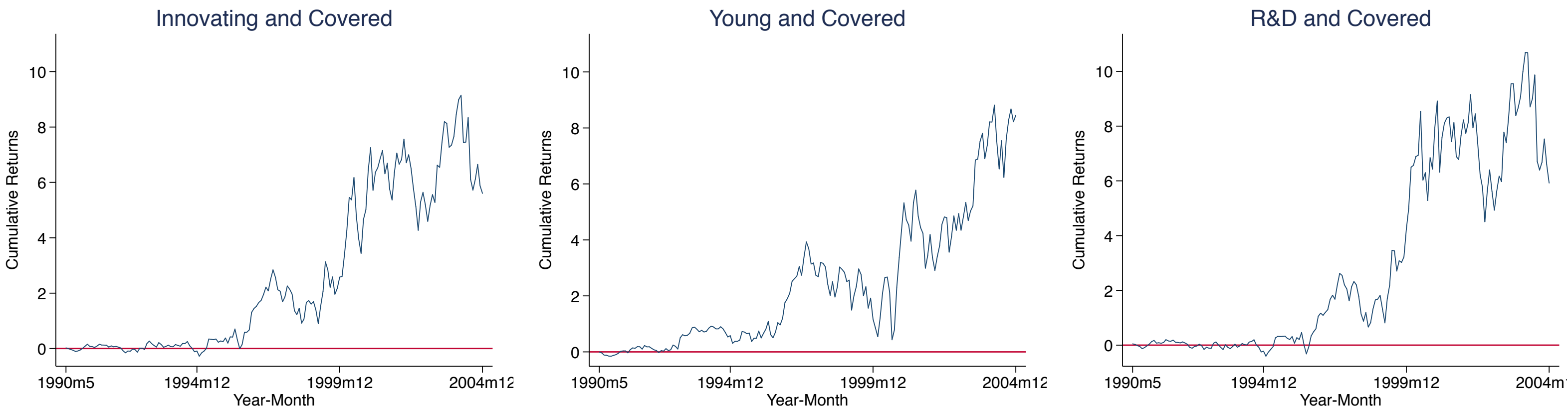

Fig. 1. This figure graphs the difference between cumulative returns of an equal-weighted portfolio of firms affected by the legislation and the cumulative returns of an equal-weighted portfolio of the control firms from April 1990 to December 2004. Portfolios are re-balanced each month.

Table A1.
Variable Description

Table A1 reports definitions of variables used in our regressions. Our financial and corporate data are obtained from the CRSP-Compustat Merged database; Compustat (CRSP) variable names are referred to in square brackets (parentheses) below. Patent data are from the Thomson Innovation database. We replace the missing values for depreciation, R&D expense, capital expenditure, and number of patent citations with zeros. All dependent variables are truncated at the 1% level.

Variable	Description	Calculation
Dependent Variables		
Tobin's Q		(total assets [at] + price [prcc_c] × commonshare [csho] − equity [ceq] − deferred taxes [txdb]) / assets [at]
tobin's q	Natural Logarithm of Tobin's Q	
ROE	Return on Equity	(operating income before depreciation [oibdp] − depreciation [dp]) / total common equity [ceq]
roe	Natural Logarithm of (1+ ROE)	
ROA	Return on Assets	(operating income before depreciation [oibdp] − depreciation [dp]) / total assets [at]
roa	Natural Logarithm of (1+ ROA)	
Leverage		liabilities [lt] / total assets [at]
leverage	Natural Logarithm of Leverage	
R&D	Research and Development Expense	[xrd]
R&D	Natural Logarithm of (1+R&D)	
CAPEX	Capital Expenditure	[capx]
CAPEX	Natural Logarithm of (1+CAPEX)	
Patents	Number of patents applied by the firm that were eventually granted	
Patents	Natural Logarithm of (1+Patents)	
Citation-Weighted Patents	Patents weighted by the number of citations each patent received in subsequent years until 2015	
Citation-Weighted Patents	Natural Logarithm of (1+Citation-Weighted Patents)	
Matching and Control Variables		
Book to Market		(equity [ceq] + deferred taxes and investment credit [txditc]) / market cap [prcc_f × csho]
Assets	Total Assets	[at]
assets	Natural Logarithm of Assets	
Age	Firm Age (in years)	Number of years since first observed PERMNO on CRSP
age	Natural Logarithm of Firm Age	
Indicator Variables		
Post	Post-legislation indicator	equals 1 if the fiscal year end occurred after 1990.
Treat	Treatment indicator	equals 1 if the firm is a MA incorporated firm without staggered board prior to 1990
No Pre-1990 SB	Non-staggered-board indicator	equals 1 if the firm had no staggered board prior to 1990
MA firm	MA indicator	equals 1 if the firm is a MA incorporated firm
Other Variables		
Info Asymmetry	Amihud illiquidity ratio	Daily average of 1000000×|(ret)| / |(prc)|×(vol) from January 1 to March 30 of 1990

Table 1.
Summary Statistics on Matched Sample

This table reports comparisons of means in 1990 in firm characteristics between the control firms (non-Massachusetts-incorporated firms with no staggered boards in 1990), reported in column (1), the treated firms (Massachusetts-incorporated firms with no staggered boards in 1990), reported in column (2), their differences, reported in column (3), and the t-statistics associated with the differences in means, reported in column (4). t-statistics are computed based on cluster-robust standard errors, clustered by firm. Columns (4) and (5) report the percent of firms in 1990 in the CRSP-Compustat Merged (CCM) database and the Investor Responsibility Research Center (IRRC) database, respectively, with values lower than the treated sample mean.

	(1)	(2)	(3)	(4)	(5)	(6)
Firm Characteristics	Control	Treated	Δ	T	Pctile CCM	Pctile IRRC
Total Assets	255.974	779.305	523.331	1.582	79.9%	35.6%
Firm Age	11.283	11.389	0.106	-0.005	65.2%	22.9%
Tobin's Q	1.340	1.420	0.080	0.502	68.8%	75.8%
Return on Equity	0.081	0.090	0.009	0.143	39.5%	18.7%
Return on Assets	0.049	0.064	0.015	0.723	49.6%	28.7%
Leverage	0.462	0.475	0.013	0.375	38.1%	19.0%
Info Asymmetry	11.296	11.077	-0.219	-0.040	75.8%	99.8%
R&D Expense	5.898	7.349	1.451	0.632	76.9%	25.7%
Capital Expenditure	13.129	21.472	8.343	0.876	85.0%	29.0%

Table 2.

Local Average Treatment Effect on *tobin's q* & Tobin's Q

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

	(1)	(2)	(3)	(4)
Treat x Post	0.1413**	0.1443**	0.1403**	0.2631**
	(0.061)	(0.062)	(0.060)	(0.133)
Treat	-0.0316	-0.0342	-0.0343	-0.0397
	(0.058)	(0.060)	(0.050)	(0.102)
Post	-0.0269			
	(0.054)			
Time FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Firm Controls	No	No	Yes	Yes
Observations	2,473	2,473	2,473	2,473
Adj R^2	0.0079	0.0394	0.2054	0.1653

Table 3.
Median Difference-in-Difference

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

	(1)	(2)	(3)	(4)
Treat x Post	0.1924**	0.1755*	0.1295**	0.1757*
	(0.087)	(0.095)	(0.066)	(0.095)
Treat	-0.0443	-0.0409	-0.0468	-0.0581
	(0.073)	(0.081)	(0.051)	(0.069)
Post	-0.0360			
	(0.057)			
Time FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Firm Controls	No	No	Yes	Yes
Observations	2,473	2,473	2,473	2,473
Psuedo R^2	0.0043	0.0195	0.1235	0.0854

Table 4.

Heterogeneous Effects on *tobin's q*

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

	(1)	(2)	(3)	(4)
	Innovating	Non-Innovating	Innovating and Covered	Non-Innovating or Non-Covered
Treat x Post	0.1838**	-0.0563	0.2151**	0.0867
	(0.073)	(0.050)	(0.099)	(0.073)
Treat	-0.0628	0.1619***	-0.0988	0.0165
	(0.058)	(0.059)	(0.065)	(0.058)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	2,084	389	1,082	1,391
Adj R^2	0.1568	0.5082	0.2218	0.2026

Table 5.

Investments

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

	(1)	(2)	(3)	(4)	(5)	(6)
		CAPEX			*R&D*	
	All	Innovating and Covered	R&D Intensive and Covered	All	Innovating and Covered	R&D Intensive and Covered
Treat x Post	-0.0101	0.1929**	0.1977*	-0.0714	0.0304	0.2635*
	(0.074)	(0.092)	(0.109)	(0.082)	(0.134)	(0.141)
Treat	0.0955	0.0749	0.0775	-0.0236	0.0561	0.3692*
	(0.070)	(0.122)	(0.143)	(0.113)	(0.232)	(0.218)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,509	1,086	656	2,500	1,075	645
Adj R^2	0.8637	0.8429	0.8528	0.6240	0.6296	0.7706

Table 6.
Patents

This table reports the results of OLS regressions using *Patents* (columns 1–4) and *Citation-Weighted Patents* (columns 5–8) as the dependent variables. Column 1 (5) reports the results using the full sample of treatment and matched control firms. Column 2 (6) reports results using the subsample of "innovating and covered" treatment firms and their matched control firms. Column 3 (7) reports results using the subsample of "young" treatment firms and their matched control firms. Column 4 (8) report results using the subsample of "young and covered" firms and their matched control firms. Innovating firms are those with positive R&D expense or those who are young, i.e., whose age (as of 1990) is below the median of the CRSP-Compustat Merged database universe. Covered firms are those firms with analyst coverage in at least one of the four quarters prior to the legislation. *assets* and *age* are included as firm controls. All variables are defined in Table A1. Standard errors are two-way-cluster robust, clustering at the firm and year and reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Patents				*Citation-Weighted Patents*			
	All	Innovating and Covered	Young	Young and Covered	All	Innovating and Covered	Young	Young and Covered
Treat x Post	0.0019	0.0962	0.2614**	0.5397**	0.0137	0.3536	0.4676*	0.9824**
	(0.093)	(0.201)	(0.125)	(0.233)	(0.194)	(0.400)	(0.267)	(0.477)
Treat	0.0932	0.1655	0.0804	0.1051	0.2481	0.4187	0.3000	0.4873
	(0.097)	(0.217)	(0.100)	(0.213)	(0.216)	(0.460)	(0.270)	(0.515)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,477	1,050	1,181	541	2,481	1,054	1,176	537
Adj R^2	0.1578	0.2320	0.1258	0.2337	0.1497	0.2289	0.1148	0.2272

Table 7.
Operating Performance and Leverage

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

	(1)	(2)	(3)	(4)	(5)	(6)
	roe		*roa*		*leverage*	
	All	Innovating and Covered	All	Innovating and Covered	All	Innovating and Covered
Treat x Post	0.0059	0.0520	-0.0002	0.0205	0.0066	0.1068
	(0.035)	(0.044)	(0.019)	(0.026)	(0.067)	(0.096)
Treat	-0.0030	-0.0325	0.0022	-0.0111	-0.0307	-0.0252
	(0.026)	(0.042)	(0.013)	(0.025)	(0.074)	(0.111)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,416	1,054	2,468	1,070	2,512	1,089
Adj R^2	0.1410	0.1773	0.1347	0.1969	0.1797	0.1015

Table 8.
Robustness: Diff-in-Diff-in-Diff in *tobin's q*

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

	(1) All	(2) Innovating	(3) Innovating and Covered
No Pre-1990 SB x MA firm x Post	0.1806*	0.2110	0.2520
	(0.101)	(0.153)	(0.196)
No Pre-1990 SB x Post	-0.1425**	-0.1770*	-0.1695^{+}
	(0.072)	(0.098)	(0.112)
MA firm x Post	-0.0391	-0.0303	-0.0520
	(0.082)	(0.135)	(0.178)
No Pre-1990 SB x MA firm	0.0051	-0.0631	-0.1573
	(0.078)	(0.110)	(0.126)
No Staggered Board Pre-1990	0.0601	0.1060	0.0942
	(0.061)	(0.076)	(0.092)
MA firm	-0.0285	0.0158	0.1013
	(0.051)	(0.087)	(0.112)
Time FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Observations	3,870	2,847	1,542
Adj R^2	0.2580	0.1871	0.1933

Table 9.

Robustness: Stability in Treatment Effect on *tobin's q*

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

	(1) All Years	(2) Exclude 1990	(3) Exclude 1990-1991	(4) Exclude 1990-1994
Treat x Post	0.1403**	0.1417**	0.1501**	0.1684**
	(0.060)	(0.063)	(0.068)	(0.076)
Treat	-0.0343	-0.0360	-0.0346	-0.0337
	(0.050)	(0.053)	(0.053)	(0.053)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	2,473	2,313	2,158	1,727
Adj R^2	0.2054	0.2023	0.1986	0.2051